H i C N Households in Conflict Network

The Institute of Development Studies - at the University of Sussex - Falmer - Brighton - BN1 9RE
www.hicn.org

The Impact of Conflict on Education Attainment and Enrollment in Colombia: lessons from recent IDPs[1]

Ruth Uwaifo Oyelere[2] and Kate Wharton[3]

HiCN Working Paper 141

April 2013

Abstract: Forty years of low-intensity internal armed conflict have made Colombia home to over 3 million Internally Displaced Persons (IDPs), the world's largest population. The effect of violence on a child's education is of particular concern because of the critical role that education plays in increasing human capital and productivity. This paper explores the education accumulation and enrollment gaps created by being directly affected by conflict. We proxy for this direct impact by focusing on IDPs. First, we show that measuring the impact of conflict on children using levels of conflict at the municipal level underestimates the education enrollment and accumulation gaps. We subsequently estimate the education accumulation and enrollment gaps for IDPs in comparison to non-migrants and other migrants using various econometric techniques. Our results suggest a significant education accumulation gap for children of IDPs compared to non-migrants that widens to approximately half a year at the secondary level. We find no evidence of enrollment gaps at the primary level when appropriate controls are included, but we do find a lower probability of enrollment at the secondary level. The disparity in effects when we focus on direct exposure to conflict versus a dummy that captures living in a municipality with high conflict suggests the need to be careful when using the latter to estimate the impact of conflict.

JEL Classification: I24, O12, O15, J10

Key words: Education Attainment, School Enrollment, Colombia, Internal Displacement, Conflict

---

[1] The authors would like to thank Ron Oaxaca, Gautam Gowrisankaran, Jenny Aker, and Jeremy Magruder for insightful comments. We are also grateful to seminar participants at the University of California at Berkeley, University of Arizona, University of Illinois at Urbana Champaign, University of Southern Florida, and conference participants at the Fifth Southeastern International Development Economics Workshop at the Federal Reserve Bank of Atlanta and the Southern Economic Association(SEA). We thank Fabio S´anchez for sharing the Colombia CEDE conflict data with us. Of course all errors and omissions are ours. Data for this paper was derived primarily from IPUMS International. Comments are appreciated.

[2] Corresponding author. ruth.uwaifo@econ.gatech.edu, School of Economics, Georgia Institute of Technology, 221 Bobby Dodd way, Atlanta, GA 30332

[3] kate.owens.wharton@gmail.com,Georgia Institute of Technology, 221 Bobby Dodd way, Atlanta, GA 30332.

1 Introduction

For over forty years, Colombia has been troubled by armed conflict. The primary aggressor, the Revolutionary Armed Forces of Colombia (FARC), emerged as a revolutionary Marxist organization in the 1960s in response to political exclusion of the rural poor. Later, right-wing paramilitaries formed formed to counter the leftist FARC. Both irregular armed groups have since come to rely on narcotrafficking and other illegal activities to finance the conflict and are criticized internationally for human rights abuses targeting civilians.[1] Although peace talks have occurred under nearly every president, attempts at negotiation and disarmament have failed to bring lasting peace.

One result of this disruptive, long-term conflict is the mass displacement of Colombians. Currently, Colombia has the most internally displaced persons (IDPs) of any country in the world, with 3.6 million registered since 1997 according to the UNHCR 2010 Global Trends report. Empirical evidence suggests that IDPs move as a direct result of fighting, land confiscation, massacre, fear of forced recruitment into the armed groups, death threats, death of family or community members, and other fear-inducing elements of conflict (Kirchoff and Ibáñez 2002). After moving, IDPs face obstacles to social and economic integration in receptor locations, including psychological trauma, reduced social capital, family fragmentation, difficulty finding employment, and loss of assets. For example, Kirchhoff and Ibáñez (2002) show that 83% of landowners in their study were forced to abandon their land without compensation. The particular challenges of forced displacement suggest that IDPs are a highly vulnerable group requiring special attention in order to successfully integrate into the larger community.

Although efforts have been made to protect IDPs and to assist them in resettlement, there is anecdotal evidence that IDPs are still vulnerable many years after migration.[2] One possible explanation for this is that government aid to IDPs is restricted to the first three months of displacement. Meanwhile, longer-term income generation programs have had only limited success in helping IDP families return to their previous economic status (Ibáñez and Moya 2009).

Education significantly improves an individual's chances to increase welfare and escape poverty and plays a critical role in socioeconomic development. Our goal in this paper is to show how being directly affected by conflict impacts education outcomes of school aged children in Colombia. In addition, we explore whether exposure to conflict and being directly affected by conflict similarly impact education outcomes.

[1] It should be noted that the Colombian Army has also been criticized for 'false positive' killings of civilians.

[2] The 1997 Law 387 dictates government policy concerning assistance to IDPs and establishes the Network of Social Solidarity (RSS) as the coordinating entity for the strategic plan for the management of internal forced displacement. However, government programs reach only a small portion of the registered IDP population, and UN agencies and other humanitarian organizations play an important role in assisting the displaced.

Our paper contributes to a growing body of literature including Akresh and de Walque (2008), Shemyakina (2011) , Chamarbagwala & Morán (2011) and Rodriguez & Sanchez, (2012), who examine the impact of conflict on education outcomes in Rwanda, Tajikistan, Guatemala and Colombia respectively. However, out paper differs in that we estimate the direct impact of conflict by focusing on people we know were affected by conflict (IDPs) rather than using exposure at the municipality or community level. Although in general, using municipality-level exposure may help avoid potential selection issues linked with evaluating the treatment directly, in the case of Colombia we show that exposure to conflict does not create similar effects as being directly affected, and potential selection bias is minimal.

To meet these objectives, we answer four related questions.

- First, are there education accumulation and enrollment gaps for children living in high conflict municipalities compared to those living in low-conflict municipalities?
- Second, does being directly affected by conflict affect education accumulation and enrollment for children?
- Third, does living in a high conflict municipality create a similar education gap as being directly affected by conflict?
- Finally, how do recent IDPs compare to other migrants in terms of education accumulation and enrollment?

Question 2 is our main question of interest, while the first and third questions help address whether exposure to conflict creates a similar education gap as being directly affected by conflict. The possible argument that non-migrants are not a good comparison group for IDPs motivates our last question.[3]

To answer these questions, we primarily make use of data from the Colombia 2005 Census. Data on conflict and municipal level capacity comes from the Office of the Coordination of Humanitarian Assistance OCHA in collaboration with the Universidad Santo Tomas. Data for our instruments come from the Colombia Coca Survey and the Center for Development Studies, (CEDE) part of the Faculty of Economics, Universidad de Los Andes. We address each question separately for children 6-11 years old and 12-17 years old, representing elementary school and secondary school ages, respectively. For the first question, we estimate the gap in attainment and enrollment between

[3] Although we are of the opinion that comparing IDPs with non-migrants provides results that are more clearly interpreted given the selectivity associated with migrants, it is possible to argue that migrants are a better comparison group for IDPs than non-migrants.

those who live in high conflict regions and those who do not. To answer the second question, we estimate the gap in education attainment between children of IDPs and children of non-migrants. To answer the third question, we restrict our sample to only those in high conflict municipalities and test for the persistence of the gap between IDPs children and non-migrants. For our last question, we restrict the sample to solely migrants and compare the education gap between IDPs and other migrants.

For questions pertaining to education accumulation, we estimate the gap in years of schooling using the ordinary least squares (OLS) estimator, controlling for factors that affect schooling. For questions related to enrollment, we estimate linear probability and probit models and compute marginal effects. We subsequently address potential omitted variable and selection bias in the estimate of our variable of interest using several econometric techniques such as fixed effect models, matching estimators, and instrumental variable (IV) analysis.

In response to the first question, we find a slight gap in education accumulation of about 0.036 years for those in municipalities with conflict above the mean and a gap in probability of enrollment of about 1% for the same group. However, if we cluster at the municipal level, most of these estimated impacts are insignificant. For our second question, we find that regardless of our method or estimation technique, children of IDPs who are between 6-11 years have about one-fifth fewer years of schooling in comparison to non-migrants, and children 11-17 years old have a gap of almost half a year. With respect to enrollment, our results are mixed, with the enrollment gap in our OLS model falling significantly once we control for selection and possible omitted variable bias. Specifically, we find that IDP children ages 6-11 share a similar probability of being enrolled in school as non-migrants, while IDP children ages 12-17 are approximately 3.8% less likely to be enrolled in school than non-migrants.

With respect to the question of living in a high conflict region versus being directly affected by conflict, we find that for IDP children ages 6-11 and 12-17, the education accumulation gaps range from 0.22-0.32 and 0.45-0.61 fewer years of schooling, respectively, depending on the level of conflict the sample is restricted to. With respect to enrollment, the evidence is mixed. For the 6-11 age cohort, we do not find an enrollment gap. However, at the secondary level, IDP children in high conflict regions are significantly less likely than non-migrants to be enrolled in school (6%-11%). These estimated education gaps in high conflict municipalities largely mirror the gaps we see when considering all municipalities, particularly with regard to education accumulation.

For our final question comparing IDPs with other migrants, we find no gaps for the 6-11 age cohort after controlling for selection and omitted variable bias. However, we still note both an attainment and an enrollment gap for the 11-17 age cohort. Specifically, we find that IDP children ages 12-17 have 0.34 fewer years of schooling than other migrants and are 4.1% less likely to be enrolled in school.

The findings from our analysis suggest that conflict reduces education accumulation. We find that the impact of having parents directly affected by conflict is more prominent for children at the secondary level. In addition, our results suggest that in Colombia, though living in a conflict region may affect a child's education accumulation, the effect is far less than being directly affected by conflict (i.e. being an IDP). With respect to enrollment, using exposure to conflict suggests a gap for the 6-11 age cohort, while our fixed effects analysis looking at those directly affected by conflict suggests that such a gap does not exist. For the 12-17 age cohort, the estimated enrollment gap using exposure is much smaller than the gap using fixed effect models.

This paper contributes to the literature by highlighting the direct impact of conflict on education accumulation and enrollment in Colombia estimated looking at those directly affected. Although the past literature has looked at the impact of conflict on school related outcomes, it has focused on the impact of living in a municipality with high conflict versus being directly impacted by this conflict. Though the former is useful and could potentially avoid selection bias, looking at individuals who have been directly impacted by conflict can provide more insight on the impact of conflict on education outcomes when conflict is of a low grade intensity as in Colombia. Also, to the best of our knowledge, we are the first to focus on estimating the impact of conflict focusing on the educational accumulation and enrollment gaps for recent IDPs in Colombia using census data.

The rest of our paper proceeds as follows. In section two we review the past literature on conflict and its education impacts and also highlight past literature on IDPs in Colombia. Section three is a summary of the data sets we used in this paper. In section four, we provide descriptive analysis of the data. Our empirical model is in section five, and section six provides a detailed summary of our finding and robustness checks. We conclude in section seven.

2 Literature Review

Education outcomes and the factors that affect these outcomes are considered extensively in the literature. Specifically for Colombia, one factor considered is the opportunity to attend private school through a voucher program. Angrist et al (2002) examine the short term effects of the use of vouchers on students who applied for the vouchers in Bogotá in 1995. The longer term effects of this program are also considered by Angrist et al (2006). They find that the voucher program increased secondary school completion rates by 15-20%. Returns to education in Colombia have also been estimated by several authors[4]

Factors that affect school attainment and enrollment have been analyzed both within and outside Colombia. Migration, income shocks, loss of life, and institutional quality are examples of factors examined in the literature.[5] Another factor that affects attainment and enrollment highlighted in the more recent literature is conflict. However, the challenges of collecting accurate household level data during armed conflict has limited the volume of studies on this topic.

In one of the few studies to assess the impact of conflict on education attainment using microeconomic data, Shemyakina (2011) studies the impact of the 1992-1998 civil conflict in Tajikistan on school attainment and enrollment. Shemyakina finds that regional-level exposure to the Tajik civil conflict had little or no effect on boys' enrollment. However, it had a large negative effect on girls' school enrollment. Akresh and de Walque (2008) study the effects of the 1994 Rwandan genocide on schooling. The authors find that children who lived through the Rwandan genocide, concentrated during a 100-day period, lost nearly a half year of schooling compared to their peers who were not exposed. They were also 15% less likely to complete grades three and four. For Guatemala, Chamarbagwala and Morán (2011) examine the impact of exposure to the 36-year civil war on education outcomes for the rural Mayan population. Exposure is measured as a department-level variable indicating number of human rights violations and acts of violence. In this disadvantaged group, the authors find a strong negative impact of conflict on education accumulation. For the three periods of the civil war identified, rural Mayan males showed a 0.27, 0.71, and 1.09 year decline in education attainment, while females showed a 0.12, 0.47, and 1.17 year decline. With very low education attainment overall, this amounts to a 23% and 30% decline in years of schooling during the third period of the war for males and females, respectively. Taken together, the literature on Tajikistan, Rwanda, and Guatemala suggests that exposure to conflict negatively affects education outcomes. However, each of these studies looks at regional-level exposure to violence during a high-intensity conflict. Compared to the conflicts in Tajikistan, Rwanda, and Guatemala, the conflict in Colombia is much more protracted, low-intensity, and involves a greater number of irregular actors. These actors employ strategies that directly target civilians for expulsion, recruitment, and assassination. In this situation, we cannot assume that every individual in a high conflict region is equally exposed to violence.

[4]See Poveda and Sossa (2006), Gaston and Tenjo (1992), Psacharopoulos and Velez (1992,1993) and Psacharopoulos (1994).

[5]See McKenzie and Rapoport (2010) for the impact of economic migration on education attainment in rural Mexico, Evans and Miguel (2007) for the effect of losing a parent and the importance of institutions, and Glewwe and Jacoby (1994) for the impact of availability and quality of school facilities on education attainment in Ghana. Also see Jacoby and Skoufias (1997), Duryea et al (2001), and Thomas et al (2004) for the impact of income shocks on schooling decisions in peaceful environments.

