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The People want the Fall of the Regime: Schooling, Political Protest, and the Economy

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Abstract

We examine several hypotheses regarding the determinants and implications of political protest, motivated by the wave of popular uprisings in Arab countries starting in late 2010. While the popular narrative has emphasized the role of a youthful demography and political repression, we draw attention back to one of the most fundamental correlates of political activity identified in the literature, namely education. Using a combination of individual-level micro data and cross-country macro data, we highlight how rising levels of education coupled with economic under-performance jointly provide a strong explanation for participation in protest modes of political activity as well as incumbent turnover. Political protests are thus more likely when an increasingly educated populace does not have commensurate economic gains. We also find that the implied political instability is associated with heightened pressures towards democratization.

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

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

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1 Introduction

Starting in late 2010, the political order in many Arab countries was shaken by a series of popular uprisings. Most notably, in Tunisia and Egypt, longstanding and seemingly stable incumbents were forced out by a groundswell of discontent and public protest. What conditions contributed to the galvanizing of public opinion and the subsequent unraveling of these regimes? Short-term developments on the ground, such as the recent bout of worldwide inflation, surely played a role in the timing of events. That said, the popular narrative has pointed to a combination of at least two structural forces, namely demographics and economic conditions, that had already set the stage for these protest movements. On the one hand, these were countries with a sizeable cohort of young, tech-savvy individuals who had aspirations for the future; the successful uprising in Egypt for example was quickly dubbed the "Youth Revolution".[1] Combined with the simmering dissatisfaction over negative economic conditions, including long-term unemployment and deep-seated corruption, this provided a powerful tandem that eventually motivated large numbers to demonstrate publicly for change.[2]

This narrative speaks, at least indirectly, to 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 that were at the forefront of the recent episodes in the Arab World, such as attending political events and demonstrating.[3] As it turns out, the widely used Barro-Lee data on education around the world reveals that many Arab countries have in fact witnessed aggressive increases in schooling in recent years. Egypt and Tunisia, for instance, all registered large gains in total years of schooling among the population aged 15 and above, respectively rising from 2.6 to 7.1 years and from 3.2 to 7.3 years between 1980 and 2010. Out of the top 20 countries in the world as ranked by increases in general schooling during this period, there were eight Arab League countries.[4]

Digging deeper beneath the surface, the popular narrative on the Arab world revolts is also in line with recent research showing that favorable economic circumstances can mute the propensity of more educated individuals to engage in political activities (Charles and Stephens 2009). In particular, there is

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[1]In the words of Wael Ghonim, the Google executive who figured prominently in the Egyptian protests: "This is the revolution of the youth of the internet, which became the revolution of the youth of Egypt, then the revolution of Egypt itself" (BBC News, 2011).

[2]For example, Khalaf (2011) states: "In a region where more than 60 per cent of the population is under 25, (...) [t]his is a movement born of youth fretting about their future and watching their societies slip behind the rest of the world."

[3]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), and Glaeser et al. (2007). See also Chong and Gradstein (2009) who find a positive association between education and pro-democracy views.

[4]In descending order, these were UAE, Algeria, Bahrain, Jordan, Libya, Egypt, Saudi Arabia, and Tunisia (Barro and Lee 2010). From the non-Arab Middle East, and also a prominent site of recent turmoil, Iran was also in the top 20.

strong evidence showing that factors at the country or industry level which raise the labor market returns to the skills acquired through education in turn have a systematic impact on an individual's incentives to apply her human capital towards political activities (Campante and Chor 2011). In short, the higher the returns to human capital in the production sector, the less likely that it will be channeled to political participation instead. Of note, many Middle Eastern economies are far from skill-intensive. For instance, Clemens et al. (2008) estimate that skilled Egyptians who migrate to the US earn almost nine times as much as those who stay in their home country. The increasingly educated populaces within the Arab World have thus not seen a commensurate improvement in terms of the labor market returns to their newly acquired human capital.[5] In sum, it appears that an interaction between individual skills and the dearth of economic opportunities that reward those skills lies at the heart of the political turmoil that has shaken the Arab world.

This paper attempts to extract broader lessons from that interaction, in order to uncover some key identifiers for the propensity for protest movements and the vulnerability of incumbent regimes. We examine whether some of these insights motivated by the recent Arab experience hold up when confronted with individual-level data pooled from across countries, as well as with aggregate cross-country data.

Our investigation proceeds in two parts. First, we ask whether individuals who earn a low income relative to what their level of human capital would be expected to command are more likely to be politically involved, especially in time-intensive activities such as demonstrations that are most threatening to authoritarian regimes. Second, we investigate whether this interaction translates into a greater threat to incumbent regimes at the broader cross-country level. Specifically, are increases in schooling, when coupled with worsening macroeconomic conditions that detract from the returns to that schooling, associated with a greater probability of incumbent turnover?

As it turns out, the answer to both of the above questions is affirmative. For the analysis at the individual level, 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 observable characteristics, including education. We find strong evidence that this measure of residual income influences patterns of political participation. In particular, the larger an individual's residual income, the less responsive to education is one's propensity to participate in political activities. In other words, individuals whose actual income under-performs that which would be predicted by observables are

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[5]It is telling that the Tunisian street vendor, Mohamed Bouazizi, whose self-immolation was the spark behind the wave of protests, was rumored to be a university graduate. Although that last detail about his education status now appears to be apocryphal (Fahim 2011), the fact that it gained such currency is itself illustrative of the broader trend of graduate unemployment in Tunisia. Similarly, many observers have drawn attention to high unemployment levels among the increasing ranks of the educated, as well as the stifling of economic opportunity, in places like Tunisia and Egypt (Ammous and Phelps 2011, Cassidy 2011, Eichengreen 2011, Khalaf 2011).

more likely to devote their human capital towards active political involvement. Of note, the above pattern holds true precisely for protest modes of participation, namely demonstrations, strikes or occupying buildings, but not for more civic and less time-intensive modes of participation such as signing petitions or discussing politics. There is thus a link between economic under-performance at the individual level and political protest.

At the aggregate level, we explore the determinants of the turnover of incumbent regimes using data on the latter variable coded in Campante et al. (2009). Using the Barro-Lee data on country schooling in a panel that covers the years 1976-2010, we show that the interaction of increases in schooling with deteriorating macroeconomic conditions (specifically real GDP per capita) raises the likelihood and frequency that incumbent rulers will be displaced. This holds even after controlling for the potential role of having a large youthful population cohort. Our specifications here include both country and time fixed effects, so that our results are estimated in part off the within-country variation in economic and political conditions. This is important as it implies that it is not just about rulers being more vulnerable in poorer countries, but also that incumbents can become more imperiled if an increasingly educated populace is faced with worsening economic prospects. Last but not least, the same interaction between country schooling and income per capita has a significant correlation with country democracy scores (from the Polity IV dataset), suggesting that incumbent turnover is frequently accompanied by some degree of democratization.

Put together, these two sets of results provide a compelling picture consistent with the idea that an increase in education without a corresponding increase in economic opportunities can expose incumbent regimes to the threat of public protests and even turnover. Needless to say, several caveats are in order with regard to how to interpret these results. First, this does not mean that investments in education are bad from any country's or dictator's perspective. The importance of human capital as a pre-condition for economic growth is not in dispute, not to mention the intrinsic desirability of improved levels of education. Nevertheless, increases in schooling can open the door to political instability when they are combined with an absence of commensurate economic gains. Conversely, a healthy economy that rewards skilled labor in particular would raise the opportunity cost of political protest, as well as be more successful in meeting the employment expectations of an educated populace.[6] Second, our results do not imply the inevitability of political protests in such circumstances, let alone that of a successful revolution. Rather, our conclusions regarding incumbent turnover are strictly of a probabilistic nature. Finally, while we focus on the role of education and economic forces in our analysis, this is not to say that political forces do not matter in determining the prospects for incumbent instability. In the specific cases of Tunisia

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[6]Consistent with this logic, many incumbents in the Middle East have announced raises in public sector wages or disbursements of more cash to the public in response to the recent wave of protests, at least as a stop-gap measure to stave off public dissatisfaction (*The Economist* 2011b, 2011c).

and Egypt, for example, the longstanding public revulsion towards repression from the authorities was clearly important in stoking the protests against the regime.

Interestingly, while the narrative of a youthful disaffected population has been widespread in discussions on the wave of protests in the Middle East, the role played by education in that combustible mix was to a large extent initially overlooked. As an illustrative popular example, *The Economist* (2011a) came up with a "shoe-thrower's index" of the vulnerability of Arab incumbents that included among its indicators the share and absolute number of people under age 25 and economic performance (GDP), in addition to measures of corruption, press freedom, and longevity. Education and the economic underperformance specifically of skilled individuals was conspicuously missing from the original version of the index; adult literacy was only subsequently added to an updated list of indicators about a month later (*The Economist Online* 2011).

In addition to the aforementioned literature on the nexus between schooling and political participation, our results also relate to a broader body of work in political economy studying regime transitions. Acemoglu and Robinson's (2001, 2005) theory of democratic transitions emphasizes the role of economic circumstances in affecting the opportunity cost of staging revolutions. A related result highlighting the importance of the opportunity cost for "extreme" forms of political participation comes from the literature on civil wars (Grossman 1991, Collier and Hoeffler 2004), which was extended to multiple forms of political violence by Besley and Persson (2010). In contrast with these contributions, we focus on the interaction of this opportunity cost logic with education, both at the individual and country levels.

The rest of the paper is organized as follows. Section 2 presents the data and evidence at the individual level. Section 3 examines the country-level evidence on incumbent turnover. Section 4 concludes.

2 Schooling, Political Protest, and Economic Circumstances at the Individual Level

We start off by investigating the extent to which individual economic circumstances affect one's propensity to devote human capital to political protest.

2.1 Data

We use data from the World Values Survey (WVS), a comprehensive survey of sociocultural and political attitudes around the world. The latest version of the data comprises five waves (conducted in 1981-1984, 1989-1993, 1994-1999, 1999-2004, 2005-2007), although our eventual regression sample will draw mostly on Waves 3-5 as the set of variables is considerably more limited in earlier waves. In all, the pooled data consists of close to 200,000 observations from 148 surveys (84 distinct countries), as documented in

Appendix Table 1. Note that the sample contains a broad spectrum of both developed and developing countries from all continents. Several Middle Eastern countries are also present, including Egypt.

