Do democracies provide better education? Revisiting the democracy–human capital link

Abstract

Democracy is widely regarded as the superior regime type when it comes to providing access to education, and several studies find that democracy enhances educational enrollment and years of schooling. Yet, we do not know whether democracies provide better education. We offer the first systematic cross-national study of whether democracy affects education quality, and thus the skills and knowledge of citizens. We contend that democracies should not be expected to outperform autocracies in offering high-quality education. First, reforming education systems in order to enhance quality is inherently difficult, even for well-intentioned governments with ample resources. Second, education quality is less visible to voters than, e.g., expanding education enrollment, making quality-enhancing policies a less attractive option for office-seeking democratic politicians. We employ a recent dataset comparing international student achievements tests for 128 countries, from the 1960s onwards, and find clear evidence that democracies do not systematically offer better education than autocracies.
1 Introduction

A vast literature, employing different measures and drawing on contemporary and historical data from different regions of the world, suggests that democracies are more likely than autocracies to provide more (and cheaper) education to their citizens (Lake and Baum, 2001; Baum and Lake, 2003; Bueno de Mesquita et al., 2003; Lindert, 2005; Brown, 1999; Brown and Hunter, 2004; Stasavage, 2005; Engerman and Sokoloff, 2005; Engerman, Mariscal and Sokoloff, 2009; Ansell, 2010; Gallego, 2010; Huber and Stephens, 2012; Acemoglu et al., 2013; Harding and Stasavage, 2014). Countries experiencing successful democratic transitions can thus, over the coming years, expect increased public spending on education, lower schooling fees, a larger share of their children enrolled into primary and secondary school, and that their youth will, on average, spend more years in school (but, see Murtin and Wacziarg, 2014). The literature also suggests very plausible reasons for why democracy induces these outcomes. For instance, democratic politicians accountable to a wide constituency – many of whom have kids in schooling age – are incentivized to respond to calls for more education spending, lower school fees, or expand education access to large population groups in order to be re-elected.

But, does democracy promote education quality? In other words, do democracies provide citizens with an education that raises their knowledge and skill levels relative to the education provided in autocracies? Recent studies have reported large variations in the extent to which education systems boost learning outcomes, and thereby strengthen human capital (i.e., the skills and knowledge of citizens). The latter is, in turn, argued to promote a number of beneficial outcomes such as economic growth, social trust, and higher political participation.

With one notable recent exception (Harding and Stasavage, 2014), there is a lack of systematic empirical studies on regime types and education quality – the main reason presumably being that cross-country data on education quality have been scarce. One might intuitively expect the clear answer to the question of whether democracy promotes education quality to be “yes”. Why would voters not pressure politicians to give their kids a high-quality education, and not only increase the number of years they stay in school? In autocracies, truly competitive elections are lacking, leaving citizens with one less tool available for pushing for an education system that properly teaches their children skills such as reading and basic mathematics. Instead, autocratic regimes might
expend effort and resources to provide high-quality education only for the children of their (often fairly narrow) supporting groups, or use the national education system to indoctrinate government ideology rather than teaching cognitive skills.

A very plausible hypothesis is thus that democracy enhances not only “education quantity”, but also “education quality”. Yet, we remain skeptical. First, previous empirical studies suggest that education quality is intrinsically hard to increase by legislation or by simply increasing the education budget (see, e.g., Hanushek and Woessmann, 2007). Second, democratic politicians may not have as strong incentives to expend resources and efforts to increase education quality after all. Following Harding and Stasavage (2014), we highlight that the quality of education (and policies that may improve it) is often difficult to verify and monitor by voters. Hence, in a world of budget constraints, democratic politicians may be wise to rather channel resources to measures increasing (more visible) features associated with “education quantity”. We elaborate on this argument below, before subsequently testing for a potential effect of democracy on education quality.

While studies on democracy and education quantity abound, we provide the first systematic analysis drawing on cross-country data to assess the relationship between democracy and education quality. To do so, we employ the recent dataset by Angrist (2013) containing comparable measures on international students achievements tests from 128 countries. Both anecdotal evidence and recent studies reviewed in the next section suggest that the gap between education quantity and education quality can be huge.¹

In Section 2, we briefly review relevant literature on democracy and education, as well as recent studies using measures of education quality to study other phenomena. Thereafter, in Section 3, we elaborate on our (two-pronged) argument for why democracy might not lead to higher-quality education outcomes. In Section 4, we present the data, focusing on our preferred measure of education quality, before we proceed to the empirical analysis in Section 5. We first replicate the finding that democracy is positively related to measures of education quantity (average years

¹Consider, for example, Africa, where, according to the Brookings Institution, nearly 111 million out of Africa’s 128 million school-aged children attended school in 2010 after a massive education expansion in recent years. However, 37 million of them learn so little in school that they will “not be much better off than those kids who never attend school” (van Fleet, 2012).
of schooling as well as primary and secondary enrollment ratios). However, the core finding – which is a novel, important, and perhaps (to many) surprising null finding – is that democracy does not relate to education quality. This result holds across different cross-section and panel data specifications, samples, and for different measures (with exception for some models employing reading skills only as dependent variable). One might hypothesize that the null-relationship between democracy and nation-wide measures of education quality is simply due to the expansions of enrollment rates under democracy; more students entering school puts pressure on teaching facilities and new groups with worse initial prospects for learning (such as kids living in poor rural areas) are channeled into the schooling system. However, we test for this potential alternative explanation and find no support for it. Democracy is not associated with education quality, even when accounting for the expanded access to education. In sum, democracy may increase the number of kids in school and boost funding to primary and secondary education, but it does not improve the measurable capacities, skills and knowledge of its young citizens.

2 Democracy, education, and education quality

The theoretical arguments in the literature are mainly concerned with how going from autocracy to democracy incentivizes politicians to promote aspects of education quantity, such as expanding education access or channeling a larger chunk of public budgets to schooling. Meanwhile, extant empirical studies use education measures that reflect these arguments, such as enrollment rates, the share of the population having completed primary, secondary and tertiary education, or the abolition of school fees. We will not survey this vast literature in its entirety, but only mention some prominent arguments and results:

Lake and Baum (2001) describe how the political competition within democracies generate political dynamics that increase the provision of public services at the expense of rents extracted by leaders. In contrast, autocratic politicians can extract substantial rents by limiting such services without facing grave consequences. A slightly different argument for why democracy widens access to and increases funding of education is provided by Bueno de Mesquita et al. (2003). They highlight that broad-based education systems can be characterized as public goods, and that investing in such systems is a cost-effective way to maintain political support for leaders

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with large “winning coalitions” (as in democracies), and relatively expensive for leaders with small coalitions (as in many autocracies). Acemoglu and Robinson (2006) also theorize that the relatively poor majority will push for universal schooling under democracy (where they hold more political power), whereas autocratic elites will shun such expensive schooling systems that are also benefitting the non-elites (see also Meltzer and Richards, 1981; Saint-Paul and Verdier, 1993; Boix, 2003; North, Wallis and Weingast, 2009). Finally, Stasavage (2005), focusing on the African context, develops a model explaining why democratically elected leaders will invest more in primary education. Populous rural groups in African countries have clear preferences for spending limited public resources on primary education rather than policies benefiting urban elites (such as expensive university systems), and these rural groups face far lower costs of organizing collective action under democracy than autocracy (see also Bates, 1981).