Several authors investigate the relationship between violence and education in Colombia. Barrera and Ibáñez (2004) develop a theoretical framework to explore the three ways in which violence can affect education. First, violence directly reduces the utility of individuals. Second, it destroys physical capital, creating uncertainty, deterring investment, and reducing productivity. Third, it reduces returns to education because education is not viewed as a value-enhancing commodity.[6] The authors also show a statistically significant gap in school enrollment rates between municipalities above and below the median national homicide rate. They find that violence has a negative impact on school enrollment at all ages, and that this effect is particularly large for young adults. While the paper shows the negative effects of living in a violent municipality on school enrollment, it does not provide evidence on the impact of armed conflict on education outcomes of those directly affected by violence–the IDPs. It also does not discriminate between generalized violence and violence occurring as a direct result of the armed conflict. Dueñas and Sanchez (2007) go a step further by looking specifically at the impact of armed conflict. The authors look at the impact of conflict on another school related outcome, drop-out rates. Focusing on households in the eastern part of Colombia, they show using a duration model that the presence of illegal armed groups increases dropout rates, with increased effects for the poorest households. Though this paper considers the impact of the presence of armed conflict in an area on an education outcome, it does not look directly at IDPs, which we feel is important given the nature of the conflict in Colombia. Rodriguez and Sanchez (2012) build on Dueñas and Sanchez (2007) by considering the joint decision to drop out of school and enter the labor market. The authors suggest that conflict-related violence does not seem to affect education investments or child labor decisions for younger children, but it does negatively impact children over age 12. Also, the authors find that the effect of violence varies primarily with age rather than with gender or household wealth. Although looking at the impact of regional-level exposure to violence on education outcomes is useful, it may not reveal the full impact of conflict on those directly affected especially when the conflict is low intensity, less random and more targeted.

Though past research does not focus on the education gap of IDPs in Colombia, the plight of the displaced in general has been considered. First, Kirchoff and Ibáñez (2002) study the probability of individual or household migration in Colombia and find that households that have been directly affected by violence–either assassination or death threats–have a higher probability of migrating. Of the IDPs interviewed, 58.2% reported that a household member received a death threat, compared to only 9.1% of non-displaced from the high conflict regions. The authors provide extensive descriptive data on the IDP population gained from interviews in three urban centers. Results indicate that "security considerations are not the only determinants of the displacement decision." Rather,

[6] These channels through which armed conflict affects schooling are confirmed by Shemyakina (2011).

displacement may be motivated by individual characteristics, such as risk aversion, or external factors such as direct targeting by guerrilla and paramilitary groups. Ibáñez and Moya (2006, 2009) look at the vulnerability of IDPs over time and find that because IDPs are unable to successfully integrate into the urban economy, well-being decreases and households are forced to take drastic measures in order to smooth consumption. Lozano-Gracia et al (2010) find that while the majority of IDPs migrate to geographically proximate locations, individuals from municipalities in the top 10% of violence levels will move far from their municipality of origin, perhaps in hope of distancing themselves from the conflict. Taken together, the literature on IDPs in Colombia suggests vulnerability stemming from loss of assets, potential psychological effects of fear and targeting, and loss of income, all of which we expect to influence education outcomes.

Given the low grade protracted conflict in Colombia has led to the largest displacements in the world, and its effects on human capital investments are largely unexplored, this paper will investigate the direct impact of conflict on enrollment and attainment focusing on the displaced.

3 Data

The primary data we use to answer our questions of interest come from four sources: the Colombia 2005 Census, the Office for the Coordination of Humanitarian Assistance, United Nations Office on drugs and crime in Colombia's (UNODC) coca survey 2001-2005 and conflict-related data from 2000-2005 from the Center for Economic Development Studies, (CEDE) which is part of the Faculty of Economics, Universidad de Los Andes. We accessed the 2005 Census, via IPUMS-International,[7] and the majority of the data for this study comes from this source. This data includes over two million observations, a 5 percent sample of the 2005 Colombian Census, which is notable for its accuracy and coverage.

We are able to identify IDPs from this data using those who state that they migrated in the past five years and select "violence or insecurity" as the reason for migrating. This technique of identifying IDPs has two potential limitations. First, we are unable to identify IDPs who moved more than five years ago. This is because though we can identify indirectly all who have migrated by comparing birth place to current place of residence, the question that allows us to identify IDPs is restricted to those who migrated in the last five years. This limitation implies that we are unable to look at the long term enrollment and attainment effects of direct exposure to conflict. However, this possible limitation works in our favor because we are interested in exploring the direct impact of conflict rather than the effect of displacement across generations. The education impact of being

[7]Minnesota Population Center. Integrated Public Use Microdata Series, International: Version 6.0 [Machine-readable database]. Minneapolis: University of Minnesota, 2010.

directly affected by conflict is most precisely estimated by looking at children whose parents were recently affected by conflict (last five years) versus those whose parents were affected by conflict many years ago, possibly before children are even born.[8]

Second, the responses to the question of why you migrated are mutually exclusive, so each person selects only one motivation for migration. This could potentially be an issue if an individual migrated for more than one reason. However, we find this restriction advantageous because it forces people to pick their most important reason for migrating. This allows us isolate those who were directly affected by conflict (true IDPs) because there is no incentive in the census for selecting conflict as a reason for migration. Often, government benefits and programs for displaced people provide an incentive for people who did not necessarily migrate because of conflict to register as an IDP. This may help explain why the proportion of IDPs in our sample is much smaller than that reported by the government.[9] It is also worth noting that about 0.32% of the sample of recent migrants do not report any reason for migration, so there is a potential that this group could also include IDPs. We create a separate category for these observations in our analyses. However, given that the sample size of this group is small, we are not too worried about the potential impact of these observations.

The census data is appropriate for this analysis not only because it has a large sample size and allows us identify IDPs, but also because it has a wide range of variables that we can use as controls in our enrollment and accumulation models. The major limitation of the data is the lack of information on income. However, the Colombian census has many indicators for poverty and wealth that we can use as proxies for income. Still, this could also be problematic because of the endogenous nature of some potential proxies for wealth.[10] We are careful not to choose proxies like home ownership given its potential endogeneity. Instead, we focus on proxies that reflect wealth but are not assets typically lost through the conflict, such as car ownership.

The source of data concerning humanitarian risk, conflict, capacity, social, and economic levels per municipality is the Humanitarian Situation Risk Index (HSRI), calculated by the Office for the Coordination of Humanitarian Assistance in collaboration with the Universidad Santo Tomas in 2008. The HSRI was developed with the purpose of calculating the probability that a humanitarian situation will occur at the municipality level in Colombia. Four sub-indices of risk are calculated and

[8]In this paper we refer to recent IDPs who our analysis is focused on simply as IDPs.

[9]In our sample, 1.2% are IDPs which is larger than the 0.7% and 0.8% of the sample who are migrants for natural disasters and health respectively. We do not suggest that our method identifies all those who officially registered with the government or international organizations as IDPs during our data period. However, we are confident that we capture most of those whose move was primarily driven by direct exposure to conflict from 2000-2005.

[10]Income affects schooling and typically is controlled for in these kinds of analysis. Because it could be endogenous given that the lower income of many IDPs is linked with displacement, excluding income as a control might not be problematic.

used to determine the overall humanitarian risk: conflict, response capacity, social, and economic. [11]. A HSRI value and a value for each of the risk indicators is provided for each of 1,100 Colombian municipalities. However, only 532 municipalities or groups of municipalities are defined in the 2005 Census because in many cases small municipalities are grouped together. In order to incorporate the HSRI and its sub-indices data into the 2005 census data, we assign to individuals whose current municipalities are not grouped in the census, the value of the indices in their current municipality. For individuals whose municipality is a grouping of small municipalities, we impute the average HSRI and other sub-index values for all of the municipalities in that particular grouping.[12]

We also use data from the Coca cultivation survey from 2001 to 2005, conducted by the government of Colombia and the United Nations Office on Drugs and Crime (UNODC) in Colombia's Banco de Información Espacial Proyecto SIMCI. We derive data on hectares of coca farmed for each year from 2001-2005 in each municipality in Colombia. As with data from HSRI, this data provides information for all municipalities separately. We apply the same averaging technique described above to import this information into our census data. Data on total hectares of coca cultivated in a municipality is useful given the documented link between conflict, displacement and narcotic drugs in rural areas in Colombia. We use this data to create an instrument to predict being an IDP in the later part of this paper.

Municipality-level conflict data was derived from the Center for Economic Development Studies (CEDE) in the Faculty of Economics, Universidad de Los Andes. This dataset contains a wide variety of variables measuring conflict, allowing us to extract information from 2000-2005 on captures, homicides, and massacres linked to the armed conflict. We impute this information into our census data in the same manner described for the conflict index and coca data. Later, we use this conflict data to construct instruments.

4 Descriptives

In this section we present some descriptive statistics to further motivate our discussion on the education attainment and enrollment of IDPs.[13]

[11] Information used to calculate the sub-indices comes from the National Administrative Statistics Department (DANE), the Ministry of Social Protection, the National Planning Department, the Social Action and Unified Registry System, the National Education Ministry, the Central Judicial Police Directorate, the Center for Criminological Investigations, the Vice-presidency's Mines Observatory, Free Country (an NGO), the Center for Conflict Analysis Resources (a think-tank), and the World Bank. See Appendix 1 for variables included in the Conflict and capacity

[12] Although we impute all indices into the census data, we only make use of the capacity and conflict indices in our analysis.

[13] It should be noted that through out this study, "IDPs" refers to Colombians who have migrated because of conflict, according to the 2005 census. Natural disaster migrants are treated as a separate category.

Table 1 presents summary statistics of basic indicators for IDPs in comparison to other migrants. Notice that in comparison to other migrants, IDPs are older, more likely to be male, have more children in the family, less likely to be married, less likely to be in an urban area, more likely to be black or indigenous, less likely to be literate, less likely to be employed, and more likely to be disabled. Compared to non-migrants, IDPs are younger and have fewer years of schooling, and there is less of a difference with respect to being married, gender, living in an urban area, employment, and race. This may be because other migrants represent a select group. Notice that IDPs differ significantly in some variables that may be indicative of experiences characteristic of those who have been directly affected by conflict. For instance, IDPs are more likely than any other group to be disabled, and they have the lowest likelihood of owning a dwelling. With respect to education accumulation, Table 1 shows that IDPs have a lower mean education attainment than non-migrants and other migrants. IDPs also have more children than other groups despite having similar probability of being married and similar mean age. This difference may suggest that families with children are more likely to be directly affected by conflict than those without or that families with children are more likely to migrate if directly affected by conflict. The summary in Table 1 confirms the existing literature that IDPs are vulnerable. Although the literature suggests that migrants are a select set with exceptional drive, the findings below suggest that IDPs do not fit the mould of other migrants on average.

Table 1: 2005 Census: Descriptive Statistics

Category	IDPs	Other Migrants	Non-Migrants	All
Age	27.059	26.691	29.060	28.586
Male	0.509	0.492	0.503	0.501
No. children	0.984	0.787	0.809	0.807
Married	0.370	0.401	0.354	0.363
Urban	0.523	0.702	0.553	0.581
Race: White	0.705	0.836	0.796	0.803
Race: Black	0.170	0.110	0.103	0.105
Race: indigenous	0.072	0.030	0.071	0.063
Yrs school	4.354	6.321	5.125	5.349
Literacy	0.738	0.823	0.739	0.755
Employed	0.307	0.366	0.273	0.291
Disabled	0.082	0.058	0.073	0.070

To further motivate our discussion, we present some enrollment statistics. Table 2 shows proportion enrolled in school by age cohort and migration status. These summary statistics suggest that the displaced are less likely to be enrolled in school than any other group in all school age categories. The enrollment gap is particularly substantial over age 12. We do not infer from these summary statistics that being displaced created these enrollment differences. It is possible that the displaced come from municipalities with less access to quality education facilities or move to communities with less access which may lead to lower probability of enrollment.[14]. However, given the priority given by the government to IDPs in access to public education in destination communities, finding significantly lower enrollment levels for IDPs is worth highlighting suggests a potential negative effect of conflict.

Table 2: 2005 Census: Proportion Enrolled in School

Age Cohort	IDPs	Other Migrants	Non-Migrants	All
6-11	0.830	0.901	0.891	0.892
12-17	0.645	0.724	0.735	0.732
18-21	0.253	0.281	0.278	0.278
22-25	0.097	0.149	0.135	0.138

Table 3 highlights the education attainment of the displaced, other migrants, and non-migrants. It indicates that IDPs have much lower attainment than any of the other groups, and that the gap grows over time. These results can also be seen graphically in Figure 1. The gap grows significantly in the 12-17 age cohort, with the displaced achieving approximately 1.05 fewer years of schooling than non-displaced migrants and 0.92 fewer years than non-migrants. The large difference between this group and other migrants is likely attributable to the large proportion of other migrants who have moved for study and therefore reach very high education attainment.

Table 3: 2005 Census: Education Attainment

Age Cohort	Displaced	Other Migrants	Non-Migrants	All
6-11	1.948	2.188	2.317	2.287
12-17	5.484	6.535	6.408	6.419
18-21	6.868	8.692	7.992	8.132
22-25	6.732	9.064	7.992	8.253

The highlighted descriptive statistics all suggests that IDPs are vulnerable in terms of education. Even if we focus solely on potentially vulnerable migrants (migrants who have moved because of

[14]However, Kirchhoff and Ibanez (2002) suggest that the displaced have a higher probability of being enrolled in school after displacement, which may suggest movement to better off communities

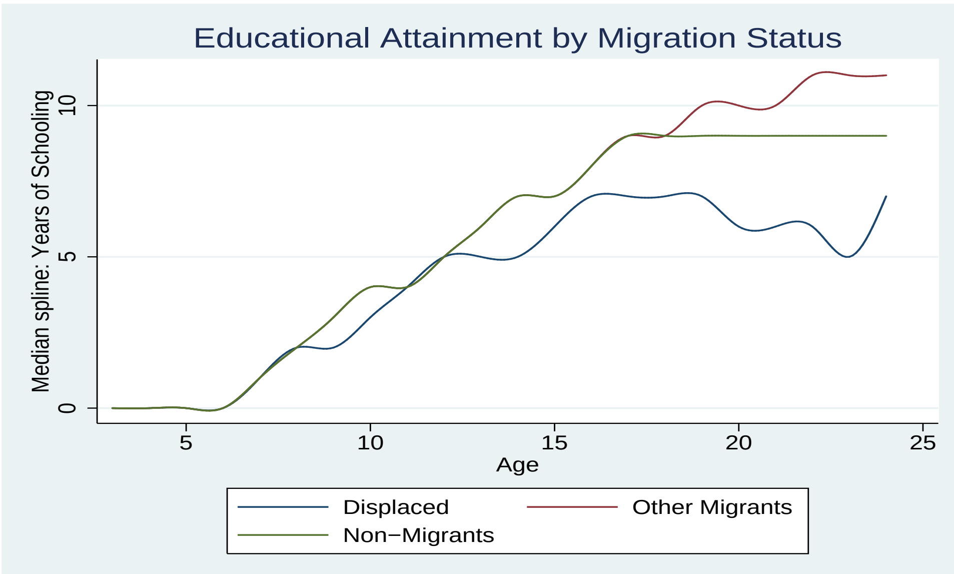

Figure 1:

natural disaster, health, or displacement) as in Figure 2, we still see that IDPs end up with fewer years of schooling. Notice that IDPs appear to achieve similar years of schooling as other vulnerable migrants until secondary school, when health migrants move ahead and the displaced fall behind. By age 17, the gap has widened even more, with the displaced achieving approximately 3 years less schooling than those who migrated for health reasons, and natural disaster victims falling somewhere in between.

