Three Essays in Development Economics

Doctoral thesis for obtaining the academic degree
Doctor of Economics (Dr. rer. pol.)

submitted by

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at the

Universität Konstanz

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Konstanz 2025

Date of the oral examination: 25 June 2025

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ACKNOWLEDGEMENTS

First and foremost, I want to express my deep gratitude to my advisors, colleagues and everyone who supported me during my doctoral journey.

I sincerely thank my supervisor, Anke Höffler, for guiding my dissertation. I am deeply grateful for your feedback on my research, for consistently granting me the freedom to follow my own research direction, and for the incredible opportunity to work on our research project in the DR Congo. I also greatly appreciate the trust you placed in me to teach "Development Economics" at the Department of Political Science, allowing me to engage with students and lead a seminar on causal inference and development economics. I'm also incredibly grateful for your funding that made this doctoral work a reality and for convincing me in several conversations to leave Vienna and begin my academic journey in your group. Furthermore, I would like to thank Guido Schwerdt for taking on the supervision and for your consistent support in my research, inspiring questions and literature suggestions during seminars, as well as your invaluable assistance with scholarship and administrative matters. Lastly, I want to express my gratitude to Tilman Brück in Berlin for taking on the third supervision and for hosting me for over a year at the International Security and Development Center (ISDC) in Berlin.

A special thanks to all seminar participants in Budapest, Paris, Rome, and particularly the organizers of the EAAE Workshop in Budapest for bringing together what belongs together. I thank my colleagues at the Department of Economics and Political Science, the doctoral community at the Graduate School of Behavioral and Social Sciences and all current and former members of the Development Research Group, my co-author Alina, Jutta Obenland for her help and advice, and Stefan Angel in Vienna for encouraging me to pursue this PhD journey. A very special thanks to "Caffè a Casa" in Vienna for all the conversations and the inspiration for Paper 1. Last but not least, I am highly grateful for the support from all my friends and family and one person in particular - Marie - your presence has made all the difference.

SUMMARY

(ENGLISH VERSION)

How young people make economic decisions stands at the heart of microeconomic research. Economic decisions of young people are both immediately consequential for them as well as predictive of future behaviors, including investments in human capital and labor market outcomes. Understanding young people's economic decisions is particularly important in the Global South, where rapid demographic changes are underway. Between 2020 and 2050, the global working-age population will grow by over 1 billion people, with nearly 70% of this increase occurring in Sub-Saharan Africa—a dramatic shift from previous decades. Most of the new working-age population are children today, thus understanding their economic decisions is crucial for understanding labor market dynamics and human capital development of tomorrow. However, economic decisions of young people are not simply exogenously given. They begin to take shape early in life and they are influenced by several external factors beyond a child's control, such as the environment in which they are raised, the household they grow up in or the school they attend. Understanding how young people's exposure to external factors influences them in their economic decision making is therefore a central imperative for the 21st century research in development economics.

Against this background, and given the limited economic research on young people as economic agents, this thesis explores how external factors shape young people's economic decision-making across three unique settings. Following a brief general introduction, the thesis is structured into three distinct chapters, each identifying the causal impact of a specific external factor, the environment, the household and the school, on young people's decisions making.

The first chapter, uses a quasi-natural experiment, the 2010 boom in global coffee prices, to investigate the effect of positive income shocks from coffee price booms on educational decisions of young people in Rwanda. Microeconomic theory suggests that income shocks can facilitate the accumulation of human capital by increasing households' financial resources, which may be allocated to education, but at the same time, they raise the opportunity cost of schooling and

thus discourage long-term human capital accumulation. I show that during coffee booms, young people in coffee-growing regions are more likely to attend school and that these short-term effects translate into long-term educational benefits, as young people from coffee-growing regions who experienced higher coffee prices during their school years also complete more years of schooling by the age of 18. I provide evidence that increased household income outweighs any potential increased opportunity costs of schooling. However, the positive income effects are concentrated among young people living near privately managed coffee washing stations. In contrast, I find no significant effects for young people living near cooperative-run stations, suggesting that the way cooperatives distribute income gains may result in fewer direct benefits for households. My findings highlight the crucial role of income and local economic structures in shaping young people's educational decisions.

In my second chapter, co-authored with Dr. Alina Greiner, I study how mass violence shapes violent attitudes against children, and I examine how these attitudes are transmitted across generations in the context of the Rwandan genocide. Leveraging a natural experiment, the post genocide empowerment conditions for women in regions highly affected by the Rwandan genocide, I find that genocide violence causes young women to hold less violent attitudes compared to older women from the same regions, and compared to women in the same age cohorts from less genocide-affected regions. Second, I am the first to provide causal evidence on the inter-generational transmission of these violent attitudes, showing that descendants of these young women who experienced genocide induced empowerment also develop less violent decision-making patterns. Thus, women's empowerment after mass violence makes post-conflict societies less violent and has a strong causal intra-household transmission channel that shapes young people's economic and social decision-making.

My third chapter examines another external factor shaping young people's economic decisions—perhaps the most critical one after their environment and household—their school. In particular, I study the causal effect of violent teachers on students' educational decisions in Malawi. I employ a difference-in-differences design and estimate value-added models to estimate how teachers' violent behavior—including physical, emotional, and sexual violence—affects students' academic outcomes. I show that exposure to violent teachers reduces grade progression by 30 percentage points and decreases math scores by 15 percentage points over one academic year. The effects are most pronounced for early secondary students, while no significant impact is found on English and Chichewa test scores. My results highlight how school environments, much like environmental and household conditions shape young people's decisions, particularly their ability to invest in their human capital.

SUMMARY

(GERMAN VERSION)

Wie junge Menschen ökonomische Entscheidungen treffen, ist zentraler Bestandteil mikroökonomischer Forschung. Diese Entscheidungen sind sowohl unmittelbar für sie selbst bedeutsam als auch entscheidend für zukünftiges Verhalten, einschließlich Humankapital- und Arbeitsmarktinvestitionen. Das Verständnis der ökonomischen Entscheidungen von jungen Menschen ist besonders wichtig im Globalen Süden, wo derzeit rasche demografische Veränderungen stattfinden. Zwischen 2020 und 2050 wird die weltweite Erwerbsbevölkerung um mehr als 1 Milliarde Menschen wachsen, wobei fast 70 % dieses Anstiegs in Subsahara-Afrika stattfinden wird – ein dramatischer Wandel im Vergleich zu den vorherigen Jahrzehnten. Der größte Teil dieser neuen Erwerbsbevölkerung ist heute noch ein junger Mensch, weshalb das Verständnis ihrer gegenwärtigen ökonomischen Entscheidungen entscheidend für das Verständnis der zukünftigen Arbeitsmarktdynamik und Humankapitalentwicklung ist.

Ökonomische Entscheidungen von jungen Menschen sind jedoch nicht einfach exogen gegeben. Sie formen sich früh im Leben und werden von diversen äußeren Faktoren beeinflusst, die außerhalb ihrer Kontrolle liegen, wie etwa die Umgebung, in der sie aufwachsen, die Haushalte in denen sie leben, oder die Schule, die sie besuchen. Das Verständnis, wie diese äußeren Faktoren die ökonomischen Entscheidungen von junge Menschen beeinflussen, ist daher ein zentrales Anliegen der entwicklungsökonomischen Forschung des 21. Jahrhunderts. Vor diesem Hintergrund und angesichts der begrenzten ökonomischen Forschung zu junge Menschen als ökonomischen Akteuren, untersucht diese Dissertation, wie äußere Faktoren die ökonomische Entscheidungsfindung von jungen Menschen beeinflussen. Nach einer kurzen Einführung ist die Dissertation in drei Hauptkapitel gegliedert, von denen jedes den kausalen Einfluss eines spezifischen äußeren Faktors – des Umfelds, des Haushalts und der Schule – auf die Entscheidungen von jungen Menschen untersucht.

Das erste Kapitel nutzt ein quasi-natürliches Experiment, den globalen Kaffee-Preisanstieg aus dem Jahr 2010, um den Effekt positiver Einkommensschocks, auf die Bildungsentscheidungen

von jungen Menschen in Ruanda zu untersuchen. Mikroökonomische Theorie legt nahe, dass Einkommensschocks die Akkumulation von Humankapital fördern können, indem sie Bildung erschwinglicher machen, gleichzeitig jedoch die Opportunitätskosten der Schulbildung erhöhen und damit die langfristige Akkumulation von Humankapital verringern können. Ich zeige, dass Kinder in Kaffeeanbaugebieten während des Kaffee-Preisbooms häufgier die Schule besuchen und bis zum Alter von 18 Jahren auch mehr Schuljahre absolvieren. Ich liefere Belege, dass gestiegenes Haushaltseinkommen die potenziell erhöhten Opportunitätskosten der Schulbildung überwiegt. Allerdings konzentrieren sich die positiven Effekte auf junge Menschen in der Nähe von privat geführten Kaffeewaschstationen und sind für junge Menschen in der Nähe von Kooperativen nicht zu finden. Das deutet darauf hin, dass Kooperativen möglicherweise zu weniger Einkommensgewinnen für Haushalte beitragen. Meine Ergebnisse suggerieren, dass Einkommen und lokale Wirtschaftsstrukturen Bildungsentscheidungen von Kindern beinflussen.

Im zweiten Kapitel, das ich zusammen mit Dr. Alina Greiner verfasst habe, untersuche ich im Kontext des ruandischen Völkermords, wie Massengewalt gewalttätige Einstellungen gegenüber Kindern prägt und wie diese Einstellungen über Generationen hinweg übertragen werden. Mithilfe eines natürlichen Experiments, des post-genozidalen "Empowerments" von Frauen in stark vom ruandischen Völkermord betroffenen Regionen, zeige ich, dass Genozidgewalt dazu führt, dass junge Frauen weniger gewalttätige Einstellungen haben im Vergleich zu älteren Frauen aus denselben Regionen und zu Frauen im gleichen Alter aus weniger vom Genozid betroffenen Regionen. Zweitens liefere ich erste kausale Belege für eine intergenerationelle Übertragung dieser Einstellungen und zeige, dass Nachkommen dieser Frauen ebenfalls weniger gewalttätige Einstellungen entwickeln. Somit kann das "Empowerment" von Frauen Gesellschaften nach gewaltsamen Konflikten weniger gewalttätig machen und prägt, über einen intra-haushaltlichen Übertragungskanal, die Entscheidungen und Einstellungen von Kindern.