Concerning empirical analysis, Lake and Baum (2001) reports a strong positive cross-country correlation between democracy and various proxies of human capital, including measures of persistence of students to the fourth grade, pupil-teacher ratio, various school enrollment ratios, and even literacy rates (which is admittedly a crude proxy of education quality). But also several other studies – including case studies, small-n comparative studies and statistical studies employing time series variation – suggest that democracy expands access to and improves funding of both primary and secondary schooling (Lindert, 2005; Stasavage, 2005; Engerman, Mariscal and Sokoloff, 2009; Huber and Stephens, 2012). For instance, the historical expansion of access to lower-level education in the US, UK and Scandinavian countries seemingly followed from the expansion of political rights, and suffrage extensions in particular, during the 19th and early 20th century (see Lindert, 2005). Studying post-colonial Africa, Stasavage (2005) shows how democracy is strongly correlated with spending on primary education in particular, and Harding and Stasavage (2014) detail how elections lead to subsequent abolition of school fees. Interestingly, Harding and Stasavage (2014) also report evidence from surveys of Kenyan voters suggesting that they condition their voting behavior on school fees being abolished, providing more direct evidence for the model.

\(^2\) Also other arguments suggest that democratic politics could increase education spending. For example, Jennings (2015) argues that even voters without material incentives to further increase education spending could vote “expressively” for it (knowing she is unlikely to affect policy individually) due to ethical concerns.
evidence for the type of theoretical argument reviewed above.

One concern is that the above findings reflect a causal effect running from education to democracy. Indeed, such an effect is among the central propositions of “modernization theory” (e.g., Lipset, 1959), and the thorough study by Murtin and Wacziarg (2014) suggests that the correlation mainly reflects a “reverse effect”. Nonetheless, other stringent studies (e.g., Acemoglu et al., 2005) do not find that education systematically enhances democracy, casting doubt on the reverse relationship, and yet other identify a causal effect of democracy on measures of education quantity even when accounting for the potential endogeneity of democracy (e.g., Gallego, 2010; Acemoglu et al., 2013). Hence, while contested, a substantial effect of democracy on, for instance, lower-level school enrollment or years of schooling seems more plausible than not.

This proposed causal effect is suggested to have wider consequences, as schooling is anticipated to affect a range of social, political and economic phenomena. One important such factor is economic growth, as human capital is typically considered among the key ingredients to generating at least medium-term (e.g., Mankiw, Romer and Weil, 1992) and possibly long-term (e.g., Lucas, 1988) growth. Indeed, different statistical studies suggest that education is among the key mediators through which democracy enhances economic growth (Baum and Lake, 2003; Tavares and Wacziarg, 2001; Doucouliagos and Ulubasoglu, 2008), drawing on measures such as secondary school enrollment rates.

Yet, economic growth theory is not merely concerned with amount of schooling offered to students, but rather the skills and capabilities that (prospective) workers acquire, in school or otherwise. Years of schooling or education spending are, at best, only distant proxies of human capital, which is the theoretical concept of interest. Acknowledging this, the wider economic growth literature has recently turned its focus towards more valid proxies of human capital. More specifically, it has started taking education content into account, revealing – as theoretically expected – that education quality is a much stronger determinant of growth than indicators of education quantity. For instance, Hanushek and Woessmann (2008a) finds that cognitive skills are positively related to economic growth, and that any positive effect of education quantity on growth disappears when controlling for skills (see also Hanushek and Kimko, 2000; Hanushek and Woessmann, 2008b). Moreover, Jamison, Jamison and Hanushek (2007) and Hanushek and Woessmann (2012a) find that education quality is positively related to economic growth when using instrumental variables.
regression analysis to account for endogeneity problems.

Such studies have become possible to conduct due to a recent data set, which we employ below, comparing education quality across numerous countries using international student performance tests in mathematics, reading, and science. Drawing on this new data source, it has also been documented that the human capital stock in developing countries has been exaggerated when using indicators of average years of education. For example, previous studies of Latin America have noted that the relatively low economic growth rates in the region can not be explained by the high education levels, but when accounting for the poor quality of Latin American schools this “growth puzzle” is solved (Hanushek and Woessmann, 2012b). Meanwhile, the differences in long-run growth performance across OECD countries can be explained by variations in education quality, as opposed to education quantity (Hanushek and Woessmann, 2011). Hence, if we are to properly evaluate the potential indirect effect of democracy on growth via human capital, there is a need for studying how regime type impacts on education quality. The same is true for other potential indirect effects of democracy on substantively interesting outcome variables such as civil war and social trust (see, e.g., Thyne, 2006).

3 Why democracy may not enhance education quality

Thus, democracy likely improves access to and funding of education. However, we can not automatically expect democracies to provide better schooling than autocracies. In brief, there are two parts to the argument for why democracy does not clearly relate to higher education quality. First, democratically elected politicians (and others) may simply be incapable of increasing education quality even if they desire to do so. There is fairly strong evidence that the quality of education systems is (highly) path dependent, and does not respond clearly, and especially not immediately, to pieces of legislation or to increased education spending. Second, democratic politicians may not even have the incentives to try to pursue legislation or other measures anticipated to increase education quality, since the measures anticipated to increase quality are not very transparent to voters (making it hard for politicians to verify improvements in education quality and take credit for them). We elaborate below.

First, improvements in education quality are inherently difficult to achieve. Even some of
today’s rich, advanced democracies, such as Norway and the United States (where politicians at
the very least pay lip-service to raising education quality), struggle to improve it. Even when
democratically elected politicians are genuinely motivated to bolster the quality of schooling, the
intended outcome is not ensured; indicatively, different studies find no straightforward relationship
between education spending and educational outputs (e.g., Hanushek and Kimko, 2000). One
underlying reason may simply be that it is hard to determine what exact policies are effective
at enhancing student learning. Pedagogy is an academic field that is still alive and kicking. It
is, for example, still unclear which types of (and even whether) homework boosts learning (see,
e.g., Cooper, Civey Robinson and A. Patall, 2006), what manipulable factors determine teacher
quality (see, e.g., Harris and Sass, 2011), and whether number of students in the classroom affects
learning (see, e.g., Hoxby, 2000).

Such uncertainty not only makes it difficult to identify exactly what a quality-enhancing ed-
ucation reform looks like in practice, but it also provides fodder for those opposed to (resource-
demanding) education reform. More generally, democratically elected politicians often face well-
organized interest groups that make legislating and effectively implementing education reforms
difficult. One interest group that has received much attention in the literature is organized teacher
unions, whose members often bear different costs associated with reform (see, e.g., Loveless,
2000). Even if democratically elected governments were blessed with knowledge about the poli-
cies that boost education quality, and initially want to invest in and prioritize them, these policies
may be difficult to implement. In sum, the uncertainty concerning what quality-enhancing re-
forms actually look like, and the difficulty of implementing education reforms, may help explain
why previous studies have shown that a society’s overall level of skills and knowledge is strongly
path-dependent (see, e.g., Crayen and Baten, 2010; Baten and Juif, 2014). Enhancing education
quality is difficult to achieve by the stroke of a pen, even if the pen is held by an education minister,
prime minister or president.

