Late Bloomers? Identifying Critical Periods in Human Capital Accumulation. Evidence from the Rwanda Genocide*

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Abstract

We use the severe and short-lived shock created by the 1994 Rwanda genocide to identify key ages in the accumulation of human capital. Using the post-genocide Demographic and Health Survey for Rwanda and neighboring countries we explore how exposure to the genocide affected women's health as measured by their adult height. We show that the effect of the shock decreases with age as younger girls were affected more severely. However, the effect is not zero for older ages. We find large negative impacts even for those who were between 13 and 18 at the time of the genocide. These results are robust to a large set of possible confounding factors including the possible nonrandom survival rates of the genocide. Our findings suggest that the sensitive periods for this aspect of human capital accumulation go well beyond early childhood.

JEL codes: I10, I15, N4, O15.

Keywords: Rwanda, Nutrition, Health, Civil War, Women.

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1. Introduction

Recent studies in economics have identified the relevance of investing in the human capital of children before they start school. For example, interventions before age seven have important consequences later in life including gains in health and ability (e.g., Currie and Thomas, 1996; Alderman *et al*, 2006; Case and Paxson, 2009; Chay et al, 2009); but also in earnings and later education (e.g., Garcés *et al*, 2002; Bleakley, 2007; Bharadwaj et al, 2010). Furthermore, following the seminal work by Currie and Hyson (1999) there is a growing literature identifying the effects that shocks *in utero* have on outcomes later in life (e.g., Behrman and Rosenzweig, 2004; Currie and Moretti, 2007, Black et al, 2007, Oreopoulus et al, 2008; Royer, 2009 and Field et al, 2009)[1]. However, as discussed by Almond and Currie (2011), one major and still unanswered question is whether certain ages matter more than others.

Our paper addresses this pending issue by considering the effect of the 1994 Rwanda genocide on the health of women. Despite its short duration (approximately 100 days between April and June), the genocide resulted in the deaths of some 800,000 Rwandans. The economy contracted by almost 50 percent in per capita terms that year but recovered soon after gaining 90 percent of the pre-genocide levels in 1995. By 2009, the per capita GDP was 43 percent larger than the 1993 level. Our identification strategy uses a difference-in-difference methodology and compares the adult height of women who had not yet completed their full adult height at the time of the genocide to older cohorts and to women in neighboring countries (Zimbabwe) that did not experience the genocide. As shown below, Zimbabwe is a valid comparison group, as there is a clear

[1] For a recent summary of the literature about the human capital in early life see Almond and Currie (2011).

parallel trend in the adult height of Rwandan and Zimbabwean women in the pre-genocide period[2].

Using this methodology, we find large negative effects of the genocide on adult height. Younger women are 20 percent of a standard deviation (0.2 SD) shorter than older cohorts and are 50 percent more likely to be stunted as adults. Furthermore, we find that the effect varies by the age at the time of shock. The youngest women in our sample experienced a much larger impact. Those aged between five and six at the time of the genocide are 0.5 SD shorter than their older counterparts. However, women who were adolescents in 1994 are also shorter. The adult height of women aged 15 during the genocide is 0.25 SD below the reference group while those aged 18 were 0.17 SD shorter as adults. Thus, the health effects of the Rwandan genocide extend well beyond early childhood. As described below, our results are robust to alternative specifications (e.g., including the use of other control groups, nonrandom selection in the probability of surviving the genocide, other possible shocks at birth at the country and province level) suggesting that our findings are unlikely to be driven by other confounding factors.

Given the nature of the shock, our paper contributes to the also growing literature on the effects of war. Our paper differs from the previous paper in this literature in two important ways. First, much of the existing literature on the effects of conflicts papers has focused on education. For example, León (2009) and Shemyakina (2006) analyze the conflicts in Peru and Tajikistan, respectively, during the 1980s and 1990s on schooling attainment. Closer in spirit to our paper is a paper by Akresh and de Walque (2010), which shows a negative effect of the Rwanda genocide on schooling attainment of

[2] Akresh and de Walque (2010) and Akresh and Verwimp (2011) use within-country variation to study the effect of the 1994 genocide and pre-genocide shocks.

children. However, since the dropout rate for Rwandan children by age 15 was 50 percent even before the genocide, the focus on schooling significantly understates the true full cost of conflict on human capital accumulation. In this paper, we focus on an alternative indicator of human capital accumulation –nutrition– one we are able to observe for the full sample of children and adults. As discussed later, protein and caloric consumption decreased dramatically in the period after the genocide, suggesting possible large negative effects on the nutritional status of children.

Second, unlike Bundervoet *et al.* (2009), who show that civil war in neighboring Burundi adversely impacted the nutritional status of children aged five or less, our analysis is not limited to individuals who were exposed to the Rwanda genocide at an early age. While early childhood is certainly a critical stage in the development of human height, an important growth spurt also occurs during adolescence (*adolescent peak height velocity.*) See for example Case and Paxson (2008). Thus, restricting the sample to only infants and toddlers results in an underestimation of the full effect of war on individual health capital

The rest of the paper is organized as follows. In the next section, we briefly describe the Rwandan genocide with emphasis on the decline of health inputs that followed. In section 3, we discuss the household surveys that we use to estimate the impact of the genocide, while section 4 discusses the empirical strategy of the paper. The empirical results are discussed in section 5, while section 6 carries the conclusions.

2. The Rwanda genocide and health inputs

Between April and June of 1994, an estimated 800,000 persons – accounting for roughly 10 percent of the total population – were killed in a civil war that raged between two rival tribes, the Hutus and the Tutsis, in Rwanda. The United Nations describes the killings in Rwanda as *genocide* (Gourevitch, 1998). The long civil conflict between the Tusti-led rebel group called the Rwanda Patriotic Front (RPF) and the Hutu-led government reached its highest point during those three months of 1994. The massacre ended when the RPF gained control of Kigali, the capital, and overthrew the government in June.

The immediate effects of the civil war on the economy were severe. As shown in Figure 1, GDP per capita contracted by almost 50 percent in 1994. Also, in Figure 2 we show that for a large set of macroeconomic indicators of economic activity there is a clear decline in 1994 followed by a recover soon after. This is observed for capital and private investments (Panel A), value added including agriculture and manufacturing (Panel B) and exports (Panels C and D). Furthermore, Figure 3 shows that very few countries have experienced such a dramatic one-year decline in the post World War II period.

In the next section, we discuss the household surveys that can be used to estimate the long-run effects of the civil war on health outcomes.

3. Data sources

The data we use come from the Demographic and Health Surveys (DHS) of Rwanda and its neighboring country of Zimbabwe but also from Kenya, Mozambique, Tanzania and Zambia. The DHS are standardized nationally representative (cross-sectional) household surveys that have been conducted in over 50 developing countries since 1986. Female respondents aged 15-49 years are interviewed on their birth histories, fertility preferences, use of family planning, and their socio-economic and marital status, among other characteristics.[3] Since the mid-1990s, the DHS also collect anthropometric measures (viz, height and weight) of children below the age of 5 years and their mothers. In Rwanda, the DHS was conducted in 1992, 2001, and 2005; [4] only the last two obtained data on height for the main respondent (viz., a woman of reproductive age). Thus, our analysis is based on the 2001 and 2005 surveys.

