Education, public support for institutions, and the separation of powers

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Abstract

A successful democratic transition requires citizens to embrace a new set of political institutions. Citizens' support is vital for these institutions to uphold the burgeoning constitutional and legal order. Courts, for example, often rely on citizens' support and threat of electoral punishment against the government to enforce their rulings. In this article, I consider whether education under democracy can engender this support. Using regression discontinuity, difference-in-differences, and difference-in-difference-in-differences designs, I find an additional year of schooling after the fall of the Berlin Wall has similar positive downstream effects on East Germans' support across institutions. Since schooling similarly affects public support for judicial, legislative, and executive institutions, citizens are not necessarily inclined to electorally punish the other branches when they ignore a court's ruling. This potential inability of courts to constrain unlawful government behavior threatens the foundation of the separation of powers and the survival of democracy.

We have come to take democracy for granted, and civic education has fallen by the wayside. In our age, when social media can instantly spread rumor and false information on a grand scale, the public's need to understand our government, and the protections it provides, is ever more vital.
–Chief Justice John Roberts, 2019 Year-End Report on the Federal Judiciary

Introduction

How does education affect public support for political institutions? Social scientists argue that education provides citizens the tools to effectively interact with a democratic government (e.g., Dewey, 1916; Lipset, 1959). If socialization into democratic citizenship through education is critical for democratic governance, then, new democracies face a difficult challenge. Research documents that autocracies make widespread investments in mass schooling (e.g., Paglayan, 2021) and may indoctrinate citizens to believe that the existing regime is preferable to other alternatives (e.g., Cantoni et al., 2017). The ability of mass schooling under autocracy to shape political attitudes raises a series of important questions: Can attending school in a democracy counteract these effects and increase support for democratic governance?[1] Are these effects different for some

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[1]I thank anonymous Reviewer 2 for providing valuable feedback in situating my contribution within the larger scholarship on schooling under autocracy and democracy.

democratic institutions relative to others? If so, what are the implications for the separation of powers?

When schooling is designed to cultivate obedience and suppress dissent against the regime, citizens are more likely to have a coercive—as opposed to consensual—relationship with authorities (e.g., Tyler, 2006). Such schooling under an autocratic regime may lead to low support for political institutions after the transition to democracy. Following a democratic transition, however, education reform is natural step for policymakers and may lead to greater acceptance of the new democratic institutions (e.g., Finkel, 2002; Finkel and Smith, 2011). Scholars theorize that childhood socialization in democratic political values is an important determinant of public support for and participation in political institutions (e.g., Easton and Dennis, 1967). Specifically, school environments that encourage open discussion of government policies and political disagreement may cultivate higher trust in democratic institutions (e.g., Torney-Purta, 2002; Campbell, 2008; Holbein, 2017). Experiencing such political disagreement in school provides students with the conceptual foundations for understanding and accepting the institutionalized disagreement inherent in separation of powers politics. Nonetheless, considerable empirical evidence also suggests that education may not positively affect support for or participation in political institutions at all (e.g., Langton and Jennings, 1968; Kam and Palmer, 2008; Croke et al., 2016), imperiling the durability of democracy itself.

The existing scholarship, furthermore, does not provide insights on whether education may have differing effects on public support across judicial, legislative, and executive institutions, despite its relevant implications for separation of powers politics. These effects are particularly consequential for courts in new democracies. Scholars argue that courts, lacking the power of the purse or the sword, rely on the public's support and threat of electoral punishment to compel the executive to implement their decisions (e.g., Vanberg, 2005; Staton, 2010; Krehbiel and Cheruvu, 2022). While education may increase public support for courts, if it results in similar increases in public support for the executive (legislature), citizens are not inclined to punish the executive (legislature) when it disobeys a court. Providing evidence for the relationship between education and public support across political institutions would further build upon the burgeoning empirical literature on the extent and efficacy of judicial power (e.g., Bartels and Kramon, 2020; Bartels et al., 2021; Carlin et al., 2022).

To evaluate whether education under democracy affects public support for political institutions, I leverage the fall of the Berlin Wall as an external shock to the educational environment of the former German Democratic Republic (GDR, East Germany). Taking advantage of a birth date cutoff for childhood school enrollment that exogenously assigns whether a child experiences an additional year of schooling under democracy, I find similar positive downstream effects on public trust for the German Federal Constitutional Court (FCC), Bundestag, and federal government when using regression discontinuity, difference-in-differences, and difference-in-difference-in-differences designs. Additionally, examining these data across birth years and survey years, I provide evidence that education's effect becomes stronger over time and is correlated with similar increases in individual evaluations of their own financial situation. These findings provide evidence that education may increase public support for political institutions in new democracies, but does not suggest that citizens would be more likely to uphold the separation of powers by supporting a court when it rules against the legislature or executive.

This paper is organized as follows. First, I explain why public support is necessary for the efficacy of the separation of powers and how childhood socialization through schooling may affect public support. Second, I describe schooling under the East German regime and schooling following the fall of the Berlin Wall. Third, I empirically test my theory and provide causal evidence. Finally, I conclude by discussing my findings' implications for the separation of powers in democracies, and for the relationship between education and political attitudes.

Education, public support, and the separation of powers

The survival of modern liberal democracy critically depends on whether courts can meaningfully protect citizens by overruling government actions that violate the legal order. Despite this important function, Gibson et al. (1998, 343) describe the fundamental tension of courts in separation of powers politics as the following: "with limited institutional resources, courts are therefore uncommonly dependent upon the goodwill of their constituents for both support and compliance. Indeed, since judges often make decisions contrary to the preferences of political majorities, courts, more than other political institutions, require a deep reservoir of goodwill." With the inability to directly enforce their decisions, courts require tools to incentivize political actors to comply with their rulings. Public support is one such tool. When citizens support their courts, the threat of electoral punishment may compel political actors to comply with courts' rulings (e.g., Vanberg, 2015). Courts' ability to induce compliance with their decision-making can be broadly understood as *judicial power* (e.g., Staton, 2010) and a recent scholarship has endeavored to explore the extent of public support for such power (e.g., Bartels and Kramon, 2020; Bartels et al., 2021; Carlin et al., 2022).

A theoretically-motivated source of public support for courts is socialization in democratic values. While scholars debate whether public support for courts is durable over time (e.g., Gibson and Nelson, 2014; Christenson and Glick, 2015; Bartels and Johnston, 2020), they agree that citizens' democratic values and knowledge are important determinants of their public support. Scholarship in legal socialization, defined by Trinkner and Tyler (2016, 417) as "the process whereby people develop their relationship with the law via the acquisition of law-related values, attitudes, and reasoning capacities," focuses on the role of school environment. Importantly, school environment may affect whether citizens have a consensual orientation toward the law or a coercive orientation toward the law.[2] Citizens with a consensual orientation toward the law obey legal authorities because they feel a duty to do so, not because the authorities are coercing them (e.g., Tyler, 2006).