Mein drittes Kapitel untersucht einen weiteren Faktor, der die ökonomischen Entscheidungen von jungen Menschen beeinflusst – vielleicht der entscheidendste nach ihrem Umfeld und dem Haushalt – die Schule. Hier untersuche ich den kausalen Effekt von gewalttätigen Lehrkräften auf die Bildungsentscheidungen von Schülern in Malawi. Ich verwende ein "Difference-in-Differences"-Design und schätze ein "Value-Added"-Modell, um zu ermitteln, wie gewalttätige Lehrkräfte – durch physische, emotionale und sexuelle Gewalt – den akademischen Weg von Schülern beeinflusst. Ich zeige, dass gewalttätige Lehrkräfte das Versetzen in die nächste Klasse um 30 Prozentpunkte verringeren und Noten in Mathematik um 15 Prozentpunkte über ein akademisches Jahr zurückgehen. Die Effekte sind am stärksten bei Schülern der frühen Sekundarstufe, wohingegen ich keine signifikanten Auswirkungen auf Englisch- und Chichewa-Testergebnisse finde. Meine Ergebnisse zeigen, dass Schulumgebungen, ähnlich wie Umwelt- und Haushaltsbedingungen, die Entscheidungen von jungen Menschen prägen, insbesondere die Investition in ihr Humankapital.

General Introduction

How young people make economic decisions stands at the heart of microeconomic research. These decisions are both immediately consequential for them as well as predictive of future behaviors, including investments in human capital and labor market outcomes. Studying children's economic decision-making is particularly important for research and policymaking in the Global South and particularly Sub-Saharan Africa, where rapid demographic changes are underway. Between 2020 and 2050, the global working-age population is expected to grow by over 1 billion people, with nearly 70% of this growth concentrated in Sub-Saharan Africa (Figure I). Sub-Saharan Africa, which accounted for less than 10% of the world's working-age population until 2010, will see its share more than double by 2050, with the working-age population growing by 700 million people—from 600 million in 2020 to 1.3 billion in 2050. This means that, solely to maintain the current employment rate, the region will need to generate approximately 2 million new jobs every month throughout this period (Lam & Elsayed, 2021a,b).

This growing youth population offers both, significant opportunities and substantial challenges. If educated and integrated into the labor market, Africa's youth could serve as a powerful engine of economic growth, innovation and productivity in the 21^{st} century. A well-educated and skilled workforce could help drive Africa's economic transformation, contributing to a demographic dividend that fuels long-term prosperity (Gust et al., 2024; Glewwe et al., 2014). However, without adequate investments in human capital, this demographic boom could lead

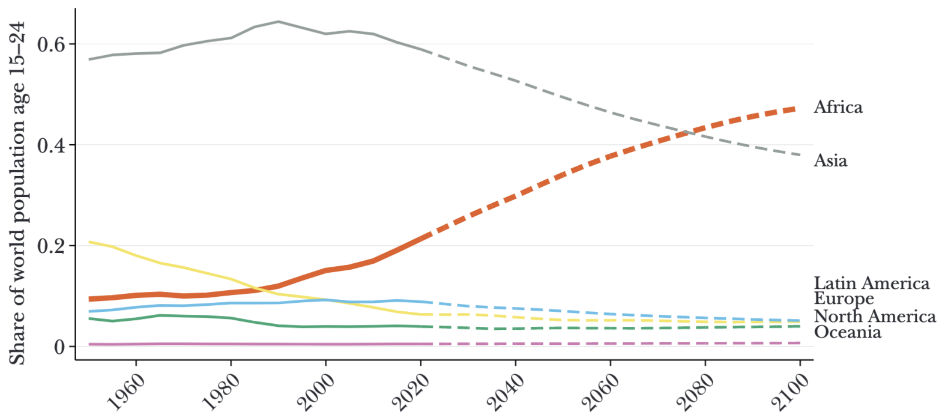

Fig I: Share of world population age 15–24, by region. Source: Bandiera et al. (2022) based on calculations from the UN World Population Prospects (2019). Note: Regional shares of the global population aged 15–24 are based on estimates and projections from the UN World Population Prospects (2019) (source). Dashed sections of the curves indicate projected values. The regions follow the geographical classifications used in the original dataset. The figure represents the 15–24 age cohort, as population data is reported in five-year intervals.

to widespread unemployment, informality and exclusion from formal labor markets (Lam & Elsayed, 2021a). Such outcomes could exacerbate poverty, inequality and migration pressures, as young people struggle to find meaningful employment opportunities. Moreover, without the right educational frameworks, young workers may face difficulties in adapting to rapidly changing industries and technologies, further limiting their potential economic contributions.

A key aspect of this demographic transformation is that many of the new working-age individuals are children today. Understanding their economic decision-making today is crucial for understanding labor market dynamics and human capital development of tomorrow. For example, the choices children make about education will significantly influence the region's future economic prospects and the level of human capital available to drive regional growth (Gust et al., 2024). However, economic decisions of young people are not simply exogenously given. They begin to take shape early in life and they are shaped in light of several external factors beyond a child's control, such as the environment in which they are raised, the household they grow up in, or the school they go. Understanding how children's exposure to external factors influences them in their economic decision making is therefore a central imperative for the 21st century research in development economics.

In recent years, two key approaches have emerged in economic research that study young adults and their decision-making. The first approach, drawing from psychology and neuroscience, uses data on children's behavior to test economic theories (List et al., 2021; Sutter et al., 2019). On the one hand, this approach seeks to understand young people's decision-making for immediate purposes, such as education (Lavecchia et al., 2016) with the idea of understanding children's inputs in educational production functions (Todd & Wolpin, 2003). On the other hand, several studies explore the decisions and preferences of children to potentially foreshadow adult behavior (List et al., 2021; Castillo et al., 2018, 2011). The second approach focuses on understanding children's decision-making in relation to external factors that influence their choices. This line of research examines how elements such as family background, socioeconomic environment, education systems, cultural norms, and broader economic conditions shape children's economic behavior. By analyzing these external influences, economists seek to understand how early-life experiences affect long-term outcomes (Alam et al., 2020).

Against this background, and given the limited economic research on children as economic subjects, this thesis contributes to both strands of the literature. I show how children's choices have consequences for human capital development and I examine how external factors influence children's economic decision-making across three distinct contexts. The thesis is structured into three chapters, each identifying the causal impact of a specific external factor—namely, the environment, the household and the school—on children's decision-making. The first chapter studies the 2010 boom in global coffee prices, to investigate the effect of positive income shocks caused by coffee price booms on educational decisions of children in Rwanda. Microeconomic theory suggests that income shocks can facilitate the accumulation of human capital by making education more affordable, but at the same time, they raise the opportunity cost of schooling and thus discourage long-term human capital accumulation. I show that coffee booms causally increase short- and long-term schooling outcomes for children in coffee-growing regions. Unlike similar contexts, in the smallholder farming context of Rwanda, young people do not drop out of education but rather increase their schooling years through a channel of higher family income. With this contribution, I highlight that local economic structures and family income are central in shaping children's educational decisions. In my second chapter, co-authored with Dr. Alina Greiner, I study how mass violence shapes violent attitudes against children, and I examine how these attitudes are transmitted across generations in the context of the Rwandan genocide. I am the first to provide causal evidence of intergenerational transmission of these attitudes, showing that children of women who were exposed to high levels of violence and who experienced genocide induced empowerment conditions after, develop less violent decision-making patterns. I show that women's empowerment after mass violence makes post-conflict societies less violent and has a strong causal intra-household transmission channel that shapes children's economic and social decision-making. My third chapter examines another external factor shaping children's economic decisions—perhaps the most critical after their environment and household—their school. I show that being victim of violent teachers reduces grade progression and decreases math scores significantly. The results of this chapter highlight how school environments, much like environmental and household conditions shape children's decision, particularly their ability to invest in their human capital.

Many factors impact children in their economic decision making. In this thesis, I provide causal evidence on three critical but distinct influences on children's economic decision-making. I show that Rwanda's small-scale coffee sector is not just an agricultural pillar but a driver of education and potentially long-term prosperity. I demonstrate that parental exposure to historical violence has lasting consequences, shaping the attitudes and decision-making of the next generation. And, I causally assess how teacher violence actively undermines human capital accumulation by lowering test scores and hindering grade progression. While these insights are rooted in the specific context of Rwanda and Malawi, they carry broader implications for the field of development economics. The micro-level approach—focusing on particular contexts and using careful identification strategies and robust data sources—is critical to understanding the nuances of children's decision-making. The future of development economics should continue to prioritize this micro-based approach, as it offers policymakers evidence-based recommendations that can be adapted to their unique contexts. It is through understanding the intricate, often overlooked factors that influence children's decisions at the micro level that development economist can advise policymakers of generating meaningful, sustainable economic progress—and, in doing so, contribute to alleviating the historical injustices that have shaped current global inequalities.

