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Education as Liberation?

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ABSTRACT

Scholars have long speculated about education's political impacts, variously arguing that it promotes modern or pro-democratic attitudes; that it instills acceptance of existing authority; and that it empowers the disadvantaged to challenge authority. To avoid endogeneity bias, if schooling requires some willingness to accept authority, we assess the political and social impacts of a randomized girls' merit scholarship incentive program in Kenya that raised test scores and secondary schooling. We find little evidence for modernization theory. Consistent with the empowerment view, young women in program schools were less likely to accept domestic violence. Moreover, the program increased objective political knowledge, and reduced acceptance of political authority. However, this rejection of the status quo did not translate into greater perceived political efficacy, community participation, or voting intentions. Instead, the perceived legitimacy of political violence increased. Reverse causality may help account for the view that education instills greater acceptance of authority.

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1. Introduction

Education policies have often been motivated by views on their political impacts. Several U.S. states historically restricted the education of slaves for fear that it would encourage revolt (Woodson 1915), and Belgian colonial authorities in Africa enacted similarly restrictive education policies (Hochschild 1999). Post-independence authorities in Africa expanded education with a goal of promoting national identity and integration (Nyerere 1973), as it had earlier been used in Europe (Weber 1976). The view that expanding girls' educational access is a key to speeding the rise of female politicians and women's empowerment was a factor in the adoption of the third United Nations Millennium Development Goal (Herz and Sperling 2004; Levine et al. 2008, Lloyd 2009). Girls' scholarship programs in particular have been used to move towards this goal. Officials in Bangladesh cite women's empowerment as a main objective of their scholarship program (Rynor and Wesson 2006; Khandker et al 2003). There are also large programs in Egypt (Save the Children 2005, Iqbal and Riad 2004), Cambodia, Pakistan and India (Filmer and Schady 2008; Chaudhury and Parajuli 2006; India edunews.net 2010).

This paper exploits a randomized merit scholarship competition for adolescent girls in Kenyan schools to estimate the political and social impacts of these programs and shed light on education's impact more generally. Other research demonstrated that the incentives created by the program led to higher academic test scores (Kremer, Miguel, and Thornton 2009). In the current paper we analyze data from a follow-up survey collected four to five years after the scholarship competition, when the young women were 17 to 21 years of age.

Less developed regions have experienced massive increases in both education and democracy over the past half century, as illustrated by Kenya (appendix figure A1), and there has been extensive debate on how these trends might be interrelated. A widespread claim is that the

recent democratic transitions in North Africa and the Middle East – the world’s least democratic region, together with sub-Saharan Africa – have been propelled by increasingly well-educated youth populations, notably including young women (Saunders 2011). Scholars have long speculated about education’s political impacts. Modernization theorists argued that education weakens traditional ascriptive attachments based on gender, hereditary position, ethnicity and religion, in favor of merit (Levy 1966). Lipset (1959) argues that education promotes democratization. Dahl (1971) similarly asserts that socioeconomic development increases the potential for successful democracy, as a literate populace engages in the types of participation necessary to maintain representative government, and Huntington (1991) claims education contributed to the “Third Wave of Democratization” in the 1970s and 1980s. The modernization view has been influential, from post-colonial leaders seeking to use education as a nation building tool, to journalists arguing that it weakens support for violent extremism (Kristof 2010), to contemporary scholars studying the relationship between education and democracy (see Acemoglu et al. 2005, 2008, Barro and Lee 2001, Boix and Stokes 2003, Boix 2009, Epstein et al. 2006, Papaioannou and Siourounis 2005, Przeworski and Limongi 1997, among others).

Another view argues that education serves as a tool of cultural indoctrination and social control, instilling obedience to authority (Lott 1999, Pritchett 2003, Kremer and Sarychev 2008). Bowles and Gintis (1976) claim that U.S. education reinforces the class structure by training citizens to obey authority within the hierarchical modern corporation. Gramsci (1971) and other social theorists (Freire 1972, Fanon 2005) advance related points on its central role in bolstering the cultural hegemony of ruling elites, while simultaneously emphasizing that alternative forms education could be instruments for social change favoring the “liberation” of the oppressed.

A third school of thought views education as promoting individual autonomy and empowerment. In observational studies, education is correlated with greater individual political knowledge (Almond and Verba 1963, Verba and Nie 1972, Hanushek 2002, Bratton et al. 2005, Mattes and Bratton 2007); interest in obtaining political information (Dee 2004); greater dissatisfaction with existing institutions, and more support for women's rights (Weakliem 2002).¹ While correlations have been documented between education and political interest, participation and voting among individuals in wealthy countries (Verba and Nie 1972, Wolfinger and Rosenstone 1980, Inglehart et al 1998, Weakliem 2002, Dee 2004, Milligan et al 2004, Glaeser et al 2007), Galston (2001) notes that historical increases in U.S. education have not been followed by higher aggregate voter turnout or political knowledge. Moreover, the evidence in less developed countries is mixed. While Logan and Bratton (2006) find a positive correlation between education and political participation using AfroBarometer data from 15 countries, Blaydes (2006) finds a negative association between voting and education in Egypt, and argues that this result is due primarily to vote-buying. Education is seen as politically empowering for women in particular. Basu and King (2001) find that educated Bangladeshi women are more likely to participate in political meetings and to speak up.

Political empowerment need not be benign. Davies' (1974) "J-curve theory" posits that rapid expansions in opportunities build up expectations which, if unmet, can lead to frustration and violence. This theory was used to explain the relatively high schooling levels among participants in U.S. urban social unrest during the 1960s and 1970s (Miller et al 1977, Mason and Murtagh 1985). Krueger and Maleckova (2003) find that education predicts greater participation in Hezbollah activities in Lebanon.

¹ Lochner (2011) and Oreopoulos and Salvanes (2011) survey this literature.

A major difficulty in estimating the impact of education on individual attitudes and behavior is the possibility of reverse causality. For example, if those who are less willing to accept authority are less likely to stay in school, cross-sectional correlations between education and acceptance of authority will confound the causal impact of education on willingness to accept authority with the impact of acceptance of authority on education. Most existing studies estimate correlations between education, attitudes and behaviors in ways that are potentially vulnerable to this critique.² One strategy for addressing this problem, which we adopt in this paper, is to exploit randomized variation in education to separately measure the impact of such education on acceptance of authority. The use of experimental designs in comparative politics is growing (e.g., Wantchekon 2003, Humphreys et al 2006, Gugerty and Kremer 2008, Paluck and Green 2009, Habyarimana et al 2009, Olken, 2010, Vicente and Wantchekon 2010). A limitation of this micro-experimental approach is that we measure the impact of education induced by a particular policy in a single population. Yet insofar as similar policies oriented at boosting girls' education have been widely advocated and implemented in low-income countries, creating evidence on the impact of these programs is itself an important objective and can complement existing non-experimental analyses.

This paper takes advantage of the experimental Girls Scholarship Program (GSP) in Kenyan primary schools, which persistently boosted academic test scores and increased secondary school enrollment among girls from treatment schools.³ We find that exposure to the program reduces young women's acceptance of the right of men to beat their wives and children

² There are exceptions. Exploiting compulsory schooling laws, Milligan et al (2004) find impacts on political knowledge and voting in the U.S., with weaker effects in the U.K. Dee (2004) uses variation in community college availability and child labor laws in the U.S., and finds that education leads to higher voter turnout. Smith et al. (2009) use school supply variation to show that education increases autonomy over spousal choice for Vietnamese women.

³ In another study, Jakiela et al (2010) exploit the random assignment of the GSP program to estimate the impact of education on respect for earned property rights using lab experiment data.

and there is evidence it reduces the likelihood that parents are involved in choosing their daughter's spouse. These findings are broadly consistent with both modernization theory as well as the view that education promotes a desire for autonomy and empowerment, but are harder to reconcile with the claim that education tends to reinforce existing patterns of authority.

The evidence on attitudes beyond the household is not consistent with a modernization perspective but is more readily explained by the empowerment view of education. In particular, there is no evidence that the human capital created by the GSP leads to more pro-democratic or secular attitudes, or weakened ethnic identification. In fact, there is suggestive evidence that ethnic identity grows stronger among program beneficiaries, despite the Kenyan school curriculum's stated aim of promoting feelings of national unity.

Consistent with the view that education leads to a greater desire for autonomy, GSP participants are more likely to read newspapers and better able to identify a favorite news source. They possess more objective knowledge about politics and express less satisfaction with Kenya's democracy and current economic conditions. However, these impacts do not translate into greater perceived political efficacy or involvement as measured by voting intentions or participation in community groups. Instead, the young women express greater willingness to accept the legitimacy of the use of violence in politics. The increased acceptance of violence may not be surprising given the limited scope for our study participants to affect change in Kenya's fledgling democracy; their greater dissatisfaction with, but no greater participation in (or belief in) democracy; and the finding that ethnic identification does not diminish with education. It also resonates with the fact that violence has been a central feature of political change in Kenya, from the anti-colonial Mau Mau uprising in the 1950s to the contested 2007 presidential election.

Going back to the theoretical perspectives outlined above, our findings appear equally inconsistent with the view that education promotes “modern” attitudes and with the claim that education instills acceptance of existing authorities. Rather the results suggest that education promotes a desire for personal autonomy among the marginalized young rural women that we study, but that this desire is not necessarily expressed through democratic means. In the political realm we see evidence of greater knowledge and raised expectations, but not of actual political involvement, and perhaps as a result, we document greater frustration and acceptance of political violence. This suggests that while young women may be less willing to accept violence directed against them by others, this does not stem from an abstract rejection of violence, and that indeed they may be more willing to accept political violence as necessary in some circumstances.

Methodologically, it is useful to know whether a non-experimental analysis would yield similar results. We find large differences between experimental IV estimates derived from the randomized design and non-experimental correlations, suggesting that non-experimental correlations cannot be interpreted as causal impacts of education. Under a simple model of bi-directional causality, our findings can be interpreted as suggesting that those who are less willing to accept authority are likely to accumulate considerably less human capital in Kenya.

To further get at the channels of impact, and in particular at whether these effects are directly due to receiving additional education through the merit-based scholarship competition, or to the money or prestige garnered by winning a scholarship itself, we examine a subset of girls who had very low ex ante odds of winning the scholarship based on their baseline test scores. As shown in Kremer et al. (2009) these girls also experience test score gains through the program. The main impacts on social and political variables also hold in this subsample, supporting the view that the effects are due to human capital gains rather than winning an award per se.

The paper is organized as follows: section 2 provides background on the setting; section 3 summarizes the data and empirical methods, and program impacts on human capital; section 4 reports results on household autonomy; Section 5 reports political attitude impacts; Section 6 develops a framework for analyzing the interaction between the willingness to accept authority and education, and compares experimental to non-experimental estimates; Section 7 examines channels of impact, and the final section concludes.