Furthermore, the impact of reform on learning may also take time to materialize. While the
time lag will likely differ depending both on the type of reform and context, Fullan, for instance,
proposes that “it takes about three years to achieve successful change in student performance in
an elementary school. Depending on size, it takes about six years to do so in a secondary school”
(Fullan, 2000, 1). Below, we experiment with different lag lengths when estimating the effects of
education quality, and our baselines employ 5-year lags on the independent variables.
Second, democratic incumbents (and opposition leaders) may not even have strong incentives to try to pursue policies that enhance education quality (see also Harding and Stasavage, 2014). Whereas a democratic system creates strong incentives for the office-seeking politician to expand education enrollment in order to gather popular support, it does not necessarily incentivize him/her to pursue quality-improving programs. Expanding access to schooling (or abolishing school fees or increasing the education budget) is fairly visible to voters: Experiencing that your child or grandchild finally gets to go to school will likely make a clear impression. In contrast, few citizens have exhaustive knowledge about the quality of an education system. Investments in measures that improve student achievements (e.g., developing new methods of class-room teaching) are often less transparent and less verifiable to voters (Harding and Stasavage, 2014), and are thus less easy to promote and explain at campaign rallies or televised debates.\(^4\) The opaque relationship between concrete policy measures and improvements in education quality means that incumbents are less likely to convince retrospective voters, even those concerned with education outcomes, in the next election.

Further, if education quality is not an important source of popular support, democratically elected leaders may even accept a depreciation in the quality of schooling in order to, for instance, raise enrollment rates. This is particularly pertinent for governments presiding over limited financial resources. For instance, addressing the Ugandan abolishment of primary school fees in 1996, Stasavage (2005) argues that it was most likely the introduction of democratic politics that prompted President Museveni to promise and implement this reform. Yet, as Stasavage points out, although the Ugandan education reform secured universal primary education, the massive expansion in education enrollment was followed by a drop in education quality due to subsequent shortage of basic materials and a dramatic increase in pupil-teacher ratios. Studying education policy in current England, Jennings (2015) argues that also other education policies (such as banning private schools) may be supported by many voters on ethical (and sometimes instrumental)

\(^4\)True, the public release of student achievements tests, such as PISA, have received attention in the public debate, especially in some OECD democracies. This could contribute to increasing transparency for attentive voters, for example by providing measuring sticks for cross-country comparisons. Yet, this is mainly a recent phenomena (occurring towards the end of our time-series).
grounds, leading to “expressive voting” for such policies even if they induce sub-optimal education quality outcomes. Hence, there may even be negative effects of democracy on education quality due to the types of education policies that are electorally attractive. For a democratic government, expanding education enrollment, for instance, is a very powerful way of satisfying constituencies, whereas improving education quality is less visible to voters. The latter is thus less likely to be prioritized by elected officials, particularly if budget constraints force a trade-off between quality-enhancing measures and expanding education access (or, e.g., subsidizing education by reducing school fees).

While plausible, and congruent with systematic evidence from Kenya (Harding and Stasavage, 2014), the claim that democracy does not boost education quality has never been systematically evaluated using cross-country data. But, it is in line with other suggestive evidence. For instance, the PISA test score studies reveal very large variations in student achievements – which, as we argue below, is a strong indicator of education quality – among OECD democracies (see Fuchs and Wößmann, 2008). Furthermore, there seems to be large variation also in the extent to which education systems enhance students’ cognitive skills and knowledge across autocratic regimes. In some, the education system focuses on indoctrinating government ideology, and less on basic writing and math skills or knowledge of science. One example is North Korea, where students spend more than one-third of their time on “political education” in the Juche ideology (Martin, 2004, 167). In contrast, other autocracies have historically produced high-quality education systems. Prussia in the 19th century, for instance, provided a high-quality system that was widely emulated by other countries (see, e.g., Lindert, 2005; Clarke, 2006), and the quality of the education in mathematics and the natural sciences in the Soviet Union and other Eastern European Communist regimes was considered high (Boesman, 1993; Balzer, 1993; Chengze, Overland and Spagat, 1999). South Korea and Taiwan are two more recent examples, with students achieving top scores on cross-national skills tests before (Stiglitz, 1997, 884) democratization (but also after, see OECD, 2007). We proceed by investigating the relationship between regime type and education quality more systematically.
4 Data, measures and validity issues

4.1 Measuring education quality

We shall assume that education quality is strongly related to the actual skills and knowledge (i.e., human capital) that students gain from schooling. Indeed, our interest in education quality is, in large part, driven by a concern with the knowledge and skills that students actually gain from their education. The latter is the outcome often hypothesized to, in turn, produce various other beneficial outcomes such as economic growth and political participation.

Thus, we draw on measures of students’ knowledge and skills to operationalize education quality, and more specifically a measure using international student achievement test scores. Although there are several challenges related to measuring education quality (and also skills and knowledge more directly), we submit that our preferred indicator comes close to capturing the outcome of interest, and far closer than more distal proxy indicators such as education spending and the ratio of teachers to students (measuring education inputs rather than education outputs).

While we acknowledge and discuss validity issues and limitations with these measures below, we first describe and highlight the beneficial aspects of these data, for instance in terms of coverage:

Angrist (2013) is the first data set on student achievements tests across a majority of countries globally, covering 128 countries around the world from 1965 to 2010 (in 5-year intervals). This is a clear extension of the cross-sectional coverage of previous data sets (up to about 50 countries), which directly resulted from the limited coverage of any single international achievement test such as PISA. Building on Hanushek and Woessmann (2012a), Angrist (2013) solves this by linking regional tests to international tests using countries that participated in both as reference points. Furthermore, different international assessment tests are linked by using the United States – which has participated in almost all tests since their inception – as a reference point. In this way, regional assessments such as ‘South and Eastern African consortium for Monitoring Educational Quality” (SACMEQ) and the “Latin American UNESCO Laboratorio Latinoamericano para la Evaluacion de la Calidad de la Educacion” are converted to the same metric as international assessments such as PISA and Trends in International Mathematics and Science Study (TIMSS). In addition to an aggregated test score for each country-period, the Angrist dataset contains test scores on three different subjects (math, reading and science) for two different education levels (primary
Figure 1 shows that education quantity, measured as average years of education (for 25-year olds) from the Barro and Lee (2000) data set, is correlated with education quality by 0.60. The latter measure, which we also employ in most of our regressions, is an aggregate measure from the Angrist (2013) data set aggregating primary and secondary level test-score performances in math, reading and science (although we note that data are often only directly available on some of the disaggregated test score measures for a particular country-period). Hence, countries with high education quantity are, on average, more likely to have a high quality level, but many observations also deviate from this pattern. Japan, for instance, is the country that overall gains the highest score on student achievements in this period, but in terms of average years of schooling Japan does not even fall within the top 25 percentile in our sample. Countries such as Jordan and South Africa on the other hand, perform better on quantity than, say, China in 1990 (only 5.6 years of average schooling), but their education quality is dramatically lower than China’s. Furthermore, when accounting for income level – which presumably affects both the quality and the quantity of education provided – the partial correlation between average years of schooling and mean student achievement test score is only .20. The countries that provide their children with more years in school are often not the same that provide their children with an education boosting their cognitive skills and knowledge.