To maximize our sample size, we include all women with valid anthropometric data as long as they were between 6 and 40 years of age in 1994, which means that they would have to be born between 1953 and 1988. Therefore, some women might have not reached their full adult height when interviewed in 2001. To mitigate this problem, we use the z-score measure of height, which standardizes individual height for age using data on heights for a reference population as calculated by the World Health Organization.[5]

As we describe later in more detail, our identification strategy relies on a comparison of heights across a "treated" group (i.e., Rwandan women aged 6-40 in 1994) and a "control" group (women aged 6-40 in 1994 in neighboring Zimbabwe), with the comparison being done across small age intervals. To obtain data on heights of the control group, we use two rounds of the Zimbabwean DHS conducted in 1999 and 2005-2006.[6]

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[3] The DHS are available at www.measuredhs.com.

[4] The DHS was also conducted in Rwanda in 2007 and 2010. The 2007 DHS is considered as an interim survey but does not include height measurements while the 2010 DHS has not been made publicly available yet.

[5] The z-score is the number of standard deviations that a person is below (or above) the WHO reference weight for his or her age and sex. A person is typically considered moderately stunted when his or her height is more than two standard deviations below the WHO reference height. Severe stunting is said to occur when height is more than three standard deviations below the WHO reference height.

[6] As further robustness checks, we compare the height of Rwandan women with their counterparts in Kenya (using the 1998 and 2003 DHS), Tanzania (using the 1999 and 2004-2005 DHS), and Mozambique (using the 2000-2001 and 2006 DHS) and Zambia (2000 and 2005 DHS).

In Table 1, columns 3-8 allow us to compare the (observable) characteristics of women in Zimbabwe and Rwanda. It is observed that Zimbabwean women are taller, on average, than their Rwandan counterparts, as measured by the z-score. Rwandan women are one standard deviation below the mean height of the WHO reference group, while their Zimbabwean counterparts are 0.64 of a standard deviation below the reference mean.[7] Women in Zimbabwe are also more likely to be literate and have more schooling (by slightly more than three years). They tend to have fewer children overall but also younger children. As we discuss in the next section, these observed differences between Zimbabwean and Rwandan women do not affect our results as long as they remain constant over time, which they do.

In addition, we control for another important determinant (besides the 1994 genocide) of women's height – GDP per capita (in constant dollars PPP) at the time of birth. GDP data are obtained from the World Bank's World Development Indicators database.[8] In some of our estimations, we also control for rainfall, using provincial-level data on rainfall since 1970.[9] We standardize rainfall by subtracting mean (across all provinces) rainfall from provincial rainfall and then dividing the difference by the standard deviation of rainfall (calculated again over all the provinces).

Table 1 provides descriptive statistics for both the sample with information available on height (columns 3 and 4) and the sample for which information on height is not available (columns 1 and 2). The sample with height information tends to one year older,

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[7] The difference in absolute height between these two populations is 2.6 centimeters or 1.02 inches.
[8] The fact that GDP data are available only from 1960 onwards reduces our sample by about 9 percent.
[9] Richard Akresh and Craig Richardson provided us with rainfall data for Rwanda and Zimbabwe, respectively. Since rainfall data are available only from 1970 onwards, our sample size is further reduced by 23 percent when we control for rainfall.

more likely to be from Zimbabwe, and has more schooling than the sample with no information on height. The two samples do not appear to differ in terms of fertility, as the average number of children ever born, as well as the proportion of women giving birth five and one year(s) prior to the survey, are roughly the same. Thus, the two samples are not qualitatively different, and it is unlikely that our results, which are obviously based on the sample of women for whom data are available on height, are biased.

The final sample we have used includes 27,910 women in Rwanda and Zimbabwe across four DHS rounds (descriptive statistics are shown in columns 3 and 4 of Table 1). The average woman in our sample was 20 years old in 1994. At the time of the surveys, she had completed only 5.8 years of schooling and had an average of 2.5 children. Half of the women in the sample gave birth in the five years prior to the survey date and only 17 percent gave a birth in the twelve months preceding the survey. Our sample is mostly rural (70 percent) and equally divided between Rwanda and Zimbabwe. Average height in our sample of is 158.8 cm. or 5'4"; as noted earlier, Zimbabwean women are taller on average than Rwandan women. In the next section, we discuss in detail the identification strategy we have used to estimate the effect of the genocide on height.

4. Identification strategy

A. Econometric model

We estimate the following equation to quantify the effect of the 1994 genocide on female adult height:

$$y_{ijt} = \alpha + \beta Young_t + \gamma Rwanda_j + \delta(Young_t * Rwanda_j) + \rho X_{ijt} + S_{jt} + e_{ijt} \qquad (1)$$

where y_{ijt} is the height-for-age z-score of woman *i* born in year *t* in country *j* or the probability of woman *i* being stunted (i.e., having a z-score less than two standard deviations below the reference population mean).

The variable $Young_t$ is equal to one if the woman was younger than 21 in 1994 and zero otherwise. We assume that women have reached their full adult height by the age of 21 (Deaton, 2007). [10] $Rwanda_j$ represents the country fixed-effect and takes the value of one for Rwanda and zero for Zimbabwe. The parameter of interest is δ, as it captures the difference-in-difference across cohorts and country of birth ($Young_t$*$Rwanda_j$). In model 1, we assume that no other observable factors are associated with our measures of height after controlling for age, country of birth, and their interactions. That is, we assume that ρ=0 in model 1. In model 2, we relax this assumption and add location (urban versus rural) and survey-year fixed-effects as indicators of vector X_{ijt}.

B. Threats to validity

As discussed earlier, Table 1 shows that women in Rwanda differ along several covariates from their counterparts in Zimbabwe. Therefore, it is possible that our results are biased upwards if the observed differences in height across women in these two countries are not the result of the genocide but other unobserved time-invariant country effects. We can rule out this possibility by including country-fixed effects as captured by the variable $Rwanda_j$.

Our estimation is predicated on the assumption that there exists a parallel trend between the two countries. We validate this assumption in Figure 1, which plots GDP per capita and the growth rate of GDP in the two countries over a period of nearly 30 years,

[10] Our results are robust to the use of alternative cut-off points (not included but available upon request.)

and Figure 4, which plots the standardized height of women from ages 6 to 40 in the two countries in 1994. Figure 1 presents a clear parallel trend in terms of both GDP per capita growth across the two countries. Note, for example, that the growth rate patterns between 1984 and 1993 overlap across Rwanda and Zimbabwe. The parallel trends assumption is also validated in Figure 4 when observing the height-for-age z-score of women aged 21 or older in 1994 – ages at which women are expected to have attained their full adult height. The scalar differences in height across the two countries are captured by the inclusion of a country fixed effect.

Nevertheless, it is possible that other time-variant variables might be correlated with both age at the time of the genocide and height. There is evidence, for instance, that women born during a time of severe weather conditions, such as droughts, tend to be shorter (Alderman *et al*., 2006; Akresh and Verwimp, 2010). Thus, if younger women in Rwanda experienced negative shocks earlier in life relative to their older and Zimbabwean counterparts, the effect attributed to the genocide in equation (1) will be biased upwards.