2. Background

2.1 The Study Setting

The Kenyan girls in the setting we study are socially marginalized and politically disempowered. They are female and young in a society where older males hold authority; residents of a rural backwater; and overwhelmingly from the quite politically weak Luhya and Teso ethnic groups. Women are also disadvantaged in Kenyan society more broadly. Female participation in parliament is low, at just 10% (Gathigah 2010). Spousal violence is also widespread, with 75% of women claiming abuse in recent reports (FIDA 2008). In the household realm, Kenyan girls are generally subject to their parents' – and in particular their father's – authority until they leave the household, and then are often subject to their husband's authority. Traditionally, parents played a central role in selecting husbands for their daughters and received bridewealth from the groom in the form of livestock (Government of Kenya 1986). However, while this system remains normative in some sense, in practice it has largely broken down in favor of a system in which young people “elope” with partners of their choice, and bridewealth is either never paid or is paid later (if the groom accumulates sufficient resources). In rural areas, unmarried women, unlike men, are not permitted to build their own separate house on a family compound and are

thus more directly subject to parental authority. Many young women therefore marry at an early age, at least in part to escape parental control.

Our study area is Busia, a district in western Kenya with below average income levels.⁴ Ethnic Luhyas comprise roughly 80% of the sample, with some Luos and Tesos (Table 1). Although Luhyas are among Kenya's most numerous groups, Luhya politicians have been unsuccessful in the competition for the presidency. Kenya's first President was Jomo Kenyatta, from the Kikuyu ethnic group, its second (Daniel arap Moi) was a Kalenjin, and the third, Mwai Kibaki, a Kikuyu. There is a widespread perception in Busia that these presidents' ethnic groups wielded disproportionate power during their rule, to the detriment of western Kenya.

Like many African countries, Kenya became a de facto single-party state shortly after independence in the 1960s and underwent democratic reforms in the early 1990s at the end of the Cold War (Barkan 1994). The Kenyan African National Union retained power until 2002, when a multi-ethnic coalition led by Mwai Kibaki defeated Moi's handpicked candidate. When Kibaki in 2005 proposed a new constitution seen as preserving a strong "imperial" presidency and favoring Kikuyu elites, voters overwhelmingly rejected it, including voters in our study area. Thus during 2005-2007 when our survey data collection took place, Kenya had an imperfect but reasonably competitive multi-party democracy. This is reflected in its Freedom House score of 3 during the period (on a scale of 1 to 7, with 1 being most democratic, appendix figure A1).

Yet the fragility of this democracy was starkly demonstrated in late 2007 and early 2008 (after the end of surveying). The incumbent was widely seen as having stolen the presidential election (Gibson and Long 2009). Individuals living in the study area overwhelmingly supported

⁴ 62% of Busia households fall below the poverty line compared to 41% nationally. As Kenyan per capita income is somewhat higher than the sub-Saharan African average (if South Africa is excluded), the fact that Busia is somewhat poorer than the Kenyan average arguably makes the district more representative of rural Africa as a whole.

the challenger Raila Odinga. Protests against the incumbent's declaration of victory became violent, ethnic clashes broke out across the country, and some observers believed Kenya was on the verge of civil war. Due to a combination of internal and external pressure, the incumbent eventually acceded to a power-sharing agreement with the challenger.

Since independence from Britain in 1963, Kenya has experienced massive increases in education, with adult literacy rising from just 32% in 1970 to 87% today (UNDP 1993, 2010). Although there are, huge gender disparities in other areas of Kenyan society, census data indicate that girls' enrollment has increased at a faster rate than boys' at lower levels of education (appendix figure A1), and there is now gender parity in primary school enrollment (UNDP 2009). Kenya's increase in education is dramatic, but many other low-income countries have also seen similar gains. To illustrate, in 1960 the average working-age person in low-income countries had 1.8 years of education, while by 2000 they had over five (Barro and Lee 2001).

As background, the Kenyan school system is quite authoritarian. Corporal punishment is commonplace (Human Rights Watch 1999), and challenges to teacher authority are not tolerated. Student prefects help teachers maintain classroom control, students wear uniforms, learning is by rote, and creativity and critical thinking in the classroom are not highly prized. It is unlikely that Kenyan schools would be viewed as instruments of liberation in the sense of Freire (1972).

Despite this emphasis on discipline and rote learning, the Kenyan school curriculum officially attempts to promote democratic values, and a national Kenyan - as opposed to ethnic - identity.⁵ For example, two chapters of "History and Government", a first year (Form 1) high school text approved by the Ministry of Education, are entitled "Citizenship" and "National Integration". It states: "Citizens have the responsibility of participating in the democratic process through which our leaders are elected." Under "Elements of Good Citizenship" it lists

⁵ We focus on Form 1 since the GSP increased secondary school participation, as shown below.

nationalism, explaining, “a nationalist works for one’s country and devotes oneself to serving the nation and the unity of his/her nation. Thus a Kenyan nationalist is devoted to Kenya and seeks to unite fellow countrymen above interests of race, tribe, religion or parochialism.”

Primary school in Kenya runs through grade 8, after which students take a national exam and those with sufficiently high scores continue on to secondary school. Historically, attending secondary school has been associated with higher social status, since secondary schools are selective, much more expensive than primary school, and many are boarding institutions.

2.2 The Girls Scholarship Program (GSP)

In March 2001, ICS, a Dutch non-profit organization, introduced a merit scholarship competition in 34 primary schools in the western Kenya district of Busia, with 35 other schools serving as the control group. This Girls Scholarship Program (GSP) provided an award to grade 6 girls in treatment schools whose performance on the government’s standardized end-of-year exam placed them in the top 15% (among all girls in the treatment schools). The award included a grant of 500 KSh (or roughly US\$6.40 at the time) paid to the girl’s school to cover school fees, and a cash grant of 1000 KSh (or US\$12.80) paid to the girl’s family to pay for other school expenses, in each of the two years following the competition, covering the last two years of primary school. Thus the total award for winners was valued at nearly US\$38 over two years, an amount comparable to the large-scale girls’ scholarship programs in other less developed countries mentioned above. For comparison, Kenya’s annual per capita income was roughly US\$400 in 2001. The awards were presented at local community assemblies.⁶

⁶ Although primary school fees were eliminated in 2003, certain expenses remained, and Duflo et al. (2006) and Evans et al. (2010) find that these remained an important barrier to participation.

The randomization into treatment and control schools was carried out using a computer random number generator, after first stratifying by administrative division and participation in a previous NGO program (that distributed flip-charts as classroom learning aids) also carried out by ICS. All 34 schools invited to participate chose to take part in the project. GSP treatment and control schools in Busia are similar on observable baseline characteristics (Table 1, Panel A), indicating that the randomization worked in generating similar groups; the first column in Table 1 presents the mean (and s.d.) in the control group, and the second column presents the coefficient estimate on the treatment indicator variable. The NGO did not conduct other activities at these schools during the study so we can attribute impacts to the GSP. There was a parallel evaluation in neighboring Teso district that is discussed in Kremer et al (2009). However, since the Teso sample was far smaller, had considerable attrition during the original study, and did not experience an obvious increase in human capital as a result of the program, the follow-up surveys were only conducted in Busia district and we thus focus on the Busia program in this paper.

The Kenyan school year runs on the calendar year, from January to December (Appendix figure A2). The program was publicly announced early in 2001. The competition was carried out a second time in treatment schools in 2002 among students in grade 6 in that year (and eligibility was restricted to those girls who had been initially enrolled in grade 5 in the same schools in 2001, to eliminate the possibility of selective transfers into treatment schools). There were thus two cohorts in the program, those in grade 6 in 2001 and those in grade 6 in 2002.

The average 2001-2002 treatment effect of GSP incentives in Busia district was nearly 0.3 standard deviations, and the 2001-2002 estimated effect in our follow-up subsample was 0.34 standard deviations (Table 1, Panel B; Kremer et al 2009)⁷. These are considered large gains in

⁷ The effect size in the follow-up sample is similar to that in the original sample, but it is slightly less statistically significant in the follow-up sample because of the reduced sample size.

the education literature. As is standard, test scores are normalized such that the control group distribution has a mean of zero and a standard deviation of one⁸. Importantly, the program generated higher test scores in treatment schools both for those who were most likely to benefit from the scholarship – girls with high baseline scores – but also for those who were unlikely to win, and the hypothesis that treatment effects were the same throughout the baseline test score distribution cannot be rejected. The explanation for the gains among low-performing girls offered in Kremer et al. (2009) is that the incentives led to improved teacher performance and student effort that generated positive classroom externalities (including for boys, who also show moderate gains despite being ineligible for the scholarship). This is supported by the substantial increases in both pupil and teacher attendance in treatment schools (Table 1, Panel B).

3. Data Collection, Estimation Strategy, and Program Impact on Human Capital

3.1 Follow-up Data Collection (2005-2007)

To assess the persistence of these academic gains and other long-term impacts of the program, a follow-up survey was undertaken from October 2005 through February 2007, approximately four to five years after the GSP competitions, when sample individuals were young women between 17 and 21 years of age. This involved tracking down the two cohorts of girls from both treatment and control schools. Because the original intervention was aimed at girls, and the research budget was limited, the follow-up data collection sample was limited to females.

Respondents were followed by the survey team wherever they moved in Kenya or Uganda using an approach similar to Baird et al. (2011). Overall, 84.0% of respondents were effectively located by the field team, with 81.6% surveyed while 2.4% were either deceased,

⁸ While it is also possible to normalize separately within cohorts, here we normalized the entire sample together, and include cohort controls in all regressions, to simplify the exposition.

refused to participate, or were found but were unable to be surveyed (Table 2, Panel A).⁹ The effective survey rate among those still alive is 81.6%. These are quite high rates for a mobile young adult population, and one that places this project among the more successful longitudinal survey efforts in a low-income country (Thomas et al, 2001, 2010). The final analysis sample with baseline survey data, 2001 or 2002 test scores, and follow-up data includes 1,387 girls.

There are no statistically significant differences in follow-up survey attrition across the treatment and control groups (Table 2, Panel A). We also do not find that survey attrition over time is significantly related to the baseline 2000 test score, the presence of a toilet, iron roof, or mosquito net in the home compound, time spent on chores and schoolwork, schooling attitudes, and number of siblings, nor are these characteristics significant predictors of survey attrition when interacted with the treatment indicator (not shown), evidence that differential survey attrition across the treatment and control groups does not appear to be a leading concern. As with the baseline survey data, the follow-up subsample is balanced along observed baseline individual and household characteristics across the treatment versus control samples (Table 1, Panel A).

Once respondents were located, enumerators administered short tests on English vocabulary, Swahili vocabulary, arithmetic, reading, and spatial reasoning (using a Raven's matrix module). The survey also included questions on schooling, marriage, fertility, migration, and social and political attitudes. To the extent possible, these latter questions were adapted from questions in the World Values Survey and Afrobarometer Surveys, building on Bratton et al. (2005), Logan and Bratton (2006), and Weakliem (2002), while some new questions were

⁹ During an initial phase, all individuals were tracked. In early 2007, a random subsample containing one fifth of the remaining unfound respondents was drawn to be tracked "intensively" (in time and travel costs). We re-weight the "intensive" sample to maintain representativeness. As a result, all figures are "effective" tracking rates (ETR), calculated as a fraction of those found, or not found but searched for during intensive tracking, with appropriate weights. The ETR is a function of the regular phase tracking rate (RTR) and intensive tracking rate (ITR), $ETR = RTR + (1 - RTR) * ITR$, where RTR is 47.5% and ITR is 65.2%. This is related to the approach in the U.S. Moving to Opportunity study (Kling *et al.* 2007, Orr *et al.* 2003).

developed specifically for the Kenyan context. In the tables, we denote questions drawn from the World Value Survey with “WVS” and Afrobarometer with “AFB”. Indicator variables are denoted “0-1”. Questions asked on a four- or five-point scale were rescaled so the lowest (highest) answer takes on a value of zero (one), and these are denoted “0 to 1” in the tables.