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CHAPTER I

Coffee Price Booms and Schooling: Evidence from Rwanda

Abstract

This paper provides novel evidence on the short and long term effects of coffee price booms on school attendance and attainment in the context of Rwanda. Exploiting a quasi-natural experiment, the boom periods in global coffee prices between 2010 and 2014, I show that children in coffee-growing regions are more likely to attend school when coffee prices are high. I also show that these short term effects translate into long-run human capital formation, as children from coffee growing regions who faced higher coffee prices during their school-going years also complete more years of schooling by the age of 18. Exploring mechanisms, I provide evidence that income effects are the main mechanism and dominate increased opportunity costs of schooling. However, the overall effects are primarily driven by children residing near privately organized coffee washing stations, rather than their counterparts near cooperatives, which also sheds new light on important treatment heterogeneity and the, in parts, ambiguous role of cooperatives for development in Rwanda.[1]

Keywords: Natural Resources, Schooling, Coffee, Child labor, Cooperatives, Rwanda
JEL codes: O13, O12, I20

[1] I would like to thank Marie Kammer, Anke Hoeffler, Guido Schwerdt, Romaine Loubes, Marco Sanfilippo, Bruno Martorano, Leonard le Roux, Donato Romano, Marta Tarruella Cayuela, Jule Beck, Alina Greiner, Urs Fischbacher, Susanne Goldlücke, Sofía Fernández Guerrico, Giovanni Generale, Mariapia Mendola, Clemens Nollenberger, Diogo Souza Monteiro, Viola Asri, Nicolas Coeurdacier, Pia Ennuschat and Antonio José León Fernandez for their helpful feedback and comments. I am further grateful to participants at the HEC Paris Economics Doctoral Conference, the European Association of Agricultural Economists PhD workshop Budapest, the 4th Development Economist Meeting at Roma Tre University, the Development Research Seminar Series and the Microeconomics Seminar at the University of Konstanz for their useful feedback and comments. I gratefully acknowledge the financial support provided by the German state of Baden-Württemberg through its promotion of young scientists and artists program (Landesgraduiertenförderungsgesetz). I thank Rocco Macchiavello and Ameet Morjaria, the Rwandan National Agricultural Export, and the Alliance for Coffee Excellence for sharing their data with me.

1.1 Introduction

Do aggregate income shocks, such as those caused by commodity price booms, affect children's schooling in Rwanda? Prior research has shown that commodity price booms impact economic development through multiple channels. They can increase local wages and consumption (Loayza & Rigolini, 2016; Boone et al., 2022), investment (Bazillier & Girard, 2020) and employment (Bourassa-Viau et al., 2022; Kotsadam & Tolonen, 2016), but they may also heighten the vulnerability of countries and regions to corruption and violence (Dube & Vargas, 2013; Axbard et al., 2021) and appreciate local currencies with ambiguous effects on other sector outputs of the economy (Sachs & Warner, 2001) and institutions (Sala-i Martin & Subramanian, 2013).

An alternative mechanism through which commodity price booms affect economic development is human capital accumulation (Bonilla Mejía, 2020; Ahlerup et al., 2019). The effect is theoretically ambiguous as commodity price booms generate income and substitution effects. On the one hand, booms can raise wages and local tax revenues and thus facilitate private investments in education by making education more affordable (income effect) (Edmonds & Schady, 2012; Edmonds et al., 2010; Alam et al., 2020). On the other hand, higher local wages could constitute an attractive source for young adults and children to enter the work force, raising the opportunity cost of schooling and thus discourage educational investments (substitution effect) (Carrillo, 2020; Black et al., 2005). Previous research has found that coffee booms negatively impact education outcomes in Brazil (Kruger, 2007) and Colombian coffee regions (Carrillo, 2020), suggesting that rising coffee prices may increase child labor at the expense of schooling. This is because coffee cultivation is a labor-intensive process—particularly the handpicking of coffee cherries (Carrillo, 2020). However, these prior studies focus on high-volume coffee-producing countries, where production is concentrated on large-scale coffee farms with distinct labor dynamics. In contrast, coffee cultivation in Sub-Saharan Africa is primarily done by smallholder farmers, operating under very different conditions. Despite the importance of coffee to rural livelihoods in these regions, little is known about how coffee price booms affect children in these smallholder farming contexts in Sub-Saharan Africa.

In this paper, I address this gap and provide novel evidence on the effect of coffee price booms on education outcomes in Rwanda, where 90% of the country's coffee is grown by smallholder farmers. To study the effects, I exploit a quasi-natural experiment, the boom in international coffee prices between 2010 and 2014. I estimate a number of difference-in-differences estimations, in which the treatment comes from variations in global coffee prices and spatial variation of coffee cultivation intensity across Rwanda. The idea behind the identification strategy is that Rwanda is a price taker in international coffee markets, and changes in coffee prices are exogenous to Rwandan production. As a result, higher prices directly translate into increased income for farm families in coffee-producing regions. I combine geo-referenced administrative data from the Rwandan National Agricultural Export Board (NAEB) on the number of coffee trees across Rwandan municipalities with data on the location of coffee washing stations (CWS) from Macchiavello & Morjaria (2021). This dataset is then merged with nationally representative household surveys from the Demographic and Health Survey (DHS) to track schooling decisions. To capture individuals' exposure to coffee booms, I measure international Arabica coffee prices during the survey month to study individual's exposure to different coffee prices.

The results show that during coffee booms, children in coffee-growing regions are more likely to attend school by around 4%. I also show that these short-term attendance effects translate into long-term educational benefits, as children from coffee-growing regions who experienced higher coffee prices during their school years also complete more years of schooling by the age of 18. Children from municipalities with above-median coffee production complete up to two-thirds of a year more schooling when exposed to periods of high prices during their school-going years. I provide evidence that increased household income, which outweighs any potential increased opportunity costs of schooling, is the key mechanism behind this effect. However, the positive income effects are concentrated among children living near privately managed coffee washing stations, which drive the observed educational improvements. In contrast, I find no significant effects for children living near cooperative-run stations, suggesting that the way cooperatives distribute income gains may result in fewer direct benefits for households, limiting their impact on children's schooling. The results are robust across several robustness checks, including analyses on a sub-sample of families with no migration history, placebo tests using price shocks from other crops, and replications using data from a separate sample of high-quality, specialty coffee producing coffee washing stations.

The main contributions of this paper are threefold: First, this study adds to the large body of work examining the effects of commodity price booms on children's education. Previous research has produced mixed evidence and suggest that the effects are highly context-dependent (Ferreira & Schady, 2009; Cogneau & Jedwab, 2012). While pro-cyclical patterns, i.e. higher income effects, have predominantly been found in low-income settings (Aragón & Rud, 2013; Bonilla Mejía, 2020; Beshir & Maystadt, 2023; Edmonds & Schady, 2012; Sharma, 2022), evidence from middle or high income countries points towards counter-cyclical patterns, where substitution effects prevail (Black et al., 2005; Shah & Steinberg, 2017; Mosquera, 2022; Boone et al., 2022; Ferreira & Schady, 2009; Álvarez & Vergara, 2022; Santos, 2018). Yet, compared to other commodities, coffee has received relatively little attention (Chant et al., 2008; Miller & Urdinola, 2010). I add to this literature by testing the channel of increased coffee prices on education outcomes in the Rwandan smallholder coffee production context. As such, my paper is closely related to Kruger (2007) and Carrillo (2020) who found counter cyclical educational patterns during coffee booms in Colombia and Brazil. Carrillo (2020) in particular showed how individuals affected by the 1984 coffee boom during childhood in Colombia have fewer years of schooling and lower adult earnings, most likely by discounting future consequences of dropout decisions when faced with immediate income gains from coffee. I bring value to several aspects of this literature: First, my analysis is based on detailed geographic information allowing me to measure coffee cultivation at a much higher spatial resolution than in previous studies. Second, I shed light on the previously overlooked heterogeneity between cooperatively and privately organized coffee washing stations by showing that privately organized coffee washing stations are the main driver of the positive effects on children's education. Third, to the best of my knowledge, this is the first study to investigate the impact of coffee price booms on schooling outcomes in a Sub-Saharan African country, a region dominated by smallholder coffee cultivation with distinct labor dynamics for farmers and children. Fourth, this study also contributes to the relatively scarce literature on the long term effects of commodity price booms on education and I provide evidence that income effects are the main mechanisms affecting long term human capital formation.

Second, I add value to studies on child labor (Basu & Van, 1998; Ravallion & Wodon, 2000). Particularly, I add to the few recent studies on the complex interplay between i) child labor at home, ii) outside home and iii) the domestic work of other family members (Dinku et al., 2019;

Busquet et al., 2021). Much of the existing literature on the economics of child labor assumes that child labor is synonymous with employment in income-generating activities, such as working on coffee farms. However, evidence also suggests that there is a high degree of substitution between the domestic labor of a mother and that of her children (Goldin, 1979). Theoretically, coffee booms could negatively impact children's education by creating a substitution effect between adult and child labor within the household. If mothers increase their labor force participation in response to higher coffee prices, e.g. by working on farms or selling cherries to CWS, children may take on more domestic responsibilities, potentially reducing their time for schooling (Dinku et al., 2019). This is particularly relevant in the context of of Rwandan coffee. In 2000, the Rwandan government adopted the National Coffee Strategy that aimed to shift the sector to fully washed coffee production to improve participation in the international specialty coffee market (Boudreau et al., 2023). This strategy aimed at helping farmers to establish coffee washing stations in their communities with positive effects on female labor force participation and women's earnings in the vicinity of coffee washing stations (Sanin, 2023). Yet, whether these positive effects on household welfare created negative spillovers on children's education has not been studied and are highly uncertain. My study adds to this debate, by showing that for private coffee washing stations, any potential negative spillovers are outweighed by the positive income effects. However, I show that the effects are less clear in the context of cooperatives.

