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How persistent is the effect of conflict on primary education? Long-run evidence from the Rwandan genocide

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HIGHLIGHTS

- This paper examines the long-term effect of conflict on primary education in Rwanda
- I exploit variation in conflict across areas in a difference-in-differences framework
- Children whose education had been reduced by the 1994 genocide caught up by 2010

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ABSTRACT

I examine the long-term effect of conflict on primary education using data from Rwanda. Previous research has shown that the 1994 genocide reduced primary schooling in the short-term, but the long-term effects are not known. To identify the causal effect of the genocide, I analyze data from four cross-sectional household surveys and use a difference-in-differences identification strategy that exploits variation in conflict intensity across localities. I find that children whose educational attainment had been negatively affected by the genocide eventually caught up. By 2010, there were no detectable effects of the genocide on completed years of education.

1. Introduction

Although primary enrollment in Sub-Saharan Africa increased by 50 percent between 1990 and 2012, 33 million children are currently unenrolled from primary school and the region has not yet achieved universal primary education (MDG Monitor, 2017). Among other factors, the high incidence of armed conflict may have slowed down gains in enrollment (Blattman and Miguel, 2010; Justino, 2011; MDG Monitor, 2017). Several studies have shown that the deleterious effects of conflict on education are long lived, but children's ability to recover varies significantly across gender, socio-economic status and timing of exposure to the conflict (León, 2012; Justino et al., 2013; Verwimp and Van Bavel, 2014; Singh and Shemyakina, 2016).

I examine the long-term consequences of the 1994 Rwandan genocide – a short, intense ethnic conflict that significantly affected the country's education system (Obura, 2003) – on primary education. Akresh and de Walque (2011) show that six years after the end of the mass killings, children who were of primary school age or younger at the time of the genocide completed a half year less education than children who had already completed primary schooling in 1994. Guariso and Verpoorten (2015) replicate Akresh and de Walque's analysis using Census data and estimate an effect twice as large eight years after the genocide. Using detailed data on genocide intensity and four cross-sectional household surveys, I show that the affected children caught up by 2010.

2. Data

Data on schooling are from the Household Recode of the 1992, 2000, 2005 and 2010/2011 Rwandan Demographic and Health Surveys (DHS). I follow children who were of primary school age or younger at the time of the genocide (aged 0 to 14 in 1994) over time and observe them when they are aged 7 to 20 (2000 DHS), 12 to 25 (2005 DHS) and 17 to 30 (2010/2011 DHS). Their educational outcomes are contrasted with those of individuals of similar age in the pre-genocide survey (1992 DHS).

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Measures of genocide intensity are obtained from the records of the gacaca jurisdictions, which were the local, community-based courts charged with the task of trying genocide suspects. These records provide data on the number of suspected genocide perpetrators in each of three categories: organizers of killings, those committing the murders and looters. Additionally, the records provide data on the number of genocide survivors categorized by type of genocide-induced condition: widows, orphans and disabled. These counts are at the sector level.[1] I aggregate them at the commune level using the matching method described in La Mattina (2017) and divide them by the commune population from the 1991 census.[2] Following Verpoorten (2012), I create an index that combines the six normalized variables using the first component of the principal component analysis.[3] I then convert the index to a binary variable that equals one if the index of genocide intensity in the child's commune of residence is above the mean. Using a binary variable instead of a continuous one allows me to utilize the same conflict measure in both the regressions and in the parallel-trends graph. I use the continuous index of genocide intensity as a robustness check. Fig. 1 illustrates variation in the binary index of genocide intensity across communes.

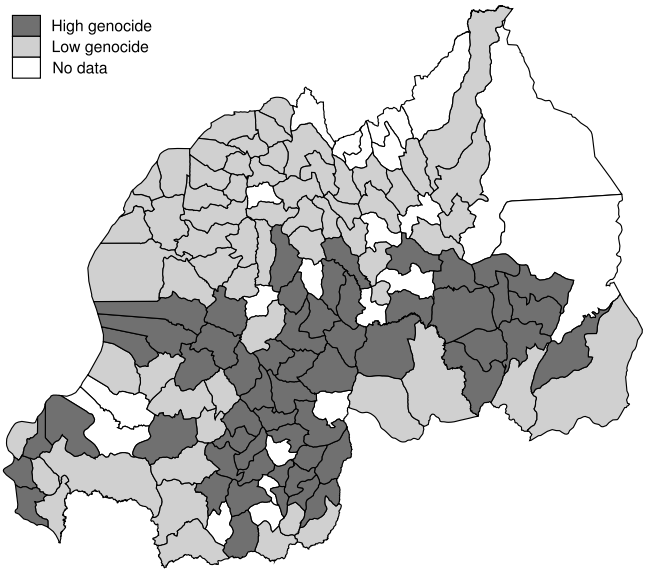

Fig. 1. Geographic variation in genocide intensity. *Notes:* Data from the records of the gacaca courts.