We deal with this issue in two ways. First, we include in vector X_{ijt} GDP per capita at birth and, in a different specification, rainfall also at the time of birth. The former represents an aggregate economic shock, as the GDP data are only available at the country level, while the rainfall variable represents a more local (provincial) shock. As noted earlier, controlling for GDP per capita and rainfall reduces the sample size because both variables are available for a smaller set of years.

We also consider an alternative strategy that controls for considerable unobserved heterogeneity yet preserves all sample observations. This is represented in equation (1) by including the term S_{jt} that represents country-specific time trends in height.

It is possible that our results could be biased due to the non-random selection into surviving the genocide. In particular, if women of a certain age were more likely to survive we could not be able to separate the stunting effects from the selection effects (Gørgens et al, 2012). Thus, we test whether probability of surviving the genocide depends on the year of birth. We use the 10 percent public micro sample[11] of the pre- and post-genocide censuses of 1991 and 2002 in order to estimate the following regression for women only:

$$\ln(CS_{it})=\psi C2002_t+ \Sigma\theta_i YOB_i + \Sigma\pi_i(YOB_i*C2002_t)+ v_{it} \qquad (2)$$

where $\ln(CS_i)$ is the (log) size of the *i*-th cohort observed in census t={1991, 2002}. A cohort is defined by year of birth (*YOB*) and the variable *C2002* takes the value of one if the observation comes from the 2002 census (zero otherwise). Figure 5 plots the point estimates for vector π_i for the years of birth relevant to our sample together with the 95 percent confidence intervals. As expected, the parameters are negative and statistically different from zero reflecting the decline in the cohort size as a result of the genocide. However, the survival probabilities (or the "missing rate") do not depend on the year of birth. Thus, we can reject the hypothesis that the surviving rates depend on age at the time of the genocide for the sample under study.

Finally, to account for potential correlation of residuals across cohorts, we cluster all our standard errors by a woman's age in 1994.

[11] The data is available for free after registration from IPUMS International (http://international.ipums.org.)

5. Results

A. Main results

Table 2 presents the results of estimating equation (1) using the data described in section three. Column one uses height-for-age z-scores as the measure of height under model 1 without control variables (i.e., ρ=0). This column shows that younger women (aged 21 or less at time of the genocide) in both countries are shorter than their older counterparts by 9.6 percent of a standard error. Also, as shown in Figure 4 and Table 1, women in Rwanda are systematically shorter than those in Zimbabwe by an average of 32 percent of a standard deviation as captured by the parameter associated with the variable $Rwanda_j$. The difference-in-difference parameter δ, capturing the effect of the genocide on the z-score, is negative and statistically different from zero. A Rwandan woman aged 21 or less in 1994 is 20 percent of a standard error shorter than their counterparts who are older and were born in Zimbabwe.

However, the large negative effect of the genocide on younger women is robust to the addition of other control variables. In Table 2, column 2 adding controls --such as whether she lives in urban or rural areas and survey year fixed-effect (model 2)-- changes the parameter only marginally from -0.199 to -0.196. In column 3, we consider the effect of the genocide on a different part of the distribution of height. Exposure to the genocide while young increases the probability of being stunted (z-score below -2 standard deviations) by 7.3 percentage points. Considering that 13.5 percent of women in the sample are stunted, the genocide increased the stunting rate by 54 percent for younger women. Again, this effect is large and it is not sensitive to the inclusion of other controls.

As shown in column 4, the genocide increased the proportion of stunted women by 52 percent (=0.070/0.135).

It is possible that these results are still upward biased due to confounding factors not included in our previous specification. Younger cohorts in Rwanda relative to older cohorts and those in Zimbabwe could be shorter if they lack resources early in life and not because of the genocide per se. This is unlikely to be the driving force behind our results. First, as shown in Figure 1, there is a parallel trend in the performance of the aggregate economy between Zimbabwe and Rwanda. Second, we expand our econometric model by including three important new variables.

In Table 3 we show that our results are robust to the inclusion of variables capturing aggregate or local shocks early in life. In particular, we include GDP per capita in the year of birth for some specifications and rainfall data by province also in the year of birth, for others. Panel A of Table 3 shows the effect of including GDP per capita at birth when the outcomes is the height-for-age z-score. In column 1 we reproduce the results from Table 2 (model 2) to serve as a benchmark. To avoid redundancy and for simplicity, we only include the difference-in-difference parameter, but the full set of results is available upon request. GDP per capita data are available only from 1960 onwards so we need to restrict our sample to women aged 33 or less in 1994. This reduces our sample by nine percent. In column 2 we show that estimating the model without including the GDP at birth information but limiting the sample to those aged 33 or less in 1994 does not affect our estimates, compared to the full sample. In the full sample the parameter is -0.196 (column 1) and it reduces (in absolute value) only marginally to -0.194. Column 3 shows that including the (log of the) GDP per capita at birth changes the estimate to -0.179. While this value is lower (in absolute value) than the one in column 2 it is not statistically different from it. We conclude that our estimates are not driven by aggregate shocks that vary by time and country as captured by GDP per capita.

Our conclusion remains unaltered when we considered more localized shocks. In column 4 we rerun our main specification (model 2) without including rainfall data but limited to the subsample where rainfall data is available. As explained in section three, rainfall data are available from 1970 onwards, so the sample is reduced by 23 percent and it is restricted to women aged 24 or less in 1994. Comparing the estimates from the full sample (column 1) and the restricted sample (column 4) shows that the latter is smaller (in absolute value) but again these parameters are not statistically different from each other. In column 5 we added the rainfall data and the parameter decreases furthermore (in absolute value). Young enough women exposed to the genocide are 16 percent of a standard deviation shorter than their older counterparts and those from Zimbabwe. While this number is smaller than the 19.6 percent reported earlier (column 1), the effect is still large.

In Panel B, we replicate the analysis but now considering the proportion of women who are stunted as our height outcomes. As in the case of the z-score, the inclusion of GDP per capita or rainfall lowers the magnitude of the effect but it is far from eliminating the full effect. Due to the significant loss of data when including these shocks at birth and the fact that the estimates are relatively insensitive to their inclusion, in the rest of the paper we consider model 2 as our preferred specification.

In Table 4 we consider the possibility that country-specific trends could bias our initial results. Unlike the inclusion of GDP or rainfall data, using country-specific trends does not reduce our sample and allow us to control for all possible time-varying unobservables that differ by country. Panel A shows the effect of introducing alternative trends when height is measured by the z-score. Column 1 reproduces our results using model 2, which includes a dummy for urban and survey fixed-effects. As a reminder, the difference-in-difference parameter is estimated to be -0.196. In column 2, we add a linear trend. The effect (in absolute value) is reduced by half. Exposure to the genocide at the age 21 or less is associated with a reduction in height of 9.9 percent of a standard deviation. As discussed before, this is still a large impact. Note however, that when considering a logarithmic trend (column 3) the effect is not statistically different from column 1[12].

Panel B reproduces these alternative estimates when the outcome is measured as the probability of being stunted. Again, the linear trend lowers the initial estimate and the log-trend is not different from the initial estimate. The corresponding effect of the genocide represents an increase in the probability of being stunted by 36 percent in the linear specification. Thus, while the impact associated with the genocide is reduced with the inclusion of country-specific trends, the perverse effect of the genocide on height is far from zero and large.