These preliminary descriptive statistics suggest that children of IDPs are vulnerable with regard to education. In this paper, we focus on the education accumulation and enrollment impact of direct exposure to conflict on children. Even though the above results suggest lower mean education accumulation and enrollment for IDPs or their children, this result does not imply that conflict is responsible for the gap. It is possible that children of IDPs have parents with lower education or who are poorer independent of conflict. In this scenario, a gap in education attainment or enrollment is expected. To evaluate the possible effect of direct exposure to conflict, we turn to econometric analysis.

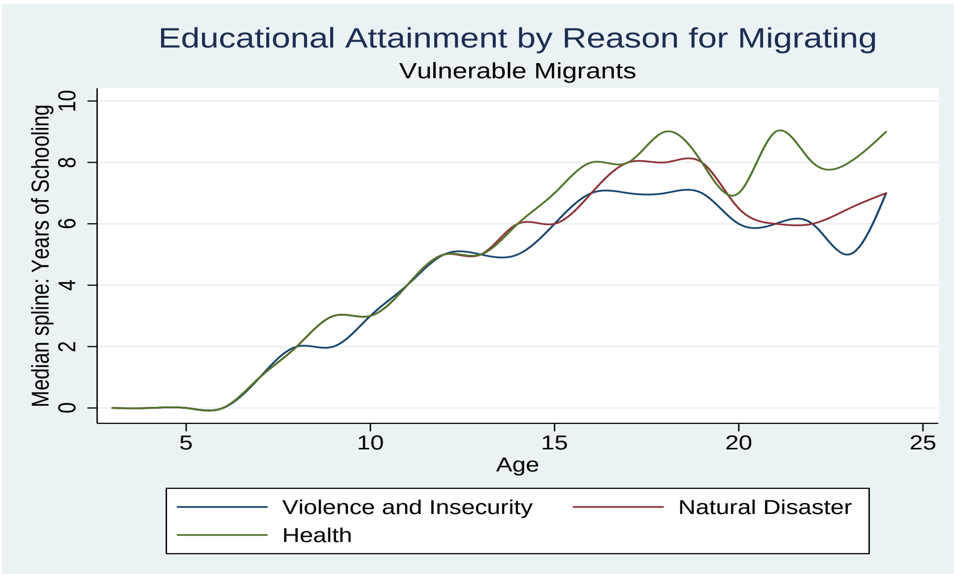

Figure 2:

5 Empirical Strategy

5.1 General econometric model

As mentioned above, we focus on four main questions in this paper. First, is there an education enrollment and accumulation gap for children of those living in municipalities with high conflict in comparison to those living elsewhere? Second, does being directly affected by conflict affect education accumulation and enrollment for children? Third, does living in a high conflict municipality create similar education accumulation and enrollment gaps as being directly affected by conflict? Finally, how do IDPs compare to other migrants in education accumulation and enrollment? Our underlining goal in answering these four questions is to provide overwhelming evidence that direct exposure to conflict leads to an education gap and a different education outcome than simply living in a high conflict area.

To answer our first question, we estimate a school accumulation empirical model (equation (1)) and a school enrollment empirical model (equation (2)) for a typical individual.

$$y_i = \alpha_0 + \alpha_1 x_{i1} + \alpha_2 x_{i2} + \alpha_3 x_{i3} + ... + \alpha_j x_{ij} + \beta R_i + \epsilon_i \tag{1}$$

For the estimate of β to be consistent, we assume that the regression error ϵ_i is uncorrelated with R and the other regressors. Here, y is years of schooling for individual i in a particular age cohort

c. We focus on two cohorts in our analysis, ages 6-11 and ages 12-17, because these age groups corresponds with Colombia's school system - primary and secondary school. x_1 to x_j are variables that affects a child's schooling in cohort c. These variables include gender, wealth correlates, family size, mother's years of schooling, father's years of schooling, number of children of mother, class of work of father, class of work of mother, age, the social capacity of a municipality, employment status of father, dummy variables that are important to control for such as state of residence, if an area is urban or rural, and race. R is a dummy variable that takes a value of 1 if a child is in a municipality with high conflict and 0 if otherwise. We define a municipality with high conflict as one with conflict above the mean. ϵ is the error term. The inclusion of parental variables reduces the potential of omitted variable bias given the importance of parents' education as a predictor of child's education and also serves as a good proxy for wealth. However, including these variables comes at a cost because not all children in the sample have information about parents. We drop all children who do not have information on both parents' education from the sample we estimate. However, later in the paper we address whether using the restricted sample for which parental information is available creates a biased or non-generalizable estimate.

To answer the second part of question 1, we assume that a child being enrolled in school is a function of a set of variables Z. In this case, our independent variable is a binary variable that takes a 1 when a child in a particular age cohort is enrolled in schooling and 0 otherwise. We rewrite equation (2) assuming a probit modeling strategy. The Φ in equation (3) of our empirical school enrollment model indicates the Cumulative Distribution Function (CDF) of the standard normal distribution. The description of the variables is the same as in equation (1) above. Using a probit model, we estimate equation (3) and find the marginal effects. The marginal effects represent the impact of a unit change in each independent continuous variable on the probability of being enrolled in school. This provides a straight forward interpretation of estimated results from the probit models. For dummy variables like R, which is the focus of the first question, the interpretation of marginal effects is slightly different. The marginal estimate captures the difference in the probability of being enrolled in school for a particular group dummy relative to the baseline group. In the case of R, the estimated marginal effect captures the probability of being enrolled in school for a certain age cohort for those living in a high conflict municipality relative to those living elsewhere.

$$Prob(S = 1|Z) = F(Z'\beta) \tag{2}$$

$$Prob(S = 1|X) = \Phi(\alpha_0 + \alpha_1 x_{i1} + \alpha_2 x_{i2} + \alpha_3 x_{i3} + ... + \alpha_j x_{ij} + \beta R_i) \;\; i = 1, ..., N \tag{3}$$

For the second question, our empirical strategy is to first estimate equation (4) using OLS. Notice equation (4) is very similar to equation (1). The difference lies in the matrix M in equation (4) replacing dummy variable R. To answer the second part of question two, we also alter our enrollment empirical model, equation (3), dropping R and including M in equation (5). Once again we compute and report marginal effects for the enrollment model.

$$y_i = \alpha_0 + \alpha_1 x_{i1} + \alpha_2 x_{i2} + \alpha_3 x_{i3} + ... + \alpha_j x_{ij} + \sum_{m=1}^{9} \beta_m M_i + \epsilon_i, \;\; i = 1, ..., N \qquad (4)$$

M is a vector of matrix variables that divides the population based on cause for migration in the last five years. For these dummy variables, the base group is people who have not moved in the last five years. We call this group non-migrants. Among the migrant cause dummies, we have a dummy for those who migrated because of violence or insecurity. This is our identifier of IDPs and the dummy we will focus on in answering our second question.[15]

$$Prob(S = 1|X) = \Phi(\alpha_0 + \alpha_1 x_{i1} + \alpha_2 x_{i2} + \alpha_3 x_{i3} + ... + \alpha_j x_{ij} + \sum_{m=1}^{9} \beta_m M_i) \;\; i = 1, ..., N \qquad (5)$$

To address the third question, we re-estimate equations (4) and (5) on the sample of those exposed to conflict. To test the sensitivity of our result, we try different ways of defining the population exposed to conflict. First, we consider states with a conflict index above the mean. Next, we consider municipalities with conflict index above the mean. Lastly, we consider municipalities with very high conflict (in the top quartile of the conflict index).

We address the last question by again altering equations (1) and (3). In contrast to the first two questions for which we focus on both migrants and non-migrants, here we restrict our sample to only migrants. In addition, we alter the dummy variable R. Recall that for the first question, R=1 if a person lives in a municipality with high conflict. For this question, R takes the value of 1 if a person is an IDP and 0 if the person is a migrant for any other reason.

6 Potential channels through which conflict can affect education outcomes in Colombia

The impact of forced displacement on victims of conflict is substantial. For the purpose of this paper, we zero in on the empirical question, what is the education impact of conflict? While we

[15]Other dummies include migrating for work, school, natural disaster, health, migration reason not stated and migration for other reasons.

may expect children directly affected by conflict to drop out of school temporarily, repeat grades, fall behind their peers in education attainment, and be less likely to be enrolled at any given time compared to non-migrants, we cannot presume this outcome. Government programs to support this disadvantaged population immediately after displacement may negate any potential education gap. For example, the government assists displaced people through humanitarian emergency assistance (HEA), which provides nutritional support, food, water, housing, psychological help, and medical services to the displaced during the first few months after displacement. Following this period, the government offers socioeconomic stabilization programs focused on creating the necessary conditions for the displaced to return to productive day to day activity. Access to these government programs is contingent upon registration in a system called SUR. The SUR system is described in detail by Ibáñez and Velazquez (2006). Beyond the broad assistance highlighted above, the displaced are given priority in public schools. This additional layer of support for an IDP could potentially lead to increased education attainment and enrollment for IDPs compared to non-migrants who do not have the same support. Unfortunately, such services for IDPs increase incentives for non-displaced individuals to register as IDPs, although the government tries to curb false claims under SUR.

On the other hand, direct contact with conflict can lead to lower education accumulation. The displaced often abandon land and lose assets and sources of income, which may create a constraint on future income streams and expenditures, including expenditure on children's education. However, this cannot be the only channel because those who migrate because of natural disaster are also likely to have lost assets and have constrained expenditures. While they do show an education gap compared to non-migrants, it is not even half the size of the gap for IDPs.[16] Another possible explanation is that being directly affected by conflict leads to injury or death of family members. This could lead to reallocation of household duties, including children dropping out of school to care for a disabled family member or going to work. Ibáñez and Valasquez (2006) note that the conflict in Colombia has led many women to become head of household and many children to leave school to engage in income-generating activities. Table 1 also seems to support this channel, showing that IDPs are more likely to be disabled. Another channel beyond the scope of economics could be the psychological impact of being directly affected by conflict. Combining this potential psychological effect with the two other highlighted channels and the disruption in education that comes with moving for exogenous reasons, one may expect displacement to have a negative effect on education accumulation and enrollment of IDP children. Given that we are unable to determine *a priori* which effect will dominate, the effect of conflict in Colombia can only be determined empirically.

[16]See earlier version of the paper for tables showing estimated education gaps for other migrant groups.

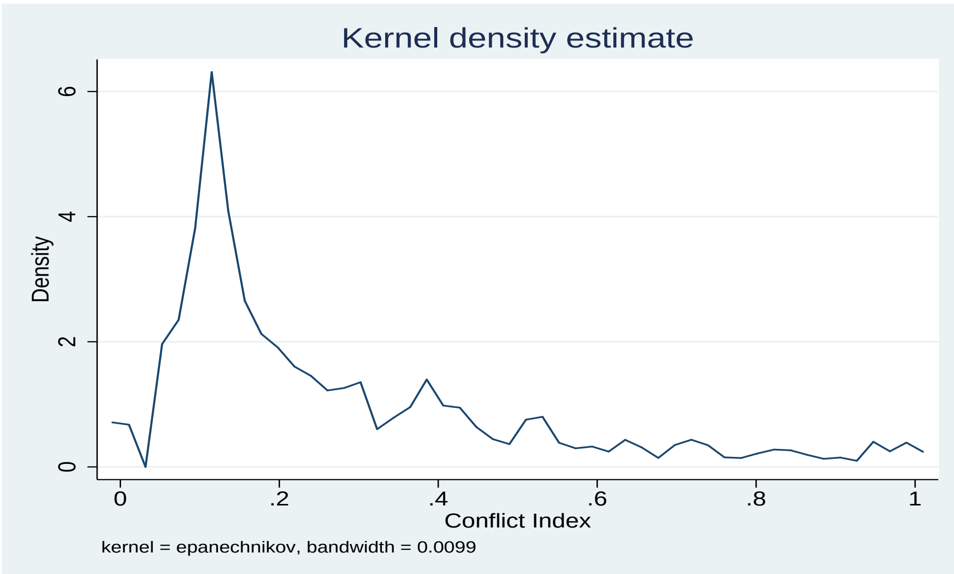

Figure 3: Kernel Density for Conflict Index

7 Results

7.1 Basic OLS results with controls

7.1.1 Does living in a high conflict area affect educational outcomes?

The first question we try to answer as a motivation for our main question is if living in a high conflict region leads to a gap in education. The purpose of this analysis is to compare the results to what has been noted in the prior literature on Colombia using other datasets. Figure 3 shows the density function for the conflict index. We designate any municipality with conflict over 0.3 as a region with high conflict. The mean conflict index is 0.276. Using this benchmark, 34.6% of the sample is exposed to high levels of conflict. We use this information to create a dummy variable which we include in our school accumulation and enrollment models. Individuals with a conflict index above 0.3 are assigned a 1 and all others are assigned a 0. Controlling for the factors that can affect education outcomes both at the individual and regional level, the results in Table 4 indicate that children who live in a high conflict region have about 0.036 fewer years of schooling than those living elsewhere. With respect to the probability of being enrolled, we note that children living in high conflict regions have a lower probability of being enrolled in school (0.8% and 0.9% lower at the elementary and secondary school levels respectively). Our results in Table 4 are quite different from Rodriguez and Sanchez (2012) and much smaller. They predict that without conflict, the average educational attainment of

Table 4: Does living in a high conflict region affect educational outcomes?

