

# “The People Want the Fall of the Regime”: Schooling, Political Protest, and the Economy\*

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## Abstract

We provide evidence that economic circumstances are a key intermediating variable for understanding the relationship between schooling and political protest. Using the World Values Survey, we find that individuals with higher levels of schooling, but whose income outcomes fall short of that predicted by their biographical characteristics, in turn display a greater propensity to engage in protest activities. We discuss a number of interpretations that are consistent with this finding, including the idea that economic conditions can affect how individuals trade off the use of their human capital between production and political activities. Our results could also reflect a link between education, “grievance”, and political protest, although we argue that this is unlikely to be the sole explanation. Separately, we show that the interaction between schooling and economic conditions matters too at the country level: Rising education levels coupled with macroeconomic weakness are associated with increased incumbent turnover, as well as subsequent pressures toward democratization.

*Keywords:* Education; Human capital; Political protest; Demonstrations; Economic under-performance; Incumbent Turnover; Democratization.

*JEL Classification:* D72, D78, I20, I21, O15

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# 1 Introduction

Every so often, longstanding and seemingly stable incumbents have been challenged by a groundswell of discontent and public protest, and even been forced out as a result, as vividly illustrated by the recent events of the Arab Spring. While it is understood that simmering public dissatisfaction can suddenly erupt in revolution (Kuran 1989), we are naturally led to ask what systematic forces or structural conditions might lead such dissatisfaction to arise and get translated into widespread political action.

A natural starting point for this investigation is one of the best-known empirical relationships in the social sciences, namely the strong positive correlation between schooling and political participation. It is well-known that more educated citizens display a greater propensity to engage in virtually all forms of political activity, ranging from more mundane acts such as voting and discussing politics, to the more public forms of mobilization such as attending political events and demonstrating.<sup>1</sup> Several recent studies have shown, however, that favorable aggregate economic circumstances in the labor market can mute the propensity of individuals to engage in political activities (Campante and Chor 2012a, Charles and Stephens 2013). Conversely, the high unemployment rates that persisted in many Arab countries for a broad swath of educated workers have proved to be relevant for understanding the political upheaval of the Arab Spring (Campante and Chor 2012b).

This paper attempts to extract broader lessons from this interaction between schooling and economic conditions. We start off by looking at individual-level data, asking whether personal economic circumstances affect the propensity of educated individuals to be involved in political activities. To address this, we use data from the World Values Survey (WVS), which includes information on different forms of political participation across a broad sample of countries. We first construct a measure of the extent to which an individual's actual income deviates from that which is predicted by a comprehensive set of biographical characteristics, including education. This residual thus constitutes a measure of relative income performance, in comparison with other individuals with similar attributes.

We find robust evidence that the interaction between this income residual and one's education level is negatively correlated with political participation. In particular, the more positive an individual's income residual, the less responsive to education is one's propensity to engage in political activities. Conversely, more highly educated individuals whose actual income nevertheless under-performs that predicted by their biographical characteristics are more likely to devote their human capital toward active political involvement. Of note, the above pattern holds true precisely for protest modes of participation such as

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<sup>1</sup>This extensive literature includes: Verba and Nie (1987), Rosenstone and Hansen (1993), Putnam (1995), Verba et al. (1995), Bénabou (2000), Schlozman (2002), Dee (2004), Freeman (2004), Milligan et al. (2004), Hillygus (2005), Glaeser et al. (2007), and Sondheimer and Green (2010). For interesting evidence specific to the Middle East, see El-Said and Rauch (2012). While many of these contributions suggest a causal effect, a recent literature has called this into question particularly for the act of voting (e.g., Tenn 2007; Kam and Palmer 2008; Berinsky and Lenz 2011).

demonstrations, strikes or occupying buildings, but not for more civic and less time-intensive activities such as signing petitions or discussing politics. In short, we see that economic under-performance at the individual level conditions the link between schooling and political protest.

We discuss at some length several interpretations of this strong empirical regularity. First, the above patterns could be driven by the decisions that individuals make over the competing uses of their human capital in production versus (effort-intensive) political participation: Positive productivity shocks or shifters that raise one’s ability to earn income from one’s human capital would naturally discourage its use in political activities, an idea which we develop formally in the Model Appendix. Alternatively, it could be that the link between economic under-performance and political protest operates through “grievance”: An individual’s disappointment with his personal economic situation can lead to political disaffection, and perhaps especially so for the more educated. We will present some evidence indicating that grievance motives are relevant, but that they are also unlikely to fully account for our findings, as our key results related to the income residual survive when we control directly for survey measures of individual dissatisfaction.<sup>2</sup> A third possibility is that our results could reflect a form of self-selection. For example, there could be an omitted individual characteristic (such as ideology) which tends to raise one’s intrinsic propensity to protest, but which also could be correlated with education and labor market outcomes. Though the nature of our data makes it difficult for us to tease out a causal effect, we can attempt to proxy for such an omitted characteristic with variables that would presumably correlate strongly with it. As it turns out, our results are robust to including such proxies as an individual’s political positioning and religiosity. This suggests that the relationships we have found are unlikely to be due solely to the self-selecting behavior of ideologically-extreme individuals, although we acknowledge that other non-causal stories cannot be ruled out.

In the second half of the paper, we explore whether the interaction between schooling and economic circumstances also has aggregate consequences, in particular for the stability of incumbent regimes. We assemble a panel comprising more than 120 countries and spanning 1976-2010, combining updated data on incumbent turnover collected in Campante et al. (2009) and information on country schooling from the Barro and Lee (2010) dataset; in particular, we focus on average years of secondary schooling as a measure of country-level schooling outcomes. Motivated by our earlier findings, we first proxy for country economic circumstances by using the median income residual (computed from the WVS) for the subset of individuals in each country who had attained at least some secondary education. We later turn to real GDP per capita as an alternative measure of economic conditions, as this latter variable offers more balanced coverage that allows us to apply more powerful panel data estimation techniques (including controlling for country fixed effects). Our results here indicate that the combination of high levels of

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<sup>2</sup>We use the terms “grievance”, “dissatisfaction”, and “disaffection” interchangeably in this paper.

country secondary schooling and relatively weak economic prospects is indeed associated with a higher frequency of incumbent turnover.

We also find that the same interaction between country secondary schooling and income per capita exhibits a robust negative correlation with country democracy scores, taken from the Polity IV dataset. This is consistent with the increased incumbent turnover being accompanied by some degree of institutional change, in the direction of greater democratization. These patterns suggest that any effect of increased education on the prospects for democracy could be contingent in an important way on macroeconomic circumstances.

In sum, we find that individual economic circumstances play a key intermediating role in governing the strength of the positive association between the individual's education and her propensity toward political protest. These relationships appear to extend to the country level, where rising levels of education without commensurate economic gains are associated with greater uncertainty of tenure for the political incumbent.

This paper builds naturally on Campante and Chor (2012a). While the focus in this earlier paper is on how the interaction between individual schooling and *aggregate* economic conditions affects political participation, we show here that a similar interaction is present when looking at income shocks at the *individual* level. We further explore here the connection with country-level political outcomes, a link which had been hinted at in the specific context of the Arab Spring in Campante and Chor (2012b).

In terms of the broader literature, our findings relate to a large body of work stressing the role of the cost of political engagement in terms of other productive uses of resources. This has been emphasized in the study of regime transitions (Acemoglu and Robinson 2001, 2005; Burke and Leigh 2010; Brückner and Ciccone 2011; Aidt and Leon 2012), civil wars (Grossman 1991; Collier and Hoeffler 2004), and political violence in general (Besley and Persson 2011), and is a standard feature of rational models of political participation (e.g., Riker and Ordeshook 1968). We specifically focus on the interplay of this logic with individual education and one's propensity toward political protest. The various interpretations of our evidence that we offer combine elements of the "resource" view that links socioeconomic status (such as education) with political engagement (e.g., Verba and Nie 1987), as well as of the "disaffection" view of political protest (e.g., Gurr 1970). The interaction with relative income performance also ties in with a longstanding sociological literature that links political protest with concepts such as "relative deprivation" and "status inconsistency" (e.g., Lenski 1954; also, see Orum 1974 or Gurney and Tierney 1982 for critical appraisals).

Our argument is further related to a venerable line of thinking that has linked modernization, and specifically increases in education, to political turmoil. For instance, Huntington (1968) has argued that higher education in many modernizing countries failed to provide skills that were relevant to the coun-

tries’ needs, leading instead to alienation and instability. We provide evidence that is suggestive of such a connection, while placing an added emphasis on the tradeoff between the use of human capital in production and time-intensive political activities. We also speak to the vast literature on the “modernization hypothesis”, and in particular, the link between education and democracy. This hypothesis can be traced back at least to the seminal contributions from Dewey (1916) and Lipset (1959), and has been the subject of considerable empirical scrutiny, recent examples of which include Acemoglu et al. (2005, 2008, 2009), Bobba and Coviello (2007), Glaeser et al. (2007), *inter alia*. While there is still an active debate here on whether education causes democracy, our results suggest that an alternative line of inquiry would be to examine under which conditions the effects from schooling to democracy are seen to be stronger.

We proceed as follows. Section 2 describes the data and the empirical strategy that we employ to study political protest at the individual level. Section 3 presents these results. In Section 4, we turn to the country-level evidence on incumbent turnover and democratization. Section 5 concludes. Details of our model of political participation and our data sources are contained in the Appendix to this paper. (A separate online appendix contains all the Appendix Tables referred to in the text.)

## **2 Schooling, Political Protest, and Economic Circumstances at the Individual Level: An Empirical Framework**

### **2.1 Data**

We start by studying the relationship between schooling and political protest at the individual level. For this purpose, we draw on data from the World Values Survey (WVS), a well-established survey of sociocultural and political attitudes around the world. The WVS contains five waves, conducted in 1981-1984, 1989-1993, 1994-1999, 1999-2004, and 2005-2008 respectively. Our eventual regression sample will draw mostly on Waves 3-5, as the set of variables related to political participation is considerably more limited in the earlier waves. In all, the sample that we use contains over 190,000 individuals from 148 surveys (from 84 distinct countries or territories), as listed in the Data Appendix. This contains a broad spectrum of both developed and developing countries from all continents.

The WVS includes questions on a range of modes of political participation. We seek to understand patterns in these measures, with an emphasis on the distinction between protest and civic modes of participation. With regard to the former (political protest), we make use of four WVS questions (E026-E029) eliciting one’s propensity toward: (i) “attending lawful demonstrations”; (ii) “occupying buildings or factories”; (iii) “joining in boycotts”; and (iv) “joining unofficial strikes”. These are forms of political activity which are often associated with public displays of protest, and that also tend to be demanding in terms of the time and effort required of the participants. For our analysis, we have recoded the categorical

responses to these questions to be increasing in the degree of involvement, specifically: “Would never do” (response=0), “Might Do” (=1), and “Have Done” (=2).

We contrast the above against a set of three additional measures that correspond to “softer” modes of political activity that are more civic in nature. These are: (i) “signing a petition” (question E025); (ii) “discuss politics” (A062); and (iii) “vote” (E257). The first of these variables on signing a petition is coded on the same 0-2 scale as the preceding protest measures. The second variable solicits how regularly one discusses political matters with friends; we recode the response options for this question as follows: “Never” (=0), “Occasionally” (=1), and “Frequently” (=2). Last but not least, “vote” is a binary variable asking whether the individual voted in the country’s recent parliamentary election. As this last variable is only available in Wave 5 of the WVS, the number of observations for “vote” is considerably smaller. We have classified these three activities as non-protest modes of participation, primarily because they tend to be less time- and effort-intensive. For example, signing a petition or having a discussion about politics is typically less strenuous than demonstrating in a public square. As for voting, this has commonly been described in the political science and political psychology literatures as a relatively passive form of participation, being among the least demanding in terms of its human capital requirements (e.g. Milbrath and Goel 1977; Verba and Nie 1987; Brady et al. 1995).<sup>3</sup>

The WVS also contains a large set of respondent biodata and characteristics. We use these as controls in our regressions, namely age, gender, marital status, and number of children. A key explanatory variable for our purposes will be the respondent’s highest level of educational attainment. This is coded in the WVS on a categorical scale that ranges from a low of 1 (“Inadequately completed elementary education”) to a high of 8 (“University with degree / Higher education - upper-level tertiary certificate”). While the WVS makes a concerted attempt to standardize this coding, there are a number of lingering concerns related to how consistently this has been recorded across countries with diverse education systems. In our empirical section, we will thus discuss several checks that we performed to mitigate such worries.

Separately, we also require information on respondent income, in order to examine how economic outcomes affect political participation. The WVS unfortunately does not report the actual income level of each respondent, but instead records the within-country income decile to which she belongs. It is nevertheless important to note that the local survey administrators determined beforehand the income levels that would correspond to the decile thresholds, and then proceeded to ask respondents which thresholds their income levels fell within, instead of asking respondents to make a subjective assessment of their position in the income distribution.

Table 1 reports summary statistics for the above political participation measures and respondent

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<sup>3</sup>For example, Milbrath and Goel (1977) write: “The act of voting does not require as much information and motivation as do most other political activities. Many people vote who are not politically involved, and conversely some who are involved may not bother to vote” (pp.12-13).

characteristics. Not surprisingly, respondents tend to report a lower propensity to engage in the protest modes of political activity, such as “Occupy” (mean=0.151 on the 0-2 scale) and “Strike” (mean=0.280), as compared to the less effort-intensive modes of participation, such as “Petition” (mean=0.884).<sup>4</sup> There is nevertheless a lot of variation in the data, as evidenced by the relatively large standard deviations compared to the mean values for all of these participation measures.