Despite capturing central learning outcomes that should be strongly linked to education quality, and despite the thorough measurement modelling and extensive information used to generate the Angrist measure, there are potential validity issues. First, despite the fact that different test scores in different subjects and at different levels of education are used, there may still be noise in the measure. Unsystematic measurement error in our dependent variable would not produce systematic bias in the regression results, but would increase standard errors. Still, given that the coefficient on democracy flips sign between models, and t-values are very low, it is implausible that this is driving our results (the magnitude of such errors would have to be extremely high).

Second, and potentially more problematic, there could be systematic errors in our education quality measure. One well-known example is that China only conducts PISA tests in Shanghai (plus in Macau and Hong-Kong), and students in this relatively wealthy urban area presumably score better on such tests. China alone does not drive our results – we re-ran our regression omit-
Figure 1: Education quality (“schooling achievements” given by aggregated measure from Angrist (2013)) and quantity (average years of schooling for 25-year olds, from Barro and Lee (2010))

ting China, and results are almost identical. But, if autocracies, in general, systematically only allow areas with expected high-performing students to participate, or otherwise manipulate test scores (more blatantly than democracies do), we could underestimate the relationship between democracy and education quality. However, this bias would need to be extremely large in order to explain the (clear) null-results below. Further, if “manipulation”, such as testing only particular schools or areas, is fairly stable within countries, persisting also after democratization, controlling for country-specific effects should mitigate biases. The country-specific effects should also mitigate biases potentially related to PISA and other tests capturing particular topics and skills (within mathematics, reading and science) that are emphasized more in some countries than in others due, e.g., to cultural or historical differences.

Third, our main measure is based on test results for mathematics, science and reading. If one operates with a fairly broad concept of education quality, and democracies tend to put more resources into, and enhance learning outcomes in, subjects such as social sciences and civics education, we could potentially underreport differences in education quality. We find this to be a plausible hypothesis, which we unfortunately cannot test given the lack of measures with comprehensive coverage. If data become available, future research could test for regime differences
in education outcomes outside the core areas of mathematics, reading and science. With these caveats in mind, we proceed to our empirical analysis.

4.2 Data and measures for democracy and main control variables

The relationships between education (both quantity and quality) and democracy that we report below hold across quite different measures of democracy, for example the minimalist measure from Cheibub, Gandhi and Vreeland (2010) and the maximalist measure from Freedom House. Mainly for pragmatic reasons, we restrict the attention in the next section to models employing the Polity Index from the Polity IV Project (Marshall, n.d.), and report results for other democracy measures in the appendix. Alongside the Freedom House Index, Polity is the most widely used democracy measure in the literature addressing relationships between democracy and education variables. Polity ranges from -10 to 10 (most democratic), and its dimensions are competitiveness and openness of executive recruitment, constraints on the chief executive, and competitiveness and regulation of political participation.

As for the choice of democracy measure, the inclusion or exclusion of particular control variables do not have much influence on our results either (particularly for education quality). Again, we report various specifications in the appendix, and we opt for a parsimonious baseline model controlling only for income level (Ln GDP per capita; mainly from the Maddison project, but we also test models using data provided by Gleditsch (2002)). Income level, which also correlates with other aspects such as class structure and urbanization that may affect education quality, is a particularly important control. Income is strongly correlated with democracy, and richer societies are presumably more likely to have the capacity to build and maintain high-quality education systems. Further, the children of wealthier citizens are – for various reasons such as better access to information technology (Jackson et al., 2006) and increased parental involvement in children’s education (Desimone, 1999) – more likely to perform better in school.

In many of our panel data models, we also control for country- and time-fixed effects since different country-specific factors (e.g., related to the historical characteristics of the education system or particular cultural traits) may affect education quality, and there may be systematic time trends (e.g., education quality might generally increase over time as we learn more about
the measures improving it). Also other factors can potentially affect education and correlate with
democracy. We therefore report models including measures of income inequality (the market
income Gini from Solt, 2009), natural resource income (ln oil and gas income, using data from
Ross, 2012), and country size (ln population, from World Bank, 2011) in addition to income and
the country and year dummies.

5 Empirical analysis

In this section, we start out by replicating the finding that democracy is positively related to mea-
sures of “education quantity”, before moving on to test the relationship between democracy and
education quality. Finally, we present some extensions, investigating how democracy relates to
different types of variability in education performance, and some nuances related to disaggregat-
ing the dependent variable and considering how democracy relates to education performance in
particular subjects, such as math or reading.

5.1 Democracy and measures of “education quantity”

We tested a number of specifications, and a selection of these are reported in Table 1 (several
others are reported in the appendix). In accordance with much of the literature, we consistently
find positive, and mainly statistically significant, relationships. The dependent variable in Table
1 is average years of schooling (for 25-year olds) from the Barro and Lee dataset, but the results
are quite similar for measures such as primary and secondary enrollment ratios (see appendix; the
results are also robust to using different democracy measures).

Model A1, Table 1 is a parsimonious OLS cross section regression (with robust errors), run
on 120 countries. In this model the years of schooling measure (averaged over 2005–2009) is
regressed on the Polity Index and ln GDP p.c. (both averaged over 2000–2004). The coefficient
for Polity is positive (with a t-value of 2.37), suggesting that an increase from the minimum (-10)
to the maximum (+10) is associated with 1.3 more average years of schooling. The result holds
up quite well to adding various plausible controls; Model A2, for instance, adds the gini measure
of market income inequality, ln oil and gas income, and ln population.

Model A3 is run on the entire 5-year period panel dataset, where the first time period is 1965–
69 and the last is 2005–09, to incorporate information stemming from within-country variation over time. The model controls for country-fixed effects on schooling and for income. Indeed, both the Polity coefficient and its t-value increase relative to the cross section models, suggesting that the democracy–schooling relationship is not driven by country-specific omitted variables related to, e.g., culture, geography or political history. The coefficient is weakened, but remains positive and with a p-value of 0.11, when adding time-period dummies, oil and gas income, income inequality and population controls to the fixed effects model (A4). Also models trying to account for democracy being endogenous to years of schooling consistently show a positive, and often statistically significant, estimated effect of democracy. For example, Models A5 and A6 are fixed effects 2SLS models that instrument for democracy by using WAVE from Knutsen (2011). This instrument taps exogenous international-political and regional sources of variation in countries’ democracy levels by recording whether or not the last regime change (as identified by Polity’s duration variable) was within or outside one of Huntington’s “reverse waves of democratization” (Huntington, 1991). The parsimonious Model A5 reports a very large and statistically significant (t=10.6) Polity coefficient, whereas the extensive Model A6 reports a smaller, positive estimate with a p-value of 0.11.