B. Placebo test

We now consider a placebo test using equation (3) as follows

$$H_{ijt}=\alpha+\beta Age1994_t+\gamma Rwanda_j+\delta(Age1994_t*Rwanda_j)+\rho X_{ijt}+S_{jt}+e_{ijt} \qquad (3)$$

[12] We also considered a quadratic trend but an F-test rejected its validity.

where all variables remain as defined earlier but now $Age1994_t$ represents the woman's age in 1994 (as computed by her year of birth and the date of the survey). Thus, δ represents again the difference-in-difference parameter. However, its interpretation varies. A positive sign is now associated with a perverse effect of the genocide on height as older women in 1994 will be less affected than younger ones. Thus, δ captures the marginal "gain" in height per each additional year of age in 1994. To be consistent with our previous results, we consider model 2 but extended to include linear country-specific trends.

The basic idea for our placebo test rests on the assumption that women who already had reached their full adult height by 1994 should not have had their height being affected by the genocide. For women aged 20 or more in 1994 we should observe a (very) small and statistically insignificant estimate of δ. Thus, we estimate equation (3) but limited to this sample. In column 1, where the dependent variable is the z-score, the difference-in-difference estimated parameter is still negative but it is very small (-0.021) and it is not statistically different from zero.

Column 2 considers the proportion of stunted women as an outcome. The difference-in-difference parameter is not only small and insignificant statistically but it has also the opposite sign. These results together with the fact that aggregate and local shocks at birth and country-specific trends do not eliminate our results reinforce our conclusion that the effects estimated here are unlikely to be driven by other confounding effects.

C. Effects by age in 1994

We now relax our definition of "being young" during the genocide defined by those aged 21 or less in 1994 and explore the effects at different ages. In equation 4 we use age-specific dummies allowing the effect of the genocide to vary by each age (using model 2 and including country-specific linear trends) as follows:

$$H_{ijt} = \alpha + \Sigma_a\beta_a(Age94_t\!=\!a) + \gamma Rwanda_j + \Sigma_a\delta_a(Rwanda_j{*}Age94_t\!=\!a) + \rho X_{ijt} + S_{tj} + e_{ijt} \qquad (3)$$

In Figures 6 (z-score) and 7 (stunting) we present the estimates for δ_a with a={6,24} together with the 95 percent confidence intervals where the omitted category corresponds to those aged 25 or more in 1994. The results confirm our previous estimates. The negative effect on height is larger for younger cohorts relative to their counterparts in Zimbabwe. A woman aged eight in 1994 is 17 percentage points more likely to be stunted than her counterparts aged 25 or above. A woman aged 10 during the genocide is "only" 12 percentage points more likely to be stunted (see Figure 7).

It is important to recall that the sample has been limited to women beyond their most critical growth stage. Following the medical literature, we are assuming that this takes place in the first three years of life and we therefore restricted the sample to those aged six or more in 1994. Hence, our results indicate that, at least in the case of Rwanda, civil conflict or war in general, has a negative effect even in periods where growth is less pronounced.

Furthermore, these figures show that the effects are observed even for women aged 20 in 1994. This is not necessarily surprising. Besides the critical growth stages that takes place between 0-3, there is a second critical stage during adolescence (also known as the adolescent peak height velocity). Furthermore, it has been shown that the timing of this second critical stage tends to arrive earlier as the economic and the nutritional status increases (Case and Paxson, 2008). In Africa, where the levels of income and nutrition are lower it is possible that the adolescent peak height velocity might be delayed. In this case, the height of women as old as 18 or 20 could still be highly sensitive to the availability of food and nutritional intake. Our results seem to confirm this conjecture. From Figure 6, for example, the effect on the z-score for women aged 18 during the genocide is smaller than for those aged 10 or less, but is still an important 25 percent of a standard deviation.

D. Discussion

How did the genocide create a substantial reduction in height as our results show? There is an extensive literature in nutrition and in economics showing that height captures past nutritional intake (see Berhman and Deolalikar, 1998 for a summary). Genetics, income, nutrition and other variables related to the "environment" are frequently associated with height.

In Figure 1, we show that Rwanda's economy contracted by almost 50 percent in per capita term the year of the genocide. It grew in 1995 by over 35 percent but of course, from a very low base. Since then, the economy has experienced slow recovery and the growth rate has been non-negative since 2001. Thus, part of mechanisms explaining the shortness in the younger cohorts could be associated with the lack of resources.

At the aggregate level we can further identify reductions in nutritional intake in Rwanda. The data from FAO (2010) computes energy, protein and fat consumption for most countries around the world for selected groups of years. In particular, FAO estimates the consumption of these nutrients per person per day based on information from food imports and local production. In Figure 8 we plot the consumption in Rwanda relative to Zimbabwe and to the period prior to the genocide (1990-92). The evidence shows an important decline in the consumption of calories and proteins. The former went from 1830 kcal/per person/per day to 1730, which represents a decline of 13 percent relative to Zimbabwe's consumption between 1990 and 1992. Protein consumption decreased by 18 relative to Zimbabwe and before the genocide. The consumption of fat remains the same between 1990-92 and 1995-97 and, like the other measures, shows an increase in the later years, even surpassing the Zimbabwe's levels in 1990-92. This reduction in nutritional intake is consistent with the lower height observed in women young enough to be at a vulnerable stage during the genocide.

6. Conclusions

This paper shows the negative effects of the Rwandan genocide of 1994 on the height of women who survived. Unlike the previous literature we focus on the effects of those aged six and above at time of the genocide. We compare women's height in Rwanda and Zimbabwe collected after the genocide and identify the effects based on the variation created by country of residency and age.

Our results show that large negative shocks, such as the Rwandan genocide, have effects that go beyond the first critical growth stage. At least for the case of women in Rwanda, our paper suggests that the vulnerability period extends into late adolescence. The reported effects are large and robust to other possible explanations, including the presence of shocks early in life at the aggregate and the local level as well as country-specific trends in unobserved characteristics.

This has clear effects on the design of policies that attempt to provide a safety net in terms of nutrition. The current literature is very robust regarding the long-term consequences of negative shocks early in life, but as Almond and Currie (2011) suggest, it is critical to identify the life stages where interventions are the most cost-effective.

While our paper shows that the effects are larger for younger women exposed to the genocide, the magnitudes are still quite large even for those aged 18 or 20 at the time of exposure. Therefore, if reaching women when they are adolescents is less costly compared to when they are much younger, it is not obvious whether policies should have a clear bias in favor of younger cohorts. Further research on the cost effectiveness of policies targeting different age groups is needed.

