

# Repressed Productive Potential and Revolt: Insights from an Insurgency in Burundi

Cyrus Samii and Emily A. West\*

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\*Samii is Associate Professor, Department of Politics, New York University, 19 West 4th Street, New York, NY 10012; West is Assistant Professor, Department of Political Science, University of Pittsburgh; Email: (Samii) cds2083@nyu.edu and (West) eawest@pitt.edu. The data come from the *Wartime and Post-Conflict Experiences in Burundi* study, which was sponsored by the Folke Bernadotte Academy, Sweden, and the United States Institute of Peace, and was approved by the New York University IRB as protocol no. HS-5279. For helpful discussions, we thank Ariel BenYishay, Chris Blattman, Thomas Flores, Omar Garcia Ponce, Michael Gilligan, Guy Grossman, Macartan Humphreys, Patrick Kuhn, Eric Mvukiyehe, Gwendolyn Taylor, Matthew Winters, and participants at the Columbia-NYU Contemporary African Political Economy Research Seminar, Cornell Institute for African Development, Midwest Political Science Association annual meeting, and William & Mary Development Economics Seminar.

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## Abstract

The relationship between participation in revolt and individuals' economic conditions is among the most debated in political science. While conventional economic theory suggests that those who face the poorest economic prospects are most inclined to fight, extant evidence is decidedly mixed. We address this puzzling variation by analyzing the interplay between macro-structural conditions and individuals' micro-level circumstances. Under conditions of severe group repression, we show how a "glass ceiling" logic may operate: among the repressed group, those with relatively high productive potential may be most motivated to revolt. We test this with in-depth analysis of participation in the 1993-2003 Burundian insurgency. The data are consistent with numerous implications of the glass ceiling logic and inconsistent with extant alternative explanations.

Keywords: inequality; rebellion; ethnic conflict; human capital; opportunity costs.

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# Introduction

Understanding participation in armed movements is crucial for determining appropriate responses to conflict. Current literature focuses both on conflict's structural conditions, such as "horizontal inequality," as well as individual-level characteristics that predict participation in conflict such as education, wealth, and employment. However, there is sparse exploration of the way that these structural conditions and micro-level characteristics interact to affect participation. We develop a theory explaining how group-based repression (a structural condition) might make self-perceived "productive potential" (a micro-level factor) salient in motivating participation in revolt. We provide a micro-foundational theory and new evidence from the case of Burundi to understand why horizontal inequality—that is, severe group-level differences in access to opportunities—leads to armed conflict.

Conventional economic theory suggests that those who face the poorest economic prospects should be most inclined to participate in revolt (Grossman, 1991). However, the evidence on such relationships is decidedly mixed, suggesting that pathways to participation vary tremendously depending on context. As a result, the question of whether and how economic conditions affect participation in revolt remains open (Berman et al., 2011; Blattman and Miguel, 2010).

In an attempt to tackle this puzzle, we consider how country-level structural conditions shape the motivations to revolt among individuals who differ in their economic conditions. We theorize that conditions of severe group repression may interact with individual-level characteristics to alter the economic logic of insurgent participation. Many societies exhibit "horizontal inequality" (Stewart, 2008), and such inequality often originates from and is maintained through repression.

This induces sentiments of relative deprivation among members of repressed groups (Gurr, 1970). We posit that among members of these repressed groups, it is those with relatively high productive potential that chafe hardest under the “glass ceiling” preventing them from realizing their potential. Our evidence shows that these individuals have the strongest motivation for revolt against the custodians of the status quo.

These “glass-ceiling” motivations may help us in making sense of patterns anomalous to conventional economic accounts, including militant participation among relatively well-off individuals, as has been documented in Egypt, Lebanon and Palestine (Gambetta and Hertog, 2007; Krueger, 2007). The “glass ceiling” logic also provides micro-foundations for the cross-national correlation between horizontal inequality and civil war onset (Cederman, Gleditsch and Bugaug, 2013; Østby, Nordas and Rød, 2009).

Our goal is to push harder on the logic of a “glass-ceiling” revolt to see how well it explains fine-grained patterns in a particular case: the 1993-2003 armed insurgency in Burundi. The Burundi case is interesting for three reasons. First, Burundi’s history provides insights into the dynamics of “ranked” ethnic systems, which have historically defined horizontal inequality and some of the hardest conflict resolution challenges (Horowitz, 1985). Second, Burundi is important for understanding conflict dynamics in a region that is of special significance to conflict research given the scale and duration of its armed conflicts. Finally, the case offers a historic macro-level shock to group repression, allowing for a cohort-based empirical test for the role of a hard-to-test macro-level variable, repression.

We use survey data on Hutu male participants and non-participants in the insurgency. Our ba-

sic test examines how productive potential, as proxied by individuals' own as well as their fathers' levels of education, affects participation. Further tests validate the use of these proxies for productive potential and demonstrate exposure to repression among Hutu males. Through these tests, we find that the glass-ceiling logic has predictive validity. Participation likelihood is increasing in individuals' own as well as fathers' education levels. The result is robust to controlling for age, provincial fixed effects, and differential survival. Further results show that Hutu men were subject to repression before the war, but that such repression was lifted through the insurgency.

We also leverage a historical shock to levels of repression in 1972, developing a cohort-based test. In the aftermath of a local Hutu insurrection in 1972, repression of Hutu men was increased dramatically nationwide. Cohorts who completed their education prior to this shock would have had their self-perceived productive potential reinforced by their own educational attainment. These individuals would have chafed especially hard under the increased repression—the door to mobility was slammed in their faces. We find that the relationship between participation and productive potential is strongest among members of these older cohorts.

Father's and own education could be associated with a variety of factors that increase the likelihood of insurgent participation; therefore, these variables are imperfect proxies for productive potential. Thus, we study five alternative mechanisms that might link education to participation. We do not find support for any of these alternative explanations. Our theory and these tests of its core predictions contribute to the growing literature dedicated to understanding the varied individual-level motivations for participation in political violence.

## Repression, Perceived Productive Potential and Revolt

At the macro level, current empirical literature is mixed in relating inequality to revolt. While there may be a weak correlation between conflict onset and inequality (Collier and Hoeffler, 2004; Fearon and Laitin, 2003), given severe data limitations, these studies by no means provide the last word (Blattman and Miguel, 2010). In fact, there is a strong macro-level correlation between civil war onset and specific forms of inequality, namely horizontal inequality (Østby, Nordas and Rød, 2009; Stewart, 2008).

At the micro level, recent studies have found numerous personal characteristics—including economic welfare, personality, social status and networks—to be associated with conflict participation (Arjona and Kalyvas, 2011; Kalyvas and Kocher, 2007; Petersen, 2002). With respect to economic conditions, some find that participation rates decrease in indicators of economic prospects (Humphreys and Weinstein, 2008), while other cases show the opposite (Berman et al., 2011; Gambetta and Hertog, 2007; Krueger, 2007). Others still demonstrate that broad trends mask important fine-grained variation (Lee, 2011). Such varied results suggest an incoherent variety of individual-level reasons for taking up arms.

As Balcells and Justino (2014) propose, a fruitful step may be to consider the interplay between macro-structural conditions and individual-level attributes. We consider how repression and horizontal inequality interact with individual-level economic conditions to shape rebellious motivations. Specifically, we develop a logic of “glass ceiling revolt,” which critically builds on relative deprivation and horizontal inequality literatures, but makes an important departure. The Appendix (available online) contains a formal presentation of this theory based on a “tipping point” model of

insurgency (Medina, 2007; Wood, 2003). Here we describe key features.

Existing studies show how inequality across groups can lead to violence (Cederman, Gleditsch and Bugaug, 2013; Stewart, 2008). Complementary to this, Gurr (1970) and others claim that feelings of deprivation relative to other groups can inspire individual-level participation in movements for social change. Our theory draws on these same psychological mechanisms, but in our account, the “referent” is one’s own unrealized potential (Gurr, 2000). Feelings of relative deprivation may be based on a comparison between one’s actual experience and the counterfactual of potential opportunities under no repression (Olson and Roese, 2002). Our primary hypothesis at the individual level is that the greater the gap between one’s realized potential and one’s perceived productive potential, the higher the likelihood of participation in revolt.

Repression is the macro-level scope condition for our primary hypothesis. Group based repression operates as a “glass-ceiling” that keeps members of the repressed group from realizing their productive potential while still allowing them to witness the opportunities enjoyed by the non-repressed. Repression blocks mobility, and therefore creates the gap between repressed group members’ realized and productive potential. Circumstances of group-based repression are common historically, particularly in “ranked” ethnic systems (Horowitz, 1985) like Sunni-minority dominated Iraq, Lebanon with Sunni-Christian hegemony, and racial repression under Jim Crow in the US South.

This theory of “glass ceiling revolt” provides micro-foundations for the positive correlation between horizontal inequality and violence. The theory also provides important nuance to relative deprivation theories by focusing on how variation in individual-level productive potential interacts

with repression to produce feelings of relative deprivation. We know that barriers to mobility become targets of political change for members of repressed groups (DeNardo, 1985; Medina, 2007); our theory adds that under such conditions, those who chafe hardest under the “glass ceiling” of repression will have the strongest motivation to revolt.

This implies that, among members of a repressed group, those with higher perceived productive potential are more likely to reside at a margin where economic or political shocks could tip them into preferring to fight over acquiescing to the status quo. Those with lower self-perceived productive potential will be “infra-marginal” because they stand to gain relatively little from the removal of barriers to mobility. The assumption is that the constraints from repression bind among those who seek to reach heights in society, and that their resourcefulness or skills do not allow them to easily evade such repression. If such individuals with high productive potential are not able to access sufficient resources under the status quo, then these individuals may have ample motivation to join the revolt themselves. It is in such cases of severe repression that the logic of “glass ceiling revolt” is most relevant, leading to the following hypothesis:

*H<sub>a</sub>: Among members of harshly repressed groups, participation in revolt will be increasing in self-perceived productive potential, all else equal.*

$H_a$  identifies the subpopulation of the repressed group that is most likely to revolt, something that classical theories often fail to specify (Rule, 1988, p. 208).

The “all else equal” in hypothesis  $H_a$  refers primarily to three factors. (The formal analysis in the supporting information models these factors explicitly.) First, participation in insurgency may come at the cost of pursuing peaceful options. Many economic accounts of insurgency presume



that such opportunity costs will be determinative (e.g., Collier and Hoeffler, 2004). However, under severe repression, opportunities among the repressed may vary only slightly from person to person, while productive potential varies significantly. Second, any account of participation in insurgency needs to address the problem of free-riding. Despite the hardships that one takes on as a fighter, it is typically reasonable to assume that members of a *successful* insurgency will enjoy certain privileges that non-participants do not. Such outcome-dependent rewards can be the basis of successful insurgent collective action (Medina, 2007). Third, participation is only rational under beliefs that the insurgency has some chance of success. Conditional on such beliefs, those with higher productive potential would have the strongest motivation to fight in a movement against the custodians of a repressive status quo.

## **Insurgency in Burundi**

The nature of repression and horizontal inequality in Burundi is such that it provides for a compelling test of our hypothesis. In Online Appendix Section B we provide a more detailed discussion of the historical background for the case. Crucially, that the case allows us to study how macro-level variation in the level of repression interacts with individual economic conditions. We have two sources of variation in macro-level conditions: variation across ethnic groups and variation over time. With respect to ethnic groups, the insurgency in Burundi was largely catalyzed by resentment among Hutus originating from their political and economic repression at the hands of a ruling Tutsi elite (Lemarchand, 1994). Over time, the level of this repression varied, with an especially dramatic ratcheting up occurring as the result of a crackdown by the Tutsi-dominated

regime following a 1972 Hutu insurrection. As such, we have variation in both the macro-level factors (repression) and micro-level factors (productive potential) that are relevant to our theory. The case also fits the premises of our theory because of the apparently voluntary nature of participation in the insurgency, in contrast to other cases where forced recruitment has been more important (Blattman, 2009; Humphreys and Weinstein, 2008). As discussed in the appendix, our survey data suggest that only a minority of insurgent participants (28%) indicated that they feared harm by the rebels if they did not participate, and a very small fraction (7%) indicated that they had been harmed prior to joining. When asked how they were convinced to join, 56% indicated that they convinced themselves and 25% indicated that their friends convinced them. These results suggest that insurgent recruitment was more a product of individual volition and peer pressure than abduction by the rebel organization.

## **Data**

We use survey data collected in Burundi in 2007 (Samii, 2013; Samii, Mvukiyehe and Taylor, 2009) in order to test our theory. The data was collected as close to the end of the war as possible, thereby minimizing the gap between rebel participation decisions and data collection. The national survey included 446 ex-rebel Hutu combatants as well as 1,169 non-combatants. We use the data on the background and participation information of these individuals in order to test our hypothesis. Information about sampling and survey techniques can be found in the Appendix.

We restrict the sample of both civilian and ex-combatant respondents to Hutu males aged 13-42 in 1993, since it is these individuals for whom the hypothesis is relevant. Tutsi participation

Table 1: Summary Statistics

	Mean	Std. Dev.	Min.	Max.
<i>Civilian Hutu Male Sample (N=459)</i>				
Age in 1993	22.62	7.86	13	42
Years of Education (before war)	4.35	3.35	0	17
Father's Years of Education	1.72	3.24	0	18
<i>Ex-Combatant Hutu Male Sample (N=446)</i>				
Age in 1993	20.29	7.17	13	42
Years of Education (before war)	6.18	2.61	0	14
Father's Years of Education	3.28	3.53	0	14
<i>Tutsi Male Sample (N=185)</i>				
Age in 1993	22.87	7.34	13	40
Years of Education (before war)	5.80	3.83	0	17
Father's Years of Education	2.17	4.54	0	39

in the rebel groups was exceedingly rare, as was the participation of children younger than 13. We also restrict the sample to individuals 42 or younger because this is the maximum age in our sample of combatants; matching this age range in our sample of civilians controls for age differences. Finally, we focus only on men because our sample of rebel combatants, which was sampled in a manner that was independent of gender, includes a small fraction (5%) of women. Given Burundian gender norms, factors associated with women's participation were likely quite distinct, but the small number of women in our sample precludes analysis. Summary statistics are displayed in Table 1. We show summary statistics for the two proxy variables used for self-perceived productive potential: one's own education and father's education. Table 1 shows that ex-rebels had higher educational attainment levels and more educated fathers compared to both Hutu and Tutsi non-rebels.