Table 1: Democracy and average years of schooling

<table>
<thead>
<tr>
<th></th>
<th>A1 OLS Cross-sec.</th>
<th>A2 OLS Cross-sec.</th>
<th>A3 OLS (FE) 5-yr panel</th>
<th>A4 OLS (FE) 5-yr panel</th>
<th>A5 FE 2SLS 5-yr panel</th>
<th>A6 FE 2SLS 5-yr panel</th>
</tr>
</thead>
<tbody>
<tr>
<td>Polity Index</td>
<td>0.065* (2.37)</td>
<td>0.076* (2.04)</td>
<td>0.118** (8.52)</td>
<td>0.014 (1.61)</td>
<td>0.240** (10.62)</td>
<td>0.023 (1.59)</td>
</tr>
<tr>
<td>Ln GDP p.c.</td>
<td>1.879** (9.69)</td>
<td>1.856** (6.51)</td>
<td>1.963** (2.72)</td>
<td>0.559** (9.73)</td>
<td>1.681** (4.64)</td>
<td>0.574**</td>
</tr>
<tr>
<td>Ln oil+gas income p.c.</td>
<td>0.092 (1.21)</td>
<td>-0.087* (-2.28)</td>
<td>-0.088** (-3.80)</td>
<td>0.009* (2.00)</td>
<td>0.009* (3.75)</td>
<td></td>
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<tr>
<td>Gini (market income; reversed)</td>
<td>0.044 (1.58)</td>
<td>0.009 (1.42)</td>
<td>0.009* (2.09)</td>
<td>0.009* (2.09)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ln population</td>
<td>-0.244+ (-1.75)</td>
<td>0.699* (2.00)</td>
<td>0.717** (3.75)</td>
<td>0.717** (3.75)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Period dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Country dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: + p < .10; * p < .05; ** p < .01. T-values in parentheses. Dependent variable is average years of schooling for 25 year olds. All independent variables are lagged by 5 years, and standard errors are robust. WAVE from Knutsen (2011) is used as instrument for Polity in the FE 2SLS models (see appendix for first stage regressions). First time period is 1965–69, and final time period is 2005–2009. Constant, country dummies and period dummies are omitted from the table.

In the appendix we report more regressions testing alternative measures of education and democracy, alternative estimators (OLS with PCSE, Random Effects and GMM models), and alternative control sets. We consistently find a positive relationship between democracy and ed-
ucation quantity, and the relationship is most often statistically significant at conventional levels. One potential remaining issue with the results reported in Table 1 is sample selection bias, as many countries (especially fairly autocratic countries) do not report data on measures of education. To deal with this, we employed a multiple imputation model using the Amelia II software from Honaker and King (2010) to predict missing values at the country-year level. The imputation model – the exact specifications and tests evaluating its performance are described in the appendix – was used to construct 5 datasets, and we ran our regression models on these data sets using country-year as unit and employing imputation corrected standard errors. Indeed, the estimated relationships between democracy and measures of education quantity are even clearer when also including the imputed data (see appendix for tables).

In sum, while the relationship is not entirely robust, we replicate the result reported in many previous studies that democracy is positively related to education quantity.

5.2 Main analysis: Democracy and education quality

Whereas democracy is strongly related to measures of how many citizens obtain education, and of how long they stay in school, there is no systematic relationship between democracy and indicators of the quality of education. For starters, the raw correlation between the Polity Index and the measure from Angrist (2013) capturing the average student’s achievement test score (primary- and secondary-level students, for tests in reading, math and science) for the 2005–2009 period is .34, but the partial correlation is only .14 when accounting for differences in income level (ln GDP per capita). The partial correlation is identical (.14) when lagging the Polity Index with 5-years, and even lower when rather employing Freedom House’s Political Rights Index (.04) or Civil Liberties Index (.11) as democracy measures. In contrast, the partial correlation between the student test score measure and ln GDP per capita is close to .7. To further illustrate, Figure 2 shows the deviation from the predicted mean test score – where the prediction is based on an OLS regression with ln GDP p.c. as the independent variable – along the y-axis and the Polity index score along the x-axis for 95 countries (measured in 2005–2009). The figure shows that when taking income level into account, there is basically no relationship between democracy and education quality. Strongly overachieving countries, relative to their income levels, can be found both among autocr-
Figure 2: Democracy and residualized mean student achievement test scores (net of Ln GDP per capita).

Note: The figure plots the residual (i.e., predicted - observed value from a regression with Ln GDP per capita as independent variable) of the mean student achievement test score measure from Angrist (2013) (y-axis), and the Polity Index (x-axis).

This finding is corroborated by a large number of regression models. Whether we consider cross-section regressions or various panel data models incorporating time series information, democracy as exogenous or endogenous, or when using imputed datasets or original data only, there is simply no clear relationship between democracy and education quality. Other specification choices, such as choice of democracy measure or the set of control variables, are not consequential for this finding either. For convenience, the bulk of our specifications are reported in the appendix, and we highlight only a selected few.

Table 2 reports similar models to those in Table 1, but which substitute average years of school-
Table 2: Democracy and mean student achievements test score

<table>
<thead>
<tr>
<th></th>
<th>B1 OLS Cross-sec.</th>
<th>B2 OLS Cross-sec.</th>
<th>B3 OLS (FE) 5-yr panel</th>
<th>B4 OLS (FE) 5-yr panel</th>
<th>B5 FE 2SLS 5-yr panel</th>
<th>B6 FE 2SLS 5-yr panel</th>
</tr>
</thead>
<tbody>
<tr>
<td>Polity Index</td>
<td>0.165</td>
<td>0.146</td>
<td>0.052</td>
<td>-0.029</td>
<td>-0.016</td>
<td>-0.017</td>
</tr>
<tr>
<td></td>
<td>(1.08)</td>
<td>(0.79)</td>
<td>(0.22)</td>
<td>(-0.19)</td>
<td>(-0.67)</td>
<td>(-0.07)</td>
</tr>
<tr>
<td>Ln GDP p.c.</td>
<td>5.765**</td>
<td>6.776**</td>
<td>0.620</td>
<td>1.915</td>
<td>1.360</td>
<td>1.924</td>
</tr>
<tr>
<td></td>
<td>(8.19)</td>
<td>(8.34)</td>
<td>(0.36)</td>
<td>(0.65)</td>
<td>(0.92)</td>
<td>(0.77)</td>
</tr>
<tr>
<td>Ln oil+gas p.c.</td>
<td>-0.420</td>
<td>-0.322</td>
<td>-0.322</td>
<td>-0.67</td>
<td>-0.64</td>
<td>-0.64</td>
</tr>
<tr>
<td></td>
<td>(-1.40)</td>
<td>(-0.67)</td>
<td>(-0.64)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gini (market income; reversed)</td>
<td>0.429**</td>
<td>0.013</td>
<td>0.013</td>
<td>(5.88)</td>
<td>(0.15)</td>
<td>(0.16)</td>
</tr>
<tr>
<td></td>
<td>(1.76)</td>
<td>(1.86)</td>
<td>(2.07)</td>
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</tr>
<tr>
<td>Period dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Country dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>91</td>
<td>84</td>
<td>341</td>
<td>321</td>
<td>317</td>
<td>297</td>
</tr>
</tbody>
</table>

Notes: + p < .10; * p < .05; ** p < .01. T-values in parentheses. Dependent variable is mean student achievement test scores. All independent variables are lagged by 5 years, and standard errors are robust. WAVE from Knutsen (2011) is used as instrument for Polity in the FE 2SLS models (see appendix for first stage regressions). First time period is 1965–69, and final time period is 2005–2009. Constant, country dummies and period dummies are omitted from the table.

ing with the mean student achievement test score measure. The cross section OLS regressions in Models B1 and B2 corroborate the lack of any systematic cross-country correlation between democracy and education quality, with t-values of 1.1 and 0.8, respectively. Given the lack of any relationship when drawing on cross-country comparisons, there is little reason to expect panel models controlling for country-fixed effects or accounting for the endogeneity of democracy to identify a relationship. If certain cultural (see, e.g., Woodberry, 2012) or political-historical (see, e.g., Acemoglu et al., 2005) factors affect both education quality and democracy, they are likely to affect them in the same direction (thus upwards biasing the cross-section results). Likewise, if democracy is endogenous to education quality, modernization theory suggests the effect of education quality should be positive – while we are unaware of plausible arguments suggesting that higher education quality should negatively affect democracy. Still, we run models accounting for country-fixed effects and the endogeneity of democracy to exclude such possibilities. As expected, the fixed effects OLS and 2SLS models (B3–B6) do not identify any systematic relationship either, as the t-values of the Polity coefficients range from -0.7 to 0.2.

