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## Civil Conflict, Sex Ratio and Intimate Partner Violence in Rwanda<sup>1</sup>

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**Abstract:** This paper examines the long-term impact of civil conflict on intimate partner violence and women's decision-making power using post-genocide data from Rwanda. Household survey data collected 11 years after the genocide indicate that women who became married after the genocide experienced significantly increased intimate partner violence and decreased decision-making power relative to women who became married before. The effect was greater for women in localities with high genocide intensity. I find that variation in the marriage market sex ratio across localities and over time explains part of the effect of the genocide on intimate partner violence.

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## 1. Introduction

Intimate partner violence against women is a widespread phenomenon that spans across societies. It affects one out of three women aged 15 and over throughout the world (Devries et al. 2013). The consequences extend beyond women's safety; intimate partner violence has negative consequences for the health of both women and children (Campbell 2002). For instance, Aizer (2011) shows that experiencing violence during pregnancy reduces birth weight. These effects are costly—in 2003, 4.1 billion dollars were spent on medical care services for victims of intimate partner violence in the United States (CDC 2003). Despite the pervasive and harmful nature of intimate partner violence, only recently have economists started considering it as a research topic (Tauchen et al. 1991; Farmer and Tiefenthaler 1997).<sup>1</sup>

Intimate partner violence can also inform economists about the functioning of the household because it may reflect the distribution of bargaining power within the family (Farmer and Tiefenthaler 1997; Aizer 2010).<sup>2</sup> As has been shown, intra-household allocation affects investment in children's human capital (Thomas 1990; Duflo 2003). Therefore, identifying factors that shift intra-household allocation could help to reduce differences in human capital. Intra-household allocation may be affected by numerous economic, demographic and institutional factors, such as women's income opportunities, women's property rights and marriage market conditions (Grossbard-Shechtman 1993; Duflo 2003; Porter 2007; Wang 2014).

This paper examines the long-term impact of civil conflicts on intimate partner violence using data from post-genocide Rwanda. In 1994, an estimated 800,000 individuals out of a population of 7 million were killed in just three months. Because the Rwandan genocide was a short and high-intensity conflict, it deeply affected the structure of the population. For instance, census data indicate that the sex ratio (men to women) for the 18-50 age groups decreased from .95 in 1991 to .88 in 2002. Higher male mortality during the genocide, male incarceration and

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<sup>1</sup> Tauchen et al. (1991) and Farmer and Tiefenthaler (1997) were the first to study intimate partner violence in economics. An incomplete list of recent contributions includes Bloch and Rao (2002), Aizer (2010), Jacoby and Mansuri (2010), Card and Dahl (2011), Hjort and Villanger (2011), Chin (2012), Sekhri and Storeygard (2012), Heath (2012), Hidrobo and Fernald (2013), Hidrobo et al. (2013), Bobonis et al. (2013) and Chin (2013).

<sup>2</sup> Farmer and Tiefenthaler (1997) and Aizer (2010) incorporate domestic violence in a household bargaining model. Factors that improve the woman's threat point, such as increases in the woman's potential earnings outside the household, enhance her bargaining power in the marriage and decrease the risk of domestic violence.

out-migration contributed to such decline (Verpoorten 2005; Straus 2006; de Walque and Verwimp 2010). Therefore, the Rwandan context provides an ideal opportunity to study the impact of armed conflict on intimate partner violence through the previously overlooked channel of the marriage market.<sup>3</sup>

Identifying factors that may cause intimate partner violence in post-conflict societies is crucial to improving women's and children's well-being. Several studies have shown that civil conflicts have negative long-term implications for children's health and education (Akresh et al. 2012; Justino et al. 2011; León 2012; Minoiu and Shemyakina 2012).<sup>4</sup> Through intimate partner violence and intra-household allocation, the negative effects of civil conflicts on human capital may even extend to future generations not directly affected by wars.

Studying the effect of civil conflict on intimate partner violence in Rwanda is challenging because data on domestic violence were collected for the first time in 2005 (11 years after the genocide).<sup>5</sup> In this paper, I exploit both variation in genocide intensity across localities and time variation in women's years of marriage. I use both these variations to test whether the conflict affected the risk of intimate partner violence in couples that formed after the end of the genocide, relative to couples that formed before. By combining recently released customary courts' records, census data and a household survey, I create a novel data set. I improve on pre-existing matches by using ArcGIS as well as a merge based on localities' names to combine the three data sets.

In my study, I find that becoming married after the genocide increased the probability of intimate partner violence for women, relative to women who became married before the conflict. In addition, I find that the effect was greater in localities with a high genocide intensity than in localities with low genocide intensity. The estimated effect is quite large: an increase of one standard deviation in genocide intensity in the commune of residence increases the probability of

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<sup>3</sup> Intimate partner violence against women is common and generally tolerated in Rwanda, as reflected by proverbs that state "a woman who is not yet battered is not a real woman" (Human Rights Watch 1996) and "a girl is ready food to be eaten any time" (USAID).

<sup>4</sup> Justino et al. (2011) and Leon (2012) analyze the long-term effects of civil conflict on education in Timor Leste and Peru; Akresh et al. (2012) and Minoiu and Shemyakina (2012) show that children who were exposed to civil wars in Ethiopia and Cote d'Ivoire had lower height-for-age z-score compared to similar children who were not exposed to the conflict. Because children's height is an important determinant of later educational and labor market outcomes, these effects are likely to be long-lived.

<sup>5</sup> Data on domestic violence was first collected in Demographic and Health Surveys (DHS) in the early 2000s (Kishor 2005).

spousal violence by 5.6 percentage points. This effect is equivalent to an increase of 30% with respect to the sample mean.<sup>6</sup> The results are robust to a falsification test using domestic violence between the woman's parents as a dependent variable to test for pre-existing differential trends in intimate partner violence across localities. Furthermore, I show that the results are robust to using the woman's year of birth as an instrument for her year of marriage, to relieve concerns that the timing of marriage is endogenous. I also show that the results are not driven by selection into marriage or an increase in reporting of intimate partner violence in localities with higher genocide intensity after the genocide.

Upon investigation, the most plausible mechanism for this finding is a change in the balance of the sexes in the marriage market (sex ratio), which is an important determinant of the relative well-being of men and women within the marriage (Becker 1973 and 1981).<sup>7</sup> I show that women who faced higher competition for grooms at the time of marriage—as measured by a lower sex ratio—have a higher probability of intimate partner violence. When I include both the sex ratio and the index of genocide intensity in the regression, the coefficient estimate on the index of genocide intensity declines and becomes statistically insignificant, while the coefficient on the sex ratio stays negative and statistically significant. This result indicates that the decline in the sex ratio explains at least one-third of the effect of the genocide on intimate partner violence. Showing the first evidence of a relationship between sex ratio and intimate partner violence is the primary contribution of this paper.

Furthermore, I show that intimate partner violence is not the only aspect in which women experienced worse marital outcomes post-genocide. Women who married after the genocide in localities where the conflict was more intense are less likely to have the final say on several issues, including the allocation of household income, relative to women who married before the end of the conflict and women in localities with low genocide intensity. These results provide supporting evidence for the hypothesis that the genocide affected intra-household allocation

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<sup>6</sup> This estimate means that women who live in a locality in the 75<sup>th</sup> percentile of genocide intensity are on average 8.2 percentage points more likely to experience domestic violence than women who live in a locality in the 25<sup>th</sup> percentile of genocide intensity. This effect represents an increase of 43% in the sample mean.

<sup>7</sup> At the national level, the sex ratio for the 18-50 age group decreased from .95 in 1991 to .88 in 2002. This implies that the “surplus of women” increased by a factor of 2.5.

through the marriage market. By affecting women's decision-making power, the conflict could affect the human capital of future generations.

Finally, I consider alternative mechanisms for the main result: increased spousal violence. For instance, other effects of genocide might have increased men's propensity to commit spousal violence, such as a lack of social order or increased prevalence of post-traumatic stress disorder (World Bank 2011; Cesur and Sabia 2013). I show that these mechanisms are likely not driving the results. First, I would expect increased traumatic stress disorder to increase spousal violence for all women, regardless of their time of marriage. As such, these mechanisms do not explain the greater increase in intimate partner violence for women who married after the genocide relative to women who married before. Additionally, a retrospective panel data set created using women's self-reported history of domestic violence indicates that the genocide did not increase the risk of spousal violence for women who married before the outbreak of the conflict. Second, I would expect the lack of social order to increase any type of violent behavior, not only intimate partner violence. I rule out this conjecture by showing that the genocide did not increase the probability of being a victim of violence by individuals other than partners. Altogether, these results dismiss trauma-induced violent behavior and the lack of social order as potential mechanism behind the main results.

This paper contributes to an extensive literature base on the effects of marriage market sex ratios.<sup>8</sup> Because sex ratios may in some cases be endogenous, studying their effects is challenging. As such, previous research has exploited changes in the sex ratio caused by wars to estimate its impact on marital and bargaining outcome.<sup>9</sup> This paper uses the plausibly exogenous variation in the marriage market sex ratio caused by the Rwandan genocide to shed light on the relationship between sex ratios and intimate partner violence. To my knowledge, this is the first paper to investigate that relationship.

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<sup>8</sup> See for example Bergstrom and Lam (1988), Angrist (2002), Amuedo-Dorantes and Grossbard (2007), Brainerd (2007), Porter (2007), Abramitzky et al. (2011), Francis (2011), Bethmann and Kvasnicka (2013), Lafortune (2013), Edlund et al. (2013), Schindler and Verpoorten (2013).

<sup>9</sup> Abramitzky et al. (2011) exploited changes in sex ratio caused by male mortality during WWI in France; Brainerd (2007) exploited changes in sex ratio related to WWII in Russia; Schindler and Verpoorten (2013) used changes in sex ratio associated with the genocide to study the effect of sex ratios on marital outcomes in Rwanda.

Besides this main contribution, this paper also adds to the growing literature on the effects of civil conflicts on gender inequality.<sup>10</sup> Two recent studies analyze the effect of low intensity, short duration conflicts on intimate partner violence in Latin America. Calderón et al. (2011) find that displaced women in Colombia are more likely to work and contribute to household income but are also more likely to experience intimate partner violence. Gallegos and Gutierrez (2011) use data from Peru to show that women who were exposed to civil war events at a young age are more likely to become victims of domestic violence and to be tolerant of domestic abuse. Unlike these studies, this paper deals with a high intensity, short duration conflict that dramatically affected the demographics of the country. This context allows me to explore mechanisms through which civil conflicts affect the marriage market and intimate partner violence.

This study is also closely related to projects that study the impact of the Rwandan genocide. Previous research has found that the genocide had a direct negative effect on income and consumption (Justino and Verwimp 2013; Serneels and Verpoorten 2013). Moreover, it affected income indirectly by reducing schooling and height for cohorts that were directly exposed to the genocide (Akresh and de Walque 2008; Agüero and Deolalikar 2012). The genocide also affected age at first marriage and age at first birth (Jayaraman et al. 2009) and fertility (Schindler and Brueck 2011).<sup>11</sup> In addition to providing the first evidence on the effect of the genocide on intimate partner violence, this paper uses different and newly matched data to study the effect of the genocide on family outcomes.<sup>12</sup> Some of the previous studies used death within the family, house damage or land loss as proxies to represent measures of exposure to the conflict. Conversely, this paper uses recently released data obtained from the records of the gacaca courts to measure conflict intensity in the locality of residence. This is helpful because the genocide may have affected intimate partner violence through mechanisms that go beyond household victimization. I also improve on previous research by conducting the analysis at the commune level rather than at the province level.<sup>13</sup> The commune level is a finer, more accurate unit of analysis, which improves precision and reduces measurement error associated with larger

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<sup>10</sup> This literature was recently summarized by Buvinic et al. (2013).