Third, I contribute to the literature that highlights the role of income as an important determinant in educational production. Existing research has demonstrated that income is one of the most important driver of children's schooling (Grimm, 2011; Björkman-Nyqvist, 2013), particularly in context where people are liquidity constraint (Johnson, 2013). I contribute to this literature by demonstrating that positive income shocks from coffee price booms are transmitted to children primarily through improvements in household income. A key contribution of my study lies in the examination of how the organizational structure of the coffee sector shapes these effects. I uncover significant treatment heterogeneity, with private coffee washing stations generating positive effects on schooling, while children living near cooperatives show less responsiveness to price fluctuations. As such, I also add value to recent studies on the ambiguous role of cooperatives for development, particularly in Rwanda (Elder et al., 2012; Ortega et al., 2019; Munir, 2022). There are multiple reasons to assume that cooperatives and private coffee washing stations create different spillovers to children residing in close proximity. First, cooperatives tend to be less efficient in delivering high-quality management practices (Boudreau et al., 2023), although studies have repeatably shown that effective CWS-management is a key driver in creating positive local spillovers for farmers in Rwanda (Macchiavello & Morjaria, 2021; Blouin & Macchiavello, 2019). Second, prior research has shown that, in Rwanda, cooperatives tend to have weaker transmission of income to participants compared to private stations, and may even be linked to farmers experiencing higher levels of gender-based violence (Munir, 2022). Additionally, from other contexts we know that poor individuals often struggle to afford membership in cooperatives and evidence suggests that cooperatives tend to exclude the poor or fail to effectively reach them (Bird et al., 2022). I bring value to this strand of the literature by highlighting that cooperative CWS do not lead to positive income effects that translate into improved educational outcomes.

The remainder of this paper is organized as follows: Section 1.2 introduces a brief theoretical model behind households schooling decisions during coffee booms. Section 1.3 provides background information on the history and geography of coffee production in Rwanda. Sections 1.4 and 1.5 describe the data and empirical strategy. In Section 1.6 presents the main results and discusses the key mechanisms through which coffee booms affect children's educational outcomes. Finally, Section 1.7 discusses the findings and concludes.

1.2 Theoretical underpinnings

What happens to children's education when coffee prices change? The theoretical mechanisms are not obvious because coffee price changes create substitution and income effects, which influence schooling in opposite directions. On the one hand, when earnings in the coffee sector, e.g. at coffee farms or coffee washing stations, are attractive and jobs are available, children and young adults might have incentives to work, and decide to drop out of schooling (substitution effect). Even if children decide to stay in school, a price shock could affect individual preferences for higher education (e.g. for non-compulsory secondary education) when the shift in the opportunity cost of studying is perceived as permanent and could thus discourage long term human capital formation (Carrillo, 2020; Bonilla Mejía, 2020). On the other hand, coffee price booms may improve education through an increase in local income or higher public investment in schools. By selling coffee cherries to a higher price, parents of children may have more resources to afford a child's education and no longer rely on child work to generate household income (income effect). These intra-family dynamics can also involve parents and children weighting out competing interests (Bursztyn & Coffman, 2012). I follow Bonilla Mejía (2020), and model these mechanisms in a single theoretical framework.

Suppose a representative household that is comprised of two members, a parent and a child. The child allocates a unit of time to either study (s) or work (l): $1 = s + l$. The household utility function is additive in consumption (c) and the child's human capital (h):

$$U(c, h) = u(c) + \alpha v(h) \tag{1.1}$$

with α being a positive coefficient capturing the household's preferences for education. Human capital is the product of time spend for studying (s) and a positive education technology coefficient (β) that captures all external environmental factors, such as public school spending, teacher quality or school infrastructure

$$h = \beta s \tag{1.2}$$

The household consumes the total labor income of parents (I) and children (wl)

$$c = I + wl \tag{1.3}$$

The marginal rate of substitution between human capital and consumption is positively related to wages and negatively to education technology and preferences for education:

$$-\frac{\delta v}{\delta h} / \frac{\delta u}{\delta c} = \frac{w}{a\beta} \tag{1.4}$$

The implications are threefold: First, if a boom in international coffee prices raises the local wages in coffee growing regions w, the opportunity cost of schooling also raise. Second, the price boom can also reduce the opportunity cost of schooling, through changes in β, e.g. by increasing public spending on schools in coffee growing areas. And third, opportunity costs of schooling can also be affected through changes in household's preferences for education α induced by the change in local incomes, e.g. when the shift in local incomes is perceived as permanent.

One can show that the effect of higher local wages on education can be decomposed into income and substitution effects, using the Slutsky equation:

$$\frac{\delta h}{\delta w} = \frac{\delta h}{\delta \bar{I}} l + \frac{\delta h*}{\delta w} \mid_{u=\bar{u}} \tag{1.5}$$

Where h and h* are the Walrasian and Hicksian demand functions of schooling. The first term on the right-hand side of the identity in equation (5) is the income effect, which is always positive, while the second term, the substitution effect, is negative. The total effect of a local income shock such as those caused by higher global coffee prices on the child's educational decision therefore depends on the relative strength of these two effects (Bonilla Mejía, 2020).

1.3 Coffee production in Rwanda

1.3.1 Coffee cultivation

Rwanda has a long history of coffee cultivation, dating back to the early 1900s when the crop was first introduced by colonial settlers (Guariso et al., 2011).[2] Today, Rwandan coffee is renowned for its high-quality production and some of the world's best green coffees produced. Coffee in Rwanda is primarily cultivated by smallholder farmers, who account for around 90% of the country's coffee cultivation. The farmers typically own less than half a hectare of land and cultivate coffee as a cash crop alongside subsistence crops like maize, beans, and bananas. Rwandan coffee is mostly grown in the Western and Central regions of the country, where the climate and altitude are ideal for coffee cultivation (Carrillo, 2020).[3] Coffee beans are a relatively homogeneous good, although there are some sub-varieties that differ in terms of size, color, resistance to pest and diseases, and conditions required for cultivation. Rwanda produces Arabica coffee, the most popular variety of coffee (Gerard et al., 2022) and Arabica beans, known for their

[2]Guariso et al. (2011) provide an extensive overview of the history of coffee production in Rwanda.

[3]The most suitable areas for coffee cultivation in Rwanda are the hillsides, which are abundant in the country, characterized by steep slopes and heavy rainfall. Ideal climatic conditions for coffee growth include temperatures ranging from 15 to 24°C, annual rainfall between 1500 and 2000 mm, and relative humidity levels between 70% and 90% (Boudreaux, 2009).

more aromatic flavor profiles, are the most popular variety in the global coffee market. Figure 1.1 shows the geographical distribution of coffee production in Rwanda.

The production of Arabica coffee encompasses two stages, cultivation and processing, which are both time and labor-intensive stages that directly affect the quality of the final product. Coffee cherries, which contain the coffee beans, are produced by coffee trees. Coffee trees are grown on rough and steep terrains, which do not allow mechanical picking of coffee beans (Carrillo, 2020). From planting, it takes at least three years for coffee trees to start producing cherries. However, the cherries do not ripen all at once, so harvesting must be done by hand, as selective picking is required to ensure only fully ripe cherries are collected, making it a labor-intensive task. Workers often need to visit each tree multiple times over the span of several weeks to ensure all the cherries are harvested when they are fully ripe. The harvest season in Rwanda lasts for approximately four to five months between March and July, with a peak in May and June. Since it does not require physical strength or complex knowledge, it naturally raises concerns about child labor (Carrillo, 2020).

Once the cherries are harvested, coffee undergoes a two-stage procedure. The first stage is processing, where the outer layer of the coffee beans, known as the pulp, is removed. Rwanda produces both washed and natural processed coffees. Washed coffees are processed using the wet method, which involves removing the outer layers of the cherry and washing the beans in water to remove the mucilage. This process requires heavy machinery that is often provided by a coffee washing stations in the community. Natural processed coffees, on the other hand, are dried with the cherry still attached to the bean. The cherries are spread out on raised beds and left to dry in the sun for several weeks. The dried cherries are then removed, leaving behind the coffee beans. The second stage involves sorting the dried coffee beans manually, separating defective beans based on their color and size. This process is similarly labor-intensive to the harvesting process. As most of Rwandan coffee producers are smallholder farms, with the average farm being less than one hectare in size, only very few farmers have the facilities to sort (process) the coffee cherries they harvest themselves. Therefore, a CWS is needed. CWS are cooperatively or privately managed and purchase their coffee cherries directly from hundreds of smallholder farmers in the area (Sanin, 2023; Boudreau et al., 2023). The presence of a CWS can therefore influence local economic conditions by improving access to markets, creating employment opportunities, and potentially increasing incomes (Macchiavello & Morjaria, 2021).[4] Rwanda is also renowned for some high quality coffee beans, classified as ”specialty coffee”. Specialty coffee refers to beans that meet rigorous quality standards.[5]

1.3.2 Coffee exports and prices

From a global perspective, Rwanda is a small country, and only contributes a small share of less than 2% to the global coffee trade volume, making it a price-taker in global coffee markets.[6] As coffee production is a highly integrated commodity market, primarily grown for export and the international market, Rwanda's smallholder farms are particularly exposed to global coffee price fluctuations (Blouin & Macchiavello, 2019; Adhvaryu et al., 2021). Since coffee beans are a relatively homogeneous commodity, their price is primarily driven by the production levels of the four largest coffee-producing countries—Brazil, Vietnam, Colombia and Indonesia. In 2010, a series of external events, including weather shocks in Brazil, led to sudden and dramatic fluctuations in global coffee prices (Figure A2). Because these shocks originated entirely outside Rwanda, international coffee prices can be considered plausibly exogenous to local schooling decisions, creating a natural experiment. Figure A2 supports this assumption. Rwanda's coffee sector is export-oriented, thus a sharp increase in global prices should affect trade value but not necessarily export quantity. This is what occurred in Rwanda in 2010 and the following years: higher global Arabica prices led to a significant rise in trade value of Rwandan coffee exports in 2010–2012, while total export quantity remained largely unchanged. This pattern suggests that fluctuations in global coffee prices induce quasi-random variation in income from coffee cultivation, which serves as the key source of identification for the empirical analysis in this paper.

[4] Until 2016 farmers were free to choose the washing station to sell their product to. In 2016, the Rwandan government introduced a zoning system for coffee sales to reduce side-selling and improve relationships between farmers and CWS. Under this system, farmers within a specific zone are required to sell their coffee to a designated CWS, effectively limiting their options for where to sell (Gerard et al., 2022).