We use own education and father's education as proxies for an individual's assessment of his productive potential. While direct, own education is potentially biased for the current analysis for

three reasons. First, the oppressive regime prior to 1993 may have led many Hutus to forgo education given that opportunities for employment were highly limited, making the measure endogenous. Second, Hutus that join the rebellion at school-age would not be able to pursue more years of education. Third, after 1972, Hutu students were explicitly discriminated-against for scholarships and entry into higher level schools (Jackson, 2000). Thus, own education may not accurately measure productive potential given that repression likely stifled realization of this proclivity for educational attainment post-1972.

To avoid these sources of bias, we use father’s education as a second proxy for productive potential. Father’s education is plausibly related to an individual’s self-perceived productive potential: individuals likely use their father’s success as a baseline for their own potential. But, importantly for our analysis, the level of father’s education for Hutu men would have been set *prior to 1972*, so there is little scope for either anticipation or the consequences of post-1972 discrimination to impact this measure.

## Specifications and Tests

As a first test, we use the following specification on our sample of Hutu combatant and civilian men:

$$\text{Participated in armed revolt}_i = \beta_0 + \beta_1 \text{Education}_i + X_i' \beta_2 + \epsilon_i, \quad (1)$$

where “Education” is measured either in terms of years of the subject’s own education or the subject’s father’s education. The outcome is a binary variable taking the value 1 for participants and

0 for non-participants.  $X_i$  is a vector of controls: age in 1972, pre-war province fixed effects, and survival probability. Age is an obvious confounder; average educational attainment increases in age and there are probable differences in participation likelihood due to age. Province fixed effects control for levels of urbanness and other contextual variability. We also control for survival probability. A section in the Appendix provides details on survival and attrition patterns between 1993 and the time of data collection. Our survey collects data on survival patterns by recording information on household members who died during the war, including both civilians and combatants. Based on these data as well as other administrative data, we estimate that the survival rate for civilians in our target population was between 85% to 87%, while for those in the rebellion, it was between 62% and 69%. Among rebels, those with educated fathers were also somewhat less likely to die. Without accounting for such patterns, we risk obtaining spurious results for the parameters of interest. Thus, we estimate a penalized logistic regression of survival on father's education, province fixed effects, and other key variables, and then control for a linearized (log-odds) transformation of the predicted survival probability. To avoid post-treatment bias, our main specifications avoid including individual attributes that are likely themselves to be affected by educational attainment (Rosenbaum, 1984). Nonetheless, we account for many alternative consequences of education when examining alternative explanations below. We assume the error is clustered at the commune level. We fit the model using weighted least squares regression (applying the survey weights) and cluster-robust standard errors. Our hypothesis implies that  $\beta_1 > 0$ .

The Appendix replicates our results using logistic regression, and results are nearly identical. In addition to this baseline, we conduct further tests to shore up the plausibility of our interpretation.

The first is an analysis that we use to validate our theoretical assumption that repression is binding. We add data from *Tutsi* men of the same age range as our sample's Hutu men. On average, Tutsi men faced much less repression when it came to pursuing their productive potential. To show this, we fit a weighted least squares regression to the following equation:

$$Y_i = \gamma_0 + \gamma_1 \text{Father's ed.}_i + \gamma_2 \text{Hutu}_i + \gamma_3 \text{Father's ed.} \times \text{Hutu}_i + X_i' \gamma_5 + \nu_i. \quad (2)$$

For the outcome  $Y_i$ , we use educational attainment and a ranking of occupations (assigning -1 for no occupation, 0 for unskilled, and 1 for skilled). This equation allows us to study the extent of discrimination that Hutu men faced relative to their Tutsi counterparts. Under no discrimination, the expectation is that father's education would be positively associated with educational and occupational attainment. Discrimination against Hutus relative to Tutsis would appear in the form of an attenuation of the relationship between father's education and one's own attainment among Hutus. Thus, we expect  $\gamma_1 > 0$  as the "natural" relationship between inter-generational levels of attainment,  $\gamma_2 < 0$  as the level difference in attainment by Hutus, and most importantly  $\gamma_3 < 0$  as the effect of discrimination preventing those Hutus with high-attaining fathers from realizing their productive potential. The same control variables are used as for Equation (1).

A third stage of analysis bolsters our main findings using different outcome variables and subsamples. We study the extent to which one's own education and father's education predict the occupational attainment of Hutu men before and after the war. The expectation is that the relationship will be stronger after the war, since the war's outcome lifted some constraints to Hutu mobility. Such a pattern would validate our assumption that these educational attainment variables

proxy for self-perceived productive potential, and that these constraints motivated the revolt. We also study the extent to which these educational variables predict incoming and outgoing ranks within the rebellion. We expect upward mobility within the ranks to be possible among those Hutu participants with high productive potential because, unlike the repressive Tutsi regime, the rebel organization allowed for advancement. Such a pattern would shore up our assumption that the educational attainment variables are reasonable proxies for productive potential.

We then use the discontinuous increase in levels of repression in 1972 to construct a cohort-based test. By providing variation in levels of repression, this allows us to test the impact of the macro-level variable in our theory of glass-ceiling revolt. Resulting from a local insurrection in a central province, there was a ramping-up of repression in 1972. This shock to levels of repression helps to increase the credibility of our interpretation in terms of a “glass ceiling revolt.” To understand the logic of this cohort-based test consider the following two points. First, the clampdown on Hutu access to education and opportunities following 1972 generates two cohorts: the older had the opportunity to pursue education for a time with relatively few constraints, and the younger faced severe constraints earlier in their lives. Thus, the older cohort would have their self-perceived productive potential reinforced through their accomplishments prior to 1972, only to have the door to further pursuit of opportunities slammed. Younger cohorts did not have such opportunities to reinforce their self-perceived productive potential, nor did they experience such a sharp shock to their ability to pursue post-education opportunities. And, again, we are able to distinguish individuals on the basis of their father’s education levels, which would have been set prior to 1972.

Based on this logic, a few patterns should emerge. First, to validate that there were meaningful differences in access to education before and after 1972, we expect to see a stronger relationship between educational attainment and father’s education for the older cohort. These older cohorts were not affected by the clampdown until later in life and thus had time to attain education in a manner that is consistent with the example of their fathers. Second, because the intensification in repression would have come as a harsher shock in terms of employment opportunities to the older cohort, we should find that the relationship between father’s education and participation is especially strong for this older cohort as compared to the younger.

We use the following regression specification to test these predictions:

$$Y_i = \alpha_0 + \alpha_1 \text{Father's ed.}_i + \alpha_2 \text{Older cohort}_i + \alpha_3 \text{Father's ed.} \times \text{Older cohort}_i + X_i' \alpha_4 + \eta_i, \quad (3)$$

where “Older cohort” is an indicator variable,  $X_i$  are the same controls as above, and  $\eta_i$  is an error term assumed to be clustered at the commune level. For the first test,  $Y_i$  is years of education attained by the subject, which tests the prediction that the older cohorts had access to more educational opportunities since they were educated pre-1972. For the second test,  $Y_i$  is participation in the armed revolt, which tests our prediction that this older cohort would have subsequently had more motivation to revolt. In both cases, our hypothesis is that  $\alpha_3 > 0$ . To distinguish between older and younger cohorts, we consider indicators that distinguish between cohorts above and below 12 and 16 years of age in 1972. These correspond, respectively, to ages of completion of primary school and either completion of junior secondary school or, more likely, entry into the work force (UNESCO 2015).



## Basic Results

The results of the basic test, formalized by Equation (1), are shown in Table 2. We see in Models 1 and 2 that subject's own education is positively related to participation likelihood. The estimates are the same controlling for age via a continuous specification (Model 1) or using age-specific fixed effects (Model 2). Models 3 and 4 show that father's education is also positively correlated with participation. From a base participation probability of about 0.015 (not shown), participation probability increases by about 0.003 (about 20% over the base rate) for each year increase in either own or father's education.

We described three effects that influence the relationship between own and father's education as measures of productive potential: anticipation effects; the mechanical effect from people leaving school to join the rebellion; and the fact that repression changes the mapping from productive potential to each of these measures. We find that the coefficients are almost identical for both measures, suggesting that these effects offset each other. It is also important to note that father's education is still significantly correlated with participation in Model 7 which includes own education. This suggests that, even in the face of individual-level variation in access to opportunities to realize one's own productive potential, perceptions of what one *could have* accomplished remain predictive of participation. Models 5 and 6 account for survival probability; the results remain similar. The Appendix shows robustness to accounting for the effects of out-migration, by limiting the analysis to subjects who resided in interior provinces as opposed to border provinces.<sup>1</sup>

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<sup>1</sup> The opportunity to leave may not have been so compelling as to undermine the revolt. First, conditions for migrants were often harsh, even for the relatively wealthy; options to flee were limited by upheaval in neighboring countries. Second, migrant communities in neighboring countries were sites of recruitment, due to their safe distance from the Burundian government.

Table 2: Participation in revolt as a function of own and father's education

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
(Constant)	0.049*	0.066**	0.050*	0.064**	0.051*	0.054**	0.043*
	(0.020)	(0.024)	(0.020)	(0.024)	(0.020)	(0.020)	(0.020)
Age in 1972	-0.001*		-0.001*		-0.001*	-0.001*	-0.001*
	(0.000)		(0.000)		(0.000)	(0.000)	(0.000)
Age in 1972 sq.	0.000		0.000		0.000	0.000	0.000
	(0.000)		(0.000)		(0.000)	(0.000)	(0.000)
Subject's ed. (yrs.)	0.003**	0.003**			0.003***		0.003***
	(0.001)	(0.001)			(0.001)		(0.001)
Father's ed. (yrs.)			0.003*	0.003*		0.003*	0.002 <sup>†</sup>
			(0.001)	(0.001)		(0.001)	(0.001)
Logit survival pr.					-0.007**	-0.006**	-0.006**
					(0.002)	(0.002)	(0.002)
<i>N</i>	905	905	905	905	905	905	905
<i>R</i> <sup>2</sup>	0.018	0.024	0.017	0.023	0.025	0.022	0.026

<sup>†</sup> significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$ . Weighted least square estimates. Standard errors account for clustering at the commune level (number of clusters is 65). Dependent variable is participation in armed revolt (0,1). Primary independent variables of interest are subject's education and father's education. All models control for pre-war home province fixed effects. All models control for age, with models 2 and 4 using age-specific fixed effects. The logit of survival probability controls for predicted survival probability, to account for attrition bias.

We now turn to analysis that tests motivating assumptions of our empirical strategy. Table 3 shows results for a test of our theory's primary empirical assumption. These results show that Hutu men experienced severe repression that did not allow them to realize their perceived productive potential. We measure this effect of repression using data from both Hutu and Tutsi men, as formalized by Equation (2). The regression controls for age, as well as province fixed effects and survival probability (coefficients omitted for space).

Education and occupational attainment should be increasing in father's education for Tutsi men, but this relationship should be attenuated for Hutu men. Our results indicate this. The results for Tutsi men show that education increases in age, but in a manner that levels off at a certain point (hence the positive coefficient on age, and negative on age-squared). Those with more educated

fathers tended to pursue more education themselves. These patterns are substantially attenuated for Hutu men. The partial effect of age is nearly zeroed out. Furthermore, the partial relationship between father's and own educational attainment for Hutus is about half that among Tutsis. For occupational attainment, the regression coefficients are not statistically significant, although the signs point in the expected direction. These results suggest that, while father's education is a strong predictor of one's own education under no constraints, it has far less predictive power for education levels attained by Hutus who suffered barriers of repression.

Tables 4 and 5 show results for tests of the assumptions that educational attainment is associated with productive potential and that constraints were present before the war. We expect that these measures of productive potential should provide little traction in explaining occupational attainment *prior* to the war; but after the war, when many constraints to mobility were lifted, the positive relationship between education and occupation should strengthen. Table 4 provides partially consistent evidence. The pattern holds for *father's* education as a measure of productive potential (columns 2 and 4), as we see an order of magnitude difference in the strength of the association going from pre-war to post-war occupational attainment. The relationship does not hold as strongly with respect to *subject's own* educational attainment. A possible interpretation is that anticipation effects mask the expected relationship. If in the pre-war period Hutu men could accurately anticipate whether educational attainment would pay off, and responded accordingly, then a consistent relationship between occupational and educational attainment may hold over time.<sup>2</sup>

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<sup>2</sup> We also see that the coefficient on age has a different sign as we go from models predicting prewar to post-war occupation. Younger Hutu men were perhaps better able to pursue higher status occupations after the war than older Hutu men, while before the war, the younger males in our sample were simply too young to do so. We thank a reviewer for pointing this out.

Table 3: Subject's education and occupational ranking as a function of father's education (all-ethnicity sample)

	Pre-war ed.	Pre-war occ. ranking
(Constant)	5.217*** (0.725)	0.355** (0.122)
Age in 1972	0.223*** (0.065)	0.045** (0.015)
Age in 1972 sq.	-0.018** (0.006)	-0.003* (0.001)
Father's ed. (yrs.)	0.369** (0.113)	0.005 (0.023)
Hutu	-1.054 (0.690)	-0.058 (0.093)
Father's ed.X Hutu	-0.212† (0.126)	-0.003 (0.026)
Age X Hutu	-0.253** (0.083)	-0.017 (0.017)
Age sq. X Hutu	0.019* (0.008)	0.002 (0.001)
<i>N</i>	1090	1090
<i>R</i> <sup>2</sup>	0.277	0.202

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$ .  
 Weighted least square estimates. Standard errors account for clustering at the commune level (number of clusters is 66). D.V. in Model 1 is respondent's own years of education before the war (0-18). D.V. in Model 2 is respondent's own occupational ranking before the war (-1,0,1). Sample includes both Hutu and Tutsi male respondents. All models control for pre-war province fixed effects and survival probability.