<sup>11</sup> The paper is also related to studies that investigated the impact of civil conflicts on marriage-related outcomes in different contexts. See for example Shemyakina (2013).

<sup>12</sup> Finnoff (2012) analyzes the cross-sectional correlates of domestic violence in post-genocide Rwanda using the 2005 DHS. She shows that women who work for a wage and whose husbands do not work are more likely to be victims of domestic sexual violence in post-genocide Rwanda.

<sup>13</sup> Serneels and Verpoorten also 2013 conducted the analysis at the commune level.

regions. Finally, in another differentiation from previous studies, I distinguish between measures of sex ratios that include or exclude the prison population; I find that higher incarceration rates worsen the negative effects of gender imbalances.<sup>14</sup>

The rest of the paper is organized as follows: Section 2 provides a historical background for the Rwandan genocide; section 3 describes the data used; section 4 outlines the empirical strategy; section 5 presents the results; section 6 discusses evidence of various mechanisms that affect the outcomes; and section 7 concludes.

## **2. Background**

This section provides a background for the social and historical context in which the Rwandan genocide took place.

The population of Rwanda is divided between Hutus and Tutsis. Before the genocide, Tutsis accounted for 8% of the population.<sup>15</sup> Traditionally, these group titles reflected differences in economic activity and socioeconomic status rather than differences in tribes (Straus 2006). In fact, before colonialism, the two ethnic categories were to some extent fluid; it was even possible to move from one group to the other as a form of upward or downward social mobility (Prunier 1995; Straus 2006). Hutus and Tutsis did not live in isolated communities, as shown by data collected before the genocide. They spoke the same language, lived in the same localities, practiced the same religion and intermarried (Prunier 1995; Straus 2006). Moreover, they had similar family and marriage outcomes as measured by marriage rates, sex ratio, labor force participation among women, number of children and frequency of polygamous unions. In sum, evidence suggests that gender roles were not different between Hutus and Tutsis. This is especially important for this study, given that I do not observe ethnicity in my data.

Belgian colonial officers reinforced the existing differences in socioeconomic status between Hutus and Tutsis and transformed them into race categories (Straus 2006). After Rwanda became independent in 1962, Hutus initiated anti-Tutsi violence. As a result, thousands of Tutsis went to exile in neighboring countries, where they began a rebel movement named Rwanda Patriotic Front (RPF). In 1990, the RPF started a civil war against the Hutu-dominated

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<sup>14</sup> This result is consistent with studies that demonstrate the negative effect of incarceration on women's outcomes in the United States (Charles and Luoh 2010; Mechoulam 2011).

<sup>15</sup> Data on ethnicity were not collected after the genocide.

government that lasted until August 1993, when the two parties signed an agreement (Straus 2006).

The political and social tension between Hutus and Tutsis continued to escalate during the period that preceded the genocide. On April 6, 1994, the Rwandan president, Habyarimana, died in an aviation accident in Kigali (Prunier 1995; Straus 2006). The extremist fringe quickly took advantage of the accident, took control of the country and instigated mass violence against the Tutsi population and, to a lesser extent, moderate Hutus. The ethnic violence quickly spread from the capital to the other cities and the rural regions. The radio fomented a large fraction of the civilian population to participate in the killings (Prunier 1995; Gourevitch 1998; Straus 2006; and Yanagizawa-Drott 2012). In just 100 days, an estimated 800,000 people out of a population of 7 million were killed. The genocide ended on July 17, 1994.

Economists have previously examined the characteristics of the genocide victims, such as ethnicity, gender, education and socioeconomic status. Verpoorten (2005) argued that 77% of the Tutsi population of Rwanda was killed. Additionally, she found that thousands of Hutus were massacred. De Walque and Verwimp (2010) investigated the characteristics of genocide victims using sibling mortality data included in the 2000 Rwanda DHS. They showed that educated adult men with an urban background were more likely to die in 1994. Other authors reported that men were more likely than women to die during the genocide (Gourevitch 1998; Des Forges 1999; Ministry of Finance and Economic Planning, National Census Service 2003). Generally, these studies suggest that the genocide might have affected the marriage market by reducing both the sex ratio and average educational attainment for men.

After the genocide, the marriage market sex ratio declined dramatically. This happened for three main reasons: higher male mortality during the conflict (Gourevitch 1998; Des Forges 1999; Ministry of Finance and Economic Planning, National Census Service 2003; de Walque and Verwimp 2010), higher male incarceration and higher male outmigration because of higher male participation in the killings (Verpoorten 2005; Straus 2006).<sup>16</sup> Figure 2 illustrates the decline in the sex ratio—defined as the relative number of men to women—by age in 1991 and

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<sup>16</sup> The fact that the most perpetrators were men is reflected in the gender composition of the prison population after the genocide. According to the 1991 Census, 0.11% of the population lived in prison in 1991. This proportion skyrocketed ten years later: in 2002, 1.3% of the population was living in prison. More than 95% of those living in prison in 2002 were male.



2002. The figure clearly shows a large decline in the relative number of men to women aged 20 to 45.

### **3. Data and Descriptive Statistics**

The analysis in this paper requires data on intimate partner violence and women's characteristics, as well as measures of conflict intensity and data on the marriage market. This section discusses the data and the matching method used to combine the various data sets together.

#### **3.1. Variables and Summary Statistics**

##### **3.1.1. Data on Genocide Intensity**

Measures of genocide intensity were obtained from the records of the gacaca courts. The gacaca courts are part of the transitional Rwandan justice system charged with judging suspects in the 1994 genocide.<sup>17</sup> Their records contain information on the number of accused genocide perpetrators (those who organized the killings, those who killed and those who looted) and genocide survivors (widows, orphans and disabled) at the sector level. The variables were aggregated at the commune level using the matching method described below. Then they were normalized using population counts from the 1991 census.<sup>18</sup>

Summary statistics on key variables included in the analysis are presented in Table 1 and Table 2. The proportion of survivors and perpetrators, genocide intensity, sex ratio, population density and literacy are measured at the commune level. All other variables are measured at the individual and household level. On average, 11.1% of the commune population was accused of participating in the genocide, and about 2% was classified as genocide survivors. Following Verpoorten (2011), I created an index that aggregated the six variables from the gacaca records using the first component of the principal component analysis. I standardize the index to mean zero and standard deviation one. As expected, the index of genocide intensity is strongly correlated with the variables from the gacaca records. The correlation is higher than .87 for widows, orphans and perpetrators suspected of organizing the killings or killing, and it is higher than .72 for disabled survivors and perpetrators suspected of looting.

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<sup>17</sup> A detailed assessment of the measures obtained from the gacaca records can be found in Verpoorten (2011).

<sup>18</sup> It is important to note that the measure of genocide intensity used in this paper is not a measure of the death toll. Variation in the genocide index comes from variation in the intensity of participation in genocide and variation in the number of survivors that are still alive in the commune.

Figure 1 shows geographic variation in the index of genocide intensity. The genocide was more intense in the Southern and Western regions of the country. Genocide intensity is highly correlated with the pre-genocide proportion of Tutsi population (Verpoorten 2011) and the distance from the border with Uganda (Akresh and de Walque 2008; Serneels and Verpoorten 2013). For the latter reason, Southern and Western regions were more heavily affected.

### **3.1.2. Data on Marriage Market Sex Ratios**

I use data from the 1991 and 2002 censuses to create measures of marriage market conditions. As highlighted by Neelakantan and Tertilt (2008), the marriage sex ratio tends to be lower than the sex ratio at birth and the population sex ratio. This happens for two reasons: first, the fact that men marry younger women together with population growth leads to an excess supply of women in the marriage market; and second, higher mortality rates for men also lower marriage market sex ratios. The genocide increased mortality rates of men relative to women and thus decreased the marriage market sex ratio.<sup>19</sup> I construct a measure of the sex ratio that takes into account that the average age gap between husband and wife is 5 years, with men being older.<sup>20</sup> I define the sex ratio in a commune as the number of men aged 20 to 54 living in the commune divided by the number of women aged 15 to 49 living in the commune.<sup>21</sup> Unlike previous studies that always included incarcerated individuals in the sex ratio, in some specifications I excluded incarcerated individuals from the population count.<sup>22</sup>

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<sup>19</sup> Additionally, high incarceration rates and emigration of genocide perpetrators also contributed to decreasing the sex ratio in the short term. Unlike higher mortality rates for men, the effect of incarceration and emigration on the sex ratio might be temporary—inmates might eventually be released and exiled individuals might return to Rwanda. Nonetheless, as noted in section 2 of the paper, the prison population was still high in 2002.

<sup>20</sup> The average age gap between husband and wife is 5.8 for women who married before the genocide and 5.2 for women who married after.

<sup>21</sup> A similar measure of the sex ratio was used by Abramitzky et al. (2011).

<sup>22</sup> The 1991 and 2002 census data downloadable from IPUMS allows me to identify individuals living in prison. Unfortunately, the disaggregated version of the 2002 census that I use in this paper does not identify individuals in prison. Information on prisons' locations was collected on the website <http://www.rcs.gov.rw/prisons.html> (last access on 10/3/2013). I matched sector-level 2002 census data with government data on prisons' locations and I dropped the sectors in which a prison was located when computing the sex ratio. 2002 census data indicate that the sectors in which a prison was located had much higher average age, sex ratio and migration rate compared to all other sectors within the same commune. The district-level sex ratio obtained using this method is very similar to the district-level sex ratio obtained by dropping individuals in prison from the IPUMS version of the 2002 Census. These results are available upon request.

Figure 3 and Figure 4 show the distribution of the sex ratio across cohorts in 1991 and 2002. The sex ratio already seems quite low in 1991. High population growth and emigration (Verpoorten 2005) are potential explanations for this evidence.<sup>23</sup> The sex ratio decreased significantly in 2002, and by excluding incarcerated individuals I significantly decrease the number of outliers (Figure 4). How are genocide intensity and changes in the sex ratio related? The correlation between genocide intensity and the absolute change in the sex ratio between 1991 and 2002 measured at the commune level is 0.08. The correlation is significantly larger when I exclude incarcerated individuals from the sex ratio (0.38).

### **3.1.3. Data on Intimate Partner Violence**

The primary source of microeconomic data on intimate partner violence and household characteristics is the 2005 DHS. Information on domestic violence was collected through a domestic violence module. Approximately 4,000 households were selected and all eligible women in the household (women aged 15-49) were interviewed for the core questionnaire, but only one woman in each selected household was randomly selected to be interviewed in the domestic violence module.<sup>24</sup> Among the selected women, never married and widowed women did not respond to the questions on intimate partner violence.<sup>25</sup> In addition, approximately 150 separated women did not respond to the survey, leaving 2,544 observations. Lastly, I excluded from the main analysis 399 women who were married more than once, for whom information on the date of the current union could not be recovered with certainty. Ultimately, the primary sample included roughly 2,000 women.

The main outcome of interest is a binary variable that takes value 1 if a woman suffered any aggressive behavior by her current partner in the 12 months before the survey. Violent behavior includes pushing, shaking, throwing something, slapping, twisting an arm, striking with a harmful object, kicking or dragging, attempting to strangle or burn, threatening with a weapon and attacking with a weapon. Other outcomes of interest include whether the woman was ever a victim of physical violence by a current or former partner, any sexual violence committed by the

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<sup>23</sup> Given that men marry younger women, systematic growth in cohort sizes implies that each cohort of men is matched with a larger cohort of women, giving rise to a marriage squeeze on women, i.e., their excess supply (Bhaskar 2012).