[5] Specialty coffees earn a score of 80 or above on a 100-point scale from certified coffee graders. Specialty coffee processing can encompass newer, experimental methods, such as anaerobic fermentation that involves fermenting the beans in sealed, oxygen-free tanks. This process is similarly resource-heavy, requiring specialized equipment and infrastructure. Specialty coffee production focuses on careful cultivation, processing, and handling to ensure the beans retain their best possible flavors. It involves detailed attention to processing methods, and even picking and sorting of cherries that requires more complex knowledge.

[6] Rwanda is only ranked 30th in global coffee producing countries (https://worldpopulationreview.com/country-rankings/coffee-producing-countries)

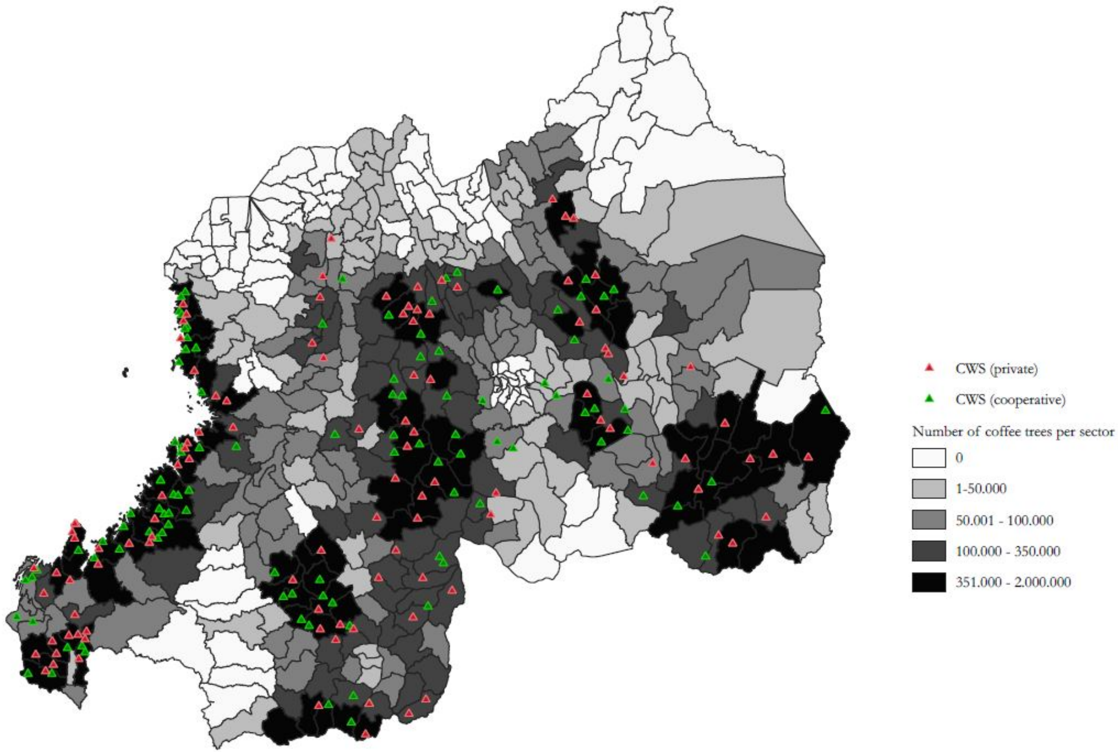

Fig 1.1: Geography of coffee cultivation in Rwanda. Sources: Author's own calculations based on data on the number of coffee trees per municipality and the exact geo-location of coffee washing stations from the National Agricultural Export Development Board Rwanda (NAEB). Information on CWS' structure (private, cooperative) from Macchiavello & Morjaria (2021)

1.4 Data

I construct a repeated cross-sectional data set using various sources. These include several measures of coffee production from different sources as well as micro data on household characteristics and education from four rounds of the Demographic and Health Surveys (DHS) of Rwanda.

1.4.1 Coffee intensity

I begin my analysis by identifying regions with a high probability of producing coffee. Figure 1.1 shows the spatial distribution of coffee cultivation patterns in Rwanda. To measure coffee cultivation intensity, I first draw on data from the Rwandan Coffee Census (2009) providing information on the number of productive coffee trees used by farmers to grow coffees per municipality.[7] The data is provided by the National Agricultural Export Development Board (NAEB) of Rwanda, which is responsible for promoting and regulating the country's agricultural exports. For the survey, farmers and washing stations provide information on the number of coffee trees they own as well as the size of their plantations through a mobile data collection system. The NAEB aggregates this data at the municipality level (416 sectors). Using this data, I measure a municipality's coffee intensity as the total number of coffee trees within its boundaries, scaled by the municipality's total area. This measure reflects the total area covered by coffee trees in each municipality and is measured on a continuous scale. To facilitate interpretation when interacting the variable with continuously measured coffee prices, I use a binary treatment variable for coffee growing intensity: (0) for municipality with a number of coffee trees below the median and (1) for above.

1.4.2 Coffee washing stations

The data on CWS stems from two sources. First, I use the data by Macchiavello & Morjaria (2021) providing detailed information on the universe of all CWS in Rwanda in 2012. The data is geo-coded and includes information on the characteristics of the CWS, like ownership status (cooperative, private), age of CWS, elevation, slope, total cherries purchased and the start year of operation for each CWS. I restrict this sample to CWS that were already in operation prior to 2010—the year coffee prices began to rise. Table 1.1 presents summary statistics for the sample of CWS. I follow Macchiavello & Morjaria (2021) and Sanin (2023) and define the main catchment area as a 5 km radius around the CWS (see Figure A1 in the appendix). However, since roads to CWS are often in poor condition, the effective catchment area for a CWS can extend up to 8 km (Macchiavello & Morjaria, 2021). Thus, I use 5-8 km distance measures in my main analysis. To ensure that my estimates reflect the true impact of CWS proximity, in several estimations I additionally implement a "donut" approach (see Appendix A1), where I exclude communities within the 5-8 km radius from the analysis, treating them as a potential overlap zone. The intuition behind this approach is that areas within the 5-8 km range of a CWS might experience less pronounced effects due to logistical and market constraints (Macchiavello & Morjaria, 2021)

[7] Municipalities, in Rwanda called sectors, are the third-level administrative units in Rwanda, with a mean area of approximately 50 km^2. They represent the lowest level at which the NAEB maintains regularly updated records of the number of coffee trees.

Table 1.1: Summary statistics and balance tests of coffee washing stations.

	Cooperative CWS		Private CWS		Differences		
Variable	Mean	SD	Mean	SD	Estimate	SD	p-value
Age of CWS	4.89	2.57	4.43	2.78	0.46	0.39	0.29
Age of CWS squared	30.50	32.02	27.29	37.66	3.21	5.11	0.57
NGO supported (=1)	0.04	0.21	0.45	0.50	-0.40***	0.05	0.00
Spring in 5km	0.03	0.03	0.03	0.02	-0.00	0.01	0.90
Average Tree density within 5km (in 1000)	12038.62	8333.62	10789.60	8163.04	1249.02	1212.993	0.30
Average Elevation (m) within 5km	1632.33	167.20	1638.15	131.39	-5.82	22.26	0.79
Average Slope (m) within 5km	11.13	3.06	11.08	2.99	0.05	0.44	0.89
Average Kilometers of road within 5km	1788.31	650.21	1800.93	448.30	-12.61	83.00	0.87
Total Cherries Purchased	2743.39	2063.67	2220.57	2053.55	522.82	302.56	0.10

Notes: The number of observations used to calculate the summary statistics for is n=87 (cooperatives) and n=99 (private).

than the ones close to the CWS. As roads to CWS are often in poor conditions, they can limit the accessibility and effectiveness of these stations for households beyond the immediate vicinity. Therefore, while households located within 5 km of a CWS are likely to experience the most significant effects, households located in the 6-8 km range are still exposed to the price shock but with more moderate intensity. By excluding these "donut" areas (5-8 km), I isolate communities that are genuinely exposed to the full benefits of CWS proximity and compare them to those that are truly unexposed, i.e. outside of 8km radius.

Second, I collect data on a separate sample of farms that produce, high quality, specialty coffee.[8] I identify high-quality coffee producers by using web-scraped data on all coffee washing stations that participated in the Cup of Excellence (COE) initiative. The COE is a prestigious international coffee competition and auction that recognizes and rewards the highest quality coffees produced in various countries around the world.[9] Participation in the COE has been

[8] Although most of the Rwandan coffee produced is fully washed, leading to generally higher quality, not all of it is specialty coffee (Guariso et al., 2011). Specialty coffee represents the highest quality coffee in the market, evaluated by specifically trained Q-graders. To qualify as specialty coffee, beans must score 80 or above on a 100-point scale, meeting strict standards for flavor, aroma, and defect-free processing. Achieving this quality standard requires not only fully washing the beans but also careful selecting ripe coffee cherries and other post harvest processing techniques (e.g. fermentation). For instance, if washing stations process all cherries they receive—including immature, overripe, or defective cherries—the resulting product may fail to meet specialty coffee standards. In such cases, even though farmers bear the costs of fully washing their coffee, they may not receive a specialty premium.