Table 5 shows the results for the test on the military ranks (1-15, from simple soldier to general) obtained by those who participated in the rebellion. Patterns are consistent with our expectations. The association between education and outgoing ranks is strong for both one's own educational attainment and that of one's father. The association with incoming rank is weaker. These results help to validate our use of educational attainment as a proxy for productive potential: within the insurgent organization, individuals' productive potential was not repressed as it was by the Tutsi regime. As such, those with higher education were able to achieve upward mobility within the

Table 4: Occupational ranking as a function of own and father's education

	Pre-war occ.	Pre-war occ.	Post-war occ.	Post-war occ.
(Constant)	-0.153 (0.141)	0.087 (0.138)	0.047 (0.104)	0.180 <sup>†</sup> (0.107)
Age in 1972	0.030*** (0.007)	0.028*** (0.008)	-0.010* (0.004)	-0.010* (0.004)
Age in 1972 sq.	-0.001* (0.001)	-0.001* (0.001)	0.000 (0.000)	0.000 (0.000)
Subject's Ed. (yrs.)	0.060*** (0.009)		0.051*** (0.007)	
Father's ed. (yrs.)		0.003 (0.010)		0.020* (0.010)
<i>N</i>	905	905	905	905
<i>R</i> <sup>2</sup>	0.306	0.195	0.284	0.197

<sup>†</sup> significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$ . Weighted least square estimates. Standard errors account for clustering at the commune level (number of clusters is 65). D.V. is occupational rank (-1,0,1). All models control for pre-war province fixed effects and survival probability.

ranks. These results also demonstrate that educated Hutus may have had a higher expected value from participation in the insurgency. The revolt itself represents a new institution that allows Hutu men to express their productive potential.

The findings in Tables 3 to 5 show that, prior to the war, opportunities for Hutu men were relatively flat across father's education. Father's education nonetheless predicts participation. Moreover, the relationship between attainment and father's education changed dramatically after the war, once barriers to mobility were lifted. These results suggest that it was *perceived productive potential* rather than *pre-war status per se* determining likelihood of participation.

Table 5: Military ranks as a function of own and father's education (rebels only)

	Incoming rank	Incoming rank	Outgoing rank	Outgoing rank
(Constant)	0.634 (0.486)	2.420*** (0.389)	1.505* (0.616)	6.794*** (0.715)
Age in 1972	-0.021 (0.021)	-0.013 (0.022)	-0.051 <sup>†</sup> (0.027)	-0.036 (0.032)
Age in 1972 sq.	0.002 (0.001)	0.001 (0.002)	-0.003 (0.002)	-0.003 (0.003)
Subject's Ed. (yrs.)	0.241*** (0.038)		0.704*** (0.050)	
Father's ed. (yrs.)		0.019 (0.033)		0.200*** (0.042)
<i>N</i>	432	432	435	435
<i>R</i> <sup>2</sup>	0.254	0.164	0.426	0.152

<sup>†</sup> significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$ . Weighted least square estimates. Standard errors account for clustering at the commune level (number of clusters is 65). D.V. is military rank (1 simple soldier - 15 general). All models control for pre-war province fixed effects and survival probability.

## Cohort-Based Test

We now turn to our analysis of the discontinuous increase in repression after 1972. Table 6 serves to validate the assumptions that motivate this analysis. Using the specification set forth in equation (3), we examine cohort-specific differences in the relationship between father's educational attainment and one's own educational attainment. The results show that for the older cohort, father's education is a stronger predictor of one's own educational attainment. This holds whether the older cohort is defined by the cut point at age 12 or 16 in 1972, providing evidence for dramatic differences after 1972 in opportunities that young Hutu men faced in following in the footsteps of their fathers.

Table 7 shows results for the test on participation. When we define the older cohort at the cut point of age 16 (when school typically ends) in 1972, we find that there is a substantively large

Table 6: Education as a function of father’s education, by age categories in 1972

	Model 1	Model 2	Model 3
(Constant)	3.585*** (0.371)	3.541*** (0.338)	3.577*** (0.377)
Age in 1972	-0.026 (0.046)	-0.031 (0.047)	-0.029 (0.047)
Age in 1972 sq.	0.000 (0.004)	-0.000 (0.005)	0.001 (0.005)
Father’s ed. (yrs.)	0.154* (0.061)	0.138* (0.059)	0.149* (0.061)
Age in 1972 > 12		-0.083 (1.368)	
Father’s ed. (yrs.) X Age in 1972 > 12		1.089** (0.400)	
Age in 1972 > 16			-0.726 (1.100)
Father’s ed. (yrs.) X Age in 1972 > 16			0.463* (0.191)
<i>N</i>	905	905	905
<i>R</i> <sup>2</sup>	0.201	0.217	0.203

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$ . Weighted least square estimates. Standard errors account for clustering at the commune level (number of clusters is 65). Dependent variable is respondent’s years of education before the war (0-18). All models control for pre-war province fixed effects and survival probability.

partial relationship between father’s education and participation (*each additional year* of father’s education increases participation likelihood by about six percentage points). For younger cohort members, the estimated relationship is weaker.<sup>3</sup> This is consistent with the story of how a ramping up of repression had a strong effect on Hutus who were able to finish education pre-1972 but then unable to pursue opportunities post-1972.

Figure 1 allows us to visualize the basic patterns in the raw data. Over all age levels, the participation rate for individuals with fathers that had some formal education (solid line) was higher

<sup>3</sup> Figure G.1 in the Appendix displays how the strength of the relationship between father’s education and participation varies continuously over age. The results suggest a sharp “kink” in this relationship around the age of 15 or 16.

Table 7: Participation in revolt as a function of father's education, by age categories in 1972

	Model 1	Model 2	Model 3
(Constant)	0.054** (0.020)	0.053* (0.021)	0.056** (0.020)
Age in 1972	-0.001* (0.000)	-0.001* (0.000)	-0.001* (0.000)
Age in 1972 sq.	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Father's ed. (yrs.)	0.003* (0.001)	0.002* (0.001)	0.002* (0.001)
Age in 1972 > 12		-0.005 (0.010)	
Father's ed. (yrs.) X Age in 1972 > 12		0.011 (0.009)	
Age in 1972 > 16			0.000 (0.010)
Father's ed. (yrs.) X Age in 1972 > 16			0.058† (0.035)
<i>N</i>	905	905	905
<i>R</i> <sup>2</sup>	0.022	0.023	0.029

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$ . Weighted least square estimates. Standard errors account for clustering at the commune level (number of clusters is 65). Dependent variable is participation in armed revolt (0,1). All models control for pre-war province fixed effects and survival probability.

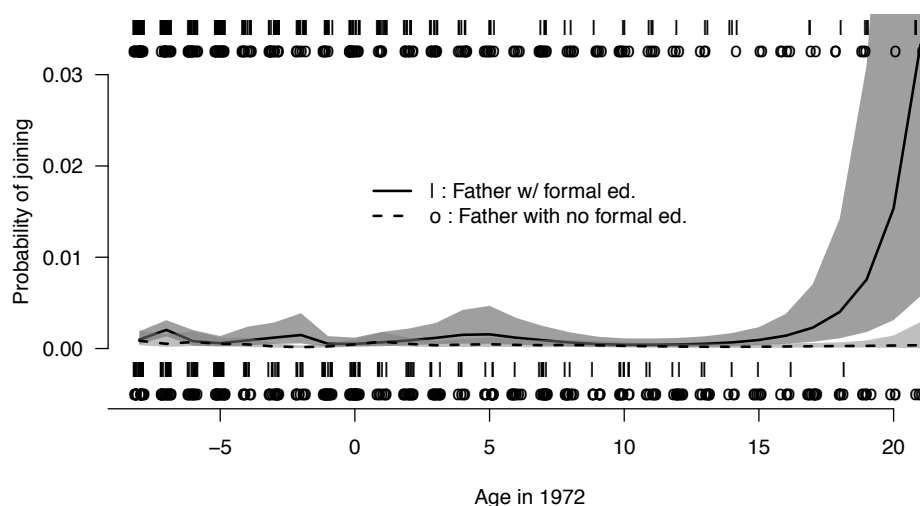
than for those without any formal education (dashed line). However, there is a precipitous widening of the gap at around age 16, the age of those who would have suffered the most dramatic shocks to their access to opportunities following the post-1972 crackdown.

## Checking Alternative Explanations

We explore five potential alternatives to the glass ceiling logic for explaining the positive relationship between education and participation. The Appendix contains a detailed section with analyses of these alternatives. Here we summarize the findings.



Figure 1: Participation probability for Hutu men with educated versus uneducated fathers, by age



The figure shows how the participation rate varies over age in our sample for those whose fathers had no formal education (dashed line) as compared to those whose fathers had at least completed primary school (solid line). The lines are local linear regression smoother fits with 95% confidence bands in gray. The rug plots at the top/bottom of the graph show the raw sample data for rebel joiners and non-joiners respectively, with non-educated (“o”) and educated (“|”) fathers. The graph shows a kink around age 16, corresponding to the cohort-based estimates in Table 7.

There may have been revenge motivations based on the fact that the 1972 crackdown targeted Hutu elites. If the fathers of more educated individuals had been more likely to be killed, then these individuals may have been motivated to avenge their fathers’ deaths rather than frustrations precipitated by pushing against a “glass ceiling.” We do not have explicit information in our survey data on who was targeted in 1972. But we have information on whether respondents’ fathers were alive in 1993. Thus, as an indirect test we fit both Equation (1) and cohort-specific Equation (3) using the indicator for whether the respondent’s father was alive in 1993 as the outcome. Confounding would appear as significant negative coefficients on  $\beta_1$  and  $\alpha_3$ . Another test comes from questions

on the survey that asked rebel recruits their reasons for joining the insurgency. We use the sample of rebel recruits to fit both the basic Equation (1) and cohort-specific Equation (3); the outcome is whether the respondent gave “revenge” as one of their stated reasons for joining the rebellion. If this alternative hypothesis were valid, father’s education would be positively correlated with this revenge response, especially among older cohorts.

As reported in the Appendix, these tests find no support for revenge motivations driving participation. We find no evidence that father’s survival to 1993 was decreasing in father’s education levels. Neither are there negative cohort effects. The education variables do not predict that rebel recruits indicated having participated for revenge purposes. Moreover, recall that in Table 2 we found that one’s own educational attainment was positively associated with participation even after controlling for father’s education. To the extent that vengeance motives were the primary driver, we would expect father’s education to be the primary driver of the effect, and the apparent effect of one’s own education would be spurious. This does not appear to be the case.

The second alternative explanation is the possibility that rebel movements selectively recruited more educated people (Bueno De Mesquita, 2005). To assess this possibility, we use data from our sample of Hutu men who *did not* participate in the insurgency. The survey asked these men whether they had *considered* joining. To test the selective recruitment explanation, we run our basic Equation (1) on this civilian sample, using their responses to this question as the outcome. If recruitment was selective, we would expect the coefficients on education and father’s education to be negative, indicating that undereducated Hutu civilians considered joining but never ended up participating, possibly due to selective recruitment. The results (see Appendix) show that one’s

own education is positively correlated with having considered joining the rebellion (significant at the 10% level), which indicates that there were a significant number of *well-educated* civilians who considered joining the movement but never ended up participating. This contradicts the alternative explanation that rebel leaders selectively recruited educated individuals.

The third alternative explanation proposes that we consider household-level opportunity costs, rather than opportunity costs pertaining only to individuals. The argument is one of omitted variable bias: the relationship between education and participation in rebellion may be spurious as an indication of the effects of “productive potential.” Rather, households with more educated fathers and more educated sons may be wealthier and therefore more able to “afford” a son to join the rebellion. We examined two measures of household opportunity costs that fit this alternative: (1) an index of prewar household wealth and (2) the number of brothers that the would-be recruit had. Each of these should affect the extent to which a household could “afford” one of its sons joining the rebellion. The index of pre-war wealth is the sum of “yes” responses to questions asking about whether the respondent’s household owned land, cows, a radio, and beds with bed sheets.<sup>4</sup> If the findings with respect to education are indeed spurious, then controlling for these factors should nullify the relationship between educational attainment and rebellion. We show that the relationship between educational attainment and participation remains strong after controlling for number of brothers and pre-war wealth.

The fourth alternative explanation is that education yields resources—such as skills or networks—that facilitate collective action in ways that are distinct from economic opportunities, and thus our

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<sup>4</sup> The last two items were selected based on focus group discussions, which indicated that these factors would be highly discriminating of households in terms of their wealth in 1993, while being very easy for respondents to recall.

focus on economic opportunities may be misguided. We assess this possibility by examining a few additional variables. The first is whether the family possessed a radio prior to war, which would facilitate access to collective-action relevant political information. The second is whether respondents reported that someone from a rebel faction had ever told them about the possibility of joining the group, which would be indicative of access to the insurgent recruitment network. The third is whether the respondent tended to spend most time with friends or family prior to the war, which is intended to measure whether the respondent was active in peer networks that were more likely to be the social settings for political engagement and insurgent participation. We show in the Appendix that our education variables do indeed positively predict these indicators of collective action resources, but controlling for them does not weaken the association between the education and participation in our results, suggesting that they do not substantially mediate this relationship.

The fifth alternative is that education is a gateway to political socialization, which can motivate participation in distinct ways (Krueger, 2007). To assess this, we used a question from the survey asking respondents which political party they supported in 1993. The available responses were to support the incumbent and predominately Tutsi UPRONA party, the challenger and Hutu-dominated FRODEBU party, none of these parties, or some “other” party. Only a small number (26 respondents in total) reported “other,” and when asked for more details, these respondents overwhelmingly reported supporting the more radical Hutu-liberationist PALIPEHUTU party. So, we can take “other” responses as proxies for more radical political positions. We checked whether (i) more educated respondents were either *less* likely to indicate support for “none” (implying a political socialization effect) or *more* likely to support “other” (implying a radicalization effect) and

(ii) controlling for these political support variables reduces the strength of the relationship between the education variables and participation. Results offer no indication of either a political socialization or radicalization effect, and controlling for these variables does not weaken the association between the education variables and participation.