An earlier scholarship discusses the relationship between education and support for courts with regards to the US Supreme Court. Easton and Dennis (1969) argue that through education children have a "youthful idealization" of the Supreme Court and believe that it is the branch of government least likely to make mistakes. Caldeira (1977) finds that school children that display knowledge of the Supreme Court did not express any negative affect toward it. More broadly, this scholarship provides evidence that children that are knowledgeable about the Supreme Court are more likely to support it (e.g., Murphy and Tanenhaus, 1968; Casey, 1974; Tanenhaus and Murphy, 1981). Contemporary scholarship similarly argues that exposure to judicial symbols increases citizens' support for courts (e.g., Gibson and Caldeira, 2009), and that education is a means through which citizens learn the meaning of these judicial symbols (e.g., Gibson and Nelson, 2018).

This scholarship, however, has not rigorously evaluated whether this relationship between education and public support for courts exists at all, and if it does exist, this relationship's magnitude is relative to legislatures and executives. Education's (lack of) impact on support and engagement across political institutions has implications for the separation of powers. If democratic education causes greater increases in public support for a court relative to the other institutions of government, the public may empower the court to meaningfully constrain the executive (legislature). However, if democratic education has similar (or greater) effects on public support for legislatures and executives relative to courts—including having no effect at all across the institutions—courts may not have the ability to check executive power and compel compliance. Ura and Wohlfarth (2010, 942) provide empirical evidence for this phenomena in the American context and argue, "Congress's allocation of resources and discretion to the Supreme Court should be a function of both public confidence in the Court and public confidence in Congress, rather than the

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[2]See Tyler and Trinkner (2017) for a thorough overview of this point.

level of public support for the Court alone." Therefore, understanding education's (lack of) effect on public support for institutions relative to one another is more informative to the interactions among the judiciary, legislature, and executive within the separation of powers.

Education may (not) increase public support for courts in the absolute sense, but evidence suggests any effect would be the same for other institutions as well. Scholars argue that education leads to greater political knowledge (e.g., Campbell and Niemi, 2016), participation (e.g., Hillygus 2005; Mayer 2015), and trust (e.g., Hooghe et al., 2015). Importantly, these effects tend to be stronger when citizens are exposed to democratic education while they are school children (e.g., Torney-Purta, 2002). Alternatively, education may not have much of an effect on public support for institutions at all and, in fact, may have negative effects. Kam and Palmer (2008) argue that education simply proxies for other background variables, such as parenting styles (e.g., Jennings and Niemi, 1968) or the presence of siblings (e.g., Healy and Malhotra, 2013), that may affect citizens' dispositions toward their institutions and also affect access to education. Croke et al. (2016) provide evidence that education may in fact decrease political participation and involvement in new democracies, as citizens are less likely to have trust in institutions that they may perceive as empowering the incumbents. Additionally, Marshall (2016) argues that education increases support for right-wing political parties. If such a political party were to support court-curbing measures (e.g., Clark, 2011; Kelemen, 2012), its unclear whether citizens that have high support for their courts would support upholding their courts' institutional integrity over achieving partisan goals (e.g., Ginsburg and Huq, 2018; Nelson and Gibson, 2019; Bartels and Johnston, 2020; Svolik, 2020).

This relationship between education and public support for institutions may be especially consequential following a democratic transition. Children that were socialized through education into one regime are charged with evaluating the institutions of another as adults. In many authoritarian contexts, the school environment is tightly controlled and students are discouraged from questioning the regime. This control over the school environment, often with harsh sanctions placed on those questioning authority, creates a coercive orientation toward the law among students. Therefore, students defer to school authorities because they face consequences for defying authorities.[3] Scholars provide evidence that these citizens, having experienced such a relationship with school authorities, tend to have lower trust in, and are less likely to participate in, democratic institutions (e.g., Kupchik and Catlaw, 2015). Additionally, authoritarians often design education to indoctrinate citizens with the regime's ideology and homogenize the preferences of citizens with those of the elites (e.g., Cantoni et al., 2017). This ideological indoctrination during schooling may have persistent effects on citizens' attitudes toward institutions and policies even after a regime's collapse (e.g., Voigtländer and Voth, 2015). Since education under authoritarianism is predicated on legitimizing executive control of all aspects of society, the conceptual foundation of the separation of powers that the executive, legislature, and judiciary are coequal in the governing process is likely absent in schooling.[4]

Following a democratic transition, however, the school environment may not be as keen to suppress dissent. Students in school environments in which they can express dissent and can discuss their disagreements may be more likely to develop a consensual orientation toward the law,

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[3]To be clear, similar school environments exist in democracies as well. In fact, the majority school environment scholarship focuses on western democracies. For the purposes of this article, I assume that on average school environments in autocracies are more likely to create a coercive orientation with the law than school environments in democracies.

[4]Indeed, Hamilton, preempting the skepticism of his readers who had recently liberated themselves from the authoritarian governance of Great Britain, acknowledges in Federalist 78 that the idea of coequal branches of government may be confusing to those who are unfamiliar to the concept. Hamilton states, "Some perplexity respecting the rights of the courts to pronounce legislative acts void, because contrary to the Constitution, has arisen from an imagination that the doctrine would imply a superiority of the judiciary to the legislative power." Hamilton goes on to explain that the ability of the courts to pronounce a legislative act void does not suppose "superiority of the judicial to the legislative power" and tries to persuade his readership about the necessity of judicial institutions.

potentially resulting in support for democratic institutions. When students are explicitly allowed to deliberate about government policies, studies find that students are likely to be more knowledgeable about, and have higher support for, their institutions. For example, the open classroom environment—meaning teachers emphasize discussion among students on political issues—is often linked to students having greater civic knowledge relative to teaching about civics without discussion (e.g., Kahne et al., 2013; Persson, 2015). Campbell (2008) finds that discussing contentious political issues positively impacts students' appreciation and acceptance of institutionalized political conflict. Furthermore, these favorable attitudes toward institutions may be cultivated when citizens are allowed to voice concerns and issues with school authorities. When students are allowed to voice their concerns, they may be more likely to perceive the school environment as fair (Gottfredson et al., 2005). Scholars find that students' perceptions of fairness in schools lead to more positive attitudes toward political institutions outside of the school context (e.g., Gouveia-Pereira et al., 2003; Resh and Sabbagh, 2014a,b). Nonetheless, it is unclear if education following a democratic transition will have any meaningful effects, let alone differential effects across political institutions. This theorizing leads me to the following hypothesis:

Hypothesis 1 Attending school under democracy will have similar effects on citizens' support for executive, legislative, and judicial institutions

The scholarship on education's affect on support for and engagement with political institutions also explores temporal variation. For example, citizens with more education under democracy may be more likely to have better long-term economic outcomes. These economic outcomes may, in turn, lead to greater access to social networks and subsequently affect attitudes toward political institutions. Fuchs-Schündeln and Masella (2016), for example, provide evidence that East Germans that received an extra year of education under democracy are more likely to be employed and are more likely to have higher wages. Additionally, Marshall (2016) provides evidence that an additional year of educational attainment led to higher income among British citizens and increased the likelihood that they voted for the conservative party. Similarly, if support for political institutions—or lack thereof (e.g., Croke et al., 2016)—is a function of instrumental benefits, citizens that are economically better off may be more likely to support their institutions. Furthermore, such instrumental benefits most likely do not accrue to citizens until they are fully integrated into the workforce and have economic gains from their democratic education. Building upon hypothesis 1, the scholarship does not provide a compelling reason to expect this relationship to affect public support across institutions differently. This theorizing leads me to the following hypothesis

Hypothesis 2 The effect of attending school under democracy on a citizen's support for executive, legislative, and judicial institutions will become stronger over time

Application: regime change in East Germany
Education in East Germany
East German education was characterized by a classroom environment that rigidly indoctrinated students with regime ideology, encouraged student engagement with the regime insofar as it was aligned with regime activity, and disciplined students for dissent. The foundations of this education originated as a geopolitical consequence of allied bargaining at the end of World War II in 1945, resulting in the division between East and West Germany. In 1949, the Federal Republic of Germany (FRG), or West Germany, and the GDR officially became separate states. As the allies set the groundwork for the denazification and democratization of the FRG, the GDR, under the strong influence of the Soviet Union, quickly centralized power under the Socialist Unity Party of Germany (SED) and commenced the creation of a communist state.

The denazification and sovietization process in the GDR included reformulating the education system to align with communist values and purging teachers who refused to comply. By 1949, the US High Commissioner for Germany estimated that over 80 percent of school staff in East Germany were new teachers (Fulbrook, 2015, 125). The 1959 Law Relating to the Socialist Development of Education in the GDR organized primary and secondary education as follows: students would spend their first ten years of education in *Zehnkassige allemeine polytechnische Oberschule* (ten-year general polytechnical schools, POS) and the following two years in either an *Erweiterte Oberschule* (extended upper school, EOS) for the academically gifted or a vocational school organized in units of socialist production. Selection into an EOS was based on academic achievement and a student's political attitudes as determined by their teachers (Weiler et al., 1996).

The GDR school environment was one designed for indoctrination as opposed to open discussion of ideas.[5] Fulbrook (2015, 194) explains, "Pupils were taught to repeat approved positions rather than develop independent points of view [...] East German youth learned to become at least outward conformists and gained little experience of genuine debate and the toleration of alternative points of view." Although the regime designed the school environment to create obedient subjects, this obedience was not necessarily passive. Teachers were to encourage students to actively participate in state youth organizations and were to rouse students' active engagement with the state insofar as they were ideologically aligned with the regime. These activities of the students were often key factors in teachers' comprehensive evaluations of students' personalities.

Teacher evaluations were instrumental in determining a student's future career prospects (Weiler et al., 1996). Thus, students were strongly incentivized to maintain political attitudes in line with the regime. Fulbrook (2015, 185) explains, "In the East political conformity was a prerequisite for career advancement and upward social mobility; or, put differently, political nonconformity would actively block chances of advancement, while political conformity was a necessary but not sufficient prerequisite for promotion prospects." The link between political attitudes and social mobility discouraged students to openly express their dissent and encouraged students to conform to the "socialist personality" the GDR government was trying to create in each student. Importantly, this rigid adherence to state doctrine permeated vocational education as well. While those in vocational schools had less direct teaching time dedicated to socialism following their graduation from POS than their EOS counterparts, they were often unable to choose their desired apprenticeship training. Available job training was completely controlled by central planning with local authority councils that had offices dedicated to monitoring local needs. Pritchard (1999, 128) explains that from the sixth grade on "Pupils job aspirations were systematically collected and transmitted to the advisory centers so that they could be matched up with actual needs [...] State planning resulted in a lack of freedom for individuals [...] Many apprentices were denied their top career preference."

Even if students, or their parents, wanted to express their discontent with the methods of teaching or the curriculum more generally, they were not provided institutional avenues to do so and were actively punished for questioning authority. Weiler et al. (1996, 40) explain, "unless parents were in high places they rarely were able to overrule the school's decision [...] If a student did not comply with the rules or acted up in class, a 'well-oiled machine' [...] was set in motion that backed a teacher's authority." This "well-oiled machine" included calling upon other parents put in charge of the student's "class collective" to discipline the student. The socialist school had primacy above both parents and students in deciding what was best for the student's educational progress. Although parents were heavily involved in the school system, the purpose of the involvement was to draw a strong connection between home and school and draw parents into

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[5]As Pritchard (1999, 129) describes, "Personal development was subordinated to the postulate of 'societal usefulness' and the 'activity principle' in education was subordinated to a rigid political line leaving little scope for innovation or for a genuinely learner-centered curriculum."

supporting the educational process at school (Rust and Rust, 1995). Students in East Germany, therefore, in a school environment dominated by government control and without the means to challenge the state, were not well-equipped with the tools necessary to engage with democratic institutions and were more likely to develop a coercive-orientation toward the law.

Education after the fall of the Berlin Wall

To the shock of many in Germany and the international community, the Berlin Wall fell on 9 November 1989. In the immediate aftermath, education in East Germany changed radically. Most importantly, teachers in East Germany gained autonomy in the classroom in the midst of the rapid political change. Likewise, "many teachers broke away from the old party-line peda-gogy and began to teach in an experimental manner as they sought new methods and entered into open discussions about pedagogical themes" (Rust and Rust, 1995, 145). The virtually overnight removal of central control of the educational system and mode of teaching served to naturally create a more open school environment. The systems by which students were disciplined if they questioned or dissented with school curricula were eliminated.