However, the absence of any relationship in the four latter models could be due to the relatively few observations and small number of time series units for each panel. The maximum length of the panel is only 9 time periods (each covering 5 years), and the average is 3.2. With so few time series units, fixed effects estimates are both inefficient and downward biased (see Nickell, 1981). But, further investigation suggests that the null-results remain for other (less restrictive) models.
incorporating time series information. For example, Table 3 reports OLS with Panel Corrected Standard Errors (Beck and Katz, 1995) models (C1 and C2) allowing for both cross section and time series variation to inform the results. Even these efficient models – which runs the risk of producing Type II errors given that country-specific omitted factors could systematically enhance both democracy and education quality – do not identify any relationship (t=0.43 for the parsimonious C1 and t=-0.24 for the extensive C2). The null-result is also retained in Random Effects (RE) models (C3 and C4), as well as System GMM models (C5 and C6), which are designed for short panel data structures and for estimating effects of slow-moving variables (see Blundell and Bond, 1998). In contrast with the null findings for democracy, several of the control variables yield substantively large coefficients, and are often statistically significant. For instance, the PCSE and RE models suggest that richer, more egalitarian and more populous countries have better education quality. We also find that natural resource rich countries produce worse education results.

Table 3: Democracy and mean student achievements test score: Alternative estimators

<table>
<thead>
<tr>
<th></th>
<th>C1</th>
<th>C2</th>
<th>C3</th>
<th>C4</th>
<th>C5</th>
<th>C6</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS PCSE</td>
<td>OLS PCSE</td>
<td>RE</td>
<td>RE</td>
<td>System GMM</td>
<td>System GMM</td>
</tr>
<tr>
<td></td>
<td>5-yr panel</td>
<td>5-yr panel</td>
<td>5-yr panel</td>
<td>5-yr panel</td>
<td>5-yr panel</td>
<td>5-yr panel</td>
</tr>
<tr>
<td>Polity Index</td>
<td>0.039</td>
<td>-0.023</td>
<td>0.025</td>
<td>0.008</td>
<td>0.243</td>
<td>0.000</td>
</tr>
<tr>
<td>(0.43)</td>
<td>(-0.24)</td>
<td>(0.16)</td>
<td>(0.07)</td>
<td>(0.71)</td>
<td>(0.00)</td>
<td></td>
</tr>
<tr>
<td>Ln GDP p.c.</td>
<td>6.566**</td>
<td>6.956**</td>
<td>5.440**</td>
<td>6.088**</td>
<td>2.628</td>
<td>5.376</td>
</tr>
<tr>
<td>(15.35)</td>
<td>(14.60)</td>
<td>(8.29)</td>
<td>(9.94)</td>
<td>(1.30)</td>
<td>(1.56)</td>
<td></td>
</tr>
<tr>
<td>Ln oil+gas income p.c.</td>
<td>-0.433**</td>
<td>-0.371*</td>
<td>(-3.06)</td>
<td>(-2.10)</td>
<td>(-1.691**)</td>
<td>(-3.43)</td>
</tr>
<tr>
<td>Gini (market income; reversed)</td>
<td>0.330**</td>
<td>0.261**</td>
<td>0.248</td>
<td>0.248</td>
<td>(0.39)</td>
<td></td>
</tr>
<tr>
<td>(5.87)</td>
<td>(4.40)</td>
<td>(1.47)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ln population</td>
<td>1.320**</td>
<td>1.072*</td>
<td>0.793</td>
<td>0.793</td>
<td>0.793</td>
<td>0.793</td>
</tr>
<tr>
<td>(3.48)</td>
<td>(2.51)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lagged dep. var.</td>
<td>-0.123</td>
<td>0.047</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1.34)</td>
<td>(0.20)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Period dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
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<tr>
<td>N</td>
<td>341</td>
<td>321</td>
<td>341</td>
<td>321</td>
<td>190</td>
<td>185</td>
</tr>
</tbody>
</table>

Notes: + p < .10; * p < .05; ** p < .01. T-values in parentheses. Dependent variable is mean student achievement test scores. All independent variables are lagged by 5 years, and standard errors are robust for random effects and system GMM models, and account for panel-specific AR(1) autocorrelation and panel-level heteroskedasticity in OLS PCSE models. The System GMM models treat the Polity Index as endogenous, and there are no restrictions on the number of lags used for instrumentation. First time period is 1965–69, and final time period is 2005–2009. Constant and period dummies are omitted from the table.

We also probed different specifications to investigate whether our models, for example, fail to pick up a potential non-linear relationship between education quality and democracy, or whether particular types of autocracies systematically fare better or worse than democratic regimes (see appendix for Tables). Regarding the non-linear relationship, we included a squared polity-term to investigate the possibility that “semi-democratic” regimes fare worse in terms of education
quality than both democracies and autocracies. However, only Fixed Effects OLS models identify such a relationship, while the other panel models and the cross section regressions do not show signs of any non-linear relationship between democracy and education quality. We also tested models including the autocratic regime dummies from Hadenius and Teorell (2007), and do find suggestions that autocratic monarchies perform systematically worse than democracies, and some indications that multi-party autocratic regimes perform better than democracies (but only when we omit the Polity variable from the regression). However, neither military regimes or one-party autocracies differ systematically from democracies.

Returning to our core specifications, the fact that we identify fairly clear (and sensible) results for our control variables, indicate that the relatively low number of observations is not an impenetrable barrier to identifying effects on education quality, if one exists. Still, the inefficiencies, as well as potential selection biases, resulting from the few data points might still account for the null-result on democracy. To alleviate this, we tested our models on our five imputed data sets, constructed by using the Amelia II software (Honaker and King, 2010; Honaker, King and Blackwell, 2012). The imputation model is described and evaluated in the appendix. In brief, the imputation model takes into account the cross-section time-series structure of the data (e.g., using second-order polynomial country-specific time trends), and diagnostics and overimputation tests suggest that the imputation model performs quite well (although it predicts the most extreme education quality scores less well). In order to provide as strong a test as possible on the hypothesis that democracy does not enhance education quality, the imputation model was run so that it predicted observations for every country-year.