## Conclusion

In order to better understand how to prevent future conflict, we must understand motivations for rebellion.<sup>5</sup> We contribute along these lines by considering the logic of “glass ceiling” revolt based on macro-theories of horizontal inequality and the more micro-logic of relative deprivation. We hypothesize that in situations of horizontal inequality enforced by group repression, it is those with relatively higher productive potential whose ambitions are most frustrated and therefore have the strongest motivation to revolt. We test this argument using micro-level data from Burundi, and find evidence that is consistent with the logic. We find participation likelihood rises with one’s own and father’s education. A more fine-grained test that leverages the abrupt increase in repression following 1972 provides further evidence consistent with the predictions of the theory. We test and find no support for various alternative explanations.

As discussed by Blattman and Miguel (2010), research on civil war and political violence has tended to focus on material opportunity costs as determining individuals’ relative propensities to join armed groups (e.g., Collier and Hoeffler, 2004). Our theoretical analysis does not dismiss

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<sup>5</sup> Gurr writes in the fortieth anniversary reissue of *Why Men Rebel*, “the ultimate normative purpose of this kind of conflict analysis...is to help us—political activists, policy makers, and scholars—understand how to build more just and peaceful societies” (2011, p. xvii).

entirely the importance of opportunity costs. Holding productive potential fixed, we agree that lower opportunity costs should be associated with higher likelihood of participation. The logic we develop and the empirical results we show do, however, propose that other forces associated with relative deprivation are also at work, and that these other forces are important.

Our study has implications for debates about the role of “grievances” in civil war participation. Our results indicate the plausibility of the “glass-ceiling” logic as a micro-mechanism to explain the cross-national relationship between horizontal inequality and revolt (Cederman, Gleditsch and Bugaug, 2013; Østby, Nordas and Rød, 2009). The “glass ceiling” logic is distinct from that of Kuhn and Weidmann (2015), for whom the key mechanism is higher-income members of excluded groups recruiting lower-income members to do the fighting. The evidence presented here is not consistent with such an alternative account. Our results are consistent with findings that more educated individuals tend to join Islamic militant movements (Gambetta and Hertog, 2007; Krueger, 2007).

Others may argue that horizontal inequality could, theoretically, lead to civil war because it facilitates mobilization rather than being a source of grievance (Gubler and Selway, 2012). In addition, some suggest that participation among relatively well-off individuals may be merely a function of such individuals’ greater interest in politics (Krueger, 2007; Lee, 2011). But such alternative accounts leave unspecified the motivations for participating in insurgency. They cannot explain patterns like participation among the relatively better off *within the repressed group* or even among those with higher achieving fathers’ who were not given the opportunity to pursue their interests themselves (Tables 3 to 5).

Others still remain skeptical that such grievances can provide much explanatory leverage given that certain macro-level indicators of potential grievance seem to explain little variation in the onset of civil war after accounting for things like income (Collier and Hoeffler, 2004; Fearon and Laitin, 2003). A way to resolve this apparent paradox may be to distinguish between macro and micro levels of analysis. Given the complexity of large scale insurgent mobilization, it is no wonder that macro-level factors could intervene to obscure the relationship between grievances and the onset of civil war (Gurr, 1970, Ch. 8-10). Nonetheless, in examining why people participate in insurgency, the role of grievances could be much more pronounced. The evidence presented here, as well as in studies such as Petersen (2002), support this proposition. At the same time, as Weinstein (2006) argues convincingly, there are a variety of modes of participation and recruitment depending on structural context. The structural conditions that define the glass-ceiling logic, namely high levels of horizontal inequality enforced through repression, are important scope conditions. Policies seeking to prevent armed revolt could look into why such conditions obtain and how they may be unwound. Further research might uncover other modes of participation that link additional macro-structural factors to micro-level motivations, thus providing a richer understanding of the causes of revolt with clearer implications for policy.

Finally, our findings contribute to the literature on education and conflict. Thyne (2006) finds that at the country level, higher levels of education are associated with less civil war onset. But micro-level patterns are not so uniform. Campante and Chor (2014) find that when macro-level economic conditions are poor, high education is associated with more participation in protest. Such findings, when viewed alongside the findings in this paper, reveal how macro-level inequality may

pervert the relationship between education and peace.



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# Repressed Productive Potential and Revolt: Insights from an Insurgency in Burundi

*Appendix of Supporting Information*

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## A Formal model

We consider how repression and horizontal inequality interact with individual economic conditions to induce preferences to revolt. Our analysis builds on a “tipping model” of insurgent participation (Medina, 2007, 2013; Schelling, 1978; Wood, 2003). Suppose a society under the domination of an incumbent ruling group. Members of a non-ruling group are indexed by  $i$ . Each non-ruling group member is characterized by a value,  $a_i > 0$ , capturing lifetime, unfettered productive potential. Status quo policies, enforced by the ruling group, are conceptualized in terms of a parameter,  $0 < \tau < 1$ , that defines the share of the non-ruling group members’ product that they get to keep for themselves, with the remainder hoarded by the ruling group. Barriers to mobility under the status quo become targets of political change for members of repressed groups, and such desired change may provide motivation for revolt (DeNardo, 1985; Glaeser, Ponzetto and Shleifer, 2007; Medina, 2007; Wood, 2003). The non-ruling group members face a choice of whether to participate ( $P$ ) in a revolt or not ( $N$ ), which we denote in terms of a strategy  $s_i \in \{P, N\}$ , respectively. The probability that a revolt will be successful is given by  $\omega(f_t)$ , where  $f_t$  is the share of the non-ruling group that is participating as of period  $t$ . Participation comes at an individual-specific cost,  $c_i > 0$ . This term captures net costs of joining, regardless of the outcome of the revolt. One could think of this as a reduced form way of expressing opportunities forgone, assuming that under the condition of not joining, the individual is choosing the next best option (which could even include leaving the country).

If the revolt fails, the regime does not change. Those who participated in the failed revolt pay the cost,  $c_i$ , and receive no benefit. Those who did not participate in the failed revolt continue to receive the status quo ex ante payoff,  $\tau a_i$ . If the revolt succeeds, then those who participated receive the value of their full productive potential net of costs,  $a_i - c_i$ . Those who did not participate receive some lesser share of their productive potential,  $\phi a_i$ , where  $0 < \tau < \phi < 1$ . The parameter  $\phi$  captures the idea that veterans of a successful revolt are often privileged in the post-revolt era relative to those who never participated.

Given these parameters, the expected utility of participation is

$$\pi(P) = \omega(f_t)a_i - c_i,$$

while the expected utility of non-participation is

$$\pi(N) = [\omega(f_t)(\phi - \tau) + \tau]a_i.$$

Figure A.1 shows how payoffs for participation and non-participation vary over the probability of a successful revolt. The solid lines graph payoffs for an individual with high productive potential ( $i = 1$ ), while the dashed lines graph payoffs for an individual with low productive potential ( $i = 2$ ). We set the cost of participation to be equal for both individuals ( $c_1 = c_2 = c$ ) so that we can see clearly the effect of changes in productive potential ( $a_i$ ). For the individual with high productive potential, participation is preferred to non-participation so long as that individual believes that  $\omega(f_t) > \omega_1^*$ . For the low productive potential individual, no beliefs would lead to a preference for revolt, as  $\pi_2(N) > \pi_1(P)$  for all values of  $\omega(f_t)$ , and so this individual is always “inframarginal” relative to the participation threshold. Wood (2003, pp. 270-274) discusses various

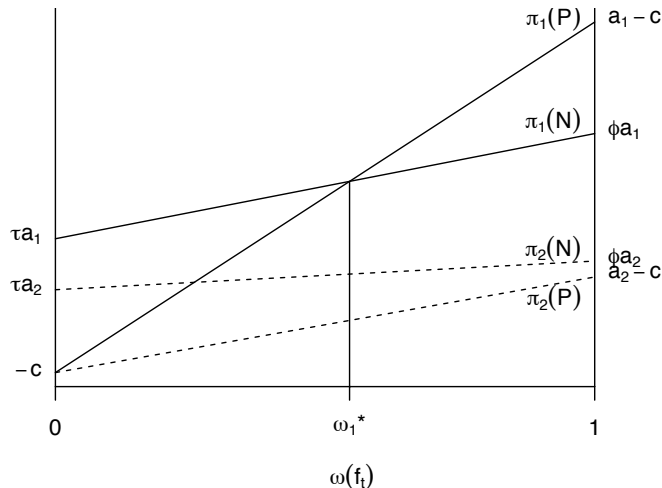


Figure A.1: Payoffs from participation ( $P$ ) and non-participation ( $N$ ) over the probability of a successful revolt ( $\omega(f_t)$ ) for individuals with high productive potential ( $i = 1$ ) and low productive potential ( $i = 2$ ) but the same costs of participation ( $c$ ).

processes through which beliefs about  $\omega(f_t)$  might be shaped, such as insurgents publicizing their victories.

These primitives yield a coordination game with high participation and no participation equilibria (Medina, 2007; Wood, 2003). If an individual believes  $\omega(f_t) > \omega_i^*$ , then that individual will participate. Then in period  $t+1$ , we can assume that such participation will induce others to update their beliefs such that  $\omega(f_{t+1}) > \omega(f_t)$ . This makes it rational for those with  $\omega(f_{t+1}) > \omega_j^*$  to want to participate, marking a step in a participation cascade (Granovetter, 1978; Kuran, 1989).

As Figure A.1 makes plain to see, the threshold value of  $\omega(f_t)$  at which an individual determines that participation is preferable to non-participation is decreasing in productive potential. Call the threshold for an arbitrary individual  $\omega_i^*$ . Then, for individuals with  $0 < \omega_i^* < 1$ ,

$$\omega_i^* = \frac{\tau a_i + c_i}{a_i[1 - (\phi - \tau)]},$$

in which case,

$$\frac{\partial \omega^*}{\partial a_i} < 0.$$

This partial relationship will be determinative insofar as the costs of participation,  $c_i$ , do not covary strongly with  $a_i$ . As discussed above,  $c_i$  measures peaceful opportunities forgone when one decides to join. Many economic accounts of insurgency presume that material opportunity costs under the status quo will tend to override whatever gains the revolt might promise to bring about (e.g., Collier and Hoeffler, 2004; Miguel, Satyanath and Sergenti, 2004). However, in situations where repression is severe, status quo opportunities may vary only slightly from person to person, while productive potential will vary substantially. Then, those with high productive potential may have

only a very limited range of status quo opportunities that they would have to forgo by joining the revolt, and they may be attracted to the rewards that a successful revolt would offer (Medina, 2007). Our theoretical discussion leads to the following hypothesis:

*H<sub>a</sub>: Among harshly repressed groups (that is, groups for which  $\tau$  is of substantial magnitude and  $c_i$  does not covary strongly and positively with  $a_i$ ), participation will be increasing in productive potential ( $a_i$ ).*

This stands the conventional economic analysis on its head, insofar as those with high productive potential are typically those whose status quo economic conditions appear favorable relative to their in-group peers. The difference is due to the interaction between the contextual and individual conditions.

This analysis focuses on a form of relative deprivation defined in terms of comparisons to a counter-factual of realizing one's own productive potential under less repression, rather than comparisons to other individuals (Olson and Roese, 2002). Knowing someone's position in the income distribution, say, would be inadequate to determine whether someone senses such deprivation. In order to determine levels of deprivation, it is necessary to measure the repressive constraints an individual faces and whether those constraints are visible enough to the individual—and attributable to the actions of certain groups—so as to provoke a comparison of what the individual feels she is capable of achieving versus what she is realistically *able* to achieve. Unfavorable counterfactual comparisons fuel resentment toward the custodians of the status quo. Individuals pressing hardest against the “glass ceiling” will have the most profound desire for political change, and thus have the strongest preferences for revolt. More immediately, individuals with high self-perceived productive potential are likely to see themselves as suited for leadership roles in the rebellion and under any new regime that it ushers in (VanBelle, 1996).<sup>1</sup>

The main macro-level scope condition for our hypothesis is the level of repression, which affects  $\tau$  and the correlation between  $a_i$  and  $c_i$ . Under less oppressive circumstances, it may be argued that a class of elites with motivation to stir-up political change would hire lower-class members of their group as the labor behind a movement to overthrow the current regime (Esteban and Ray, 2011; Esteban, Mayoral and Ray, 2012). It is when repression is severe enough, meaning the individuals with high productive potential are not able to access sufficient resources under the status quo, that individuals with high preference for revolt would have ample motivation to join the revolt themselves, and may not even be able to finance the participation of others in the revolt.

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<sup>1</sup> Chinodya (1990) provides a moving account of how opportunities afforded to those who participated in the Zimbabwean rebel movements translated into privileges and positions of power after independence and the assumption of majority rule.



## B Burundi case background

Like neighboring Rwanda, the social-political system of Burundi is marked by the caste-like Hutu-Tutsi ethnic structure—an example of what Horowitz (1985) calls a “ranked” ethnic system. The political system in place by the early 1990s was an intensified version of a hierarchical ethnic system that had prevailed at least since colonial times (1903-1962) and most likely for a considerable period prior.<sup>2</sup> Over the decade following independence, power and access to elite opportunities were concentrated in the hands of a southern Tutsi elite. Hutu “liberationist” organizations emerged in the 1970s, including the *Parti pour la liberation du peuple hutu* (PALIPEHUTU) and its armed wing, the *Forces nationales de liberation* (FNL), as well as the *Front pour la liberation nationale* (FROLINA) and its armed wing, the *Forces armees du peuple*. While Tutsi elites dominated the national political system to the exclusion of Hutus, at the more local level and in non-elite spheres, some opportunity for Hutu mobility remained.

A Hutu uprising that took place in April 1972 led to a discontinuous increase in repression of Hutu opportunities and upward mobility. We use this tragic event as the basis of a cohort-based test of the effects of increased repression. We now provide relevant historical details to explain the cohort-based test.