[TABLE 1 HERE]

## 2.2 Empirical Specification

Our objective is to examine whether individuals who experience relatively poor labor market returns are in fact more inclined to direct their human capital toward political activities. To address this, we pursue an empirical strategy in two steps. We first control for the role of respondent characteristics in explaining income differences across individuals. Intuitively, this allows us to infer a measure of the relative income performance of each individual, when compared against other respondents from the same country with similar biographical attributes (including age, education, and so on). In the second step, we then examine whether individuals who exhibit worse income outcomes relative to their peers also differ systematically in their patterns of political participation. We elaborate on this empirical strategy below.

We focus on relative income outcomes, as opposed to income levels per se, as this lets us capture two important forces shaping the incentives toward political participation. On the one hand, participation could be motivated by dissatisfaction (“grievance”) with one’s economic circumstances. Individuals who under-perform economically, relative to what they could have expected to earn given personal characteristics, can also be expected to be dissatisfied with the political establishment, on whom they might pin the blame (rightly or wrongly) for their disappointing outcomes. This “relative deprivation” – relative to some benchmark or expectation – can function as a trigger for participation in social movements or political activities (e.g., Walker and Smith 2002).

In addition, political participation can be affected by the fact that it competes for resources, and human capital in particular, which could otherwise be directed toward production. Conceptually, if income is derived from the production activities that one engages in, then the income residual would pick up the effects of idiosyncratic determinants of worker productivity that are not directly predictable from individual traits. These productivity shifters would in turn affect the individual’s incentives to allocate her human capital between labor market and political activities. Specifically, individuals with a large negative income residual (indicative of a negative productivity shock) can be expected to allocate human

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<sup>4</sup>The pairwise correlations between “Demonstrate”, “Occupy”, “Boycott”, “Strike”, “Petition”, “Discuss Politics”, and “Vote” are all positive, and range from a low of 0.0261 to a high of 0.5253. The political participation variables are thus far from collinear.

capital away from production toward politics instead. We provide a simple model in the Appendix that fleshes out this intuition more formally.<sup>5</sup>

Given the cross-sectional nature of the WVS data, a natural benchmark for relative performance is to compare the income decile of each person against that predicted for other observationally similar individuals. (Throughout this paper, this is the benchmark that we have in mind whenever we refer to “relative” income outcomes.) This motivates the following regression specification, which sets out to predict the income decile of individual  $i$  from country  $c$  in survey wave  $t$  as a function of a vector of respondent characteristics,  $V_{ict}$ . For each recorded level of education  $e \in \{1, \dots, 8\}$ , we run this regression for the subsample of respondents whose educational attainment,  $Educ_{ict}$ , was equal to  $e$ :

$$IncDec_{ict} = \alpha_{2,e}V_{ict} + \alpha_{3,e}D_{ct} + \eta_{ict,e}. \quad (1)$$

Here,  $D_{ct}$  denotes a full set of country-survey wave dummies that absorbs the effects of average differences across countries and time. In words, for respondents at each level of educational attainment  $e$ , we regress their income decile against a large set of personal characteristics and country-survey wave fixed effects. An advantage of this approach is that it allows the regression coefficients,  $\alpha_{2,e}$  and  $\alpha_{3,e}$ , to differ flexibly across different education levels  $e$ , without imposing a parametric assumption with regard to how income decile depends on education. We will thus refer to these as “Non-Parametric” residuals (with a slight abuse of terminology).

In practice, given the categorical nature of the income dependent variable, we estimate (1) via ordered logit. The results from these eight regressions are presented in full for interested readers in Appendix Table 1, where we report standard errors clustered by country. The corresponding income residual variable is then computed as:

$$IncResid_{ict} = IncDec_{ict} - \sum_{j=1}^{10} j \times \hat{Pr} \left( IncDec_{ict} \in Decile^{(j)} \right),$$

where  $\hat{Pr} \left( IncDec_{ict} \in Decile^{(j)} \right)$  is the predicted probability, from the estimation of (1), of the individual’s income falling in the  $j$ -th decile. Note that  $IncResid_{ict}$  is the union of the residuals generated from the eight separate regressions in (1). This income residual variable turns out to be centered around zero (to a first approximation), but with a considerable amount of variation (almost two income deciles) about that mean. To reiterate, this residual captures the extent to which an individual’s actual income decile exceeds that which one would expect on the basis of her level of education and other biographical characteristics. In particular, a negative value of  $IncResid_{ict}$  indicates an actual income status below

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<sup>5</sup>In particular, our modeling framework helps to clarify why it is more appropriate to use the income residual ( $IncResid_{ict}$ ) in our empirical strategy, rather than one’s actual income decile ( $IncDec_{ict}$ ): We cannot conclude that two individuals who fall in the same income decile necessarily received the same productivity shock, unless we have already explicitly controlled for the contribution of their biographical characteristics to their respective income outcomes.

what would have been predicted, which we will interpret as an under-performance in income outcomes for the individual in question.<sup>6</sup>

The vector of control variables,  $V_{ict}$ , comprises dummy variables for: (i) gender; (ii) age groups (for 25-34, 35-64, ..., 65 and over, with 15-24 being the omitted category); (iii) number of children (for 1, 2, ..., 8 and over, with no children being the omitted category); and (iv) marital status (8 categories). This allows us to control flexibly for the effects of these observed traits on income outcomes. By including this set of personal variables as well as country-survey wave dummies in (1), we effectively seek to compare each respondent against the average income decile for individuals with similar characteristics from the same country. Intuitively, we are not comparing retirees with young workers, but rather are comparing individuals who are “close” along these observed lines. The controls also help to ensure that any differences in political engagement that we find are likely associated with inherent differences in the income outcomes themselves, rather than being attributable to biographical characteristics that might also get reflected in incomes.<sup>7</sup>

We have separately considered several alternative ways of generating the income residual variable. A more basic approach would be to include  $Educ_{ict}$  as a right-hand side variable in a single regression that pooled the observations across all levels of educational attainment:

$$IncDec_{ict} = \alpha_1 Educ_{ict} + \alpha_2 V_{ict} + \alpha_3 D_{ct} + \eta_{ict}. \quad (2)$$

However, one concern with the above is that the “Pooled” income residuals thus calculated would end up being a parametric function of schooling. This potentially introduces a mechanical correlation between schooling and our empirical proxy of income under-performance, which could subsequently confound the interpretation of our findings. Yet another view is that the relevant comparison group for generating the residuals should ignore income outcomes outside of one’s own country. This would call for running the following regression separately for each of the 148 country-survey waves (indexed by  $s$ ):

$$IncDec_{ict} = \alpha_{1,s} Educ_{ict} + \alpha_{2,s} V_{ict} + \eta_{ict,s}. \quad (3)$$

A clear disadvantage of these “Country-Wave” residuals is that the coefficient estimates from (3) are less efficient than from our baseline “Non-Parametric” approach, as the latter are estimated off of more observations (from across different countries). That said, it is reassuring that the core results in this paper are preserved when using either the “Pooled” or “Country-Wave” versions of the income residual.<sup>8</sup>

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<sup>6</sup>We have also estimated (1) using ordinary least squares (OLS). This yields income residuals that are highly correlated with those generated via ordered logit, with the correlations being in excess of 0.99. Our findings on the determinants of political participation are thus very similar when the OLS-based income residuals are used instead (available on request).

<sup>7</sup>We have obtained very similar results (available on request) when further including separate sets of dummy variables for employment status, occupation, and size of town in the vector of respondent characteristics,  $V_{ict}$ . The rationale for including these variables is however more questionable, as they can be seen as being proxies for income, so we have erred on the side of caution by excluding them from our baseline.

<sup>8</sup>The correlation between any pair of these three income residual measures is in excess of 0.94.

Having constructed our measure of relative income performance, the second step in our empirical strategy then establishes how the income residual influences patterns of political participation. We pursue this with the following specification:

$$\begin{aligned}
 PolPart_{ict} = & \beta_1 Educ_{ict} + \beta_2 Educ_{ict} \times IncResid_{ict} + \beta_3 IncResid_{ict} \\
 & + \beta_4 IncDec_{ict} + \beta_5 V_{ict} + \beta_6 D_{ct} + \varepsilon_{ict}.
 \end{aligned}
 \tag{4}$$

Equation (4) seeks to explain political participation at the individual level,  $PolPart_{ict}$ , as a function of respondent characteristics, namely: (i) education; (ii) income decile; and (iii) the vector  $V_{ict}$  of control variables that entered earlier in our estimation of (1). Since our regression sample pools observations across all WVS surveys, we include country-wave fixed effects once again. The right-hand side also includes our key terms of interest, namely the income residual measure,  $IncResid_{ict}$ , as well as its interaction with education,  $Educ_{ict} \times IncResid_{ict}$ . We thus explore whether relative income performance has an effect on the level of one’s political participation, and moreover whether this effect differs across respondents with different schooling levels. The basic hypothesis that we test is whether  $\beta_2 < 0$ : Are highly-educated individuals who experience a poor relative income outcome (a more negative  $IncResid_{ict}$ ) in turn more inclined toward political activism?

We report standard errors clustered by country, to allow for possible correlated shocks that might affect the behavior of respondents from the same country. In principle, these also need to be adjusted further to take into account the use of the generated regressor,  $IncResid_{ict}$ . We have nevertheless computed bootstrap standard errors (based on 100 bootstrap samples by country cluster) for the specification in (4), and found that these were very similar in magnitude with no change to the statistical inference drawn (see Appendix Table 2 for the full results). We thus report the more conventional standard errors throughout the paper, given the computationally-intensive nature of the bootstrap procedure.

### 3 Findings on Political Protest at the Individual Level

#### 3.1 Baseline Results

We report our baseline findings in Table 2. In the upper panel, we first show the results from estimating (4) without the interaction term,  $Educ_{ict} \times IncResid_{ict}$ , between individual education and the income residual. We present the results for each political participation measure in a separate column. Since the dependent variables are again categorical in nature, we estimate the regressions using ordered logit. The one exception to this is in Column 5. There, we use the first principal component of “Demonstrate”, “Occupy”, “Boycott”, and “Strike” as our dependent variable, and perform the estimation via ordinary least squares (OLS) instead. The first principal component provides a convenient way to summarize the

common patterns underlying these four protest-related variables, while filtering out the noise that might be specific to any single measure.

[TABLE 2 HERE]

Across all columns, note that the coefficient on individual schooling is always positive and statistically significant, confirming the stylized fact in the extensive literature on the role of education as a determinant of political participation. Turning to the direct effect of the income residual variable, we find that its coefficient is typically negative, but is only significant in three out of the eight columns. While this indicates that individuals with relatively weak income outcomes appear more likely to engage in political activities, the strength of this evidence is not particularly striking.

We move on to the estimation of (4), including the interaction term of interest on the right-hand side. The lower panel of Table 2 now points strongly to a role for the income residual when it is considered in tandem with the respondent’s level of schooling. Our main result centers on the negative and significant  $\beta_2$  coefficient on  $Educ_{ict} \times IncResid_{ict}$  that we obtain for “Demonstrate”, “Occupy”, “Boycott”, and “Strike” (Columns 1-4), as well as for their first principal component (Column 5). Since these regressions control for country-survey wave fixed effects, we can interpret this as evidence that within each country-wave, more educated individuals whose incomes nevertheless fall short of what would be expected on the basis of their biographical traits are indeed significantly more likely to engage in protest modes of political expression. On the other hand, Columns 6-8 show that any such interaction effect of the income residual with schooling is statistically indistinguishable from zero for the civic modes of participation, namely “Petition”, “Discuss Politics”, and “Vote”.<sup>9</sup>

We provide here a preliminary sense of the size of the effects that are implied. It is most convenient to explore this using the Column 5 specification, which uses the first principal component measure. For a one standard deviation decrease in the income residual (equal to 1.967), we ask how much more inclined toward protest activity an individual with tertiary education would be relative to someone with incomplete primary education. The associated change in the dependent variable is:  $-0.0051 \times 7 \times 1.967 = -0.070$ , or about 5% of a standard deviation of the first principal component. As a comparison, the main effect from raising education in this way is:  $0.0678 \times 7 = 0.475$ , which is closer to one-third of a standard deviation in the measure of political participation. While the size of our interaction effect thus appears to be modest, we shall see later that the magnitudes implied are larger in our more thorough specifications.

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<sup>9</sup>The results are essentially unaltered if we control for an additional interaction term between respondent education and one’s income decile, confirming that what is driving the propensity toward political protest is in fact comparisons of one’s income against what would be expected for observationally similar individuals. See Appendix Table 3.

### 3.2 Robustness

We now include a series of further control variables, in order to confirm the robustness of our core findings. It turns out that our results remain intact, if not in fact being strengthened.