The WVS contains several key questions on a range of modes of political participation. We seek therefore 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 gauge this from four WVS questions (E026-E029) eliciting the individual's propensity towards political action in the form of: (i) "attending lawful demonstrations"; (ii) "occupying buildings and factories"; (iii) "joining in boycotts"; and (iv) "joining unofficial strikes". These are modes of political activity which are often associated with public displays of protest, and which also tend to be demanding in terms of the time and effort of the individual participants. In 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" forms of political activity that are more civic in nature. These are: (i) "signing a petition" (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, with the response options being: "Never" (=0), "Occasionally" (=1), and "Frequently" (=2). Last but not least, "vote" is a binary variable asking whether the individual voted in the most recent parliamentary election; note however that this variable is only available in the latest wave of the WVS, so the number of observations is considerably smaller. We have classified these three as non-protest modes of political engagement, primarily because they are less time- and effort-intensive activities. For example, signing a petition is typically less strenuous than demonstrating in a public square. Moreover, voting has come to be viewed in the political science literature as a generally more passive form of political expression, that is less demanding in terms of its human capital requirements than other political activities (Milbrath and Goel 1977, Verba and Nie 1987, Brady et al. 1995).

The WVS also comes with a comprehensive set of respondent biodata and personal characteristics, which we will use as potential determinants of political participation. These include for example age, gender, marital status, employment status, and occupational categories. Key among these explanatory variables is the respondent's highest education level attained, which is coded on a categorical scale that ranges from 1 ('Inadequately completed elementary education') to 8 ('University with degree/Higher education - upper-level tertiary certificate'). Given our focus on economic performance or status and how this affects the opportunity cost of political participation, we will also use the respondent data on income decile. One may arguably be concerned about the self-reported nature of both the education and income variables. Likewise, actual income figures would be preferable to the coarser income decile

variable that is available in the WVS. However, to the extent that these variables are inherently noisy, this would only bias us against finding significant effects in our regressions.

Appendix Table 2 reports summary statistics for the political participation measures and respondent characteristics in the WVS. Some forms of political activity such as "Occupy" (mean=0.151 on the 0-2 scale) and "Strike" (mean=0.280) are less commonly pursued than others (such as "Petition", mean=0.884). There is nevertheless clearly a lot of variation in the data, as evidenced by the relatively large standard deviations relative to the mean values for all of these variables.

2.2 Main Specification and Results

Our main objective is to examine whether poor labor market returns to education will in fact raise the incentive for individuals to direct their human capital towards political activities, particularly protest modes of participation. One way to address this question is to ask whether individuals who somehow have an income status lower than that which would be predicted by observables, including their level of schooling, are systematically more likely to engage in political activities. We would specifically want to check whether this is the case for those forms of political participation that are more intensive in time and human capital, and that would thus entail a steeper opportunity cost in terms of labor income from market activities foregone.

To operationalize this approach, we require a measure of under-performance in incomes. To that end, we run the following regression that seeks to predict the income decile of individual i in country c at time t, as a function of education, $Educ_{ict}$, and other respondent characteristics, V_{ict}:

$$Income_{ict} = \alpha_1 Educ_{ict} + \alpha_2 V_{ict} + D_{ct} + \tilde{\eta}_c + \eta_{ict}, \tag{1}$$

We include in V_{ict} a comprehensive set of variables, namely: age, age squared, gender, number of children, as well as a full set of dummy variables respectively for marital status, employment status, occupation, and size of town categories.[7] The D_{ct} are country-survey wave fixed effects to absorb the differences in average income levels across countries and time. When estimating the above, we cluster the standard errors by country, to allow for correlated shocks ($\tilde{\eta}_c$) in income levels for individuals from the same country over time. We then obtain the income residual, $IncResid_{ict} \equiv \tilde{\eta}_c + \eta_{ict}$, from this regression.[8] Intuitively, this provides a measure of the extent to which an individual's income decile exceeds that which is predicted by her level of education and other relevant biographical and demographic characteristics. In particular, negative values of $IncResid_{ict}$ indicate an under-performance in actual income status relative

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[7]The estimation of (1) is naturally closely-related to the Mincer regression. While work experience and parent characteristics are often included in the Mincer regression literature, these additional variables were not available in the WVS.

[8]The income residual calculated in this manner has been closely studied by labor economists. These have documented a rise in residual wage inequality in the US over the past 30 years, which appears to have been an important component in the overall rise in inequality during this period. For an overview of the issues and recent debates, please see Katz and Autor (1999), Acemoglu (2002), and Lemieux (2006).

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to what would have been expected from observables. Moreover, since (1) pools observations across countries, the income residual thus calculated incorporates information about one's income performance relative to expectations when compared against similar individuals across countries. It therefore speaks for example to the issue of how the expected income premium earned by Egyptian university graduates over another Eyyptian with only some secondary education compares against the same premium for an observationally-identical US university graduate.

We present first the results from estimating (1) in Table 1. Columns 1-4 perform this using ordinary least squares (OLS), while Columns 5-8 use an ordered logit, in light of the categorical nature of the income dependent variable. For each estimation method, a baseline regression that controls for personal biographic characteristics – age, age squared, gender, number of children, marital status dummies, and employment status dummies – is reported in the first column. The second column adds occupation dummies, while the third column includes a set of dummies to control for the size of town of residence. The fourth column then replicates this last full specification while limiting the sample to Waves 3-5 of the WVS, given that data limitations severely restrict the number of observations from earlier waves which we can actually use. The findings turn out to be extremely stable across all specifications, with education not surprisingly being positively and significantly associated with income. Of note, the comprehensive set of respondent characteristics accounts for about 30% of the actual variance in income decile, so there is still a substantial amount of variation due to unobservables that is captured by the income residual terms.[9]

[**TABLE 1 HERE**]

We adopt Column 7 as our preferred specification, since this uses the full set of controls and the largest possible sample. In order to compute the income residual in the context of the ordered logit, we generate for each individual observation the predicted probabilities of its falling into each of the 10 ordered income deciles, $\hat{Pr}_D(Income_{ict} \in Decile_{ct}^{(j)})$, where $Decile_{ct}^{(j)}$ denotes the j-th decile ($j = 1, 2, \ldots, 10$). We then calculate the predicted expected income decile as: $\sum_{j=1}^{10} j \times \hat{Pr}_D(Income_{ict} \in Decile_{ct}^{(j)})$. The actual minus this predicted income decile provides us with the relevant income residual. The $IncResid_{ict}$ measure thus calculated displays substantial variation across individuals: Its 5th and 95th percentile values are respectively -3.067 and 3.470, with a standard deviation of 1.988 (bear in mind that the theoretical upper and lower bound values for the residual are -9 and 9). Reassuringly, the income residuals calculated from the other specifications in Table 1, including those estimated via OLS, are all highly correlated, with a pairwise correlation of at least 0.97, so that it is effectively immaterial which

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[9]For the ordered logit specifications, the "Variance explained" statistic reported in Table 1 is calculated as the variance in predicted income deciles divided by the variance in actual income deciles. This closely parallels the concept of the R^2 from OLS.

specification we use to generate the income residual.[10]

Our core results come from exploring how these income residuals relate to patterns of political participation. We pursue this with the following regression specification:

$$
\begin{aligned}
PolPart_{ict} \; = \; & \beta_1 Educ_{ict} + \beta_2 Educ_{ict} \times IncResid_{ict} + \beta_3 IncResid_{ict} \\
& + \beta_4 IncDecile_{ict} + \beta_5 V_{ict} + D_{ct} + \tilde{\varepsilon}_c + \varepsilon_{ict},
\end{aligned} \tag{2}
$$

Following the literature, this seeks to explain political participation, $PolPart_{ict}$, as a function of respondent characteristics, notably including education and income decile. Note that we include in the vector, V_{ict}, all the individual controls which entered into the full Column 7 specification in Table 1. Since we pool our observations across WVS surveys, we include country-wave fixed effects, while once again clustering the standard errors by country.

We include on the right-hand side our key variable of interest, namely the income residual measure which we have computed from Table 1, as well as its interaction with education, $Educ_{ict} \times IncResid_{ict}$. This explores whether one's income performance relative to that which is predicted from observables affects the level of one's political engagement, as well as the responsiveness of that participation across different schooling levels. The basic hypothesis that we test is whether $\beta_2 < 0$. This would be consistent with the interpretation that individuals with a more negative $IncResid_{ict}$, namely whose actual income falls short of that which can be expected from his/her education level (and other characteristics), are in turn more inclined towards political activism instead.

Table 2 shows these core results from estimating (2), for each of the separate political participation measures. These results point strongly to a role for the income residual variable in explaining patterns of political participation, and especially political protest. The upper panel of the table first reports baseline results controlling for the level effects of education and the income residual, while the lower panel then further includes the $Educ_{ict} \times IncResid_{ict}$ interaction. Our core result centers on the negative β_2 coefficient on $Educ_{ict} \times IncResid_{ict}$ that we obtain in the lower panel. This negative and significant interaction effect applies especially for the protest modes of political activity, "Demonstrate", "Occupy", and "Boycott", that feature as the dependent variables in Columns 1-3; the coefficient for "Strike" in Column 4 is only marginally insignificant at the 10% level. Not surprisingly, this effect shows up strongly in Column 5, which is based on the first principal component of the four protest measures.[11] On the other hand, Columns 6-8 illustrate that any interaction effect of the income residual with schooling is statistically indistinguishable from zero for the softer activities, such as "Petition" and "Vote". In other

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[10]Note however that based on pairwise t-tests, the income residuals from the OLS specifications tend to exhibit a slightly larger mean value than those from the ordered logit columns.

[11]Wave 5 of the WVS included new questions that more specifically ask whether individuals have recently taken part in various political activities, namely demonstrations, boycotts, and petitions. When using these as dependent variables instead, the results remain generally consistent, although slightly weaker in terms of statistical significance due to the smaller sample size (available on request).

words, at the within-country level, more educated individuals whose incomes fall short of expectations display a greater propensity to engage precisely in protest modes of political expression. (Note that all regressions in Table 1 are estimated via ordered logit, except for Column 5 which is run using OLS.)