Political unrest began in the southwest of the country and triggered the mutiny of certain Hutu troops in the army. The uprising has been variously attributed to Hutu militants originally associated with the Burundi Workers’ Party, as well as renegade members of the Burundian gendarmerie, among other accounts (Lemarchand, 1994; Manirakiza, 1992; Reyntjens, 1994). Whatever the cause, the consequence was a genocidal crackdown implemented by the southern-Tutsi-dominated military. Death toll figures typically mentioned are between 100,000-200,000, almost exclusively Hutu (Ngaruko and Nkurunziza, 2000). The army’s response in 1972 is sometimes characterized as a “decapitation” of Burundi’s Hutu subpopulation.<sup>3</sup> The army’s strategy was apparently based on a fear that upwardly mobile and ambitious Hutus would inevitably seek to overthrow the southern-Tutsi-dominated regime. Therefore, Hutu men in relatively high positions—teachers, doctors, bankers—were targeted for imprisonment or execution. This drove many Hutu intellectuals into exile, largely to Rwanda. It instilled fear among Hutu families that caused many to avoid sending their children to school, widening the gap in formal educational attainment across ethnic lines (Jackson, 2000). The generation of Hutus that grew up in the aftermath of 1972 faced intensified constraints on their ability to realize their ambitions through education or occupational mobility (Lemarchand, 1994, 106-117). In our empirical investigation, we analyze cohorts of men whose opportunities would have been most affected by this shock to see whether they exhibited a heightened proclivity to join the rebellion years later in 1993.

Moving ahead a few decades, the southern-Tutsi dominated government reacted to both internal and external pressures and initiated tentative steps toward democracy in the late 1980s. Pressures

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<sup>2</sup> Lemarchand (1996) and Chretien (2003) provide descriptions of the historical origins of ethnic and political divisions in the Great Lakes region, a topic that is subject to many disagreements.

<sup>3</sup> This characterization is based on authors’ own interviews with journalists in Burundi, family-members of victims of the 1972 violence, and retired army officers who were active at the time of the massacre.

Table B.1: Estimated Sizes of Civil War Forces as of January 2004

Name of Force	Estimated Size
National army & armed police (Forces Armées Burundaises)	45,000
Rebel factions	
CNDD-FDD I (Nkurunziza faction)	25,000
CNDD-FDD II (Ndayikengurukiye faction)	3,000
FNL-PALIPEHUTU I (Rwasa faction)*	3,000
CNDD (Nyangoma faction)	1,000
FNL-PALIPEHUTU II (Mugabarabona faction)	1,000
FROLINA (Kalumba faction)	1,000
PALIPEHUTU (Karatasi faction)	1,000

Source: World Bank (2004), p. 17.

\*Not a party to the peace process until September 2008 and so members were not included in the survey.

and fears embedded in the ethnically divided system mixed toxically and made for a tumultuous democratization process. Competitive and relatively free elections were held in June 1993, which brought Melchior Ndadaye and his *Front pour la démocratie au Burundi* (FRODEBU) to power by huge margins. Ndadaye became the first leader in Burundi's history that was simultaneously civilian, non-Southern, and Hutu. However, Ndadaye's term was abruptly cut off in October 1993 when he was assassinated in a bungled coup attempt by military officers from a hardline Tutsi faction. The coup attempt resulted in what an independent United Nations commission described as genocidal reprisals by Hutu mobs against Tutsi men in the countryside, followed by massacres of Hutus by the national army (United Nations, 1996).

As Alexis de Tocqueville wrote, "evils which are patiently endured when they seem inevitable become intolerable once the idea of escape from them is suggested" (de Tocqueville, 1856). This sentiment captures perfectly the moment for those who had voted Ndadaye into office, only to witness his assassination. Prominent Hutu politicians defected from the government to organize militant resistance to the re-consolidation of a Tutsi-led military oligarchy. These defectors organized politically under the banner of the *Conseil national pour la défense de la démocratie* (CNDD), and soon established a military wing, the *Forces pour la défense de la démocratie* (FDD). The CNDD and FDD claimed that their main goal was not "Hutu liberation" per se, but rather a return to "genuine democracy."

The FDD began to recruit people into the struggle to reinstate a political system that would allow the country's Hutu majority to exercise what its members saw as its fair share of power. The first guerrilla attacks attributed to the FDD took place in January 1994 in the suburbs of Bujumbura, spreading to other areas of the country. The media also recorded an escalation of attacks by the FNL over the ensuing year. Table B.1 shows the breakdown of participant numbers by factions throughout the 1993-2003 war.

Micro-level research by Humphreys and Weinstein (2008) on the Revolutionary United Front in Sierra Leone and Blattman (2009) on the Lord's Resistance Army in Northern Uganda has focused researchers' attention on the role of abduction in insurgent recruitment. Data from the

survey of Burundian rebels (described below) describe a very different situation from these two insurgent groups. While it is true that 67% of the ex-rebels that were interviewed indicated that they feared harm if they did not participate, 83% of these feared harm by the government, and only 28% of these feared harm by the rebels. (The numbers sum to more than 100% because the survey allowed people to indicate fear from both.) Only 7% indicated that they themselves had been physically mistreated by the rebel group prior to their becoming a member, and 15% indicated that someone in their family had been. This is in contrast to reports of mistreatment by government forces, which tally at 56% for respondents themselves and 73% for their families. More than half (51%) indicated that they had desired to join the rebel group for a while before they actually did so. When asked how they were convinced to join, 56% indicated that they convinced themselves, while 25% indicated that their friends convinced them. Finally, 66% indicated that they had friends in the group before they joined. The picture that these self-reports paint is not one where abduction plays a major role, but rather a combination of individual volition, peer pressure, and threats from government forces, similar to what Kalyvas and Kocher (2007) describe for Vietnam and Greece.

This brief historical summary serves to demonstrate that the Burundian civil war is a rich case for the study of the relationship between the structural condition of horizontal inequalities and individual-level motivations for violence. Hutus suffered severe constraints to their educational and professional opportunities under Tutsi minority rule.<sup>4</sup> Our theoretical analysis predicts that those Hutu men who had higher perception of their productive potential would have stronger motivation to revolt. Our basic empirical analysis below tests this prediction using measures of educational attainment as a proxy for self-perceived productive potential. The 1972 uprising led to a discontinuous ratcheting up of constraints on Hutu upward mobility. This allows us to construct a more refined test. The discontinuous increase in repression generated a cohort who had completed school, and thus had their productive potential most clearly revealed to them in the classroom, only to have doors to further opportunities slammed in their faces. Our theoretical analysis suggests that these individuals would have especially strong motivation to rebel.

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<sup>4</sup> Jackson (2000) is a thorough account of obstacles that Hutus faced in the education system. Ngaruko and Nkurunziza (2000, pp. 389-391) discuss barriers to Hutus' access to employment in the public bureaucracy as well as other barriers to Hutu mobility. Lemarchand (1994, 106-117) describes the construction of a "Tutsi ethnocracy" in the aftermath of the 1972 violence.

## C Survey design

As indicated in the main text, the survey sample was a stratified multi-stage sample that was meant to be representative of both the ex-combatant and non-combatant populations as of 2007. Burundi is divided into seventeen provinces, each of these provinces is divided into communes and each of these communes is divided into collines, the smallest administrative unit in Burundi. The first stage of civilian sampling was conducted by randomly selecting communes that were stratified according to size. Then, within each commune, three-quarters of the civilian surveys were conducted at the commune center, and the remaining surveys were conducted in randomly selected outlying collines where the population was more sparse. When the collines were sufficiently small, households were numbered and then randomly selected. In the larger collines and in the city centers, households were randomly selected by having enumerators walk for a specified interval of time, and then surveying the nearest household. Finally, each household respondent was randomly chosen from among a list of all adults residing in the household.

The sample of ex-rebel combatants includes both those who had been demobilized as well as those who were integrated into the new army and police. A minority of ex-rebels were integrated into the new army and police, and the rest were put into the national disarmament, demobilization and reintegration (DDR) program. This selection was dominated by the CNDD-FDD and also involved technical vetting by existing army members and the United Nations. That is why it is crucial that our sample contains both types of ex-rebel combatants. The sample of demobilized combatants was randomly selected from the national DDR program's administrative list, stratified by former group affiliation and rank. Note that inclusion on this list was subject to negligible amounts of self-selection. This is because the material benefits being offered were quite attractive, and there were only negligible numbers of demobilized rebels who could afford to forgo such benefits (Gilligan, Mvukiyehe and Samii, 2013). The sample of army and police members was drawn at random from barrack personnel lists, with the selected barracks being any and all barracks present in the communes selected for the civilian sample. The analyses below use sampling weights to account for the fact that sampling rates differed from commune-to-commune (depending on the population sizes as well as sampling targets) and also to account for the "case-control" or "response-based" sampling design, whereby we purposefully sampled on the basis of rebel participation status, the outcome of interest (Korn and Graubard, 1999, Ch. 9; Manski, 1995, Ch. 4).

The survey sample was a stratified multi-stage sample that was meant to be representative of both the ex-combatant and non-combatant populations as of 2007. The survey was multipurpose, and therefore asked questions about family background; living conditions prior, during, and after the war; participation in armed groups during the war; motivations for joining and perceived aims of the armed groups; experiences with armed groups and third party intervening actors; and attitudes toward post-conflict policies and conditions.<sup>5</sup> Burundi is divided into seventeen provinces, each of these provinces is divided into communes and each of these communes is divided into collines, the smallest administrative unit in Burundi. The first stage of civilian sampling was conducted by randomly selecting communes that were stratified according to size. Then, within each commune, three-quarters of the civilian surveys were conducted at the commune center, and the

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<sup>5</sup> The full survey instrument is available at [\[\[SITE ADDRESS TO BE ADDED\]\]](#).

remaining surveys were conducted in randomly selected outlying collines where the population was more sparse. When the collines were sufficiently small, households were numbered and then randomly selected. In the larger collines and in the city centers, households were randomly selected by having enumerators walk for a specified interval of time, and then surveying the nearest household. Finally, each household respondent was randomly chosen from among a list of all adults residing in the household.

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Because the data are from a retrospective case-control survey, there are certain vulnerabilities to bias that must be addressed. First, there is the possibility of selection bias due to non-random survivorship during the war. We address this issue by controlling for survivorship probability. Another section of this Appendix contains details on differential survival and our methods for addressing it. Briefly, our survey data include rosters of those killed during the war. We use the data from these rosters to estimate survival probability for Hutu men age 13-42 in 1993 and then control for this survival probability in our analysis.<sup>6</sup> A second issue is that retrospective surveys can suffer from the fact that individuals may have poor recall, which can result in noisy measurements. However, given that this study focuses on objective information, such as the respondent's father's level of education, the risk for this type of bias is mitigated.<sup>7</sup>

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<sup>6</sup> We use the linear predictor (or log-odds) from a penalized logistic regression of survivorship on home province fixed effects, father's education, family size, age, and rebel participation status, accounting for survey design features, using a flexible specification.

<sup>7</sup> Questions of this objective nature can be contrasted with survey questions that ask a respondent to remember his or her attitudes or feelings at a certain point in time, which potentially suffer from a much higher concern for recall bias.

## D Survival and attrition

In this section, we describe issues related to survival and the controls that we introduce to account for differential survival probabilities that we observe. We focus on the population of interest for this study—namely, the living population of Hutu males who were between 13 and 42 years of age at the onset of the civil war in 1993. The survey administered to both civilians and those who joined armed groups included a roster that recorded information on all of the respondents’ family members who died between 1993 and the administration of the survey in 2007. The roster also collected information on whether or not the deceased individuals had joined an armed group prior to their death. The roster allowed us to gather data on 488 Hutu males that were aged 13 to 42 years in 1993 but who passed away prior to 2007, of whom 76 had joined the rebellion prior to their death. Note that these figures do not account for the survey weighting, e.g., to account for higher rates of sampling among armed group participants relative to civilians. They merely represent the raw tabulation of the survey data.

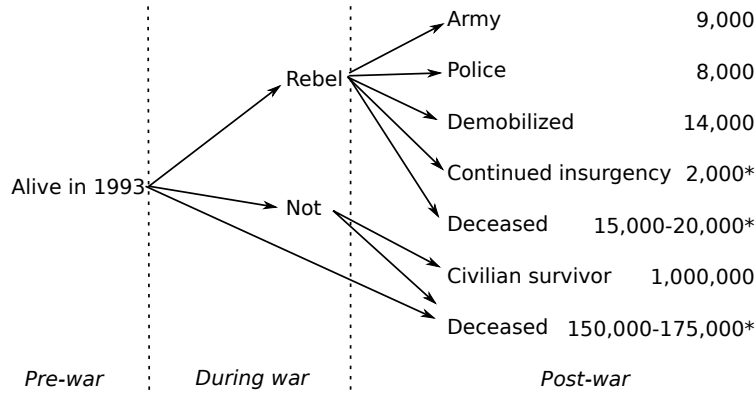


Figure D.1: Wartime and post-war life trajectories for Hutu men aged 13 to 42 in 1993. Figures to the right are estimates on the numbers of men who were in each of the seven listed outcome states as of 2007. Figures with asterisks estimates based on rosters collected in the survey that gathered information on those who died during the war. Sources: authors’ calculations from survey data and Gilligan, Mvukiyehe and Samii (2013, Suppl. Appendix).

Using our various data sources, we can construct a life trajectory diagram for Hutu males above the aged 13 to 42 in 1993. This combines information from our roster on deaths, our survey data on armed group participation, census figures for 1993, the data on the numbers of individuals who joined the various armed groups from Table B.1 in the main text, and finally conventional estimates for the size of the FNL-PALIPEHUTU forces as of 2007. This diagram is displayed in Figure D.1. Starred values in the figure are for those types of individuals that the survey could not sample directly. Our rosters provide proxy information for the deceased. The rosters suggest that the number of deceased was very large. This generally corresponds to conventional estimates used by international organizations such as the United Nations and World Bank, who put the combined

death toll for the entire population at about 300,000.<sup>8</sup>

These deaths represent a form of attrition for our analysis of the 1993 male population. Such attrition could bias our analysis. Specifically, suppose that the probability of death were relatively high among either (i) rebel participants with low levels of education for themselves or their fathers, or (ii) non-participant civilians with high levels of education for themselves or their fathers. Either of these patterns would confound the 2007 relationship between rebel participation and education. Even if there were no causal relationship between education and rebel participation, either of these patterns could induce a spurious positive correlation between rebel participation and education.