When employing the imputed datasets, some parsimonious OLS with PCSE models report a statistically significant Polity coefficient (see appendix). However, this coefficient is likely driven by omitted variable bias; when running either Random Effects or Fixed Effects models, Polity once again turns statistically insignificant (in contrast, models run on measures of education quantity show a clear relationship also for the imputed data when controlling for country-specific factors). The lack of a positive and significant coefficient is retained, for instance, when employing alternate democracy indices, lag structures, and sets of control variables, and when first-differencing the dependent variable in order to investigate how democracy affects changes in education quality (see appendix).
Table 4: Democracy and mean student achievements test score: Imputed data

<table>
<thead>
<tr>
<th></th>
<th>D1 RE 1-yr panel</th>
<th>D2 OLS (FE) 1-yr panel</th>
<th>D3 RE 1-yr panel</th>
<th>D4 OLS (FE) 1-yr panel</th>
<th>D5 RE 1-yr panel</th>
<th>D6 OLS (FE) 1-yr panel</th>
</tr>
</thead>
<tbody>
<tr>
<td>Polity index</td>
<td>0.094</td>
<td>0.024</td>
<td>0.083</td>
<td>-0.037</td>
<td>0.073</td>
<td>-0.044</td>
</tr>
<tr>
<td></td>
<td>(1.38)</td>
<td>(0.34)</td>
<td>(1.29)</td>
<td>(-0.57)</td>
<td>(1.10)</td>
<td>(-0.62)</td>
</tr>
<tr>
<td>Ln GDP per capita</td>
<td>5.074**</td>
<td>3.414**</td>
<td>4.701**</td>
<td>1.478</td>
<td>2.824**</td>
<td>0.786</td>
</tr>
<tr>
<td></td>
<td>(5.12)</td>
<td>(3.51)</td>
<td>(3.92)</td>
<td>(1.33)</td>
<td>(2.56)</td>
<td>(0.76)</td>
</tr>
<tr>
<td>Primary school enrollment ratio</td>
<td>-0.014</td>
<td>-0.019</td>
<td>(0.65)</td>
<td>(0.95)</td>
<td>0.078**</td>
<td>0.025+</td>
</tr>
<tr>
<td>Secondary school enrollment ratio</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Year dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Country dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>N (per imputed dataset)</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
</tr>
<tr>
<td>Countries</td>
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<td>117</td>
<td>117</td>
<td>117</td>
</tr>
</tbody>
</table>

Notes: + p < .10; * p < .05; ** p < .01. T-values in parentheses. Dependent variable is mean student achievement test scores. Country-year is unit of analysis, and all independent variables are lagged by 1 year. The coefficients are averaged over 5 imputed data sets, and standard errors are robust and imputation-corrected. The time series run from 1970–2011 (balanced panel). The log-transformed GDP p.c. variable is constructed as ln(GDP p.c.+18000), due to the multiple imputation being conducted without bounds and the resulting occurrence of negative GDP p.c. values in the imputed data sets (see appendix for details on the imputation model). Constant, country dummies and period dummies are omitted from the table.

Table 4 reports six of the many Random Effects (RE) and Fixed Effects (FE) models that we tested on imputed data. These are parsimonious models with a 1-year lag on the independent variables. The coefficients reported are averaged over the 5 imputed datasets, and the standard errors are imputation corrected. Models D1 (RE) and D2 (FE) only include income level as a control variable, whereas D3 (RE) and D4 (FE) also include year dummies to account for time-specific effects. In neither of these models are the Polity coefficients statistically significant at conventional levels, with t-values varying between -0.6 and 1.4.

However, we highlighted in our theoretical discussion that the relationship between democracy and education quality might be attenuated by democracies expanding access of education to more citizens. This may generate capacity issues (fewer teachers, textbooks, and square meters of class room per pupil, since the denominator increases), at least in the short term. On the other hand, democracy reportedly increases education spending, potentially balancing out this capacity effect. Still, our results on nation-wide averages of student achievement test scores may also be attenuated for a more subtle reason that should be considered more of a measurement issue: It may be that the background characteristics of the children that are typically given access to education under democracy (but not autocracy; for example the kids of poor, rural voters. See, e.g., Stasavage, 2005) make them less likely to perform well in school and on student test scores, ceteris paribus, than the children that typically have access both under autocracy and democracy (e.g., the children of richer, urban elites). Thus, the lack of any positive relationship between democracy
and aggregated education quality might simply reflect expanded education access driving down the country’s average test scores. Possibly the children who would hypothetically also be in school under autocracy actually perform better under democracy, implying that democracy raises education quality after all.

This conjecture is admittedly difficult to test absent more nuanced measures or individual-level data (although our results in the next subsection, conducted on PISA 2012 results, suggest that democracy is not associated with higher variability in test scores within student populations, further alleviating concerns that this alternative mechanism is driving our results). Still, one implication is that the relationship between democracy and education quality should become stronger positive once controlling for measures of education access. This turns out not to be the case. We tested a number of models controlling for primary and secondary enrollment ratios, and there is no evidence that the Polity coefficient increases (or is statistically significant, for that matter). Models D5 (RE) and D6 (FE) represent two such models, where both primary and secondary enrollment ratios are added to D3 and D4, respectively. In both models, the Polity coefficients barely move – actually, the coefficient size decreases slightly in both instances – suggesting that the null-relationship between democracy and the nation-wide average on student test scores does not come from democracy expanding education access to more kids. These results hold up also when, for instance, adding enrollment ratios as controls in models not including imputed data. In sum, our analysis leaves little ground for optimism on the part of democracy in improving education quality.

5.3 Extensions and some nuances

The results presented above all rely on the aggregate indicator of student achievements tests that are based on (up to) six subcomponent scores, namely math, reading, and science test scores at the primary level and at the secondary level. Although we find no effect of democracy on this aggregate measure, democracies could still be systematically better at promoting certain types of education. For example, democracies may be better at promoting reading skills, but not science and math skills. Many East Asian countries and the former Communist dictatorships in Eastern Europe have placed an emphasis on education in technical subjects, engineering, or natural
sciences as opposed to social sciences and humanities (see, e.g., Hayhoe, 1995). One could hypothesize that a dictator looking to increase state revenues may find boosting natural sciences skills as one attractive option (since it increases the tax base by enhancing the productivity of the labor force), which at the same time is less likely to serve oppositional forces wishing to build up protest movements or campaigns (see Stasavage, 2005).

Table 5: Democracy and subcomponents of student test scores. Random effects models.

<table>
<thead>
<tr>
<th></th>
<th>E1 Maths primary</th>
<th>E2 Reading primary</th>
<th>E3 Science primary</th>
<th>E4 Maths secondary</th>
<th>E5 Reading secondary</th>
<th>E6 Science secondary</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
</tr>
<tr>
<td>Polity index</td>
<td>0.0531 (1.04)</td>
<td>0.118** (2.73)</td>
<td>0.173*** (3.84)</td>
<td>0.0585 (0.94)</td>
<td>0.132** (3.24)</td>
<td>0.0480 (1.05)</td>
</tr>
<tr>
<td>Ln GDP per capita</td>
<td>3.900* (2.42)</td>
<td>5.175*** (4.10)</td>
<td>4.268*** (4.01)</td>
<td>4.958*** (3.75)</td>
<td>4.898*** (4.27)</td>
<td>4.300** (3.11)</td>
</tr>
<tr>
<td>Year dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>N (per imputed dataset)</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
</tr>
</tbody>
</table>

Notes: * p < .10; ** p < .05; *** p < .01. T-values in parentheses. Dependent variable is mean student achievement test scores. Country-year is unit of analysis, and all independent variables are lagged by 1 year. The coefficients are averaged over 5 imputed data sets, and standard errors are robust and imputation-corrected. The time series run from 1970–2011 (balanced panel). The log-transformed GDP p.c. variable is constructed as ln(GDP p.c.+18000), due to the multiple imputation being conducted without bounds and the resulting occurrence of negative GDP p.c. values in the imputed data sets (see appendix for details on the imputation model). Constant and year dummies are omitted from the table.