3.2 Estimation Strategy

We first estimate the impact of the Girls Scholarship Program on outcomes (*POLIT*) in a reduced form specification, by regressing *POLIT* on the indicator variable for GSP treatment schools (*TREAT*). We also include some additional control variables (denoted *X*) – an indicator for student cohort, age at time of the survey, the mean pre-program (2000) school test score, parent educational attainment, and month of the survey – to address any minor baseline differences between the treatment and controls schools that exist despite the randomization. Since the randomization successfully produced treatment and control groups balanced along most characteristics (Table 1), the inclusion of controls does not meaningfully alter treatment effect estimates but sometimes improve statistical precision. Equation 1 presents the reduced form:

$$(eqn. 1) \quad POLIT_{is} = \alpha + \beta TREAT_s + X_{is}'\gamma + \varepsilon_{is}$$

where individual *i* is in school *s*. The coefficient of interest is β . Disturbance terms are clustered by school. We employ OLS for both continuous and limited dependent variables in part to facilitate the use of the mean effects approach (described below), but results for indicator variable outcomes are nearly unchanged in probit specifications (not shown).

The GSP program could potentially affect political attitudes through human capital or through other impacts of winning a scholarship, such as the cash transfers and public recognition received by winners. Recall that girls in the top 15% in the treatment schools won scholarships.

We find that girls with normalized baseline test scores less than +2 standard deviations have just an 8% chance of winning the scholarship, as compared to a 58% likelihood among girls with baseline scores above +2 s.d.¹⁰ These treatment school girls with low baseline scores are thus unlikely to receive the prize and recognition, but as shown in Kremer et al (2009), they nonetheless attended school more often and have test score gains. As we discuss in section 6 below, estimating treatment effects in this subgroup sheds light on the relative importance of the “public recognition” and “human capital” channels, suggesting that the human capital channel is much more important.

To convey a sense of the magnitude of the human capital effects, and to allow comparison of our experimental estimates with non-experimental estimates, we report IV estimates of the impact of human capital on political attitudes by using the follow-up test score measure as the endogenous variable, denoted H . We view the test score as an attractive summary measure of human capital. We focus on the normalized average test score across all subjects (arithmetic, English, Swahili, reading, spatial reasoning) as our best measure of overall skills and human capital, where the normalized variable is mean zero with a standard deviation of one (the common approach in the education literature). The first stage equation is:

$$(eqn. 2) \quad H_{is} = a_1 + b_1 TREAT_s + X_{is}'c_1 + e_{1,is} .$$

The predicted values from this regression allow us to estimate human capital impacts in an instrumental variable two-stage least squares (IV-2SLS) specification, where the second stage is:

$$(eqn. 3) \quad POLIT_{is} = a_2 + b_2 H_{is} + X_{is}'c_2 + e_{2,is} .$$

For comparison with observational studies, we also provide similar estimates but running the second stage equation (equation 3) using the actual test score rather than the predicted score,

¹⁰ Approximately 2% score more than 2 s.d. above the mean of the normalized distribution.

while restricting attention to the control group to isolate non-experimental variation. For each set of outcomes, we then compare the analogous OLS and IV coefficient estimates, and compute the statistical significance of the difference between them using the method in Hausman (1978).

For various categories of outcomes, we estimate the “mean effect” of either GSP treatment or human capital on each set of outcomes following Kling et al. (2007). The groupings of related outcome variables are denoted by Y_k , $k = 1, \dots, K$. We standardize each outcome by subtracting the mean and dividing by the standard deviation of the outcome variable among the GSP control group, such that the control group mean (s.d.) is zero (one). The standardized variables are denoted Y_k^* . With these, we form $Y^* = \sum_k Y_k^* / K$, a single index of outcomes, and regress this index on *TREAT* (as in equation 1) or on *H* (equation 3). The resulting coefficient estimate is called the mean effect size and, due to the normalization, it captures the average impact in terms of standard deviations of the outcome variables. This normalization facilitates comparison of impact magnitudes across outcomes, as well as possibly across studies.

3.3 Impacts on Human Capital

We first discuss the program’s impact on human capital. The human capital gains that occurred as girls competed for scholarships persisted, with significant test score gains in treatment schools relative to the control group (Table 2, Panel B). In specifications analogous to the first stage regression (equation 2), test scores improved in all five subjects in the 2005-2007 follow-up survey, with statistically significant gains in four subjects. The mean effect across all tests is 0.208 standard deviations (s.e. 0.092), which is significant at 95% confidence.

Test score impacts are nearly unchanged among two subgroups with relatively low chances of actually winning a GSP award, namely those with baseline normalized test scores less

than +2 s.d., at 0.145 s.d. (s.e. 0.082), and among those in schools predicted to have five or fewer GSP winners, at 0.177 s.d. (s.e. 0.081). The predicted number of GSP winners is estimated in treatment schools by regressing the actual number of winners on quantiles of the baseline test score distribution (among students in that school); the predicted number of winners is then assigned to both treatment and control schools. The persistent human capital gains we document thus appear to be driven by competing for a scholarship rather than winning.

Beyond test scores, several other education measures also improved in the treatment group: they were 8.7 percentage points (s.e. 4.1) more likely to have attended at least some secondary school, an increase of nearly a third on the control rate of 30%. GSP treatment individuals were also 7.9 percentage points more likely to be enrolled in school, an increase of 15 percent relative to the control group, where 52% were enrolled. Since more than half the sample is still in school, the attainment data is severely censored, and perhaps in part as a result, the program is estimated to have a positive but not significant impact on educational attainment.

4. Impact on Autonomy within the Household

As noted in the introduction, many argue that education will empower young women to address broader gender inequalities (Lloyd 2009). Others have argued that women in most societies face relatively less disadvantage in education than other spheres, and that efforts to reduce gender imbalances could be more productive elsewhere (World Economic Forum 2010). Modernization theory would also imply that education should erode support for traditional gender roles.

We do not find significant changes in views regarding equal rights for women versus traditional gender roles (Table 3). Most respondents already strongly support gender equality, with average support for traditional roles (namely, the statement that “Women have always been

subject to traditional laws and customs and should remain so”, as opposed to “Women should have equal rights and receive the same treatment as men do”) at only 0.17 on a normalized zero to one scale. Treatment reduces this by 0.01 but the effect is not significant.

In contrast, treatment produces changes on two concrete issues limiting female autonomy that are likely to be personally relevant for many respondents: domestic violence and arranged marriages. Again on a zero to one scale, treatment leads to a 0.068 reduction (s.e. 0.024) in support for the claim that “Men can beat their wives and children if they misbehave” as opposed to the statement that “No one has the right to use physical violence against anyone else” (Table 3), a reduction of roughly one quarter on average support of 0.25 in the control group.

We next consider marriage patterns, and divide marriages into “arranged marriages”, where parents played a role in spouse choice, and “elopements”, where they did not. Roughly 20% of respondents were married by the follow-up survey, with three quarters being elopements in the control group, and one quarter, or 4.2%, arranged. However, this latter figure falls by more than half, or 2.4 percentage points (s.e. 1.3) in the treatment group (Table 3), and this effect is significant at 90% confidence. In contrast, we find no change in the likelihood of elopement, suggesting that the reduction in marriages with family involvement does not reflect a broader trend in marriage rates but rather a shift in power from parents to their daughters. When we use a mean effects approach to look at a summary measure of lack of autonomy combining the two measures of support for traditional gender norms with the measure of arranged marriage we find a significant reduction of -0.181 standard deviations (s.e. 0.077).

There are no significant GSP program impacts on fertility, or on knowledge of contraception, age of marriage, or basic spouse characteristics (not shown) although statistical power was limited in some cases given the limited proportion of married women in the sample.

The results on young women's greater control over marriage patterns and stated opposition to domestic violence are relevant not only from a policy perspective but also shed light on the theories described in the introduction. They are inconsistent with the idea that education simply instills greater acceptance of authority, but are compatible with both modernization theory and with the view that education can empower disadvantaged groups. While these results are inconsistent with the simplest version of the theory that education is a tool for entrenching existing power structures, they are arguably consistent with a more nuanced view, in which Kenya's education system is controlled not by traditional village elites, but rather by a state seeking to instill certain "modern" values that weaken pre-capitalist "fetters" on female labor. To distinguish between these two perspectives, we next consider impacts on political and social attitudes beyond the household.

5. Impacts on Political and Social Attitudes

In subsection 5.1, we first find little support for a direct impact of education in promoting "modern" values. Subsection 5.2 reports evidence that runs counter to the view that education simply instills acceptance of existing authority, but is consistent with the notion that education promotes an awakening of political consciousness that is arguably the first step to actual empowerment, specifically through changes in media consumption, increased political knowledge and dissatisfaction with authority. However, subsection 5.3 suggests that rather than translating into increased participation in politics or community affairs, or in social capital, these shifts generate greater expressed willingness to accept the use of violence in politics.

5.1 Impact on "Modern" Attitudes

Impact on Ethnic and Religious Attitudes

From a theoretical perspective, the impact of schooling on feelings of ethnic and religious identity is unclear. Modernization theory predicts education will reduce particularistic ethnic attachments and promote secularism. Yet other scholars argue that in pre-colonial times the most important groups were local “sub-ethnic” kinship groups, with ethnic boundaries fairly fluid (Shetler 2010) and thus see contemporary notions of tribal identity (among groups such as the Luhya in our sample) as essentially modern. In this second view, education could potentially promote political mobilization along ethnic lines, although recall from the background section that the Kenyan curriculum seeks to promote nationalism. The standard Kenyan school curriculum also includes Christian and Islamic religious education, and schools often sponsor religious youth groups, so education could also potentially strengthen, rather than weaken, religious attachment.

We find no evidence that increased schooling weakened ethnic attachments, and if anything the program may have strengthened them. In general, respondents were strongly attached to their own ethnic group, with only 11% of the control group not reporting ethnicity as “very important” to them. GSP treatment reduced this by 3.3 percentage points, or approximately 30 percent, although the difference is not significant (Table 4, Panel A).

We next exploit the fact that ethnic identification is more salient closer to contested democratic elections in Africa (Eifert et al. 2010), likely because ethnic electoral appeals are widespread. We find that among the 43 respondents surveyed in early 2007 – a national election year – all but one stated that their ethnic affiliation was “very important” to them, far above the average among those surveyed earlier; this “censoring” of responses suggests that our survey instrument was insufficiently sensitive to finer distinctions in the degree of ethnic feeling during

election periods. We next focus on respondents surveyed in 2005-2006, before the lead-up to the election, and find that treatment reduced the proportion who did not report ethnicity as “very important” by a significant 4.2 percentage points (s.e. 2.1, Table 4), a 40 percent drop. In other words, in non-election years the program appears to heighten feelings of ethnic identification.

A related set of questions on ethnicity ask whether respondents trust members of their own ethnic group (tribe) as well as members of other groups. Point estimates suggest treatment slightly increased trust in co-ethnics and reduced trust in members of other ethnic groups, but the effects were not significant at traditional confidence levels (not shown). We similarly find no program impacts on migration out of the local area, which might have brought people into closer contact with other ethnic groups thus leading to more inclusive ethnic attitudes, and which might itself be taken as reflecting more inclusive attitudes (not shown).