The data indicate that such patterns are in fact present. This means that it is important to control for survival probability in our analyses (and in fact we do for the tables presented in the main text). Because the deaths data were collected for siblings of respondents, we know the level of fathers' education for each of the deceased. We do not have data on the level of educational attainment of the deceased. In any case, this would be a censored variable for those who died prior to completing their education. As such, we can conduct an empirical analysis of death rates by father's education, distinguishing between those who joined the rebellion and those who did not. The results of this analysis are shown in Table D.1. The first column shows results of a regression of survival on father's education, whether or not the individual joined the rebellion (recall that we have this information even for the deceased), and then their interaction. (The analysis include home province fixed effects, along the lines of all analyses presented in the main text.) We see that the probability of death is much higher for rebels than civilians, but that this probability declines (by about 6.5 percentage points) for each year of father's education. For civilians, the correlation between father's education and the probability of death is negligible. The second column introduces controls, including age and the number of brothers that someone had. The reason that we control for the number of brothers is because the likelihood that a death would be picked up by the survey is a function of the number of brothers available to be interviewed. We see that the patterns from the first column do not change appreciably upon introducing controls.

These findings indicate that we do indeed need to account for probability of survival so as to reduce the potential for estimating a spurious relationship between education and rebel participation. To do so, we apply the methods proposed by Angrist (1997) and Newey (2009), which is to estimate the probability of survival, and then to control for this probability in one's analysis. We estimate survival probability using age, father's education, number of brothers, whether the individual joined the rebellion or not, and home-province dummy variables. We also include the survey sampling probabilities into the model, given that they contain information relevant to the sampling strata (see the discussion on sampling in the main text). We use a flexible specification, including quadratic terms for all continuous variables and then all two-way interactions. Because we are interested in the predicted values, rather than the coefficients, we use a penalized logistic regression, which is known to have better predictive performance in situations where one includes many higher order terms, interactions, and dummy variables.<sup>9</sup> The coefficients from the penalized

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<sup>8</sup> See, e.g., United Nations News Service, 2007, "Burundi faces hurdles despite progress, Security Council told," *UN Daily News*, p. 6, available at <http://www.un.org/news/dh/pdf/english/2007/21052007.pdf>

<sup>9</sup> We fit the model using the `bayesglm` function in the `arm` package for R. See Gelman et al.

Table D.1: Death as a function of father’s education, rebel participation, and control variables

	Model 1	Model 2
(Constant)	0.333*** (0.089)	0.243* (0.104)
Father’s ed. (yrs.)	0.009 (0.006)	0.010 (0.006)
Rebel participant	0.621*** (0.093)	0.550** (0.169)
Father’s ed. X rebel	−0.065*** (0.020)	−0.064*** (0.019)
Age		0.003 (0.003)
No. brothers		0.024* (0.011)
Age X rebel		−0.005 (0.010)
No. brothers X rebel		0.015 (0.033)
<i>N</i>	1393	1393
<i>R</i> <sup>2</sup>	0.134	0.147
adj. <i>R</i> <sup>2</sup>	0.120	0.132
Resid. sd	9.662	9.599

Weighted least squares with standard errors accounting for clustering at the commune level.

All models account for pre-war province fixed effects.

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

logistic regression fit are displayed in Table D.2.

As a technical note, we find that while number of brothers is strongly predictive of survival probability, it does appear to have a strong relationship to rebel participation after accounting for the other factors that we consider in our main specifications in the main text. These results are presented in the first and third results columns in Table E.4 of the appendix to the main text. (Those results are in a separate section because they are part of a separate discussion that concerns number of brothers as a predictor of participation.) This provides a source of plausibly exogenous variation in our survival probability model, which means that our ability to control for survival probability is not dependent solely on non-linearities in our specification for the model presented in Table D.2 (Angrist, 1997).

In our regressions, we control for survival probability by using the linear predictor (or log-odds) from the penalized logistic regression. This provides a robust semi-parametric approach to controlling for selection bias (Angrist, 1997; Newey, 2009).

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(2008).



Table D.2: Modeling survival probability (penalized logistic regression coefficients)

	Model 1
(Constant)	-0.242 (0.347)
Age	-0.030 (0.024)
Age sq.	0.003* (0.001)
Father's ed.	-0.107† (0.064)
Father's ed. sq.	0.010* (0.005)
Sampling weight	0.002*** (0.000)
Sampling weight sq.	-0.000*** (0.000)
No. brothers	-0.303** (0.114)
No. brothers sq.	0.016 (0.011)
Age X father's ed.	-0.004 (0.003)
Age X sampling weight	-0.000 (0.000)
Age X No. brothers	-0.004 (0.005)
Father's ed. X sampling weight	-0.000 (0.000)
Father's ed. X no. brothers	-0.007 (0.011)
Sampling weight X no. brothers	0.000 (0.000)
Rebel participant	3.064*** (0.395)
Rebel X Age	0.016 (0.023)
Rebel X father's ed.	0.114* (0.051)
Rebel X no. brothers	-0.041 (0.081)
Rebel X sampling weight	-0.008*** (0.002)
<i>N</i>	1393
Standard errors in parentheses	
† significant at $p < .10$ ; * $p < .05$ ; ** $p < .01$ ; *** $p < .001$	

## E Checking alternative explanations

We explore five potential alternative explanations for the positive correlation among Hutu males between participation in the insurgency and one’s father’s and own education. The first alternative explanation has to do with possible revenge motivations based on the fact that the 1972 crackdown targeted Hutu elites. The second is based on the possibility that rebel movements selectively recruited more educated people, as proposed by Bueno De Mesquita (2005) in a review of evidence from Krueger (2007)’s similar findings regarding the positive correlation between socio-economic status and participation in Islamic militancy. The third is based on the possibility that *household* opportunity costs may have been lower for higher-education households, and that our results are spurious to such household opportunity costs. The fourth is based on the idea that education yields resources—such as skills or networks—that facilitate collective action in ways that are distinct from economic opportunities. The fifth is that education is a gateway to political socialization that can motivate participation in ways that are distinct about grievances regarding economic opportunity. This is how Krueger (2007) interprets his own findings.<sup>10</sup> Figure E.1 is a causal diagram that maps these various alternative explanations along with the “perceived productive potential” mechanism that is emphasized by the glass ceiling theory. We see that one’s father’s and own education can each affect self-perceived productive potential, possibly in ways that reinforce such self-perceptions over time, but that education can trigger other mechanisms that could affect participation.

The sections that follow consider these five alternative explanations. Evidence in favor of any of these alternatives would not necessarily render the glass ceiling logic irrelevant, but it would raise questions about its preeminence for interpreting the positive correlation between education and participation. That said, as detailed below, we do not find compelling evidence for these alternatives over and above the glass ceiling logic. This does not prove the truth of the glass ceiling logic, but it does mean that the glass ceiling logic remains as a parsimonious explanation for the various empirical patterns analyzed here (Clarke and Primo, 2012). Table E.7 below gives a summary overview of the findings.

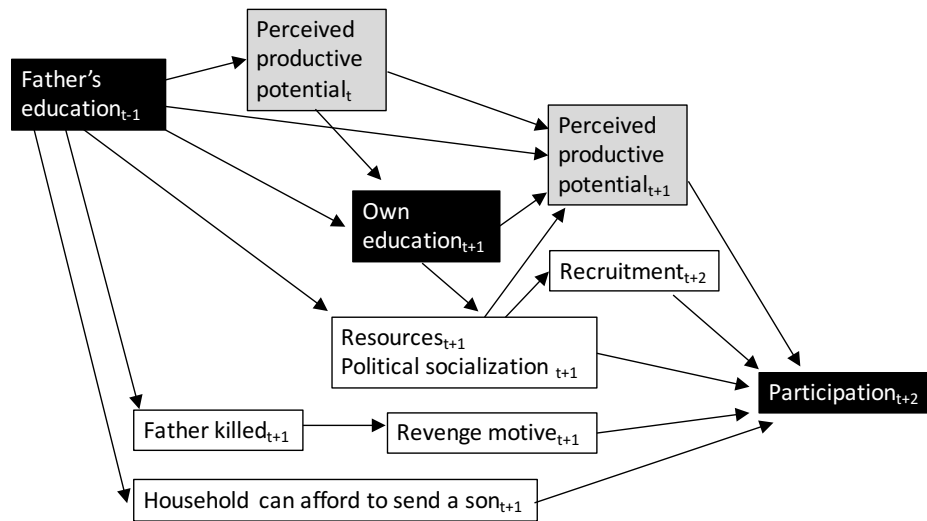
### E.1 Revenge motivations

Higher rates of participation among more educated individuals could have been based on revenge motivations, given that the Tutsi-dominated military regime targeted educated Hutus in the crackdown that followed the 1972 Hutu uprising. If the fathers of more educated individuals were more likely to be killed, then these more educated individuals may have been motivated by the desire to avenge their fathers’ deaths rather than frustrations precipitated by pushing against the “glass ceiling.” We do not have detailed information on who in our sample, exactly, was targeted in 1972. But we do have information on whether respondents’ fathers were alive in 1993. Thus, as an indirect test we fit both the basic equation (1) and cohort-specific equation (3) using as the outcome the indicator for whether the respondent’s father was alive in 1993. Confounding would appear as significant negative coefficients on  $\beta_1$  and  $\alpha_3$ .

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<sup>10</sup> We thank anonymous reviewers for proposing the other potential alternative explanations.

Figure E.1: Causal diagram illustrating potential alternative explanations



The figure shows various causal pathways through which father's and own education may cause participation in the insurgency. The black boxes show the variables that we have measured and include in the statistical analysis. The gray boxes show the causal mechanism that involves "perceived productive potential," which is the mediating variable that is highlighted by the glass ceiling theory. The white boxes show alternative channels that we attempt to address in the analyses in this section.

The survey also asked rebel recruits their reasons for joining the insurgency. As a second attempt to test this alternative revenge hypothesis, we use the sample of rebel recruits to fit both the basic equation (1) and cohort-specific equation (3), where the outcome is whether the respondent gave "revenge" as one of their stated reasons for joining the rebellion. If this alternative hypothesis were valid, father's education would be positively correlated with this revenge response, especially among older cohorts.

Tables E.1 and E.2 show results for these tests of vengeance motivations. We find no evidence that father's survival to 1993 was decreasing in father's education levels (Table E.1, Model 1). Neither are there negative cohort effects (Models 2 and 3). Finally, recall that in Table 2 in the main text we found that one's own educational attainment was positively associated with participation even after controlling for father's education. To the extent that vengeance motives were the primary driver, we would expect father's education to be the primary driver of the effect, and the apparent effect of one's own education would be spurious. This does not appear to be the case. If anything, fathers' survival probabilities are increasing in education, and this positive relationship is stronger for older cohorts. As Table E.2 in the appendix shows, none of the productive potential variables significantly predict a response that a rebel recruit participated for revenge purposes. At first glance this may appear as puzzling given that the historical record suggests the targeting of high status individuals in 1972. However, the 1972 killings by the regime were largely concentrated in the

Table E.1: Father alive in 1993 as a function of father’s education, by age categories in 1972

	Model 1	Model 2	Model 3
(Constant)	0.920*** (0.097)	0.908*** (0.091)	0.918*** (0.100)
Age in 1972	-0.026*** (0.005)	-0.026*** (0.005)	-0.026*** (0.005)
Age in 1972 sq.	0.000 (0.000)	0.001** (0.000)	0.001 (0.001)
Father’s ed. (yrs.)	0.003 (0.008)	0.000 (0.008)	0.002 (0.008)
Age in 1972 > 12		-0.306* (0.153)	
Father’s ed. (yrs.) X Age in 1972 > 12		0.038 (0.034)	
Age in 1972 > 16			-0.110 (0.161)
Father’s ed. (yrs.) X Age in 1972 > 16			0.053 (0.034)
<i>N</i>	905	905	905
<i>R</i> <sup>2</sup>	0.250	0.260	0.251

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 65.)

Dependent variable is father alive in 1993 (0,1).

All models control for pre-war province fixed effects and survival probability.

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

‡ jointly significant at  $p < .10$ .

southwestern and central portions of the country, in which case the killings targeted a specific segment of Hutu men, while the increased repression applied to Hutus throughout Burundi. For this reason, when looking at national-level patterns, it is quite possible that father’s survival was generally increasing in parts of the country outside the areas targeted by the regime in 1972, and that the sons of high attainment fathers suffered the consequences of a nationwide repression of Hutu mobility.

## E.2 Selective recruitment

For the second alternative explanation, we use data from our sample of Hutu men who *did not* participate in the insurgency. The survey asked these men whether they had *considered* joining. To test the selective recruitment explanation, we run our basic equation (1) on this civilian sample, using their responses to this question as the outcome. If selective recruitment was occurring, we

Table E.2: Participation for “revenge” as a function of father’s education and by age categories in 1972

	Model 1	Model 2	Model 3
(Constant)	0.122*	0.116*	0.126*
	(0.052)	(0.053)	(0.053)
Age in 1972	0.004	0.005	0.004
	(0.003)	(0.003)	(0.003)
Age in 1972 sq.	−0.000†	−0.000	−0.001
	(0.000)	(0.000)	(0.000)
Father’s ed. (yrs.)	0.006	0.007	0.007
	(0.006)	(0.006)	(0.006)
Age in 1972 > 12		−0.102	
		(0.136)	
Father’s ed. (yrs.) X Age in 1972 > 12		−0.016	
		(0.010)	
Age in 1972 > 16			0.130
			(0.163)
Father’s ed. (yrs.) X Age in 1972 > 16			−0.021
			(0.015)
<i>N</i>	446	446	446
<i>R</i> <sup>2</sup>	0.050	0.053	0.053

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 65.)

Dependent variable is participation in armed revolt (0,1).

All models control for pre-war province fixed effects and survival probability.

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

would expect the coefficients on education and father’s education to be negative, since this would indicate that undereducated Hutu civilians considered joining but never ended up participating, possibly due to selective recruitment. As the results in Table E.3 in the appendix show, one’s own education is positively correlated with having considered joining the rebellion (significant at the 10% level), which indicates that in fact there were a significant number of *well-educated* civilians who considered joining the movement but never ended up participating. This contradicts the alternative explanation that rebel leaders selectively recruited based on educational attainment. If recruitment had been selective on education, we would find a concentration of relatively low-educated men in our civilian sample who wanted to join, implying a negative coefficient. Father’s education is not significantly correlated with having considered joining, although the sign on the coefficient is negative. Aside from the statistical imprecision of the estimate, this evidence does not strike us as compelling since it is difficult to image that recruitment would targeted subjects’ fathers’ education more than subjects’ own education.