In Table 3, we include on the right-hand side of (4) interaction terms between individual education and income decile ( $Educ_{ict} \times IncDec_{ict}$ ), as well as between individual education and the set of country-survey wave dummies ( $Educ_{ict} \times D_{ct}$ ). The former seeks to ascertain that what matters for patterns of political protest is indeed the income residual, as opposed to one’s actual income decile. The latter set of controls is motivated by the evidence in Campante and Chor (2012a), who show that underlying features of the country can systematically influence the responsiveness of political participation to education at the individual level. In particular, they find that various proxies for the returns to skill at the country level tend to dampen the positive association seen between political engagement and individual schooling. The  $Educ_{ict} \times D_{ct}$  terms therefore provide a flexible way to control for the mediating effects of such country characteristics, without which the estimated coefficient of  $Educ_{ict} \times IncResid_{ict}$  is potentially exposed to omitted variables bias. As displayed in Table 3, our results are indeed robust to the inclusion of this extensive set of controls for each of the five political protest variables.<sup>10</sup> Once again, no negative interaction effect is found with the two civic participation variables, “Petition” and “Discuss Politics”. Interestingly, the pattern for “Vote” starts to resemble more that which we have seen for the protest variables, as the  $\beta_2$  coefficient in Column 8 is now negative and significant. Note moreover that the implied size of the interaction effect of interest on the propensity toward protest is now much larger relative to the corresponding Table 2 baselines. In Column 5 for example, a one standard deviation drop in the income residual for an individual with complete university education relative to one with incomplete elementary schooling would imply a change of:  $-0.0215 \times 7 \times 1.967 = -0.296$ , or about a decrease of one-fifth of a standard deviation for the first principal component.<sup>11</sup>

[TABLE 3 HERE]

The above stylized fact continues to stand out even when we perform a number of checks related to the income residual measure itself. We earlier discussed two alternative estimating equations from which we generated the “Pooled” and “Country-Wave” versions of the income residuals. It turns out that our results are reassuringly similar across these different ways of constructing  $IncResid_{ict}$ ; these are reported in Appendix Tables 4 and 5 respectively, where the regressions run are based on the full specification

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<sup>10</sup>The reader may be concerned that the coefficient of  $Educ_{ict}$  here is no longer always positive and significant. Bear in mind however that with the inclusion of the  $Educ_{ict} \times D_{ct}$  terms, the coefficient of  $Educ_{ict}$  loses its interpretation as the average direct effect of individual schooling on political participation across all country-waves.

<sup>11</sup>We have verified that our results are similar when splitting the sample into countries with below (respectively above) median values of the Polity IV democracy score. Separate regression tables for the different regions of the world are also available on request.

from Table 3. Returning to our “Non-Parametric” residuals, a next natural concern is that outlier values might be driving our findings, given the large dispersion in this variable that we have already noted. Once again, for the protest-related dependent variables in Columns 1-5, the statistical significance of the negative  $\beta_2$  coefficient is largely similar even after we drop both the upper and lower tail 5% of  $IncResid_{ict}$  values for either the whole sample or within each country-survey wave (see Appendix Tables 6 and 7 respectively).

We implemented a further, more stringent test to ascertain that the income residual is indeed capturing economically meaningful information. We did this through a permutation test, in which we randomly reallocated the  $IncResid_{ict}$  values across individuals within each country-survey wave while keeping all other personal characteristics unaltered, and then re-ran the regression specifications from Columns 1-5 of Table 3. This was done for 500 independent sets of permutations of the income residual, to examine how frequently the coefficient on  $Educ_{ict} \times IncResid_{ict}$  turned up negative and statistically significant. If significant results are common, this would be grounds for concern that our results were ascribable to mere chance. It turns out that for each of the protest variables (“Demonstrate”, “Occupy”, “Boycott”, “Strike”, and their first principal component), only a small share – between 1.8% and 3.4% – of the 500 regressions based on permuted income residuals yielded a  $\beta_2$  coefficient that was negative and significant at the 5% level. Of note, even when the  $\beta_2$  coefficient from a set of permuted residuals was significant, it was always smaller in magnitude than the corresponding coefficient in Table 3 obtained with the true  $IncResid_{ict}$  variable. We view this as striking evidence that our income residuals do contain useful information about individual-specific conditions that affect income outcomes.

A separate set of concerns relates to how the education variable is measured in the WVS. There are natural difficulties associated with mapping levels of educational attainment across different countries onto a single 1-8 categorical scale. While any such measurement error should in principle attenuate the estimated effects of education and its interaction with the income residual, we have nevertheless undertaken a few further exercises. First, in several countries such as Germany and Japan, technical and vocational education is often regarded on par with secondary education of a university-preparatory nature, whereas the WVS coding implicitly ranks the latter above the former. We thus experimented with a shorter coding of the education variable based on a 1-6 scale, in which both forms of secondary education were combined into the same coding.<sup>12</sup> We then re-computed the income residuals and re-ran our analysis using this recoded education variable (see Appendix Table 8). Second, in our regressions on the determinants of political participation, we have been treating the 1-8 education scale as essentially

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<sup>12</sup>Specifically, the recoded categories are: 1 = Inadequately completed elementary; 2 = Completed elementary; 3 = Incomplete secondary (either technical/vocational or university-preparatory); 4 = Completed secondary (either technical/vocational or university-preparatory); 5 = Some university without degree or lower-level tertiary certificate; 6 = University with degree or upper-level tertiary certificate.

a linear variable. However, this may not provide an accurate reflection of the actual resources that are invested in acquiring that education. We have thus also looked at how robust our results are when replacing the education variable with the age at which one completed (or expected to complete) one’s education, this being question X023 in the WVS (see Appendix Table 9).<sup>13</sup> Finally, we also individually cross-checked the education variable against the country-specific descriptions of education levels where available (question X025CS). We dropped those countries for which there were discrepancies, before repeating our analysis using “Non-Parametric” residuals computed off the smaller country sample (see Appendix Table 10).<sup>14</sup> Reassuringly, our overall conclusion on the negative interaction effect involving individual education and the income residual for the protest modes of political participation is never overturned.

As a final robustness check, we ask whether our results continue to hold when controlling more extensively for the role of age and demography, this being motivated by earlier work that has identified a correlation between “youth bulges” and political violence (Urdal 2006). Note that our regressions so far have already included age group dummies in the respondent controls. Nevertheless, one could be concerned that the role of age needs to be considered in tandem with income under-performance, namely that younger individuals who experience relatively poor income outcomes could be more inclined to protest. Table 4 explores this alternative thesis, by further adding the interactions between each of the age group dummies and  $IncResid_{ict}$  to the right-hand side of the Table 3 specifications. The results confirm that what matters more is individual education as opposed to age *per se*: The  $Educ_{ict} \times IncResid_{ict}$  interactions remain negative and significant for the protest modes of participation (Columns 1-5), except in Column 2 for “Occupy” where it is marginally insignificant. Conversely, no consistent pattern is detected for the interactions involving the younger age categories. Since the 15-24 age cohort is the omitted category, the coefficient of  $IncResid_{ict}$  is itself the average effect of the income residual for this youngest age group. We also examine whether the interaction effect for the age 25-34 cohort (summing across the coefficients of  $IncResid_{ict}$  and  $Age\ 25-34 \times IncResid_{ict}$ ) is different from zero; the p-value from this test is reported in a separate row in Table 4. For both of these youthful cohorts, we do not find significant evidence that a more negative income residual is associated with a greater propensity toward protest, except when “Demonstrate” is the dependent variable (Column 1). Although the “youth revolt” story might be useful for understanding specific episodes of political protest, the role of individual education and income under-performance is clearly still relevant in the broader cross-country survey data.

**[TABLE 4 HERE]**

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<sup>13</sup>Note that this issue is not a concern for the first step of our empirical strategy, since the education scale is not assumed to be linear when the “Non-Parametric” income residuals are generated. We thus only replace the education variable with the age of education completion in the second step where we seek to explain patterns of political participation.

<sup>14</sup>The countries/territories dropped were: Algeria, Bangladesh, Brazil, China, Iraq, Israel, Japan, Kyrgyzstan, Moldova, Netherlands, New Zealand, Singapore, South Korea, Switzerland, Taiwan, Turkey, Uganda, USA, and Zimbabwe.

In sum, the evidence points to a significant negative interaction effect between individual schooling and relative income outcomes on the propensity toward political protest: Individuals with a higher level of education are more likely to translate their human capital into political protest when their income falls short of what one would expect from their characteristics.

### 3.3 Interpretation

What mechanisms can explain this key empirical regularity? Our findings are open to a number of potential interpretations, three of which we now turn to discuss at more length.

A first interpretation draws on the “resource” view of political participation. The key idea here is that effort-intensive forms of political activity are a competing use of productive human capital. We develop this idea formally in the Model Appendix, but the core intuition is straightforward to convey. We start from the premise that education acts to expand an individual’s capacity to engage in all tasks.<sup>15</sup> In other words, schooling raises the effective units of labor that the individual possesses, which she subsequently decides how to allocate between production and political activities. In earlier work in Campante and Chor (2012a), we developed a model along such lines. There, we showed that an economic environment where the returns to using human capital in market production are low, for example because of a scarcity of factors of production that are complementary to human capital, would naturally incline individuals toward devoting more of their human capital to political participation. This response should moreover be stronger for those with more education, given the larger amount of effective labor that they have at their disposal.

We can extend this basic logic, as we do in our Model Appendix, to show that the same insights are true when the low returns to labor are specific to the individual, as opposed to being features of the aggregate economic environment. For example, an individual who suffers a negative shock to her productivity in the workplace would face the same incentive to shift human capital toward political participation instead. To the extent that the income residual serves as a proxy for these productivity shocks, the mechanism just described would match the patterns which we have uncovered in the data. Of note, this logic provides a possible rationalization for why the key interaction effects are present for protest modes of participation, but not for “softer”, more civic forms such as signing a petition or discussing politics. In principle, the tradeoff regarding the use of one’s human capital should be especially acute for protest activities, which are more demanding of one’s time and effort, so that the propensity toward political protest would be more sensitive to the productivity shocks that one experiences.

A second complementary interpretation of our findings centers around “disaffection”, or what we

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<sup>15</sup>This harks back to a point raised by Frey (1971) to address the apparent paradox on why high-income individuals (who presumably should have a high opportunity cost of time) nevertheless exhibit higher levels of political participation: Such individuals are typically also more productive in their political engagement.

term a “grievance” effect. As earlier discussed, individuals could develop a sense of personal grievance if they under-perform in the labor market relative to what they could have expected to earn given their personal characteristics, and this could well have ramifications for political engagement. If it is also the case that individuals with more schooling are more prone to react to poor income outcomes, and that such grievances are disproportionately manifest through effort-intensive modes of participation (namely, protest), this could then account for the interaction effects which we have documented. This interpretation is in fact quite natural, in light of the fact that grievance motives have received a fair bit of attention in the literature on political participation (e.g., Verba et al. 1987; Opp 1988).

That said, we can attempt to assess the relevance of the grievance effect for our results by going back to the data. The WVS contains several questions which gauge each respondent’s degree of satisfaction with the political establishment in their country, thus providing us with an opportunity to control directly for potential grievance motives in our regressions. Specifically, respondents were asked to rate the degree of confidence that they had in their government (question E079). We recoded the answers to generate the variable  $ConfidenceGovt_{ict}$  along the following scale: “None at all” (response=0), “Not very much” (=1), “Quite a lot” (=2), and “A great deal” (=3). Low scores in  $ConfidenceGovt_{ict}$  would arguably correlate with a greater sense of unhappiness with the performance of the incumbent.

Table 5 displays the results from re-running the specifications from Table 3, after including this measure and its interaction with individual schooling as further right-hand side variables. The former would account for the direct effect of the grievance motive on individual political participation, while the interaction term addresses the possibility that this grievance effect may be heterogeneous across different levels of education. The point estimates obtained for this latter interaction coefficient do suggest that more educated individuals who express less confidence in their government appear more likely to engage in political protest, but the statistical significance of these effects is generally weak. At the same time, our core results for the negative  $Educ_{ict} \times IncResid_{ict}$  interaction are largely unaffected, relative to what we had seen in Table 3.<sup>16</sup> In sum, the above exploration suggests that while there may well be a role for grievances, these do not appear to fully account for the connection between schooling, economic under-performance, and political protest which we consistently continue to find in the data.

**[TABLE 5 HERE]**

A third potential interpretation is that our findings may be the result of self-selection, rather than

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<sup>16</sup>The WVS contains another survey question (E125) which can be viewed as an alternative proxy for political grievance. This asks respondents to rate their satisfaction “with the way the people now in national office are handling the country’s affairs”. From this, we coded a variable  $SatisfactionGovt_{ict}$  as follows: “Very dissatisfied” (=0), “Fairly dissatisfied” (=1), “Fairly satisfied” (=2), and “Very satisfied” (=3). The key disadvantage of this variable is that it is available for fewer countries, and we lose about a quarter of our observations. Similarly, we can proxy more specifically for “economic grievance” by using the responses provided to WVS question C006, where surveyees were asked to rate on a scale of one to ten how satisfied they were with the financial situation of their household. The results obtained with these alternative measures are nevertheless very similar to that in Table 5 (available on request).

necessarily reflecting a causal link. Specifically, some individuals might by their nature be more inclined toward protest activities, and this would in turn affect their decisions on human capital acquisition as well as their subsequent labor market outcomes. For example, such individuals could be led both to acquire more education – if for example universities are seen as hotbeds of activism – and to fare worse than their peers in their income outcomes – since they might self-select into careers that are aligned with their political interests, even if these do not deliver the highest labor market returns.