Variable:	Age 6-11		Age 12-17	
	Accumulation Model	Enrollment Model	Accumulation Model	Enrollment Model
	(1)	(2)	(3)	(4)
Conflict Region	-0.036***	-0.008***	-0.036*	-0.009**
	(0.01)	(0.00)	(0.02)	(0.00)
Sex	-0.132***	-0.009***	-0.493***	-0.042***
	(0.01)	(0.00)	(0.02)	(0.00)
CAP	0.165***	0.007	-0.237***	-0.022**
	(0.03)	(0.01)	(0.06)	(0.01)
Urban	0.006	0.016***	0.457***	0.062***
	(0.01)	(0.00)	(0.02)	(0.00)
Mom yrs of sch.	0.044***	0.005***	0.109***	0.010***
	(0.00)	(0.00)	(0.00)	(0.00)
Dad yrs of sch.	0.019***	0.003***	0.055***	0.008***
	(0.00)	(0.00)	(0.00)	(0.00)
N	171083	171393	148447	148650
F	1794.31		858.66	
$P(F) > 0$	0.000		0.000	
R^2	0.661		0.472	
χ^2		5672.32		7443.67
$P > \chi^2$		0.000		0.000
Pseudo R^2		0.151		0.196

Note: Also controlled for age, race, family size, disability, employment status of father, automobile ownership, wall type, computer ownership, class of father's work, class of mother's work, mother's number of children, and department.
Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

children between 6-11 years of age residing in conflict areas would have been 0.1-0.2 years greater, and for children between 12-17, 0.6-1.2 years greater. This difference in results is possible for several reasons. First, we look at the education gap by comparing children in high conflict regions to those in lower-conflict municipalities, which is different from comparing average schooling with violence to predicted average schooling without violence. Second, Rodriguez and Sanchez use a duration model based on a conflict variable assuming that children currently in school have never dropped out and estimate drop-out probability and survival rate which they use to predict length of schooling. In contrast, we look at differences in education between children currently living in a high conflict region compared to those living elsewhere. Finally, Rodriguez and Sanchez use the 2003 Colombia household survey covering 24,090 households in 128 municipalities, while we use the 2005 census with a sample size of 2,003,186 individuals across 533 municipalities. Breaking with past research, our results suggest that living in a high conflict area does not necessarily drive the education gap.

In fact, when we cluster our standard error at the municipality level (because our conflict variable is at the municipality level), we get no significant effects in all cases apart from the probability of enrollment for children ages 6-11 years.

It is also important to mention that our estimate could be biased because of the possible endogenous nature of the dummy variable for living in a high conflict area. If living in a high conflict area is correlated with an omitted variable that can affect education accumulation or enrollment, then the estimated coefficient could be biased. Although we control for many variables that could fit this profile with our capacity index,[17] one variable that we do not control for and that Rodriguez and Sanchez (2012) point to as potentially leading to lower schooling outcomes is pressure to join militant groups in high conflict regions. Hence, if we do not control for this omitted variable, we could attribute to living in a conflict region the impact of the pressure to join militant groups.

Assuming this is true, our estimated impact of living in a conflict region could be upward biased, implying an even smaller gap between children living in conflict region and children who do not. Another way to interpret these results is to think of the estimated impact as an upper limit on the difference as long as there is no omitted variable positively correlated with the dependent variable and exposure to conflict.[18]

The above results suggest living in a high conflict municipality has only a very small effect on education outcomes. This finding leads to our second question of how being directly affected by conflict impacts education accumulation and enrollment of children.

As highlighted in our model specification, we estimate an OLS regression, controlling for potential heteroskedasticity and controlling for the general predictors of school accumulation. For our enrollment model, we estimate a probit model and derive the marginal effects. In both cases, we include dummies that allow us divide the population into subgroups. Specifically we include dummies for if an individual is a non-recent migrants and eight dummies capturing the different types of recent migrants: work migrants, family migrants, study migrants, natural disaster migrants, health migrants, other migrants, non-specified migrants, and our group of interest, IDPs. These dummies allow us to compare the different groups of migrants to a base group of non-migrants.

7.1.2 OLS Results: The Impact of Direct Conflict on Education

The results summarized in Table 5 help answer the question whether being directly affected by conflict creates education accumulation and enrollment gaps for IDP children in comparison to non-

[17]The capacity index is a measure of a municipality's infrastructure. We also tried alternative models with more poverty correlates like home ownership, floor type, number of rooms, and the result does not change significantly. Given this result, for the rest of the paper we restrict ourselves to just a few poverty correlates.

[18]We could not think of any such variable that we did not control for, but it is still a possibility.

migrant children. Specifically, Table 5 shows estimates from the school accumulation and enrollment models (Equations 4 and 5) using OLS and correcting for potential heteroskedasticity. Although this estimation includes the whole sample both migrants and non-migrants, we only highlight in the table the gaps for the displaced.[19] In panel A we show estimates for the attainment gap, and in panel B we show estimates for the enrollment gap. We estimate these gaps separately for ages 6-11 and 12-17 and present both results.[20]

Table 5: Do migrants who move because of violence or insecurity face an education gap compared to non-migrants?

	Ages 6-11			Ages 12-17		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Education Accumulation Model					
IDP	-0.199***	-0.199***	-0.197***	-0.513***	-0.513***	-0.515***
	(0.04)	(0.04)	(0.04)	(0.09)	(0.09)	(0.09)
M Yrschl	0.044***	0.044***	0.044***	0.108***	0.108***	0.108***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
F Yrschl	0.020***	0.020***	0.020***	0.056***	0.056***	0.056***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	171083	171083	171083	148447	148447	148447
F	1677.81	1657.45	1635.96	795.21	785.77	777.69
P(F)	0.000	0.000	0.000	0.000	0.000	0.000
R2	0.663	0.663	0.663	0.474	0.474	0.474
	Panel B: Education Enrollment Model					
IDP	-0.017*	-0.016*	-0.016*	-0.063***	-0.063***	-0.063***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
M Yrschl	0.005***	0.005***	0.005***	0.010***	0.010***	0.010***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
M Yrschl	0.003***	0.003***	0.003***	0.008***	0.008***	0.008***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	171393	171393	171393	148650	148650	148650
χ^2	5699.98	5722.44	5709.09	7366.80	7441.71	7444.77
$P > \chi^2$	0.000	0.000	0.000	0.000	0.000	0.000
Pseudo R^2	0.151	0.152	0.152	0.196	0.196	0.196

Note: Also controlled for age, race, family size, disability, employment status of father, automobile ownership, wall type, computer ownership, class of father's work, class of mother's work, mother's number of children, department, sex, municipality conflict index, municipality capacity index, and urban.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column one of panel A, we control for age, sex, race, family size, sector (urban or rural), disability, employment status of father, class of work of father, class of work of mother, years of school

[19] In an earlier version of this paper, the gaps for the other migrant groups are also highlighted.
[20] We highlight the other control variables included in our estimation under the table.

of mother, year of school of father, number of children of mother, and department of residence. We also include some proxies for wealth because the census data does not have information on income. There were several potential variable that we could use as proxies. Our choice reflected variables that we believe could do a better job of capturing variation in wealth. These variables are number of cars, if an individual has a computer, and the type of walls of residence of an individual.[21] Although we are interested in the accumulation gap for IDPs in comparison to non-recent migrants, we also present the marginal effects for other recent migrant groups.[22] The results in column one do not control for conflict in the municipality or the social capacity in the municipality. Hence, estimates may exhibit upward bias.

These result suggests that children of IDPs between ages 6-11 have approximately 0.2 fewer years of schooling than non-migrants. When we add a control for conflict at the municipality level, we do not notice any change in the accumulation gap between children of IDPs and non-migrants (Table (5) column (2) panel A). In the model captured in column (3), we also control for municipality capacity, which is a necessary control to reduce omitted variable bias in the estimate of the direct impact of conflict. The estimated gap does not differ considerably in column (3) from the two previous models. However, the conflict variable is negative and significant, and the capacity variable is positive. Notice that this 0.2 gap is much bigger than the estimated gap in the model summarized in Table 4. The significantly smaller attainment gap based on conflict exposure (Table 4) compared to the gap for those directly affected by conflict (Table 5) suggests that a dummy for living in a high conflict region in Colombia may not be as effective in identifying the impact of conflict on education outcomes. Notice our R^2s are relatively high, meaning our model explains between 60-65% of the variation in school accumulation for children ages 6-11.

In the second half of panel 1, we restrict the sample to children ages 12-17. The trend in the estimates is the same. Although our R^2 drops, it is still relatively high. Focusing on column (3), our results suggests that children of IDPs ages 12-17 have about half a year gap in education accumulation in comparison to non-migrants.[23]

Table 5 panel B presents the estimated marginal effects for the enrollment model. These estimates lead to a similar conclusions as above. We estimate the probability of being enrolled in school for children 6-11 years and next, re-estimate for children ages 12-17. In contrast to the accumulation model for which we present estimates, for our enrollment model, we present the marginal effects from

[21] We chose these three proxies for wealth but our results for IDPs do not change significantly with alternative choice combination of potential wealth proxies like type of floor, having a toilet, ownership of dwelling, etc.

[22] In this paper, we will refer to non-recent migrants as non-migrants.

[23] Although we do not highlight the estimates of the gap between other migrant groups and non-migrants in this version of the paper, we note that the enrollment gap for the displaced is larger than the gap for the children of all the other migrant groups apart from those who migrated for non specified reasons.

the probit model estimation. The results in column (3), our preferred model, suggest that children of IDPs ages 6-11 are 1.6% less likely to be enrolled in school in comparison to non-migrants. The results for older children show an even larger gap of 6.3%.

It is important to note that the estimated effects using the OLS estimator are significant, but these preliminary estimates could be upward biased if being an IDP is correlated with unobservables related to enrollment or attainment. However, our findings are in line with Rodriguez and Sanchez (2012), who predicts much larger effects of exposure to conflict using an IV analysis at the secondary level versus at the primary level.

7.1.3 OLS Results for Question 3: directly affected by conflict vs. living in a conflict region

To answer the third question, we further investigate whether lower school attainment for IDPs is linked with being directly affected by conflict, living in a high conflict area in the past, or a combination of both factors. We try to answer this question in several ways, starting with the assumption that if exposure to conflict alone (i.e. living in a high conflict municipality) is the primary cause of education gaps, then those who did not migrate but live in a high conflict area should have similar education outcomes as IDPs in those areas. Hence, if we restrict our sample to only those living in areas with high conflict, the estimate on the IDP dummy should be 0 or close to 0. We explore three alternative ways of restricting the sample. Our conflict index runs from 0-1 with a mean of 0.28. In Table 6 column (1), we restrict our sample to departments with a mean conflict of over 0.3. This restriction leads to a reduction in the sample of departments in the analysis from 33 to 14. We then re-estimate our original model on this sub-sample. The result of this estimation is contrary to what may be expected. We find that children of IDPs living in high conflict departments have 0.24 fewer years of education than children of non-migrants who live in high conflict departments. However, averaging over the department may be misleading. Restricting the sample based on high conflict municipalities versus high conflict departments may be more informative. In columns (2) and (3) of Tables 6, we restrict the sample to only those living in municipalities with conflict indices greater than 0.3 and 0.5 respectively. We still find that children of IDPs living in high conflict areas have a lower education attainment (Panel A) and lower enrollment (Panel B). Specifically, when we restrict the sample to regions with conflict above the mean, we find that children of IDPs who are 6-11 years living in high conflict regions have 0.22 years less of schooling and have a 1.9% lower probability of being enrolled in school in comparison to non-migrants living in high conflict regions. For children of IDPs 12-17, the gap is significantly greater, at 0.56 fewer years of schooling and a 10.5% lower probability of being enrolled in school.

It is worth noting from Tables 6 that the gap in education accumulation increases when we focus on municipalities with the most intense conflict (index > 0.5).These finding are surprising if we expect exposure to conflict to be a good predictor of direct impact. For ages 6-11, the gap increases to 0.32 years while for ages 12-17, the gap increases to 0.61 years. We note a similar increase in the probability of not being enrolled in school in municipalities with the most intense conflict. What these results suggest is that being directly impacted by conflict and mere living in a municipality with a high conflict index leads to very different outcomes.

In columns (4) and (8) of Tables 6, we compare all IDPs with those who live in high conflict regions. This is another robustness check to avoid the possible argument that when we compared IDPs in conflict regions with others in those regions, we might be dealing with a select type of IDP. This is because we will be focusing on IDPs who are supposed to have migrated for the sole reason to avoid conflict but still end up in a place with high conflict. This may suggest that such IDPs are highly vulnerable, have no other options and are different from other IDPs who do not migrate to a high conflict region. By comparing all IDPS whether or not they live in conflict regions with migrants and non-migrants who live in conflict region, we avoid this potential issue.

The results in columns (4) and (8) suggest that children of IDPs ages 6-11 have about 0.1 fewer years of schooling than other migrant children living in high-conflict municipalities but similar probability of being enrolled in school. Children of IDPs ages 12-17 have 0.44 fewer years of schooling and a 5.7% lower probability of being enrolled. Notice that the education gap shrunk somewhat in this model compared to the other results in Table 6, suggesting that IDPs living in a high-conflict municipalities are to some extent different from the average IDP. Still, it is important to highlight the persistence of the gap, particularly at the secondary level, even after including all IDPs.

These results combined with the results in Table 4 provide evidence that though exposure to conflict might affect education accumulation and enrollment for children, being directly affected by conflict impacts these outcomes more significantly. Therefore, living in a region with high conflict does not lead to the same education impact as being directly affected by conflict.

In the next section, we measure the impact of conflict using migrants as the reference group rather than non-migrants.