[9] The initiative was founded by the Alliance for Coffee Excellence (ACE) in 1999, with the goal of promoting and celebrating exceptional coffee and the farmers who produce it. In Rwanda, the COE initiative was introduced in 2008, and has since become an important platform for promoting the country's high-quality specialty coffee. The competition involves a rigorous selection process, in which a panel of international judges (coffee graders) evaluates samples of coffee from different regions and selects the top-scoring coffees as COE winners. The winning coffees are then auctioned off to international buyers, providing an opportunity for Rwandan coffee farmers to access premium prices and markets. The COE allows specialty coffee roasters from global markets to identify potential trade partners for direct trade relationships, bypassing traditional supply chains and intermediaries, often resulting in high quality coffee as well as higher profit margins for coffee producing farms (Wilson & Wilson, 2014).

proven to be a valuable opportunity for coffee farmers to showcase their products and gain access to premium prices and markets (Wilson & Wilson, 2014). These price premiums can also protect farmers or CWS from large fluctuations of the international coffee prices and it is expected that differences in coffee quality might have different effects on local income transmissions (Wilson & Wilson, 2014; Elder et al., 2012). The data from the COE is also geo-coded providing the exact location of the CWS for all participating CWS as well as ownership type, start year of operation and score in the COE initiative. There are 77 cooperatives and 46 private CWS in the sample scoring 80 points or higher and all of which have started their operation prior to 2010. Since not all coffee washing stations participate in the contest, I can only approximate quality for those CWS that have participated in the contest. Therefore, the analysis provides lower bound estimates for these effects. This data has never been exploited before and helps understanding of how different organizational schemes might affect the effect of coffee price booms on education.

1.4.3 Educational outcomes

I match this data to nationwide representative household surveys from the Demographic and Health Survey (DHS) to measure schooling decisions. I measure educational outcomes at the short and long term extensive margin using four rounds of DHS data, i.e., the 2005, 2010 rounds for school attendance and 2015, 2019/20 for attainment. The DHS surveys collect GPS coordinates for every cluster of households. Using the GPS coordinates, I spatially merge the DHS data with the coffee data in the municipality and measure the distance to the closest CWS. Important to note is, that the DHS displaces geo-coordinates to protect respondents' confidentiality (Perez-Haydrich et al., 2013). Urban clusters are randomly displaced by up to 2 km and rural clusters by up to 5 km. My choice of 5 km radius catchment area is thus further motivated by the spatial uncertainty introduced by the DHS's random displacement of GPS coordinates. Given that 80% of the sample reside in rural areas, using a threshold smaller than 5 km would introduce significant measurement error in distinguishing treatment and control locations. By setting the distance threshold at 5 km, I ensure that any classification of individuals as residing near a CWS is robust to these positional errors.[10]

[10]While this approach limits my ability to analyze more localized exposure effects, it is justified by the need to minimize measurement error.

Table 1.2: Summary statistics (DHS sample)

	Mean	SD	Min	Max	Observations
Panel A: School enrollment					
Enrollment	0.84	0.36	0.00	1.00	22175
Urban	0.80	0.40	0.00	1.00	22175
Gender	0.49	0.50	0.00	1.00	22175
Age of HH member	12.13	3.48	6.00	18.00	22175
Trees in Sector (in 1,000,000)	26.04	40.30	0.00	199.00	22175
Trees in District (in 1,000,000)	28.91	37.80	0.00	162.00	22175
Private CWS in 5km	0.27	0.45	0.00	1.00	22175
Cooperative CWS in 5km	0.26	0.44	0.00	1.00	22175
Specialty CWS in 5km	0.14	0.35	0.00	1.00	22175
Prices past 12 Months (in US$)	152.34	50.85	87.12	226.32	22175
Days with Rainfall in 2005	101.89	11.66	72.30	140.75	22175
Days with Rainfall in 2010	125.21	17.60	92.30	180.20	22175
HH Wealth	3.13	1.44	1.00	5.00	22175
Cluster Altitude (in m)	1741.39	286.05	1041.00	2653.00	22175
Road Density (meters per $2km^2$)	1619.42	1235.12	59.00	6316.00	22175
Panel B: Completed years of schooling					
Years of Schooling	6.46	3.64	0.00	19.00	10003
Urban	0.66	0.47	0.00	1.00	10003
Gender	0.45	0.50	0.00	1.00	10003
Age of HH member	24.22	4.28	18.00	35.00	10003
Trees in Sector (in 100000)	17.52	30.41	0.00	197.00	10003
Trees in District (in 100000)	23.65	37.58	0.00	162.00	10003
Private CWS in 5km	0.21	0.41	0.00	1.00	10003
Cooperative CWS in 5km	0.27	0.44	0.00	1.00	10003
Specialty CWS in 5km	0.13	0.33	0.00	1.00	10003
Average Prices past 12 Years	117.92	17.59	103.69	167.56	10003
Days with Rainfall in 2010	125.21	17.60	92.30	180.20	10003
Days with Rainfall in 2005	131.39	12.36	69.30	167.73	10003
HH Wealth	3.27	1.50	1.00	5.00	10003
Cluster Altitude (in m)	1743.29	311.37	968.00	3095.00	10003
Road Density in Cluster (meters per $2km^2$)	2068.06	1650.56	0.00	6316.00	10003
Covered by Health Insurance	0.71	0.45	0	1	5884
Insecticide Treated Bednets in use	0.50	0.50	0	1	5884
Years Living in Community	8.25	7.89	0	49	4234

Note: Panel A contains summary statistics using 2005, 2010 DHS data. Coffee trees in 100000 and scaled by municipality size. Coffee price is measured by the international price for Arabica during interview month. Family wealth is the DHS wealth index (1 = poorest, 5= richest). Panel B contains summary statistics using 2015 and 2020 data. Average coffee price as the average nominal price during an individual's school going period. Wealth as DHS wealth index.

In the the short term, education is measured by whether the child, 6-18 years of age, attended school in the past school year, by using data from the household recode file, in which each household member's school attendance status is surveyed. I build a national representative dataset that is of repeated cross sectional nature and includes all children of school-going ages.

The Rwandan education system is divided into three levels: primary education, lower secondary education, and upper secondary education. Primary education lasts for six years, starting at the age of six and ending at the age of twelve and is compulsory for all children. Students who complete primary education can obtain a Primary Leaving Certificate (PLE) which allows them to proceed to lower secondary education. Lower secondary education lasts for three years and is intended for students between the ages of thirteen and fifteen. Students who complete lower secondary education can obtain a certificate that enables them to proceed to upper secondary education or vocational education and training. Upper secondary education is a three-year program designed for students between the ages of sixteen and eighteen and students can choose between general and vocational education. The general education program leads to a secondary school diploma (A2 Level) that qualifies students for admission to universities and colleges in Rwanda and abroad. Students who complete the vocational education program can obtain a Technical and Vocational Education and Training (TVET) certificate, which qualifies them for employment in various technical and vocational fields.

For the long run measure of education, I limit the sample to cohorts born between 1985 and 2006, who were 18-35 years old at the time they are observed in the survey and thus have likely completed their schooling decisions.[11] This sample thus covers cohorts that were exposed during their school-going years to high and low coffee price episodes. To estimate the extent to which individuals were exposed to coffee market conditions when they were of school-going age, I assume that the municipality where they were observed in, is the same as the one where they grew up. I provide several robustness checks for this assumption. Summary statistics for the DHS variables are presented in Table 1.2.

1.4.4 Coffee prices

This data is then merged to international prices for Arabica coffee during survey month to measure individuals' exposure to coffee booms during the month of interview (for school attendance) and during their school-going years (for school attainment). For attendance (Eq. 1.6), I measure individuals' exposure to coffee prices during the past 12 months. For attainment, I create an overall measure of world coffee market conditions in childhood for each birth year cohort by

[11] The upper age limit of 35 is due to data availability on coffee prices.

averaging the international coffee prices observed during cohorts' 144 months (12 years) of school-going years. The effect of coffee booms, is then given by the interaction between these average coffee prices during school years and (i) the intensity of coffee cultivation in the municipality or (ii) the distance to the closest CWS.

$$\bar{p}_t^{(12)} = \bar{p}_t^{\text{last school year}} = \frac{1}{N}\sum_{k=1}^{N} p_{t-k}^{\text{arabica, global}} \quad \text{where } N = 12\,\text{months} \tag{1.6}$$

$$\bar{p}_t^{(144)} = \bar{p}_t^{\text{school going years}} = \frac{1}{M}\sum_{k=1}^{M} p_{t-k}^{\text{arabica, global}} \quad \text{where } M = 144\,\text{months} \tag{1.7}$$

1.4.5 Complementary data

Lastly, I include DHS cluster controls such as cluster altitude, and days of rainfall in 2005, 2010, 2015 and 2020 to control for weather shocks. I also include a measure of road density in a 2x2 km raster from Meijer et al. (2018) to proxy for local road infrastructure. In a robustness check, I use administrative data from the Ministry of Education that provides annual figures at the district level, including the number of schools and the number of teachers in each district.[12] These variables serve as key indicators of the supply of public education, allowing me to asses whether coffee price shocks also affect the availability of schooling infrastructure.

1.5 Empirical strategy

This paper estimates the causal effect of coffee booms on human capital accumulation. I exploit two sources of variation: temporal (global coffee prices) and spatial ((i) household distance to coffee washing stations or ii) households residing within municipalities of varying coffee trees) in order to distinguish treated and non-treated individuals. The idea is that the price for coffee beans is the main driver of increased income in coffee producing municipalities and individuals in areas where the intensity of coffee cultivation is high should be more affected by changes in coffee prices, while those in low-intensity or non growing municipalities serve as a control group. The validity of this identification strategy relies on the assumption that international coffee prices are

[12]Districts are the second-level administrative unit in Rwanda

exogenous to local schooling decisions. Since Rwanda is a price taker in international markets this is a plausible assumption (see Figure A2 in the Appendix). My identification therefore identifies an intention to treat effect (ITT). For school attendance I estimate the following baseline model:

$$Y_{i,c,j,t} = \beta_0 + \beta_1(\bar{p}_t^{(12)} * C_j) + \lambda_j + \gamma_t + \sigma X_c + \delta X_i + \kappa T_{jt} + \epsilon_{icjt} \tag{1.8}$$

where the dependent variable Y is a dummy variable on whether child i (6-18 years of age) in cluster c and municipality j, at survey month t is currently attending school. $\bar{p}_{t,i}^{(12)}$ are month of interview specific coffee prices faced by individual i during the past 12 months. Coffee region C_j measures i) coffee intensity in municipality j scaled by municipality size, ii) a binary variable for above (1) or below (0) median trees or iii) a binary indicator for whether individual i resides in a catchment radius (5km) of a coffee washing station, depending on the level of analysis. λ_j are municipality and γ_t survey month (month $\times$ year) fixed effects. X_c are cluster level controls and include variables such as cluster altitude, road density in a 2x2 km grid in 2010 and rainfalls in 2005 and 2010 to account for differences in the composition of surveys across DHS survey rounds. X_i are individual controls such as gender, age, age squared, age of household head, and years of education of the household head and urban status (1 if yes).[13] I also include T_{jt}, a district-specific linear time trend (District $\times$ Survey Year), to account for possible long-run dynamics in socioeconomic and other characteristics across districts. Standard errors in all regressions are clustered at the municipality level to allow for arbitrary correlation in the error term at the level of the municipality.