We also find no evidence for the modernization theory hypothesis that education leads to secularization as measured by changes in the reported importance of religion (Table 4, Panel A), although strong religiosity is nearly universal, complicating inference. There is more variation in participation in prayer groups, but there, too, impacts are small and not statistically significant.¹¹

Impact on Democratic Attitudes

Respondents were asked whether they agreed with a series of statements about the ideal organization of government and society, where responses were given on a scale from 1 to 5, ranging from “strongly disagree” to “strongly agree”, and these were then normalized to range from zero to one, as above. To illustrate, respondents were asked whether they agreed with: “We should choose our leaders in this country through regular, open and honest elections”, and “Democracy is preferable to any other kind of government”, among others.

¹¹ Glaeser and Sacerdote (2001) show that U.S. religious attendance rises with education.

There are no significant GSP program impacts on any of the seven measures of democratic attitudes (Table 4, Panel B), and even combining all of these effects together in a single index, the mean effect is small and not statistically significant, at 0.058 standard deviations (s.e. 0.089). These results are particularly interesting in light of the positive cross-sectional correlations between individual schooling and democratic attitudes documented in the existing literature, including several African studies (Bratton et al. 2005, Logan and Bratton 2006, Mattes and Bratton 2007). While some have argued that investments in education may be an effective way to promote democracy and reduce political extremism, our results suggest that, if there is any such causal relationship, it may not be a direct one.

Overall, we find no support for the hypothesis that education promotes “modern” attitudes including weakening of ethnic attachments, secularization, or greater belief in democracy. These findings partially alleviate concerns that more educated Kenyans are simply providing the “right” survey answers due to social desirability bias. In particular, to the extent that support for democratic institutions is the “politically correct” response in Kenya, we might have expected to find a strongly positive relationship between human capital and support for democracy, but we find no such relationship in our data. We next examine whether education promotes empowerment in the political realm, consistent with the household autonomy results.

5.2 Impact on Political Knowledge and Satisfaction

The first hint that the program affected political outcomes is its impact on media consumption. Individuals in treatment schools report significantly less time listening to radio – which in Kenya has largely music and entertainment content – and more days reading newspapers, which report extensively on national politics (Table 5, Panel A). Respondents were also much more likely to

identify a favorite newspaper, with an increase of 9.6 percentage points (s.e. 3.7) on a base of 66.6% in the control, and in particular, there was a large increase of 10.5 points (s.e. 3.5) in choosing the *Daily Nation*, arguably Kenya's most authoritative English language daily, as the favorite news source on a base of 30.5%. The increased affinity for the *Daily Nation* almost certainly in part reflects the treatment group's improved English skills, but may also proxy for changing partisan or ideological tastes and affect the quality of political information consumed.

Indeed, the program had a large impact on objective political knowledge. Respondents were asked to name Kenya's President, Vice President, Education Minister, and Health Minister and Uganda's President. Virtually all respondents could name the President, but the program increased the likelihood that respondents could name other officials, and the impacts are significant for naming the Health minister and the President of Uganda (not shown). The mean effect across all five questions is 0.203 s.d. and significant at 95% confidence (Table 5, Panel B).

Turning to measures of satisfaction with political authorities, the GSP treatment group shows less deference to authority in the abstract and expresses less satisfaction with Kenya's government, economy, democracy, and current authorities (Table 5, Panel C). In particular, respondents were significantly less likely to agree with the statement "We should show more respect for authority" and more likely to support the statement "As citizens, we should be more active in questioning the actions of our leaders", with a change of 0.076 in the normalized index, relative to a control group mean of 0.53. When asked whether the quality of government and the economy were better than two years ago, treatment reduced positive assessments by 5.4 and 5.8 percentage points, respectively, both significant at 90%. This despite the fact that Kenya's performance was good relative to historical benchmarks, with GDP growth of 6% in 2006-2007 and reasonably democratic politics. In control schools, satisfaction with Kenyan democracy was

0.74 (on a normalized 0 to 1 index), and treatment decreased this by 0.048 (s.e. 0.017). Taken together, expressed satisfaction falls in the treatment group by 0.239 standard deviations (significant at 99%). This sharp reduction in satisfaction with political authority parallels the rejection of male and parental authority in the household (Table 3).

There is no evidence that the program affected overall personal happiness, as captured by agreement with “taking everything together, the respondent is very happy” (Table 5, Panel C), and thus the political dissatisfaction questions do not simply reflect broader life dissatisfaction. Note that there is little consensus on the empirical relationship between education and happiness using OECD data (Frey and Stutzer 2002, Clark and Oswald 1996).

5.3 Empowered for What? Perceived Political Efficacy, Participation, and Violence

The increased knowledge and reduced satisfaction with authority generated by the program does not seem to translate into greater perceived political efficacy or more participation in politics or community affairs. Instead, there is increased acceptance of the use of violence in politics.

Impact on Perceived Political Efficacy

A large majority of respondents agreed with the statements “Politics and government sometimes seem so complicated that you can’t really understand what’s going on” and “This world is run by a few people in power, and there is not much that someone like me can do about it”, and GSP treatment does not lead to a significant change from the low levels of perceived political efficacy in the control group (Table 6, Panel A). Indeed, Kenyan politics at the time of the survey was characterized by Byzantine backroom deal-making among ethnically-based political leaders, many of whom were the sons of an earlier generation of leaders. While treatment made the

young women in the sample less satisfied with the political situation (Table 5), it apparently did not lead to any illusions about their personal ability to change the situation.

Impact on Political and Community Participation

The GSP did not increase interest or participation in politics or community affairs. In particular, 26% of control group respondents reported being interested in public affairs, versus 23% in treatment, although this difference is not significant (Table 6, Panel A). Similarly, treatment respondents of voting age were slightly less likely to report intending to vote in the next presidential election (-2.6 percentage points, s.e. 4.5), but the effect is not significant.

While some argue that education enhances civic participation (e.g., Glaeser et al. 2002), we do not find evidence for this. The survey gathered information on membership in 10 common types of community groups (women's groups, credit groups, etc.), with average membership in 1.41 groups in the control. There is no treatment effect on membership (Table 6, Panel B). The program also had no impact on trust, in the standard question "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?".

Attitudes Toward the Use of Violence in Politics

Although Kenya has never experienced a civil war, it has a long record of ethnic violence around elections, going back to the first multi-party polls in 1992. As noted above, some observers have argued that education tends to reduce political violence while others see it as raising expectations that, if unmet, could increase individuals' propensity towards violence (Davies 1974).

We find that the GSP program led to greater expressed tolerance for political violence. Treatment individuals are significantly more likely to think violence is sometimes justified in

politics (estimate 0.030, s.e. 0.012 in Table 6, panel C), off a base of 0.47 in the control group. It seems plausible that education increases acceptance of violence because it increases respondents' political knowledge and reduces satisfaction with the status quo (as shown above), while apparently not simultaneously increasing their faith in their ability to achieve change through democratic means. Since they are no more committed to democratic values as a result of education and, if anything, more ethnically identified – the key dimension of social conflict in Kenya – it may not be surprising that education increases support for political violence.

From one perspective, there appears to be a tension between the estimated impacts of education on views about the legitimacy of political violence and on domestic violence in our sample. However, both findings are consistent with the view that education reduces the knee-jerk acceptance of existing authorities, both at home and in the broader national political arena.

6. Estimating the bi-directional relationship between schooling and attitudes

As discussed in the introduction, some argue that education indoctrinates people to accept existing authority whereas others argue that it can empower people to challenge authority. Our results, taking advantage of experimental variation, provide considerable support for the hypothesis that education reduces willingness to accept authority. In this section, we argue that our data are consistent with a bi-directional relationship between education and willingness to accept authority – in which those who are to accept authority are more likely to withdraw from education but education itself leads to questioning of authority – and that such a relationship could lead non-experimental analyses to understate the extent to which education leads to less acceptance of authority.

In Section 6.1 we present a simple model in which there is a bi-directional relationship between education and willingness to accept authority, and show that in this model standard approaches to estimate the impact of education on acceptance of authority using regressions in non-experimental data will yield biased estimates if – as is particularly plausible in schooling systems such as those of Kenya, which do not allow much room for student autonomy – those who are less willing to accept authority, are more likely to drop out of school. In section 6.2, we use the non-experimental variation in our data to estimate the extent of reverse causality, in which willingness to accept authority allows people to obtain more education, and the resulting bias that would be created in non-experimental estimates in our context.

6.1 A Model of the Relationship between Schooling and Willingness to Accept Authority

We consider a framework in which education can affect willingness to accept authority, and willingness to accept authority can affect schooling decisions. Suppose that:

$$(eqn. 4) \quad R_1 = R_0 + \gamma H,$$

where R_1 is the willingness to accept authority as an adult and R_0 is willingness to accept authority as a child. H is a measure of human capital investment. γ is thus the causal impact of education on willingness to accept authority. Suppose household i chooses human capital to trade off the benefits versus the net utility and financial cost of education:

$$(eqn. 5) \quad U_i = B(H_i) - C(H_i).$$

We assume that being in a school that has the merit scholarship program raises the benefit of human capital (B), because those who score well on the exam will receive financial benefits. We also assume that individuals may differ in the benefit of education, for example, due to differences in the non-pecuniary benefit of education or differences among children in aptitude

or attitudes toward school. One component of this may be difference in willingness to accept authority, and we allow for the possibility that R_0 may also affect the cost of education. Recall that in our context, being in school requires acceptance of a great deal of authority, including acceptance of the right of teachers to impose work (such as cleaning the classroom or carrying items for the teachers), orders from student prefects, and corporal punishment. Students who are unwilling to accept the fairly rigid discipline associated with Kenyan education often have to leave school, with perhaps the most notable example for non-Kenyans being Barack Obama Sr., who, despite his stellar grades, was expelled from an elite secondary school for what was deemed “disrespectful” behavior towards his teachers (Jacobs 2011).

For simplicity, we take the benefits to be linear in the amount of education (H) and the cost of education to be quadratic in H . Thus, we specialize to the case in which for household i :

$$(eqn. 6) \quad U_i = H_i(B_{0,i} + \beta_1 R_{0,i} + \beta_2 M_i) - H_i^2,$$

where $R_{0,i}$ indicates a child’s willingness to accept authority, M_i is an indicator for attending a merit scholarship program school, and B_0 indicates an individual’s other benefits of education (per year of schooling). We assume that child willingness to accept authority is distributed normally with mean $\overline{R_0}$ and variance σ_R^2 and that the benefits are distributed normally with mean $\overline{B_0}$ and variance σ_B^2 . These are distributed independently for simplicity (although this could easily be weakened). The optimal level of human capital investment for household i , H_i^* is determined by the first order condition:

$$(eqn. 7) \quad H_i^* = (B_{0,i} + \beta_1 R_{0,i} + \beta_2 M_i)/2.$$

This implies that, ceteris paribus, for each increase of one unit in a child’s willingness to accept authority, there is an increase of $\beta_1/2$ in the human capital investment optimally chosen.