Given teachers were experimenting with the curriculum and students were no longer subject to the rigid rules that previously characterized education in the GDR, the authority relationship between teachers and students changed. In interviews conducted with teachers in East Germany, Weiler et al. (1996) found that teachers described their changing relationship with students and parents as the most profound change after the fall of the Berlin Wall. In particular, they observed, "teachers find dialogue with their students difficult because the latter are said to be either interested in being merely disruptive or argue their point [...] Students, it seems, have become incalculable, giving in to the new stimuli of fashion, media, western youth culture, and right-wing rebellion" (Weiler et al., 1996, 41).

As a result of the deteriorating power of the GDR regime around them, East German students felt empowered to question authority and actively express dissent in the classroom. The changing authority relationship between teachers and students can be understood as a sort of democratiz-ing process by which teachers, by virtue of institutional uncertainty, had no ability to suppress and control student dissent. Weiler et al. (1996, 57), writing during the early years of the transi-tion, describe, "schools in Eastern Germany have now become more like modern democratic institutions. Mobility, individuality, openness, and voice of constituencies have increased, but so have uncertainty and strife." Therefore, East German students who were in school when the Berlin Wall fell in the 1989–1990 school year or later experienced a more open school environment relative to those who had already completed school.

Data and empirical methods

To estimate the causal effects of schooling under democracy on support for political institutions, I need data on citizens' support for German political institutions and an empirical strategy to compare students with differential exposure to schooling in East Germany before and after the fall of the Berlin Wall. I use data from the German General Social Survey (ALLBUS)—a biennial survey on the attitudes of residents of Germany—for a measure of support for the German Federal Constitutional Court (FCC), the Bundestag, and the federal government. Since Germany has a fused executive, the distinction between court, legislature, and executive is not as clear cut as it is, for example, in the American context (e.g., Carrubba and Zorn 2010). Substantively, however, these measures are reflective of the amount of support that Germans have for the FCC relative to the primary lawmaking and enforcement institutions of their government.

I use the answers to the following survey question in ALLBUS to operationalize my dependent variables (*TrustFCC, Trust Parliament, Trust Federal*): "Please tell me for each institution or

organization how much trust you place in it [...] 1 means you have absolutely no trust at all, 7 means you have a great deal of trust. You can differentiate your answers using the numbers in between. What about the Federal Constitutional Court (Bundestag) (federal government)?" I rescale this variable from 0 to 1 for ease of interpretation. I utilize responses from ALLBUS surveys sufficiently post-reunification that asked this question with regards to all three institutions (2000, 2002, 2008, 2012, 2018). Additionally, I use answers to a question asking "How would you generally rate your own financial situation?" (*Financial Situation*). I rescale this variable from 0 to 1 for ease of interpretation with 1 indicating that the respondent rated their financial situation as "Very Good" and 0 indicating "Very Bad." This variable will allow me to compare support for political institutions to assessments of one's own financial status, as scholars argue that support for political institutions may be a function of financial well-being (e.g., Kam and Palmer, 2008; Marshall, 2016). Although survey respondents may falsify their preferences and not truthfully report how much trust they have in political institutions or their personal assessments of their financial status, I have no reason to believe that survey respondents are systematically falsifying preferences.

I identify those who were educated in East Germany through two survey questions asking the respondent whether they spent their youth in East or West Germany (year 2000 survey) and whether they were born in East or West Germany (all other survey years). While it is possible that respondents indicating they spent their youth in or were born in East Germany were educated in West Germany, the GDR's strong restrictions on travel to West Germany make this unlikely. To further avoid potential noncompliance problems, I only include survey respondents in the sample that indicated that they had graduated from POS. Additionally, I only include survey respondents born after the division of East and West Germany in 1949.

To compare students with differential exposure to education under democracy in East Germany, I leverage the school enrollment cutoff date in the GDR for regression discontinuity (RD), difference-in-differences (DiD), and difference-in-difference-in-differences (DiDiD) designs. Given the aforementioned centralization of educational policy in the GDR, among the uniformly implemented policies were the birth date cutoffs determining when a child began their schooling. In the GDR, children turning six on 1 June or later[6] in a given year were to start school in POS the following year in September. Importantly, ALLBUS' data only contain information about a survey respondent's birth month and birth year. Given the cutoff is on 1 June, a respondent's exact day of birth is not required in order to accurately discern their school cohort. The variable *After May* is a binary indicator for whether a survey-respondent was born on 1 June or later in a given year.

Since the Berlin Wall fell in November of 1989, students born on 1 June or later within each year between 1973 and 1982 were exposed to an additional year of education under democracy during their POS schooling relative to those students born before 1 June in each year. For example, within the 1973 birth year, students born before 1 June already completed POS before the fall of the Berlin Wall, while students born on 1 June or later were in their final year of education when the Berlin Wall fell and therefore had one year of exposure to schooling under democracy. For those not born between the years of 1973 and 1982, students born 1 June or later would have experienced similar educational environments. The variable *Cohort* is a binary indicator for whether a survey-respondent was born within the relevant birth cohorts (1973–1982).

Regression discontinuity design

Since these data only have information about each survey respondent's birth month, the running variable for the RD design is discrete. The discrete running variable creates challenges that

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[6]Fuchs-Schündeln and Masella (2016) leverage these school cutoff dates to analyze the effect of exposure to socialist education on labor market outcomes. Using a difference-in-differences design, they find that an additional year of socialist education decreases an individual's probability of obtaining a university degree and has adverse affects on long-term labor market outcomes for men.

continuity-based RD approaches cannot properly address. The continuity-based approach for calculating robust standard errors for sharp RD designs assumes that the running variable is continuous at the cutoff and requires the presence of observations close to the cutoff in large samples. This assumption, thus, "rules out discrete-valued running variables" (Calonico et al., 2014, 2299). Second, since RD designs with a continuous running variable often need a substantially larger number of observations to produce the same amount of precision as a randomized control trial (Deke and Dragoset, 2012), RD designs using discrete running variables are likely to be under powered when using continuity-based approaches. To check the power of the RD design empirically, I include power calculations following the recommendations of Cattaneo et al. (2019) and using their rdpower package in R in Figure A4 in the appendix. Utilizing robust standard errors as recommended by Calonico et al. (2014), it would require an effect size of about 20 percent—approximately one standard deviation—to reach statistical significance at the 10 percent level. The default amount most commonly used in regression discontinuity power analyses is 10 percent of a standard deviation (e.g., Holbein and Rangel, 2020).