[**TABLE 2 HERE**]

We view this negative interaction effect as consistent with an interpretation that centers on the opportunity cost of political participation. Consider a framework in which schooling acts to expand an individual's capacity to engage in all tasks, both in market production as well as in political activities.[12] If that individual's returns to applying her human capital towards market production are low, then that individual would be more inclined to devote her human capital towards political activities instead. To the extent that these low returns would correspond for example to a very negative value of $IncResid_{ict}$, this would account for the negative effect which we have uncovered for the $Educ_{ict} \times IncResid_{ict}$ interaction. It moreover rationalizes why this interaction effect is absent for softer, more civic-natured political activities such as signing a petition or voting. These have been viewed as typically less demanding of one's time and human capital, and hence one's propensity to engage in such activities would be less sensitive to the opportunity cost of production income foregone.

In addition to the interaction, we also obtain negative and frequently significant coefficients for the level effect of $IncResid_{ict}$ on the propensity to engage in protest activity. This holds true in Columns 1-4 in both the upper and lower panel specifications in Table 1. We view this as potentially capturing an overall "grievance" effect, in that an individual who under-performs in terms of his income status would be more likely to be dissatisfied towards the political establishment that sets economic policies. This effect is once again weaker and even absent for the civic modes of political activity in Columns 6-8.[13] As we shall see later, however, this negative level effect of the income residual will not be especially robust when subject to further checks.

Two caveats on these findings are in order. First, it may of course be possible that what the interaction term is picking up is an extension of the "grievance" effect: Individuals who have invested more to attain a higher level of education could be more upset if their income falls short of expectations, and hence more driven to engage in protests. The opportunity cost and grievance stories are certainly complementary. Nevertheless, it is unclear why grievance would necessarily apply only to the forms of political participation that are most intensive in time and effort. We are thus led to conclude that

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[12]The Appendix in Campante and Chor (2011) formalizes this as a model of human capital allocation between market production activities and political participation. Market production increases one's income, but political participation is necessary to check the ability of the incumbent to expropriate that income.

[13]Some readers may be concerned about the insignificant and sometimes negative coefficient on education in Table 2. It turns out that if the income residual variable is dropped from the regressions in the top panel, the positive and significant relationship between political participation and education is fully restored. For some interesting examples of papers that dispute the robustness of the positive correlation between education and voting, see Tenn (2007), Kam and Palmer (2008), and Berinsky and Lenz (2008).

at least part of what the results are picking up relates to an opportunity cost story. Second, one would worry about the issue of causality. For example, it may be that some individuals are by nature more inclined towards protest activities, this being an unobservable individual trait which we cannot control for. At the same time, this propensity towards protests could explain the income under-performance of that individual. While this is certainly a valid concern, it is harder to rationalize why an innate inclination toward protests would lead to a more negative income residual precisely for individuals who have higher levels of educational attainment.

All in all, while we cannot claim to be quantifying a causal effect of the opportunity cost mechanism, the evidence seems indicative of an important link between individual economic circumstances and propensity towards devoting human capital to political protest.

2.3   Robustness and Extensions

Our results are unchanged, if not stronger, when we probe the data to establish robustness. In the upper panel of Table 3, we further control for an interaction term between education and income decile ($Educ_{ict} \times IncDecile_{ict}$). This helps to ascertain whether our results indeed relate to the relative under-performance of one's income status, rather than its actual level. Our findings for the negative interaction effect between individual schooling and residual income are clearly robust, except for "Boycott". That said, the results are highly significant for the first principal component measure in Column 5, which helps to remove idiosyncratic noise that might be specific to any single political protest variable. Interestingly, the pattern for voting starts to resemble more that which we have established for the protest activities, as the coefficient of $Educ_{ict} \times IncResid_{ict}$ is now negative and significant at the 5% level.

[TABLE 3 HERE]

In the lower panel of Table 3, we subject the data to an even more stringent test by adding interaction terms between individual education and the full set of country-wave fixed effects ($Educ_{ict} \times D_{ct}$). This is motivated by the evidence in Campante and Chor (2011), who show that underlying features of the country systematically influence the responsiveness of political participation to education at the individual level. In particular, they find that various proxies for the skill-intensity of the economy tend to dampen the positive micro-level association between political engagement and education, consistent with the intuition that such economies feature a higher opportunity cost of devoting one's human capital towards political activities. The $Educ_{ict} \times D_{ct}$ terms thus provide a flexible way to control for the potential mediating effect of such country characteristics. Of note, our core results are in fact strengthened when we implement this check. The negative point estimates for the interaction between education and residual income are now larger in magnitude and significant at least at the 5% level precisely for the protest measures of political participation in Columns 1-5.

Separately, one may be concerned that our findings may be driven by outlier values of the income residual, given the large dispersion in this constructed variable that we have noted. This turns out not to be the case, as the statistical significance is not affected when dropping the observations that are in the tail 5% extremes of the $IncResid_{ict}$ values (available on request).

As mentioned, much of the popular narrative on the Arab world revolts has emphasized the role of a youthful demography with high aspirations for the future. Table 4 thus attempts to control for this alternative thesis, by examining whether age (and its square) interacted with our measure of income under-performance has explanatory power for patterns of political participation. (All specifications here continue to include education by country-wave fixed effects.) 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 measures of participation, whereas no consistent pattern is detected for the interactions involving age. This is reassuring, as it suggests that the education and opportunity cost element can be disentangled from the "youth revolt" story. Also motivated by the Middle East revolts, we have separately verified that the core result remains when we further control for self-reported religiosity (available on a 0-10 scale) and its square. This helps us to address the potential role that factors such as political Islam or religious extremism might play in influencing patterns of protest (regressions available on request).

[**TABLE 4 HERE**]

We examine in Table 5 whether our results differ across democratic and non-democratic countries. We re-run the Table 3 specification with education by country-wave fixed effects separately for countries with a Polity IV democracy score ≤ 7 (upper panel) or > 7 (lower panel), this being calculated as an average over the initial five years preceding each survey wave, from Marshall and Jaggers (2010). (This threshold is chosen to keep the subsamples relatively equal in size, but the results are similar with the more "neutral" cutoff score of 5.) The negative interaction effect for protest modes is indeed present in both subsamples, although statistical significance suffers somewhat from the smaller number of observations. The effect does appear to be larger in magnitude in the more democratic countries, for example when one compares the Column 5 results from the first principal component. This could presumably reflect the fact that barriers to political expression are lower in more democratic countries.

[**TABLE 5 HERE**]

How large is this interaction effect in terms of its quantitative implications? We explore this using the Column 5 specification in the lower panel of Table 3, since including the education by country-wave fixed effects appears important to control for potential omitted variables. For a one standard deviation decrease in the income residual (equal to 1.99), we ask how much more inclined toward protest activity

an individual with tertiary education would be relative to someone with incomplete primary education. The corresponding change on the dependent variable would be: $-0.0261 \times 7 \times 1.99 = -0.364$, or about one-quarter of a standard deviation of the first principal component, a fairly sizeable effect.

As a final exercise, it is useful to reconnect our income residual measure back to our original motivation from developments in the Arab world. Table 6 tabulates mean values of $IncResid_{ict}$ by country-wave for individuals with an education level of at least 3 (some secondary schooling), displaying specifically the 25 surveys with the most negative average income residual values.[14] It is quite revealing that this list contains all the Middle East countries represented in the WVS (Algeria, Egypt, Iran, Iraq, Jordan, and Morrocco), with the sole exception of Saudi Arabia where per capita income levels have generally been higher. The Middle East thus appears to have been systematically providing relatively poorer income opportunities for those with some secondary schooling. Interestingly, the presence of China on this list dovetails broadly with concerns that have been echoed elsewhere (Jacobs 2010, Eichengreen 2011) regarding the scarcity of economic opportunities for university graduates that would be commensurate with their education levels, and the potential social and political implications of such skilled under- or unemployment.[15]

[**TABLE 6 HERE**]

In sum, the evidence is consistent with the idea that individuals who earn less relative to what could be expected from their level of schooling will be more inclined to apply their human capital towards political activities. In particular, such individuals will be more likely to engage in protest modes of political expression – demonstrations, occupations, strikes – that tend to be relatively intensive in their use of human capital and time.

3 Schooling, Economic Performance, and Political Turnover at the Country Level

Our preceding evidence based on individual-level survey data point to a strong link between political protest, schooling and the economic opportunities available to skilled individuals. We turn next to ask whether there is evidence that such effects might in the aggregate affect the stability of incumbent regimes. Specifically, do poor economic prospects for an increasingly educated populace predict the

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[14]We would naturally expect the mean income residual taken across all WVS observations to be close to 0. Reassuringly, the average income residual level for all individuals with some secondary education is 0.001.

[15]Some readers may find the presence of Germany on this list to be somewhat odd. The average income decile reported in the WVS by tertiary-educated Germans tends to be very close to that reported by Germans with some basic secondary education. The mean income decile reported by those with education code equal to 8 is 5.30, whereas that for education code equal to 3 is 5.27. This could be a reflection of various forces specific to Germany such as possibly a flatter wage structure or simply a cultural aversion towards acknowledging one's higher economic status. As it turns out, our regression results are largely unaffected if we drop Germany from the sample.

likelihood that an incumbent will be removed from office? This is the natural counterpart question to ask of the cross-country data, especially in light of the rising education levels in many Arab countries which we noted in our Introduction.

3.1 Data

To address 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 for countries around the world. As a source of information, Worldstatesmen.org is extremely comprehensive and up-to-date. Political changes in real-time are typically updated within a week; for example, the recent regime changes in Tunisia and Egypt have all been reflected on their country pages. In additional, 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.[16] To guard against concerns that might be raised over the open-source nature of the website, we have compared the records in WorldStatesmen.org against Beck at 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 a binary variable coded equal to 1 if a change in country chief executive took place during a given 5-year window, say 2001-2005. 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 with a parliamentary system. There are however some key 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 formal title of president, prime minister or secretary-general, such as Deng in China, Noriega in Panama, and Qaddafi in Libya. In these instances, we treated these rulers as the chief executive given their *de facto* hold on executive power. Third, in cases of a military-controlled state, such as following a successful coup, 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 from the status quo. Since the binary indicator of incumbent turnover may discard some useful information, we will later also work with a count measure of the number of chief executive changes during each 5-year window to provide corroborating evidence. This can equivalently be viewed as a measure of the frequency of changes in the

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[16]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.

executive.