<sup>24</sup> This was done to assure privacy and safety for the respondents.

<sup>25</sup> There were 1,195 never-married women and 157 widowed women in the sample.

current partner in the last six months, violence committed by individuals other than her partner, measures of decision-making power and attitudes toward domestic violence.

Decision-making power is measured by the sum of situations in which the woman has the final say out of five hypothetical situations: the woman's own health care, making large household purchases, making household purchases for daily needs, visits to family or relatives, and the food to be cooked each day. Attitudes toward intimate partner violence are measured by the number of situations in which the woman agrees that husbands are justified in beating their wives out of five possible situations. The situations include: when she goes out without telling him, neglects the children, argues with him, refuses to have sex with him, or burns the food.<sup>26</sup>

### **3.2. Data Matching**

This paper combines data from the records of the gacaca courts with the 2005 Demographic and Health Survey (DHS) and the 1991 and 2002 Rwandan Census. Because the administrative boundaries changed over time, using these data sets together required matching between administrative units. To be more specific, the boundaries of Rwandan communes and prefectures changed 3 times between 1991 (when the pre-genocide census was administered) and 2006 (when the Organic Law that established the competencies of the gacaca courts in trying suspected genocide perpetrators was passed.)<sup>27</sup>

In contrast, the names and boundaries of sectors were roughly maintained between 1991 and 2006, which allows me to match the various data sets uniquely. The 1991 Census data are available at the sector and commune level, data from the 2002 Census and the gacaca records are available at the sector and 2006 district level, and the 2005 DHS is geocoded.<sup>28</sup> I harmonized the various data sets by using the pre-genocide administrative level of the commune as a unit of

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<sup>26</sup> All women interviewed in the core questionnaire, including never-married, widowed and remarried women, were given the opportunity to answer questions on decision-making power and attitudes toward domestic violence. Therefore the sample size is larger compared to that used in the variables on intimate partner violence.

<sup>27</sup> More precisely, until 2002 the country was divided into 1,484 sectors, 145 communes and 11 prefectures. In 2002, an administrative reform replaced the communes with 104 districts, and the prefectures were transformed into 12 provinces. Finally, the boundaries changed again in 2006, when the country was split into 30 districts and 5 regions.

<sup>28</sup> The 2002 census data from IPUMS is available at the 2002 district level. Marijke Verpoorten shared population counts from a restricted version of the 2002 census that is available at the sector level. I use her restricted data set to compute the 2002 sex ratio at the commune level.

analysis.<sup>29</sup> To do so, I aggregated the gacaca data and the 2002 Census at the commune level and assigned DHS clusters to the communes in ArcGIS.

The crucial step was creating a link between the sectors in the gacaca data, the sectors in the 2002 Census and the communes. In their work, Friedman (2011) and Yanagizawa-Drott (2012) had merged the gacaca data with the 1991 Census using sector names as key. Still, this method could lead to measurement error because many sector names were duplicates and a few sectors changed names during this period.

In this project, I combined a merge obtained in ArcGIS with a merge based on localities' names. Sectors, communes and 2006 districts were first linked in ArcGIS using their geospatial coordinates. The link obtained in ArcGIS was then merged with the gacaca data using sector names as well as 2006 district names as key.<sup>30</sup> The match was a major improvement with respect to the previous matches for two reasons. First, linking sectors to communes based on their geographic coordinates reduced measurement error created by duplicate sectors. Second, error was further reduced by merging based on sector names and district names rather than sector names only.<sup>31</sup>

#### **4. Empirical Strategy**

This section explains the empirical strategy used to identify the effect of the genocide on intimate partner violence through changes in the marriage market. Subsection 4.1 presents the difference-in-differences strategy. Subsection 4.2 discusses threats to validity.

##### **4.1. Difference-in-differences Analysis**

The primary objective of this paper is to study how civil conflict affects spousal violence through changes in local marriage market conditions. The main identification strategy used to estimate this relationship is similar to difference-in-differences. It exploits cross-sectional geographic variation in genocide intensity across communes and temporal variation in marriage timing across marriage cohorts. In other words, the empirical strategy combines a difference-in-differences strategy with a continuous treatment variable.

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<sup>29</sup> In 1991, 51,235 persons lived in a commune on average and the mean size of superficies was 153.8 square meters.

<sup>30</sup> As a robustness check, I also merged the data obtained from this merge with the 1991 census data using sector names as well as commune names as key.

<sup>31</sup> In a few cases, mostly when sectors had changed names, I matched the sectors with the communes manually. For the data on genocide perpetrators, I followed the method used by Friedman (2011), who kindly shared her code with me. For the data on genocide suspects, I matched the data manually.

I estimate the following regression equation:

$$Y_{ick} = \beta_0 + \beta_1 \text{Genocide}_c \cdot \text{Married after}_k + X_{ick}\delta + X_{ck}\sigma + \theta_c + \gamma_k + \varepsilon_{ick} \quad (1)$$

In this specification,  $Y_{ick}$  indicates whether woman  $i$  in commune  $c$  belonging to marriage cohort  $k$  (i.e. became married in year  $k$ ) was a victim of physical violence by her partner in the last 12 months.  $\text{Genocide}_c$  is a measure of genocide intensity in commune  $c$ .  $\text{Married after}_k$  is a binary indicator that takes value 1 if the woman became married after the genocide and zero otherwise.  $X_{ick}$  is a vector of variables that contains measurements of the woman's and her husband's characteristics, such as religion, the woman's age and her age at first marriage, woman's and partner's educational attainment, urban status and an asset index.  $X_{ck}$  is a vector of time-varying commune-level characteristics obtained from the 1991 and 2002 censuses, such as the literacy rate and population density. I assign women a value from the 1991 Census if they married before the genocide and a value from the 2002 Census if they married after the genocide.  $\theta_c$  is a commune-specific fixed effect that accounts for unobserved persistent heterogeneity across communes.  $\gamma_k$  represents a year of marriage fixed effect. It controls for unobserved shocks that are common to women in the same marriage cohort. I estimate equation (1) using a linear probability model (OLS) and I cluster the standard errors at the commune level to account for within-commune serial correlation (Duflo et al. 2004).<sup>32</sup> All regressions are weighted using survey weights.

The coefficient of interest is  $\beta_1$ , which represents the difference-in-differences estimator. Because the measure of genocide intensity is standardized to have mean zero and standard deviation one,  $\beta_1$  indicates the effect of a one standard deviation increase in genocide intensity on the probability that a woman who married after the genocide becomes a victim of spousal violence relative to a woman who married before. The effect is measured with respect to the mean value of genocide intensity, which is equal to zero.

## **4.2. Threats to Validity**

### **4.2.1. Parallel Trends**

The key identifying assumption is that, in the absence of the genocide, trends for intimate partner violence would have been similar in high and low genocide-intensity communes. It is

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<sup>32</sup> I choose to report the results of the linear probability model because the interpretation of the coefficient estimate on the interaction term is straightforward. Nevertheless, the results are similar when I estimate equation (1) with a Probit model. The results are available upon request.

virtually impossible to test for this assumption directly because in reality I cannot observe the counterfactual. In other words, I cannot examine how spousal violence would have evolved in high genocide-intensity localities had the genocide not occurred. A solution that has been frequently adopted in the literature is to test for parallel trends before the event of interest occurred (Angrist and Pischke, 2008). In the context of this paper, this would translate into comparing trends in high and low genocide intensity over some period before the genocide occurred. Unfortunately, the lack of a baseline survey precludes me from running such test in the analysis. Nevertheless, I can exploit information contained in the data to run a similar test.

Women are asked whether their mother was a victim of spousal violence. Because women normally give birth during their 20s, this is roughly equivalent to observing intimate partner violence for women born about 20 years before the women in the sample.<sup>33</sup> I can thus test the parallel trends hypothesis by comparing rates of spousal violence for the mothers of women who married after the genocide with rates of spousal violence for the mothers of women who married before the conflict.<sup>34</sup> To do so, I estimate equation (1) using domestic violence between the woman's parents as a dependent variable. If there were preexisting differential trends in high genocide localities, I would expect the coefficient on the interaction term to be statistically significant. Column 1 of Table 3 presents the results of the falsification test. The estimated coefficient on the interaction term is small and not statistically different from zero. Thus, the result of the placebo test provides no supporting evidence for the hypothesis of pre-existing differential trends for intimate partner violence in high-genocide localities.

Besides the aforementioned falsification test, I control for differential trends in three additional ways. First, in some specifications I include time-varying commune level controls, such as population density and literacy rate. These variables control for changes in economic and social conditions that happened after the genocide at the commune level. Second, in some specifications I control for a linear time trend at the province level ( $\gamma_P \cdot k$ ). Province-specific time trends allow communes in provinces that were differentially affected by the genocide to have different trends. Third, in some specifications I include an interaction term between province fixed effects and an indicator for becoming married after the genocide ( $\gamma_P \cdot$

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<sup>33</sup> Before the genocide, the average age at first birth was 21 (1992 DHS).

<sup>34</sup> Calderón et al. (2011) and Chin (2013) use the same variable to construct a falsification test. Their identification strategies are quite different from the one used here because they use instrumental variables.

*Married after<sub>k</sub>*). This permits me to estimate the effect of the genocide on intimate partner violence using only within-province variation in genocide intensity and holding constant all unobservable time-varying factors at the province level.

#### 4.2.2. Endogeneity of Marriage Timing

There might be a concern that marriage timing is correlated with unobservable characteristics that affect both genocide intensity and intimate partner violence. To account for the potential endogeneity of marriage timing, I exploit the fact that the cohort of birth is a strong predictor for the cohort of marriage. I estimate a reduced form model that exploits temporal variation in year of birth instead of temporal variation in marriage timing. To be more specific, I replace *Married after<sub>k</sub>* in equation (1) with a binary variable that takes value 1 if the woman was born after 1973 and zero otherwise (“Younger than 21 in 1994”). This variable is a proxy for entering marriageable age after the genocide.<sup>35</sup> I also estimate a Two-Stage Least-Squares (2SLS) model using “Younger than 21 in 1994” as an instrumental variable for *Married after<sub>k</sub>*.<sup>36</sup> The first stage F statistic for the interaction term ( $Genocide_c \cdot Married\ after_k$ ) is well above the critical value, suggesting that the instruments are not weak (Table 4).

#### 4.2.3. Selection into Marriage

Because getting married is a choice and women self-select into relationships, there could be a concern that women with a lower propensity to tolerate domestic violence dropped out of the marriage market after the genocide.<sup>37</sup> If this behavior were more frequent in localities with higher genocide intensity, then negative selection into marriage would bias the results upward.<sup>38</sup> Concerns about selection may not be well founded in this setting, where getting married is the “default” choice for the majority of women. As documented by the literature, Rwandan culture places a high value on marriage and children; women’s role in society is that of wives and mothers (Human Rights Watch 1996). For instance, 1992 Demographic and Health Survey data

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<sup>35</sup> Before the genocide, the average age of marriage was 19.

<sup>36</sup> The results of the first-stage analysis are available upon request.

<sup>37</sup> This could happen if, for example, women anticipated experiencing more intimate partner violence as a result of the genocide.