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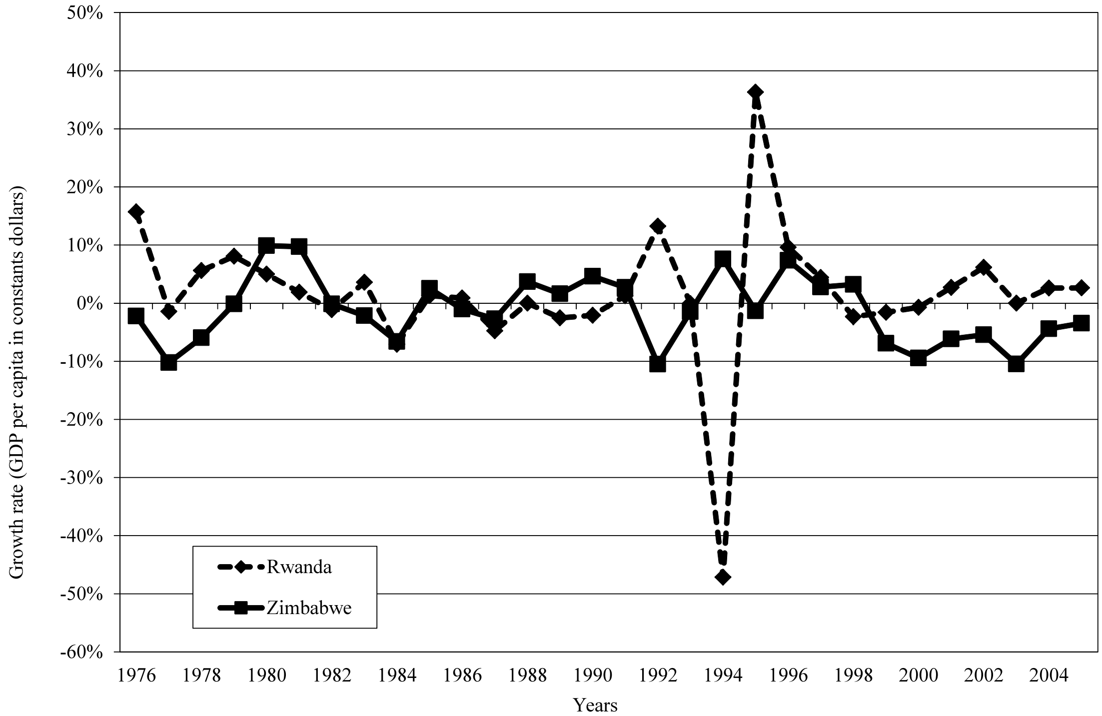

Figure 1: Annual per capita GDP growth rate in Rwanda and Zimbabwe
Note: Figures are expressed in constant dollars of 2000. Date source: World Bank's World Development Indicators.

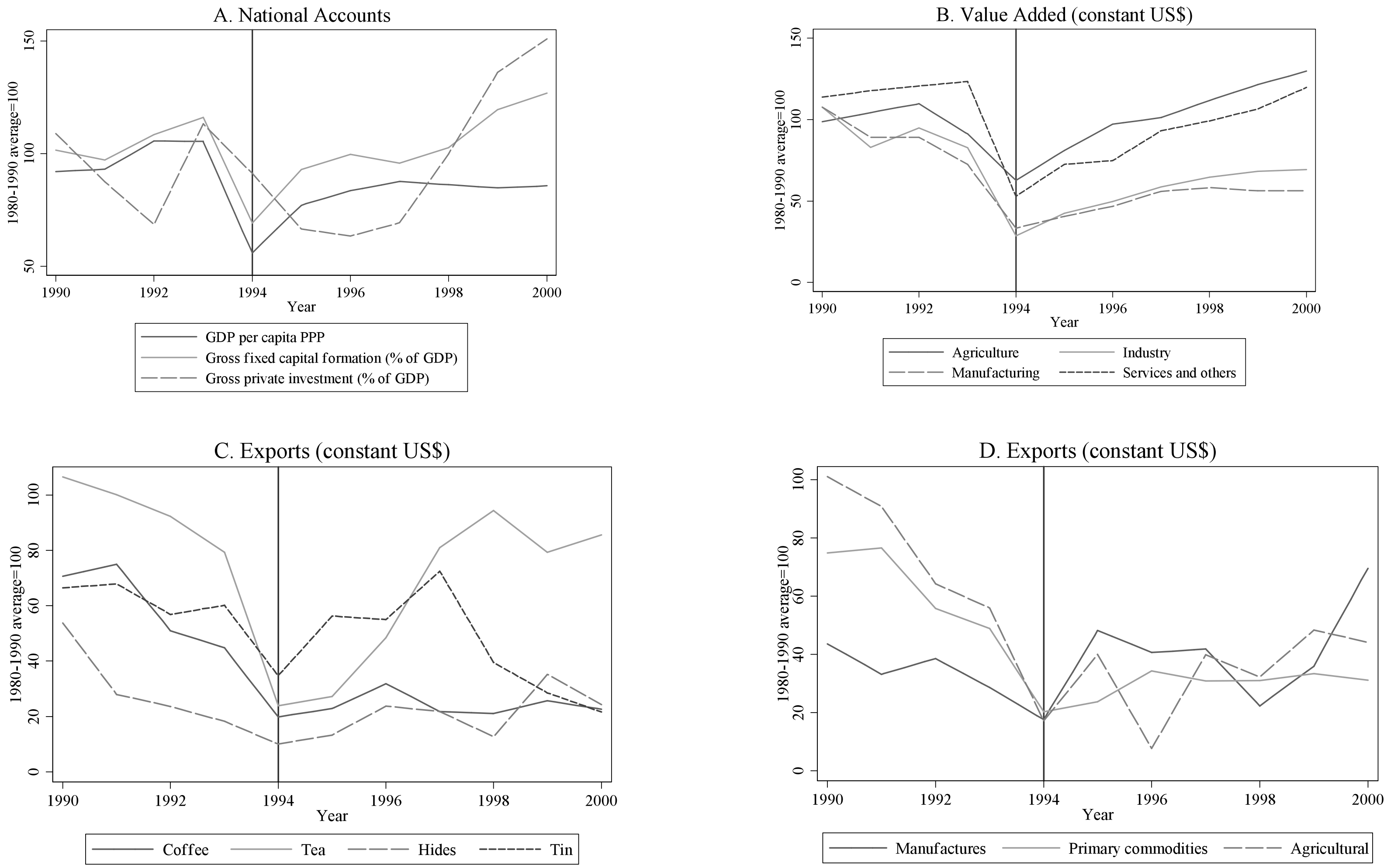

Figure 2. Macroeconomic indicators in Rwanda

Note: To facilitate comparison across variables the 1980-1990 average was set to 100. Source World Bank's African Development Indicators.