It is straightforward to see that a regression of willingness to accept authority on education will not yield a causal estimate of the impact of education in this environment with bidirectional causality. In the case where there is no merit scholarship, an OLS regression of willingness to accept authority on human capital will yield the following coefficient (see the model appendix for the algebraic details):

$$(eqn. 8) \quad \gamma_{OLS} = \frac{Cov(R_{1,i}, H_i^*)}{Var(H_i^*)} = \gamma + \frac{2\beta_1\sigma_R^2}{\sigma_B^2 + \beta_1\sigma_R^2}$$

In this case, the term $\frac{2\beta_1\sigma_R^2}{\sigma_B^2 + \beta_1\sigma_R^2}$ represents the bias in the OLS estimate, and from equation 8, we can see that the OLS estimator is biased upward, leading to an erroneous view that education leads to a more positive impact on the willingness to accept authority than is actually the case.

Now consider the case in which a merit scholarship program is introduced in a randomly chosen subset of schools. By construction M_i is orthogonal to $R_{0,i}$ and $B_{0,i}$. Using the merit scholarship as an instrument for human capital, H_i , would generate an unbiased estimates of the causal impact since merit scholarships are independent of initial willingness to accept authority and of other determinants of returns to education:

$$(eqn. 9) \quad \gamma_{IV} = \frac{Cov(R_{1,i}, M_i)}{Cov(H_i^*, M_i)} = \frac{\gamma\left(\frac{\beta_2}{2}\right)Var(M_i)}{\left(\frac{\beta_2}{2}\right)Var(M_i)} = \gamma$$

Taking the difference between the OLS and IV estimates allows us to solve for β_1 in terms of known parameters (algebraic details are once again in the appendix):

$$(eqn. 10) \quad \beta_1 = 1 / \left(\frac{\sigma_{R_1}^2 - \gamma_{IV}^2 \sigma_H^2}{2(\gamma_{OLS} - \gamma_{IV})\sigma_H^2} - \gamma_{IV} \right)$$

As shown in equation 10, we can solve for β_1 using estimates found in our data.

6.2 Comparing the Experimental and Non-experimental Relationships

To empirically compare experimental and non-experimental results, the extent to which willingness to accept authority affects education, and the extent to which education affects acceptance of authority, we first reproduce the main reduced form impacts (from Tables 3-6) in column 1 of Table 7, then present IV estimates in column 2 and non-experimental OLS estimates (for the control group) in column 3. Column 4 presents p-values on the Hausman test of equality of the IV and OLS estimates.

For the lack of autonomy index, the IV coefficient estimate implies that a one standard deviation increase in the normalized test score (in the 2005-2007 survey) is associated with an effect of -0.872 s.d. (s.e. 0.493), a large effect. To put this in context, a one standard deviation test score increase in this population is more than the gain observed (in the cross-section) by advancing by one primary school grade. This contrasts with a non-experimental OLS estimate of just -0.286, and thus the IV estimate is nearly three times as large as OLS. We reject equality of the experimental and non-experimental estimates at nearly 90% confidence (p-value=0.11).

The IV estimate for the impact of education on satisfaction with authority index is large and negative (-1.115, s.e. 0.493) and significant at 95% confidence, while the OLS estimate is just -0.177, and the difference between the two is significant at 99% confidence. This divergence mirrors the findings above for the lack of autonomy index: both of these measures capture opposition to existing authorities, one in the home and the other in the broader political arena. The difference between the IV and OLS estimates is substantively large: while both have the same sign, the IV estimate is six times larger in magnitude and leads to different conclusions about the relationship between education and the willingness to accept authority.

Under the model sketched above it is possible to solve for the impact of willingness to accept authority on education. Since both test scores and the satisfaction of authority index are normalized so that the variance is 1:

$$\beta_1 = 1/\left(\frac{\sigma_{R1}^2 - \gamma_{IV}\sigma_H^2}{2(\gamma_{OLS} - \gamma_{IV})\sigma_H^2} - \gamma_{IV}\right) = 1/\left(\frac{1^2 - (-1.115)^2 \cdot 1^2}{2(-0.177 - (-1.115))1^2} - (-1.115)\right) = 1.015. \text{ Thus in}$$

equilibrium individuals who are 1 s.d. more willing to accept authority accumulate $\beta_1/2$ or 0.507 s.d. more human capital as measured by tests.

This suggests that, all else equal, those pupils who are unwilling to accept authority are likely to invest less in education, consistent with a growing literature on the importance of individual personality traits as determinants of educational, labor market and other life outcomes (Heckman et al. 2006)

In our data, an analysis that does not allow for the bi-directional effect or account for selection – namely, the OLS estimates in the control group – does not imply that education increases the willingness to accept authority (Table 7), but we argue that the magnitude of the effect of education in reducing acceptance of authority is biased greatly towards zero in this case. However, as can be seen from equation 8, the model suggests that in other environments where baseline variation in willingness to accept authority (σ_R^2) is greater, or other sources of variation in returns to education (σ_B^2) are smaller, the bias in non-experimental estimates would be greater, potentially leading to the erroneous conclusion that education increases willingness to accept authority. Perhaps one of the reasons that some scholars have believed that education promotes acceptance of authority is that they have simply compared individuals with different levels of education without taking selection into account.

The above point estimate of the impact of willingness to accept authority on education is specific to the particular assumptions of the model, for example, to our specification of the functional form of the relationship between willingness to accept authority as a child and a young adult, and of the cost function for education. However, the findings that education reduces the willingness to accept authority by much more than would be thought based on regression

analysis in non-experimental data; that the willingness to accept authority increases educational attainment; and that in situations with more baseline variation in willingness to accept authority a naïve cross-sectional analysis could lead to the “wrong sign”, namely the erroneous conclusion that education increases the willingness to accept authority, are all more general.

Other differences in OLS and IV estimates are consistent with the model above. There is a significant difference in IV and OLS estimates of the impact of human capital on willingness to support political violence (p -value=0.02), where OLS estimates are again biased towards zero.

There is no evidence that attitudes toward ethnicity, religion, or democracy affect education. In particular, there are no statistically significant differences between the IV and OLS estimates for ethnic, religious, or democratic attitudes (with p -values of 0.11, 0.43, and 0.82, respectively), although note that the OLS relationship between human capital and democratic attitudes is significant at 99% confidence, consistent with many other observational studies discussed earlier. The measures of newspaper reading and political knowledge are positive and significant in both the IV and OLS cases although IV estimates are larger in magnitude, and the difference between IV and OLS is significant at 90% for the political knowledge index. The perceived political efficacy index and the participation in politics index are near zero and not statistically significant for either the IV or OLS specifications, and there are no significant differences between them (with p -values of 0.31 and 0.56, respectively).

Taken together, we reject the hypothesis that the IV and OLS estimates are equal for three of the ten dependent variables in Table 7 at 90% confidence (with two other p -values equal to 0.11), far more often than would be generated by chance alone under the null hypotheses that both were measuring the same underlying parameters. These large differences between non-experimental and experimental estimates suggest that it is important to carefully distinguish

treatment versus selection effects in assessing whether education instills greater willingness to accept authority, or changes in other political attitudes and behaviors, and point to the importance of research strategies that exploit experimental or quasi-experimental designs.

7. Understanding the Channels of Impact

One could entertain several hypotheses about the channels of impact. Beyond the leading explanation of a human capital effect, scholarship winners were also honored in a public ceremony that could have affected their self-image and confidence, and also received a cash prize. To distinguish between these effects, in this section we test whether subsamples who were very unlikely to win a scholarship also exhibited effects similar to those documented above.

The main results are similar when we restrict attention to those individuals with baseline 2000 test scores below +2 standard deviations (Table 8), suggesting that the findings are not being driven by scholarship winners, although note that standard errors do inevitably rise with the reduced sample size. We focus on our main outcome measures in this table. The lack of autonomy mean effect result is almost identical in this restricted subsample at -0.176 (s.e. 0.091). The satisfaction with authority mean effect is also similar (-0.147, s.e. 0.088), and we cannot reject that it is equal to the full sample estimate. As in the full sample, there are no significant impacts on “modern” ethnic, religious or democratic attitudes, nor on individuals’ perceived political efficacy or participation in civic affairs. The estimated program impact on measures of newspaper reading, political knowledge, and attitudes towards political violence remain positive but are somewhat smaller in magnitude and not significant in the restricted subsample. When we

examine estimates across the two samples (namely, all those with baseline test scores vs. those with scores less than +2 s.d.), in no case is the difference significant at even 90% confidence.¹²

Another hypothesis is that the impacts are due in part to school-wide changes in attitudes associated with the implementation of a scholarship program, if the experience of observing other girls being publicly recognized for their achievements changed norms. We cannot completely rule this out. However, there is no strong evidence in favor of this alternative view.. While one could perhaps tell a story where having a program that provided scholarships to girls led to more progressive gender attitudes (and thus could account for some of the autonomy findings in Table 3), it is harder to imagine why this would generate some of the other findings, for instance, in terms of newspaper reading, objective political knowledge, expressed satisfaction with political authority, or violence in politics.

Moreover, it seems reasonable to conjecture that any school-wide effects of the program on gender attitudes would depend on the number of girls who were publicly recognized for their academic achievement. Because scholarships were awarded to girls scoring in the top 15% in the district and there is considerable school-to-school variation in test scores, there are large differences in the number of winners across schools (for instance, 16 of the 34 treatment schools had no winners in 2001). As above, we create a measure of predicted GSP winners in both treatment and control schools. We find no compelling evidence of differences in the magnitude of program impacts across schools with different numbers of predicted winners (Table 9). In particular, the interaction terms between treatment and predicted winners are significant for just two of the ten outcomes, while for the other eight variables the t-statistic is less than one. These

¹² While impacts appear to be mostly driven by human capital, it is impossible to decompose this effect into the separate channels of primary school attendance (Table 1), secondary enrolment, and skills measured in tests (Table 2), since we lack separate instruments for these channels.

generally small and not significant estimates suggest that non-human capital channels are driving the results, although we cannot completely rule this out.

8. Conclusion

We examine the impact of increased human capital on political and social attitudes among young Kenyan women. Our experimental results contribute to a vast and mostly non-experimental literature on the relationship between education and political and social attitudes, a relationship that is of particular interest in less developed societies like Kenya that have experienced rapid educational gains in recent decades. The program leads young women to reject the legitimacy of domestic violence and reduces their propensity to enter into marriages arranged by their parents. In findings that go against some claims in modernization theory, the program does not weaken ethnic attachment, promote secularization, or increase stated support for democracy.

Consistent with the view that education can potentially enhance political consciousness and contrary to theories suggesting that education merely instills acceptance of existing authority, program participation leads to greater objective political knowledge and newspaper readership, less willingness to defer to authority, and reduced satisfaction with Kenya's political and economic situation. However, this does not translate into greater perceived political efficacy, higher rates of voting, or other forms of civic participation. This combination of heightened political awareness and reduced acceptance of the legitimacy of existing political structures, in an environment where respondents perceive little ability to effect change through Kenya's nascent democratic institutions, may help explain why the increase in education was also associated with greater acceptance of the legitimacy of political violence. .

One possible interpretation is that education allowed young Kenyans to hold more realistic views about their political system. As noted above, less than one year after our surveys were collected, Kenya held a national presidential election in which independent observers argued that the main opposition candidate – who received the vast majority of votes in our study area – won more votes but the incumbent claimed victory after vote rigging (Gibson and Long 2009). After months of violent protests and bloody ethnic clashes a power-sharing deal was reached under which the incumbent remained president and the challenger became prime minister. As individuals in our sample received more education, their declining satisfaction with the status quo and growing acceptance of political violence arguably reflects their growing awareness of the role that violence often plays in Kenyan politics.

