Crises and the Health of Children and Adolescents: Evidence from the Rwanda Genocide[*]

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PRELIMINARY. COMMENTS WELCOME

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Abstract

We study the effect of crises on health by focusing on the height of adult women exposed to the 1994 genocide in Rwanda when they were children or adolescents. Using several large household surveys, we find that the adult height of girls exposed to the genocide is much lower than older cohorts and those from neighboring countries. Furthermore, we find a large negative effect on height even for those who were between 13 and 20 in 1994. Our findings suggest that the negative effect of crises on health goes well beyond early childhood. These results are robust to other possible confounding variables.

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1. Introduction

Wars kill people, displace populations and reduce the economy's physical capital and infrastructure. However, the long-run impact of wars remains an open question. Collier et al (2003) suggest that civil wars create an analog to a poverty trap --*conflict trap*-- by reducing the country's living standards and capital stocks below a threshold. This then limits the county's ability to recover. Alternatively, following a Solow-type neoclassical growth model, the lower levels of capital caused by wars would imply higher rates of returns and a faster economic growth (Miguel and Roland, forthcoming).

The empirical evidence is also divided. The long-run effects of World War II on West Germany (Brakman, Garrtesen and Schramm, 2004), the US bombing of Japan (Davis and Weinstein, 2002) and the Vietnam War (Miguel and Roland, forthcoming) show no relation between the intensity of the conflict and measures of economic development. However, these results could be biased downwards. Reduced-form effects of war on economic development in the *long run* are likely to be attenuated by possible responsive investments. For example, the cost of the Marshall Plan that followed WWII was estimated to be around US$12.7 billion (representing five percent of the US GDP of 1948) in addition to the 12 billion between the end of the war and before the Marshall Plan (Schain, 2001). If the potential negative effect of war was offset by the large responsive investments from the Marshall Plan, then the long-run estimates will be biased and will tend to show no effects of wars.

Our paper contributes to the literature by focusing on the effects of a recently ended civil war. In particular, we study the effect of the 1994 Rwanda genocide on human capital accumulation. Despite its short duration, 800,000 Rwandans were killed during the approximately 100 days between April and June of 1994. The economy contracted by 50 percent in per capita terms in 1994, but as shown in Figure 1, it has not recovered its pre-genocide levels even after a decade. This slow recovery allows us to avoid the possible downward bias generated by the studying a war that ended long ago.[1]

We also focus on the impact of civil wars using micro-level household data as opposed to aggregate data. While there are other papers using recently-ended wars with household surveys, ours differs in several ways. First, most papers have focused on the effect related to education. For example, León (2009) and Shemyakina (2006) study, respectively, the conflicts in Peru and Tajikistan during the 1980s and 1990s on school attainment. Closer to our paper is the work by Akresh and de Walque (2010). They show a negative effect from the Rwanda genocide on school attainment of young children. However, 50 percent of Rwandans dropped out from school before they turned 15 even before the genocide. Thus, the focus on schooling for the case of Rwanda could be seen as a limited outcome of human capital accumulation. We differ from these papers by focusing on an alternative aspect of human capital accumulation: nutrition. In particular, we concentrate on malnutrition and stunting. As discussed later, protein and caloric consumption decreased dramatically in the period after the genocide, suggesting possible large negative effects on nutrition.

Second, our paper does not limit the analysis to those exposed to the Rwanda genocide at an early age. Bundervoet et al (2009) shows that the civil war in neighboring Burundi negatively impacted the nutrition of very young children (aged five or less). However, as described by Case and Paxson (2008), among others, early childhood is one

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[1] Almond and Currie (forthcoming) have made a similar argument in the case of shocks affecting the human capital accumulation before age five.

of two critical stages in the development of human height. Biologically, humans have an important growth spurt during adolescence known as the *adolescent peak height velocity*. Thus, estimating the effect of severe negative shocks such as civil war only on the health of young children could underestimate the effect of war on human capital related to health.