Our key explanatory variables for country schooling are from the latest version of the Barro and Lee (2010) dataset. This provides information on educational attainment at five-yearly intervals, including a breakdown by age cohorts. Our main measure for the prevalence of advanced education will be the percentage of the population aged 25 and above with some secondary schooling (or higher levels) attained. This in principle helps to capture the share of the local populace with a skilled background that could become potentially disaffected should the economy stagnate. While the Barro-Lee data also contain educational attainment figures for ages 15 and above, we prefer the ages 25 and above variables since these should better reflect final schooling outcomes for individuals after any investments in secondary and tertiary education have been undertaken. The Barro-Lee data additionally provide figures on the percentage with completed (rather than some) secondary schooling. Our results are nevertheless similar if either of these alternative human capital measures is used instead (available on request). Separately, we will consider average years of secondary schooling attained as an alternative education measure in our robustness checks.

The remaining variables used for this analysis are from standard sources of country data. The real GDP per capita data are from the Penn World Tables, this being a summary measure of a country's overall economic performance. We also use the total population figures in the Penn World Tables, although we rely on the Barro-Lee dataset for the breakdown of shares by age cohort. As before, we take the country democracy scores from the Polity IV dataset. Summary statistics for these cross-country variables are reported in Appendix Table 3. Note that our main sample contains seven 5-year periods (from 1976-1980 to 2006-2010).

3.2 Political Turnover Specifications

Building on the results obtained at the individual level in the previous section, we hypothesize an interaction effect: Incumbents will be more threatened when a highly skilled population is faced with circumstances where their human capital is not rewarded in production. In other words, what we seek to test is whether the interaction between country human capital and a measure of economic performance impacts the probability of a government leadership change. We capture this with the following specification:

$$
\begin{aligned}
Turnover_{ct} &= \gamma_1 Educ_{ct} + \gamma_2 Educ_{ct} \times EconPerf_{ct} + \gamma_3 EconPerf_{ct} + \\
&\quad \gamma_4 V_{ct} + D_c + D_t + \epsilon_{ct}
\end{aligned}
\tag{3}
$$

where $Turnover_{ct}$, $Educ_{ct}$ and $EconPerf_{ct}$ represent the measures of incumbent turnover, country education, and economic performance respectively. V_{ct} is a vector of additional country control variables.

The subscript c stands for country, whereas t denotes a given five-year window such as 2001-2005. This is the unit of time which we adopt in the analysis since the education data is only available at five-year intervals. In practice, we therefore explore whether initial education (say in 2000) helps to explain incumbent turnover in the subsequent five-year window (in 2001-2005). For consistency, we use lagged five-year averages (1996-2000) for $EconPerf_{ct}$ and V_{ct} on the right-hand side, although the results turn out to be very similar if we instead use contemporaneous controls (averaged over 2001-2005).

Our empirical strategy includes country and time fixed effects, D_c and D_t. The relevant question for us is whether a given country's investment in schooling, when combined with a period of poor economic performance, will augur a period of increased incumbent instability. As such, it is important to focus on the within-country variation, so as to distinguish it from the effect of cross-country differences in human capital and income. The time fixed effects further control for the possibility of broad waves of political turbulence, such as following the fall of the Berlin Wall or during the recent Arab World revolts, that might simultaneously raise the probability of incumbent turnover across many countries at a given point in time.

The baseline results using the indicator measure of incumbent turnover are displayed in Table 7. For this dependent variable, we estimate (3) with a simple logit regression, unless otherwise stated. Our primary interest is in the sign of γ_2, the coefficient of the interaction term between country education (share of population aged 25 and above with some secondary schooling) and economic performance (log real GDP per capita).

[**TABLE 7 HERE**]

Column 1 in the table contains a basic regression with only the level effects of education and GDP per capita; these alone yield relatively undistinguished results. Column 2 in turn introduces our main interaction of interest, and the result is exactly in line with our hypothesis: Increases in schooling are associated with a greater likelihood of incumbent turnover ($\gamma_1 > 0$), but this effect is dampened when economic performance is relatively good ($\gamma_2 < 0$). Put otherwise, during times of poor economic conditions, high levels of human capital can be particularly threatening to incumbent stability.

Column 3 includes further controls in the form of log country population and its interaction with education, in order to verify that our central result on the γ_2 coefficient is not driven by country scale effects. We also add a full set of education interacted with time fixed effects, motivated by the idea that certain periods may be characterized by a heightened response of educated individuals to political action across different countries, such as the recent groundswells witnessed in the Arab world. Our results are clearly robust to these, and our point estimate of γ_2 is in fact now larger in magnitude.[17]

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[17]The results are similar if either the population controls or the education by time dummies are added separately; available on request.

The next few columns confront the "youth revolution" hypothesis surrounding the Arab world revolts. To check if our human capital story is not being confounded by an age cohort effect, we separately re-run the Column 3 specification using the percentage with some secondary schooling in the 25-39 age group and in the 40-and-above age group (Columns 4 and 5 respectively). The evidence suggests that there is not much difference along this age dimension. In particular, it does not appear that youth education matters more than that of older cohorts when it comes to the interaction with economic performance.[18] Another way of examining this cohort effect is presented in Column 6. We test here whether the size of the youth cohort has an impact on incumbent stability, by controlling for population between ages 25-39 (as a share of the total population above age 25), as well as its interaction with log per capita real GDP. There appears to be some evidence that a larger cohort of economically disaffected youth is potentially dangerous to incumbents (the interaction coefficient between the youth population share and country income is significant at the 10% level), consistent with the popular narratives on the importance of the young in driving the Arab street revolts. That said, our core results regarding the γ_2 coefficient are in fact strengthened. Since we will want to control for such youth cohort effects, we will adopt Column 6 as our preferred specification for the remainder of the table.[19]

The magnitude of the effects that are implied by these coefficient estimates for the probability of incumbent turnover are generally large. Consider for example the impact of a one standard deviation increase in secondary schooling percentage that is also accompanied by a one standard deviation fall in log real GDP per capita. The marginal effect that this would have on the odds of a change in executive taking place would rise by $\exp(0.035 \times 1.161 \times 24.814) = 2.74$ or about 174%, holding all else constant. Bearing in mind that the coefficient on the main effect of schooling itself is also positive and significant, the overall increase in the odds of turnover would be even bigger. This role of the schooling interaction is also larger than that which would be predicted from a one standard deviation increase in the size of the ages 25-39 youth cohort, this latter effect being $\exp(0.053 \times 1.161 \times 8.444) = 1.68$, namely a 68% increase.

It is worth noting that what matters most for incumbent instability is not so much primary schooling, but higher levels of human capital accumulation, namely secondary and tertiary. When our preferred specification is run using the percentage share with some primary schooling instead, we find that this variable is essentially irrelevant (Column 7). However, the combination of poor economic conditions

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[18]We have also experimented with controlling simultaneously for the percentage with some secondary schooling in both the 25-39 and 40-and-above age groups. These typically lead to insignificant results on both the schooling interaction terms with log real GDP per capita, reflecting a potential multicollinearity problem.

[19]Given that political institutions tend to persist over time, one might be concerned whether this would warrant the inclusion of a lagged dependent variable in our specification. Since this would, in the presence of country fixed effects, entail more stringent conditions to ensure the consistency of the estimates, we ultimately decided to leave it out from our preferred regression. The results are very similar though, and are available upon request. Note also that the application of dynamic panel techniques is not as standard with the non-linear logit model.

and an increasing population share with some tertiary education (Column 8) is particularly negative for political stability. These point strongly to the role of providing economic opportunities to skilled individuals as a key for avoiding incumbent turnover.

In Column 9, we allay concerns that the consistency of the estimates might be compromised by a potential incidental parameters problem, in light of the relatively short time dimension of our panel. We re-estimate our specification using a conditional logit, which conditions out the country fixed effects before the maximum likelihood search to avoid having to estimate this large number of parameters. Our results are again very much robust.[20] The panel structure of the data once again clarifies that our results are driven in part by the within-country variation, and not just solely from broad differences in schooling or income across countries. This is reassuring as it indicates that changes within countries over time in human capital accumulation and economic conditions help to explain changes in the likelihood of political turnover.

Last but not least, we obtain similar results using an alternative definition of incumbent turnover, coded based on changes in the party of the chief executive.[21] For instance, one may reason that whether the individuals are removed from executive office is not as relevant as whether parties or regimes are. Column 10 confirms that this concern does not alter our conclusions.

Our analysis thus far is indicative of a negative interaction effect between country schooling and macroeconomic performance on the probability of incumbent turnover. To make fuller use of the information on leadership changes that we have collected, we explore a parallel set of specifications in Table 8 using the number of executive changes during each 5-year window as the dependent variable instead. After all, a country where power has changed hands multiple time during a short span would appear to be much more unstable than one in which a single transfer of power took place. Moreover, our previous regressions in Table 7 required us to drop some countries from the sample, namely those that either had no executive changes throughout the 1976-2010 sample period, or which had experienced at least one executive change per 5-year window. We thus estimate (3) with a Poisson regression model, to take into account the integer count nature of this new dependent variable.[22]

[**TABLE 8 HERE**]

Our results are confirmed in Table 8, which re-runs the specifications in Table 7 using the number of executive changes as the turnover measure. The interaction effect between the population share with some secondary schooling and log real GDP per capita is consistently negative. Although its standard

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[20]The robust standard errors, clustered by country, are obtained from 100 bootstrap repetitions. A similar procedure is used for the conditional Poisson specification in Column 9 of Table 8.

[21]Erring on the side of simplicity, we counted instances where a leader changed his party affiliation, but continued in his role as chief executive, as a change in party.