<sup>38</sup> By negative selection, I mean that women with a higher propensity for intimate partner violence are more likely to get married than women with a lower propensity. Positive selection may also exist. However, positive selection is less worrisome because it would bias the results downward.



indicate that only 2% of women aged 30 to 49 were never married, and 95% of married women were married by age 25. Nonetheless, because the genocide affected the sex ratio, I could expect fewer women to get married after the genocide because fewer men were available for marriage.<sup>39</sup>

Given these concerns, I test for selection into marriage in two different ways. First, I examine whether genocide intensity is correlated with women's background characteristics that are determined outside of marriage, such as years of education and height.<sup>40</sup> To do so, I estimate equation (1) using years of education and height as dependent variables. Second, I estimate a two-step Heckman (1979) selection model for the reduced form regression, which I described in subsection 4.2.2. I use the number of younger sisters of the woman as an instrument for the selection equation. The number of younger sisters is strongly negatively correlated with a woman's age at first marriage throughout the developing world (Vogl 2013), but it should not affect intimate partner violence (besides its effect on age at marriage and marital outcomes). The results of the Heckman selection model are described in section 5.

Table 5 presents the results of the analysis of background characteristics for the whole DHS sample (columns 1 and 3) and for the domestic violence sample (columns 2 and 4). It is important to note that both the height and the years of education of the youngest women in the sample could be negatively affected by the genocide (Akresh and de Walque 2008; Agüero and Deolalikar 2012). In fact, the results reported in columns 1 and 2 indicate that women who married after the genocide in localities with higher genocide intensity have *more* years of education. The result is statistically significant and holds when I restrict the sample to women interviewed in the domestic violence questionnaire. On the other hand, columns 2 and 4 show that the correlation between genocide intensity and height is negative and not statistically significant. This result implies that married women in communes with high genocide intensity were positively selected after the genocide. This result has two other important implications. First, it suggests that negative selection into marriage is not driving the main result. Instead, the estimated effect of genocide on intimate partner violence could be biased downward if more educated women were less likely to be victims of intimate partner violence. Second, this result is

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<sup>39</sup> This statement would be correct if polygyny was not common in Rwanda. Polygyny exists in Rwanda in spite of being illegal. Although polygamy was formally banned in 1978, in 2002 10% of married women were in a polygamous union (2002 Census).

<sup>40</sup> This follows Chari et al. (2013).

consistent with a model of the marriage market where a decline in the demand for women pushes women with lower “quality” out of the marriage market.

### **4.3. Selective Migration**

Finally, non-random migration could pose a threat to the validity of the estimates. Specifically, selective migration could bias the results. The genocide generated two different types of migration: emigration of “new case” refugees out of Rwanda and immigration of “old case” refugees into Rwanda. Regarding emigration, the genocide resulted in massive population displacements. Specifically, about two million individuals were displaced in refugee camps at the borders of neighboring countries. Selective emigration could bias the results upward if, for example, women with a lower tolerance for intimate partner violence migrated out of localities with high genocide intensity after the genocide; this would imply a “negative” selection for those who stayed in high-genocide localities. If this were the case, the estimate of  $\beta_1$  would be biased upward. Nonetheless, the vast majority of refugees had returned home by December 1996 (Gourevitch 1998). Therefore, they would not be considered migrants in the 2005 DHS. This suggests that selective emigration is not a major concern in this setting.

Regarding immigration into Rwanda, about one million “old case” refugees returned home from neighboring countries after the genocide. Many of the returnees were descendants of Tutsis who fled Rwanda during the 1950s and 1960s (Gourevitch 1998). This mass return migration was “a historically unprecedented, epic phenomenon” (Gourevitch 1998 p. 230). Selective immigration could bias the results upward if, for example, women with a high tolerance for spousal violence were more likely to relocate into localities with high genocide intensity.

Previous studies often dealt with selective migration by estimating the intent-to-treat rather than the average treatment effect. This is usually done by assigning individuals to treatment based on their place of birth rather than their place of residence (Angrist and Pischke 2008). Unfortunately, the Rwandan DHS contains no information on place of birth. However, the DHS does provide information on “how many years the respondent has lived in the current place of residence.” I define a household as “non-migrant” if the woman or her husband has lived in the current place of residence longer than 11 years (since before the genocide). Husband’s migration history is a good approximation for the woman’s migration history because the majority of

women married someone from the same commune of birth.<sup>41</sup> I define a woman as “non-migrant” if the woman lived in her current place of residence for at least 11 years.<sup>42</sup> I estimate the results separately for non-migrant households and non-migrant women.

## 5. Results

This section describes the main results found in this paper, estimating the effect of the genocide on intimate partner violence through the marriage market.

Table 3 shows the estimates of the impact of genocide intensity on intimate partner violence for women who married after the genocide relative to women who married before. Column 2 shows that a one standard deviation increase in genocide intensity in the commune of residence raises the probability of intimate partner violence in the last 12 months by 5 percentage points. Put differently, women who live in a locality in the 75th percentile of genocide intensity are on average 7 percentage points more likely to have experienced intimate partner violence in the last 12 months than women who live in a locality in the 25th percentile of genocide intensity. These effects are quite substantial: the estimated impact represents an increase of 37% with respect to the sample mean (0.19). Including commune fixed effects and year of marriage fixed effects does not affect the estimate (column 3).<sup>43</sup>

In column 4, the inclusion of time-varying commune-level controls increases the estimate by about 10%, suggesting that the presence of time-varying omitted characteristics at the commune level attenuates the effect of the genocide. Column 5 adds province-specific linear time trends, and column 6 adds province dummies interacted with an indicator for becoming married after the genocide. These controls hold constant any variable that could vary at the province level and

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<sup>41</sup> According to the census, in 1991 59% of married individuals in the census were married to someone from the same commune of birth. In 2002, 72% of married individuals in the sample married someone from the same district of birth.

<sup>42</sup> Using this definition leads to a migration rate higher than 50%, which is inconsistent with other sources. For instance, census data indicate that, in 2002, 24% of the population lived in a district that was not their district of birth. This definition of non-migrant households might not be accurate because the questionnaire does not precisely define the “current place of residence”. I find that over 25% of the women seemed to report the year in which they moved to the current place of residence in a manner suggesting the year in which they moved to the groom’s house after the wedding.

<sup>43</sup> Other variables affect domestic violence in a reasonable manner. Age at first marriage is negatively related to domestic violence, and the effect is statistically significant. Woman’s education is negatively correlated with domestic violence, although the effect is not significant. Women married to husbands with more than primary education (approximately 3% of the sample) are 7 percentage points less likely to be victims of domestic violence with respect to women whose husbands have no education. These results are not reported but they are available upon request.

could affect women who married after the genocide differently from women who married before. Including these controls does not affect the results. This shows that the genocide increased intimate partner violence more in communes with high genocide intensity relative to communes with low genocide intensity within the same province.

There could be a concern that the timing of marriage is itself endogenous. Column 1 of Table 4 presents the results of the reduced form model that uses age at the time of the genocide as explanatory variable, instead of year of marriage. The results indicate that the genocide increased the probability of intimate partner violence for women who entered marriageable age after the genocide (as measured by being younger than 21 in 1994) relative to older women. The effect is slightly smaller compared to the estimate presented in Table 3; a one standard deviation increase in genocide intensity in the commune of residence increased the probability of being a victim of physical violence by 3.9 percentage points. Column 2 of Table 4 displays the results of the 2SLS model, where “Younger than 21 in 1994” is used as an instrumental variable for “Married after the genocide.” A one standard deviation increase in genocide intensity in the commune of residence increased the probability of intimate partner violence by 5.06 percentage points. In this case, the 2SLS estimate is almost identical to the OLS estimate presented in Table 3.

To address nonrandom selection into marriage in the aftermath of the genocide, I estimate a Heckman selection model. Table A1 in the Appendix displays the results of the Heckman selection model. The Heckman selection model is estimated for the reduced form regression (the results of the reduced form regression are presented in column 1 of Table 4). Column 1 of Table A1 shows the results of the selection equation. Only never-married women and women who are currently married or separated are included in the analysis (widowed women and women who married more than once are excluded). The number of younger sisters of a woman, a figure which is used as instrument in the selection equation, is strongly positively correlated with being married and being part of the sample. Column 2 presents the two-step estimate. The results are very close to the reduced form estimates. However, the hypothesis that the two parts of the selection model are independent cannot be rejected, suggesting that there is no selection into marriage.

The results presented in Table 3 indicate that the genocide had a differential effect on intimate partner violence for women who married after the genocide relative to women who

married before, and that the effect was greater in communes with higher genocide intensity. The estimated effect is large in magnitude and does not seem to be driven by pre-existing trends in localities with higher genocide intensity. In section A of the Appendix, I present numerous robustness checks for this finding. The results are found to be robust using alternative controls for age, restricting the sample to non-migrants and employing alternative specifications of the dependent variable. Additionally, I test for misreporting in three different ways and find no evidence of misreporting. The results reported in Table 3 could be interpreted in a causal fashion if they were generated by mechanisms that would affect women who married after the genocide but not women who married before. In the next section, I discuss the potential mechanisms behind the main results.

## **6. Potential Mechanisms**

This section discusses potential mechanisms for the effect of genocide on intimate partner violence for women who became married after the genocide relative to women who became married before. Subsection 6.1 presents and provides evidence for the preferred mechanism: changes in the marriage market that affected the relative well-being of men and women within a marriage. Subsection 6.2 discusses and tests alternative mechanisms.

### **6.1. Marriage Market Sex Ratio**

The preferred channel for the main result is related to the sharp decline in adult sex ratios following the genocide. Economists have long been interested in the sex ratio as a potential determinant of marriage outcomes. According to Becker's theories of marriage (Becker 1973 and 1981), the relative well-being of wives and husbands within their marriage depends on factors influencing the marriage market, including the sex ratio. When the sex ratio is low, men benefit from marriage and obtain a larger share of marital output. Becker proposed consumption expenditures benefiting husbands and wives and leisure time enjoyed by husbands and wives as possible indicators of relative well-being (Becker 1973; Grossbard-Shechtman 1995). Intimate partner violence could also be an indicator of the relative well-being of husbands and wives within the marriage. A lower sex ratio could thus increase intimate partner violence.

In this paper, I examine how the sex ratio *at the time of marriage* may affect intimate partner violence. I assign a woman the sex ratio from 1991 or 2002 based on the estimated probability that she became married after the genocide as predicted by her characteristics. To do so, I estimate a probit regression for marrying after the genocide, including controls for religion, year

of birth, number of siblings and place of residence during childhood.<sup>44</sup> The results of the probit regression are reported in Table A3 in the Appendix. For each woman, I predict the probability that she married after the genocide ( $\widehat{prob\_after}$ ) and then define the sex ratio as:

$$Sexratio_c = \widehat{prob\_after} \times Sexratio_{c2002} + (1 - \widehat{prob\_after}) \times Sexratio_{c1991} \quad (2)$$

$c$  is the commune of residence of the woman.  $Sexratio_{c1991}$  is the sex ratio in commune  $c$  from 1991 census data and  $Sexratio_{c2002}$  is the sex ratio in commune  $c$  from 2002 census data. I then estimate the following regression model:

$$Y_{ick} = \beta_0 + \beta_1 Sexratio_c + X_i \delta + X_{ck} \sigma + \theta_c + \gamma_k + \varepsilon_{ick} \quad (3)$$

Where  $Sexratio_c$  is the measure of the sex ratio described in equation (2),  $\lambda_k$  are year of marriage fixed effects and  $\theta_c$  are commune fixed effects. I obtained similar results when I assigned the sex ratio from the 1991 Census to women who became married before the genocide and the sex ratio from the 2002 Census to women who became married.<sup>45</sup>

Table 6 displays the estimates for the regression model described in equation (3) when physical violence by partner in the last 12 months is used as dependent variable. The marriage market sex ratio has a *negative* effect on the probability of intimate partner violence (columns 1 and 2), and the effect is large and statistically significant when incarcerated individuals are not included in the numerator (column 2). The estimated effect is quite sizeable: a one standard deviation decrease in the sex ratio increases the probability of being a victim of intimate partner violence by 12.5 percentage points (column 2). The sign of the coefficient estimate on the sex ratio is consistent with the theoretical argument described in subsection 6.1—when the sex ratio is low, the relative well-being of women within marriage decreases.