Third, using large nationally representative datasets conducted after the genocide, our identification compares women who have not completed their full adult height in 1994 (proxy by age) against older cohorts and women in neighboring countries that did not experience the genocide.[2] We argue that Zimbabwe serves as a valid comparison group. There is a clear parallel trend between these two countries in terms of adult height for women who completed the full adult height by the time of the genocide. We show that the effects on nutrition are large and negative. Similar to the current literature, the severity of the effect is larger for younger girls. However, we find large effects even for adolescents and women aged 13 to 20 during the genocide. Thus, the effect of aggregate negative shocks on health such as the Rwanda genocide extends well beyond early childhood. Furthermore, our results are robust to alternative specifications, suggesting that our findings are unlikely to be driven by other confounding factors.

The rest of the paper is organized as follows. In the next section we briefly describe the Rwanda genocide with emphasis on the decline of health inputs that followed. In section three we present the household level surveys that allow us to estimate the impact of the genocide using the empirical strategy explained in section four. The results are shown in section five. Section six concludes the paper.

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

2. The Rwanda genocide and health inputs

Between April and June of 1994, an estimated 800,000 Rwandans or the equivalent of 10 percent of the population were killed. The United Nations describe the killings in Rwanda as *genocide*; this was the first time this word was used since the Holocaust (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 on the economy were severe. As shown in Figure 1B, GDP per capita contracted by almost 50 percent in 1994. In the next section we discuss the household surveys used to answer this question.

3. Data sources

The data sources for height are the Demographic and Health Surveys (DHS) of Rwanda and its neighboring countries. The DHS are standardized nationally representative (cross-sectional) household surveys in developing countries. Women answer a long questionnaire about their birth history, fertility preferences, family planning, their socio-economic and marital status, among other characteristics[3]. Since the mid-1990s the DHS also collects anthropometric measures (e.g., height and weight) of young children and

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[3] After registration, the DHS are available for free from www.measuredhs.com.

their mothers. In Rwanda, only the post-genocide surveys include height for the main respondent, thus, our analysis uses only the 2001 and 2005 surveys.[4]

To maximize our sample size we include all women with valid anthropometric data as long as they were between 6 and 40 in 1994. This means that they were born between 1953 and 1988. Therefore, some women might have not reached their full adult height when measured in 2005, especially younger women interviewed in 2001. Thus, it is not possible to compare the height of women when there is uncertainty about whether they have already reached their full adult height. To avoid this problem we use z-score measures of height-for-age included in the survey. The z-score compares the height, by the age of the person, against the average height of all women of her age from a reference population as calculated by the World Health Organization. This is further divided by the standard deviation (SD) of the height of the same reference group. The final measure is the z-score where a negative (positive) value indicates that a person is below (above) the average height in terms of a standard deviation. A z-score below -2SD (sometimes -3SD) indicates stunting.

As we will describe later in more detail, our identification strategy compares women by their age during the genocide in Rwanda against their counterparts in "neighboring" countries where there was no civil war in 1994. Our main control group will be Zimbabwe and we will use the Zimbabwean DHS from 1999 and 2005-2006[5].

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[4] The first DHS for Rwanda took place in 1992 followed by two more in 2001 and 2005. There is a fourth survey, called interim survey, for 2007 and a fifth one took place in 2010. The results of our analysis do not vary if the 2007 DHS is included in the analysis. The 2010 DHS has not been made publicly available yet.

[5] As further robustness checks, not included in this version of the paper, we compare the height of Rwandan women with their counterparts in Kenya (using the 1998 and 2003 DHS), Tanzania 1999 and 2004-2005 DHS) and Uganda (2000-2001 and 2006 DHS).

In Table 1, columns 3 to 8 allow us to compare the (observable) characteristics of women in Zimbabwe and Rwanda. First, Zimbabwean women are taller than their Rwandan counterparts as measured by the z-scores. Rwandan women are one standard deviation below the mean of the reference group while their Zimbabwean counterparts are only 63.6 percent of standard deviation below the mean[6]. Women in Zimbabwe are also more educated (by more than three years) and more literate. They tend to have fewer children overall but also more recent children. As we will discuss later in our presentation of the identification strategy (next section) these observed differences will not affect our results as long as they remain constant over time, as they do.