7.2 Results Question 4: How do IDPs compare to other migrants?

The results used to answer to the question of how IDPs compare to other migrants in terms of education are summarized in Table 7. Focusing on migrants is useful for several reasons. First, it is possible to argue that IDPs are migrants, and so comparing them with other migrants is more appropriate. Notice that in earlier tables, children of most migrant groups have lower accumulation

Table 6: Does living in a high conflict region create similar gap as being directly affected by conflict?

	Ages 6-11				Ages 12-17			
	Dept conflict (> 0.3)	Municipality conflict (> 0.3)	Municipality conflict (> 0.5)	All conflict (> 0.3)	Dept conflict (> 0.3)	Municipality conflict (> 0.3)	Municipality conflict (> 0.5)	All (> 0.3)
Panel A: Education Accumulation Model								
IDP	-0.242***	-0.218***	-0.322***	-0.105**	-0.445***	-0.560***	-0.610***	-0.443***
	(0.05)	(0.06)	(0.09)	(0.04)	(0.12)	(0.13)	(0.19)	(0.09)
M Yrschl	0.050***	0.049***	0.051***	0.049***	0.107***	0.113***	0.108***	0.114***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
F Yrschl	0.018***	0.022***	0.024***	0.021***	0.058***	0.063***	0.077***	0.062***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
N	74212	56722	28071	57815	64052	48081	23326	48960
F	974.04	599.17	300.38	-	-	305.98	172.02	-
P(F)	0.000	0.000	0.000	-	-	0.000	0.000	-
R2	0.665	0.649	0.641	0.647	0.476	0.476	0.487	0.476
Panel B: Education Enrollment Model								
IDP	-0.012	-0.019	-0.033*	-0.008	-0.060***	-0.105***	-0.107**	-0.057***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.05)	(0.02)
M Yrschl	0.006***	0.006***	0.007***	0.007***	0.010***	0.009***	0.010***	0.010***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
M Yrschl	0.003***	0.004***	0.003***	0.003***	0.008***	0.008***	0.008***	0.008***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	74322	56835	28136	57893	64136	48171	23384	49048
χ^2	2096.64	2580.28	1421.15	2541.03	3614.16	2679.77	1442.17	2658.44
$P > \chi^2$	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Pseudo R^2	0.143	0.174	0.180	0.173	0.195	0.191	0.192	0.190

Note: Also controlled for age, race, family size, disability, employment status of father, automobile ownership, wall type, computer ownership, class of father's work, class of mother's work, mother's number of children, department, sex, municipality conflict index, municipality capacity index, and urban. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

and enrollment than non-migrants. Hence, one could argue that the gap between IDP children and non-migrant children could be attributed part to migrant selection. While the argument of selectivity has credibility for most migrant groups, IDPs and natural disaster migrants are different because they move as a result of exogenous factors. For example, based on our estimation of equation (4), we note no accumulation gap for children whose parents migrated for study, but we do note a gap for children whose parents migrated for work.[24] This divergent results for these two types of migrants reflects selectivity. On the other hand, migration due to conflict or natural disaster is linked to an exogenous shock. Although it may be safe to assume that selectivity is not a significant issue for IDPs and migrants linked to natural disaster, as long as not every person affected by the exogenous shock of conflict migrates, we cannot fully rule out issues of selectivity.

Another related argument supporting the decision to make comparisons only among migrants is that migration can lead to a disruption in schooling. Therefore, comparisons between IDPs and non-migrants may attribute to conflict the education gap simply created by migration itself. However, there are several problems with this argument. First, if there is no selectivity issue and all migrants faced equal disruption in schooling from the act of migrating, then ceteris paribus, we should see similar education gaps across all groups when compared to non-migrants because of disruption. Instead, we find that the education gaps differ significantly across groups, suggesting that it is the reason for migrating rather than simply disruption associated with migration that matters. This idea motivates our final question.

As noted in our empirical strategy, we focus this analysis on recent migrants to make it comparable to recent IDPs.[25] Table 7 summarizes the results for this analysis.

In Table 7, Panel A summarizes results using the education accumulation model, while Panel B summarizes results using the probability model. In columns (1) and (3), we use the sample of all migrants, and in columns (2) and (4), we restrict the sample to migrants living in municipalities with conflict index greater than 0.3. The results provide support for our earlier conclusion about the direct impact of conflict. We find that even when we compare IDPs to other migrants, who are a select group and on average have lower education accumulation and enrollment than non-migrants, we still notice gaps in enrollment and attainment among the 12-17 age cohort.[26] However, we find

[24]Estimates of the gaps between non-migrants and other migrant groups can be found in previous versions of this paper.

[25]In an earlier version of the paper, we also compared the recent IDPs to anyone who has migrated over their lifetime. We are able to find other migrants apart from the recent migrants over the last five years highlighted in the census by comparing where in individual lives now to were they were born. If these two municipalities are different, we include this person in the sample. We then compare IDPs to the groups of all other migrants.

[26]This gap is not created by the inclusion of migrants who move for study and are likely to have higher education outcomes than other migrants. To verify this, we drop these children from our analysis. Our estimated gap is not statistically different with or without this group.

Table 7: Do IDPs face an education gap compared to other migrants?

	Ages 6-11		Ages 12-17	
	All Migrants	Migrants conflict (> 0.3)	All Migrants	Migrants conflict (> 0.3)
	(1)	(2)	(3)	(4)
	Panel A: Education Accumulation Model			
IDP	-0.042	-0.067	-0.341***	-0.395***
	(0.04)	(0.06)	(0.09)	(0.14)
M Yrschl	0.037***	0.036***	0.092***	0.088***
	(0.00)	(0.01)	(0.01)	(0.01)
F Yrschl	0.020***	0.027***	0.050***	0.061***
	(0.00)	(0.00)	(0.01)	(0.01)
N	34457	11410	24124	8116
F	453.48	170.13	168.88	71.87
P(F)	0.000	0.000	0.000	0.000
R^2	0.700	0.684	0.492	0.481
	Panel B: Education Enrollment Model			
IDP	-0.010	-0.022**	-0.041***	-0.088***
	(0.01)	(0.01)	(0.02)	(0.03)
M Yrschl	0.004***	0.004***	0.009***	0.008***
	(0.00)	(0.00)	(0.00)	(0.00)
M Yrschl	0.003***	0.003***	0.007***	0.010***
	(0.00)	(0.00)	(0.00)	(0.00)
N	34459	11428	24155	8110
χ^2	1299.79	553.27	1646.86	690.64
$P > \chi^2$	0.000	0.000	0.000	0.000
Pseudo R^2	0.157	0.144	0.203	0.207

Note: Also controlled for age, race, family size, disability, employment status of father, automobile ownership, wall type, computer ownership, class of father's work, class of mother's work, mother's number of children, department, sex, municipality conflict index, municipality capacity index, and urban.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

that for children 6-11 years, the gap is very mall and in most cases not significant. We find that IDP children ages 12-17 are approximately 4.1% less likely to be enrolled in school compared to other migrants, compared to 6.3% less likely compared to non-migrants. When the sample of migrants is restricted to those in regions of high conflict, the estimated enrollment and attainment gaps persist and even grow.

The results from the analysis in Table 7 focused solely on migrants suggest that the education gap experienced by IDP children is not linked with being a migrant given the persistence of a gap when IDPs are compared to other migrants. Moreover, the results in Table 7 are consistent with earlier results in Tables 4 and 6 suggesting that living in a conflict region does not create similar effects on education outcomes as being directly affected by conflict. In addition, these results suggest that the channel that leads to lower school accumulation and enrollment in IDPs cannot be explained solely by disruption of schooling caused by migration. Rather, loss of income of parents, lower probability of employment of parents or care givers who become disabled because of conflict, and psychological factors linked with being directly affected by conflict are more likely explanations for this gap. Finally, our results suggest that there might be some selection bias with respect to migrants given that the gap between IDPs and migrants is generally smaller than when IDPs are compared with non-migrants.

7.3 Robustness checks: Are the results driven by sampling bias?

Table 8: Who are we capturing?

Age	Total	With Both Parents	Percentage
6-11	261,469	180,912	69.19%
12-17	250,349	158,347	63.25%

The analyses above suggest that being directly affected by conflict affects children's probability of being enrolled in school at both the elementary level and secondary level. It also affects their education accumulation. However, the effect on younger children is smaller and not always significant. On the other hand, the gaps for older children are consistently significant and of greater magnitude.

It is important to mention that including some important variables that predict education outcomes creates secondary effects. We control for parents' education, but not every child in the sample has this information available. Therefore, including parent education variables means that we do not use the whole sample of children ages 6-17. Table 8 highlights the percentage in each age range used in our above analysis. We use 69% of the sample ages 6-11 and 63% of the sample ages 12-17, raising the question of whether an analysis on the entire sample would lead to a different outcome.

To investigate this possibility, we estimate the bias created in our estimate of the gap if we do not include all the parent related variables. To find this bias, we first re-estimate our accumulation and enrollment models with and without the parental variables, including both migrants and non-migrants and restricting our sample to those who have parent information. The results from this analysis are summarized in Table 9. Notice that the results in columns (1) and (4) are a repetition of earlier analysis and the results in column (2) and (5) are the estimated coefficients if parental controls are not included for the same sample. The upward bias in the estimates is significant if we do not include parental controls. Specifically using the OLS model for children 6-11, without parental controls we overestimate the impact of direct contact with violence on school accumulation by 0.071 years and overestimate the probability of enrollment gap for those affected by violence by 1.3%.

For children 12-17, if we do not include parental controls, we overestimate the direct impact of violence on school accumulation by 0.18 years and overestimate the enrollment gap by 2.2%. Given our knowledge of the potential bias caused by not including parental controls, we re-estimate equation (2) without parental controls on the whole sample. This means that the observations that were not included previously because of missing information about parental controls are now included in the analysis. The results are summarized in Table 10 columns (1), (3), (5) and (7). Notice that for the children 6-11 years old, these estimates are very similar to the estimates we got for the sample that have parental information when parental controls were dropped and the gap estimated. For secondary school age children, the difference is slightly larger.

In columns (2), (4), (6) and (8), we present the corrected estimate of the education accumulation and enrollment gaps using the estimated effect of not including parental controls highlighted in Table 9. These bias correct estimates will be valid as long the distribution when we use the sample with parent control is no different from the distribution when we use the whole sample. One way to check for the non-selectivity of the sample without parental controls is to test for differences in means on a set of demographic and economic variables for the sample with parental controls and the whole sample, restricting these samples to children ages 6-11 and 12-17. We find that the means are almost identical for all variables except family size. For family size, the ages 6-11 sample with parental controls has a mean of 5.7, while the mean for the whole sample is 5.5. In the 12-17 years range, the sample with parental controls has a mean of 5.9, while the sample without parental controls has a mean of 5.3. The result of our means test suggests that the sample without parental controls is very similar to the sample with parental controls, particularly for the 6-11 age range. Figures 4 and 5 in the appendix capture the years of schooling kernel density function buttresses

Table 9: Education Gap: Parental controls v. no parental controls

Variable:	Education Accumulation Model			Probability Model		
	(1)	(2)	(3)	(4)	(5)	(6)
	Parental	No Parental	Difference	Parental	No Parental	Difference
	control	controls	(2) -(1)	controls	controls	(5) - (4)
			Panel A: Age 6-11 years			
IDP	-0.197***	-0.268***	0.071	-0.016*	-0.029***	0.013
	(0.04)	(0.04)		(0.01)	(0.01)	
N	171083	171083		171393	171393	
F	1635.96	1677.78				
$P > (F)$	0.000	0.000				
R^2	0.663	0.648				
χ^2				5709.09	5571.68	
$P > \chi^2$				0.000	0.000	
$PseudoR^2$				0.152	0.123	
			Panel B: Age 12-17 years			
IDP	-0.515***	-0.692***	0.177	-0.063***	-0.085***	0.022
	(0.09)	(0.10)		(0.02)	(0.02)	
N	148447	148447		148650	148650	
F	777.69	734.11				
$P(F) > 0$	0	0				
R^2	0.4741	0.4366				
χ^2				7444.77	7696.18	
$P > \chi^2$				0	0	
Pseudo R^2				0.1964	0.1752	

Note: Also controlled for sex, conflict, capacity, urban, mothers years of schooling and fathers years of schooling, age, race, family size, disability, employment status of father, automobile ownership, wall type, computer ownership, class of father's work, class of mother's work, mother's number of children, state and reasons for migration.
Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Base group: non-migrants.

this point. These figures illustrate the schooling distribution for IDPs and non-migrants who have parental information and those who do not. Figure 5 displays the distribution for children ages 6-11 and Figure 6 for ages 12-17. Notice that the gap in the schooling distribution between IDPs and non-migrants with or without parents information are similar especially for the 6-11 age group.

7.4 Potential econometric issues with estimating the impact of conflict using OLS

The estimation of the effect of conflict measured by the gap between IDPs and non-migrants using OLS potentially could be biased for several reasons. First, IDPs are migrants, and in general, analysis focused on migrants could be plagued with issues of selectivity. Migrants are a select group of people, and usually, looking at migrants' outcomes or the impact of migration on certain outcomes

Table 10: Estimated education gap between IDPs and non-migrants for samples with and without parental controls

Variable:	Age 6-11				Age 12-17			
	Yrs of sch.	Yrs of sch Corrected	Enroll	Enroll Corrected	Yrs of sch.	Yrs of sch Corrected	Enroll Enroll	Enroll Corrected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IDP	-0.264***	-0.193	-0.029***	-0.016	-0.669***	-0.492	-0.073***	-0.051
	(0.04)		(0.01)		(0.09)		(0.02)	
N	184128		184878		160686		161497	
F	1759.36		787.88					
$P(F) > 0$	0.00				0.000			
R^2	0.64				0.424	0.		
χ^2			8119.97				8735.37	
$P > \chi^2$		0.000		0.000				
Pseudo R^2			0.135			0.172		

Note: Also controlled for sex, conflict, capacity, urban, age, race, family size, disability, automobile ownership, wall type, computer ownership, state and reasons for migration.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

without controlling for selection could lead to biased estimates. However, IDPs are a unique group of migrants in that their migration is motivated by being directly affected by conflict. We can think of IDPs as involuntary migrants linked to exogenous forces. In contrast, migrants who moved for study, family reasons or work can be viewed as voluntary migrants linked to endogenous factors. Kirchhoff and Ibáñez (2002) note that not everyone in regions of high conflict migrates. Those who do leave have usually been directly affected in a significant way by the conflict, having lost family or property or received threats of such. The authors, show that 58.2% of IDPs surveyed received a death threat before migrating. In contrast, only 9.1% of those who did not migrate living in the same high conflict region that the IDPs migrated from had received a death threat.