For the empirical analysis for school attainment, I compare cohorts on the basis of world coffee prices during their school-going years. To illustrate the idea, Figure A7 in the Appendix plots the average coffee price faced by cohorts during their school years (12 years) and the difference in accumulated years of schooling between individuals born in municipalities with high coffee intensity (above median number of trees) and those born in municipalities with a low cultivation intensity (below median number of trees). The figure shows that coffee booms are associated with increased human capital formation as the difference between the two regions decreases with

[13] I am careful with adding additional controls, such as wealth, as they are ”bad controls”, potentially confounding the relationships of interest between coffee prices, increased household income and schooling.

increasing coffee prices. This idea can be generalized to a regression framework that exploits greater variation across municipalities and cohorts by employing the following specification:

$$Y_{i,c,j,t} = \beta_0 + \beta_1(\bar{p}_t^{(144)} * C_j) + \lambda_j + \gamma_t + \sigma X_c + \delta X_i + \kappa T_{jt} + \epsilon_{icjt} \tag{1.9}$$

where the dependent variable Y is the total number of completed years of schooling by individual i (18-35 years of age) in municipality j, cluster c, and survey year t. $\bar{p}_t^{(144)}$ are the cohort specific average coffee prices faced by individual i during his or her school going period (between 6-18 years of age). Coffee region C_j is again measured as i) coffee intensity in municipality j scaled by municipality size, ii) a binary variable for above (1) or below (0) median trees or iii) a binary indicator for whether individual i resides in a catchment radius (5km) of a coffee washing station, depending on the level of analysis. This specification now includes municipality fixed effects λ_j and birth year cohort fixed effects γ_t. X_c are cluster level controls and include variables such as cluster altitude, road density in a 2x2 km grid in 2020 and rainfalls in 2015 and 2020 to account for differences in the composition of surveys across DHS survey rounds. X_i are individual controls such as gender, age, age squared and urban status (1 if yes). T_{jt} are district linear time trends. Standard errors are clustered at the municipality level. This design thus captures the long term change in schooling years in regions with high coffee exposure and during booms against that in regions with low coffee intensity.

In all estimations that explore distance to CWS, I add additional controls such as age of CWS, elevation and slope of CWS location, total cherries purchased in 2012 and the start year of operation for each CWS.

1.6 Results

1.6.1 Reduced form evidence of coffee price shocks on school attendance

I begin my analysis by estimating Eq.1.8, which examines the short-run effect of global coffee price shocks on school attendance in Rwanda. Table 1.3 shows that, across specifications, increases in global Arabica coffee prices are associated with higher school attendance in coffee producing municipalities. In the most parsimonious model (Column 1), a one-log increase in global Arabica

Table 1.3: The effect of coffee price booms on school attendance

	School attendance					
	(1)	(2)	(3)	(4)	(5)	(6)
$> \text{P50}(\text{CoffeeTrees}_{\text{municipality}}) \times \log(\bar{p}_t^{12})$	0.068***		0.064***		0.063***	
	(0.020)		(0.020)		(0.021)	
$\text{CoffeeTrees}_{\text{municipality}} \times \log(\bar{p}_t^{12})$		0.025**		0.027**		0.029**
		(0.010)		(0.011)		(0.013)
Observations	22,175	22,175	22,175	22,175	22,175	22,175
R^2	0.01074	0.01080	0.29144	0.29126	0.29185	0.29170
Within R^2			0.26853	0.26835	0.26896	0.26880
Individual controls			✓	✓	✓	✓
Cluster controls			✓	✓	✓	✓
Municipality fixed effects			✓	✓	✓	✓
Survey months fixed effects			✓	✓	✓	✓
District × Year Trend					✓	✓

Notes: LPM estimations. School attendance is based on 2005, 2010 DHS household record data. Individual controls control for respondents' age, age squared, age of the household head, years of education of the household head, urban status. Cluster controls include altitude (in meters), road density in a 2x2km grid at the cluster's centroid and days with rainfall in 2005 and 2010. The independent variable of interest is the interaction between the intensity of coffee production C and coffee prices P. Coffee production is measured as above (1) or below (0) the median number of coffee trees per municipality scaled by municipality size or the standardized measure of coffee trees per municipality. Prices $\bar{p}_t^{12}$ are measured as the logarithm of international prices for arabica coffee during the past 12 months from the survey month. In all estimations, standard errors are in parentheses and clustered at the municipality level. Significance levels are indicated as $*p < 0.1; **p < 0.05; ***p < 0.01$.

prices leads to a 6.8 percentage point increase in school attendance for municipalities that are above the median number of productive coffee trees. The estimated coefficient is highly significant at conventional levels of significance and robust, in significance and size, to the inclusion of individual controls, community controls, municipality and month of interview fixed effects (Column 3), as well as district-specific linear time trends to account for potential differential trends across districts (Column 5). The results from the fully specified model of Eq.(1.8) suggest that for a one log increase in coffee prices, school attendance increases by approximately 6.3 percentage points in above median coffee productive municipalities relative to the control municipalities. Given the average real-world price increase from 2005 to 2010, a log price increase of 0.67 points, this exogenous price shock thus contributed to a 4.2 percentage point increase in attendance in these areas.

The results remain consistent when, instead of using the binary variable for above-median coffee municipality, I employ the continuous linear treatment measure, namely the number of coffee trees per municipality, scaled by municipality size and interacted with coffee prices. For ease of interpretation, the number of coffee trees per municipalities is normalized to a mean of zero and a standard deviation of one. The estimated coefficients reflect the effect of changes in Arabica prices on attendance rates across municipalities with varying numbers of productive coffee trees and can be interpreted in two ways: first, given a fixed coffee price, they capture the effect of a one standard deviation increase in the number of coffee trees in a municipality, or second, given a fixed number of coffee trees, they reveal the effect of one log increase in the price. I find a highly significant effect of 2.9 pp in the fully specified model including all control variables (Column 6), indicating that both the binary and the continuous treatment indicators show strong positive effects of coffee price shocks on school attendance in coffee-producing areas.

1.6.1.1 Robustness checks

To assess the robustness of these findings, I re-estimate the effects using alternative measures of coffee trees and prices. First, in Table A3, I aggregate the number of coffee trees at the district level ($n = 30$) to assess whether the effects of coffee price shocks are local or more widespread across the entire district. The motivation behind this is that potential spillovers between municipalities within the same district could introduce attenuation bias at the municipality level, leading to estimates that are biased towards zero. Conversely, if the district-level estimate is smaller than the municipality-level estimate, it would suggest that the impact of price shocks is indeed local. The results from Table A3 show that the estimates remain highly significant at the district level, but they are smaller, indicating that the effects are concentrated in high coffee-producing municipalities rather than being broadly spread across districts.

Second, I test the robustness of the results by varying the time window for coffee prices. Instead of the 12-month average price, I re-estimate Eq.1.8 using 6-month and 3-month averages prior to the interview, as well as the price in the interview month. Across all specifications, the estimated effects remain stable in both magnitude and significance, confirming that the relationship between coffee price changes and school attendance is not driven by arbitrary price volatility in the 12 months period but rather reflect a systematic economic response to prices. The results are presented in Table A4 in the appendix.

Third, in Table A5, I estimate the effects separately for primary school children aged 6–12, lower secondary school children aged 13–15, and upper secondary school children aged 16–18.

The estimates suggest that both younger and older children experience a positive effect from increases in coffee prices, but the effects are larger for secondary students. This pattern is not unexpected, as primary education in Rwanda is mandatory, limiting the ability of families to adjust school attendance in response to price shocks or to reallocate labor within the household at the primary level. Consequently, the results are primarily driven by secondary school students who, in contrast, are more elastic in their schooling decisions to short-term price increases (Column 3 and 6 of Table A5).

Fourth, a potential concern is that the observed effects could be driven by non-random migration between 2005 and 2010 rather than genuine changes in school attendance due to coffee price fluctuations. In Rwanda, migration occurs predominantly from rural to urban districts, with significant movement to Kigali (World Bank, 2017). If individuals migrate in response to economic conditions, and this migration is systematically correlated with both coffee production and school attendance, it could bias the results. For instance, if lower-educated individuals disproportionately migrate to cities such as Kigali while higher-educated individuals remain in rural areas, where coffee production is high, this could bias the results by altering the local education composition. This could lead to an overestimation of the positive effect of coffee price increases on school attendance, as the observed changes may partly reflect a more educated rural population rather than a true income-driven schooling response.[14] To address this concern, I conduct two additional robustness checks. First, I estimate the main models excluding the three districts of Kigali—Gasabo, Kicukiro, and Nyarugenge—to account for potential distortions arising from high urban migration into the capital. Second, I exclude all urban districts from the analysis to ensure that rural-to-urban migration does not affect the results. The results are presented in Table A6. Across all specifications, the estimated effects remain similar in magnitude and significance to those reported earlier, suggesting that migration is unlikely to be a major confounding factor.