A natural approach to explore the relevance of this story would be to try to control directly for individual attributes that are plausibly correlated with any such inherent proclivity toward protest. Along these lines, it would clearly be useful to control for the intensity of one’s ideological inclinations. The WVS contains several questions that help in this regard. Specifically, WVS respondents are asked about their political positioning, to identify where they stand on a one to ten left- to right-wing ideological scale (question E033). We use this to create an extremism variable,  $PolExtremism_{ict}$ , which is computed as the absolute value of the difference between one’s self-reported political position and 5.5 (the median of the scale). We allow for the effects of political extremism to differ between left- and right-wing respondents, by multiplying  $PolExtremism_{ict}$  with dummy variables for whether one’s self-reported ideology lies between 1 and 5 or between 6 and 10 respectively. In addition, we make use of information on religiosity (question F034). We code this variable,  $Religiosity_{ict}$ , as follows: “An atheist” (response=0), “Not a religious person” (=1), and “A religious person” (=2).

Table 6 shows that our results are not affected when we augment the specifications from Table 3 with these controls for political extremism and religiosity, together with their respective interactions with schooling. Thus, even after controlling as best we can for personal characteristics that might be correlated with an intrinsic propensity to engage in protests, the interaction between individual schooling and the income residual remains a significant explanatory variable for patterns of political protest. We have also examined what happens when we include the political extremism and religiosity variables as additional explanatory variables for one’s income decile in the first step of our empirical strategy, since these traits may well affect a person’s income outcomes following the logic of the self-selection story. Reassuringly, the negative and significant effects of  $Educ_{ict} \times IncResid_{ict}$  on political protest in the Table 6 specifications are not affected when using this alternatively-generated income residual variable (see Appendix Table 11). While the limitations of our empirical setting prevent us from ruling out other possible non-causal stories, these results suggest that the relationships we have found are unlikely to be due solely to the self-selecting behavior of ideologically-extreme individuals.

**[TABLE 6 HERE]**

To sum up our presentation of the survey-based evidence, we should be clear in acknowledging that

all three interpretations we have discussed are not mutually exclusive, as they could all very well be at play in reality. What these additional exercises do suggest though is that grievance motives and the self-selection story alone are unlikely to be the sole explanations behind the empirical patterns of political protest which we have uncovered.

## 4 Schooling, Political Turnover, and the Economy

We turn next to ask whether the effects which we have found in the individual-level data have aggregate implications, at the country level. If the combination of high schooling levels and weak economic prospects is associated with a higher frequency and intensity of protest activity, one might by extension expect that this combination of conditions, if present at the country level, would also negatively impact the stability of the incumbent government.

### 4.1 Country Data

To explore the above question, we use turnover data on country leaders compiled from Worldstatesmen.org, an encyclopedia that provides detailed chronologies of heads of state and heads of government around the world. As a source of information, Worldstatesmen.org is extremely comprehensive and up-to-date, with political changes in real-time typically being updated within a week. In addition, political changes that are of a transitional nature (such as an acting president) are all recorded with the official dates of the delegation of power. We extend this data, originally compiled by Campante et al. (2009), to cover the 1976-2010 period.<sup>17</sup> To guard against possible concerns over the open-source nature of the website, we have compared the records in WorldStatesmen.org against Beck et al.'s (2001) Database of Political Institutions (DPI), as a cross-check for the years in which political transitions occurred.

Our primary measure of incumbent turnover is the number of times there was a change in country chief executive during a given five-year window, say 2001-2005. (We adopt this as the unit of time in the analysis, since the education data, as described below, is only available at five-year intervals.) This can equivalently be viewed as a measure of the frequency of changes in the executive. For corroborating evidence, we also consider as an alternative a binary variable coded equal to 1 if any change in country chief executive took place during each five-year window. (Note that our total sample contains seven such five-year periods, from 1976-1980 to 2006-2010.)

We use data on country-level schooling attainment from the latest version of the Barro and Lee (2010) dataset. This provides information at five-year intervals, including a breakdown by age cohorts. To capture the prevalence of higher levels of schooling in the population, we use the average years of

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<sup>17</sup>Although it is possible to extend the turnover data further back in time, we opted not to do this given the large number of countries that had yet to gain independence prior to 1975.

secondary education in the population aged 25 and above.<sup>18</sup> We will also look at measures of primary and tertiary schooling (analogously defined) for comparison. We prefer to use variables that apply to those aged 25 and above since these should better reflect final schooling outcomes after any investments in secondary and tertiary education have been undertaken.

The remaining variables are from standard sources of country data. We take the total population figures and real GDP per capita from the Penn World Tables (Version 7.0), while we rely on the Barro-Lee dataset for the breakdown of population shares by age cohort. Summary statistics for the cross-country variables are reported in Table 7.

[TABLE 7 HERE]

## 4.2 Determinants of Political Turnover

Building on our findings with the individual-level data, we hypothesize an analogous interaction effect: Incumbents will be more threatened when a highly-educated population is faced with economic conditions in which their human capital is not rewarded well in production activities.

In order to connect to our earlier analysis, we start off by proxying for economic performance using the median income residual variable from the WVS data. Specifically, we take the income residual measures from before, and compute the median value of  $IncResid_{ict}$  across individuals with an education score of at least 5 (some secondary education of a pre-university nature), separately for each available country-survey wave. A country with a very negative median income residual can thus be viewed as one where under-performance in income outcomes is widespread among the relatively educated.<sup>19</sup> In practice, however, we are unable to use the within-country variation in this median residual measure, as the existing country coverage of the WVS across different survey waves would leave us with a short and unbalanced panel. We therefore focus on the cross-country variation in this measure, by using only the median income residual calculated from the most recent WVS wave for each country.<sup>20</sup>

This yields the following specification:

$$Turnover_{ct} = \gamma_0 Turnover_{c,t-1} + \gamma_1 YrsSec_{c,t-1} + \gamma_2 YrsSec_{c,t-1} \times MedIncResid_c + \gamma_3 MedIncResid_c + D_t + \epsilon_{ct}, \quad (5)$$

where  $Turnover$ ,  $YrsSec$  and  $MedIncResid$  denote respectively the measures of incumbent turnover (the number of executive changes), education (average years of secondary schooling), and the median

<sup>18</sup>We found similar results using the share of the population aged 25 and above with at least some secondary schooling (available upon request).

<sup>19</sup>We use the median rather than the mean value, since the distribution of the income residuals within each country-wave often displays a fair amount of skew.

<sup>20</sup>We merge this with the data on political turnover, by associating the median residual from Wave 2 (1989-1993), Wave 3 (1994-1999), Wave 4 (1999-2004), and Wave 5 (2004-2008) to the 1991-1995, 1996-2000, 2001-2005, and 2006-2010 windows respectively.

income residual. Note that the subscript  $c$  stands for country, whereas  $t$  now refers to each five-year window in our data. In practice, we examine whether initial education (say, in 2000) helps to explain incumbent turnover in the subsequent five-year window (namely, in 2001-2005). We further allow for persistence in political instability, by including a lagged  $Turnover_{c,t-1}$  term on the right-hand side of the estimating equation. The  $D_t$  are a set of time dummies, to control for the possible effects of broad waves of political turbulence, such as following the fall of the Berlin Wall or the recent Arab World revolts, that might raise the probability of incumbent turnover across many countries during a given period in time. Given that our dependent variable is an integer count, we estimate (5) using a Poisson regression model. This yields a point estimate of  $\hat{\gamma}_2 = -1.070$ , with a robust standard error of 0.399, which is significant at the 1% level. This provides preliminary but nevertheless suggestive evidence that countries where relatively educated individuals faced favorable economic circumstances were also less likely to experience threats to the stability of the government.<sup>21</sup>

However, the exercise based on (5) has inherent limitations. The absence of time variation means that we cannot make inferences related to within-country developments over time, for example whether a given country's investment in schooling, when combined with a period of poor economic performance, is more likely to augur a period of increased incumbent instability. Put differently, it would be useful for us to be able to include country fixed effects, in order to control for the effects of unobserved country characteristics that do not change over time.

This motivates our use of real GDP per capita as an alternative measure of prevailing economic conditions, under the assumption that periods where income per capita is high would be associated with a relative abundance of well-paying labor market opportunities. While this is admittedly a broader proxy, it nevertheless allows us to work with a more balanced data panel. It also has the important advantage of allowing for a more direct comparison with the vast literature cited in our Introduction that has studied the joint evolution of GDP per capita, political institutions, and education, in the context of the modernization hypothesis. We thus run the following analogous specification:

$$\begin{aligned}
 Turnover_{ct} = & \tilde{\gamma}_0 Turnover_{c,t-1} + \tilde{\gamma}_1 YrsSec_{c,t-1} + \tilde{\gamma}_2 YrsSec_{c,t-1} \times GDPpc_{c,t-1} + \tilde{\gamma}_3 GDPpc_{c,t-1} \\
 & + \tilde{\gamma}_4 V_{c,t-1} + D_c + D_t + \tilde{\epsilon}_{ct},
 \end{aligned} \tag{6}$$

where the political turnover and education variables are defined as before, and  $GDPpc$  refers to log real GDP per capita;  $V$  is a vector of additional country controls. To be consistent with the timing of  $Educ$ , we use lagged measures for  $GDPpc$  and  $V$  (say, an average over 1996-2000) to explain turnover in

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<sup>21</sup>For completeness, the other coefficient estimates and their associated robust standard errors (in parentheses) are as follows:  $\hat{\gamma}_0 = 0.131$  (0.102),  $\hat{\gamma}_1 = 0.003$  (0.094), and  $\hat{\gamma}_3 = 4.143$  (1.243). Of these, only  $\hat{\gamma}_3$  is statistically significant. It is also useful to note that the significance of the  $\gamma_2$  coefficient is robust to controlling further for log population and its interaction with country education, as well as for the interaction between country education and time fixed effects, these being controls that we will use later in Tables 8 and 9.

the subsequent period (namely, 2001-2005); the results are very similar if we use the contemporaneous five-year averaged values as controls instead. The regression now includes both country ( $D_c$ ) and time ( $D_t$ ) fixed effects. We will moreover cluster the standard errors to allow for correlated shocks in the error term at the country level.

For the sake of robustness, we have also experimented with replacing GDP per capita in the specification in (6) with a real “GDP per capita residual”. This is motivated by an analogy with our earlier individual-level analysis, and represents an attempt to extract a country-level productivity shock. That said, it is not obvious from a conceptual point of view how individual-level productivity shocks would aggregate up to the country level, and we also have relatively little guidance as to what observed country variables one should control for. In practice, we computed a “GDP per capita residual” by running an OLS regression of log real GDP per capita on country measures of education (years of primary, secondary, and tertiary schooling, as described earlier) and demography (log population, and the share of the population aged 15-64).<sup>22</sup> A separate cross-country regression was run for the observations from each 5-year window in our dataset. One can loosely interpret the residual obtained from this as capturing how the economy was performing relative to what one might have expected from basic schooling and demographic characteristics. In light of the caveats we have raised though, we necessarily view the evidence from this “GDP per capita residual” as being more corroborative in nature.<sup>23</sup>

The results for our key specification in (6) using log real GDP per capita are displayed in Table 8; our conclusions are essentially unchanged when using the “GDP per capita residual” instead (see Appendix Table 12).<sup>24</sup> Column 1 reports a basic Poisson regression with our main interaction term of interest. The findings here are exactly in line with our working hypothesis: Increases in schooling are associated with a greater frequency of incumbent turnover ( $\tilde{\gamma}_1 > 0$ ), but this effect is dampened when the country’s economic performance is strong ( $\tilde{\gamma}_2 < 0$ ). In other words, when economic conditions are weak, high levels of educational attainment can be especially threatening to incumbent stability. Column 2 adds further explanatory variables on the right-hand side, in the form of log country population and its interaction with education, in order to verify that our central result on the  $\tilde{\gamma}_2$  coefficient is not driven by country scale effects. We also include education interacted with a full set of time fixed effects, to accommodate the possibility that certain periods may be characterized by a heightened response of educated individuals to political action across different countries, such as was seen during the Arab Spring. Our main results

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<sup>22</sup>The population share variable is computed from the Barro-Lee dataset as the share of the population aged 15 and above that is in the 15-64 age group.

<sup>23</sup>It is interesting to note that the median WVS income residual for the 71-country sample in regression (5) is weakly positively correlated with log real GDP per capita (0.1098), but is instead *negatively* correlated with the “GDP per capita residual” (−0.1088). This suggests that the latter residual variable may not necessarily be the better proxy of how individual-level productivity shocks aggregate up at the country level.

<sup>24</sup>The one exception is that we lose the negative interaction effect in the logit specification in Column 7 of Table 8, where the dependent variable is the dummy variable for the occurrence of a change in executive.

are clearly robust to these, with the point estimate for  $\tilde{\gamma}_2$  now larger in magnitude.