To disentangle the relative contribution of genocide intensity and the sex ratio, I also include the interaction term between genocide intensity and marrying after the genocide ( $Genocide_c \cdot Married\_after_k$ ) in equation (3). The results are shown in columns 3 and 4 of Table 6. When both sex ratio and genocide intensity are included in the regression, the estimated coefficient on the sex ratio remains significant but the estimated coefficient on the interaction term decreases by 38% (column 4). This suggests that at least one-third of the effect of the genocide on domestic

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<sup>44</sup> Place of residence during childhood is measured using binary indicators for whether she grew up in the countryside, in a town or in the capital.

<sup>45</sup> This follows Abramitzky et al. (2011). These results are reported in Table A4 the Appendix.

violence happened through the sex ratio. The results presented in Table 6 are robust to alternative definitions of the sex ratio (Table A4).

If the genocide affected intimate partner violence through changes in the marriage market, I would expect other measures of intra-household allocation to be affected as well. Therefore, I examine the effect of genocide intensity and changes in the sex ratio on women's decision-making power within the household. Table 7 displays the results for the regression model described in equation (3) using as the dependent variable indicators for whether the woman has *no say* in five different situations.<sup>46</sup> The results show that women who married after the genocide in localities where the genocide was more intense have *lower* decision-making power within the household, even in decisions on how to spend family resources (Panel A). Specifically, a one standard deviation increase in genocide intensity is associated with a 3.6% *increase* in the probability that the woman has no say on large purchases, which represents an 8% increase with respect to the sample mean (Panel A). A lower sex ratio is associated with lower decision-making power within the household for women who married after the genocide. However, the coefficient estimate on the sex ratio is not always statistically significant (Panel B and Panel C).

The results presented in Table 7 support the hypothesis that changes in the marriage market were a plausible mechanism through which the genocide affected intimate partner violence and intra-household allocation. The results complement findings that lower sex ratios in Rwanda increased marriage rates for men and decreased marriage rates for women after the genocide (Schindler and Verpoorten 2013).<sup>47</sup> The results are consistent with previous findings that higher sex ratios in China and Taiwan increased women's decision-making power within the household (Edlund et al. 2013) and increased human capital investments in children (Francis 2011; Porter 2007). The genocide might have long-lasting harmful implications for human capital investments in children through the negative effect on women's decision-making power.

## **6.2. Alternative Mechanisms**

Section 5 provides evidence that higher genocide intensity in the commune of residence is associated with higher risk of intimate partner violence for women who married after the end of

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<sup>46</sup> The sample size is larger with respect to the analysis of the effect of genocide on domestic violence because I do not have to restrict the analysis to women who responded to the domestic violence module.

<sup>47</sup> In an earlier paper, Schindler (2010) showed that lower sex ratios at the province and cohort level were associated with higher work within the household for young unmarried women, but not for married women.

the conflict relative to women who married before. The results described in Section 6.1 show that part of this effect can be explained by low marriage market sex ratios. Moreover, higher genocide intensity and, marginally, lower sex ratios are associated with lower decision-making power for women. These results are consistent with sex ratios at the time of marriage affecting the well-being of women within the marriage. Nonetheless, the estimated relationship between genocide intensity and intimate partner violence may be driven by various mechanisms, only one of which is captured by the sex ratio. Here I discuss such mechanisms. Some channels are related to the marriage market, such as changes in the quality of available grooms due to selective killings and a decrease in the opportunity cost of marriage. Other channels are not related to the marriage market and include psychological trauma and social disorder created by the genocide.

### **6.2.1. Alternative Mechanisms Related to the Marriage Market**

Alternative channels related to the marriage market include non-random mortality during the genocide and a decrease in the opportunity cost of marriage. Non-random mortality might have affected the quality of men available for marriage. Selection in victimization might have occurred along both observable and unobservable characteristics that could be correlated with intimate partner violence. For example, de Walque and Verwimp (2010) find that men with an educated background were more likely to be killed during the genocide.

I examine whether women who married after the genocide in communes with high genocide intensity were more likely to marry men with certain observable characteristics relative to women who married before the genocide and women who live in localities with low genocide intensity. The results are reported in Table 8. In columns 1 through 4, the coefficient on the interaction term between genocide intensity and marrying after the genocide is negative, indicating that higher genocide intensity is associated with “lower quality” husbands in terms of observable characteristics. However, the coefficient is small in magnitude and not statistically significant. Overall, the data provide no evidence that husbands differed in their educational attainment, occupation, age or attitude towards domestic violence. These results do not support the hypothesis that selection in the killings based on observable characteristics is behind the main results in this paper. Nonetheless, it is plausible that selection based on unobservable characteristics explains part of the differential effect of genocide on domestic violence for women who married after the conflict.



The genocide may have affected the value that women attach to being married. The idea behind this argument is quite simple, and is also drawn from Becker's (1973, 1981) seminal theories of marriage. Individuals compare their output when single to their output when married, and they will only marry if their expected output from being married is larger than their expected output from being single (the opportunity cost of marriage). The genocide may have decreased the opportunity cost of marriage in several ways. For instance, sexual violence was widespread during the genocide: an estimated 250,000 women were raped and some women were held captive by militias or forced into marriage. As a result, women might demand more physical protection.

Similarly, women whose parents or siblings died might be in need of economic support in the aftermath of the war. Poverty was exacerbated by customary rules preventing women from owning land, which could also contribute to the decreased opportunity cost of marriage. Therefore, after the genocide, women might enter marriages with a lower expected output than they would have before the genocide, leading to a decrease in the well-being of women within the household. While I don't provide direct evidence for this mechanism in this paper, reports from various sources are consistent with this channel. For example, the Human Rights Watch report "Shattered Lives" (1996) provides anecdotal evidence that women's opportunity cost of marriage decreased after the genocide:

"In Rwandan society, where women are valued primarily for their role as wives and mothers, the issue of marriageability is extremely important. Further, for many women, marriage is their best option to obtain economic security and some protection. 'It's always sad to see a girl who survived [the genocide],' said Jeanne, herself a rape survivor. 'They have no future. Some marry without really wanting to, because they are all alone. They can't farm their parents' land—they need a husband to help work the fields.'" (p. 43)

### **6.2.2. Alternative Mechanisms Outside of the Marriage Market**

Alternative channels that are not related to the marriage market include social disorder created by the genocide and psychological trauma due to participation or victimization during the killings.

The genocide might have increased men's propensity to perpetrate intimate partner violence against their partners through a decrease in the expected penalties for crime and violence due to a lack of security and social order (World Bank 2011). On the other hand, the genocide could have

spurred a trauma-related increase in violent behavior. A few studies in economics have examined how exposure to conflict and combat affects violent behavior. Rohlfs (2010) exploits variation in exposure to the Vietnam War across cohorts to show that combat exposure increased self-reported criminal and violent behavior among African American men. Miguel et al. (2011) find that the extent of civil conflict in a soccer player's country of origin is associated with more violent behavior on the soccer field, as measured by yellow and red cards. Cesur and Sabia (2013) estimate that exposure to combat increases domestic violence, and that part of the effect can be explained by increased stress. Outside economics literature, the evidence is mixed. Gupta et al. (2010) find a positive correlation between married men's experience of human rights violations during the apartheid in South Africa and the perpetration of physical violence against their female partners. On the contrary, Annan et al. (2011) find no relationship between child abduction and aggressive behavior among former abductees in Northern Uganda.

The lack of a baseline survey prevents me from estimating the impact of these channels on intimate partner violence for women across the board. Given the limitations imposed by the data, I examine whether these channels could explain the main results of this paper (the differential effect of genocide on women who married after the genocide relative to women who married before). In principle, I expect these mechanisms to increase the propensity to commit intimate partner violence in the same way for men who married after the genocide and for men who married before. As such, these mechanisms cannot explain the results of the difference-in-differences estimation. In fact, the cross-sectional relationship between genocide intensity and intimate partner violence for women who married before the genocide is negative (albeit not statistically significant).<sup>48</sup> Furthermore, I use a retrospective panel to examine whether the genocide increased the probability of spousal violence in marriages that formed before the outset of conflict. The retrospective panel analysis, which is described in the Appendix, provides no evidence that the genocide increased the risk of spousal violence among surviving marriages that formed before the outset of the conflict (Appendix, section B).

Neither the cross-sectional analysis nor the retrospective panel analysis is immune from criticism. The cross-sectional analysis could suffer from omitted variable bias. The retrospective panel analysis assumes that women correctly recall the year in which the first episode of spousal

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<sup>48</sup> These results are available upon request.

violence occurred. In fact, some memory bias could occur (Gibson and Kim 2005) and it would be a concern if that bias varied with genocide intensity. For these reasons, I provide two additional tests for the conjecture that the genocide increased intimate partner violence through social disorder or psychological trauma.

If a breakdown of social justice and moral conduct were behind the results of the difference-in-differences analysis, I would expect to observe an effect on other types of violent behavior besides intimate partner violence. To test this hypothesis, I estimate equation (4) using violence perpetrated by other family members (columns 1 and 2) and by individuals outside the family (column 3) as the dependent variable. The results are reported in Table 9. The point estimate for the effect of genocide on violence committed by other family members, in-laws and individuals outside the family is small, negative and not statistically significant. The results are similar when I consider violence committed by any individual other than one's partner as the dependent variable (column 4). In sum, the results shown in Table 9 do not support the hypothesis that an increase in violent behavior in Rwandan society as a whole is behind the main results of the paper.

As noted above, I would expect all men in this period to be vulnerable to trauma and suffer from post-traumatic stress disorder. Nonetheless, there could be a concern that younger men were more vulnerable to trauma because they were more likely to be victimized or involved in the killings. If this was the case, a trauma-driven increase in men's propensity to perpetrate domestic violence might explain the difference-in-differences results; women who became married after the genocide married younger men, as compared to women who married before the outbreak of the conflict.<sup>49</sup> In column 5 of Table 9, I test whether the effect of the genocide on domestic violence is heterogeneous across husband's age. If anything, the results show that conditional on marrying after the genocide, women who married older men are more likely to be victims of intimate partner violence. Consequently, these results do not support the hypothesis

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<sup>49</sup> In fact, the evidence on the age profile of genocide perpetrators does not support the hypothesis that young men were more likely to participate in the genocide compared to middle-aged men. Verwimp (2005) studies the characteristics of genocide perpetrators using a representative sample and finds that participation in the genocide increased with age and peaked around age 38. Straus (2006) interviews imprisoned genocide perpetrators and compares their characteristics to the population averages from the 1991 Census. He finds that the majority of perpetrators were aged 30 to 39.

that an increase in violent behavior in the society as a whole is behind the main results of the paper.

## **7. Conclusion**

This paper examines the long-term effects of civil conflict on intimate partner violence using data from post-genocide Rwanda. I combine recently released data on genocide perpetrators and genocide survivors with a household survey and census data. I document that the genocide increased intimate partner violence for women who became married after the genocide relative to women who became married before, and the effect is greater for women who live in localities with high genocide intensity. These results shed light on the legacies of the genocide on women's well-being. After the genocide, the Rwandan government implemented various reforms aimed at improving women's property rights and increasing their political representation. However, I show that the genocide had long-lived negative effects on women's well-being, and these effects were still sizeable 11 years after the end of the killings.