[22]When experimenting with a negative binomial regression, we do not fail to reject the hypothesis of equality between the mean and variance of the count variable. This indicates that the Poisson model provides an appropriate fit to the data.

error is large in the Column 2 baseline, statistical significance is restored once we additionally control for the role of population size, as well as schooling by time fixed effects (Column 3). The results are particularly strong in our full specification that includes the effects of the youth cohort size (Column 6); note once again that the interaction between the youth population share and log income per capita is negative and significant, so that the "youth revolution" hypothesis appears to be complementary to our core story centered on the role of mass education. Once again, primary schooling does not appear to matter as much as secondary schooling (Column 7). However, unlike for the binary dependent variable, there is no significant effect when the role of tertiary education is considered (Column 8). That said, our results are robust when using an alternative Poisson maximum likelihood estimator that conditions out the role of the country fixed effects (Column 9), as well as when focusing on a count measure of party changes (Column 10).[23]

We have also checked our findings against different ways to measure our key country schooling variable. To this end, Table 9 adopts the average years of various levels of schooling attained in the population aged 25 and above, as an alternative to the population share measure that we have used so far. The first three columns reproduce our preferred specification (Column 6 from Tables 7 and 8) using the binary dependent variable of executive change, together with average years of primary, secondary and tertiary education respectively on the right-hand side. There is once again a negative interaction effect between schooling and economic performance. Importantly, this effect is statistically significant only for secondary and tertiary years of schooling, and is moreover quantitatively larger for tertiary years (Column 2 and 3). This once again confirms our intuition that what matters is not so much basic schooling that equips students with basic literacy, but more advanced schooling that equips individuals with workforce skills. Using the count measure of executive changes yields broadly similar patterns (Columns 4-6), although we do find larger standard errors. We return to a discussion of the final three columns in the last subsection on democratization below.

[**TABLE 9 HERE**]

Separately, we have also explored controlling for other measures of macroeconomic conditions that might reasonably matter for citizens' welfare or be indicative of the opportunity cost of political engagement. For example, we have explored including inflation and its interaction with schooling, as well as unemployment and its interaction with schooling (both variables from the World Development Indicators). Our main result for the interaction effect between country schooling and real per capita income is largely unaffected by this, while we generally did not obtain significant effects for the interaction of

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[23]Our specification estimates are little changed when we drop Hong Kong and Macau, these being territories where executive changes are often driven by decisions in mainland China. Further dropping Bosnia-Herzegovina and Switzerland does not change our results, these being countries which practise a system of regular rotations in the executive and thus show up with high turnover counts.

schooling with these alternative economic performance variables. Our sample size does drop however due to limitations on these new macroeconomic variables, so we chose not to repeat these specifications in full (available on request).

3.3 Democracy Score Specifications

A key caveat of our results 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 earlier emphasis on protest modes of political participation, one could argue that we should be concerned primarily with coding up 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 conducted 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 does indeed appear to ensue from periods in which economic conditions fail to keep up with the skill profile of the population. Specifically, we consider the commonly-used Polity IV democracy score as a dependent variable, and ask: Does the combination of increased levels of secondary schooling and a depressed economic performance entail an opportunity not only for leadership change, but also for democratization? Or is it mostly about transitions within an initially democratic or non-democratic context, or even perhaps about sliding back towards autocracy?

[TABLE 10 HERE]

Table 10 attempts to shed light on these questions. We reproduce the Table 7 specifications with the 0-10 democracy score as the dependent variable; unless otherwise stated, these are estimated with OLS.[24] These regressions take on a similar flavor to the recent work by Acemoglu et al. (2005, 2008, 2009) which consider the role of education and income as determinants of democratization using a similar cross-country panel context.[25] Our shortest specification in Column 1 shows essentially no effect of either schooling or income on democracy, similar to previous studies which tend to find no evidence for such an effect once country dummies are included in the specification. Interestingly, however, we find a negative and highly significant interaction effect for the combination of schooling and income per capita once this is introduced into the model (Column 2). The finding survives the same set of sensitivity checks which we had imposed in earlier tables, namely controlling for the role of total population, education by year fixed

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[24]Using a tobit regression to take into account the truncated nature of the democracy score leads to very similar results.
[25]This builds on earlier work in this vein by Barro (1999) and Przeworski et al. (2000), among others.

effects, as well as the youth cohort size hypothesis (Columns 3-6).[26] Note now however that the share of the population between ages 25-39 does not appear to affect the prospects for democracy. Once again, it is precisely higher levels of education and not primary schooling that matter for the democratization effect (Columns 6-8). This is also confirmed in the last three columns of Table 9, which use years of schooling as the education measure in explaining the democracy score; the effects there are most significant and sizeable for tertiary years of schooling (Table 9, Columns 7-9).

With the linear estimator in this table, it is now possible to explore some dynamic panel GMM specifications. Column 9 of Table 10 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 the model.[27] Column 10 then further includes time-differenced variables as additional instruments in a levels version of equation (3), which in principle yields more efficient estimates (Arellano and Bover 1995, Blundell and Bond 1998).[28] Our results are robust to these attempts to take the dynamic structure of the panel more seriously.

This leads us to conclude that the cocktail of increased schooling and poor economic opportunities to match, while potentially threatening to incumbent stability, actually tends to be accompanied by moves toward greater democratization.

4 Conclusion

Motivated by the narratives that have tried to explain the recent upheaval in the Middle East, we have sought to uncover broader lessons on the link between human capital, economic circumstances, and political protest. At the individual level, our evidence shows that individuals whose income under-performs what would have been predicted given their level of schooling are more inclined to use their human capital in political activities such as demonstrations, strikes, and occupation of buildings. We argue building on our previous research that this is consistent with the hypothesis that depressed economic prospects for human capital reduce the opportunity cost of engaging in effort- and skill-intensive political activities, namely protests. Note in particular that this argument is different from a simple "grievance" effect: It is not that a combination of high skill and poor rewards is dangerous simply because people will be upset about it, but also because the opportunity cost of engaging in political protest for skilled

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[26]The Column 6 specification in particular implies that a one standard deviation increase in schooling combined with a one standard deviation fall in country incomes tends to be accompanied by an increase in the democracy score of about 0.22 standard deviations.

[27]We use two periods of the lagged dependent variable in this estimation. The Column 9 regression passes the usual set of diagnostics that test for possible model mis-specification. We reject the null hypothesis of no first-order autocorrelation in the first-differenced errors (p-value = 0.000), but do not find evidence against second-order autocorrelation (p-value = 0.2287). In addition, we do not reject the Sargan test for overidentifying restrictions (p-value of χ^2 statistic = 0.1016).

[28]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.

individuals will be lower.

We have also looked at country-level data, and found evidence that countries that invest a lot in human capital, but fail to see that investment matched with commensurate rises in economic opportunities or living standards, are more likely to see incumbents removed from office. That combination seems to have been present in many of the Middle Eastern countries that have witnessed political turmoil recently, and our analysis suggests that it is a pattern that is manifest in the broader cross-country experience beyond the Arab World. In addition, the evidence indicates that such circumstances are often associated with improvements in democratic institutions.

Naturally, the above analysis makes no guarantees about the outcomes for specific country cases, including the various Arab countries that have inspired this analysis, although it does give cause for some cautious optimism. We also refrain from making definitive causal claims, particularly in the cross-country analysis, given the nature of the data which we take as observed. Notwithstanding these caveats, we believe the evidence is informative about the underpinnings of political protest, and what it means for the incumbents who face it.

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6 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. Respondent characteristics used include: age (X003), gender (X001), and number of children (X011). We use a set of dummy variables for the full set of possible categorical responses for marital status (X007). Employment status dummies are generated from question X028, namely: 1 for full time; 2 for part time; 3 for self-employed; 4 for retired; 5 for housewife; 6 for students; 7 for unemployed; 8 for other. Occupation dummies are generated from the first digit of the self-reported occupation code (X036), namely: 1 for employer/manager; 2 for professional or non-manual worker; 3 for manual worker; 4 for agricultural worker; 5 for military; 6 for never employed; 8 for other. Size of town dummies are generated from the response categories to question X049, ranging from: 2,000 and less; 2,000-5,000; . . . to 500,000 and more. We include a separate category for unreported/missing data for each of the employment status, occupation, and size of town dummies.

B. Country-level variables

Turnover: Following Campante et al. (2009), coded from Worldstatesmen.org. See Section 3.1 for details.

Schooling: From Barro and Lee (2010). The total percentage of a given age cohort with some secondary schooling is obtained by summing the 'ls' (some secondary) and 'lh' (some tertiary) variables, on the reasonable assumption that individuals with some tertiary schooling would have also had some secondary schooling. Likewise, the total percentage of a given age cohort with some primary schooling is obtained by summing 'lp' (some primary), 'ls' and 'lh'. Note that in the Barro-Lee data, 'lu' (no schooling), 'lp', 'ls' and 'lh' sum to 100%. Separately, years of schooling data are also used as reported. The schooling variables for the 25-39 year age cohort are calculated as weighted averages of schooling for the 25-29, 30-34, and 25-39 year age cohorts, using cohort population sizes as weights.

GDP per capita: From the Penn World Tables, version 6.3. Real GDP per capita chain series (constant purchasing power parity prices) is used.

Population: Total population is from the Penn World Tables, version 6.3. The population share by age cohort is computed from Barro and Lee (2010), which is in turn based on the UN population database.

Democracy: From the Polity IV dataset (Marshall and Jaggers 2010). Democracy score, on a scale of 0 to 10.