Experimental and non-experimental estimates of the impact of education on acceptance of authority differ considerably, suggesting that observational cross-sectional correlations are biased. To make sense of these differences, we hypothesize that one's ability to stay in school is closely related to her willingness to accept authority, which leads to omitted variable bias in non-experimental estimates that could lead observers to erroneously conclude that education instills greater acceptance of authority. In the context of a simple model, we assess the extent of reverse causality, which appears to be considerable. Somewhat ironically, it appears that only those who are sufficiently willing to accept a loss of autonomy when young (to succeed in school) actually have the opportunity to experience the gains in autonomy that education generates later in life.

Estimating these causal effects of education is, we believe, a meaningful step towards understanding the positive cross-country empirical association between education and democracy. While interpretation of that relationship remains controversial, the results suggest that if education does lead societies to be more democratic, it is not necessarily through growing

pro-democratic attitudes. Rather the presence of an educated, well-informed and critical citizenry could lead democracies to function more successfully. Recent research argues that, regardless of individuals' support for democracy *per se*, a necessary foundation for a successful democratic system is the existence of politically knowledgeable and engaged citizens willing to actively participate in political life and challenge political authorities (Moehler 2008; Glaeser et al. 2007). Alternatively, the higher levels of political dissatisfaction and tolerance of political violence generated by rising education could hasten the replacement of non-democratic regimes.

As with all micro-studies conducted in a given context – here, among young Kenyan women – caution is required in generalizing the results to other populations, most notably for males and older adults, for whom we lack data. Our results can be taken as relevant for rural Kenyan females at the margin of transitioning from primary schools – which are very much part of the fabric of village life – to secondary schools, which offer a much broader perspective of Kenya and the world. To what extent should we expect our results to generalize? The effects of education could surely change as individuals' age. It is impossible to know for certain without further work, but a natural conjecture is that the education of disadvantaged social groups – such as the rural women from politically marginalized ethnic groups in our sample – will induce greater awareness of politics, increase the desire for autonomy, and reduce tendencies to simply accept their powerlessness as a natural, immutable fact about the world. In our context, there is some indication that this translates into increased ethnic attachment and greater acceptance of the use of violence in politics. Yet it is plausible that in other contexts where there is greater scope for meaningful engagement in democratic politics, the heightened political consciousness and reduced acceptance of authority might instead lead to greater civic involvement, potentially contributing to a more vibrant democracy.

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Table 1: Girls Scholarship Program (GSP) Baseline Characteristics and Short-run Impacts (2001-2002)

Dependent variable:	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on program indicator
<u>Panel A: Baseline characteristics (2001-2002 surveys)</u>		
Student age (2001)	13.3 (1.44)	-0.14 (0.15)
Iron roof ownership	0.82 (0.38)	-0.048 (0.038)
Mother years of schooling	8.71 (4.18)	0.79* (0.40)
Father years of schooling	10.47 (3.99)	0.55 (0.49)
Proportion ethnic Luhya	0.79 (0.41)	0.067 (0.056)
Proportion ethnic Luo	0.104 (0.31)	-0.054 (0.038)
Proportion ethnic Teso	0.055 (0.23)	0.018 (0.033)
Test score pre-program, all subjects (normalized)	0.00 (1.00)	0.12 (0.20)
<u>Panel B: Short-run Impacts (2001-2002)</u>		
Test score post-program, all subjects (normalized)	0.00 (1.00)	0.34* (0.20)
Student school attendance	0.788 (0.36)	0.060* (0.032)
Teacher school attendance	0.822 (0.262)	0.069*** (0.025)

Notes: Each row is from a separate OLS regression. Significant at 90% (*), 95% (**), 99% (***) confidence. The outcome variable is regressed on the GSP program (treatment) indicator. Standard errors are clustered by school. The sample size in Panel A ranges from 789 to 1387 observations depending on the dependent variable. The sample consists of female students in the Girls Scholarship Program schools in Busia who were interviewed in the long-run follow-up and will be included in subsequent analysis. The academic subjects tested included English, geography/history/civics, mathematics, science, and Swahili. The attendance data for both pupils and teachers was collected during unannounced visits to schools in 2001 and 2002. The sample size in Panel B is 993 students in the test score regressions, and 1351 students and 666 teachers in the attendance regressions, respectively.

Table 2: Educational Outcomes in Follow-up survey (2005-2007)

Dependent variable:	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on program indicator
<u>Panel A: Sample attrition</u>		
Surveyed in follow-up round (2005-2007)	0.816 (0.388)	-0.007 (0.035)
<u>Panel B: Test Scores</u>		
Arithmetic test (normalized)	0.00 (1.00)	0.135 (0.102)
English vocabulary test (normalized)	0.00 (1.00)	0.162* (0.092)
Raven's matrix test (normalized)	0.00 (1.00)	0.182** (0.075)
Reading test (normalized)	0.00 (1.00)	0.124* (0.071)
Swahili vocabulary test (normalized)	0.00 (1.00)	0.218** (0.088)
Test score mean effect	0.00 (1.00)	0.208** (0.092)
Test score mean effect, among those with baseline scores < +2 s.d.	0.06 (0.96)	0.145* (0.082)
Test score mean effect, among schools with ≤5 predicted GSP winners	-0.06 (1.01)	0.177** (0.081)
<u>Panel C: Schooling outcomes</u>		
Attended at least some secondary school (0-1)	0.30 (0.46)	0.087** (0.041)
Still in school, at time of survey (0-1)	0.52 (0.50)	0.079* (0.044)
Grades of school completed	7.8 (1.2)	0.088 (0.103)

Notes: Each row is from a separate OLS regression. Significant at 90% (*), 95% (**), 99% (***) confidence. In Panels B-C the outcome variable is regressed on the GSP program (treatment) indicator, an indicator for student cohort, student age at time of the survey, the mean pre-program (2000) school test score, educational attainment of each parent, and timing of the follow-up survey (coefficient estimates not shown). There are no additional regression controls in Panel A. Standard errors are clustered by school. The sample size is 1,387 observations in Panels B-C. Details on the mean effect analysis are in the text. The test score mean effect in Panel B includes the arithmetic, English, Raven's matrix, reading, and Swahili test results.

Table 3: Autonomy in Household Outcomes in Follow-up survey (2005-2007)

Dependent variable:	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on program indicator
“Women have always been subject to traditional laws and customs and should remain so.” (0 to 1) (vs. “Women should have equal rights and receive the same treatment as men do.”) [AFB]	0.17 (0.31)	-0.009 (0.022)
“Men can beat their wives and children if they misbehave.” (0 to 1) (vs. “No one has the right to use physical violence against anyone else.”) [AFB]	0.25 (0.38)	-0.068*** (0.024)
Ever married (0-1)	0.21 (0.41)	-0.018 (0.034)
Ever married, with family involvement in spouse choice (0-1)	0.042 (0.201)	-0.024* (0.013)
Ever married, without family involvement in spouse choice (0-1)	0.165 (0.371)	0.005 (0.031)
Total fertility	0.400 (0.764)	-0.030 (0.065)
Lack of autonomy mean effect	0.00 (1.00)	-0.181** (0.077)

Notes: Each row is from a separate OLS regression. Significant at 90% (*), 95% (**), 99% (***) confidence. The outcome variable is regressed on the GSP program (treatment) indicator, an indicator for student cohort, student age at time of the survey, the mean pre-program (2000) school test score, educational attainment of each parent, and timing of the follow-up survey (coefficient estimates not shown). Standard errors are clustered by school. The sample is N=1,387. Details on the mean effect analysis are in the text. The autonomy mean effect includes the two attitude questions and the “Ever married, with family involvement in spouse choice” variables.

Table 4: Attitudes with Respect to Ethnicity, Religion and Democracy in Follow-up survey (2005-2007)

Dependent variable:	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on program indicator
<u>Panel A:</u> Ethnic and Religious Attitudes		
Ethnic identity is not “very important” to respondent (0-1)	0.110 (0.313)	-0.033 (0.020)
Ethnic identity is not “very important” to respondent (0-1), among those surveyed in 2005 and 2006	0.126 (0.332)	-0.042* (0.021)
Migrated out of Busia and Teso districts	0.257 (0.437)	0.006 (0.036)
Religious identity is not “very important” to respondent (0-1)	0.014 (0.116)	0.005 (0.006)
Member of a prayer group (0-1)	0.371 (0.483)	0.018 (0.034)
<u>Panel B:</u> Democratic attitudes		
Agree with: "We should choose our leaders in this country through regular, open and honest elections." (0 to 1)	0.93 (0.19)	0.001 (0.014)
Agree with: “Democracy is preferable to any other kind of government” (0 to 1) [AFB, WVS]	0.69 (0.46)	-0.005 (0.035)
Disagree with: “Only those who are sufficiently well educated should be allowed to choose our leaders.” (0 to 1) [AFB]	0.27 (0.38)	0.022 (0.030)
Disagree with: "Only one political party should be allowed to stand for election and hold office." (0 to 1) [AFB]	0.35 (0.40)	0.032 (0.029)
Disagree with: "All decisions should be made by a council of elders." (0 to 1) [AFB]	0.27 (0.36)	0.023 (0.026)
Disagree with: "The military should come in to govern the country." (0 to 1) [AFB]	0.61 (0.41)	0.002 (0.033)
Disagree with: "Elections and the Parliament should be abolished so that the president can decide everything." (0 to 1) [AFB]	0.64 (0.41)	-0.006 (0.032)
Democratic attitudes mean effect	0.00 (1.00)	0.058 (0.089)

Notes: Each row is from a separate OLS regression. Significant at 90% (*), 95% (**), 99% (***) confidence. The outcome variable is regressed on the GSP program (treatment) indicator, an indicator for student cohort, student age at time of the survey, the mean pre-program (2000) school test score, educational attainment of each parent, and timing of the follow-up survey (coefficient estimates not shown). Standard errors are clustered by school. The sample is N=1,387, except in the restricted sample for ethnic identity, where only those interviewed in 2005 and 2006 are included, in which case N=1,346. Details on the mean effect analysis are in the text. The mean effect at the bottom of Panel B includes all variables in that panel.