Furthermore, we will also control for other possible "events" that could affect women's height beyond the 1994 genocide. In particular, we include GDP per capita (in constant dollars PPP) at the time of birth and in each country using data from the World Bank's World Development Indicators. GDP data are available starting in 1960. This reduces our sample by nine percent. Also, when Zimbabwe is used as a "control" group we are able to include rainfall data at the province level for women born in 1970 or later for Rwanda and Zimbabwe[7]. Unfortunately, this reduces our sample by 23 percent. The rainfall data has been standardized by subtracting the provincial average for the entire sample and dividing them by the provincial standard deviation.

Table 1 also allows us to compare the sample with height information (columns 3 and 4) against those without it (columns 1 and 2). The sample with height information tends to one year older, more likely to be from Zimbabwe and has more education than the

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[6] The difference between these two populations translates into almost 2.6 centimeters or 1.02 inches.

[7] We deeply appreciate the generosity of Richard Akresh and Craig Richardson who, respectively, shared their rainfall data for Rwanda and Zimbabwe.

height-less sample. They do not differ in their fertility decisions, as the average number of children ever born is roughly the same as well as the proportion of women giving birth five and one year prior to the survey. This evidence reduces the possibility to infer results for the entire population. In the rest of the paper when we discussing the effects on Rwandan women it should be clear that we are referring to the sample under analysis.

The final sample used in the main regressions includes 27,910 women in Rwanda and Zimbabwe in four DHS as shown in Table 1 (columns 3 and 4). The average woman in our sample was 20 years old in 1994. At the time of the surveys, she completed only 5.8 years of schooling and had an average of 2.5 children. Half of the women included gave birth in the five years prior to the survey date and only 17 percent in the twelve months prior to the interviews. Our sample is mostly rural (70 percent) and equally divided between Rwanda and Zimbabwe. The average height of these women is 158.8 cm. or 5.20 feet tall (not shown), however Zimbabwean women are taller as we discussed earlier. In the next section we discuss in detail the identification strategy to estimate the effect of the genocide on height.

4. Identification strategy

A. Econometric model

We use the following equation to estimate the effect of the genocide on height:

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

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

The variable $Young_t$ is equal to one if the woman was younger than 21 in 1994 and zero otherwise. Following Deaton (2007), women are expected to reach their full adult height by the age of 25 or before. Using 21 as our cut-off point we are comparing women that reached their full adult height (22 or more in 1994) against those who did not (21 or less in 1991). [8]

Similarly, $Rwanda_j$ represents the country fixed-effects 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 will assume that no other observable factors are associated with our measures of height after we control for age, country of birth and their interactions. That is, we are assuming $\rho=0$ in model 1. In model 2, we relax this assumption and add location (urban versus rural) and survey-year fixed-effects as variables in 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 between 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 $Rwanda_j$.

This assumption implies that there exists a parallel trend between these countries. We validate this assumption in Figure 1 and 2. The former presents a clear parallel trend in terms of GDP per capita between these two countries. Note for example, that the growth

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

rates patterns between 1984 and 1993 overlap. These parallel trends assumption is also validated when observing the height-for-age z-score of women aged 25 or more during the genocide. As discussed before, women are expected to have reached the full adult height by the age of 25 or before. In Figure 2 we show a clear parallel trend for the 25+ subgroup between these countries. Thus, these differences are captured when we include country fixed effects as described above in equation (1).

Nonetheless, it is possible that other time-variant variables might correlate with age at the exposure of the genocide and height. For example, women born during a time of severe weather conditions, such as droughts, tend to be shorter --see Alderman et al (2006) for the case of Zimbabwe and Akresh and Verwimp (2010) for Rwanda pre-genocide. Thus, if younger women in Rwanda experienced negative shocks earlier in life relative to their older and Zimbabwean counterparts, the attributed effect to the genocide in equation (1) will be biased upwards.