[TABLE 8 HERE]

Column 3 compares the importance of different levels of educational attainment. We use as additional explanatory variables the average years of primary and tertiary schooling (in the population aged 25 and over), together with their respective interactions with log GDP per capita. The results suggest that it is higher levels of schooling, in particular secondary schooling, that matter the most for political outcomes at the country level. It is perhaps not too surprising that we do not see an effect of primary schooling, since the skills imparted at the primary level deal more with basic numeracy and literacy, as opposed to preparing students for the labor market. We however do not see a significant effect for tertiary education also, although this could be due to the smaller cross-country variation in this specific schooling measure (see Table 7).<sup>25</sup>

We address the “youth revolution” hypothesis in Column 4, following up on our earlier discussion on the possible connection between large youth cohorts and protest movements. To do so, we control for the respective shares of the population aged between 15-24 and between 25-39 (as a share of the total population aged 15 and over), as well as their interactions with country log GDP per capita. Our main coefficient of interest – on the interaction between secondary schooling and GDP per capita ( $\tilde{\gamma}_2$ ) – is now less precisely estimated, although it is still negative and broadly consistent with the estimates from previous columns. The results do indicate that a larger age 25-39 cohort is associated with more incumbent turnover during periods when country income is lower, but there is no evidence of an analogous effect for the younger age 15-24 cohort. This is consistent with the idea that broader economic conditions matter more for the incumbent, insofar as they affect the prospects of those in the slightly older, economically active 25-39 age bracket who would arguably be more immediately concerned about their labor market prospects.

Our findings on the importance of the  $\tilde{\gamma}_2$  coefficient are restored when we apply dynamic panel techniques to estimate this specification. Although this means that we are now viewing (6) as a linear model, such techniques are still useful to consider since it has been observed that political institutions tend to persist over time, which increases the stringency of the requirements for the consistency of the fixed-effects estimator; this may matter more in our setting, given the short time dimension of our panel with just seven periods. Column 5 reports the results from the Arellano and Bond (1991) procedure, which uses lagged levels of the right-hand side variables as instruments in a first-differenced version of (6).<sup>26</sup> Column 6 then further includes time-differenced variables as additional instruments to estimate a

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<sup>25</sup>An alternative interpretation is that workers with tertiary education might be less vulnerable to bad economic shocks at the country level.

<sup>26</sup>We use one period of the lagged dependent variable in Columns 5 and 6. Both regressions pass the usual diagnostics

levels version of equation (6), which in principle yields more efficient estimates (Arellano and Bover 1995, Blundell and Bond 1998). Our key negative interaction effect between secondary schooling and country income levels is now found to be highly significant and much larger in magnitude.

Last but not least, we examine the use of our alternative, binary measure of political turnover. This is useful to consider, since our count variable may be over-stating the extent of real political instability with the mechanical count rule that we adopted. Reassuringly, we obtain results in the logit specification in Column 7 that are very consistent with those seen in the preceding columns. It is also interesting to compare the magnitudes of the effects implied for the schooling and population share variables. Consider for example the impact of a one standard deviation increase in average years of secondary schooling that is also accompanied by a one standard deviation fall in log real GDP per capita. Using the Column 7 estimates, this would imply a rise in the log odds of seeing a change in executive of  $0.662 \times 1.310 \times 1.311 = 1.137$ . This is about twice as large as the corresponding marginal effect that would be predicted for a similar one standard deviation increase in the population share of the age 25-39 cohort, which is given by:  $0.106 \times 4.198 \times 1.311 = 0.583$ .<sup>27</sup>

In sum, using a variety of estimation methods and measures, we have found that the combination of increased levels of secondary schooling and poor macroeconomic performance tends to be associated with a greater frequency of incumbent turnover. The role of schooling in this regard (as a determinant of political turnover) appears to be quantitatively larger than the corresponding role played by youth bulges, although these results need to be interpreted cautiously in light of the caveats associated with identifying causal relationships with cross-country data.

### 4.3 Determinants of Democracy

A potential limitation of our analysis based on the executive turnover measures is that our coding of these variables does not differentiate between peaceful and non-peaceful political change. Given our emphasis on protest modes of political participation, one could argue that we should be concerned primarily with instances where the removal of an incumbent was accompanied by protest movements and even conflict. We did not pursue this approach in part because it inherently entails subjective judgement calls regarding events surrounding each episode of change. For example, even peaceful changes in political leadership that are in line with constitutional procedures may be undertaken as a result of negative public opinion that threatens to spill over into demonstrations and protests.

We nevertheless offer evidence in this subsection that substantial political and institutional change

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for possible model misspecification. As reported in the tables, we reject the null hypothesis of no first-order autocorrelation in the first-differenced errors, but do not find evidence against the null of no second-order autocorrelation. We do not use the Sargan overidentification test, as the properties of this test have not been established when the standard errors are heteroskedastic. Note that we report robust standard errors in all the dynamic panel specifications in Tables 8 and 9.

<sup>27</sup>In these exercises, we use a one standard deviation change calculated from the sample of 644 observations in Column 7.

does appear to ensue from periods in which economic conditions fail to keep up with the education profile of the population. Specifically, we consider the commonly-used Polity IV democracy score (Marshall and Jaggers 2010) as a dependent variable, and ask: Does the combination of increased levels of schooling and a weak macroeconomy entail an opportunity not only for leadership change, but also for democratization? These questions are also interesting given their connection with the large body of work on the “modernization hypothesis”, in particular the idea that education is a pre-condition for democracy: They underscore the possibility that this link might be conditioned by economic circumstances.

Table 9 sheds light on this issue. We estimate the specification in (6) using instead a five-year average of the 0-10 democracy score as the dependent variable; unless otherwise stated, these are run with OLS using explanatory variables that are one-period lags. These regressions take on a similar flavor to the recent work by Acemoglu et al. (2005, 2008, 2009), which considers the role of education and income separately as determinants of democratization using a similar cross-country panel context.<sup>28</sup> While the regressions here focus on log real GDP per capita as the country measure of income, we can report that the results are very similar when using the “GDP per capita residual” instead (see Appendix Table 13). Our shortest specification in Column 1 of Table 9 shows essentially no effect of either schooling or income on democracy, consistent with the above-cited studies which tend to find no evidence for such an effect once country dummies are included in the specification.<sup>29</sup> Interestingly, however, we obtain a negative and significant interaction effect for the combination of schooling and income per capita once this is introduced into the model (Column 2). This finding survives the same set of sensitivity checks which we had imposed in Table 8, namely controlling for the role of total population, education interacted with population, and education by year fixed effects (Column 3), the roles of primary and tertiary education (Column 4), as well as the youth cohort size hypothesis (Column 5). Note in particular that it is once again secondary schooling (relative to other education grades) that appears to matter most for subsequent democratization.

**[TABLE 9 HERE]**

The final two columns explore the use of dynamic panel GMM estimation.<sup>30</sup> We first consider the Arellano-Bond procedure in Column 6, followed by the Arellano-Bover / Blundell-Bond method in Column 7.<sup>31</sup> Our key results remain robust, with in fact a much larger coefficient estimate for the

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<sup>28</sup>This builds on earlier work in this vein by Barro (1999) and Przeworski et al. (2000), among others.

<sup>29</sup>Glaeser et al. (2007) find a similar result with country fixed effects, but show that a positive effect of education on democratization is recovered if one considers less persistent measures of schooling investment, such as enrollment.

<sup>30</sup>Note that we now use two periods of the lagged dependent variable in this estimation. This is because the one-period lag model is rejected by the autocorrelation diagnostics, but the two-period lag model is not. The main finding of a negative and significant interaction between secondary schooling and log income is not sensitive to this choice of lagged models.

<sup>31</sup>The increased efficiency of the latter estimator could be important in this context. For example, Bobba and Coviello (2007) contend that the significance of the link from schooling to democracy is restored once the Arellano-Bover / Blundell-Bond procedure is applied.

negative effect of secondary schooling interacted with income. In terms of implied magnitudes, the estimates in Column 7 suggest that a one standard deviation increase in secondary schooling (1.401 years), coupled with a similar fall in income per capita (1.259 log units), would be associated with an increase in democracy score of slightly over 1 point on the 10 point scale ( $0.632 \times 1.401 \times 1.259 = 1.115$ ), or just above a quarter of a standard deviation in the regression sample.

The above evidence therefore suggests that while the cocktail of increased schooling and poor economic opportunities is potentially threatening to incumbent stability, it actually tends to be accompanied by moves toward greater democratization.

## 5 Conclusion

This paper has sought to uncover broader lessons on the link between human capital, economic outcomes, and political protest. At the individual level, we have found in the WVS data that respondents whose income under-performs that predicted on the basis of their biographical characteristics are more inclined to apply their human capital toward protest activities, such as demonstrations, strikes, and the occupation of buildings. We discussed possible interpretations for this empirical pattern, including the idea that there is a tradeoff between the productive and political uses of human capital. We also do not rule out the possible role of grievance effects or self-selecting behavior, although we would add that these alone are unlikely to fully account for our empirical findings.

We have also examined the country-level data, and found evidence that countries that have raised educational attainment, but failed to see that investment matched with commensurate rises in economic opportunities or living standards, are more likely to see political incumbents removed from office. The evidence indicates that such circumstances are often also associated with subsequent improvements in democratic institutions.

Given the nature of our analysis, we clearly do not intend our regression models to be used to provide conclusive forecasts for the outcomes in specific country cases. Notwithstanding this, we believe the evidence is informative about the economic underpinnings of political protest, and what it means for the incumbents who face it. We would moreover argue that these findings highlight the usefulness of taking a more nuanced view that moves beyond the debate on whether education determines political development or democratization, toward studying instead the conditions under which this determination or influence is more (or less) likely to hold true.

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## Model Appendix

This Appendix presents a model of individual behavior in which human capital is allocated between production and political activities, building on that in Campante and Chor (2012a). We use the model to better understand the forces that shape the tradeoffs that individuals face when making this human capital allocation decision, with a specific focus on the effects of shocks to an individual's productivity in the production sector. While the model is admittedly stylized, it nevertheless provides a framework that motivates our empirical strategy, illustrating in particular why our empirical income residual reasonably captures the role of productivity shocks at the individual level.

Consider a setting in which individuals derive utility from two activities: (i) consumption,  $c$ , which is denominated in monetary units; and (ii) political participation,  $x$ , which is denoted in units of human capital (time and effort) that the individual sets aside for this activity. The latter embodies the idea that individuals may obtain an intrinsic benefit from political participation, such as from performing a civic duty or expressing one's political views.

We denote the utility function by  $u(c, x)$ . We shall assume that  $u$  is increasing in both of its arguments ( $u_c, u_x > 0$ ), but also exhibits diminishing marginal utility (namely,  $u_{cc}, u_{xx} < 0$ ). Moreover, the level of consumption that the individual enjoys is determined entirely by her wage income, which is in turn a function of the amount of human capital that she sets aside for production activity,  $h$ . We denote this wage function by  $w(h)$ , with the natural assumptions here that this is increasing in  $h$  but (weakly) concave:  $w'(h) > 0$  and  $w''(h) \leq 0$ . (The latter assumption admits for the possibility of decreasing marginal productivity.)

As a final feature of the setup, individuals face a human capital constraint:  $h + x = H$ . Here,  $H$  denotes the individual's level of human capital or equivalently, her total effective units of labor. In this setting, education serves the role of raising  $H$ , thus increasing the total effective units of labor that the individual can subsequently allocate to both production and political activities.

Bearing in mind that  $c = w(h)$ , the individual's allocation problem over the use of her human capital can thus be written as:

$$\begin{aligned} \max_{h,x} \quad & u(w(h), x) \\ \text{s.t.} \quad & h + x = H. \end{aligned}$$

The corresponding first-order conditions are:

$$\begin{aligned} u_c(c, x)w'(h) &= \lambda, \\ u_x(c, x) &= \lambda, \end{aligned}$$

where  $\lambda$  is the associated Lagrange multiplier. Equating the two first-order conditions yields:

$$u_c(w(h), H - h)w'(h) = u_x(w(h), H - h). \tag{7}$$

Now, let us suppose further that  $u_{cx} \geq 0$ , namely that utility from consumption and political participation are weakly complementary. Moreover, it is natural to specify an appropriate set of Inada-type conditions, namely  $u_c(0, x)$  and  $u_x(c, 0) \rightarrow \infty$ , so that the marginal utility from either the first incremental unit of consumption or political participation is infinite. We will also assume that the marginal utilities  $u_c$  and  $u_x$  are finite whenever  $h$  and  $x$  are strictly positive. It is then straightforward to check that

the left-hand side of (7) is strictly decreasing in  $h$ , while the right-hand side will be strictly increasing in  $h$ . The Inada conditions moreover imply that the left-hand side of (7) tends to infinity at  $h = 0$ , while the right-hand side tends to infinity at  $h = H$ . The intermediate value theorem then guarantees that there is a unique interior solution for the optimal allocation of human capital to production activities, which we can denote by  $h^*$ ; the corresponding allocation to political activities is given simply by  $x^* = H - h^*$ .