Table E.3: Whether civilian considered joining, as a function of own and father’s education

	Model 1	Model 2
(Constant)	0.068 (0.148)	0.142 (0.151)
Age in 1972	-0.019** (0.006)	-0.020** (0.007)
Age in 1972 sq.	0.001 (0.001)	0.001 (0.001)
Subject’s ed. (yrs.)	0.012 <sup>†</sup> (0.007)	
Father’s ed. (yrs.)		-0.006 (0.011)
<i>N</i>	459	459
<i>R</i> <sup>2</sup>	0.193	0.187

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 61.)

Dependent variable is whether civilian considered joining (0=no,1=yes).

All models control for pre-war province fixed effects and survival probability.

<sup>†</sup> significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

### E.3 Household opportunity costs

The third alternative explanation proposes that we consider household-level opportunity costs, rather than opportunity costs that pertain only to individual would-be recruits. The argument here is essentially one of omitted variable bias: the relationship between education and participation in rebellion may be spurious as an indication of the effects of “productive potential.” Rather, the argument goes, households with more educated fathers and more educated sons may be wealthier and therefore more able to “afford” a son to join the rebellion. First, it should be noted that this is quite a departure from the conventional “opportunity costs” account, which focuses on the material opportunity costs to the individual recruit. For this alternative account to have traction, it must be that would-be recruits are either altruistic toward their household members, or heads of households can dictate what their sons do.

We examined two measures of household opportunity costs that fit this alternative logic: (1) an index of prewar household wealth and (2) the number of brothers that the would-be recruit had. Each of these should affect the extent to which a household could “afford” one of its sons joining the rebellion. The index of pre-war wealth is the sum of “yes” responses to questions asking about whether the respondent’s household owned land, cows, a radio, and beds with bed sheets.<sup>11</sup> If the findings with respect to education are indeed spurious, then controlling for these

<sup>11</sup> The last two items were selected based on focus group discussions, which indicated that these factors would be highly discriminating of households in terms of their wealth in 1993, while

Table E.4: Evaluating whether factors related to household opportunity costs confound estimates of the relationship between one’s own education and participation

	DV=No. bros.	DV=Rebel	DV=Wealth	DV=Rebel	DV=Rebel
(Constant)	3.694*** (0.696)	0.042* (0.021)	1.929*** (0.157)	0.030 (0.021)	0.029 (0.021)
Age in 1972	-0.009 (0.017)	-0.001* (0.000)	0.002 (0.014)	-0.001* (0.000)	-0.001* (0.000)
Age in 1972 sq.	-0.001 (0.001)	0.000 (0.000)	0.002 (0.001)	0.000 (0.000)	0.000 (0.000)
Subj. ed.	0.050 (0.042)	0.003*** (0.001)	0.078*** (0.023)	0.002** (0.001)	0.002** (0.001)
Father’s ed.	0.004 (0.040)	0.002† (0.001)	0.059* (0.028)	0.002 (0.001)	0.002 (0.001)
Number of brothers in 1993		0.000 (0.001)			0.000 (0.001)
Wealth index				0.007* (0.003)	0.007* (0.003)
<i>N</i>	905	905	905	905	905
<i>R</i> <sup>2</sup>	0.112	0.026	0.178	0.028	0.028
adj. <i>R</i> <sup>2</sup>	0.089	-0.000	0.156	0.002	0.001
Resid. sd	48.175	4.252	27.804	4.248	4.250

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 65.)

All models control for pre-war province fixed effects and survival probability.

In results columns 1 and 3, subj. ed. and father’s ed. are jointly significant with  $p < .05$ .

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

factors should nullify the relationship between educational attainment and rebellion. Table E.4 shows the results of this test. Results columns 1 and 3 show that the education measures are positively correlated with both the number of brothers and wealth (in both cases, subjects’ own and father’s education are jointly significant). We also find that participation probability is increasing in household wealth, although not so for number of brothers.<sup>12</sup> However, what is most important is that the relationship between participation and either one’s own educational attainment or one’s father remains very similar to what we showed in the main text, even after controlling for number of brothers and pre-war wealth. The coefficient on subject’s own education is attenuated only somewhat (from 0.003 to 0.002 change in participation probability per year of education).

being very easy for respondents to recall.

<sup>12</sup> This positive coefficient on wealth provides further empirical indication that wealthier households were not paying poorer individuals to join the rebellion on their behalf.

## E.4 Collective action resources

Insurgent organization requires collective action, and collective action is facilitated by various individual-level resources—for example, network ties and access to organizing information. The function of these resources are captured in our formal model above, in particular in the ways that they affect beliefs about others’ participation (i.e., beliefs about  $f_t$ ). Perhaps the primary role of father’s and own education is to increase networks and access to information, which in turn may make it easier to learn about others’ willingness to participate ( $f_t$  in the model), even if there is no effect on self-perceived productive potential ( $a_i$  in the model)? We assess this possibility by examining a few additional variables from the survey. The first is whether the family possessed a radio prior to war, which would facilitate access to collective-action relevant political information. The second is whether respondents reported that someone from a rebel faction had ever told them about the possibility of joining the group, which would be indicative of access to the insurgent recruitment network. The third is whether the respondent tended to spend most time with friends or with family prior to the war, which is intended to get at whether the respondent was active in peer networks that were more likely to be the social settings for political engagement and insurgent participation.

Table E.5 displays the results of statistical analyses as to whether these factors confound our interpretation of the relationship between education and participation. For these information- and network-related factors to be confounders we should see that (i) they are predicted by the education variables and that (ii) controlling for these factors in the analysis weaken the correlation between the education variables and insurgent participation. Results columns 1, 3, and 5 show that the education variables do indeed positively predict radio access, whether one was informed about joining a rebel group, or tended to spend time more time with peers. However, results columns 2, 4, and 6 show that including these factors into the model that predicts insurgent participation does not affect the coefficients on the education variables.

## E.5 Political socialization

The final alternative that we consider is that one’s father’s and own education may increase access to opportunities for political socialization. Indeed, Krueger (2007) interprets his own finding that militants tended to have higher education and socio-economic status in this way. The argument is that educational institutions themselves are sites of political socialization, and that education itself allows one to read and interpret political dynamics more clearly. Of course, this leaves open the question as to why militancy or insurgency would be the way that educated people engage in politics. Nonetheless, given the weight put on this line of thinking in the existing literature, we examine the potential for such socialization to confound our interpretation of the positive association between education and insurgent participation. The survey asked respondents which political party they had supported in 1993. The question was asked in a way that asked about “support,” not about voting, because many of the respondents in our sample would have been too young to vote in 1993. The available responses were to support the incumbent and predominately Tutsi UPRONA party, the challenger and Hutu-dominated FRODEBU party, none of these parties, or some “other” party. Only a small number (26 respondents in total) reported “other,” and when asked for more



Table E.5: Evaluating whether collective action resources confound estimates of the relationship between education and participation

	DV=Radio	DV=Rebel	DV=Informed	DV=Rebel	DV=Friends	DV=Rebel
(Constant)	0.649*** (0.066)	0.042* (0.020)	0.365*** (0.110)	0.042* (0.020)	0.364*** (0.074)	0.043* (0.020)
Age in 1972	-0.002 (0.006)	-0.001* (0.000)	-0.003 (0.010)	-0.001* (0.000)	-0.016* (0.007)	-0.001* (0.000)
Age in 1972 sq.	0.001† (0.000)	0.000 (0.000)	-0.000 (0.001)	0.000 (0.000)	0.001† (0.001)	0.000 (0.000)
Subj. ed.	0.023** (0.008)	0.003** (0.001)	0.014 (0.011)	0.003** (0.001)	0.043*** (0.009)	0.003** (0.001)
Father's ed.	0.004 (0.009)	0.002† (0.001)	-0.002 (0.012)	0.002† (0.001)	0.015 (0.010)	0.002† (0.001)
HH had radio pre-war		0.003 (0.004)				
Informed by faction pre-war				0.005 (0.005)		
Spent time w. friends pre-war						0.002 (0.004)
<i>N</i>	905	905	905	905	905	905
<i>R</i> <sup>2</sup>	0.133	0.026	0.095	0.027	0.196	0.026
adj. <i>R</i> <sup>2</sup>	0.111	-0.000	0.072	0.000	0.174	-0.000
Resid. sd	11.886	4.252	14.425	4.252	13.346	4.252

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 65.)

All models control for pre-war province fixed effects and survival probability.

In results columns 1, 3, and 5, subj. ed. and father's ed. are jointly significant with  $p < .05$ .

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

details, these respondents overwhelmingly reported supporting the more radical Hutu-liberationist PALIPEHUTU party. So, we can take “other” responses as proxies for more radical political positions. Bear in mind that the survey was carried out in 2007, so these are retrospective self-reports. Assuming there are no systematic biases in the ways that respondents answered this question, we want to check whether (i) more educated respondents were either *less* likely to indicate support for “none” (implying a political socialization effect) or *more* likely to support “other” (implying a radicalization effect) and (ii) whether controlling for these political support variables reduces the strength of the relationship between the education variables and insurgent participation.

Table E.6 shows the results of analyses to check for such patterns. Results columns 1, 2, 3, and 4 offer no indication of either a political socialization or radicalization effect. Result column 5 shows that controlling for the political support variables does not weaken the association between the education variables and participation.

Table E.6: Evaluating whether political socialization confounds estimates of the relationship between education and participation

	DV=Sup. UPRONA	DV=Sup. FRODEBU	DV=Sup. none	DV=Sup. other	DV=Rebel
(Constant)	0.158* (0.072)	0.640*** (0.097)	0.180 (0.130)	0.002 (0.008)	0.029 (0.020)
Age in 1972	0.016** (0.005)	0.030*** (0.006)	-0.047*** (0.007)	0.001 (0.001)	-0.001* (0.001)
Age in 1972 sq.	-0.001* (0.000)	-0.002** (0.001)	0.002*** (0.001)	-0.000 (0.000)	0.000† (0.000)
Subj. ed.	-0.004 (0.006)	-0.019† (0.011)	0.020† (0.011)	0.001 (0.001)	0.003*** (0.001)
Father's ed.	0.011 (0.013)	-0.013 (0.009)	0.004 (0.013)	-0.001 (0.002)	0.003* (0.001)
1993 support for FRODEBU					0.020*** (0.004)
1993 support for none					0.008 (0.006)
1993 support for other					0.126† (0.071)
<i>N</i>	905	905	905	905	905
<i>R</i> <sup>2</sup>	0.135	0.174	0.315	0.045	0.034
adj. <i>R</i> <sup>2</sup>	0.113	0.152	0.297	0.020	0.006
Resid. sd	10.309	13.585	12.542	2.723	4.240

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 65.)

All models control for pre-war province fixed effects and survival probability.

In results columns 1, 2, and 3, subj. ed. and father's ed. are jointly significant with  $p < .05$ .

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

## E.6 Additional considerations

More complex causal stories are possible, but we do not think that they necessarily run contrary to our interpretation. Lee (2011), for example, finds that participation in Bengali militancy is generally increasing in socio-economic status, but that such is also true of participation in non-violent political activism. When one compares militants to non-violent activists, one finds that militants tended to be of relatively lower socio-economic status. Such a pattern is perfectly consistent with our theoretical account when the option of non-violent activism is viable. But this may not always be the case, and to the extent that such political activism is in pursuit of changes of the status quo, a situation such as what we document in Burundi may be expected. Kalyvas and Kocher (2007) point to the fact that in many insurgencies, individuals are forced to join due to threats of victimization by the government. Thus, one might propose that it was the fact that more educated Hutus were targeted by the regime that drove them to participate, rather than the “glass ceiling” logic. Such a proposition merely raises the question, *why* would more educated Hutus be targeted in the first place? A likely answer is that the regime *anticipated* that educated Hutus would be the most likely to organize to fight against the repressive status quo. Most likely, officials proposing to target educated Hutus had in their minds something akin to the glass ceiling logic.

A lingering puzzle is why more educated Hutus did not simply seek to leave rather than fight against the repressive regime. Our sense is that the option to flee was available to only a very small number of highly privileged individuals. Given the extent of repression, such an “exit” option would not have provided enough of a safety valve for the regime. And even for those who left, it

is worth reiterating from our contextual discussion in this Appendix that a number of Hutu leaders who agitated for change had spent time abroad, only to return to Burundi to participate in the fight at home.

Our study focuses primarily on socio-economic factors that relate to insurgent participation. This is not to dismiss the role of religion, culture, or other symbolic factors that may motivate participation (Atran, 2010). We feel that a focus on socio-economic factors is important because of the direct connection between such motivations and the types of economic policy levers that are often applied to mitigate conflict (Berman et al., 2011). Our analysis also seeks to investigate micro-foundations for the important macro-level correlation between horizontal inequality and conflict. A thorough investigation of other, non-economic factors would require much more space than we have here. Nonetheless we encourage complementary research along those lines.

## **E.7 Summary of key results and assessments of alternatives**

Table E.7 provides an overview of the main results from the paper as well as results from statistical checks of potential alternative explanations contained in this appendix. Assessing the totality of the results, the glass ceiling logic remains as a parsimonious and robust explanation for these patterns.