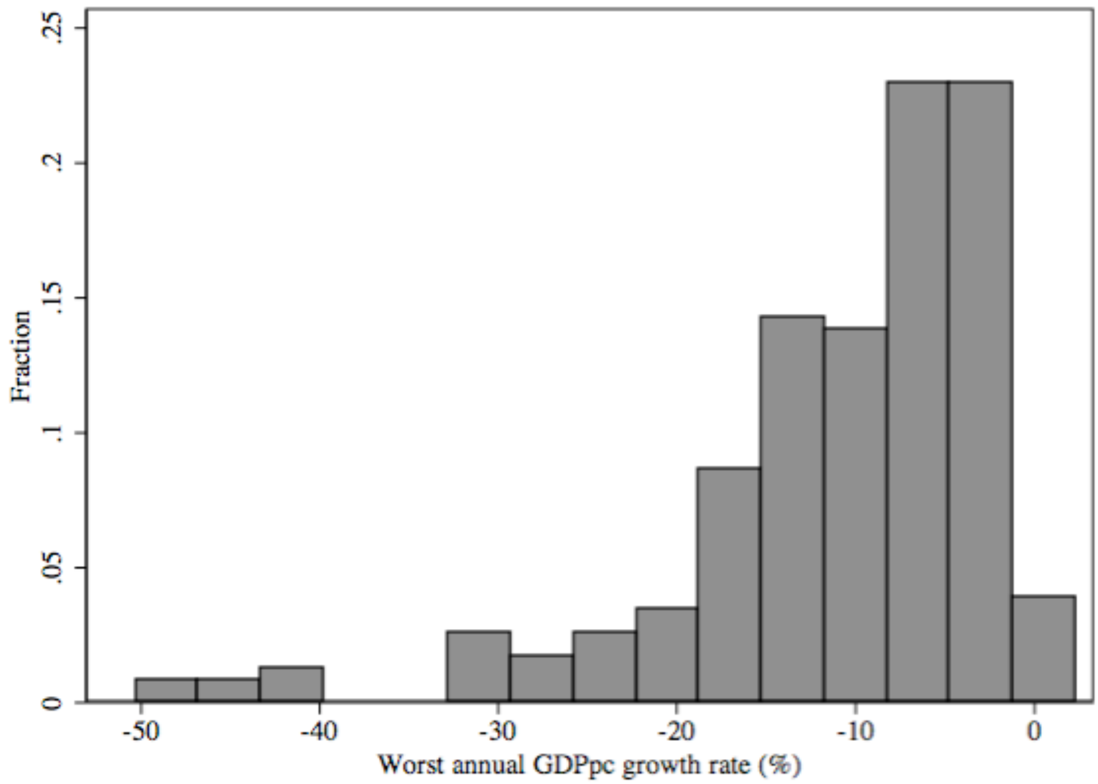

Figure 3. Distribution of the worst annual growth rate.
Note: Figures are expressed annual growth rate of per capita GDP in constant dollars of 2000. Date source: World Bank's World Development Indicators.

Figure 4. Women's height (z-score) in Rwanda and Zimbabwe by age in 1994.
Note: Each circle/rhombus represents the average height for age z-score by country and age in 1994. Data sources: 2000 and 2005 DHS for Rwanda and 1999 and 2005-2006 for Zimbabwe.

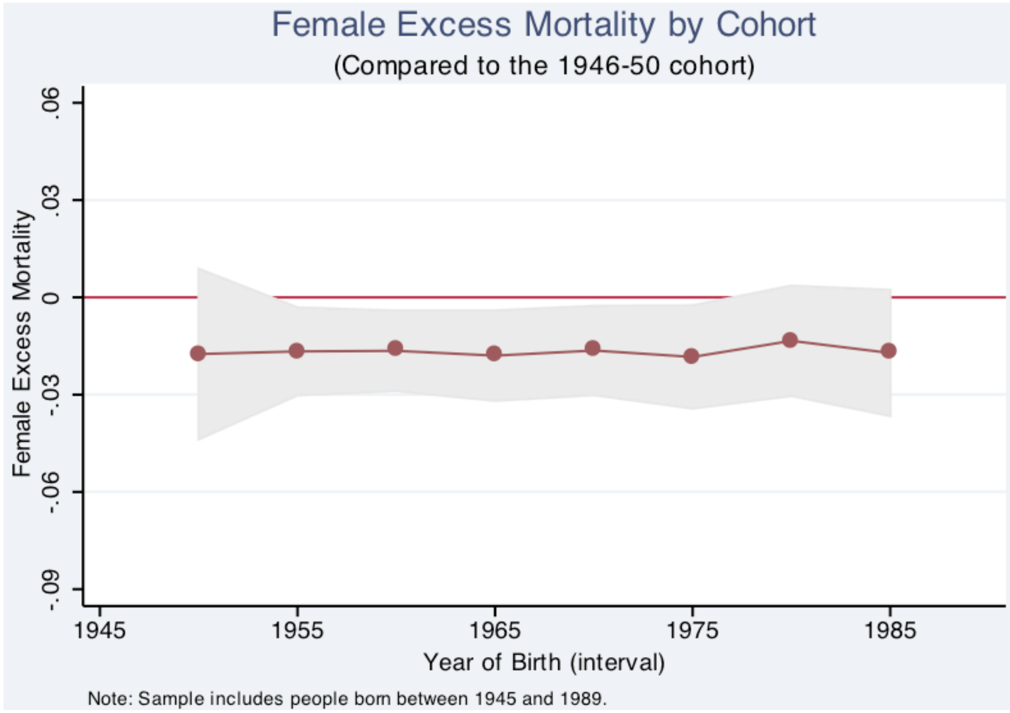

Figure 5. Female excess mortality by year of birth

Note: The regression includes census-year and year of birth fixed effects. Robust standard errors clustered by year of birth are shown as the shaded areas. Data source: 10 percent micro sample of the 1991 and 2002 Rwanda censuses.

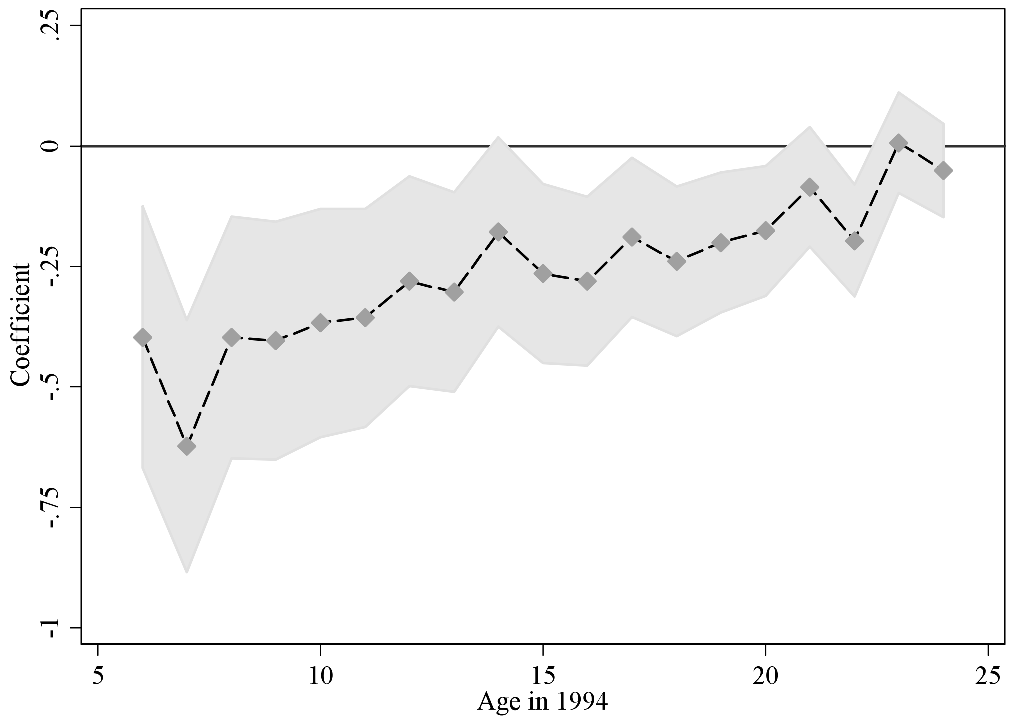

Figure 6. Effect of the genocide on height z-score by age in 1994.
Note: The regression includes age specific dummies where the omitted category is women aged 25 or more in 1994; country fixed-effects, linear country trends, a dummy for Rwanda, a dummy for living in urban area and survey fixed effects. The 95 percent robust confidence intervals clustered by the age in 1994 are shown as shaded areas. Data sources: 1991 and 2005/06 Zimbabwe DHS and 2000 and 2005 Rwanda DHS.