**Role of Education:** We first show that an increase in educational attainment that raises  $H$  will lead the individual in question to allocate more human capital to both production and political activities. This can be seen by totally differentiating (7), which yields:

$$\begin{aligned} \frac{dh^*}{dH} &= \frac{\frac{u_{cx}}{u_c} - \frac{u_{xx}}{u_x}}{\frac{u_{cx}}{u_c} - \frac{u_{xx}}{u_x} + \left(\frac{u_{cx}}{u_x} - \frac{u_{cc}}{u_c}\right)w' - \frac{w''}{w'}}, \text{ and} \\ \frac{dx^*}{dH} &= \frac{\left(\frac{u_{cx}}{u_x} - \frac{u_{cc}}{u_c}\right)w' - \frac{w''}{w'}}{\frac{u_{cx}}{u_c} - \frac{u_{xx}}{u_x} + \left(\frac{u_{cx}}{u_x} - \frac{u_{cc}}{u_c}\right)w' - \frac{w''}{w'}}. \end{aligned}$$

In particular, the second equation for  $\frac{dx^*}{dH}$  comes from using the expression for  $\frac{dh^*}{dH}$ , as well as the fact that  $\frac{dx^*}{dH} + \frac{dh^*}{dH} = 1$ . One can check that the assumptions regarding the signs of the derivatives of  $u(c, x)$  and  $w(h)$  indeed ensure that the numerators of both  $\frac{dx^*}{dH}$  and  $\frac{dh^*}{dH}$  are strictly positive, while their common denominator is also strictly positive. Since  $\frac{dx^*}{dH} + \frac{dh^*}{dH} = 1$ , it follows that  $\frac{dx^*}{dH}, \frac{dh^*}{dH} \in (0, 1)$ , so that the allocation of human capital to both sets of activities increases, as claimed.

**Introducing Productivity Shocks:** We now incorporate a role for individual shocks to labor market productivity. Suppose in particular that the wage function for individual  $i$  is given by  $w_i(h) = \phi_i \tilde{w}(h)$ , where  $\phi_i = \phi_i^P + \phi_i^U$ . Here,  $\phi_i$  is an individual-specific productivity level, with  $\phi_i^P$  denoting the component of productivity that is predictable given an individual's biographical characteristics (and which in principle can be controlled for in a regression model), while  $\phi_i^U$  refers to the unpredictable component which we can think of as an idiosyncratic productivity shock. It will be helpful to assume that the expected value of  $\phi_i^U$  is 0. Note that we have introduced the subscript 'i' as a reminder that these productivity terms are specific to each individual. (To be consistent, we should attach this subscript too to  $x$ ,  $h$ , and  $H$ , but we suppress this to keep the notation cleaner.)

We now illustrate that our model gives rise to a key interaction effect between individual schooling and labor market productivity on the allocation of human capital. More formally, this involves understanding what determines the sign of  $\frac{d^2x^*}{dHd\phi_i}$ . It will be helpful here to adopt a specific functional form for both  $u(c, x)$  and  $w(h)$  to shed light on these effects. (In principle, one can also proceed by making assumptions about the third-derivatives of the utility and wage functions, but such assumptions are in general less intuitive.) We consider in particular:

$$\begin{aligned} u(c, x) &= c^{\rho_c} + x^{\rho_x}, \quad \rho_c, \rho_x \in (0, 1), \text{ and} \\ \tilde{w}(h) &= h^\beta, \quad \beta \in (0, 1). \end{aligned}$$

It is straightforward to check that these expressions satisfy the first- and second-derivative assumptions that we have spelt out earlier. Note moreover that we do allow the  $\rho_c$ ,  $\rho_x$  and  $\beta$  parameters to vary with individuals, even though we do not explicitly write down the subscript 'i' here to save on notation. For example, decreasing returns to production effort may kick in faster for some individuals than others, which would correspond to differences across individuals in their  $\beta$  parameters.

With the above, the expression for  $\frac{dx^*}{dH}$  now simplifies to:

$$\frac{dx^*}{dH} = \frac{1 - \beta\rho_c}{(1 - \rho_x)\frac{h^*}{x^*} + 1 - \beta\rho_c}.$$

The sign of  $\frac{d^2x^*}{dHd\phi_i}$  is thus determined by how  $h^*/x^*$  responds to  $\phi_i$ . To pin this down, we substitute the expressions for  $u(c, x)$  and  $w(h)$  into (7) to obtain:

$$(h^*)^{1-\beta\rho_c} = \frac{\rho_c}{\rho_x} \phi_i^{\rho_c} \beta (x^*)^{1-\rho_x}.$$

Total differentiation then implies that:

$$\frac{dh^*}{d\phi_i} = \frac{\frac{\rho_c}{\alpha}}{\frac{1-\rho_x}{x^*} + \frac{1-\beta\rho_c}{h^*}} > 0,$$

where we have also made use of the fact that  $\frac{dx^*}{d\phi_i} = -\frac{dh^*}{d\phi_i}$ , since  $x^* + h^* = H$ . It thus follows that  $h^*/x^*$  increases when  $\phi_i$  increases, so that  $\frac{d^2x^*}{dHd\phi_i} < 0$ . In words, a higher  $\phi_i$  corresponds to a positive improvement to productivity that raises the marginal return to devoting human capital to production activities. This correspondingly lowers the incentives to devote a marginal unit of additional human capital to political participation,  $x^*$ . Note that since  $\phi_i = \phi_i^P + \phi_i^U$ , both the predictable and unpredictable components of productivity have the same comparative static implications. (We assume here that  $\phi_i^P > |\phi_i^U|$ , so that the  $\phi_i > 0$ . In words, the predictable component of productivity is large relative to the productivity shocks, so that the overall productivity term remains positive.)

**Mapping to the Empirics:** In the first step of our empirical strategy (Section 2.2), we estimated an ordered logit regression of the determinants of an individual's income decile. We now show why the income residual measure that we constructed from this regression would be positively correlated with the unpredictable productivity shocks in the context of the above framework. In the above model, an individual's (log) income level is given by:  $\log w(h^*) = \log \phi_i + \beta \log h^*$ . Although we do not have a closed-form expression for  $h^*$ , we can nevertheless write it as a general function of the deep parameters of the model, namely as  $h^*(\phi_i^U, \phi_i^P, \rho_c, \rho_x, \beta, H)$ .

We now take a first-order Taylor approximation of  $\log w(h^*)$  around  $\phi_i^U = 0$  (the mean value of the unpredictable productivity shocks):

$$\log w(h^*) \approx \log(\phi_i^P) + \beta \log h^*(0, \phi_i^P, \rho_c, \rho_x, \beta, H) + \phi_i^U \beta \frac{h_1^*(0, \phi_i^P, \rho_c, \rho_x, \beta, H)}{h^*(0, \phi_i^P, \rho_c, \rho_x, \beta, H)}. \quad (8)$$

Here,  $h_1^*$  refers to the partial derivative of  $h^*$  with respect to its first argument,  $\phi_i^U$ .

To map this to the data, recall that we only observe the income decile to which each individual belongs, and not  $\log w(h^*)$  itself. Denote the (log) income values that determine the decile cutoffs in ascending order by:  $\log w^{(0)}, \log w^{(1)}, \dots, \log w^{(9)}, \log w^{(10)}$ , where  $\log w^{(0)}$  is defined as  $-\infty$  and  $\log w^{(10)}$  is defined as  $+\infty$ . Then, the probability that individual  $i$  will belong to income decile  $k \in \{1, \dots, 10\}$  is given by:

$$Pr \left[ \left( \log w^{(k-1)} - \log(\phi_i^O) - \beta \log h^* \right) < \phi_i^U \frac{\beta h_1^*}{h^*} < \left( \log w^{(k)} - \log(\phi_i^O) - \beta \log h^* \right) \right]$$

where we have suppressed the arguments of  $h^*$  and  $h_1^*$  to keep the expressions compact. The above would fit within the framework of an ordered logit regression under the following two conditions. First,

$\phi_i^U$  is drawn from a logistic distribution centered around 0. (The presence of the  $\frac{\beta h_1^*}{h^*}$  term means though that we should allow for heteroskedasticity in the individual error terms. Short of modeling this heteroskedasticity more explicitly, we estimate the regression with robust standard errors.) Second, the terms  $\log(\phi_i^P) + \beta \log h^*$  can be adequately controlled for by a linear function of respondent biographical characteristics and country-survey wave fixed effects which are the explanatory variables in the ordered logit regression.

Accordingly, we evaluate the income residual for each individual  $i$  as:

$$y_i - \sum_{k=1}^{10} k \times Pr \left[ \left( \log w^{(k-1)} - \log(\phi_i^P) - \beta \log h^* \right) < Z < \left( \log w^{(k)} - \log(\phi_i^P) - \beta \log h^* \right) \right]$$

where  $y_i$  is the reported income decile of the individual, and the second term above is her predicted income decile. Note that the probabilities are evaluated treating  $Z$  as a random variable from a logistic distribution with mean 0.

The following two thought exercises are now useful for clarifying why the income residual measure constructed as such is useful for capturing the role of the unpredictable productivity shocks,  $\phi_i^U$ . First, consider the case of two individuals who have identical biographical characteristics, but different calculated values for the income residual. These differences must be driven by differing observed values of  $y_i$ , since the expected income decile that we predict would be the same for both individuals. In particular, the individual with the higher reported  $y_i$  would have a higher calculated income residual. Note however that  $y_i$  is (weakly) increasing in  $i$ 's actual (log) income level, which in our framework is given by  $\log w(h^*)$ . But from (8), we know that differences in  $\log w(h^*)$  between the two individuals must be driven by differences in  $\phi_i^U$ , since both individuals are otherwise identical. It follows that the individual with the higher calculated income residual must have experienced a higher  $\phi_i^U$ . (Recall here in particular that  $h_1^* > 0$ , so that  $\log w(h^*)$  is in fact increasing in  $\phi_i^U$  from the first-order approximation.) Conditional on respondent biographical traits, the income residual measure is thus positively correlated with the labor productivity shock.

Second, consider a situation where two individuals have the same  $y_i$ , but differ in their biographical characteristics. It follows (except in a knife-edge situation) that the two individuals must have experienced different  $\phi_i^U$ 's. In this case, information on the individuals' income decile itself is not useful for distinguishing which of the two actually experienced the higher productivity shock. The income residual measure would instead be more useful for this purpose: One can infer that the individual with the larger income residual would have experienced the larger productivity shock, given that one would otherwise have expected her to fall in a lower income decile when basing this prediction solely on the basis of observed biographical traits.

# Data Appendix

## A. Individual-level data

**World Values Survey (WVS):** From the WVS website. The measures of political participation used are described in the main text (Section 2.1). The education and income decile variables are respectively from questions X025 and X047. The eight categorical responses for the education variable are as follows: 1 = Inadequately completed elementary education; 2 = Completed elementary education; 3 = Incomplete secondary education: technical/vocational type; 4 = Complete secondary education: technical/vocational type; 5 = Incomplete secondary education: university-preparatory type; 6 = Complete secondary education: university-preparatory type; 7 = Some university without degree or lower-level tertiary certificate; 8 = University with degree or upper-level tertiary certificate.

The respondent characteristics used include dummies for: gender (X001), age groups (generated from question X003), and number of children (X011). The marital status dummies are based on question X007, with dummies for each of the following categories: married; living together as married; divorced; separated; widowed; single/never married; living apart but steady relation; unreported/missing.

The 84 countries/territories in our sample are listed below. We indicate in parentheses the survey waves for which data was available for each country. Albania (3-4); Algeria (4); Andorra (5); Argentina (3-4); Armenia (3); Australia (3,5); Azerbaijan (3); Bangladesh (3-4); Belarus (3); Bosnia and Herzegovina (3-4); Brazil (2,3,5); Bulgaria (3,5); Burkina Faso (5); Canada (4-5); Chile (3-5); China (3-5); Colombia (3); Cyprus (5); Czech Republic (3); Dominican Republic (3); Egypt (4-5); El Salvador (3); Estonia (3); Ethiopia (5); Finland (3,5); France (5); Georgia (3,5); Germany (3,5); Ghana (5); Great Britain (3,5); Guatemala (5); Hong Kong (5); India (2-5); Indonesia (4-5); Iran (4-5); Iraq (4-5); Italy (5); Japan (4-5); Jordan (4); Kyrgyzstan (4); Latvia (3); Lithuania (3); Macedonia (3-4); Malaysia (5); Mali (5); Mexico (3-5); Moldova (3-5); Morocco (4-5); Netherlands (5); New Zealand (3,5); Nigeria (2-4); Norway (3,5); Pakistan (3-4); Peru (3-5); Philippines (4); Poland (3,5); Puerto Rico (3-4); Romania (3,5); Russia (3,5); Rwanda (5); Saudi Arabia (4); Serbia and Montenegro (3-4); Serbia (5); Singapore (4); Slovakia (3); Slovenia (5); South Africa (2-5); South Korea (2-5); Spain (3-5); Sweden (3-5); Switzerland (2,3,5); Taiwan (3,5); Tanzania (4); Thailand (5); Trinidad and Tobago (5); Turkey (2-5); Uganda (4); Ukraine (3,5); United States (3-4); Uruguay (3,5); Venezuela (3-4); Vietnam (4-5); Zambia (5); Zimbabwe (4). Note that we drop wave 5 of the Netherlands from our regression sample when “strike” is the dependent variable. This is because there are only two surveyees who responded to this question, both of whom gave the same response. The country-survey wave dummy therefore perfectly predicts their responses.