We deal with this issue in two ways. First, we will include in vector X_{ijt} information about GDP per capita at birth and, in a different specification, rainfall data also at birth. The former is of course representing an aggregate shock as the GDP data are only available at the country level. The rainfall data, however, represents a more local shock as they are available at the province level. Unfortunately, this strategy reduces the sample size because both variables are not available for all the years as explained in section three above.

Second, we consider an alternative strategy that preserves all observations as shown in equation (2)

$$H_{ijt}=\alpha+\beta Young_t+\gamma Rwanda_j+\delta(Young_t*Rwanda_j)+\rho X_{ijt}+S_{jt}+e_{ijt} \qquad (2)$$

where the additional term S_{jt} is adding country-specific time trends. Thus, we are allowing each country to have different trends in term of height. These two strategies try to account for possible observed and unobserved trends in height for each country.

To account for possible correlated errors by cohort, all our standard errors are clustered by women's age in 1994. The results of applying all the different econometric models using the DHS are shown in the next section.

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., $\rho=0$). 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 2 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[9].

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

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

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

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

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 3 (z-score) and 4 (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 4).

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 3, 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, panel B, 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 5 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 (forthcoming) 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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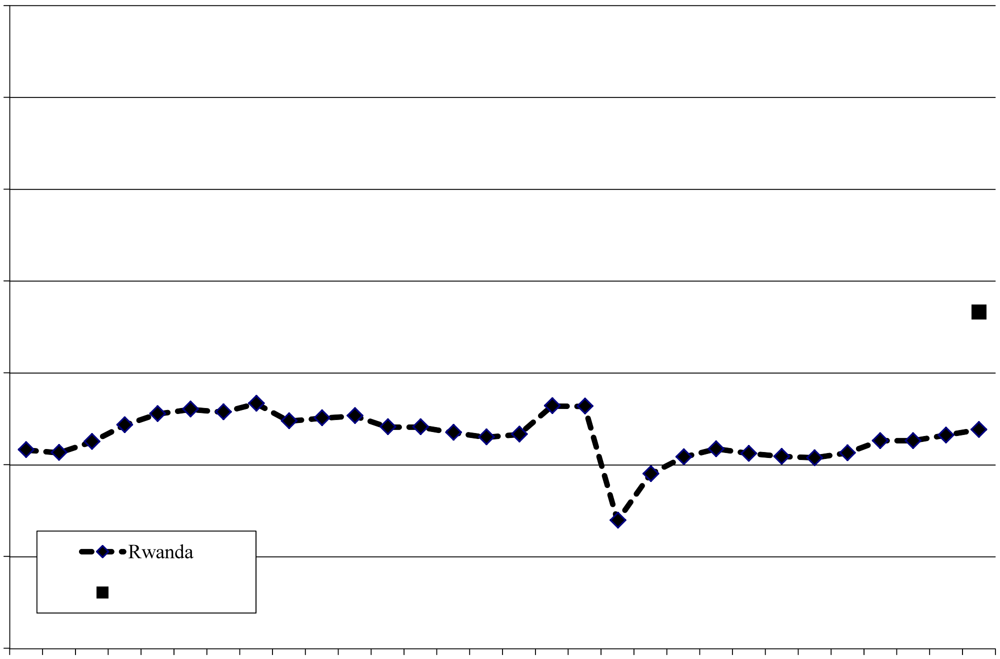
Panel A. GDP per capita in Rwanda and Zimbabwe