Table 1
First Stage: Determinants of Income in the World Values Survey

	Dependent variable: Income Decile							
	OLS				Ordered Logit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.3270***	0.2825***	0.2709***	0.2611***	0.2892***	0.2532***	0.2440***	0.2344***
	[0.0209]	[0.0200]	[0.0197]	[0.0161]	[0.0180]	[0.0180]	[0.0177]	[0.0139]
Age	0.0149**	0.0132*	0.0121*	0.0091	0.0143**	0.0128**	0.0119**	0.0092
	[0.0070]	[0.0067]	[0.0066]	[0.0065]	[0.0063]	[0.0061]	[0.0060]	[0.0060]
Age squared	-0.0001*	-0.0001*	-0.0001*	-0.0001	-0.0001**	-0.0001**	-0.0001**	-0.0001*
	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0001]
Female? (1=Yes; 0=No)	0.0316	-0.0137	-0.0204	-0.0331	0.0282	-0.0113	-0.0175	-0.0296
	[0.0290]	[0.0322]	[0.0315]	[0.0317]	[0.0258]	[0.0288]	[0.0282]	[0.0286]
Number of children	-0.0357**	-0.0321**	-0.0275**	-0.0242*	-0.0337***	-0.0311**	-0.0271**	-0.0234**
	[0.0136]	[0.0132]	[0.0134]	[0.0126]	[0.0124]	[0.0122]	[0.0125]	[0.0118]
Country-wave fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marital Status dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment Status dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation dummies?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Size of town dummies?	No	No	Yes	Yes	No	No	Yes	Yes
Observations	191302	191302	191302	184131	191302	191302	191302	184131
R-squared / Variance explained	0.2892	0.3005	0.3058	0.3038	0.2904	0.3015	0.3069	0.3046
Pseudo R-squared	-	-	-	-	0.0806	0.0840	0.0858	0.0849
No. of countries	84	84	84	84	84	84	84	84
No. of surveys	148	148	148	141	148	148	148	141

Notes: Standard errors are clustered by country, with ***, **, and * denoting significance at the 1%, 5%, and 10% levels respectively. All columns include country-survey wave fixed effects, martial status dummies and employment status dummies. The estimation in Columns 1-4 uses ordinarly least squares, while that in Columns 5-8 uses ordered logit. Columns 4 and 8 restrict the sample to Waves 3, 4, and 5 of the WVS. The "Variance explained" statistic for the ordered logit columns is calculated as the variance of predicted expected income decile divided by the variance of the actual income decile in the data.

Table 2

Education, Unexplained Income and Political Participation at the Individual Level (WVS)

Dependent variable:	Demonstrate (1)	Occupy (2)	Boycott (3)	Strike (4)	First PC (OLS) (5)	Petition (6)	Discuss Pol (7)	Vote (8)
Education	0.1147***	-0.0320	0.0701	-0.0104	0.0095	0.1590***	0.2704***	0.0489
	[0.0424]	[0.0405]	[0.0490]	[0.0454]	[0.0335]	[0.0432]	[0.0362]	[0.1252]
IncResid	-0.0784	-0.3245**	-0.1996	-0.3498**	-0.2649**	0.0757	0.3955***	-0.1907
	[0.1464]	[0.1485]	[0.1678]	[0.1638]	[0.1260]	[0.1545]	[0.1395]	[0.4488]
Income Decile	0.0870	0.3031**	0.2217	0.3546**	0.2749**	-0.0252	-0.3527**	0.2032
	[0.1446]	[0.1470]	[0.1645]	[0.1618]	[0.1256]	[0.1531]	[0.1395]	[0.4496]
Observations	166593	103526	164005	108040	95590	168233	125543	57672
(Pseudo) R-squared	0.0926	0.1037	0.1246	0.0975	0.1573	0.1693	0.0735	0.2160
No. of countries	80	61	80	64	60	81	67	45
No. of surveys	138	88	137	94	86	140	99	45
Education	0.1151***	-0.0311	0.0706	-0.0100	0.0101	0.1591***	0.2707***	0.0487
	[0.0424]	[0.0406]	[0.0491]	[0.0454]	[0.0335]	[0.0431]	[0.0364]	[0.1251]
IncResid	-0.0602	-0.2799*	-0.1843	-0.3332**	-0.2455*	0.0764	0.4036***	-0.1954
	[0.1465]	[0.1484]	[0.1693]	[0.1613]	[0.1249]	[0.1531]	[0.1424]	[0.4482]
Education * IncResid	**-0.0034****	**-0.0077*****	**-0.0027***	**-0.0029**	**-0.0036*****	**-0.0001**	**-0.0014**	**0.0008**
	[0.0017]	**[0.0022]**	**[0.0016]**	**[0.0020]**	**[0.0012]**	**[0.0018]**	**[0.0020]**	**[0.0032]**
Income Decile	0.0854	0.2986**	0.2201	0.3532**	0.2727**	-0.0253	-0.3539**	0.2043
	[0.1447]	[0.1475]	[0.1648]	[0.1619]	[0.1257]	[0.1529]	[0.1400]	[0.4493]
Observations	166593	103526	164005	108040	95590	168233	125543	57672
(Pseudo) R-squared	0.0927	0.1038	0.1247	0.0975	0.1574	0.1693	0.0735	0.2160
No. of countries	80	61	80	64	60	81	67	45
No. of surveys	138	88	137	94	86	140	99	45

Notes: Standard errors are clustered by country, with ***, **, and * denoting 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 controls for age, age squared, a gender dummy, number of children, marital status dummies, employment status dummies, occupation dummies, and size of town dummies; as well as (ii) country-survey wave fixed effects. The IncResid variable is that generated from the Column 7 ordered logit specification from Table 1.

Table 3
Robustness: Controlling for other Interaction Terms with Individual Education

Dependent variable:	Demonstrate (1)	Occupy (2)	Boycott (3)	Strike (4)	First PC (OLS) (5)	Petition (6)	Discuss Pol (7)	Vote (8)
Education	0.1159***	-0.0320	0.0730	-0.0115	0.0068	0.1586***	0.2722***	0.1001
	[0.0426]	[0.0425]	[0.0484]	[0.0472]	[0.0338]	[0.0421]	[0.0361]	[0.1268]
IncResid	0.1329	-0.1039	-0.0643	-0.1209	0.0666	-0.0423	0.2578*	0.5376
	[0.1564]	[0.2136]	[0.1941]	[0.2145]	[0.1319]	[0.1540]	[0.1438]	[0.5402]
Education * IncResid	**-0.0121****	**-0.0159****	**-0.0078**	**-0.0130***	**-0.0187*****	**0.0053**	**0.0056**	**-0.0243****
	[0.0059]	**[0.0078]**	**[0.0072]**	**[0.0070]**	**[0.0067]**	**[0.0066]**	**[0.0066]**	**[0.0118]**
Income Decile	-0.1071	0.1234	0.1003	0.1425	-0.0380	0.0931	-0.2085	-0.5292
	[0.1582]	[0.2181]	[0.1971]	[0.2167]	[0.1338]	[0.1531]	[0.1438]	[0.5424]
Education * Income Dec	0.0087	0.0080	0.0051	0.0098	0.0149**	-0.0054	-0.0069	0.0253**
	[0.0059]	[0.0077]	[0.0075]	[0.0063]	[0.0063]	[0.0061]	[0.0069]	[0.0111]
Observations	166593	103526	164005	108040	95590	168233	125543	57672
(Pseudo) R-squared	0.0927	0.1038	0.1247	0.0976	0.1579	0.1693	0.0735	0.2163
No. of countries	80	61	80	64	60	81	67	45
No. of surveys	138	88	137	94	86	140	99	45
Education	0.0625**	-0.2812***	0.0426	-0.2808***	-0.1042***	0.0799**	0.3783***	0.2235**
	[0.0315]	[0.0489]	[0.0362]	[0.0423]	[0.0261]	[0.0406]	[0.0426]	[0.1110]
IncResid	0.3017**	-0.1284	-0.0056	-0.2450	0.0254	0.0741	0.4576***	0.9010**
	[0.1373]	[0.2033]	[0.1738]	[0.1738]	[0.1103]	[0.1172]	[0.1412]	[0.4401]
Education * IncResid	**-0.0161*****	**-0.0135****	**-0.0109****	**-0.0191*****	**-0.0261*****	**0.0009**	**0.0044**	**-0.0241****
	[0.0056]	**[0.0064]**	**[0.0054]**	**[0.0071]**	**[0.0047]**	**[0.0059]**	**[0.0051]**	**[0.0112]**
Income Decile	-0.2754**	0.1492	0.0433	0.2721	0.0045	-0.0290	-0.4117***	-0.8954**
	[0.1364]	[0.2094]	[0.1777]	[0.1745]	[0.1129]	[0.1166]	[0.1418]	[0.4426]
Education * Income Dec	0.0123**	0.0050	0.0078	0.0149**	0.0217***	-0.0008	-0.0059	0.0259**
	[0.0054]	[0.0062]	[0.0053]	[0.0070]	[0.0045]	[0.0057]	[0.0061]	[0.0102]
Observations	166593	103526	164005	108040	95590	168233	125543	57672
(Pseudo) R-squared	0.0966	0.1083	0.1287	0.1021	0.1682	0.1732	0.0785	0.2219
No. of countries	80	61	80	64	60	81	67	45
No. of surveys	138	88	137	94	86	140	99	45

Notes: Standard errors are clustered by country, with ***, **, and * denoting 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 controls for age, age squared, a gender dummy, number of children, marital status dummies, employment status dummies, occupation dummies, and size of town dummies; as well as (ii) country-survey wave fixed effects. All specifications in the bottom panel further include individual education interacted with a full set of country-survey wave fixed effects. The IncResid variable is that generated from the Column 7 ordered logit specification from Table 1.

Table 4
Robustness: Age Interactions with IncResid

Dependent variable:	Demonstrate (1)	Occupy (2)	Boycott (3)	Strike (4)	First PC (OLS) (5)	Petition (6)	Discuss Pol (7)	Vote (8)
Education	0.0674**	-0.2771***	0.0488	-0.2737***	-0.1001***	0.0828**	0.3817***	0.2274**
	[0.0315]	[0.0478]	[0.0359]	[0.0417]	[0.0260]	[0.0405]	[0.0420]	[0.1113]
IncResid	0.2737**	-0.0397	-0.0792	-0.2336	-0.0049	0.0315	0.3083**	0.7919*
	[0.1358]	[0.2175]	[0.1756]	[0.1729]	[0.1119]	[0.1172]	[0.1428]	[0.4445]
Education * IncResid	**-0.0150***	**-0.0119***	**-0.0096***	**-0.0176****	**-0.0249***	**0.0017**	**0.0053**	**-0.0226****
	[0.0057]	**[0.0063]**	**[0.0055]**	**[0.0071]**	**[0.0048]**	**[0.0058]**	**[0.0050]**	**[0.0113]**
Income Decile	-0.2852**	0.1452	0.0314	0.2561	-0.0033	-0.0339	-0.4159***	-0.9033**
	[0.1375]	[0.2083]	[0.1771]	[0.1754]	[0.1137]	[0.1170]	[0.1418]	[0.4440]
Education * Income Dec	0.0121**	0.0049	0.0075	0.0148**	0.0216***	-0.0009	-0.0061	0.0255**
	[0.0054]	[0.0062]	[0.0053]	[0.0071]	[0.0045]	[0.0057]	[0.0061]	[0.0102]
Age	0.0430***	0.0129*	0.0424***	0.0251***	0.0213***	0.0441***	0.0619***	0.1371***
	[0.0059]	[0.0078]	[0.0075]	[0.0062]	[0.0046]	[0.0046]	[0.0056]	[0.0161]
Age sq	-0.0005***	-0.0003***	-0.0005***	-0.0005***	-0.0003***	-0.0005***	-0.0006***	-0.0012***
	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0002]
Age * IncResid	0.0008	-0.0059**	0.0029**	-0.0014	0.0009	0.0017*	0.0068***	0.0050**
	[0.0010]	[0.0025]	[0.0013]	[0.0016]	[0.0012]	[0.0010]	[0.0014]	[0.0025]
Age sq * IncResid	0.0000	0.0001***	-0.0000	0.0000*	-0.0000	-0.0000	-0.0001***	-0.0000
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Observations	166593	103526	164005	108040	95590	168233	125543	57672
(Pseudo) R-squared	0.0967	0.1085	0.1289	0.1023	0.1684	0.1732	0.0778	0.2221
No. of countries	80	61	80	64	60	81	67	45
No. of surveys	138	88	137	94	86	140	99	45