## B. Country-level variables

**Incumbent Turnover:** Following Campante et al. (2009), coded from Worldstatesmen.org. While political systems and titles differ from country to country, we took the chief executive to be the *de facto* head of government. In practice, this would most commonly be the president for countries with a presidential system of government, and the prime minister for countries that adopt a parliamentary system. There are however some exceptions. First, for most communist states, we coded the secretary-general of the communist party as the chief executive. Second, there are a handful of countries for which

executive power clearly lay in the hands of a leader who never assumed the associated formal title, such as Deng in China, Noriega in Panama, and Qaddafi in Libya. In such instances, we treated these rulers as the chief executive given their *de facto* hold on power. Third, in cases of a military-controlled state, we coded the military junta as the chief executive. Finally, we counted the emergence of interim and acting heads of government as leadership changes, even if the original chief executive returned to power thereafter. This mechanical rule views the need for such interim arrangements as a potential indication of instability relative to the status quo.

**Schooling:** From Barro and Lee (2010). Primary, secondary, and tertiary years of schooling for the population aged 25 and above are used.

**GDP per capita:** From the Penn World Tables, version 7.0. The real GDP per capita chain series (constant purchasing power parity prices) is used. For China, the “CH2” observations are used; these adjust the PPP price levels to account for the over-sampling of urban prices in the raw data.

**Population:** Total population is from the Penn World Tables, version 7.0. The population shares for the age 15-24 and age 25-39 cohorts is computed from Barro and Lee (2010), which is in turn based on the UN population database. These are calculated as shares of the population aged 15 and over.

**Democracy:** From the Polity IV dataset (Marshall and Jaggers 2010). We use the democracy score, which is on a scale of 0 to 10. Observations scored as  $-66$ ,  $-77$ , and  $-88$  in the original data (to reflect periods of foreign interruption or domestic transition) are dropped.

**Table 1**  
**Summary Statistics: World Values Survey**

	25th	Median	75th	Mean	Std Dev
<b><u>Measures of political participaton</u></b>					
Demonstration (Range: 0 to 2)	0	1	1	0.665	0.735
Occupy (Range: 0 to 2)	0	0	0	0.151	0.409
Boycott (Range: 0 to 2)	0	0	1	0.489	0.665
Strike (Range: 0 to 2)	0	0	0	0.280	0.551
First Principal Component, Protest modes	-1.240	-0.534	0.853	0.000	1.492
Petition (Range: 0 to 2)	0	1	2	0.884	0.809
Discuss Politics (Range: 0 to 2)	0	1	1	0.840	0.670
Vote (Range: 0 or 1)	0	1	1	0.745	0.436
<b><u>Individual-level controls</u></b>					
Age	28	38	51	40.69	15.71
Gender (0=Male; 1=Female)	0	1	1	0.51	0.50
Number of children	0	2	3	1.99	1.82
Education (1=Lowest; 8=Highest)	2	4	6	4.42	2.33
Income decile (1=Lowest; 10=Highest)	3	4	6	4.54	2.39
Income Residual ("Pooled")	-1.418	-0.160	1.281	-0.016	2.030
Income Residual ("Non-Parametric")	-1.332	-0.141	1.223	-0.015	1.967
Income Residual ("Country-Wave")	-1.265	-0.112	1.206	0.003	1.923
Confidence in Government (Range: 0 to 3)	1	1	2	1.43	0.94
Satisfaction with Government (Range: 0 to 3)	1	1	2	1.27	0.87
Financial Satisfaction (Range: 0 to 10)	3	5	8	5.48	2.63
Political self-positioning (1=Left; 10=Right)	4	5	7	5.69	2.43
Political extremism (left)	0	0.5	1.5	0.87	1.32
Political extremism (right)	0	0	1.5	1.06	1.52
Religiosity (Range: 0 to 2)	1	2	2	1.69	0.55

**Notes:** Tabulated for the sample of 191,302 observations used in our regressions of income decile on respondent characteristics, which provide the basis for computing the income residual variable.

**Table 2**  
**Baseline Specification: Education, the Income Residual, and Political Participation**

Dependent variable:	Demonstrate (1)	Occupy (2)	Boycott (3)	Strike (4)	First PC (OLS) (5)	Petition (6)	Discuss Pol (7)	Vote (8)
Education	0.1274*** [0.0163]	0.0436* [0.0263]	0.1120*** [0.0171]	0.0817*** [0.0207]	0.0679*** [0.0169]	0.0953*** [0.0182]	0.1365*** [0.0168]	0.1038*** [0.0205]
IncResid	-0.0910** [0.0409]	-0.0480 [0.0617]	-0.0894 [0.0551]	-0.0491 [0.0609]	-0.0749 [0.0503]	-0.1948*** [0.0448]	-0.1305*** [0.0296]	-0.0153 [0.0465]
Income Decile	0.0978** [0.0472]	0.0228 [0.0695]	0.1100* [0.0639]	0.0529 [0.0671]	0.0830 [0.0569]	0.2386*** [0.0447]	0.1649*** [0.0281]	0.0303 [0.0478]
Individual controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-wave fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	166593	103526	164005	108038	95590	168233	125543	57672
R <sup>2</sup> / Pseudo R <sup>2</sup>	0.0897	0.1012	0.1225	0.0936	0.1521	0.1679	0.0720	0.2039
No. of countries	80	61	80	63	60	81	67	45
No. of surveys	138	88	137	93	86	140	99	45
Education	0.1272*** [0.0162]	0.0429 [0.0262]	0.1120*** [0.0171]	0.0816*** [0.0206]	0.0678*** [0.0169]	0.0953*** [0.0182]	0.1364*** [0.0167]	0.1038*** [0.0204]
Education * IncResid	-0.0053*** [0.0017]	-0.0086*** [0.0023]	-0.0045*** [0.0017]	-0.0042** [0.0018]	-0.0051*** [0.0013]	-0.0010 [0.0017]	-0.0025 [0.0019]	0.0011 [0.0031]
IncResid	-0.0654 [0.0415]	-0.0046 [0.0604]	-0.0677 [0.0533]	-0.0276 [0.0597]	-0.0509 [0.0511]	-0.1901*** [0.0493]	-0.1186*** [0.0352]	-0.0203 [0.0504]
Income Decile	0.0984** [0.0473]	0.0236 [0.0698]	0.1104* [0.0640]	0.0534 [0.0672]	0.0834 [0.0569]	0.2386*** [0.0446]	0.1652*** [0.0279]	0.0304 [0.0478]
Individual controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-wave fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	166593	103526	164005	108038	95590	168233	125543	57672
R <sup>2</sup> / Pseudo R <sup>2</sup>	0.0898	0.1014	0.1226	0.0936	0.1523	0.1679	0.0720	0.2039
No. of countries	80	61	80	63	60	81	67	45
No. of surveys	138	88	137	93	86	140	99	45

**Notes:** Standard errors are clustered by country; \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels respectively. All columns are estimated by ordered logit, except Column 5 which uses the first principal component of Demonstrate, Occupy, Boycott, and Strike as the dependent variable, and is estimated by OLS. All Columns include: (i) individual-level dummies for age group, gender, number of children, and marital status; as well as (ii) country-survey wave fixed effects.

**Table 3**  
**Full Specification: Controlling for Education interacted with Country-Survey Wave Fixed Effects**

Dependent variable:	Demonstrate (1)	Occupy (2)	Boycott (3)	Strike (4)	First PC (OLS) (5)	Petition (6)	Discuss Pol (7)	Vote (8)
Education	0.0134 [0.0223]	-0.2054*** [0.0228]	0.0515*** [0.0195]	-0.1613*** [0.0295]	-0.0529** [0.0255]	0.0545** [0.0245]	0.2235*** [0.0273]	0.0143 [0.0419]
Education * IncResid	-0.0140*** [0.0043]	-0.0090* [0.0052]	-0.0122** [0.0049]	-0.0143** [0.0062]	-0.0215*** [0.0043]	0.0053 [0.0048]	0.0057 [0.0041]	-0.0198** [0.0094]
IncResid	-0.0445 [0.0299]	0.0227 [0.0692]	-0.0629 [0.0590]	-0.0001 [0.0666]	0.0263 [0.0402]	-0.2700*** [0.0515]	-0.2012*** [0.0377]	0.0930 [0.0764]
Individual controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-wave fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Inc Decile & Ed * Inc Decile?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education * Country-wave FEs?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	166593	103526	164005	108038	95590	168233	125543	57672
R <sup>2</sup> / Pseudo R <sup>2</sup>	0.0940	0.1060	0.1269	0.0985	0.1634	0.1716	0.0768	0.2098
No. of countries	80	61	80	63	60	81	67	45
No. of surveys	138	88	137	93	86	140	99	45

**Notes:** Standard errors are clustered by country; \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels respectively. All columns are estimated by ordered logit, except Column 5 which uses the first principal component of Demonstrate, Occupy, Boycott, and Strike as the dependent variable, and is estimated by OLS. All Columns include: (i) individual-level dummies for age group, gender, number of children, and marital status; (ii) country-survey wave fixed effects; (iii) income decile and its interaction with individual education; as well as (iv) individual education interacted with the full set of country-survey wave fixed effects.

**Table 4**  
**Age versus Education**

Dependent variable:	Demonstrate (1)	Occupy (2)	Boycott (3)	Strike (4)	First PC (OLS) (5)	Petition (6)	Discuss Pol (7)	Vote (8)
Education	0.0144 [0.0222]	-0.2043*** [0.0226]	0.0525*** [0.0194]	-0.1595*** [0.0294]	-0.0521** [0.0254]	0.0548** [0.0245]	0.2245*** [0.0272]	0.0133 [0.0417]
Education * IncResid	-0.0131*** [0.0043]	-0.0078 [0.0051]	-0.0111** [0.0049]	-0.0129** [0.0061]	-0.0204*** [0.0043]	0.0060 [0.0047]	0.0069* [0.0041]	-0.0189** [0.0093]
IncResid	-0.0547* [0.0305]	0.0271 [0.0741]	-0.0891 [0.0580]	-0.0014 [0.0683]	0.0160 [0.0407]	-0.2863*** [0.0513]	-0.2404*** [0.0404]	0.0494 [0.0802]
Age 25-34 * IncResid	-0.0063 [0.0080]	-0.0193 [0.0160]	0.0016 [0.0088]	-0.0319** [0.0136]	-0.0102 [0.0094]	0.0058 [0.0080]	0.0183* [0.0110]	0.0441** [0.0196]
Age 35-44 * IncResid	-0.0017 [0.0083]	-0.0409*** [0.0156]	0.0272** [0.0106]	-0.0119 [0.0134]	-0.0025 [0.0088]	0.0152* [0.0090]	0.0471*** [0.0120]	0.0604*** [0.0224]
Age 45-54 * IncResid	0.0145 [0.0096]	-0.0142 [0.0184]	0.0354*** [0.0112]	-0.0047 [0.0151]	0.0129 [0.0104]	0.0234*** [0.0088]	0.0607*** [0.0150]	0.0705*** [0.0228]
Age 55-64 * IncResid	0.0320*** [0.0109]	0.0449** [0.0185]	0.0447*** [0.0114]	0.0283* [0.0149]	0.0285*** [0.0083]	0.0253*** [0.0091]	0.0565*** [0.0145]	0.0475** [0.0229]
Age 65 and over * IncResid	0.0255* [0.0144]	0.0491** [0.0212]	0.0498*** [0.0168]	0.0535*** [0.0166]	0.0291*** [0.0109]	0.0263* [0.0147]	0.0360** [0.0173]	0.0479* [0.0276]
p-value: Age 25-34 * IncResid	[0.0361]	[0.9087]	[0.1417]	[0.6058]	[0.8806]	[0.0000]	[0.0000]	[0.2238]
Age dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-wave fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Inc Decile & Ed * Inc Decile?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ed * Country-Wave FEs?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	166593	103526	164005	108038	95590	168233	125543	57672
R <sup>2</sup> / Pseudo R <sup>2</sup>	0.0941	0.1063	0.1271	0.0987	0.1637	0.1716	0.0770	0.2101
No. of countries	80	61	80	63	60	81	67	45
No. of surveys	138	88	137	93	86	140	99	45

**Notes:** Standard errors are clustered by country; \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels respectively. All columns are estimated by ordered logit, except Column 5 which uses the first principal component of Demonstrate, Occupy, Boycott, and Strike as the dependent variable, and is estimated by OLS. All Columns include: (i) individual-level dummies for age group, gender, number of children, and marital status; (ii) country-survey wave fixed effects; (iii) income decile and its interaction with individual education; as well as (iv) individual education interacted with the full set of country-survey wave fixed effects. The reported p-value is for a test of whether the overall effect of the income residual for the 25-34 age group is equal to 0.