Panel B. Annual growth rate in Rwanda and Zimbabwe

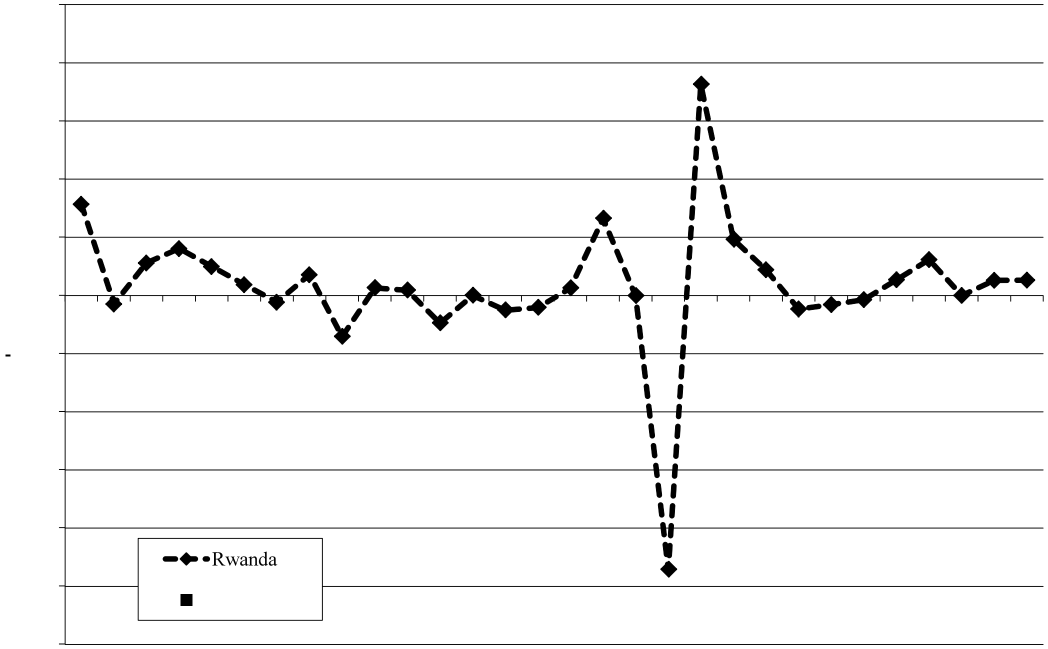


Figure 1: Income and growth in Rwanda and Zimbabwe
Note: Figures are expressed in constant dollars of 2000. Date source: World Bank's World Development Indicators