To illustrate that IDPs are not very different from other individuals in the communities they live or are associated with, we run a regression of all our controls on the probability of being an IDP. Our results are summarized in Table 15 in the appendix. In the first three columns, we compare IDPs to everyone else, and in the last three columns, we compare IDPs to non-migrants. In columns (1) and (4) we use current municipality fixed effects; in columns (2) and (5) we instead use birth municipality place fixed effects; in columns (3) and (6) we use last residence municipality fixed effects. When the estimated coefficients are 0, we insert the word 'same' to show that IDPs are no different from comparison group. Notice that with respect to age, sex, family size, ownership of a computer, and ownership of a car, IDPs are no different than anyone else from their current, birth or last resident municipality. For most of the variables for which IDPs are slightly different, such as mother having wage work, disability, father being unemployed, and having lower grade wall materials, we see that the variables are likely endogenous to being directly affected by conflict. Notice that in most specifications, parents' education is not a predictor of being an IDP. The only exogenous variable that is significant is race. Table 15 suggests that Blacks and Indigenous groups are more likely to be IDPs, although the difference is fairly small (0.2%-0.4%). We also find that IDPs are more likely to be in some states than the others[27]. The basic inference from this table is that although IDPs are not evenly distributed in the population, they do not look very different from others within the communities they come from or live in. When they do differ, it is expected because of the effect of conflict on certain characteristics. Race is the only variable for which we see some selection, but we control for race in all our analyses.

Given the uneven even distribution of IDPs across states, an OLS estimation of the attainment and enrollment gaps between IDPs and non-migrants is likely to be biased. This is because of potential omitted variables that differ at the community level, affect enrollment or attainment, and are distributed differently across IDPs and nonmigrants. For example, if IDPs are more likely to live in communities with lower access to schools or lower social capacity and infrastructure, then the estimated gap compared to non-migrants could be capturing these effects and not the direct effect of conflict. Although we control for the impact of capacity and conflict on the education outcomes using the HSRI conflict and capacity sub-indices, the use of these composite indexes may not be an effective control for the differences in conflict and social and economic capacity across municipalities. Moreover, capacity in region of birth or last residence, which we have not controlled for, may be more important than capacity in the current region. To provide evidence that our results are not driven in part by measurement error or omitted variables that vary at the municipality level, we introduce fixed effects. This allows us to control for all possible omitted variables that could affect our outcomes of interest but vary at the municipality level.

Another potential issue is the inclusion of potentially endogenous variables in an attempt to proxy for income. Although we deliberately do not include homeownership in our specifications as a proxy for wealth despite its relevance because it is endogenous, one could argue that our other wealth indicators could also be endogenous.[28] We defend the use of proxies such as car and computer ownership because of their inability to predict being an IDP. However, other variables such as father's and mother's employment status could also be endogenous but are important to control for because

[27] Dummy coefficients for states not displayed in table 15

[28] The endogeneity of homeownership is linked with the fact that IDPs are less likely to own their own home given their recent displacement.

they affect children's schooling, especially at the secondary level when education is not free.

Another variable that several authors have noted to be endogenous when included in an accumulation or enrollment model estimation is conflict. Conflict could be correlated with individuals being poor or living in an area with low levels of social capacity. Because both of these factors are important for human capital investment, the estimated effect of conflict could be inconsistent and overstated if we do not address the potential endogeneity in the conflict variable. Although the level of conflict in a region is merely a control rather than our primary variable of interest, the conflict index is used to define the exposure to conflict dummy for our first question. We try to avoid the potential bias in estimating the impact of exposure to conflict by controlling for the capacity in a municipality and also by including poverty correlates and wealth indicators.[29]

Rodriguez and Sanchez (2012) highlight another potential channel of omitted variable bias in estimating the impact of conflict on dropout rate.[30] They suggest that although exposure to conflict affects school enrollment, pressure to drop out of school also affects dropout rates and is correlated with conflict. Therefore, it is possible to attribute to conflict the impact of this pressure. Although Rodriguez and Sanchez (2012) use lagged homicide capture rates as an instrument, this variable may not satisfies exclusion restrictions.[31] One possible way to deal with this omitted variable which we explore in our paper is to restrict the sample to high conflict communities. In these communities, we can assume that the pressure to drop out should on average be the same. Hence, the estimated enrollment and attainment gaps for IDPs in comparison to non-migrants living in these high conflict region will not be upward biased because the missing variable has similar distribution across both groups. Still, high conflict communities could differ with respect to other community characteristics correlated with being an IDP.

A better way to deal with most of the kind of biases noted above is to introduce community level fixed effects. Rodriguez and Sanchez (2012) note that pressure to migrate depends on level of conflict. This measure is calculated at the municipality level. Moreover, social and economic infrastructure and other related variables that potentially could affect enrollment and accumulation do not vary significantly within a municipality.

It is important to mention that the use of municipality fixed effects does not deal with any omitted variable that varies within the municipality and is correlated with IDP status. We have controlled for most such variables in our analysis, such as race and parent's education. However, there might exist other variables that are distributed differently across IDPs and others within a

[29]More on the capacity index can be found in Table 17 in the appendix.
[30]Enrollment, our focus in this paper, is inversely related to dropout rate.
[31]See Staiger and Stock 1997 for more on weak instruments.

community that we have not controlled for directly or indirectly. Although we cannot readily think of a variable that fits this category, we cannot rule out this possibility.[32]

7.5 Robustness check: Fixed Effects

The results estimating our accumulation and enrollment models using fixed effects technique are summarized in Tables 11 and 12. In Table 11, we estimate fixed effects models based on an individual's current municipality of residence. Panel A summarizes the attainment model, and panel B summarizes the enrollment model. Columns (1) and (2) show estimates of the education gap comparing IDPs with non-migrants, while columns (3) and (4) show estimates comparing IDPs with other migrants. The analysis comparing IDPs with non-migrants is our preferred analysis, with the analysis comparing IDPs and migrants serving as a check for the sensitivity of our results.

The results in Table 11 columns (1) and (2) confirm our earlier OLS results of the impact of conflict on attainment. These results suggest that IDP children ages 6-11 have 0.17 fewer years of schooling than non-migrants, and IDP children ages 12-17 have almost half a year less. If we look solely at migrants, we do not find any evidence of a gap among younger children but we find evidence of a gap of about 0.32 years among older children. We are unable to use the fixed effect technique for our enrollment model because there does not exist an adequate statistic that allows the fixed effects to be conditioned out the likelihood and estimates based on assuming an uncondition fixed effect are biased (see Stata manual on fixed effect and probit models for more details).

An imperfect alternative is to estimate a linear probability model using the fixed effects technique. Although the linear probability specification of the binary choice model provides ease of interpretation, unless restrictions are placed on estimates, coefficients can imply probabilities outside the unit interval. These estimates offer evidence of an enrollment gap for children ages 12-17 but not for children ages 6-11. We are more cautious interpreting these estimates given the potential issue with the linear model.

In the above analysis, we employ fixed effects that allows us compare individuals within their current municipality. However, it is possible to argue that birthplace municipality characteristics matter more than current municipality characteristics. Comparing IDP children with other children who live in the same municipality might not be as informative as comparing them with other children who lived in the same municipality where they were born.

Given this possibility, we redo the analysis creating fixed effects at the birthplace municipality.

[32]One variable that affects education outcomes generally that we have not controlled for is ability. We do not worry about variables such as this because while they can affect education outcomes, they can be assumed to be evenly distributed across the population, regardless of IDP status and hence cannot affect deriving a consistent estimate of the attainment or enrollment gap.

Table 11: Does being directly affected by conflict create an education gap (Fixed Effects)?

Variable:	IDPs versus non-migrants		IDPs versus other migrants	
	6-11 years	12-17 years	6-11 years	12-17 years
	(1)	(2)	(3)	(4)
	Panel A: Attainment Model			
IDP	-0.169***	-0.486***	-0.029	-0.318***
	(0.04)	(0.09)	(0.04)	(0.09)
Mother Yrs sch.	0.043***	0.105***	0.037***	0.088***
	(0.00)	(0.00)	(0.00)	(0.01)
Father Yrs sch.	0.020***	0.053***	0.020***	0.047***
	(0.00)	(0.00)	(0.00)	(0.01)
R-squared	0.672	0.487	0.710	0.513
N	170955	148396	34447	24120
	Panel B: Enrollment Model			
IDP	-0.021	-0.066***	-0.027*	-0.062***
	(0.02)	(0.02)	(0.02)	(0.02)
Mother Yrs sch.	0.006***	0.008***	0.004***	0.009***
	(0.00)	(0.00)	(0.00)	(0.00)
Father Yrs sch.	0.003***	0.006***	0.004***	0.006***
	(0.00)	(0.00)	(0.00)	(0.00)
N	171828	148890	34577	24179
R^2	0.123	0.188	0.111	0.190

Note: Also controlled for age, sex, sector, race, family size, disability, employment status of father, automobile ownership, wall type, computer ownership, class of father's work, class of mother's work, mother's number of children. In column (1) and (2) we control for migration status so dummies for other types of migrants are estimated but not presented. The base group is non-migrants in column (1) and (2). In column (3) and (4) we have a binary indicator $IDP = 1$ and Other $migrants = 0$
Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This allows us to compare IDP children with other children who have the same initial municipal level influences or factors. The results are summarized in Table 12 panel A. Another alternative is to compare IDPs with those from the same municipality of last residence. Although for many migrants, birthplace municipalities are the same as the last place of residence municipality, differences exist for some individuals and for such children, comparisons to other children in their last place of residence is more informative. The results using last place of residence municipalities are summarized in the panel B of Table 12 and are our preferred estimates.

The results in Table 12 confirm our OLS estimated impact of conflict for our attainment model. Specifically, we find a gap of approximately one fifth of a year of schooling (0.18-0.19) for children ages 6-11, and a little less than half a year of schooling (0.41-0.45) for children ages 12-17. These results show that selection bias caused by community level characteristics is not a significant issue

Table 12: Does being directly affected by conflict create an enrollment gap (Estimates using birthplace and past residence fixed effects)?

Variable:	IDPs versus non-migrants		IDPs versus other migrants	
	6-11 years	12- 17 years	6-11 years	12- 17 years
	(1)	(2)	(3)	(4)
(Panel A: Attainment GAP- Birth Place)				
IDP	-0.175***	-0.447***	-0.009	-0.270***
	(0.04)	(0.09)	(0.04)	(0.09)
R-squared	0.671	0.487	0.712	0.521
N	170955	148396	34447	24120
(Panel A: Enrollment GAP-Birth Place)				
IDP	-0.011	-0.035***	-0.013	-0.046**
	(0.01)	(0.02)	(0.01)	(0.02)
R-squared	0.126	0.190	0.146	0.210
N	171828	148890	34577	24179
(Panel B: Attainment GAP- Last Place of Residency)				
IDP	-0.186***	-0.405***	-0.020	-0.256***
	(0.04)	(0.08)	(0.04)	(0.09)
R-squared	0.672	0.488	0.711	0.522
N	170955	148396	34447	24120
Panel B: Enrollment GAP-Last Place of Residence				
IDP	-0.014	-0.038***	-0.011	-0.033*
	(0.01)	(0.02)	(0.01)	(0.02)
R-squared	0.129	0.192	0.145	0.213
N	171828	148890	34577	24179

Note: Also controlled for age, sex, sector, race, family size, disability, employment status of father, automobile ownership, wall type, computer ownership, class of father's work, class of mother's work, mother's number of children. In column (1) and (2) we control for migration status so dummies for other types of migrants are estimated but not presented. The base group is non-migrants. In column (3) and (4) we have a binary indicator $IDP = 1$ and Other $migrants = 0$
Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

with OLS estimates once appropriate controls are included, as the comparable estimates from Table 4 are 0.20 and 0.52 years respectively. In contrast, we find some evidence of selectivity bias in the basic enrollment estimates.

Specifically, when we include fixed effects and compare IDP children to those from their previous communities, we find no enrollment gap for the 6-11 age cohort. In contrast we find a gap when using either a probit model or linear probability model without including fixed effects. At the secondary level, we also find evidence of selection bias with OLS. With fixed effects included, we find a much smaller enrollment gap of 3.8% compared to 6.9% using a linear probability model without fixed effects.[33]

[33] Our earlier estimates from the enrollment model are derived using a probit model and are not directly comparable to the linear probability fixed effect estimates. The estimated gap with a probit model not accounting for selection suggests a 6.3% gap.

When we compare IDPs with migrants using our fixed effect specification, we find no evidence of a gap in attainment or enrollment for children 6-11years. For the older children, we find a gap in attainment of about 0.26 years of schooling. This gap is smaller than the 0.4 years of schooling gap when we compare IDPs with non-migrants (our preferred specification) but this finding comes as no surprise given the selectivity of migrants.

7.6 Robustness check: Matching Estimators

As an alternative check on our results, we employ a matching estimation technique and focus solely on the education attainment gap. We assume being an IDP as the treatment and estimate the average effect of the treatment on the treated. We compare the outcome of IDPs with those of matched non-migrants using a nearest neighbor matching estimation. We choose to exclude other migrants from this analysis because of the selectivity issues surrounding most migrants. One advantage of matching estimators is that it typically does not require one to specify the functional form of the outcome equation. This reduces the potential for mispecification bias. We match IDPs with non-migrants based on several characteristics (see notes to Table 13) and focus only on the average treatment effects on the treated (ATT). In columns (1) and (4) we match using current municipality; in columns (2) and (5) we match using birth municipality; in columns (3) and (6) we match using last residence municipality. Our rationale for not estimating the average treatment effect (ATE) is the likelihood that ATE estimate would not be consistent because finding adequate matches for the control group would be difficult given the large sample size of this group compared to the treatment (IDPs are less than 5% of the sample). The results are summarized in Table 13 and though larger in magnitude, the estimates are still close to those derived using OLS and the fixed effects models. We find an ATT of being displaced of about one fourth of a year for children at the primary level and just over half a year for children at the secondary level. Our estimates are similar whether we match children with those born in their birth place municipality, last residence municipality, or current municipality. However the matching estimator is limited by the variables we use in the match. If IDPs differ on unobservables or variables that we have not included in our match but affect schooling outcomes, our ATT could still potentially be biased.