Table 5: Media Consumption, Political Knowledge, and Satisfaction with Authority in Follow-up survey (2005-2007)

Dependent variable:	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on program indicator
<u>Panel A: Media Consumption</u>		
Days listened to the radio in last week (0 to 7)	3.59 (2.87)	-0.642 ^{***} (0.198)
Days read a newspaper in last week (0 to 7)	0.439 (1.010)	0.134 [*] (0.074)
Has a favorite newspaper	0.666 (0.472)	0.096 ^{**} (0.037)
Favorite newspaper is the <i>Daily Nation</i>	0.305 (0.461)	0.105 ^{***} (0.035)
<u>Panel B: Political Knowledge</u>		
Political knowledge mean effect	0.00 (1.00)	0.203 ^{**} (0.085)
<u>Panel C: Satisfaction with Authority</u>		
“We should show more respect for authority.” (0 to 1) (vs. “As citizens, we should be more active in questioning the actions of our leaders.”) [AFB]	0.53 (0.46)	-0.076 ^{***} (0.026)
Kenya’s quality of government is better than two years ago (0-1)	0.56 (0.50)	-0.054 [*] (0.031)
Kenya’s economy is better than two years ago (0-1)	0.50 (0.50)	-0.058 [*] (0.033)
Satisfaction with Kenyan democracy (0 to 1) [WVS]	0.74 (0.29)	-0.048 ^{***} (0.017)
Satisfaction with authority mean effect	0.00 (1.00)	-0.239 ^{***} (0.061)
Taking everything together, respondent is “very happy” (0-1) [WVS]	0.65 (0.48)	-0.027 (0.039)

Notes: Each row is from a separate OLS regression. Significant at 90% (*), 95% (**), 99% (***) confidence. The outcome variable is regressed on the GSP program (treatment) indicator, an indicator for student cohort, student age at time of the survey, the mean pre-program (2000) school test score, educational attainment of each parent, and timing of the follow-up survey (coefficient estimates not shown). Standard errors are clustered by school. The sample is N=1,387. Details on the mean effect analysis are in the text. The mean effect in Panel B includes variables “Knows Kenyan President’s name (0-1)”, “Knows Kenyan Vice President’s name (0-1)”, “Knows Kenyan Education Minister’s name (0-1)”, “Knows Kenyan Health Minister’s name (0-1)”, and “Knows Ugandan President’s name (0-1)”; the mean effect in Panel C includes the four variables listed above the mean effect row.

Table 6: Perceived political efficacy, participation in politics and civic affairs, and attitudes towards violence in politics in follow-up survey (2005-2007)

Dependent variable:	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on program indicator
<u>Panel A:</u> Perceived Political Efficacy		
Disagree with: "Politics and government sometimes seem so complicated that you can't really understand what's going on." (0 to 1) [AFB]	0.16 (0.29)	0.009 (0.020)
Disagree with: "This world is run by a few people in power, and there is not much that someone like me can do about it." (0 to 1)	0.30 (0.37)	0.019 (0.027)
Perceived political efficacy mean effect	0.00 (1.00)	0.055 (0.066)
<u>Panel B:</u> Participation in Politics and Civic Affairs		
Interested in public affairs (0-1) [AFB, WVS]	0.26 (0.44)	-0.028 (0.028)
Respondent intends to vote in the next presidential election (0-1)	0.48 (0.50)	-0.025 (0.045)
Community group memberships	1.41 (1.39)	0.059 (0.105)
Participation in politics and civic affairs mean effect	0.00 (1.00)	-0.038 (0.073)
"Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?" (0-1) [WVS]	0.187 (0.390)	-0.018 (0.025)
<u>Panel C:</u> Political Violence		
"It is sometimes necessary to use violence in support of a just cause." (0 to 1) (vs. "The use of violence is never justified in politics.") [AFB]	0.47 (0.18)	0.030** (0.012)

Notes: Each row is from a separate OLS regression. Significant at 90% (*), 95% (**), 99% (***) confidence. The outcome variable is regressed on the GSP program (treatment) indicator, an indicator for student cohort, student age at time of the survey, the mean pre-program (2000) school test score, educational attainment of each parent, and timing of the follow-up survey (coefficient estimates not shown). Standard errors are clustered by school. The sample is N=1,387. Details on the mean effect analysis are in the text are in the text. The mean effect at the bottom of Panel A includes all variables in that panel, and the mean effect in Panel B includes all variables except for the trust question. The sample size for the "intends to vote in the next presidential election" analysis falls to 963 because the question was only asked of those respondents old enough to vote in the next election. The types of community groups include: women's groups; farmer/agricultural groups; youth groups; water groups/well committees, credit, saving, or insurance groups; prayer or bible study groups; burial committees; school committees or clubs; sports teams; other community group.

Table 7: Comparing experimental and non-experimental estimates

	Coefficient estimate (s.e.) on program indicator OLS	Coefficient estimate (s.e.) on normalized test score (2005-07) IV-2SLS [Experimental]	Coefficient estimate (s.e.) on normalized test score (2005-07) OLS [Non-experimental]	Hausman test p-value, $H_0: \beta_{IV} = \beta_{OLS}$ (columns 2, 3)
	(1)	(2)	(3)	(4)
Dependent variable:				
Lack of autonomy index	-0.181** (0.077)	-0.872* (0.493)	-0.286*** (0.078)	0.11
Satisfaction with authority index	-0.239*** (0.061)	-1.115** (0.494)	-0.177*** (0.055)	<0.01***
“It is sometimes necessary to use violence in support of a just cause.” (0 to 1) (vs. “The use of violence is never justified in politics.”)	0.030** (0.012)	0.146 (0.099)	-0.009 (0.010)	0.02**
Ethnic identity is not “very important” (0-1)	-0.033 (0.020)	-0.158 (0.105)	0.001 (0.013)	0.11
Religious identity is not “very important” (0-1)	0.005 (0.006)	0.023 (0.031)	-0.001 (0.005)	0.43
Democratic attitudes index	0.058 (0.089)	0.277 (0.355)	0.322*** (0.051)	0.82
Days read a newspaper in last week (0 to 7)	0.134* (0.074)	0.644* (0.364)	0.258*** (0.055)	0.26
Political knowledge index	0.203** (0.085)	0.975*** (0.325)	0.464*** (0.045)	0.08*
Perceived political efficacy index	0.055 (0.066)	0.265 (0.346)	-0.033 (0.050)	0.31
Participation in politics and civic affairs index	-0.038 (0.073)	-0.183 (0.363)	0.017 (0.068)	0.56

Notes: Each cell contains results from a separate regression. Significant at 90% (*), 95% (**), 99% (***) confidence. Details on the mean effect analysis are in the text. Standard errors are clustered by school. The sample is N=1,387 for all dependent variables.

In column 1, the outcome variable is regressed on the GSP program (treatment) indicator, an indicator for student cohort, student age at time of the survey, the mean pre-program (2000) school test score, educational attainment of each parent, and timing of the follow-up survey (coefficient estimates not shown). In column 2, the outcome variable is regressed on the same explanatory variables except the GSP program indicator is replaced by the normalized test score (in 2005-2007) across all subjects, and the test score is instrumented by the GSP program indicator. In column 3, the normalized test score (in 2005-2007) across all subjects is included as an explanatory variable but it is not instrumented, and the sample is restricted to the control group (N=664). Column 4 presents the Hausman test results (p-value) on the hypothesis that the coefficient estimates are equal across columns 2 and 3 for each dependent variable.

Table 8: Comparing estimates in the full sample vs. among those with “low” baseline test scores

	Coefficient estimate (s.e.) on program indicator OLS	Coefficient estimate (s.e.) on program indicator, baseline test score < +2 s.d.	Test of equality of coefficient estimates in columns 1, 2 (p-value)
	(1)	(2)	(3)
Dependent variable:			
Lack of autonomy mean effect	-0.204** (0.088)	-0.176* (0.091)	0.71
Ethnic identity is not “very important” (0-1)	-0.024 (0.024)	-0.024 (0.025)	0.99
Religious identity is not “very important” (0-1)	-0.007 (0.007)	-0.005 (0.007)	0.81
Democratic attitudes mean effect	-0.037 (0.097)	-0.062 (0.096)	0.78
Days read a newspaper in last week (0 to 7)	0.109 (0.097)	0.116 (0.105)	0.95
Political knowledge mean effect	0.109 (0.094)	0.107 (0.097)	0.90
Satisfaction with authority mean effect	-0.181** (0.089)	-0.147 (0.088)	0.79
Perceived political efficacy mean effect	0.083 (0.077)	0.086 (0.081)	0.92
Participation in politics and civic affairs mean effect	-0.144 (0.096)	-0.138 (0.098)	0.95
“It is sometimes necessary to use violence in support of a just cause.” (0 to 1) (vs. “The use of violence is never justified in politics.”)	0.012 (0.013)	0.006 (0.013)	0.62

Notes: Each cell contains results from a separate regression. Significant at 90% (*), 95% (**), 99% (***) confidence. Details on the mean effect analysis are in the text. Standard errors are clustered by school. The sample in column 1 consists of all individuals with baseline test score data (N=919). In column 2, the sample is restricted to individuals with baseline normalized test scores less than +2 s.d. (N=881).

In columns 1 and 2, the outcome variable is regressed on the GSP program (treatment) indicator, an indicator for student cohort, student age at time of the survey, the mean pre-program (2000) school test score, educational attainment of each parent, and timing of the follow-up survey (coefficient estimates not shown). Column 3 presents the test results (p-value) on the hypothesis that the coefficient estimates are equal across columns 1 and 2 for each dependent variable. To carry out this test, 1000 draws (with replacement) of sample size 899 were taken from the full baseline test score sample, and the regressions analogous to column 2 were carried out. The p-values in column 3 correspond to the hypothesis that the coefficient estimates in column 2 are drawn from this distribution.

Table 9: Effects in schools with different numbers of predicted GSP winners

	Coefficient estimate (s.e.) on program indicator OLS	Coefficient estimate (s.e.) on interaction between the program indicator and predicted number of winners
Dependent variable:	(1)	(2)
Lack of autonomy mean effect	-0.194** (0.096)	0.005 (0.001)
Ethnic identity is not “very important” (0-1)	-0.039** (0.018)	0.005** (0.002)
Religious identity is not “very important” (0-1)	-0.003 (0.007)	-0.0004 (0.0006)
Democratic attitudes mean effect	0.019 (0.080)	-0.004 (0.010)
Days read a newspaper in last week (0 to 7)	0.114 (0.077)	-0.004 (0.007)
Political knowledge mean effect	0.206** (0.036)	0.006 (0.009)
Satisfaction with authority mean effect	-0.205*** (0.063)	-0.001 (0.005)
Perceived political efficacy mean effect	0.068 (0.069)	0.002 (0.008)
Participation in politics and civic affairs mean effect	-0.024 (0.074)	-0.008 (0.009)
“It is sometimes necessary to use violence in support of a just cause.” (0 to 1) (vs. “The use of violence is never justified in politics.”)	0.021** (0.010)	0.005*** (0.001)

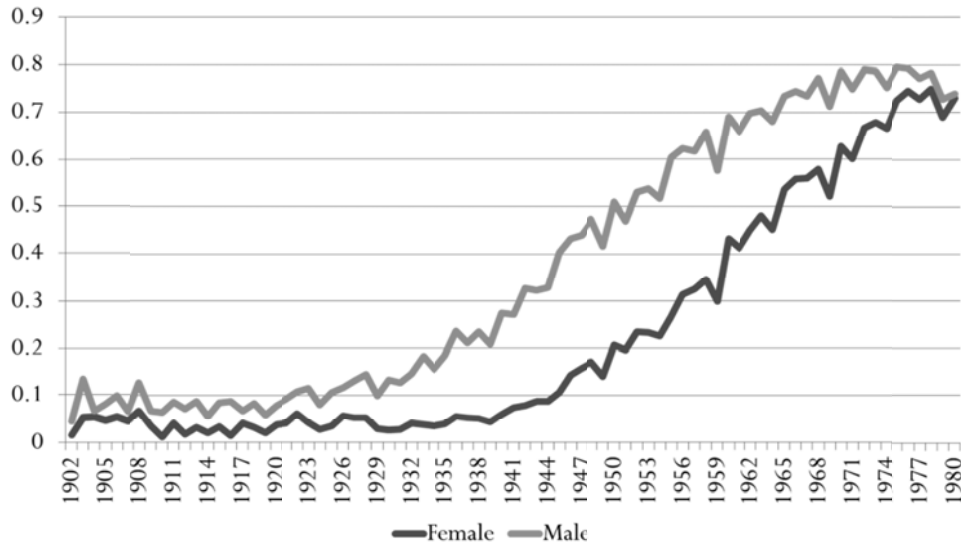
Notes: Each row contains results from a separate regression. Significant at 90% (*), 95% (**), 99% (***) confidence. Details on the mean effect analysis are in the text. Standard errors are clustered by school. The sample is N=1,385 for all dependent variables.