Notes: Standard errors are clustered by country, with ***, **, and * denoting 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 controls for age, age squared, a gender dummy, number of children, marital status dummies, employment status dummies, occupation dummies, and size of town dummies; (ii) country-survey wave fixed effects; as well as (iii) individual education interacted with a full set of country-survey wave fixed effects. The IncResid variable is that generated from the Column 7 ordered logit specification from Table 1.

Table 5
Robustness: By Level of Initial Country Democracy

Dependent variable:	Demonstrate (1)	Occupy (2)	Boycott (3)	Strike (4)	First PC (OLS) (5)	Petition (6)	Discuss Pol (7)	Vote (8)
Education	0.0940**	-0.2867***	0.0380	-0.2723***	-0.0645**	0.0651	0.4006***	0.1683
	[0.0391]	[0.0678]	[0.0483]	[0.0524]	[0.0316]	[0.0588]	[0.0481]	[0.1755]
IncResid	0.3004*	-0.2998	-0.0376	-0.2510	0.0224	0.1972	0.4894***	0.6803
	[0.1652]	[0.2661]	[0.2250]	[0.1962]	[0.1486]	[0.1970]	[0.1736]	[0.4467]
Education * IncResid	**-0.0109**	**-0.0090**	**-0.0116***	**-0.0176***	**-0.0196*****	**-0.0068**	**0.0084**	**-0.0237**
	[0.0071]	**[0.0093]**	**[0.0064]**	**[0.0101]**	**[0.0056]**	**[0.0077]**	**[0.0060]**	**[0.0153]**
Income Decile	-0.2697	0.3338	0.0780	0.2766	0.0026	-0.1645	-0.4426**	-0.6882
	[0.1684]	[0.2708]	[0.2302]	[0.1968]	[0.1515]	[0.2004]	[0.1756]	[0.4492]
Education * Income Dec	0.0054	-0.0031	0.0061	0.0113	0.0145***	0.0077	-0.0102	0.0229
	[0.0069]	[0.0087]	[0.0063]	[0.0095]	[0.0053]	[0.0079]	[0.0074]	[0.0153]
Observations	80478	57431	79750	57097	51625	81081	72974	20141
(Pseudo) R-squared	0.0908	0.1268	0.1115	0.0958	0.1301	0.0985	0.0740	0.2543
No. of countries	48	38	48	38	37	49	43	14
No. of surveys	66	50	66	49	48	67	57	14
Education	0.0379	-0.0859	-0.0506	-0.2547***	-0.1122**	-0.0133	0.2595***	0.2096
	[0.0603]	[0.0655]	[0.0516]	[0.0768]	[0.0444]	[0.0620]	[0.0750]	[0.1340]
IncResid	0.2349	0.1526	0.0152	-0.4483	0.0289	-0.1019	0.1484	0.8569
	[0.2548]	[0.2802]	[0.2416]	[0.2969]	[0.1335]	[0.2177]	[0.2069]	[0.6786]
Education * IncResid	**-0.0192****	**-0.0179****	**-0.0090**	**-0.0161***	**-0.0317*****	**0.0090**	**0.0010**	**-0.0243**
	[0.0088]	**[0.0090]**	**[0.0088]**	**[0.0093]**	**[0.0078]**	**[0.0087]**	**[0.0104]**	**[0.0192]**
Income Decile	-0.2122	-0.1424	0.0198	0.4814	0.0110	0.1591	-0.1001	-0.8331
	[0.2485]	[0.2830]	[0.2368]	[0.2938]	[0.1314]	[0.2124]	[0.2000]	[0.6803]
Education * Income Dec	0.0162**	0.0124	0.0072	0.0128	0.0268***	-0.0099	-0.0024	0.0247
	[0.0080]	[0.0083]	[0.0078]	[0.0093]	[0.0069]	[0.0077]	[0.0113]	[0.0175]
Observations	81414	42398	79587	47212	40419	82471	48679	36581
(Pseudo) R-squared	0.0993	0.0898	0.1399	0.1044	0.2108	0.1961	0.0861	0.1673
No. of countries	41	24	41	29	24	41	26	30
No. of surveys	67	34	66	41	34	68	38	30

Notes: Standard errors are clustered by country, with ***, **, and * denoting 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 controls for age, age squared, a gender dummy, number of children, marital status dummies, employment status dummies, occupation dummies, and size of town dummies; (ii) country-survey wave fixed effects; as well as (iii) individual education interacted with a full set of country-survey wave fixed effects. The IncResid variable is that generated from the Column 7 ordered logit specification from Table 1. The upper panel is restricted to countries with an initial 5-year average democracy score of <=7, while the bottom panel is restricted to those with a score of >7.

Table 6
Income Residuals by Country-Wave
(25 Most Negative Mean Values)

	Country (Wave)	Mean IncResid (if Educ >= 3)
1	Morocco (Wave 5)	-0.5928
2	India (Wave 4)	-0.4932
3	Iraq (Wave 5)	-0.4754
4	Germany (Wave 3)	-0.4487
5	Albania (Wave 3)	-0.4472
6	Iraq (Wave 4)	-0.4349
7	Germany (Wave 5)	-0.4126
8	Thailand (Wave 5)	-0.4040
9	Turkey (Wave 2)	-0.3959
10	China (Wave 3)	-0.3703
11	Spain (Wave 4)	-0.3688
12	Vietnam (Wave 4)	-0.3585
13	Indonesia (Wave 4)	-0.2950
14	China (Wave 5)	-0.2736
15	Jordan (Wave 4)	-0.2606
16	Iran (Wave 4)	-0.2406
17	Kyrgyzstan (Wave 4)	-0.2404
18	Venezuela (Wave 3)	-0.2320
19	Egypt (Wave 5)	-0.2269
20	India (Wave 3)	-0.2232
21	India (Wave 5)	-0.2186
22	Algeria (Wave 4)	-0.2107
23	Vietnam (Wave 5)	-0.2022
24	Nigeria (Wave 2)	-0.1952
25	Philippines (Wave 4)	-0.1936

Notes: The IncResid variable is that generated from the Column 7
ordered logit specification from Table 1.

Table 7
Determinants of Government Turnover I: Occurrence of a Change

Dependent variable:	Executive Change? (1 if Yes, 0 if No)									Party
Schooling (in %):	Sec >=25	Sec >=25	Sec >=25	Sec 25-39	Sec >=40	Sec >=25	Pri >=25	Ter >=25	Sec >=25	Sec >=25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	C. Logit	Logit
Schooling	0.015	0.161**	0.265***	0.235**	0.242**	0.322***	0.213*	0.704*	0.272***	0.321***
	[0.013]	[0.070]	[0.092]	[0.096]	[0.110]	[0.099]	[0.117]	[0.380]	[0.100]	[0.121]
Log (GDPpc)	-0.440	0.119	0.481	0.391	0.095	3.197*	1.177	2.116	2.735*	6.375***
	[0.305]	[0.390]	[0.432]	[0.458]	[0.389]	[1.674]	[1.579]	[1.562]	[1.576]	[2.236]
Schooling * Log (GDPpc)		-0.015**	-0.029***	-0.021**	-0.030**	-0.035***	-0.006	-0.096**	-0.030***	-0.036***
		[0.007]	[0.010]	[0.009]	[0.012]	[0.011]	[0.009]	[0.040]	[0.010]	[0.013]
% Pop 25-39						0.459	0.144	0.321	0.393	0.936**
						[0.289]	[0.252]	[0.278]	[0.280]	[0.373]
% Pop 25-39 * Log (GDPpc)						-0.053*	-0.018	-0.040	-0.045	-0.108**
						[0.031]	[0.027]	[0.030]	[0.030]	[0.043]
Country and year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log (Pop) & Sch * Log (Pop)?	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sch * Year fixed effects?	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	779	779	779	779	779	779	779	779	779	725
Pseudo R-squared	0.1727	0.1770	0.2010	0.1959	0.1960	0.2037	0.1912	0.1921	-	0.1993
No. of countries	117	117	117	117	117	117	117	117	117	110

Notes: Robust standard errors clustered by country are reported, with ***, **, and * denoting significance at the 1%, 5%, and 10% levels respectively. The dependent variable is a dummy for the occurrence of a change in chief executive during a given five-year period, except in Column 10 where the dummy is coded for the occurence of a change in party of the chief executive. Unless otherwise stated, the schooling variable is the % of the population aged 25 years and above who have some secondary schooling, calculated from the Barro-Lee dataset. All specifications include country and year fixed effects. Columns 3-10 further include: (i) Log (Population) and Schooling interacted with Log (Population), and (ii) Schooling interacted with year fixed effects. All right-hand side variables are lagged initial values. Estimation is via logit, except in Column 9 where a conditional logit is used; the standard errors for this column are computed from 100 bootstrap repetitions.