7.7 Robustness check: IV Estimates

Our earlier estimations all suggest a significant impact of conflict on education outcomes. However, the above techniques do not deal with potential within municipal level unobservable variables that can affect schooling outcomes and are distributed differently across IDPs and non-migrants. One way of dealing with this potential endogeneity is to employ instrumental variable (IV) analysis.

Table 13: Does being directly affected by conflict create an education gap (Matching Estimates)?

Variable:	Age 6-11			Age 12-17		
	Age 6-11			Age 12-17		
	Match current Municip.	Match birth Municip.	Match last res. Municip.	Match current Municip.	Match birth Municip.	Match last res. Municip.
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.249***	-0.263**	-0.261***	-0.557***	-0.531***	-0.554
	(0.037)	(0.037)	(0.039)	(0.075)	(0.075)	(0.079)

Note: Matched based on birth municipality or current municipality, age, gender, race, level of conflict exposed to currently, family size, education of mother, education of father, owns a home, race, disability, employment status of mother, employment status of father, lives in urban area, number of children of mother.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Given the challenge of finding an instrument that is both relevant and satisfies exclusion restrictions, we experiment with multiple instruments. These instruments are conflict index in the last place of residence, level of exposure to coca production at the time of migration, and number of massacres, conflict-related deaths, and conflict-related captures at the time of migration. The coca production instrument is constructed based on hectares of coca grown in the municipality of last residence for individuals reporting living in a rural area.[34] We obtain data from 2001-2005 from the Coca Cultivation Survey conducted by the government of Colombia and the United Nations Office on Drugs and Crime in Colombia. One limitation in the construction of this instrument is that we only have information on coca production from 2001-2005 at the municipal level, so we cannot capture the exact exposure level for those who migrated from rural parts of municipalities in 2000. For these individuals, we impute the 2001 levels, as this is the closest date available.[35]

Our second potential IV is constructed using the conflict index in a migrant's last place of residence. The conflict index is defined in the data section, and variables that it captures can be found in Table 17 of the appendix.

Our third potential instrument is exposure to capture, a hallmark of the conflict in Colombia. Past research suggests that many of the displaced have family or community members who have been captured, kidnapped, or threatened with such. Data on captures at the municipality level come from CEDE, described in the data section. We define exposure to capture as the number of

[34] Even within a municipality, individuals can be in either rural or urban sector.
[35] This implies that we impute for those who migrated in 2001 and 2000,the same level of cocoa exposure.

people captured in the municipality of last residence during the year of migration.[36]

Our final potential instrument is exposure to conflict related deaths in municipality of last residence. This measure is defined as homicides, massacres, and deaths linked to the various armed groups.[37] We construct our exposure to conflict-related deaths measure in the same way that we construct our exposure to captures variable. We choose to consider all deaths related to violence versus just armed conflict related deaths for two reasons. First armed conflict related death in place of last residence at time of migration may not satisfy exclusion restrictions because conflict related death could be correlated with this area being rural or urban which affects access to education. In contrast, all violence related deaths in ones place of past residence is not correlated to an area being rural or urban given the prevalence of homicides by common criminals in urban areas. Second, although we can identify homicides by common criminals and death related to certain armed groups, there a significant number of conflict related deaths in the data that are identified as unknown meaning that the police were unable to identify if the death was linked with just violence or the conflict. Given these two limitation an instrument using only deaths identified as linked with armed conflict is not preferable.

Although these four potential instruments seem relevant and somewhat exogenous, we conduct several weak instrument tests. Specifically, we calculated the partial R^2 test of instruments, F statistic and tests of Stock and Yogo and Shea Partial R^2.

It is possible to argue that the conflict index and coca exposure IVs are less like to be exogenous. For example, coca growth takes place in the rural areas where it can be hidden among other agricultural crops and although we know if an individual's current location is rural or urban, we do not have complete information on whether a migrant's past location is urban or rural. We do not observe the availability of schools in an individual's past location but know that rural areas are less likely to have reliable access than urban areas. Because coca exposure could also indicate less access to schooling and both are more likely in rural areas, coca production as an instrument may not satisfy exclusion restrictions. Likewise, the conflict index in an individual's last location may be slightly problematic because the index captures more recent levels of conflict versus past levels 3-5 years ago, limiting its power as an instrument predicting IDP status. Moreover, in principle one could argue that it may not satisfy exclusion restrictions because some of the variables captured in

[36]For migrants who do not record their year of migration, we include the average capture level in their municipality of last residence and for non-migrants, 2005 levels of capture are imputed. For migrants from abroad we impute 0 level of capture exposure and for migrants with missing and non listed municipalities of last residence (very few), we impute the capture numbers for municipalities in the CEDE data not matched to specific municipalities in the census data. This approximation may increase the noise in our IV measure and reduce the explanatory power but also decreases the probability that our exclusion restriction is not satisfied.

[37]The various groups include the FARC, AUC, ELN, common criminals, and unknown.

this index have similar issues to the coca exposure IV.

We are less worried about the possibility that capture and conflict death exposure IVs will not satisfy exclusion restrictions. Still, one could argue that capture may be negatively correlated with an area being urban or rural as government infrastructure such as police presence may be more active in urban areas. At the same time, living in a rural area may be linked to lower access to schooling, which is an unobservable. However, we do not feel that this is a significant given concrete evidence of capture in both urban and rural areas in Colombia and the lack of negative correlation between capture and living in an urban area in our data.

Moreover, we control for the capacity in an individual's past location in our IV analysis which can also help to eliminate the aforemention potential problem.[38] With respect to conflict death exposure, it is also possible to argue that conflict related death may be correlated with police presence in an area or infrastructure in an area which may also be correlated with schooling outcomes. As with capture, this should not be an issue because we control for capacity in past location which includes police presence. Moreover, we do not find a positive correlation with being in a rural area and our exposure to death variable. Although both these two instruments in principle have the potential to predict displacement, exposure to violence related deaths is more likely to me noisy and have less explanatory power given the noise in the variable caused by homicides linked with just crimes and other factors. In contrast, most captures are related directly to the conflict situation in Colombia and capture is not correlated significantly to an area being rural or urban. While both instruments have the potential to be good predictors for displacement, we prefer the measure of exposure to captures because it is most relevant to the conflict in Colombia.

Results from the IV fixed effect estimations and the various tests we carried out are summarized in Table 14. Panel A summarizes the results for children ages 6-11, and panel B summarizes the results for children ages 12-17. For compactness of presentation we only show the relevant variables. Specifically, we show the first stage estimate of the impact of the instrument(s) and for the second stage, we show the estimate of the education attainment gap. We include some tests for weak instruments, although not all tests conducted are displayed. Columns (1)-(4) use the coca production IV, conflict index IV, capture exposure IV, and death exposure IV, respectively. Notice that only the capture IV adequately passes all of the weak instrument tests for both age groups being considered. The death exposure IV is a weak instrument because it does not pass the F test basic rule of thumb of an F above 10 for the 6-11 age group. Because the capture IV is the best of our 4 instruments, we combine it with each of the other instruments and conduct an overidentification test. When we combine capture with either coca area or conflict index in last residence, the test

[38] As showed in the appendix, the capacity index includes a measure of police presence.

for the validity of the overidentification restriction is rejected. However, when we combine capture with conflict-related deaths, this restriction is not rejected. Column (5) of Table 14 summarizes our results combining the death exposure and capture IVs together and is our preferred specification. The results in Table 14 column (5) are consistent with our earlier results using the fixed effects model. Although the coefficients are slightly smaller, (-0.13 and -0.42), they are not statistically different from the coefficients using both the fixed effect model using birth place and last place of residence.

It is important to mention that although we implement an IV analysis, we cannot reject the hypothesis that being an IDP is exogenous in some instances. This is important to mention because if a variable is exogenous and we assume it is endogenous and implement an IV, the IV estimators, though still consistent, can be much less efficient than the OLS estimators. We use the Durbin-Wu-Hausman test of endogeneity assuming that being an IDP is endogenous and instrument using capture exposure (our preferred IV). This test shows that while we can reject exogeneity for the younger children, we cannot reject that variables are exogenous for the 12-17 age group. Given these mixed results of our test for exogeneity, we are of the opinion that an IV analysis is useful hence our implementation. In addition, since the results from our preferred IV analysis are only slight lower than the fixed effects and are not statistically different, we also can infer that our fixed effect analysis does not suffer from potential within community selection on unobservable and our FE estimates are not significantly biased.

8 Summary and Conclusion

In this paper we try to answer four questions related to the impact of conflict. This paper is motivated by the idea that in low-intensity conflicts, not every person living in a supposedly high-conflict area is equally affected. While past research tends to look at the impact of exposure to conflict alone on education, we investigate the question of how being directly affected by conflict (i.e. being an IDP) affects education outcomes. Our first question asks if there is an education gap for children living in municipalities with high conflict in comparison to those living elsewhere. We present this analysis as a motivation for the importance of estimating the impact of conflict by looking at those directly affected. Second, we ask if being directly affected by conflict leads to an education gap for children. Third, we ask if living in a high-conflict municipality creates a similar education gap as being directly affected by conflict. Finally, we ask how IDPs compare to other migrants with regard to education outcomes.

First, we make use of OLS and probit analysis to estimate our accumulation and enrollment mod-

Table 14: What is the effect of conflict on education attainment (IV Fixed Effects Estimates)?

	(1) coca hectares	(2) conflict index	(3) capture exposure	(4) homicide exposure	(5) capture & homicide
Variable:			Panel A Age 6-11		
			First Stage IV estimate		
IV 1	4.11e-06***	0.08	0.005***	0.0006***	0.006***
	(0.0)	(0.024)	(0.0002)	(0.0002)	(0.0003)
IV 2					-0.001***
	NA	NA	NA	NA	(0.0002)
Partial R-sq of IV	0.004	0.002	0.118	0.003	0.128
Shea Partial R2	0.004	0.002	0.118	0.003	0.128
F-stat	89.69	11.00	652.49	7.84	245.59
Prob > F	0.00	0.001	0.00	0.005	0.0
			Second Stage Stage IV estimate		
IDPvs non-migrant	-1.45***	1.33*	-0.12*	0.06	**-0.127*** **
	(0.427)	(0.794)	(0.067)	(0.48)	(0.06)
Hansen J statistic	NA	NA	NA	NA	0.158
Chi-sq(1) P-val	NA	NA	NA	NA	0.6910
Variable:			Panel B Age 12-17		
			First Stage IV estimate		
IV 1	4.62e-06***	0.117***	0.005***	0.001***	0.006***
	(0.0)	(0.0002)	(0.0003)	(0.0003)	
IV 2					-0.001***
					(0.002)
Partial R-sq of IV	0.005	0.003	0.122	0.005	0.129
Shea Partial R2	0.005	0.003	0.122	0.005	0.129
F-stat	78.16	17.26	474.82	11.73	181.88
Prob > F	0.00	0.00	0.00	0.001	0.00
			Second Stage Stage IV estimate		
IDPvs non-migrant	-4.091***	1.17	-0.39***	0.271	**-0.422*** **
	(0.853)	(1.03)	(0.136)	(0.794)	(0.131)
Hansen J statistic					0.943
Chi-sq(1) P-val					0.3316

Note: Matched based on birth municipality or current municipality, age, gender, race, level of conflict exposed to currently, family size, education of mother, education of father, owns a home race, disability, employment status of mother, employment status of father, lives in urban area, number of children of mother. Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

els respectively. Then, given the potential endogeneity or selection on unobservables with respect to our variables of interest, we explore fixed effects specifications. The possible though unlikely selection on unobservables within a community that affect schooling and could be distributed differently for IDPs and non-migrants leads us to our IV estimation. We explore different instruments, but our instrument of choice is exposure to conflict-related capture. We find that though there appears to be a slight gap in enrollment and accumulation for children of those who live in municipalities with high conflict in comparison to others, this gap is not robust.[39]. In contrast, we find once we control for selection, IDP children ages 6-11 have about one fifth fewer years of schooling than non-migrants, and IDP children ages 11-17 have about one half fewer years.[40] For our estimated enrollment gap, when we control for selection, we find no evidence of an enrollment gap for children of IDPs ages 6-11. For children 12-17 we find that they are between 3.5%-6.6% less likely to be enrolled in school than non-migrants.

Our results also suggest that living in a municipality with very high conflict does not lead to similar attainment and enrollment effects as being directly affected by conflict for children 12-17 years. For children ages 6-11, we find similar enrollment for those in high conflict regions and IDPs but attainment differences exist. Finally, we find that when we compare children of IDPs to children of other migrants, we find no enrollment and attainment gaps at the primary level, but we find both an attainment and an enrollment gap for the 12-17 age cohort. The magnitude of the gap is somewhat smaller than when we compare IDPs to non-migrants.

What can we infer from these results? First, because being directly affected by conflict negatively affects a child's education accumulation and enrollment, and mere exposure to conflict as captured by living in an area with high conflict does not seem to have similar impact on a child's education accumulation and enrollment, there is a need for caution when interpreting the latter as a measure of the impact of conflict. Second, our study highlights the need to provide a clear picture of the impact of conflict by looking at those directly affected by conflict, especially in an environment of low-intensity conflict. Given the growing literature that estimates the impact of conflict using exposure, our results suggest that isolating those directly affected by conflict and collecting more data on their specific experiences is useful. This kind of data would be important in helping to create a framework for better understanding the channels through which being directly affected by conflict affects education and other labor market outcomes. Finally, although the above suggestions are relevant for studies looking at types of conflict similar to that in Colombia, it is possible that in

[39] We find that children in municipalities with conflict above the mean are 0.1% less likely to be enrolled in school and have 0.036 fewer years of schooling

[40] Specifically our estimates for children 6-11 ranged from 0.13-0.19 fewer years depending on specification, while for the older children ranged from 0.41-0.45 fewer years.

the context of a high-intensity civil or interstate war, exposure on its own might be as important as being directly affected.