The outcome variable is regressed on the GSP program (treatment) indicator, the GSP indicator interacted with the predicted number of GSP winners, an indicator for student cohort, student age at time of the survey, the mean pre-program (2000) school test score, educational attainment of each parent, and timing of the follow-up survey (coefficient estimates not shown). The predicted number of GSP winners is estimated in treatment group schools by regressing the actual number of scholarship winners in the school on various quantiles of the baseline test score distribution for students in that school; the predicted number of winners is then assigned to both treatment and control schools.

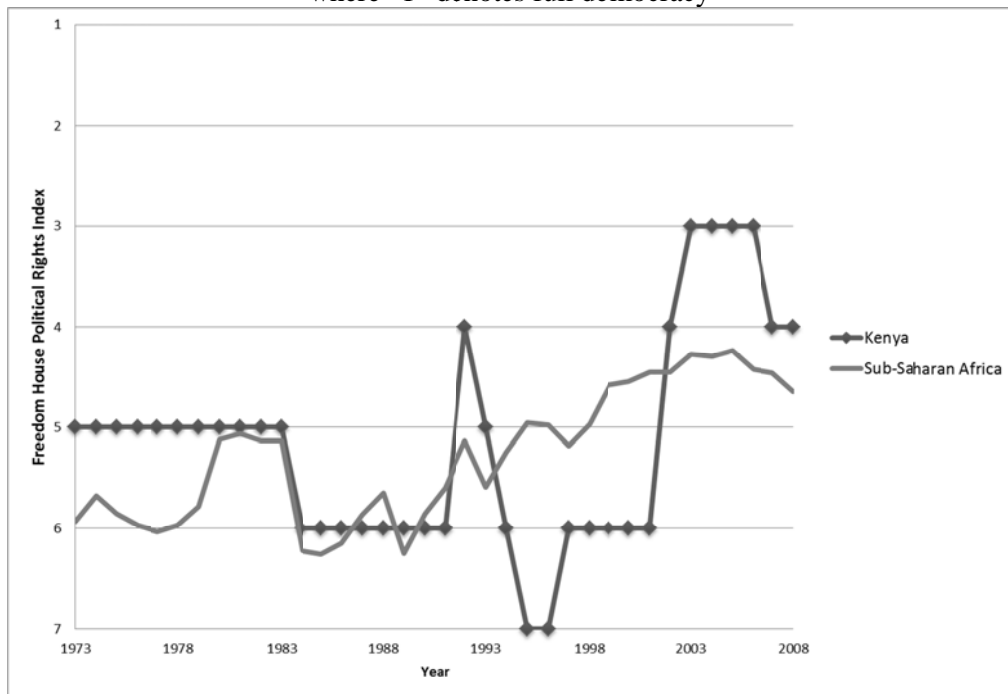
SUPPLEMENTARY APPENDIX MATERIALS [not intended for publication]

Appendix Figure A1: Schooling and democracy trends in Kenya

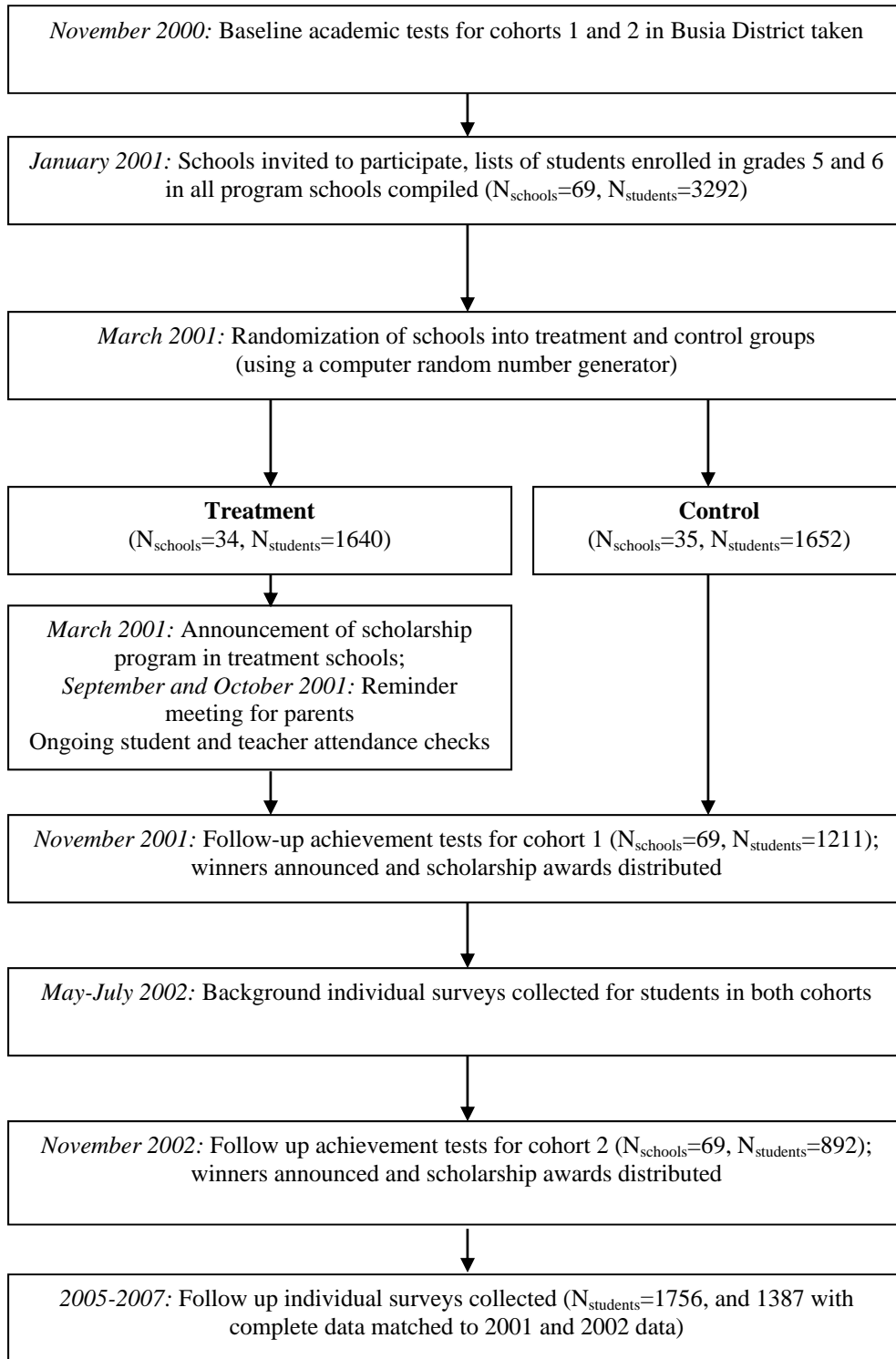
Panel A: Proportion of population completing primary schooling in Kenya, by birth cohort and gender (1902-1980 birth cohorts, source: 1999 Kenya Census)



Panel B: Freedom House Political Rights Index in Kenya and sub-Saharan Africa (1973-2008), where “1” denotes full democracy



Appendix Figure A2: Girls Scholarship Program (GSP) Timeline (2000-2007)



Appendix B: Model appendix

This appendix contains some of the algebra and analysis underlying the discussion of the model in section 7. Equation 7 implies that, in the absence of a merit scholarship program (as in our control group), the variance of education will be equal to:

$$(eqn. A1) \quad \sigma_H^2 = \frac{\sigma_B^2 + \beta_1^2 \sigma_R^2}{4}.$$

This implies that the willingness to accept authority for person i at time 1 will be:

$$(eqn. A2) \quad R_{1,i} = R_{0,i} + \gamma \left(\frac{B_{0,i} + \beta_1 R_{0,i} + \beta_2 M_i}{2} \right).$$

In the absence of a merit scholarship program, the variance of R_1 , (suppressing subscripts) is:

$$(eqn. A3) \quad \begin{aligned} Var(R_1) &\equiv \sigma_{R_1}^2 &= Var(R_0 + \gamma H^*) \\ &= Var(R_0) + \gamma^2 Var(H^*) + 2Cov(R_0, \gamma H^*) \\ &= \sigma_R^2 + \gamma^2 \sigma_H^2 + 2Cov(R_0, \gamma \frac{B_0 + \beta_1 R_0}{2}) \\ &= \sigma_R^2 + \gamma^2 \sigma_H^2 + 2[0 + Cov(R_0, \gamma \frac{\beta_1 R_0}{2})] \\ &= \sigma_R^2 (1 + \gamma \beta_1) + \gamma^2 \sigma_H^2 \end{aligned}$$

In the absence of a merit scholarship program, the covariance between H^* and R_1 will be:

$$(eqn. A4) \quad \begin{aligned} Cov(R_1, H^*) &= Cov(R_0 + \gamma \left[\frac{B_0 + \beta_1 R_0}{2} \right], \frac{B_0 + \beta_1 R_0}{2}) \\ &= Cov(R_0, \frac{B_0}{2}) + Cov(R_0, \frac{\beta_1 R_0}{2}) + \gamma Var(\frac{B_0 + \beta_1 R_0}{2}) \\ &= \frac{2\beta_1 \sigma_R^2 + \gamma \sigma_B^2 + \gamma \beta_1^2 \sigma_R^2}{4} \end{aligned}$$

Taking the difference between equations 8 and 9 in section 7, allows us to estimate the bias, and solve for β_1 in terms of known parameters:

$$(eqn. A5) \quad \gamma_{OLS} - \gamma_{IV} = \frac{2\beta_1 \sigma_R^2}{\sigma_B^2 + \beta_1^2 \sigma_R^2}$$

This implies β_1 is

$$(eqn. A6) \quad \beta_1 = \frac{2(\gamma_{OLS} - \gamma_{IV})\text{var}(H^*)}{\sigma_R^2}$$

It is possible to solve for β_1 based on observed parameters. To do so, we first rewrite equation A3 and then substitute the variance of R_0 into equation A6:

$$(eqn. A7) \quad \sigma_R^2 = \frac{\sigma_{R_1}^2 - \gamma^2 \sigma_H^2}{(1 + \gamma\beta_1)}$$

$$(eqn. A8) \quad \beta_1 = \frac{2(\gamma_{OLS} - \gamma_{IV})\sigma_H^2}{\frac{\sigma_{R_1}^2 - \gamma^2 \sigma_H^2}{(1 + \gamma\beta_1)}}$$

To simplify let $z = \left[\frac{2(\gamma_{OLS} - \gamma_{IV})\sigma_H^2}{\sigma_{R_1}^2 - \gamma^2 \sigma_H^2} \right]$,

$$(eqn. A9) \quad \beta_1 = z(1 + \gamma\beta_1)$$

Solving for β_1 yields:

$$(eqn. A10) \quad \beta_1 = \frac{1}{\left(\frac{1}{z} - \gamma \right)}$$

Re-writing yields equation 10 in the text.