In investigating potential mechanisms for this finding, I show that about one-third of the increase in intimate partner violence can be attributed to the sharp decline in the sex ratio following the genocide. Recent research on the consequences of gender imbalances on intra-household allocation has focused on countries that face a shortage of women, such as China and India (Porter 2007; Francis 2011; Edlund et al. 2013). While the genocide was specific to Rwanda, these results could provide insights on the determinants of women's well-being in countries that experience a similar shortage of men—for example, regions with low sex ratio due to high emigration of working age males.<sup>50</sup>

I show that the increase in intimate partner violence is not driven by psychological trauma or social disorders. Rather, at least part the effect seems to operate through marriage market conditions at the time of marriage, as measured by the sex ratio. Additionally, a stigma against female genocide survivors, the need for physical protection and increased economic insecurity could have led to a decrease in the opportunity cost of marriage for women. This mechanism might account for the differential increased in intimate partner violence that is not explained by changes in the sex ratio. The overall findings in this paper suggest that policies that aim to

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<sup>50</sup> In Mexico, where there are large gender differences in the probability to emigrate, the sex ratio for the age group including working age males decreased from 96 in 1970 to 89 in 2000 (Chiapa and Vlejo 2012).

increase autonomy, protection and decision-making power of women in recently married households may help to reduce intimate partner violence in post-conflict societies with low sex ratio. On the other hand, programs that provide counselling for men and programs that facilitate the reintegration of those who were involved in the killings may be less effective in reducing spousal violence in similar settings.

There are a number of caveats to the results presented in this paper. The results are based on a sample of women who survived the genocide and did not leave the country after the conflict; this sample includes women who became married after the genocide and women who became married before the genocide and whose marriage was still intact 11 years after the genocide. The implications of this study may not extend to women who migrated abroad and to women who became widows or remarried after the end of the conflict. Finally, these results provide evidence of an increase in intimate partner violence for women who married after the genocide relative to women who married before. It would have been useful to have pre-genocide data on intimate partner violence to estimate the impact of civil conflict on the whole population of women. Still, I think that this study is an important contribution looking at the long-run effects of civil conflicts through changes in marriage market conditions.

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## Tables

**Table 1 Summary Statistics**

Variable	Mean	Standard deviation
Suspected perpetrators (organized the killings) <sup>†</sup>	0.010	0.008
Suspected perpetrators (killed) <sup>†</sup>	0.058	0.038
Suspected perpetrators (looted) <sup>†</sup>	0.043	0.030
Survivors: widows and widowers <sup>†</sup>	0.004	0.004
Survivors: orphans <sup>†</sup>	0.011	0.009
Survivors: disabled <sup>†</sup>	0.002	0.002
Genocide index (standardized) <sup>†</sup>	-0.003	1.010
Literacy rate, 1991 <sup>‡</sup>	0.511	0.072
Population density, 1991 <sup>‡</sup>	447.176	389.584
Literacy rate, 2002 <sup>‡</sup>	0.561	0.073
Population density, 2002 <sup>‡</sup>	506.812	841.010
Sex ratio (Men age 20-54 : Women age 15-49), 1991 <sup>‡</sup>	0.769	0.082
Sex ratio (Men age 20-54 : Women age 15-49), 2002 <sup>‡</sup>	0.620	0.099
Observations*	140	

*Notes:* <sup>†</sup> Source: records of the gacaca courts. <sup>‡</sup> Source: 1991 and 2002 Census of Rwandan population. \*Before 2002 Rwanda was divided in 145 communes, but the 2005 DHS was conducted in 140 communes.

**Table 2 Summary Statistics**

Variable	Mean	Standard deviation	Number of observations
<i>Variables from the DHS Domestic Violence Module</i>			
Parent Violence (yes=1,no=0)	0.325	0.469	2302
Physical violence in last 12 months (yes=1,no=0)	0.182	0.386	2037
Sexual violence in last 12 months (yes=1,no=0)	0.095	0.293	2082
Ever physical violence (yes=1,no=0)	0.341	0.474	2130
Seeking help (yes=1,no=0)	0.457	0.498	781
Seeking help from other than family (yes=1,no=0)	0.356	0.479	781
Seeking help from people other than family conditional on seeking help (yes=1,no=0)	0.780	0.415	342
Ever hurt by family member(yes=1,no=0)	0.022	0.148	2391
Ever hurt by in-laws (yes=1,no=0)	0.005	0.070	2391
Ever hurt by other than family (yes=1,no=0)	0.033	0.179	2391
<i>Variables from the core DHS questionnaire</i>			
Woman has no say on own health care (yes=1,no=0)	0.371	0.483	5083
Woman has no say on making large household purchases (yes=1,no=0)	0.392	0.488	5093
Woman has no say on making household purchases for daily needs (yes=1,no=0)	0.302	0.459	5097
Woman has no say on visits to family or relatives (yes=1,no=0)	0.230	0.421	5061
Woman has no say on food to be cooked each day (yes=1,no=0)	0.091	0.287	5094
Husband's years of education	4.259	3.680	4970
Husband has higher educational attainment than wife (yes=1,no=0)	0.336	0.472	5100
Husband's occupation is non agriculture (yes=1,no=0)	0.242	0.428	4991
Age difference between husband and wife	4.977	5.916	4679
<i>Variables from the men's interview</i>			
Husband thinks that domestic violence is justified (yes=1,no=0)	0.339	0.769	1721

Notes: Data from the 2005 DHS.

**Table 3 Difference-in-differences Analysis: Main Result**

Dependent variable	(1) Parent violence	(2)	(3)	(4)	(5)	(6)
		Physical violence in the last 12 months				
Genocide intensity× Married after genocide	0.0089 (0.023)	0.0497** (0.022)	0.0510** (0.024)	0.0563** (0.025)	0.0550** (0.025)	0.0501* (0.029)
Observations	2,742	1,987	1,987	1,987	1,987	1,987
R-squared	0.0010	0.0207	0.1334	0.1365	0.1444	0.1535
Individual Controls	NO	YES	YES	YES	YES	YES
Commune FE	NO	NO	YES	YES	YES	YES
Year of marriage FE	NO	NO	YES	YES	YES	YES
Commune Controls	YES	NO	NO	YES	YES	YES
Province specific linear trend	NO	NO	NO	NO	YES	NO
Province × time of marriage FE	NO	NO	NO	NO	NO	YES
Mean of dependent variable	0.332	0.190	0.190	0.190	0.190	0.190

*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. All regressions are estimated using an OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence. \*\*\* p<0.01, \*\* p<0.05 \* p<0.1. In column 1, widowed women and women who married more than once are included in the sample, which explains why the sample size is larger compared to the other columns in the table. Regressions in columns 1 and 2 control for the genocide index and a dummy for becoming married after the genocide. Individual controls in columns 2-6 include age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index. Commune controls are obtained from 1991 and 2002 Rwanda Census; they include population density and literacy rate.

**Table 4 Reduced Form Using Year of Birth and Two-Stage Least-Squares (2SLS)**

Dependent variable: Physical violence in the last 12 months	(1)	(2)
	Reduced Form	2SLS
Genocide intensity× Younger than 21 in 1994	0.0390* (0.021)	
Genocide intensity× Married after genocide		0.0506* (0.028)
First marriage after the end of genocide - July 1994		0.0241 (0.024)
Genocide intensity		-0.0314* (0.018)
Constant	0.1446*** (0.052)	0.1420*** (0.052)
Observations	1,987	1,987
R-squared	0.0132	0.0137
<u>First-stage statistics</u>		
F stat for (Genocide× Married After)		851
F stat for (Genocide× Married After) Critical Value		618.8
F stat (Married After)		253.9
F stat (Married After) Critical Value		1119

*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. All regressions are estimated using an OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence. \*\*\* p<0.01, \*\* p<0.05 \* p<0.1. All regressions include religion, education, partner's education, urban status and a wealth index. Regression in column (2) is estimated using a Two-Stage Least-Squares model. In column (2) "Younger than 21 in 1994" is used as instrumental variable for "Married after genocide"; the interaction of "Genocide index" and "Younger than 21 in 1994" is used as an instrumental variable for the interaction of "Genocide index" and "Married after genocide". The results of the first-stage analysis are available upon request.

**Table 5 Background Characteristics of Married Women**

Dependent Variable	(1)	(2)	(3)	(4)
	Years of education		Height (cm)	
Genocide intensity× Married after genocide	0.0994**	0.1603*	-3.6592	-5.0813
	(0.045)	(0.082)	(3.666)	(6.162)
Sample	DHS	DV	DHS	DV
Observations	5,745	2,075	2,840	2,076
R-squared	0.8058	0.8038	0.0893	0.1789
Mean of dependent variable	3.793	3.796	166.3	165.8

*Notes:* Data from domestic violence module and the core questionnaire of the Rwanda 2005 DHS. All regressions are estimated using OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05 \* p<0.1 All regressions include commune fixed effects, year of marriage fixed effects, commune literacy rate, commune population density, age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index. The DHS sample includes women who received the core DHS questionnaire. The DV sample is a subset of the DHS sample and it includes women who received the domestic violence module.

**Table 6 Sex ratio, Genocide and Spousal Violence**

Dependent variable	(1)	(2)	(3)	(4)
	Physical violence in the last 12 months			
Sex Ratio	-0.2951		-0.1238	
	(0.310)		(0.318)	
Sex Ratio, exclude individuals in prison		-1.0197***		-0.8341**
		(0.298)		(0.342)
Genocide intensity× Married after genocide			0.0536**	0.0329
			(0.025)	(0.026)
Observations	1,987	1,987	1,987	1,987
R-squared	0.1354	0.1402	0.1382	0.1411

*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. All regressions are estimated using an OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05 \* p<0.1. All regressions include commune fixed effects, year of marriage fixed effects, commune literacy rate, commune population density, age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index. The sex ratio is defined according to equation (2).

<b>Table 7 Sex ratio, Genocide and Women's Decision-making Power within the Household</b>					
Dependent variable	(1)	(2)	(3)	(4)	(5)
	her own health	Woman has no say on: large purchases	daily purchases	family visits	food
<b>Panel A Effect of Genocide</b>					
Genocide intensity× Married after genocide	0.0435** (0.017)	0.0362** (0.017)	0.0334** (0.015)	0.0259* (0.014)	0.0095 (0.011)
R-squared	0.1500	0.1244	0.1239	0.1242	0.0962
<b>Panel B Effect of Sex Ratio</b>					
	(1)	(2)	(3)	(4)	(5)
Sex Ratio, exclude people in prison	-0.1628 (0.200)	-0.1002 (0.221)	-0.0435 (0.210)	-0.1439 (0.183)	-0.2186* (0.115)
R-squared	0.1486	0.1234	0.1230	0.1237	0.0966
<b>Panel C Effect of Genocide and Sex Ratio</b>					
	(1)	(2)	(3)	(4)	(5)
Genocide intensity× Married after genocide	0.0428** (0.017)	0.0363** (0.017)	0.0345** (0.015)	0.0246* (0.014)	0.0057 (0.011)
Sex Ratio, exclude people in prison	-0.0360 (0.201)	0.0074 (0.221)	0.0594 (0.208)	-0.0708 (0.177)	-0.2015* (0.121)
Observations	4,590	4,598	4,602	4,575	4,599
R-squared	0.1500	0.1244	0.1239	0.1242	0.0967
Mean of dependent variable	0.403	0.426	0.328	0.251	0.102

*Notes:* Data from the core questionnaire of the Rwanda 2005 DHS. All regressions are estimated using an OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05 \* p<0.1 All regressions include commune fixed effects, year of marriage fixed effects, age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index.