To properly estimate the RD, I instead opt for a local randomization-based approach. This approach assumes that the researcher can identify a randomization mechanism near the RD cutoff that determines treatment assignment such that the researcher can regard units close to the cutoff as part of a local randomized experiment (Lee, 2008). Leveraging this intuition and building off of the canonical scholarship on experimental analysis in which the potential outcomes are regarded as fixed (e.g., Imbens and Rosenbaum, 2005; Rosenbaum, 2007), Cattaneo et al. (2015) provide a framework and methodology using the randomization assumption to analyze RD designs. Cattaneo et al. (2017, 678), thus, state, "If the running variable is discrete, we recommend using local randomization methods as the primary analysis."

When using the local randomization approach, researchers need to decide the window around the cutoff and the polynomial fit. To maintain consistency when running models and maximize the number of observations across the different subsets of data, I use the largest possible window around the cutoff,[7] which, substantively, means that all available data are included in the RD models. I also opt for a linear polynomial fit, as evidence suggests that a relationship exists between an individual's age in their school cohort and long-term outcomes[8] that is separable from the effect of the treatment.[9] By estimating the treatment effect using a linear transformation,[10] I control for the alternative explanation that the effect at the discontinuity is simply due to a student being one of the oldest (youngest) members of their school cohort.

As applied to this specific research design, a crucial assumption for the RD is that the individuals born just before the 1st June cutoff are comparable to those born just after the cutoff. Since assignment to treatment is determined by one's birth date, this assumption is plausible. To sort in a means that would confound the treatment, parents would need to have information in advance about the fall of the Berlin Wall and use this information to plan their childbearing. To demonstrate the empirical validity of this assumption, I conduct McCrary (2008) tests in Figure A3 in the appendix. Furthermore, Figures A1 and A2 in the appendix show balance among the treatment and control groups on relevant pre-treatment covariates such as birth year, sex, and parental education. Lastly, constructing confidence intervals for the estimates requires an additional

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[7]Since the cutoff is 1 June, the bandwidth is five months on the left of the cutoff and seven months to the right of the cutoff.

[8]For example, utilizing a similar regression discontinuity design exploiting school enrollment birth day cutoffs, Matsubayashi and Ueda (2015) find that younger students in their cohort had higher mortality rates by suicide and tended to follow different career paths than relatively older members of their school cohort.

[9]Cattaneo et al. (2017, 675) state, "If we assume that the potential outcomes are related to the score via a polynomial model whose coefficients are constant among units within each treatment group, then we can transform the potential outcomes to remove the score and adopt Fisherian randomization-inference methods on the transformed outcomes."

[10]I avoid using higher-order polynomials as Gelman and Imbens (2019, 447) provide evidence that higher order polynomials lead to "noisy estimates, sensitivity to the degree of the polynomial, and poor coverage of confidence intervals."

assumption; in particular, the local stable unit treatment value assumption (Cattaneo et al., 2015, 6). Given the nature of the treatment assignment mechanism, this assumption is reasonable. Under this assumption, following the logic of Rosenbaum (2007), I calculate 90 percent confidence intervals under interference using the rdlocrand package in R (Cattaneo et al., 2016, 340).

Difference-in-differences and difference-in-difference-in-differences design

To demonstrate robustness, I employ DiD as well as DiDiD[11] designs by comparing East German students to their West German counterparts born before and after 1st June within and outside the relevant birth cohorts affected by the change in schooling. For the first difference, similar to the RD design, I exploit the school enrollment cutoff date by creating a binary variable indicating whether an individual was born on 1st June or later (*AfterMay*). The second difference compares survey respondents in East Germany to those in West Germany using a binary indicator for whether the respondent was born in East Germany (*East*). The third difference compares survey respondents within the relevant birth cohorts to those born outside of those cohorts using a binary indicator (*Cohort*).

Figure 1 visually examines the parallel trends assumption in this DiDiD design—the effect of one's birth date on the dependent variables in East Germany should trend similarly to the effect in West Germany for survey respondents born before 1973.[12] It plots the difference in means for those born before and after 1st June in each birth cohort between 1950 and 1971. The circles (triangles) represent those born in East (West) Germany with 90 percent confidence intervals. The trends become more stable after 1965, as in that year East Germany formally legislated its 1st June school enrollment cutoff date (e.g., Fertig and Kluve, 2005). The parallel trends assumption is most questionable for the *Trust Parliament* variable, however, post-1965 the effects in East Germany and West Germany are not distinguishable from 0. The effect of the cutoff in East and West Germany is similar for the other dependent variables, especially after 1965.

For the DiD models, I estimate an OLS regression of the form

$$\mathbf{Y}_i = \beta_0 + \beta_1 \cdot East + \beta_2 \cdot AfterMay + \beta_3 \cdot East \cdot AfterMay + \epsilon_i \tag{1}$$

and for the DiDiD models, I estimate an OLS regression of the form

$$\mathbf{Y}_i = \beta_0 + \beta_1 \cdot East + \beta_2 \cdot AfterMay + \beta_3 \cdot Cohort + \beta_4 \cdot East \cdot AfterMay$$
$$+ \beta_5 \cdot East \cdot Cohort + \beta_6 \cdot AfterMay \cdot Cohort + \beta_7 \cdot AfterMay \cdot East \cdot Cohort + \epsilon_i \tag{2}$$

with $\mathbf{Y}_i$ a vector of the dependent variables (*Trust FCC, Trust Parliament, Trust Federal, Financial Situation*), and survey-year fixed-effects for the models that pool all of the data and the models that examine each birth cohort individually. I also include survey weights in these models. The appropriate level of clustering for standard errors is the *East-Cohort-BirthMonth* level. However, since only 48 clusters exist for the DiDiD design and only 24 clusters exist for the DiD design (clustered at the *East-BirthMonth* level), I calculate confidence intervals using a variation of the pairs cluster bootstrap-se method from 1000 block-bootstrap replications (Cameron et al., 2008, 427).[13] Important to note is that this strategy creates asymmetric confidence intervals, as I create a series of wald statistics and use the lower 5 and upper 95

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[11]See Olden and Men (2022) for a thorough overview of the difference-in-difference-in-differences framework.

[12]As Olden and Men (2022, 8) explain, "we need the differential in the outcomes of group A and group B in the treatment state to trend similarly to the differential in the outcomes of group A and group B in the control state, in the absence of the treatment."