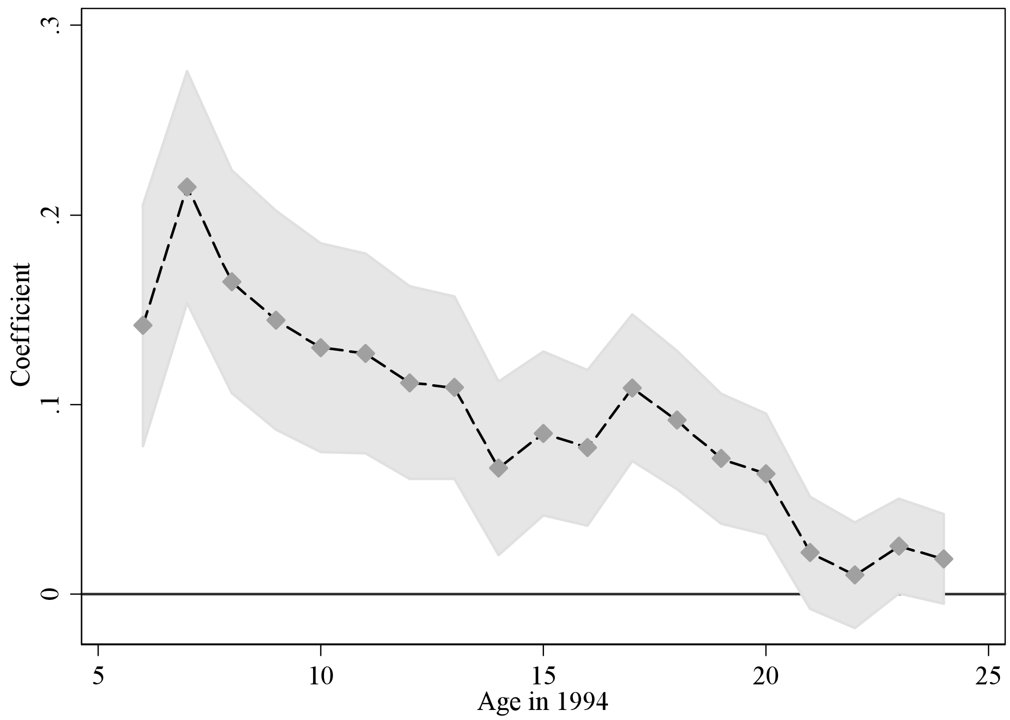

Figure 7. Effect of the genocide on the probability of stunting by age in 1994.
Note: The regression includes age specific dummies where the omitted category is women aged 25 or more in 1994, country fixed-effects, linear country trends, a dummy for Rwanda, a dummy for living in urban area and survey fixed effects. Stunted is defined as having a height-for-age z-score below -2SD. The 95 percent robust confidence intervals clustered by the age in 1994 are shown as shaded areas. Data sources: 1991 and 2005/06 Zimbabwe DHS and 2000 and 2005 Rwanda DHS.

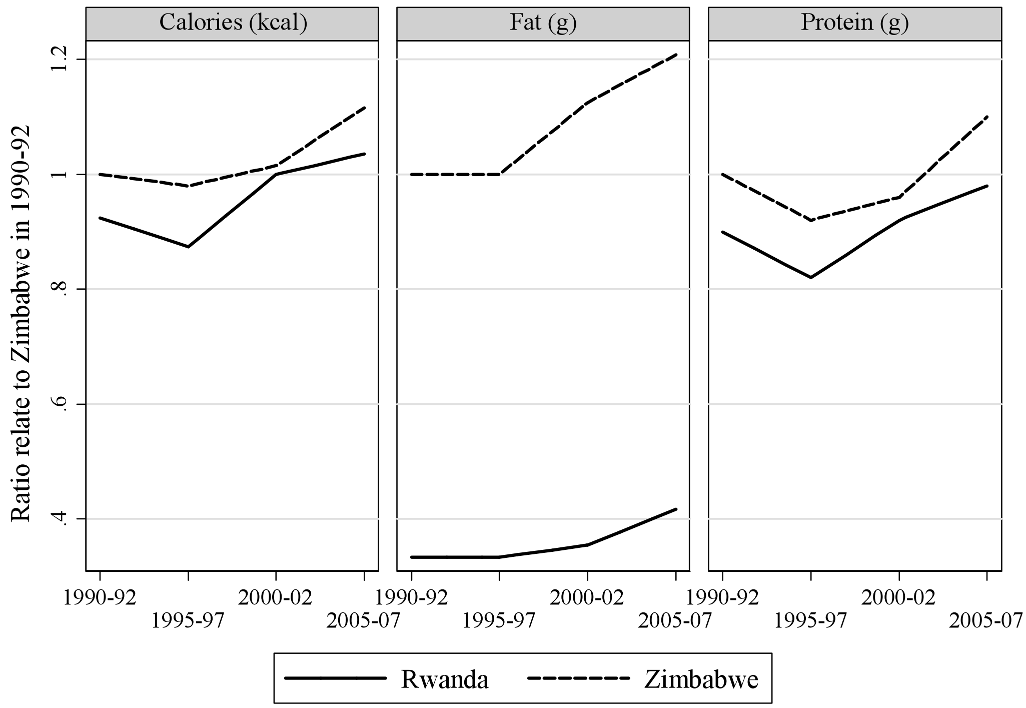

Figure 8. Energy, protein and fat consumption in Rwanda and Zimbabwe
Note: Consumption is measured in kcal or grams per person per day relative to Zimbabwean levels in 1990-92. Data Source: FAO (2010).

Table 1. Summary statistics

	Has height information:							
	No		Yes					
			All		Zimbabwe		Rwanda	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Height-for-age z-score	--	--	-0.863	1.084	-0.636	1.034	-1.065	1.089
Stunted (=1)	--	--	0.135	0.342	0.085	0.279	0.180	0.384
Age in1994	18.9	9.2	19.9	9.1	19.4	9.0	20.3	9.2
Age at the time of survey	29.3	9.3	28.0	9.1	28.1	8.9	28.0	9.2
Rwanda (=1)	0.924	0.265	0.529	0.499	0.000	0.000	1.000	0.000
Urban (=1)	0.252	0.434	0.293	0.455	0.341	0.474	0.251	0.434
Years of education	4.181	3.521	5.811	3.779	7.823	3.120	4.020	3.393
Prop. of illiterate	0.292	0.455	0.228	0.419	0.087	0.282	0.302	0.459
Number of children	2.828	2.894	2.501	2.605	2.35	2.297	2.635	2.845
Child in last 5 years (=1)	0.511	0.500	0.504	0.500	0.504	0.500	0.505	0.500
Child in the last year (=1)	0.183	0.387	0.169	0.374	0.143	0.350	0.191	0.393
Observations	5,896		27,910		13,147		14,763	

Note: SD refers to the standard deviation. Stunted is defined as having a height-for-age z-score below -2SD. Data sources: 1991 and 2005/06 Zimbabwe DHS and 2000 and 2005 Rwanda DHS.