References

[1] 2009 Global Trends: Refugees, Asylum-seekers, Returnees, Internally Displaced and Stateless Persons. (2010). United Nations High Commissioner for Refugees. Retrieved August 18th 2011 `http://www.unhcr.org/4c11f0be9.pdf`

[2] Akresh, Richard and Damien de Walque (2008). Armed Conflict and Schooling: Evidence from the 1994 Rwandan Genocide. The World Bank, Policy Research Working Paper Series: 4606.

[3] Angrist, Joshua, Eric Bettinger, and Michael Kremer (2006). Long-Term Consequences of Secondary School Vouchers: Evidence from Administrative Records in Colombia. *The American Economic Review*, 96(3), 847-862.

[4] Angrist, Joshua, Eric Bettinger, Erik Bloom, Elizabeth King, and Michael Kremer (2002). Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment. *The American Economic Review*, 92(5): 1535-1558.

[5] Barrera-Osorio, Felipe, and Ana Maria Ibáñez (2004). Does Violence Reduce Investment in Education?: A Theortetical and Empirical Approach. *Documentos CEDE 002382*, Universidad de los Andes-CEDE.

[6] Chamarbagwala, Rubiana, and Hilc'as E. Morán (2011). The human capital consequences of civil war: Evidence from Guatemala. *Journal of Development Economics*, 94(1): 41-61.

[7] Dueñas, Lusia Fernanda and Fabio Sanchez (2007). Does Armed Conflict Affect the Poorest?: The Case of School Dropout in Eastern Colombia. Working Paper, Universidad de los Andes.

[8] Duryea, Suzanne, David Lam, and Deborah Levison (2001). Effects of Economic Shocks on Children's Employment and Schooling in Brazil. *Journal of Development Economics*, 84(1):pp 188-214.

[9] Evans, David K., and Edward Miguel (2007). Orphans and Schooling in Africa: A Longitudinal Analysis. *Demography*, 44(1): 35-57.

[10] Glewwe, Paul, and Hanan Jacoby (1994). Student Achievement and Schooling Choice in Low-Income Countries: Evidence from Ghana. *The Journal of Human Resources*, 29(3): 843-864.

[11] Gaston, Noel, and Jaime Tenjo (1992). Education Attainment and Earnings Determination in Colombia. *Economic Development and Cultural Change*, 41:pp 125-139.

[12] Ibánez, Ana Maria, and Andres Moya (2006). Como el Desplazamiento Forzado Deteriora el Bienestar de los Hogares Desplazados?: Analisis y Determinantes del Bienestar en los Municipios de Recepcion. Documento CEDE No. 2006-26. Universidad de los Andes.

[13] Ibánez, Ana Maria, and Andrea Velásquez, (2006). "El Proceso De Identificación De Víctimas De Los Conflictos Civiles: Una Evaluación Para La Población Desplazada En Colombia," Documento CEDE No. 2006 002537, Universidad de los Andes.

[14] Ibáñez, Ana Maria, and Andres Moya (2009). Vulnerability of Victims of Civil Conflicts: Empirical Evidence for the Displaced Population in Colombia. *World Development*, 38(4): 647-663.

[15] Jacoby, Hanan G., and Emmanuel Skoufias (1997). Risk, Financial Markets, and Human Capital in a Developing Country. *Review of Economic Studies*, 64(3): 311-335.

[16] Kirchhoff, Stefanie, and Ana Maria Ibáñez (2002). Displacement due to Violence in Colombia: Determinates and Consequences at the Household Level. Discussion Papers 18754, University of Bonn, Center for Development Research (ZEF).

[17] Lozano-Gracia, Nancy, and Gianfranco Piras, Ana Maria Ibáñez, and Geoffrey J.D. Hewings (2010). The Journey to Safety: Conflict-Driven Migration Flows in Colombia. *International Regional Science Review*, 33(2): 157-180.

[18] McKenzie, David, and Hillel Rapoport (2010). Can Migration Reduce Educational Attainment? Evidence from Mexico. *Journal of Population Economics*, 24(4): 1331-1358.

[19] Poveda, Alexander Cotte, and Jin Anthony Cotrino Sossa (2006). Crecimiento Econ—mico y Distribuci—n del Ingreso en Colombia: Evidencia Sobre el Capital Humano y el Nivel de Educaci—n. Serie de Documentos en Economia y Violencia 002221, Centro de Investigaciones en Violencia, Instituciones y Desarrollo Economico (VIDE).

[20] Psacharopoulos, George & Velez, Eduardo, (1992). Schooling, Ability, and Earnings in Colombia, 1988, *Economic Development and Cultural Change*, University of Chicago Press, vol. 40(3), pages 629-43, April.

[21] Psacharopoulos, George & Eduardo Velez (1993). Educational Quality and Labor Market Outcomes: Evidence from Bogota, Colombia. *Sociology of Education*, 66(2): 130-145.

[22] Psacharopoulos, George. (1994). Returns to Investment in Education: A Global Update. World Development 22(9): 1325-43.

[23] Rodriguez, Catherine and Fabio Sanchez (2012). Armed Conflict Exposure, Human Capital Investments and Child Labor: Evidence from Colombia. *Defence and Peace Economics*, 23(2): 161-184.

[24] Staiger, D. and Stock, J., (1997). Instrumental Variable Regression with Weak Instruments. *Econometrica* 65:557-586.

[25] Shemyakina, Olga (2011). The Effect of Armed Conflict on Accumulation of Schooling: Results from Tajikistan. *Journal of Development Economics*, 95(2): 186-200.

[26] Thomas, Duncan, Kathleen Beegle, Elizabeth Frankenberg, Bondan Sikoki, John Strauss and Graciela Teruel (2004). Education in a Crisis. *Journal of Development Economics*, 74 (1):53-85. 2004.

[27] UNHCR (2006). The State of the World's Refugees. New York: Oxford University Press.

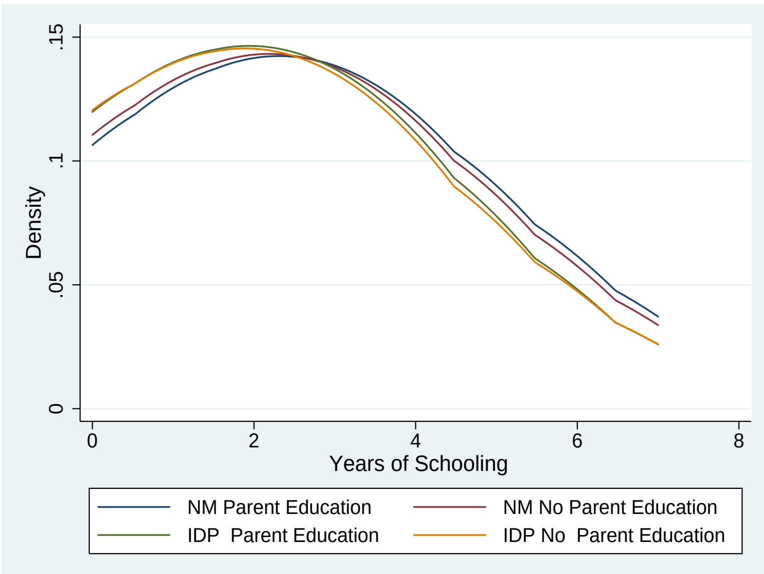

Figure 4: Kernel Densities for IDPs and Nonmigrants for Sample with and without parental variables 6-11 years

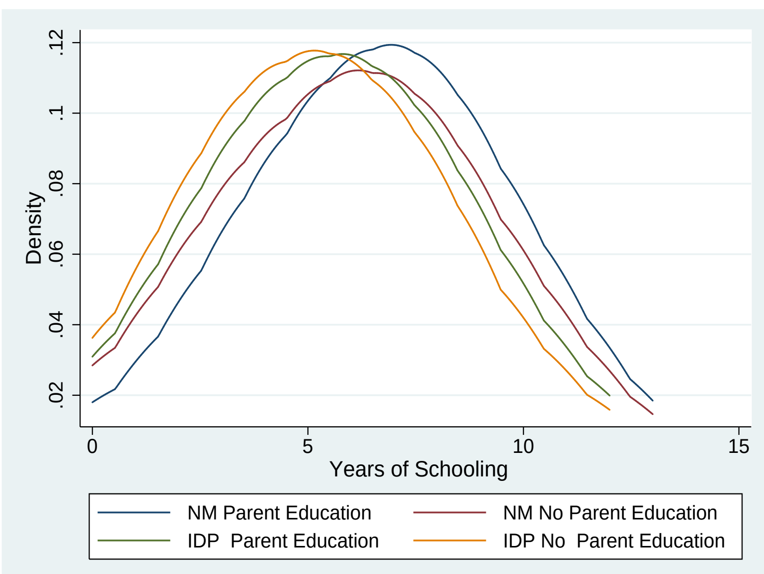

Figure 5: Kernel Densities for IDPs and Nonmigrants for Sample with and without parental variables 12-17 years

Appendix

Table 15: Are IDPs different? (Linear Probaility Model)

Variable:	IDP vs Everyone Else			IDP vs Non-migrants		
	current place residence	birthplace	last place residence	current place residence	birthplace	last place residence
	(1)	(2)	(3)	(4)	(5)	(6)
Mom yrs of sch.	Same	Same	Same	-0.001***	Same	Same
		(0.00)		(0.00)		
Dad yrs of sch.	Same	Same	Same	-0.001***	Same	Same
		(0.00)		(0.00)		
Black	0.002**	0.001	0.001	0.003**	0.001	0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Indigenous	0.001	0.003**	0.004***	0.001	0.003**	0.004***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Other race	-0.006**	-0.006*	-0.005*	-0.007**	-0.006*	-0.005*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Capacity		-0.021***	-0.023***		-0.035***	-0.042***
		(0.00)	(0.00)		(0.01)	(0.01)
Urban	0.004***	0.007***	0.007***	0.005***	0.009***	0.008***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Not Disabled	-0.004**	-0.004**	-0.004**	-0.005**	-0.005**	-0.004**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unemployed	0.015***	0.015***	0.014***	0.020***	0.020***	0.015***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Zinc/cloth wall	0.027***	0.023***	0.021***	0.032***	0.026***	0.023***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Cement Wall	-0.003	-0.006	-0.008*	-0.004	-0.008	-0.011*
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Prefrabir-	-0.002	-0.006	-0.009*	-0.003	-0.009	-0.010*
cated (0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Plastered	-0.005	-0.008*	-0.011**	-0.007	-0.012**	-0.014***
wood	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Mom wage	-0.001**	-0.001	-0.001*	-0.002*	-0.001	-0.001
worker	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Nchild mom	0.002***	0.001***	0.001***	0.002***	0.002***	0.001***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age	Same	Same	Same	Same	Same	Same
Sex	Same	Same	Same	Same	Same	Same
Famsize	Same	Same	Same	Same	Same	Same
Class of work pop	Same	Same	Same	Same	Same	Same
Conflict	Same	Same	Same	Same	Same	Same
One car	Same	Same	Same	Same	Same	Same
Computer	Same	Same	Same	Same	Same	Same
R-squared	0.02	0.039	0.061	0.032	0.056	0.158
N	633416	633416	633416	535976	535976	535976

Note: Also controlled for age, race, family size, disability, employment status of father, automobile ownership, wall type, computer ownership, class of father's work, class of mother's work, mother's number of children, and department.
Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 16: Humanitarian Situation Risk Index: Conflict Index

Variable Name	Description
Subversive Actions Rate	Hostile acts by subversive groups per 10,000 inhabitants, against police or civilian infrastructure. Includes attacks on police installation, attacks on planes, urban attacks, armed contact, ambushes and sieges.
Unilateral Attacks	Total number of incursion with no combat
Total Confrontations	Total confrontations between Public Forces and subversive groups
Total Deaths	Total deaths in combat
Mine incident rate	Number of mine incidents per 10,000 inhabitants
Homicide rate	Homicides per 10,000 inhabitants. Common homicides include all deaths by weapon with the exception of traffic-related homicides
Council member homicide rate	Homicide rate among council members, per 10,000 inhabitants
Union member homicide rate	Homicide rate among union members, per 10,000 inhabitants
Teacher homicide rate	Homicide rate among teachers, per 10,000 inhabitants
Indigenous homicide rate	Homicide rate among indigenous people, per 10,000 inhabitants
Massacre victim rate	Number of deaths in massacre per 10,000 inhabitants. A collective homicide or massacre is committed when the total killed are four or more persons. It must be committed at the same place, same time, by the same authors, and against persons unable to defend themselves
Kidnap victim rate	Kidnap victims per 10,000 inhabitants, including both simple and extorsive kidnap victims. A kidnapping is the retention or hiding of a person in order to exchange their freedom for some resource, avoid some act, or for a publicity of political end.
FARC Groups	Number of FARC groups present
ELN Groups	Number of ELN groups present
Expulsion displacement rate	Rate of forced displacement per 10,000, where the person is forcibly expelled from the municipality
Reception displacement rate	Rate of forced displacement per 10,000, where the person is forcibly expelled to the municipality

Table 17: Humanitarian Situation Risk Index: Response Capacity Index

Variable Name	Description
Teachers with higher education	Number of teachers with higher education
Student:teacher ratio	Total number of students by number of teachers
Middle education institutional presence	Number of middle education institutions per 10,000 inhabitants
SENA continuing education center presence	Number of SENA institutions per 10,000 inhabitants
ICBF Family Welfare Institute rate	Binary value, present or not present
Health center presence	Number of health centers per 10,000 inhabitants
Vaccination coverage	Percentage of population covered by vaccinations
Compensation services presence	Rate of compensation services present per 10,000 inhabitants
Judicial dispatch presence	Rate of presence of judicial dispatches per 10,000 inhabitants
Conciliation center presence	Rate of presence of conciliation centers per 10,000 inhabitants
Police station presence	Binary value, present or not present
Rural citizen soldier presence	Binary value, present or not present
Presence of major highway	Binary value, present or not present