[13]Cameron and Miller (2015, 343) write "A good choice of B is B = 999," with B denoting the number of bootstrap replications. Since my independent variables of interest are binary, some bootstrap resamples had limited or no variation and
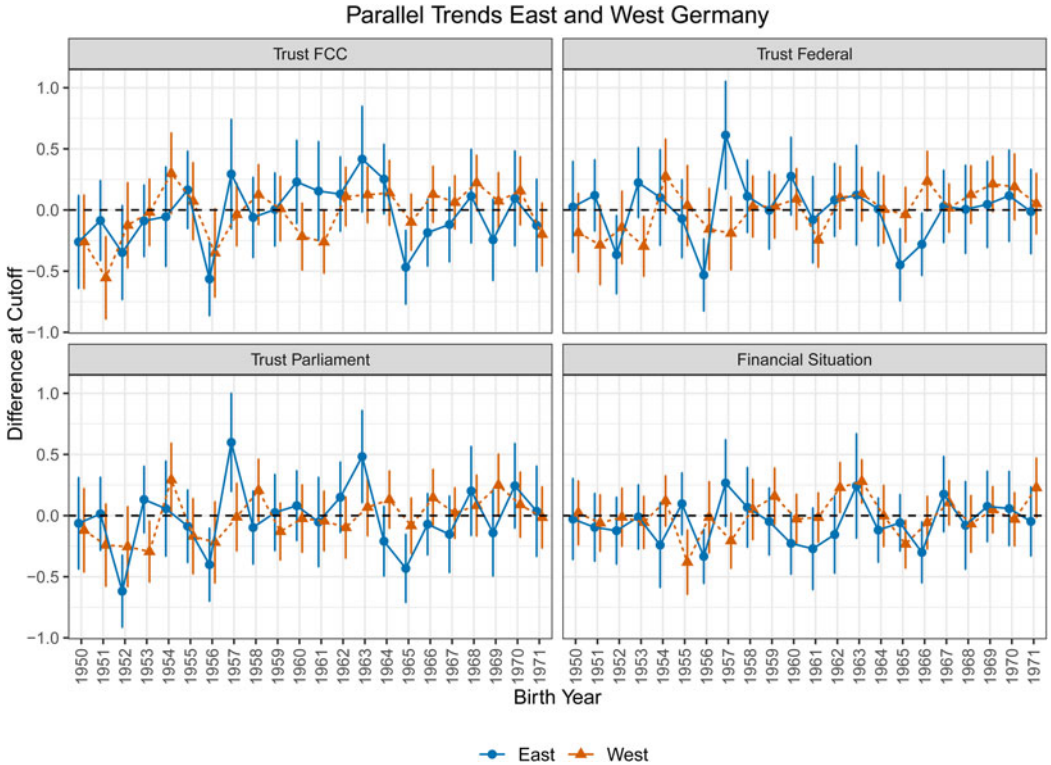
Fig. 1. Using data pooled across survey years, this figure displays the DiDiD parallel trends assumption for each of the four dependent variables. It plots the difference in means for those born before and after 1st June in each birth cohort between 1950 and 1971. The circles (triangles) represent those born in East (West) Germany with 90percent confidence intervals. The trends become more stable after 1965, as in that year East Germany formally legislated its 1st June school enrollment cutoff date.

percentile for critical values.[14] I, therefore, report confidence intervals instead of standard errors in the appendix tables.

Results and discussion

In the first test of hypothesis 1, which argues that attending school under democracy will have similar effects across institutions, I run the models on each individual birth cohort (1973–1982) in school during the fall of the Berlin Wall separately. It is possible that the number of years of exposure to schooling under democracy could have an effect on trust in political institutions. Alternatively, since these data are pooled by survey year (e.g., an individual born in 1973 that took the survey in 2000 is grouped together with an individual born in 1973 that took the survey in 2018), the estimates may be inconsistent across model specifications. If the true

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violated the full rank assumption. I, thus, generated 1027 bootstrap resamples and discarded the 27 resamples that violated the full rank assumption.

[14]For each bootstrapped replication of size 48 clusters (24 clusters for the DiD models), I calculate a Wald statistic: $w_b^* = \frac{(\hat{\beta}_{1,b}^* - \hat{\beta}_1)}{s_{\hat{\beta}_{1,B}^*}}$, with $b = 1, \ldots, B$ bootstrap replication, $\hat{\beta}_1$ the coefficient estimates from the initial regression model, $\hat{\beta}_{1,b}^*$ the coefficient estimates from each bootstrap replication, and $s_{\hat{\beta}_{1,B}^*}$ the bootstrapped standard error. To calculate the 90 percent confidence intervals, I then order the bootstrapped Wald statistics and use the values at the lower 5 ($\underline{z}$) and upper 95 percentiles ($\overline{z}$) as critical values. I calculate the lower confidence interval as $\hat{\beta}_1 - |s_{\hat{\beta}_{1,B}^*} \cdot \underline{z}|$ and upper confidence interval as $\hat{\beta}_1 + |s_{\hat{\beta}_{1,B}^*} \cdot \overline{z}|$.

mechanism by which schooling under democracy affects public support for political institutions is downstream, null effects from earlier survey years may negate positive effects in later survey years.[15] Too few observations exist to obtain statistical precision running models for each combination of birth cohort and survey year. Since these models are examining each birth cohort individually, I can only estimate DiD models on these subsets, as the DiDiD models are not fully specified.

To test hypothesis 2, I run the models on each survey year individually (2000, 2002, 2008, 2012, 2018). This strategy allows me to observe whether the effects of schooling under democracy become stronger over time, and, with regards to hypothesis 1, whether these effects are similar across institutions. Additionally, I can also discern whether support for political institutions is similarly correlated with one's financial situation. Survey respondents born in 1982, for example, are more likely to be fully integrated into the labor force and earning higher incomes in 2018 relative to 2008. As a result, it is reasonable to expect that the difference in support across the FCC, federal government, and Bundestag may be the highest in 2018 relative to the other survey years among those that received an additional year of schooling under democracy.

Hypothesis 1: similar null effects across birth cohorts

Figure 2 presents the results disaggregated by birth year. The coefficients can be understood as the effect of an additional year of schooling under democracy on the relevant dependent variable. Detailed tables can be found in the Appendix. Across all dependent variables in the birth year models, no discernible effect of schooling under democracy on trust in political institutions exists. While for specific birth cohorts I find positive and statistically significant results—for example, 1980 for *Trust FCC*, *Trust Federal*, and *Trust Parliament*, or 1973 for *Trust Federal*—a consistent pattern does not emerge. A potential alternative explanation is that only a large difference should exist with those with some exposure to schooling under democracy compared to those who only experienced schooling under autocracy (e.g., the 1973 cohort). Within some cohorts, furthermore, the coefficients for the RD and DiD models do not have the same sign. Moreover, I do not find any results that are statistically significant within both RD and DiD specifications for the *Financial Situation* variable. Substantively, across individual birth cohorts, I find that schooling under democracy has similar effects on public support for institutions, which provides evidence in support of hypothesis 1. These models, importantly, pool data across survey years for each birth cohort, which may be masking any downstream impacts of schooling under democracy.