Table E.7: Summary of empirical tests and interpretations

Table Number	Evidence	Interpretation
Table 2	Own and father's education predicts insurgent participation.	Shows that those with higher productive potential are more likely to revolt.
Table 3	Relationship between father's education and own accomplishments much weaker for Hutu as compared to Tutsi males.	Validates the assumption that Hutu men were subject to repression that limited their ability to realize their productive potential, as measured by their father's accomplishments.
Table 4	Father's education predicts own accomplishments more strongly for Hutu men after the war as compared to before.	Validates use of father's education as a proxy for productive potential and shows that Hutu men were indeed subject to repression that was then lifted as a result of the war.
Table 5	For insurgents, own education and father's education predict outgoing rank more strongly than incoming rank.	Validates use of own and father's education as a proxies for productive potential, and shows that those with high productive potential enjoyed mobility within the insurgency, providing a selective incentive.
Table 6	Father's education predicts own education more strongly for cohorts above school age in 1972, as compared to younger cohorts.	Shows that opportunities to pursue education were indeed sharply constrained after 1972, meaning that older cohorts were more likely to cultivate and recognize their own productive potential.
Table 7	Father's education predicts own participation in revolt more strongly for cohorts above school age in 1972, as compared to younger cohorts.	Shows that older cohorts who had relatively greater opportunity to cultivate, only to have doors to mobility slammed in their faces, were especially motivated to revolt.
Tables E.1 and E.2 (appendix)	Positive and insignificant correlation between father's education and father's survival; positive and insignificant cohort effect. Small and insignificant correlation between father's education and indication of "revenge" as motivation for participation among insurgents; negative and insignificant cohort effect for indicating "revenge" as a motivation for participation.	Inconsistent with alternative explanation of participation in revolt being motivated by grief or a desire for revenge over fathers being killed.
Table E.3 (appendix)	Positive and marginally significant correlation between own education and civilians' consideration of joining, and negative but insignificant correlation between father's education and civilians' consideration of joining.	No clear evidence in favor of alternative explanation that recruits were selected on the basis of their productive potential rather than being especially motivated to join on the basis of productive potential.
Table E.4	Relationship between own education and participation is not spurious to either household pre-war wealth or number of brothers.	No clear evidence in favor of alternative explanation that educational attainment is a reflection of whether household was sufficiently well-off that it could "afford" to have some sons join the rebellion.
Table E.5	Relationship between own education and participation is not spurious to measures of collective action resources, such as radio ownership, having been informed by a rebel group about joining, or tending to spend time with one's peers.	No clear evidence in favor of alternative explanation that educational attainment allowed acquisition of personal resources to facilitate participation in collective action.
Table E.6	Relationship between own education and participation is not spurious to levels of pre-war political support for different parties.	No clear evidence in favor of alternative explanation that educational attainment caused political socialization or radicalization that led to insurgent participation.

## F Statistical robustness checks

### F.1 Robustness to out-migration likelihood

Table F.1: Participation in revolt as a function of own and father’s education

	1. Full sample	2. No border prov.	3. Full sample	4. No border prov.
(Constant)	0.051* (0.020)	0.030 (0.021)	0.054** (0.020)	0.041* (0.017)
Age in 1972	-0.001* (0.000)	-0.001 (0.001)	-0.001* (0.000)	-0.001 (0.001)
Age in 1972 sq.	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Subject’s ed. (yrs.)	0.003*** (0.001)	0.002 <sup>†</sup> (0.001)		
Logit survival pr.	-0.007** (0.002)	-0.009** (0.003)	-0.006** (0.002)	-0.009** (0.003)
Father’s ed. (yrs.)			0.003* (0.001)	0.002 (0.002)
<i>N</i>	905	424	905	424
<i>R</i> <sup>2</sup>	0.025	0.024	0.022	0.024

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

Dependent variable is participation in armed revolt (0,1).

All models control for pre-war province fixed effects.

Models 2 and 4 drop observations in border provinces.

<sup>†</sup> significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

Table F.1 presents a robustness check that attempts to account for the possibility that our estimates are biased or misleading due to the potential for outmigration during the war. The sample is divided roughly evenly between those who, just prior to the start of the war, resided in areas bordering other countries and those who resided in interior provinces that did not border any other country. Those residing in border provinces would have had an easier time out-migrating than those residing in interior provinces. As such, to the extent that out-migration biases our analysis, we would expect the results to be driven to a large extent by patterns in these border provinces. We would get closer to the “true” relationship between education and participation by limiting our analysis to those who would not have easily out-migrated. We do this by re-estimating the coefficients on the subject’s own and the subject’s father’s education using just the subsample of subjects who resided in interior provinces. The results are shown in Table F.1. As a basis for comparison, Models 1 and 3 replicate the full-sample results from Models 5 and 6 of Table 2. Then, Models 2 and 4 estimate the same quantities, but on the subsample of individuals whose location at just prior to the war was in an interior province. We see that the estimated coefficients are nearly identical. The sample is cut in half in this robustness check, and so it is no surprise that the statistical

significance level drops. But there is nothing here to suggest that our basic results are biased or misleading from aggregating over places that differ in terms of out-migration opportunities.

## F.2 Logistic regression results

Table F.2: Participation in revolt as a function of own and father's education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Participate in rebellion (0/1)									
Age	-0.058** (0.018)	-0.040* (0.016)	-0.046* (0.018)	-0.038* (0.016)	-0.074*** (0.020)	-0.053** (0.018)	-0.067*** (0.019)	-0.049** (0.018)	-0.054*** (0.015)
Age squared	0.002 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.003 (0.002)	0.002 (0.002)	0.003 (0.002)		
Subject's ed. (yrs.)	0.141*** (0.037)		0.197*** (0.032)		0.165*** (0.033)		0.156*** (0.032)		
Father's ed. (yrs.)		0.095** (0.033)		0.074*** (0.021)		0.081** (0.031)	0.054 (0.031)	0.075* (0.031)	0.071* (0.031)
Logit survival pr.					-0.345*** (0.083)	-0.306*** (0.084)	-0.329*** (0.085)	-0.301*** (0.084)	-0.307*** (0.085)
Age in 1972 > 12								-0.076 (0.481)	
Father's ed. (yrs.) X Age in 1972 > 12								0.199 (0.145)	
Age in 1972 > 16									-0.033 (0.637)
Father's ed. (yrs.) X Age in 1972 > 16									0.588*** (0.165)
Observations	903	903	903	903	903	903	903	903	903

Standard errors in parentheses

Logistic regression coefficients reported.

Models 1-2 and 5-9 are weighted logistic regression with pre-war province fixed effects.

(Weights are the same as those used in the main text.)

Models 3-4 are unweighted conditional logistic regressions grouping by pre-war province.

Standard errors account for commune-level clustering (number of clusters is 65) except for models 3-4, which account for grouping factor clustering.

Models 2 and 4 use age-specific fixed effects.

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

### **F.3 Results without the age restriction on the sample**

The results in the main text worked with a restricted sample that included only Hutu males who were between 12 and 42 years of age in 1993. The lower cutoff was based on the idea that it would only be for males of at 12 years of age that the arguments regarding education would be relevant (younger youth would not have had the chance to attain substantial years of education). The upper cutoff was based on the fact that the oldest rebel in our sample was 42 in 1993, and so the insurgency participation rate for those above 42 years is exactly 0 in our sample. In the tables that follow, we replicate the statistical results from the paper without imposing these age restrictions. In qualitative terms, most of the estimates are the same in the restricted and unrestricted sample. In the unrestricted sample, some of the key estimates are attenuated (e.g., the estimated effect of father's education on participation) and some no longer hold (e.g., the interaction coefficient in the cohort-based analysis), which is to be expected given that the unrestricted sample includes units that provide little relevant variation, and in particular, because of the fact that among those older than 42, the fact that we have a large number of no-participation outcomes induces a non-linearity in the age-based conditional participation probability.



Table F.3: Participation in revolt as a function of own and father's education (full sample, without age restrictions)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
(Constant)	0.074*** (0.022)	0.075*** (0.022)	0.084*** (0.022)	0.064** (0.022)	0.077*** (0.022)	0.088*** (0.022)	0.073*** (0.022)
Age in 1972	-0.001*** (0.000)		-0.000 (0.000)		-0.002*** (0.000)	-0.001*** (0.000)	-0.002*** (0.000)
Age in 1972 sq.	-0.000 (0.000)		-0.000** (0.000)		0.000*** (0.000)	0.000 (0.000)	0.000** (0.000)
Subject's ed. (yrs.)	0.003*** (0.001)	0.003*** (0.001)			0.003*** (0.001)		0.003*** (0.001)
Father's ed. (yrs.)			0.002** (0.001)	0.002** (0.001)		0.002* (0.001)	0.001† (0.001)
Logit survival pr.					-0.007*** (0.002)	-0.007*** (0.001)	-0.007*** (0.001)
<i>N</i>	1460	1460	1460	1460	1460	1460	1460
<i>R</i> <sup>2</sup>	0.021	0.027	0.019	0.027	0.030	0.027	0.031
adj. <i>R</i> <sup>2</sup>	0.006	-0.015	0.004	-0.016	0.015	0.012	0.016
Resid. sd	4.298	4.344	4.303	4.346	4.278	4.285	4.278

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 65.)

Dependent variable is participation in armed revolt (0,1).

All models control for pre-war province fixed effects.

Models 2 and 4 use age-specific fixed effects.

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

Table F.4: Subject's education and occupational ranking as a function of father's education (all-ethnicity sample; full sample, without age restrictions))

	Pre-war ed.	Pre-war occ. ranking
(Constant)	6.048*** (0.582)	0.129 (0.115)
Age in 1972	0.180*** (0.034)	0.043*** (0.006)
Age in 1972 sq.	-0.014*** (0.003)	-0.002*** (0.001)
Father's ed. (yrs.)	0.175*** (0.045)	-0.010 (0.010)
Hutu	-1.087 <sup>†</sup> (0.591)	-0.048 (0.098)
Father's ed.X Hutu	-0.064 (0.058)	0.021 (0.014)
Age X Hutu	-0.082* (0.034)	-0.005 (0.006)
Age sq. X Hutu	0.006 (0.003)	0.000 (0.001)
<i>N</i>	1779	1779
<i>R</i> <sup>2</sup>	0.375	0.413
adj. <i>R</i> <sup>2</sup>	0.366	0.405
Resid. sd	86.126	17.661

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 66.)

D.V. in Model 1 is respondent's own level of education before the war (0-18).

D.V. in Model 2 is respondent's own occupational ranking before the war (-1,0,1).

Sample includes both Hutu and Tutsi male respondents.

All models control for pre-war province fixed effects and survival probability.

<sup>†</sup> significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

Table F.5: Occupational ranking as a function of own and father's education

	Pre-war occ.	Pre-war occ.	Post-war occ.	Post-war occ.
(Constant)	-0.246*	0.015	0.078	0.257**
	(0.096)	(0.099)	(0.089)	(0.094)
Age in 1972	0.032***	0.039***	-0.012**	-0.007 <sup>†</sup>
	(0.003)	(0.003)	(0.004)	(0.004)
Age in 1972 sq.	-0.001**	-0.002***	0.000 <sup>†</sup>	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Subject's Ed. (yrs.)	0.060***		0.042***	
	(0.010)		(0.007)	
Father's ed. (yrs.)		0.010		0.008
		(0.009)		(0.007)
<i>N</i>	1460	1460	1460	1460
<i>R</i> <sup>2</sup>	0.431	0.381	0.155	0.113
adj. <i>R</i> <sup>2</sup>	0.423	0.372	0.142	0.100
Resid. sd	16.188	16.887	14.945	15.311

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 65.)

D.V. is occupational rank (-1,0,1).

All models control for pre-war province fixed effects and survival probability.

<sup>†</sup> significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

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Table F.6: Military ranks as a function of own and father's education (rebels only)

	Incoming rank	Incoming rank	Outgoing rank	Outgoing rank
(Constant)	0.281 (0.347)	1.953*** (0.282)	1.152** (0.431)	6.136*** (0.525)
Age in 1972	-0.014 (0.019)	0.005 (0.020)	-0.062** (0.020)	-0.028 (0.022)
Age in 1972 sq.	0.000 (0.002)	-0.001 (0.002)	0.001 (0.002)	-0.001 (0.002)
Subject's Ed. (yrs.)	0.219*** (0.029)		0.642*** (0.042)	
Father's ed. (yrs.)		0.037 (0.024)		0.195*** (0.033)
<i>N</i>	704	704	706	706
<i>R</i> <sup>2</sup>	0.235	0.165	0.419	0.221
adj. <i>R</i> <sup>2</sup>	0.210	0.138	0.400	0.196
Resid. sd	9.495	9.918	13.578	15.721

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

(Number of clusters is 65.)

D.V. is military rank (1 simple soldier - 15 general).

All models control for pre-war province fixed effects and survival probability.

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

Table F.7: Education as a function of father's education, by age categories in 1972

	Model 1	Model 2	Model 3
(Constant)	4.532*** (0.311)	4.696*** (0.366)	4.754*** (0.290)
Age in 1972	0.111*** (0.020)	0.074* (0.029)	0.081*** (0.022)
Age in 1972 sq.	-0.009*** (0.002)	-0.013*** (0.002)	-0.013*** (0.002)
Father's ed. (yrs.)	0.117** (0.042)	0.081* (0.037)	0.090* (0.037)
Age in 1972 > 12		1.611 (1.033)	
Father's ed. (yrs.) X Age in 1972 > 12		0.821*** (0.147)	
Age in 1972 > 16			2.256* (1.065)
Father's ed. (yrs.) X Age in 1972 > 16			0.641*** (0.117)
<i>N</i>	1460	1460	1460
<i>R</i> <sup>2</sup>	0.301	0.330	0.322
adj. <i>R</i> <sup>2</sup>	0.290	0.319	0.311
Resid. sd	80.601	78.951	79.405

Weighted least squares with standard errors accounting for clustering at the commune level.

Number of clusters is 65.

Dependent variable is respondent's years of education before the war (0-18).

All models control for pre-war province fixed effects and survival probability.

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

Table F.8: Participation in revolt as a function of father's education, by age categories in 1972 (full sample, without age restrictions)

	Model 1	Model 2	Model 3
(Constant)	0.088*** (0.022)	0.087*** (0.022)	0.088*** (0.022)
Age in 1972	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Age in 1972 sq.	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Father's ed. (yrs.)	0.002* (0.001)	0.002* (0.001)	0.002* (0.001)
Age in 1972 > 12		0.016† (0.009)	
Father's ed. (yrs.) X Age in 1972 > 12		-0.001 (0.003)	
Age in 1972 > 16			0.023* (0.010)
Father's ed. (yrs.) X Age in 1972 > 16			0.000 (0.004)
<i>N</i>	1460	1460	1460
<i>R</i> <sup>2</sup>	0.027	0.028	0.028
adj. <i>R</i> <sup>2</sup>	0.012	0.011	0.011
Resid. sd	4.285	4.287	4.287

Weighted least squares estimates.

Standard errors account for clustering at the commune level.

Number of clusters is 65.

Dependent variable is participation in armed revolt (0,1).

All models control for pre-war province fixed effects and survival probability.

† significant at  $p < .10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

‡ jointly significant at  $p < .10$ .

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