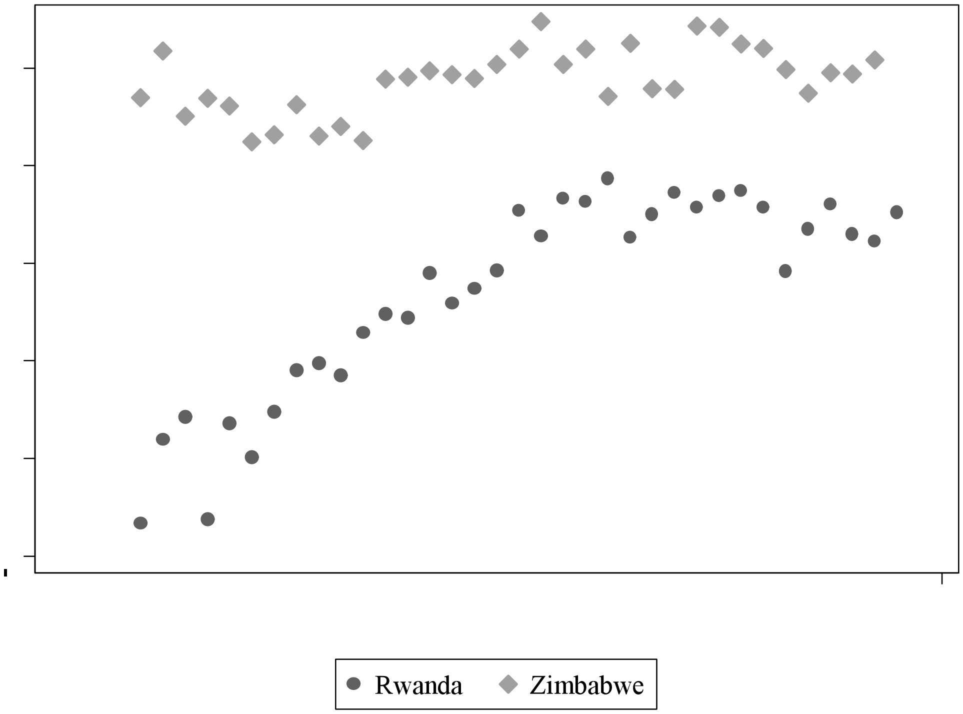


Figure 2. Women's height (z-score) in Rwanda and Zimbabwe by age in 1994.
Note: Each circle/rhombus represents the average height for age z-score by country and age in 1994. Data sources: 2000 and 2005 DHS for Rwanda and 1999 and 2005-2006 for Zimbabwe.

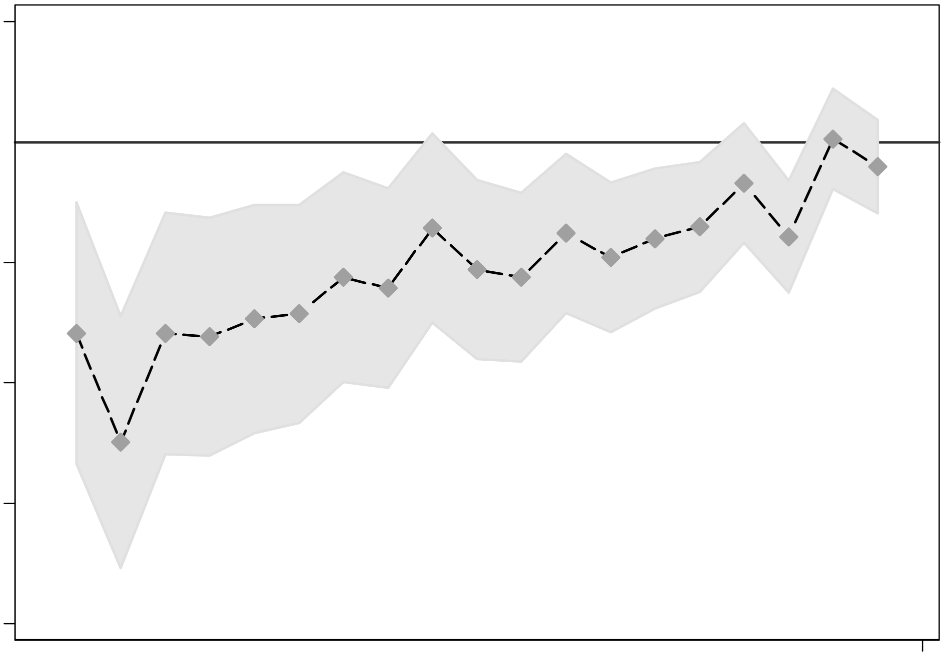


Figure 3. Effect of the genocide on height z-score by age in 1994.
Note: The regression includes age specific dummies where the omitted category is women aged 25 or more in 1994; country fixed-effects, linear country trends, a dummy for Rwanda, a dummy for living in urban area and survey fixed effects. 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.