Hypothesis 2: positive downstream effects across dependent variables by 2018

The models in Figure 3 leverage the different survey years in these data. First, I provide results pooled across survey years for reference. I find that schooling under democracy affects public support for the FCC, federal government, and parliament similarly. For none of the dependent variables, however, is the coefficient for the DiDiD model statistically significant, as the 90 percent confidence intervals overlap 0. Nonetheless, the coefficient is positive for all dependent variables. These models, similar to those for each individual birth cohort, provide support for hypothesis 1, but suffer from a downward bias due to the pooling across survey years. Next, when examining the results for the 2000, 2002, 2008, and 2012 survey years, no discernible pattern exists across the dependent variables. While the RD specifications are statistically significant for *Trust FCC* in 2008, *Trust Federal* in 2000, and *Trust Parliament* in 2000, they are not robust to the DiD or DiDiD specifications.

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[15]Since ALLBUS only surveys those 18 and older, no survey data exist in the year 2000 for respondents born in 1982.
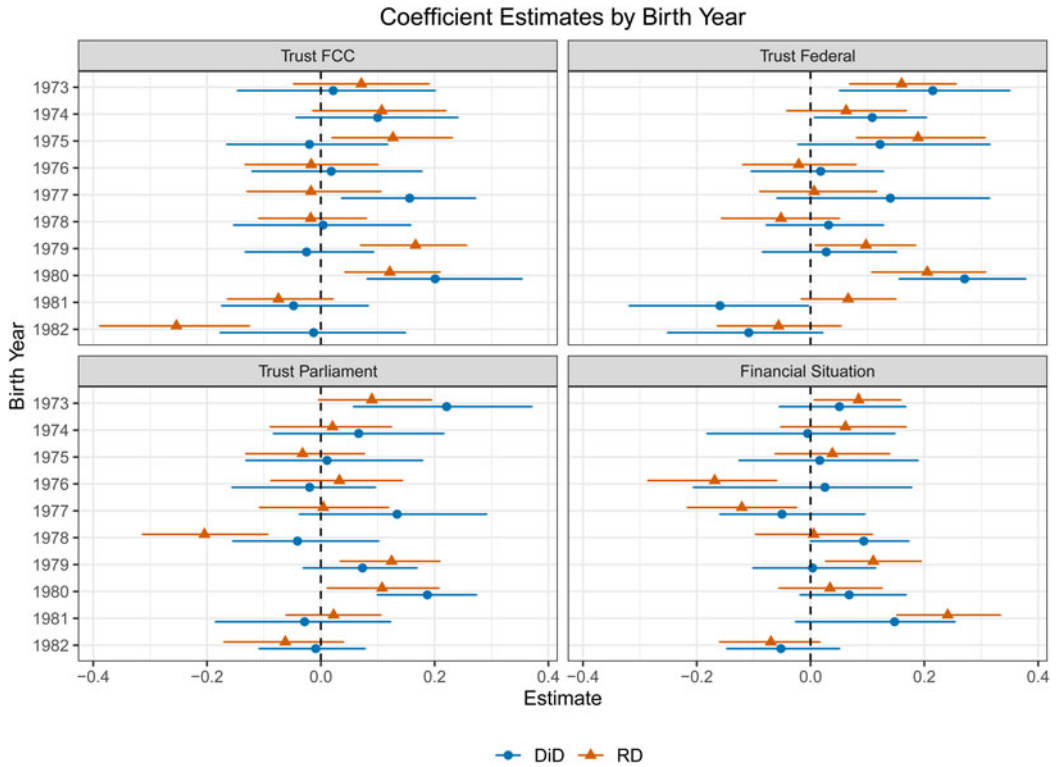
Fig. 2. This figure plots the estimated effect of an additional year of schooling under democracy on the four dependent variables separately for each birth cohort. The circles represent the DiD estimates with 90percent confidence intervals calculated from 1000 block-bootstrap replications, and the triangles represent RD estimates with 90percent confidence intervals under interference. Detailed tables are in the Appendix.

The results for 2018 provide a more consistent picture. Across all dependent variables and model specifications with the exception of the *Trust FCC* DiDiD model, I find that schooling under democracy has positive and statistically significant effect. These results support the expectation in hypothesis 1, as the effect of schooling under democracy on public support is similar across institutions. Furthermore, these results provide evidence for hypothesis 2, as the effect of schooling under democracy on public support for each of the institutions is strongest in 2018. The *Financial Situation* dependent variable provides some insight as to why this effect does not manifest until 2018. As the coefficients demonstrate, across all model specifications, survey respondents in 2018 with an additional year of schooling under democracy are more likely to rate their personal financial situation as "very good." If the true mechanism through which schooling under democracy affects public support for institutions is through labor market outcomes (e.g., Fuchs-Schündeln and Masella, 2016; Marshall, 2016) or other instrumental benefits, these results are intuitive. Given that the youngest cohort included in these analyses were age 34 in the 2018 survey, they were more likely to be established in the labor market in 2018 relative to the earlier surveys in which they had either just entered the labor force or were yet to enter the labor force.