**Table 8 Husband's Characteristics**

Dependent variable	(1) Husband's years of education	(2) Husband has more education	(3) Husband has non- agricultural occupation	(4) Age gap between spouses	(5) Husband thinks domestic violence is justified
Genocide intensity× Married after genocide	-0.0039 (0.098)	-0.0035 (0.012)	-0.0095 (0.013)	-0.2014 (0.183)	0.0074 (0.049)
Observations	5,658	5,857	5,737	4,647	1,706
R-squared	0.3107	0.1837	0.2985	0.1080	0.1545
Mean of dependent variable	4.270	0.340	0.270	5.030	0.342

*Notes:* Data from the core questionnaire of the Rwanda 2005 DHS. All regressions are estimated using an OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05 \* p<0.1 All regressions include commune fixed effects, year of marriage fixed effects, age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index.

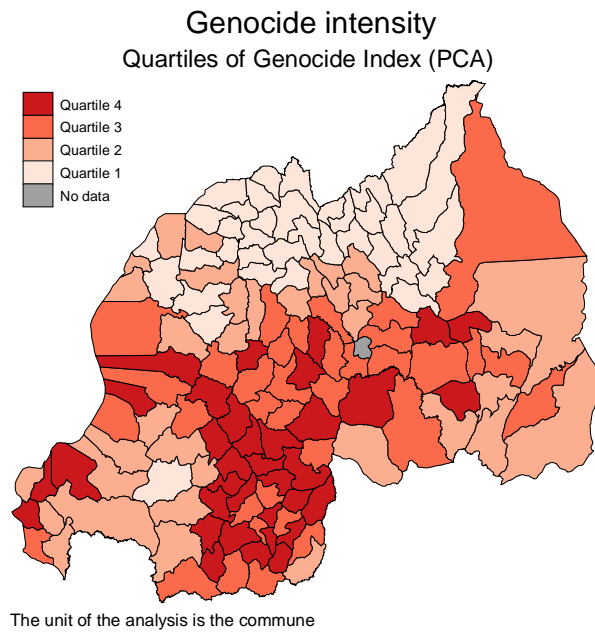
**Table 9 Ever a Victim of Violence by Individuals Other than Partner**

Dependent variable	(1) Ever hurt by family member	(2) Ever hurt by in-laws	(3) Ever hurt by other than family	(4) Ever hurt by other than partner	(5) Physical violence in last 12 months
Genocide intensity× Married after genocide	0.0004 (0.008)	-0.0039 (0.003)	-0.0065 (0.008)	-0.0110 (0.012)	0.0267 (0.075)
Husband's Age					-0.0027 (0.002)
Genocide intensity× Married after genocide × Husband's age					0.0009 (0.002)
Observations	2,325	2,325	2,325	2,325	1,881
R-squared	0.1089	0.0759	0.1591	0.1418	0.1403
Mean of dependent variable	0.0206	0.00645	0.0280	0.0529	0.0529

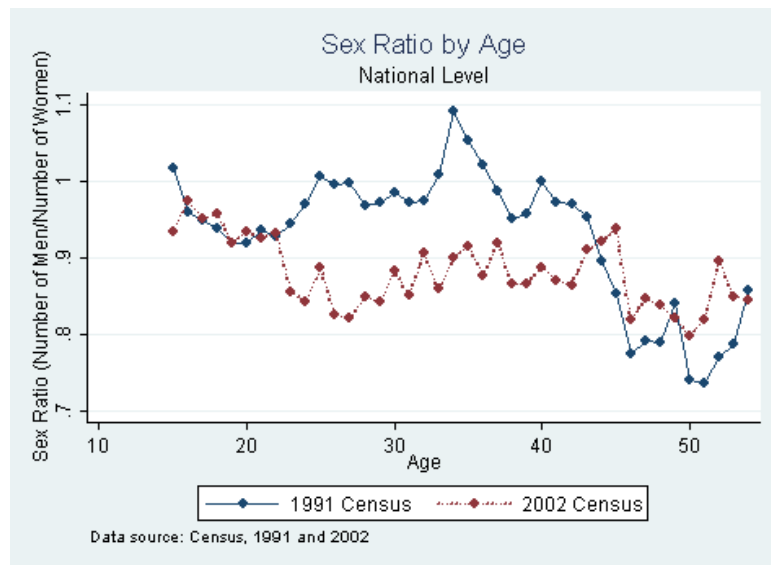
*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. Widowed women and women who married more than once are included in the sample. All regressions are estimated using an OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05 \* p<0.1. All regressions include commune fixed effects, year of marriage fixed effects, commune literacy rate, commune population density, age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index.

# Figures

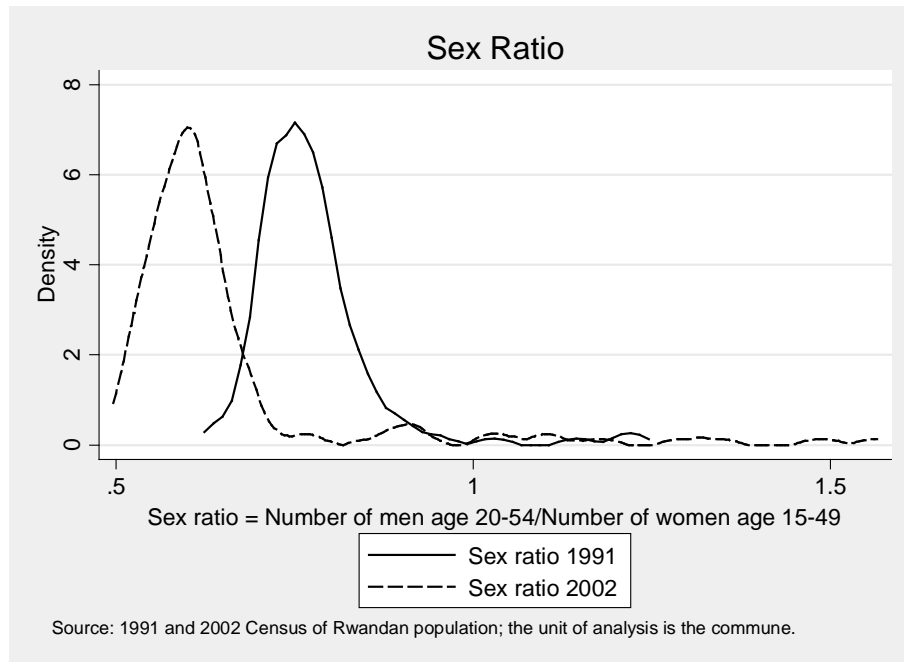
## Figure 1



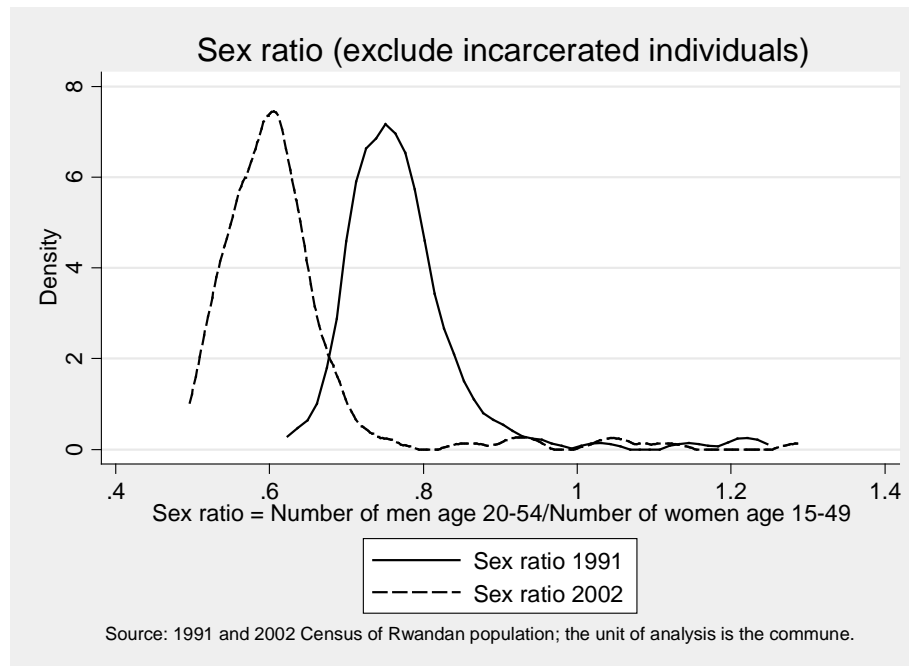
## Figure 2



**Figure 3**



**Figure 4**



## Appendix

### A. Robustness

#### I. Controls for Age

The empirical strategy of this research relies on comparing intimate partner violence outcomes in women who married at different points in time. Women who married before the genocide and women who married after the end of the killings could be different in several dimensions. For instance, women who married before the genocide tended to be older. The baseline specification controls for women's age using dummies that measure age bins of 5 years. For robustness, I include dummies that measure age using bins of 10 or 15 years (born in 1955-1964, born in 1965-1974, born in 1975-1990), or year-of-birth fixed effects. Additionally, I estimate the difference-in-differences regression, restricting the analysis to women within the common support of the age variable.<sup>51</sup> Panel A of Table A2 shows that the results are robust to alternative specifications of the age variable (Columns 1 and 2), and to the exclusion of observations that are not in the common support group (Column 3). This helps to relieve concerns that the difference-in-differences estimates may capture differences in unobservable characteristics between younger and older women rather than the effect of the genocide.

#### II. Migration

Columns 4 and 5 present the estimate for the effect of genocide intensity on intimate partner violence when I exclude households and women who did not migrate from the sample. In column 4, I restrict the sample to “non-migrant households,” defined as households where the woman (*or* her husband) is still living in her or his province of birth in 2005. In column 5, I restrict the sample further to “non-migrant women,” defined as women who are still living in their province of birth in 2005. The estimated coefficient is larger in magnitude when I restrict

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<sup>51</sup> Specifically, among women who married before the genocide, I exclude those who are older than the oldest woman who married after the genocide; among women who married after the genocide, I excluded those who are younger than the youngest woman who married before the genocide. The common support group includes women aged 24 to 45 in 2005.

the sample to non-migrants, suggesting that measurement error attenuates the effect of the genocide on spousal violence for households and women who migrated.

### III. Alternative Specifications of the Dependent Variable

The baseline specification employs an indicator for whether a woman was the victim of physical violence by her partner in the last 12 months as dependent variable. Other measures of intimate partner violence are available in the DHS data—for example, whether the woman was the victim of *sexual* violence by her current partner in the last 12 months or whether a woman has ever been a victim of intimate partner violence. Panel B of Table A2 presents estimates for the effect of genocide in intimate partner violence using these alternative measures of intimate partner violence. There is no evidence that the genocide affected sexual violence for women who married after the genocide (column 1). Similar results are obtained when I restrict the sample to non-migrant women or households (columns 2 and 3). The results are robust to using “Ever a victim of physical violence” as dependent variable, although the point estimate is not always statistically significant (columns 4 and 5). The estimated coefficient is large in magnitude and statistically significant at the 1% level when I restrict the sample to non-migrant women (column 6).<sup>52</sup>

### IV. Misreporting

Self-reported data on intimate partner violence are often subject to criticism of misreporting.<sup>53</sup> In this paper, misreporting would be an issue if it were more or less likely to

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<sup>52</sup> “Victim of physical violence in the past 12 months” is preferable to “Ever a victim of physical violence” because it minimizes recall bias due to memory loss. In addition, for women who married before the genocide, “Ever a victim of physical violence” may take value 1 for episodes of domestic violence that happened before the genocide. This is undesirable.