**Table 5**  
**Interpreting the Effects: The Role of Grievance**

Dependent variable:	<b>Demonstrate</b>	<b>Occupy</b>	<b>Boycott</b>	<b>Strike</b>	<b>First PC (OLS)</b>	<b>Petition</b>	<b>Discuss Pol</b>	<b>Vote</b>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.0241 [0.0186]	-0.1805*** [0.0273]	0.0613*** [0.0186]	-0.1835*** [0.0280]	-0.0425** [0.0185]	0.0597** [0.0269]	0.2274*** [0.0241]	-0.0075 [0.0430]
Education * IncResid	-0.0130*** [0.0039]	-0.0057 [0.0059]	-0.0128*** [0.0046]	-0.0141** [0.0064]	-0.0203*** [0.0038]	0.0033 [0.0049]	0.0082** [0.0037]	-0.0217** [0.0100]
IncResid	-0.0372 [0.0269]	-0.0025 [0.0696]	-0.0515 [0.0535]	0.0007 [0.0641]	0.0221 [0.0351]	-0.2579*** [0.0539]	-0.2217*** [0.0382]	0.0547 [0.0744]
Education * Confidence in govt	-0.0047 [0.0063]	-0.0127 [0.0088]	-0.0002 [0.0058]	-0.0138* [0.0075]	-0.0065 [0.0052]	-0.0036 [0.0048]	0.0069 [0.0057]	0.0002 [0.0084]
Confidence in govt	0.0134 [0.0402]	0.0608 [0.0450]	-0.0804* [0.0439]	-0.0259 [0.0469]	0.0024 [0.0266]	-0.0163 [0.0320]	0.0070 [0.0420]	0.2007*** [0.0415]
Individual controls?	Yes	Yes						
Country-wave fixed effects?	Yes	Yes						
Inc Decile & Ed * Inc Decile?	Yes	Yes						
Ed * Country-Wave FEs?	Yes	Yes						
Observations	147779	93108	146071	97397	86542	148727	107326	52622
R <sup>2</sup> / Pseudo R <sup>2</sup>	0.0855	0.1073	0.1217	0.0987	0.1588	0.1657	0.0739	0.1926
No. of countries	77	60	77	62	59	78	63	44
No. of surveys	129	83	129	88	82	130	88	44

**Notes:** Standard errors are clustered by country; \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels respectively. All columns are estimated by ordered logit, except Column 5 which uses the first principal component of Demonstrate, Occupy, Boycott, and Strike as the dependent variable, and is estimated by OLS. All Columns include: (i) individual-level dummies for age group, gender, number of children, and marital status; (ii) country-survey wave fixed effects; (iii) income decile and its interaction with individual education; as well as (iv) individual education interacted with the full set of country-survey wave fixed effects.

**Table 6**  
**Interpreting the Effects: Political and Religious Extremism**

Dependent variable:	Demonstrate (1)	Occupy (2)	Boycott (3)	Strike (4)	First PC (OLS) (5)	Petition (6)	Discuss Pol (7)	Vote (8)
Education	-0.0600** [0.0251]	-0.3071*** [0.0301]	-0.0227 [0.0264]	-0.3082*** [0.0299]	-0.1105*** [0.0245]	0.0207 [0.0265]	0.1519*** [0.0342]	0.0170 [0.0577]
Education * IncResid	-0.0199*** [0.0042]	-0.0138** [0.0057]	-0.0179*** [0.0055]	-0.0193*** [0.0069]	-0.0264*** [0.0045]	0.0000 [0.0053]	0.0019 [0.0048]	-0.0194** [0.0094]
IncResid	-0.0075 [0.0287]	0.0797 [0.0674]	-0.0171 [0.0558]	0.0481 [0.0681]	0.0603 [0.0452]	-0.2526*** [0.0585]	-0.2060*** [0.0385]	0.0989 [0.0843]
Education * Political extremism (left)	0.0202*** [0.0044]	0.0333*** [0.0061]	0.0175*** [0.0041]	0.0207*** [0.0048]	0.0250*** [0.0045]	0.0088*** [0.0028]	0.0102** [0.0043]	0.0191*** [0.0047]
Political extremism (left)	0.0390 [0.0266]	0.0096 [0.0394]	0.0378 [0.0248]	0.0474 [0.0307]	0.0064 [0.0196]	0.0231 [0.0204]	0.0428 [0.0349]	-0.0549* [0.0314]
Education * Political extremism (right)	-0.0020 [0.0033]	0.0017 [0.0053]	-0.0029 [0.0035]	-0.0027 [0.0057]	0.0005 [0.0031]	0.0045 [0.0028]	0.0014 [0.0032]	0.0047 [0.0054]
Political extremism (right)	0.0367*** [0.0138]	0.0362 [0.0269]	0.0334** [0.0149]	0.0205 [0.0256]	0.0216* [0.0115]	-0.0046 [0.0156]	0.0917*** [0.0196]	0.0465 [0.0303]
Education * Religiosity	0.0029 [0.0066]	-0.0029 [0.0133]	0.0035 [0.0076]	0.0106 [0.0073]	-0.0082 [0.0071]	-0.0042 [0.0064]	0.0042 [0.0088]	-0.0397*** [0.0139]
Religiosity	-0.0938** [0.0471]	-0.1734** [0.0759]	-0.1413** [0.0635]	-0.2356*** [0.0418]	-0.0907*** [0.0315]	-0.0063 [0.0460]	-0.0482 [0.0555]	0.2949*** [0.0804]
Individual controls?	Yes							
Country-wave fixed effects?	Yes							
Inc Decile & Ed * Inc Decile?	Yes							
Ed * Country-Wave FEs?	Yes							
Observations	123426	76373	120433	80746	71378	123396	85476	43104
R <sup>2</sup> / Pseudo R <sup>2</sup>	0.0939	0.1045	0.1259	0.0901	0.1674	0.1758	0.0758	0.2153
No. of countries	78	59	77	62	58	78	62	43
No. of surveys	132	85	130	91	84	132	89	43

**Notes:** Standard errors are clustered by country; \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels respectively. All columns are estimated by ordered logit, except Column 5 which uses the first principal component of Demonstrate, Occupy, Boycott, and Strike as the dependent variable, and is estimated by OLS. All Columns include: (i) individual-level dummies for age group, gender, number of children, and marital status; (ii) country-survey wave fixed effects; (iii) income decile and its interaction with individual education; as well as (iv) individual education interacted with the full set of country-survey wave fixed effects.

**Table 7**  
**Summary Statistics: Country-Level Variables**

	<b>25th</b>	<b>Median</b>	<b>75th</b>	<b>Mean</b>	<b>Std Dev</b>
Executive Turnover (0=No, 1=Yes)	0	1	1	0.606	0.489
Number of Executive Changes	0	1	1	1.069	1.281
Democracy (Range: 0 to 10)	0	6	9.4	5.097	4.171
Years of Primary schooling (ages >=25)	2.520	3.963	5.312	3.942	1.795
Years of Secondary schooling (ages >=25)	0.846	1.635	2.793	1.927	1.368
Years of Tertiary schooling (ages >=25)	0.076	0.217	0.418	0.293	0.286
Log GDP per capita	7.379	8.528	9.629	8.451	1.312
Log Population	8.045	9.038	10.043	9.012	1.686
% Population aged 15-24	21.024	31.571	35.191	28.812	7.776
% Population aged 25-39	29.495	32.514	34.059	32.075	4.220
Median Income Residual (71 countries)	-0.217	-0.020	0.099	-0.024	0.226

**Notes:** Tabulated for the sample of 787 country-year observations used in the Table 8, Column 1 regression, unless otherwise stated. All schooling data points are observed at five-yearly intervals. All other variables are constructed as averages over five-year periods. The democracy variable is summarized for the 744 observations used in the Table 9, Column 1 regression. The median income residual measure is summarized for the 71-country sample that is used in Section 4.2 of the main text.

**Table 8**  
**Determinants of Government Turnover**

Dependent variable:	Number of Executive Changes / Occurrence of Executive Change						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Poisson	Poisson	Poisson	Poisson	Arellano-Bond	Arellano-Bover/ Blundell-Bond	Logit
Lag number of exec. changes	-0.064*	-0.067*	-0.069*	-0.079**	0.112*	0.095	-1.208***
	[0.037]	[0.037]	[0.035]	[0.035]	[0.064]	[0.058]	[0.274]
Log (GDPpc)	0.303	0.397	0.357	0.626	3.139**	1.704	2.356
	[0.240]	[0.250]	[0.443]	[1.355]	[1.458]	[1.424]	[3.570]
Sec. Schooling	0.968	1.535*	1.972*	1.555	2.513	2.679	4.483
	[0.642]	[0.923]	[1.130]	[1.338]	[1.671]	[1.684]	[3.456]
Sec. Schooling * Log (GDPpc)	-0.119*	-0.169**	-0.218**	-0.165	-0.315**	-0.344**	-0.662**
	[0.064]	[0.074]	[0.103]	[0.129]	[0.151]	[0.146]	[0.326]
Pri. Schooling			-0.258	0.026	0.189	-0.831	-2.894
			[0.962]	[0.995]	[1.348]	[1.123]	[2.199]
Pri. Schooling * Log (GDPpc)			0.018	-0.018	-0.008	0.107	0.343
			[0.111]	[0.115]	[0.140]	[0.128]	[0.246]
Ter. Schooling			-2.932	-4.872	-2.674	-3.684	-6.929
			[4.670]	[4.486]	[7.475]	[7.369]	[16.785]
Ter. Schooling * Log (GDPpc)			0.270	0.453	0.193	0.272	0.670
			[0.468]	[0.444]	[0.745]	[0.734]	[1.687]
% Pop 15-24				-0.269	0.032	-0.071	-0.011
				[0.184]	[0.209]	[0.215]	[0.521]
% Pop 15-24 * Log (GDPpc)				0.026	-0.006	0.006	0.006
				[0.020]	[0.023]	[0.024]	[0.057]
% Pop 25-39				0.338*	0.521***	0.448**	1.029**
				[0.196]	[0.191]	[0.183]	[0.470]
% Pop 25-39 * Log (GDPpc)				-0.039*	-0.058***	-0.049**	-0.106**
				[0.021]	[0.021]	[0.020]	[0.050]
p-value: AR1 test on error terms					[0.0000]	[0.0000]	
p-value: AR2 test on error terms					[0.8414]	[0.7320]	
Country and year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log (Pop) & Sec. Sch * Log (Pop)?	No	Yes	Yes	Yes	Yes	Yes	Yes
Sec. Sch * Year fixed effects?	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	787	787	787	787	643	787	644
Log (Pseudo) Likelihood	-915.927	-909.038	-908.547	-902.819	---	---	-338.100
No. of countries	144	144	144	144	141	144	113

**Notes:** \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels respectively. Robust standard errors are reported; these are further clustered by country in the "Poisson" and "Logit" specifications. The dependent variable is the number of changes in chief executive during a given five-year period, except in Column 7 where an indicator variable for the occurrence of an executive change during the five-year period is used instead. All specifications include country and year fixed effects, while all other right-hand side variables are lagged explanatory variables. Estimates are from a poisson regression in Columns 1-4, an Arellano-Bond linear dynamic panel model in Column 5, an Arellano-Bond / Blundell-Bover linear dynamic panel model in Column 6, and a logit regression in Column 7.

**Table 9**  
**Determinants of Democratization**

Dependent variable:	Polity IV Democracy Score (0-10)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	OLS	OLS	Arellano-Bond	Arellano-Bover/ Blundell-Bond
Lag democracy	0.736*** [0.046]	0.724*** [0.048]	0.721*** [0.049]	0.731*** [0.047]	0.730*** [0.047]	0.766*** [0.105]	0.872*** [0.101]
Log (GDPpc)	0.142 [0.356]	0.504 [0.396]	0.509 [0.404]	0.464 [0.549]	2.217 [1.735]	3.832** [1.926]	4.274** [1.936]
Sec. Schooling	-0.009 [0.156]	1.903** [0.776]	1.422 [1.361]	2.471* [1.367]	2.724* [1.443]	6.787** [2.742]	6.608** [2.951]
Sec. Schooling * Log (GDPpc)		-0.192** [0.075]	-0.202* [0.112]	-0.331** [0.137]	-0.372** [0.149]	-0.678** [0.290]	-0.632** [0.299]
Pri. Schooling				0.118 [1.182]	-0.403 [1.159]	-1.409 [2.745]	-1.421 [2.139]
Pri. Schooling * Log (GDPpc)				0.003 [0.120]	0.070 [0.118]	0.204 [0.257]	0.169 [0.216]
Ter. Schooling				-14.654* [8.301]	-12.287 [8.990]	-11.056 [13.219]	-5.201 [12.467]
Ter. Schooling * Log (GDPpc)				1.477* [0.821]	1.221 [0.895]	1.102 [1.289]	0.538 [1.219]
% Pop 15-24					0.221 [0.271]	0.563** [0.244]	0.601** [0.258]
% Pop 15-24 * Log (GDPpc)					-0.019 [0.027]	-0.056** [0.026]	-0.061** [0.029]
% Pop 25-39					0.359 [0.247]	0.511** [0.241]	0.437* [0.245]
% Pop 25-39 * Log (GDPpc)					-0.034 [0.025]	-0.047* [0.025]	-0.039 [0.026]
Lag democracy (2 periods)						-0.219*** [0.057]	-0.265*** [0.057]
p-value: AR1 test on error terms						[0.0009]	[0.0006]
p-value: AR2 test on error terms						[0.9038]	[0.7854]
Country and year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log (Pop) & Sec. Sch * Log (Pop)?	No	No	Yes	Yes	Yes	Yes	Yes
Sec. Sch * Year fixed effects?	No	No	Yes	Yes	Yes	Yes	Yes
Observations	744	744	744	744	744	459	602
R-square	0.93	0.93	0.93	0.93	0.93	---	---
No. of countries	133	133	133	133	133	124	132

**Notes:** \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels respectively. Robust standard errors are reported, clustered by country in the "OLS" specifications. The dependent variable is the average Polity IV democracy score during a given five-year period. All specifications include country and year fixed effects, while all other right-hand side variables are lagged explanatory variables. Estimation is via OLS, except in Column 6 where an Arellano-Bond estimator is used and in Column 7 where an Arellano-Bover / Blundell-Bond estimator is used.