3. Empirical strategy

I estimate the following difference-in-differences specification:

$$Y_{icat} = \alpha + \beta_1 High_c \times Post_t + \beta_2 Post_t + X_i'\lambda + \delta_a + \theta_c + \varepsilon_{icat} \quad (1)$$

Y_{icat} is completed years of education for child i of age a who lives in commune c and is interviewed in survey year t. $High_c$ is a binary indicator that equals one if the child lives in a commune that experienced high genocide intensity and zero otherwise. $Post_t$ is a dummy that equals one if the observation comes from a post-genocide survey and zero if it comes from the pre-genocide survey. X_i includes the child's gender, the gender of the household head, the age of the household head, the maximum year of education completed by any household member, the number of children below age 5 in the household and urban status. Commune fixed effects (θ_c) control for unobservable time-invariant commune-specific factors that may be correlated with both genocide intensity and educational attainment. Age fixed effects (δ_a) capture variation in schooling by age. In some specifications, I add interaction terms between prefecture fixed effects and the post-genocide indicator to control for prefecture-specific factors that may change after the conflict. All regressions are estimated using a linear probability model (OLS) and standard errors are clustered at the commune level (Bertrand et al., 2004).

The difference-in-differences estimator (β_1) is identified under the assumption that, if the genocide had not occurred, trends in educational attainment would be similar in communes that experienced either high or low genocide intensity. I test this assumption indirectly by examining whether age trends in grade completion rates before the genocide varied by future genocide intensity in the commune. Reassuringly, 1991 Census data show that, before the genocide, age trends in grade completion rates for grades 1 to 6 were similar in communes that later experienced high and low genocide intensity (Fig. 2).[4]

Endogenous sample selection caused by selective killings of children may pose a threat to identification. Selection based on ethnicity is a primary concern because Tutsi and, to a lesser extent, politically moderate Hutu were targeted in the killings, and 1991 Census data indicate that Tutsi had more education than Hutu before the genocide. Since information on ethnicity is not available in any data collected after the 1994 genocide, there could be a concern that unobservable changes in the ethnic composition of children may bias my estimates towards finding an effect. However, De Walque and Verwimp (2010) show that excess mortality during the genocide was substantially higher for adults than for children. Since this paper's empirical strategy is based on comparing the outcomes of individuals who were children at the time of the genocide with the outcomes of similarly aged individuals in the pre-genocide survey, selection in adult mortality is likely not a concern. Additionally, I provide two tests to reduce selection concerns. First, I include interactions between the high genocide dummy and variables that were shown to be correlated with being Tutsi before the genocide: the highest level of education by any member of the household and urban status (De Walque and Verwimp, 2010). Second, I restrict the 1992 sample to children who can be linked to information on their mother's ethnicity and exclude children who were born to Tutsi mothers from the pre-genocide survey.[5]

4. Results

Table 1 reports the results. I first estimate the short-term effect of the genocide on education using data from the 1992 and 2000 DHS. This analysis replicates that of Akresh and de Walque using a different empirical strategy.[6] In the short-term, the genocide decreased educational attainment by 0.23 years (column 1). Including interaction terms between prefecture fixed effects and the post-genocide dummy reduces the effect to 0.18 years (column 2). These estimates are consistent with those of Akresh and de Walque (2011), but smaller in magnitude, perhaps because I am using a different identification strategy.

I next turn to the medium and long-term effect of the genocide on schooling using post-genocide data from 2005 and 2010/2011 respectively, and the 1992 survey as baseline. In the medium run,

[1] In 1994, Rwanda was divided in prefectures, which were divided in communes, which were in turn divided in sectors.

[2] In the analysis, I restrict the sample to the 125 communes that were in all four DHS surveys (out of 145).

[3] This index is constructed following the *GI* index defined in Verpoorten (2012).

[4] The information on years of education in the 1991 Census is not complete.

[5] This follows Guariso and Verpoorten (2015).

[6] I cannot exploit variation across cohorts like Akresh and de Walque did because I follow children of the same birth cohort over time.