<sup>53</sup> The DHS domestic violence module minimizes misreporting. More precisely, it uses a modified Conflict Tactics Scale (CTS) to measure spousal violence. Kishor (2005) describes in detail the CTS approach and the modified CTS included in the DHS module. The original CTS developed by Straus (1979, 1990) consists of a series of individual questions regarding specific acts of violence such as slapping, punching and kicking. If the respondent affirms that any one of the specified acts or outcomes has taken place, she is considered to have experienced violence. The CTS approach presents several features that are aimed at minimizing underreporting of violence. First, by asking separately about specific acts of violence, the violence measure is not affected by different understandings between women

happen in localities where the genocide was more intense. For example, younger women who lived in localities where the genocide was more intense could be more likely to underreport domestic violence relative to older women and women who live in localities where the genocide was less intense. If this were the case, then the estimated coefficient on the interaction term between genocide intensity and marrying after the genocide would be biased downward. Therefore, underreporting is not a major concern. On the other hand, over-reporting in localities with higher genocide intensity may be more worrisome. For instance, younger women in localities where the genocide was more intense could have been exposed to public health programs that increased women's awareness of gender-based violence. If this were the case, then the estimated coefficient on the interaction term between genocide intensity and marrying after the genocide would be biased upward.

I use information contained in the DHS to test for misreporting. I use three different proxies for misreporting behavior: nonresponse, seeking help after being the victim of intimate partner violence and attitudes towards domestic violence. I expect these variables to be correlated with misreporting in the following way: if women are more likely to report intimate partner violence to family, friends, acquaintances or the police then they should also be more likely to report it to the interviewer (Chin 2013). In other words, seeking help after intimate partner violence could be correlated with over-reporting. On the contrary, underreporting behavior should be positively correlated with nonresponse. Finally, if women are more aware of gender-based violence issues and are more likely to report intimate partner violence, I might also expect them to report less that domestic violence is justified (Heath 2012).

The three different proxies for misreporting are used as dependent variables in the difference-in-differences strategy described in equation (1). The results are presented in Panel C of Table A2. Columns 1 to 3 report the results for seeking help after becoming victim of intimate

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of what constitutes violence. Second, the CTS approach gives respondents multiple opportunities to disclose their experiences of violence. The original scale developed by Straus had 19 items (Straus 1979, 1990) and did not include sexual violence. The modified list used by DHS includes only about 15 acts of physical and sexual violence.

partner violence. Unfortunately, the small sample size does not allow for the drawing of conclusive evidence. If anything, women who married after the genocide in localities where the genocide was more intense are less likely to seek help after being the victim of intimate partner violence. Column 4 shows that the genocide did not affect nonresponse rate; the coefficient is small in magnitude and has a negative sign. Column 5 presents the results for women's attitudes toward intimate partner violence. There is no evidence that women who married after the genocide and live in high-genocide communes are less likely to believe that domestic violence is justified. The interaction term between genocide intensity and marrying after the genocide is small and statistically insignificant. Overall, the results provide no evidence that the genocide increased misreporting for women who married after the genocide compared to women who married before.

## **B. Retrospective Panel Analysis**

The 2005 DHS contains self-reported information on the timing of the first episode of intimate partner violence, which I use to impute a woman's history of domestic violence using two different methods. In the first imputation method, I assume that spousal violence occurred every year after the year of the first episode. In the second imputation method, I assume that domestic violence occurred every year after the first episode only if it also occurred in the last 12 months. As a result of the data imputation, I obtain an unbalanced retrospective panel. A woman enters the data set when she becomes married; then she is observed every year until the year of the survey (2005). I examine the impact of the genocide on the probability of experiencing spousal violence by estimating the following equation:

$$Y_{ict} = \beta_0 + \beta_1 Post_t + \beta_2 Genocide_c \cdot Post_t + X_{it} \delta + \alpha_i + \varepsilon_{ic} \quad (4)$$

In this specification, the dependent variable  $Y_{ict}$  denotes whether woman  $i$  in commune  $c$  experienced spousal violence in calendar year  $t$ .  $Post_t$  is an indicator that takes value 1 for each calendar year after 1994.  $X_{it}$  is a vector of time-varying individual characteristics, such as age and marital duration.  $\alpha_i$  represents an individual fixed effect. It accounts for unobserved individual heterogeneity that makes an individual more (or less) vulnerable to spousal violence.

The coefficient of interest is  $\beta_2$ , which captures the mean effect on spousal violence of being exposed to the genocide. This sample includes only surviving women who married before the outbreak of the genocide and whose marriage is still intact in 2005.

Table A5 reports the results of the retrospective panel analysis. In columns 1, 2 and 3, I define the dependent variable using the first imputation method. In columns 4, 5 and 6, I define the dependent variable using the second method of imputation. The coefficient estimate on the interaction term ( $\beta_2$ ) is small and statistically insignificant in all the specifications. The results are robust including commune-fixed effects and women-fixed effects.



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**Table A1 Two-step Heckman Selection Model**

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Dependent Variable: Physical violence in the last 12 months		(1)
<u>Selection equation</u>		
Number of younger sisters		0.0801*** (0.018)
Genocide index× Younger than 21 in 1994		-0.0280 (0.064)
Genocide index		-0.0861 (0.053)
Younger than 21 in 1994		-0.1643 (0.204)
<u>Two-step estimator</u>		
Genocide index× Younger than 21 in 1994		0.0435* (0.024)
Genocide index		-0.0209 (0.017)
Younger than 21 in 1994		-0.0122 (0.062)
Observations		2,422
Lambda		0.0458
Lambda std. error		0.156

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*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. Never married women are included in the sample. Robust standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05 \* p<0.1. All regressions include age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index.

<b>Table A2 Difference-in-differences Analysis: Robustness Checks</b>						
<b>Panel A Age controls, migration</b>						
Dependent variable	(1)	(2)	(3)	(4)	(5)	
	Physical violence in the last 12 months					
Genocide intensity× Married after genocide	0.0499** (0.022)	0.0513** (0.021)	0.0463* (0.024)	0.0648*** (0.025)	0.0687* (0.036)	
Observations	1,987	1,987	1,547	1,436	827	
R-squared	0.0184	0.0387	0.0244	0.0237	0.0265	
Age dummies or sample	10 years	Year of birth	Common support	5 years	5 years	
Mean of dependent variable	0.190	0.190	0.190	0.190	0.190	
Migration sample	All women	All women	All women	Non-migrant	Non-migrant	
<b>Panel B Other Dependent Variables</b>						
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
	Sexual violence in the last 12 months			Ever a victim of physical violence		
Genocide intensity× Married after genocide	-0.0059 (0.016)	-0.0038 (0.021)	-0.0057 (0.030)	0.0209 (0.027)	0.0436 (0.031)	0.0821** (0.036)
Observations	2,030	1,465	851	2,076	1,492	876
R-squared	0.0147	0.0170	0.0267	0.0442	0.0503	0.0476
Migration sample	All women	Non-migrant	Non-migrant	All women	Non-migrant	Non-migrant
Mean of dependent variable	0.0975	0.101	0.102	0.336	0.348	0.398
<b>Panel C Reporting and Attitudes</b>						
Dependent variable	(1)	(2)	(3)	(4)	(5)	
	Seeking help	Seeking help	Seeking help	Nonresponse	Wife-beating	
Genocide intensity× Married after genocide	-0.0677 (0.052)	-0.0809 (0.064)	-0.0196 (0.079)	-0.0093 (0.025)	0.0140 (0.016)	
Observations	764	764	335	2,325	5,748	
R-squared	0.3559	0.3118	0.5308	0.2536	0.1269	
Mean of dependent variable	0.438	0.342	0.779	0.107	0.465	

*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. All regressions are estimated using an OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05 \* p<0.1. All regressions include age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index. Regressions in Panel A and Panel B include the genocide index and a dummy for becoming married after the genocide. Regressions in Panel C also include commune fixed effects, year of marriage fixed effects, commune literacy rate and population density.

**Table A3 Probit Regression**

Dependent variable	Married after genocide
Protestant	0.0331 (0.233)
Catholic	0.1140 (0.233)
Number of siblings	-0.0071 (0.016)
Grew up in a city	0.7167*** (0.233)
Grew up in a city is missing	-0.0220 (0.088)
Observations	1,639
Year of birth dummies	YES

*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. The regression is estimated using a probit model. Standard errors in parenthesis. \*\*\* p<0.01, \*\* p<0.05 \* p<0.1. The regression includes dummies for year of birth.

**Table A4 Sex ratio, Conflict and Spousal Violence: Alternative Measure of Sex Ratio**

Dependent variable	(1)	(2)	(3)	(4)
	Physical violence in the last 12 months			
Sex Ratio (discontinuous measure)	-0.2367 (0.203)		-0.1012 (0.204)	
Sex Ratio, exclude people in prison (discontinuous measure)		-0.7216*** (0.232)		-0.5606** (0.258)
Genocide intensity× Married after genocide			0.0527** (0.027)	0.0321 (0.027)
Observations	1,987	1,987	1,987	1,987
R-squared	0.1341	0.1378	0.1366	0.1386

*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. All regressions are estimated using an OLS model and weighted using survey weights. Standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05 \* p<0.1. All regressions include commune fixed effects, year of marriage fixed effects, commune literacy rate, commune population density, age dummies, religion, age at marriage, education, partner's education, urban status and a wealth index. The sex ratio is defined as sex ratio = I(year of marriage>1994)\*Sex ratio<sub>2002</sub>+ [1- I(year of marriage>1994)]\*Sex ratio<sub>1991</sub>.

<b>Table A5 Retrospective Panel Analysis</b>						
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
	Domestic violence: Imputation method 1			Domestic violence: Imputation method 2		
Genocide index× Calendar year after 1994	0.0054	-0.0025	-0.0049	0.0019	-0.0026	-0.0078
	(0.009)	(0.008)	(0.013)	(0.007)	(0.006)	(0.012)
Genocide index	-0.0051			0.0008		
	(0.006)			(0.005)		
Calendar year after 1994	0.0286**			0.0320***		
	(0.014)			(0.011)		
Observations	16,649	16,649	16,649	16,463	16,463	16,463
R-squared	0.0778	0.2611	0.2028	0.0331	0.1935	0.0575
Individual controls	YES	YES	YES	YES	YES	YES
Commune FE	NO	YES	NO	NO	YES	NO
Woman FE	NO	NO	YES	NO	NO	YES
Calendar year FE	NO	YES	YES	NO	YES	YES
Mean of dependent variable	0.311	0.311	0.311	0.160	0.160	0.160
Number of women			846			846

*Notes:* Data from the domestic violence module of the Rwanda 2005 DHS. One observation is a woman in a calendar year. All regressions are estimated using an OLS model and weighted using survey weights. Robust standard errors in parenthesis are clustered by commune of residence \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Individual controls include age dummies, religion, years since marriage entered as a cubic, age at marriage, education, partner's education, urban status and a wealth index. Imputation method 1 uses an imputed measure of domestic violence that takes value one when the first episode of domestic violence occurs and in all the following years. Imputation method 2 uses an imputed measure of domestic violence that takes value 1 when the first year of domestic violence occurs; it takes value 1 in all the subsequent years only if domestic violence also occurred in the last 12 months before the survey.