Table 2. Effect of the genocide on different health measures

Dependent variable:	Height-for-age z-score (mean: -0.863)		Proportion stunted (mean: 0.135)	
	(1)	(2)	(3)	(4)
Young	-0.096***	-0.122***	-0.002	0.004
	(0.019)	(0.021)	(0.004)	(0.005)
Rwanda	-0.322***	-0.345***	0.055***	0.072***
	(0.017)	(0.031)	(0.005)	(0.008)
Young*Rwanda	-0.199***	-0.196***	0.073***	0.070***
	(0.031)	(0.030)	(0.009)	(0.009)
Constant	-0.579***	-0.590***	0.086***	0.088**
	(0.012)	(0.027)	(0.003)	(0.008)
Controls	N	Y	N	Y
Observations	27,910	27,910	27,910	27,910
R-squared	0.05	0.06	0.02	0.03

Note: Robust clustered standard errors by the age in 1994 are shown in parentheses. Significance at 10% is shown by *, 5% by ** and 1% by ***. Young takes the value of 1 if age is less than 21 in 1994. Stunted is defined as having a height-for-age z-score below -2SD. Controls include survey fixed effects, country trends and a dummy for living in urban areas. Data sources: 1991 and 2005/06 Zimbabwe DHS and 2000 and 2005 Rwanda DHS.

Table 3. Effects of the genocide controlling for shocks at birth

	Full sample	Sample with GDP data available		Sample with rainfall data available	
	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent variable: Height-for-age z-score					
Mean	-0.863	-0.871	-0.871	-0.859	-0.859
Young*Rwanda	-0.196***	-0.194***	-0.179***	-0.171***	-0.159***
	(0.030)	(0.032)	(0.033)	(0.034)	(0.035)
GDP per capita [a/]	N	N	Y	N	N
Rainfall [b/]	N	N	N	N	Y
Observations	27,910	25,392	25,544	21,443	21,443
R-squared	0.06	0.07	0.07	0.07	0.07
Panel B. Dependent variable: Proportion stunted					
Mean	0.135	0.137	0.137	0.135	0.135
Young*Rwanda	0.070***	0.074***	0.069***	0.072***	0.072***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)
GDP per capita [a/]	N	N	Y	N	N
Rainfall [b/]	N	N	N	N	Y
Observations	27,910	25,392	25,544	21,443	21,443
R-squared	0.03	0.03	0.07	0.04	0.04

Note: Robust clustered standard errors by the age in 1994 are shown in parentheses. Significance at 10% is shown by *, 5% by ** and 1% by ***. Young takes the value of 1 if age is less than 21 in 1994. Stunted is defined as having a height-for-age z-score below -2SD. All regressions include a dummy for being 21 or younger in 1994, country fixed-effects, a dummy for living in urban area and survey fixed effects. Data sources: 1991 and 2005/06 Zimbabwe DHS and 2000 and 2005 Rwanda DHS.

[a/] GDP per capita at the country level was obtained from the World Bank's World Development Indicators.

[b/] Province-level Rainfall data for Rwanda and Zimbabwe was generously provided by Richard Akresh and Craig Richardson, respectively. The data has been standardized by subtracting the provincial average for the entire sample and divide it by the standard deviation.

Table 4: Effects of the genocide including country-specific trends

	Country specific trends		
	No trends	Linear	Logarithmic
	(1)	(2)	(3)
Panel A. Dependent variable: Height-for-age z-score (mean: -0.863)			
Young*Rwanda	-0.196***	-0.099*	-0.182***
	(0.030)	(0.050)	(0.044)
Observations	27,910	27,910	27,910
R-squared	0.06	0.06	0.06
Panel B. Dependent variable: Proportion stunted (mean: 0.135)			
Young*Rwanda	0.070***	0.049***	0.074***
	(0.009)	(0.014)	(0.010)
Observations	27,910	27,910	27,910
R-squared	0.03	0.03	0.03

Note: Robust clustered standard errors by the age in 1994 are shown in parentheses. Significance at 10% is shown by *, 5% by ** and 1% by ***. Young takes the value of 1 if age is less than 21 in 1994. Stunted is defined as having a height-for-age z-score below -2SD. All regressions include a dummy for being 21 or younger in 1994, country fixed-effects, a dummy for living in urban area and survey fixed effects. Data sources: 1991 and 2005/06 Zimbabwe DHS and 2000 and 2005 Rwanda DHS.

Table 5. Placebo test: Effect for women aged 20 or more in 1994

Dependent variable:	Height-for-age z-score (mean: -0.760)	Proportion stunted (mean: 0.118)
	(1)	(2)
Age in 1994	0.021	-0.001
	(0.039)	(0.010)
Rwanda	0.591	0.267
	(1.900)	(0.555)
Age in 1994*Rwanda	-0.021	-0.004
	(0.045)	(0.013)
Constant	-1.491	0.118
	(1.590)	(0.438)
Observations	13,455	13,455
R-squared	0.04	0.01

Note: Robust clustered standard errors by age in 1994 are shown in parentheses. Significance at 10% is shown by *, 5% by ** and 1% by ***. Stunted is defined as having a height-for-age z-score below -2SD. All regressions include a dummy for living in urban area, linear country trends and survey fixed effects. Data sources: 1991 and 2005/06 Zimbabwe DHS and 2000 and 2005 Rwanda DHS.

Table 6. Effect of the genocide using different control groups

	Control group is country:					
	Zimbabwe (1)	Mozambique (2)	Zambia (3)	Kenya (4)	Tanzania (5)	All (6)
Panel A. Dependent variable: Height-for-age z-score.						
Mean:	-0.863	-1.183	-1.019	-0.892	-1.132	-0.984
Young	-0.122***	-0.129**	-0.202**	-0.041	-0.074*	-0.110**
	(0.021)	(0.023)	(0.032)	(0.025)	(0.034)	(0.018)
Rwanda	-0.345***	0.342**	-0.019	-0.310**	0.270**	-0.152**
	(0.031)	(0.031)	(0.028)	(0.034)	(0.039)	(0.030)
Young*Rwanda	-0.196***	-0.188**	-0.113**	-0.265**	-0.233**	-0.195**
	(0.030)	(0.036)	(0.029)	(0.026)	(0.026)	(0.024)
Panel B. Dependent variable: Proportion stunted.						
Mean:	0.135	0.212	0.168	0.145	0.200	0.166
Young	0.004	0.036**	0.034**	0.003	0.017	0.019**
	(0.005)	(0.010)	(0.008)	(0.006)	(0.011)	(0.005)
Rwanda	0.072***	-0.078**	-0.008	0.063**	-0.072**	0.030**
	(0.008)	(0.009)	(0.008)	(0.008)	(0.013)	(0.008)
Young*Rwanda	0.070***	0.040**	0.041**	0.069**	0.055**	0.054**
	(0.009)	(0.013)	(0.009)	(0.008)	(0.009)	(0.008)
Controls	Y	Y	Y	Y	Y	Y
Observations	27910	29746	27903	26040	24168	76536

Note: Robust clustered standard errors by the age in 1994 are shown in parentheses. Significance at 10% is shown by *, 5% by ** and 1% by ***. Young takes the value of 1 if age is less than 21 in 1994. Stunted is defined as having a height-for-age z-score below -2SD. Controls include survey fixed effects and a dummy for living in urban areas.