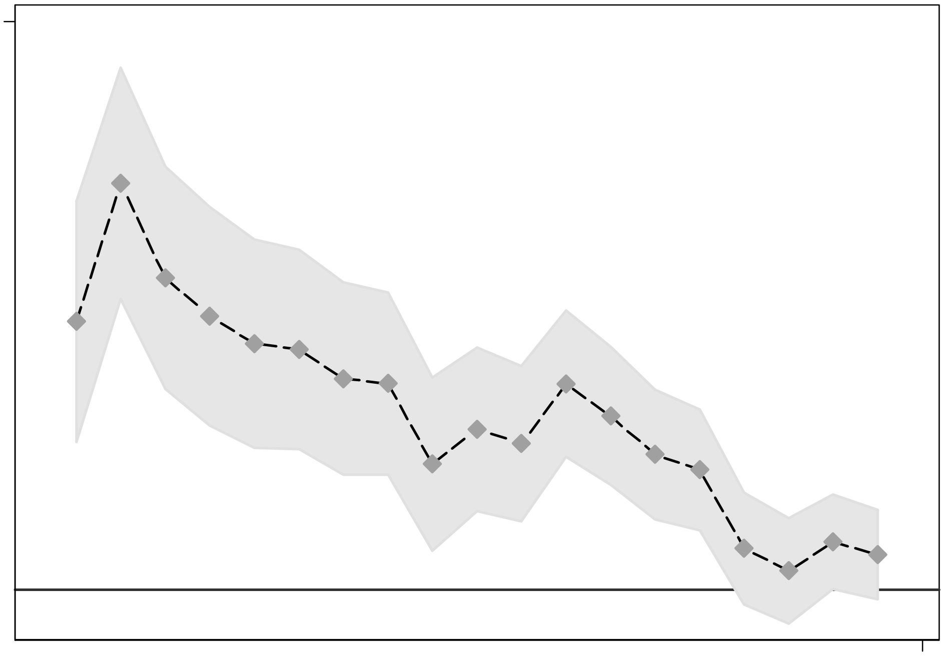


Figure 4. Effect of the genocide on the probability of stunting by age in 1994.
Note: The regression includes age specific dummies where the omitted category is women aged 25 or more in 1994, country fixed-effects, linear country trends, a dummy for Rwanda, a dummy for living in urban area and survey 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.

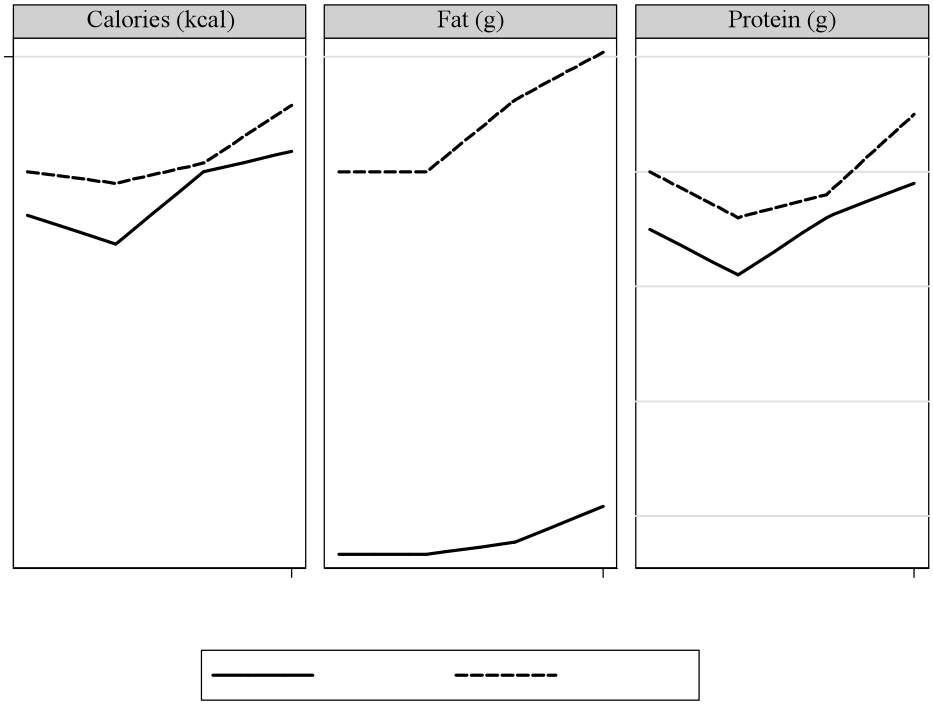


Figure 5. Energy, protein and fat consumption in Rwanda and Zimbabwe
Note: Consumption is measured in kcal or grams per person per day relative to Zimbabwean levels in 1990-92. Data Source: FAO (2010).

Table 1. Summary statistics

	Has height information:							
	No		Yes					
			All		Zimbabwe		Rwanda	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean (7)	SD (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 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 (1)	Linear (2)	Logarithmic (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) (1)	Proportion stunted (mean: 0.118) (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.