Table 8
Determinants of Government Turnover II: Number of Changes

Dependent variable:	Number of Executive Changes									Party
Schooling (in %):	Sec >=25	Sec >=25	Sec >=25	Sec 25-39	Sec >=40	Sec >=25	Pri >=25	Ter >=25	Sec >=25	Sec >=25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	C. Poisson	Poisson
Schooling	-0.001	0.050	0.075*	0.095**	0.071*	0.112**	0.090	0.018	0.112**	0.091*
	[0.006]	[0.035]	[0.042]	[0.044]	[0.040]	[0.048]	[0.069]	[0.146]	[0.049]	[0.048]
Log (GDPpc)	0.004	0.207	0.321	0.333	0.206	1.762**	0.827	1.166*	1.762**	2.542***
	[0.159]	[0.185]	[0.196]	[0.204]	[0.177]	[0.715]	[0.704]	[0.701]	[0.740]	[0.897]
Schooling * Log (GDPpc)		**-0.005**	**-0.008***	**-0.007**	**-0.008***	**-0.011***	**-0.001**	**-0.011**	**-0.011***	**-0.010***
		[0.004]	**[0.004]**	**[0.004]**	**[0.004]**	**[0.005]**	**[0.005]**	**[0.015]**	**[0.005]**	**[0.005]**
% Pop 25-39						0.236*	0.078	0.166	0.236*	0.344**
						[0.121]	[0.108]	[0.124]	[0.126]	[0.147]
% Pop 25-39 * Log (GDPpc)						-0.027**	-0.010	-0.020	-0.027**	-0.041**
						[0.013]	[0.011]	[0.013]	[0.013]	[0.017]
Country and year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log (Pop) & Sch * Log (Pop)?	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sch * Year fixed effects?	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	930	930	930	930	930	930	930	930	907	930
Log Pseudo Likelihood	-1103.8	-1102.1	-1093.6	-1093.8	-1095.8	-1091.4	-1089.6	-1094.4	-843.0	-841.3
No. of countries	144	144	144	144	144	144	144	144	138	144

Notes: Robust standard errors clustered by country are reported, with ***, **, and * denoting significance at the 1%, 5%, and 10% levels respectively. The dependent variable is the number of changes in chief executive during a given five-year period, except in Column 10 where the dummy is coded for the number of changes in party of the chief executive. Unless otherwise stated, the schooling variable is the % of the population aged 25 years and above who have some secondary schooling, calculated from the Barro-Lee dataset. All specifications include country and year fixed effects. Columns 3-10 further include: (i) Log (Population) and Schooling interacted with Log (Population), and (ii) Schooling interacted with year fixed effects. All right-hand side variables are lagged initial values. Estimation is via poisson regression, except in Column 9 where a conditional poisson is used; the standard errors for this column are computed from 100 bootstrap repetitions.

Table 9
Robustness: Alternative Measures of Country Schooling

Dependent variable:	Executive Change?			# Executive Changes			Democ		
Schooling (Average Years):	Pri >=25	Sec >=25	Ter >=25	Pri >=25	Sec >=25	Ter >=25	Pri >=25	Sec >=25	Ter >=25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Logit	Logit	Logit	Poisson	Poisson	Poisson	OLS	OLS	OLS
Schooling	3.295**	5.327***	23.766**	1.552*	1.568	1.071	0.351	3.916	21.932
	[1.603]	[1.901]	[11.653]	[0.937]	[0.994]	[4.251]	[2.375]	[3.799]	[15.147]
Log (GDPpc)	1.538	2.727*	2.027	1.026	1.470**	1.189*	-2.308	-0.048	-0.521
	[1.593]	[1.587]	[1.549]	[0.722]	[0.690]	[0.704]	[1.649]	[2.013]	[1.770]
Schooling * Log (GDPpc)	**-0.148**	**-0.619*****	**-3.102****	**-0.052**	**-0.167***	**-0.397**	**-0.216**	**-0.554**	**-2.977****
	[0.132]	**[0.212]**	**[1.210]**	**[0.079]**	**[0.096]**	**[0.437]**	**[0.193]**	**[0.349]**	**[1.391]**
% Pop 25-39	0.204	0.383	0.307	0.108	0.197*	0.169	-0.442	-0.105	-0.135
	[0.265]	[0.273]	[0.277]	[0.116]	[0.119]	[0.125]	[0.278]	[0.314]	[0.305]
% Pop 25-39 * Log (GDPpc)	-0.027	-0.046	-0.038	-0.014	-0.023*	-0.021	0.049*	0.008	0.014
	[0.029]	[0.030]	[0.030]	[0.012]	[0.013]	[0.013]	[0.028]	[0.032]	[0.031]
Country and year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log (Pop) & Sch * Log (Pop)?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sch * Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	779	779	779	930	930	779	867	867	867
(Pseudo) R-squared	0.1906	0.1998	0.1925	-	-	-	0.821	0.827	0.824
No. of countries	117	117	117	144	144	117	134	134	134

Notes: Robust standard errors clustered by country are reported, with ***, **, and * denoting significance at the 1%, 5%, and 10% levels respectively. The dependent variable for Columns 1-3 is the binary executive change variable, that in Columns 4-6 is the number of executive changes, while that in Columns 7-9 is the Polity IV democracy score, for a given five-year period. The schooling variable used is the average years of schooling in the population aged 25 years and above, for the respective categories of education (primary, secondary, and tertiary), from the Barro-Lee dataset. All specifications include country and year fixed effects, as well as: (i) Log (Population) and Schooling interacted with Log (Population), and (ii) Schooling interacted with year fixed effects. All right-hand side variables are lagged initial values. Estimation in Columns 1-3 is via logit, in Columns 4-6 is via poisson regression, and in Columns 7-9 is via OLS.

Table 10
Determinants of Democratization

Dependent variable:	Democracy Score (0-10)									
Schooling (in %):	Sec >=25	Sec >=25	Sec >=25	Sec 25-39	Sec >=40	Sec >=25	Pri >=25	Ter >=25	Sec >=25	Sec >=25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	Arellano-Bond	Arellano-Bover/ Blundell-Bond
Schooling	-0.002	0.264**	0.233	0.161	0.360	0.223	0.009	0.633	0.256**	0.232**
	[0.026]	[0.111]	[0.173]	[0.150]	[0.221]	[0.182]	[0.150]	[0.494]	[0.115]	[0.113]
Log (GDPpc)	-0.130	0.799	0.506	0.354	0.226	-0.208	-2.529	-0.759	1.964	1.362
	[0.563]	[0.608]	[0.655]	[0.660]	[0.639]	[1.918]	[1.750]	[1.727]	[2.434]	[2.426]
Schooling * Log (GDPpc)		-0.028***	-0.033**	-0.023*	-0.050***	-0.032*	-0.009	-0.090*	-0.021*	-0.020*
		[0.011]	[0.015]	[0.014]	[0.018]	[0.016]	[0.012]	[0.047]	[0.011]	[0.011]
Pop share 25-39						-0.141	-0.485*	-0.170	0.111	0.049
						[0.302]	[0.285]	[0.301]	[0.371]	[0.387]
Pop share 25-39 * Log (GDPpc)						0.013	0.053*	0.017	-0.010	-0.004
						[0.031]	[0.029]	[0.031]	[0.038]	[0.040]
Country and year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	-	-
Log (Pop) & Sch * Log (Pop)?	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sch * Year fixed effects?	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	867	867	867	867	867	867	867	867	475	610
R-squared	0.814	0.820	0.825	0.820	0.828	0.825	0.820	0.823	-	-
No. of countries	134	134	134	134	134	134	134	134	131	134

Notes: Robust standard errors clustered by country are reported, with ***, **, and * denoting significance at the 1%, 5%, and 10% levels respectively. The dependent variable is the mean Polity IV democracy score during a given five-year period. Unless otherwise stated, the schooling variable is the % of the population aged 25 years and above who have some secondary schooling, calculated from the Barro-Lee dataset. All specifications include country and year fixed effects. Columns 3-10 further include: (i) Log (Population) and Schooling interacted with Log (Population), and (ii) Schooling interacted with year fixed effects. All right-hand side variables are lagged initial values. Estimation is via OLS, except in Columns 9-10, which use the Arellano-Bond dynamic panel regression and the Arellano-Bover/ Blundell-Bond system estimator respectively.

Appendix Table 1
List of Countries in Sample

A: <u>World Values Survey</u>   (84 Countries, 148 surveys)

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 Monetenegro / Serbia (3-5);
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)

Notes: Tabulated for the regression sample in Table 1, Column 7, where the dependent variable is the income decile of the respondent. Numbers in parentheses indicate the survey waves for which data was available from that country.

Appendix Table 2
Summary statistics: WVS

	25th	Median	75th	Mean	Std Dev
Measures of political participaton					
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
Individual-level controls (WVS)					
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	-1.383	-0.156	1.264	-0.014	1.99

Notes: Tabulated for the sample of 191,302 observations in Table 1.

Appendix Table 3
Summary statistics: Cross-Country Analysis

	25th	Median	75th	Mean	Std Dev
Country-level variables					
Log GDP per capita (constant 2000 US$)	7.626	8.602	9.620	8.617	1.161
Log Population	7.964	8.954	10.017	8.935	1.701
% Population (ages 25-39)	38.491	48.588	51.542	45.873	8.444
% some Sec schooling (ages >=25)	15.108	31.017	53.607	36.217	24.814
% some Sec schooling (ages 25-39)	17.291	32.842	53.201	36.497	22.862
% some Sec schooling (ages >=40)	6.908	16.158	32.591	21.541	18.308
% some Pri schooling (ages >=25)	48.965	79.091	95.100	70.061	27.655
% some Ter schooling (ages >=25)	1.925	5.495	11.786	8.043	8.065
Years of Pri schooling (ages >=25)	2.280	3.809	5.233	3.795	1.844
Years of Sec schooling (ages >=25)	0.744	1.437	2.528	1.789	1.337
Years of Ter schooling (ages >=25)	0.064	0.185	0.383	0.263	0.262
Executive Turnover (0=No, 1=Yes)	0	1	1	0.595	0.491
Number of Executive Changes	0	1	1	1.065	1.298
Party Turnover (0=No, 1=Yes)	0	0	1	0.469	0.499
Number of Party Changes	0	0	1	0.725	1.028
Democracy	0	5.4	9	4.829	4.104

Notes: Tabulated for the sample of 930 country-year observations in the Table 8 sample. All schooling data points are observed at five-yearly intervals. All other variables are constructed as averages over five-year windows.