Table 6: Democracy and subcomponents of student test scores. OLS (Fixed effects) models.

<table>
<thead>
<tr>
<th></th>
<th>F1 Maths primary</th>
<th>F2 Reading primary</th>
<th>F3 Science primary</th>
<th>F4 Maths secondary</th>
<th>F5 Reading secondary</th>
<th>F6 Science secondary</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
<td>1-yr panel b/t</td>
</tr>
<tr>
<td>Polity index</td>
<td>-0.0563 (-0.97)</td>
<td>0.0220 (0.35)</td>
<td>0.0480 (1.02)</td>
<td>-0.0539 (-0.85)</td>
<td>0.0398 (0.97)</td>
<td>-0.0483 (-1.18)</td>
</tr>
<tr>
<td>Ln GDP per capita</td>
<td>1.290 (1.00)</td>
<td>2.240+ (1.82)</td>
<td>1.082 (1.05)</td>
<td>2.013+ (1.71)</td>
<td>2.356* (2.48)</td>
<td>1.625 (1.23)</td>
</tr>
<tr>
<td>Constant</td>
<td>27.05* (2.02)</td>
<td>13.97 (1.11)</td>
<td>29.71** (2.81)</td>
<td>30.29* (2.47)</td>
<td>22.39* (2.17)</td>
<td>29.47* (2.19)</td>
</tr>
<tr>
<td>Year dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Country dummies</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>N (per imputed dataset)</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
<td>4914</td>
</tr>
</tbody>
</table>

Notes: * p < .10; ** p < .05; *** p < .01. T-values in parentheses. Dependent variable is mean student achievement test scores. Country-year is unit of analysis, and all independent variables are lagged by 1 year. The coefficients are averaged over 5 imputed data sets, and standard errors are robust and imputation-corrected. The time series run from 1970–2011 (balanced panel). The log-transformed GDP p.c. variable is constructed as ln(GDP p.c.+18000), due to the multiple imputation being conducted without bounds and the resulting occurrence of negative GDP p.c. values in the imputed data sets (see appendix for details on the imputation model). Constant, country dummies and period dummies are omitted from the table.

To investigate this we re-ran selected models from Table 4, replacing the mean student test score with the subcomponents available from the (Angrist, 2013) dataset. We note, however, that we put less trust in these tests, as the disaggregated score (for any single component) have far
lower coverage across countries and over time than the composite measure. This also means that we can only rely on the imputed data sets for conducting our tests. The results, which are reported in in Table 5 and 6, are thus only suggestive.

In Table 5 we replicated a Random Effects model – which is more efficient than a corresponding Fixed Effects model, but potentially biased – from Table 4 (Model D5 with year dummies) for the six disaggregated components. They provide no evidence that democracy is positively related to math skills. However, we do find indications that democracy is positively related to reading skills, both at the primary (Model E2) and secondary level (E5); the coefficient estimates for Polity are positive and significant at 1%. Meanwhile, there is no effect of democracy on science achievements at the secondary level (E6), but the coefficient estimate for democracy is positive and significant for science achievements at the primary level (E3). Nonetheless, these results are far from robust. For instance, in Table 6 we replicated Model D4 from Table 4, which is a Fixed Effects model with year dummies, for the various sub-component measures. When including country dummies in these models, democracy is not systematically related to knowledge and skills in any of the three subjects (neither at the primary nor at the secondary level), with the Polity t-values ranging from -1.2 to +1.0. Hence, while there are some indications that democracy is good for stimulating reading skills, the evidence is far from robust and disappears when we account for country-fixed effects.

Another extension of our analysis relates to the variability in education quality in democracies and in autocracies, between countries, within countries over time, and within the student population in a given country. Although there is no difference in “average quality” between democracies and autocracies, it might be that autocracies produce more outcome variation – as they have been shown to do for many other outcomes (such as economic growth; see, e.g., Rodrik, 2008). For example, the higher concentration of political power within autocracies could make them more susceptible to the priorities and preferences of a small ruling elite, some of which may be highly concerned with education for various reasons, and others not. After running cross section and cross-section time series models on education quality, we therefore check for systematic differences in variation by employing the Goldfeld-Quandt tests separating relatively democratic and relatively autocratic regimes (using various operationalizations). We do, indeed, find that autocratic countries display more variability in education quality (see appendix). This holds also when
looking at variation within countries over time. Hence, even if predicted (average) education quality is not clearly related to regime characteristics, more autocratic regimes display more variation around their predicted level.

Finally, one could expect that either autocracies or democracies display more variation in achievement tests also within their populations; that is, regime type could matter for the spread in skills and performance between “good” and “bad” students, even if the mean national levels are similar. However, it is hard to anticipate the direction of any relationship. Autocratic regimes might, for example, favor the students that are associated with their more or less narrow supporting coalitions, suggesting larger spread within student populations in autocracies. Yet, we argued above that democratization expectedly extends enrollment to student groups (such as sons and daughter of poor farmers) that may not have all the pre-requisites for performing well in school, and these students are often not in school (and are therefore not tested) in autocracies. While the dataset that we used above only includes aggregated scores, we are able to conduct preliminary tests on regime type and within-population variation using data from the 2012 PISA test covering 56 countries. While these data mostly cover democracies, 10 countries score lower than 6 on the Polity scale (and 8 score below 0, including quite different countries such as China, Jordan, Kazakhstan and Qatar). The results from regressions on the within-population standard error in test performance – we separately tested this for math, reading and science, both at the primary and at the secondary level – show no signs of any systematic relationship, independent of whether we control for the mean test result in the population or not. Further analysis shows that regime type is not related to absolute gender differences in test scores either. Insofar as these cross-country tests on a modest number of countries are informative, there is no indication that democracy relates to the variability in performance within student populations.

6 Conclusion

In this paper, we have replicated the result that democracy relates positively to features of the education system such as the share of young citizens that attend school and the number of years they stay in school. This is congruent with extant findings from the literature, and with theoretical arguments highlighting how democratically elected politicians have incentives to invest in aspects
of the education systems that are fairly visible to voters. However, we argued that democracy should not readily be expected to enhance the quality of the education that young citizens are exposed to, and thus the skills and knowledge that they obtain. Altering education quality is no easy matter for politicians, and democratic politicians may not even have strong incentives to try to alter them in the first place. Employing a panel data set of international student test achievements, we find no clear evidence of any relationship between democracy and education quality. Future data collection – for other subjects than math, reading and science, as well as in more countries – will allow for even more appropriate testing of this relationship. Yet, our best guess, at current, is that democracy does not enhance the skills and knowledge of students.

Our findings have wider ramifications for venerable literatures on other subjects. It, for instance, bears on the suggested indirect effect of democracy on economic growth via increasing human capital. While several studies have identified a clear and positive indirect effect using measures of what we have termed “education quantity” (Tavares and Wacziarg, 2001; Baum and Lake, 2003; Doucouliagos and Ulubasoglu, 2008), analysis employing more appropriate measures of the actual knowledge and skills of the future work force would not identify any indirect effect (due to the non-existence of the first link in the causal chain). The same goes for proposed indirect effects of democracy via education on other outcomes of interest, since education is theorized to stimulate political participation (Hillygus, 2005) as well as higher trust and fewer incidences of political violence (Thyne, 2006) mostly through enhancing knowledge and skills among citizens, and not simply through kids being in school.
References