In sum, my findings provide evidence that a relationship between schooling under democracy and public support for political institutions exists. This relationship, nonetheless, may be conditional on instrumental benefits and may manifest over time as children become more involved in and tangibly affected by their political institutions as adults. Furthermore, these effects are similar
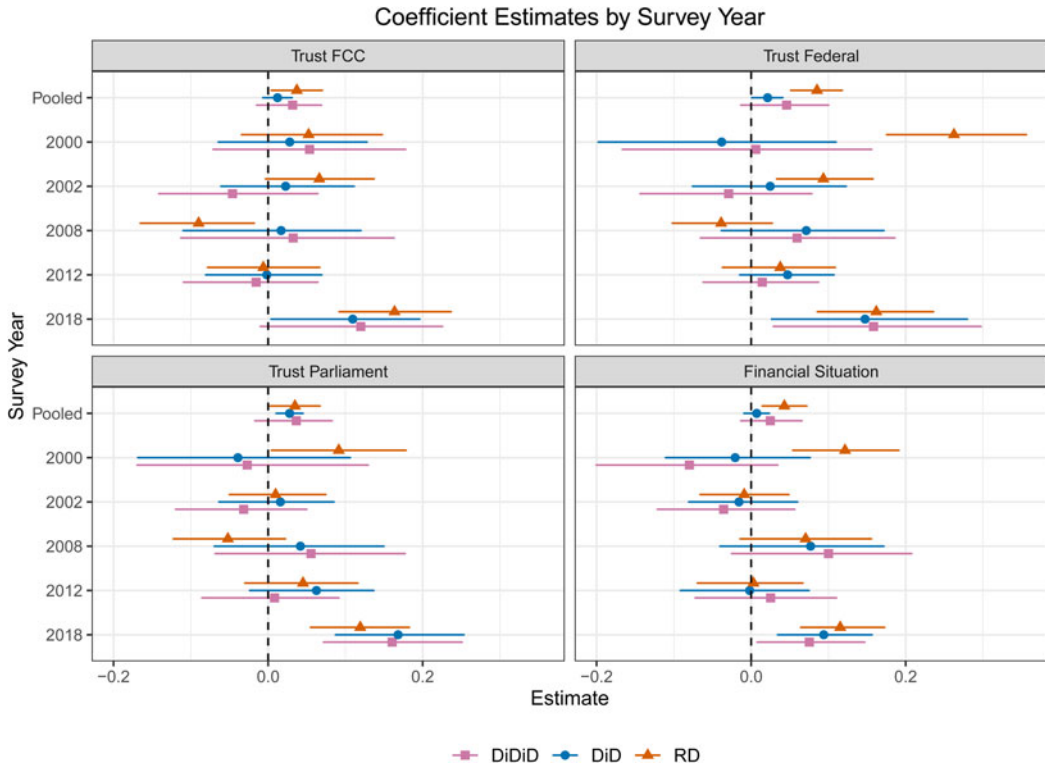
Fig. 3. This figure plots the estimated effect of an additional year of schooling under democracy on the four dependent variables separately for each survey year. The squares represent the DiDiD estimates with 90percent confidence intervals calculated from 1000 block-bootstrap replications, the circles represent the DiD estimates with 90percent confidence intervals calculated from 1000 block-bootstrap replications, and the triangles represent RD estimates with 90percent confidence intervals under interference. Detailed tables are in the Appendix.

across judicial, legislative, and executive institutions. This finding demonstrates citizens are not necessarily inclined to punish their legislative and executive institutions when they do not comply with court rulings. Without such a desire to uphold the rule of law among citizens, the efficacy of the separation of powers in a new democracy is in doubt. These dynamics—the downstream effects of schooling and similar effects across institutions—may provide insight into the failure of democratic transitions. Citizens reluctance to support their courts following a democratic transition may allow executives to consolidate power without meaningful institutional resistance. If such attempts at democratic backsliding occur early enough following a democratic transition, citizens may not have enough time to acculturate themselves to their new institutions and experience the benefits of functioning checks and balances.

Conclusion

In this article, I argue that education under autocracy has persistent effects on citizens' support for democratic institutions, and explore (1) whether schooling under education can reverse these effects, (2) if these effects become stronger over time, and (3) whether these effects differ across institutions. Ascertaining whether these effects differ across institutions has implications for the efficacy of the separation of powers. If schooling under democracy has similar affects across executive, legislative, and judicial institutions, citizens are not inclined to support a court when it aims to constrain the legislature or executive. To test these hypotheses, I run RD, DiD, and

DiDiD models leveraging the school enrollment cutoff dates in East Germany and the fall of the Berlin wall by comparing trust in the FCC, parliament, and federal government across survey respondents in East and West Germany and the (un)affected birth cohorts. Since the source of variation is exogenous, the differences in trust in each institution can be attributed to differences in schooling. I find evidence that an additional year of exposure to schooling under democracy caused similar increases in East Germans' trust in their institutions. This effect manifested in 2018 and was absent in earlier survey years. These results also reflected East Germans' perceptions of their own financial situations, which provides some evidence that support for political institutions may be a function of instrumental benefits. This article contributes to and has implications for the scholarship on separation of powers in new and established democracies, education, and historical legacies.

First, this article has implications for the efficacy of the separation of powers in new democracies. My results provide evidence that education following a democratic transition has similar effects on trust in courts, legislatures, and executives. It is, therefore, unclear whether citizens would support their court when it attempts to uphold the rule of law when in conflict with the executive and legislature. Citizens' unwillingness to support their courts may empower leaders in new democracies to attack the institutional integrity of the judiciary through court curbing measures and to not comply with adverse court rulings. While the most popular recent examples of court curbing are Hungary and Poland (Kelemen, 2017), historical examples abound in states such as Argentina (Helmke, 2005), Chile (Hilbink, 2007), Japan (Ramseyer and Rasmusen, 2003), Mexico (Staton, 2010), Russia (Herron and Randazzo, 2003), and the United States (Clark, 2011) among others. For such courts, building public support takes time. As a result, they must be cautious when exercising their judicial review powers. When public support is low, courts are compelled to act strategically over time to expand their judicial review powers to build public support. Courts that act overly aggressively, however, may lose public support if their rulings are openly defied (e.g., Carrubba, 2009).

Second, this article contributes to the scholarship on the relationship between education and political attitudes and complements the burgeoning scholarship on the nondemocratic origins of mass schooling (Cantoni et al., 2017; Paglayan, 2021). My results provide evidence that schooling under democracy can counteract the long run effects of education under authoritarianism. However, depending on the political context, we may not necessarily expect a positive relationship between increased education in democratic values and individual political outcomes (e.g., Croke et al., 2016). The existing literature often has contradictory findings when evaluating the effect of education (e.g., Galston, 2001). Carefully delineating the mechanisms through which education should affect political outcomes and the direction of these effects may help scholars make sense of findings that may seem contradictory on their face but are, in fact, conditional on important covariates.

Third, this article contributes to the extensive scholarship on historical legacies (e.g., Simpser et al., 2018). These findings are especially relevant to the scholarship on communist legacies and citizens' trust in democratic institutions (e.g., Pop-Eleches and Tucker, 2014). Related to the aforementioned importance of context, however, we may not expect these findings to generalize to the legacies of all authoritarian regimes. Depending on the historical role of courts in a regime (e.g., Moustafa, 2014), we may have differing expectations over whether citizens will have higher (lower) support for courts after a democratic transition. Lastly, we may expect that historical legacies that pre-date a given regime may also have an affect on support for institutions (e.g., Pop-Eleches, 2014). Future research can theorize over and empirically test whether, for example, socioeconomic differences that predated an authoritarian regime and persisted through to the transition to democracy affect present-day support for institutions.

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