[14] Alternatively, rising coffee prices could also attract more educated individuals to rural areas, leading to an increase in school attendance rates that is driven by demographic shifts rather than the here suggested income effect. Although this scenario is unlikely given that migration in Rwanda predominantly occurs from rural to urban areas, and only 5% of individuals in non-Kigali districts report having lived in a different district (World Bank, 2017), I am unable to directly test this channel using the DHS data from 2005 and 2010 due to the proximity of these surveys to the historical context of the 1994 Rwandan Genocide, which led the DHS to omit detailed migration data in order to protect respondents' ethnic identities. As a result, I rely on migration proxies for these years. However, I address potential migration concerns more thoroughly by utilizing migration data from the 2015 and 2020 DHS rounds in the long-term effects analysis in section 1.6.2

1.6.1.2 Heterogeneous effects by proximity to coffee washing stations

To better understand the mechanisms driving the relationship between coffee price shocks and school attendance, I examine whether the presence of a coffee washing station moderates the effects. The presence of a CWS can influence local economic conditions by improving access to markets, creating employment opportunities, and potentially increasing incomes (Macchiavello & Morjaria, 2021). Prior research has shown that local economic spillovers from resource-based industries depend on how income is distributed and whether markets allow for rapid adjustment to price changes (Aragón & Rud, 2013) and several pieces of evidence suggest that cooperatives, despite their potential to enhance farmer welfare, face structural and managerial challenges that may limit their ability to fully transmit the benefits of coffee booms to smallholder farmers: First, studies show that smallholder farmers who voluntarily join cooperatives often face the challenge of fostering a business-oriented mindset within these organizations, with a key issue being to attract and retain professional managers (Boudreau et al., 2023). Studies also indicate that cooperative members frequently emphasize the importance of having a professional, entrepreneurial manager for a cooperative's ultimate success. However, since cooperatives often prioritize hiring locally, they struggle to find individuals with the necessary expertise (Boudreaux, 2009). In contrast, private CWS tend to operate with a stronger business-oriented approach, benefiting from more developed management structures and a greater ability to recruit experienced professionals (Boudreaux, 2009; Boudreaux & Ahluwalia, 2009). These differences in management capacity could lead to variation in how income is transmitted to farmers, making cooperatives less responsive to price fluctuations compared to private coffee washing stations. Cooperatives could also operate under different pricing structures, typically relying on long-term contracts and fixed price premiums. This approach provides stability for farmers but also makes cooperatives less responsive to fluctuations in global coffee prices. As a result, while both cooperative and private CWS play a role in the local coffee sector, their ability to translate global price changes into tangible economic benefits for farmers may differ significantly.

To test these effects, in Table 1.4 I estimate heterogeneous effects based on proximity to different types of CWS, distinguishing between private and cooperative stations. Specifically, I replace the treatment variable based on coffee-trees with a distance-based measure, indicating whether the

Table 1.4: Heterogeneous Estimates: Effects by proximity to coffee washing station.

	School attendance			
	(1)	(2)	(3)	(4)
Panel A: Proximity to CWS				
$\log(\bar{p}_t^{12}) \times$ CWS in 5km	0.042*			
	(0.022)			
$\log(\bar{p}_t^{12}) \times$ CWS in 6km		0.036*		
		(0.021)		
$\log(\bar{p}_t^{12}) \times$ CWS in 7km			0.042**	
			(0.021)	
$\log(\bar{p}_t^{12}) \times$ CWS in 8km				0.055**
				(0.021)
Observations	20,504	20,504	20,504	20,504
R^2	0.28499	0.28487	0.28493	0.28521
Within R^2	0.26137	0.26125	0.26131	0.26160
Panel B: Proximity to Private CWS				
$\log(\bar{p}_t^{12}) \times$ Private CWS in 5km	0.050**			
	(0.025)			
$\log(\bar{p}_t^{12}) \times$ Private CWS in 6km		0.047**		
		(0.024)		
$\log(\bar{p}_t^{12}) \times$ Private CWS in 7km			0.046*	
			(0.024)	
$\log(\bar{p}_t^{12}) \times$ Private CWS in 8km				0.061***
				(0.023)
Observations	19,174	19,174	19,174	19,174
R^2	0.28737	0.28726	0.28728	0.28757
Within R^2	0.26256	0.26245	0.26247	0.26277
Panel C: Proximity to Cooperative CWS				
$\log(\bar{p}_t^{12}) \times$ Cooperative CWS in 5km	-0.006			
	(0.032)			
$\log(\bar{p}_t^{12}) \times$ Cooperative CWS in 5km		-0.015		
		(0.027)		
$\log(\bar{p}_t^{12}) \times$ Cooperative CWS in 5km			0.0004	
			(0.026)	
$\log(\bar{p}_t^{12}) \times$ Cooperative CWS in 5km				0.007
				(0.024)
Observations	16,624	16,624	16,624	16,624
R^2	0.28745	0.28739	0.28736	0.28777
Within R^2	0.26230	0.26224	0.26221	0.26264
Individual controls	✓	✓	✓	✓
Community controls	✓	✓	✓	✓
CWS controls	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
Survey months fixed effects	✓	✓	✓	✓
District × Year Trend	✓	✓	✓	✓

Notes: LPM estimations. School attendance is based on 2005, 2010 DHS household record data. Individual controls control for respondents' age, age squared, age of the household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters), road density in a 2x2km grid at the cluster's centroid and days with rainfall in 2005 and 2010. CWS controls include age of CWS, elevation, slope, total cherries purchased in 2012 and start year of operation. The independent variable of interest is the interaction between coffee prices P and a dummy indicator for whether the surveyed individual resides within a 5–8 km radius of a coffee washing station. Prices $\bar{p}_t^{12}$ are measured as the logarithm of international prices for arabica coffee during the past 12 months from the survey month. In all estimations, standard errors are in parentheses and clustered at the municipality level. Significance levels are indicated as $*p < 0.1; **p < 0.05; ***p < 0.01$.

individual resides within the catchment area of a CWS (5 km radius around the survey cluster). I use 6-8 km to ensure robustness. Panel A reports the overall effect of coffee price shocks on school attendance for individuals living near any CWS, while Panels B and C separately examine the effects for private and cooperative CWS. If the observed increase in school attendance is primarily driven by improvements in household income, we would expect stronger effects near CWS where price increases translate more directly into economic benefits for coffee-producing households. The results in Panel A indicate that, overall, children living near CWS are more likely to attend school when coffee prices increase, suggesting that local processing infrastructure helps transmit income gains to coffee-growing households.

However, dis-aggregating the results by CWS ownership type reveals important heterogeneity. The effect is concentrated among children living in households near private CWS, where a one log increase in the international price of Arabica coffee leads to a 4.6–6.1 pp increase in school attendance, depending on the distance threshold (Panel B, columns 1-4). In contrast, as shown in Panel C, I do not find a statistically significant effect for cooperative CWS and the positive effect of overall CWS proximity is solely driven by private CWS, suggesting that the positive impact of coffee price increases on school attendance is not uniform across all coffee-producing areas but is instead moderated by the local economic infrastructure. The diverging results for private and cooperative CWS might also be attributable to the different organizational structures. I test some potential mechanisms below.

1.6.2 Effects of coffee prices on completed years of schooling

I now turn to the long-run effects of coffee price shocks and study whether the short-term increases in school attendance translate into long-term gains in completed years of schooling. Table 1.5 presents the effects of coffee price booms on completed years of schooling, which align with the findings for school attendance. Columns 1-3 show results from a specification with no covariates, other than the two-way fixed effect estimators. Columns 4-6 incorporate all the covariates used earlier. I find strong evidence that coffee price shocks also impact completed years of schooling across all specifications. Specifically, for individuals in municipalities with above-median coffee tree densities, the effect is highly significant, with a coefficient of 1.78 (Column 4). To put these numbers into perspective: the difference in log coffee prices between the lowest-price cohort (born

Table 1.5: The effect of coffee price booms on years of schooling

	Completed years of schooling					
	(1)	(2)	(3)	(4)	(5)	(6)
$> \text{P50}(\text{CoffeeTrees}_{\text{municipality}}) \times \log(\bar{p}_t^{144})$	2.76***			1.78***		
	(0.655)			(0.548)		
$\text{CoffeeTrees}_{\text{municipality}} \times \log(\bar{p}_t^{144})$		1.08***			0.680***	
		(0.297)			(0.230)	
$> \text{P50CoffeeTrees}_{\text{municipality}} \times <$ 18 years of age			0.639***			0.467***
			(0.172)			(0.158)
Observations	11,510	11,510	11,510	11,510	11,510	11,510
R^2	0.01776	0.00760	0.01924	0.20746	0.20699	0.20329
Within R^2				0.03541	0.03484	0.03034
Individual controls				✓	✓	✓
Community controls				✓	✓	✓
Municipality fixed effects				✓	✓	✓
Birth year fixed effects				✓	✓	✓
District × Year trend				✓	✓	✓

Notes: LPM estimations. Years of schooling are based on 2015 and 2020 DHS household record data for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2x2km grid at the cluster'centroids. The independent variable is the interaction between the intensity of coffee production C and coffee prices P during school going years. Coffee production is measured as the number of coffee trees per municipality scaled by municipality size or above(1)/below(0) the median number of trees. Prices P are measured as the logarithm of international arabica prices during birth year cohort's schooling years. The variable <18 years of age is a dummy variable indicating that the surveyed individual belongs to an age cohort that was still of school age during the boom in 2010/11. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

in 1985) and the highest-price cohort (born in 2006) is 0.43 which corresponds to 0.77 more years of schooling (0.43 × 1.78 = 0.77 years). This means that children who experienced peak coffee prices during their schooling years completed nearly three-quarters of an additional year of education compared to those who faced the lowest prices.

I estimate several robustness checks for these results. First, the estimated coefficient is robust and highly significant for alternative coffee intensity measures. For the continuous measure of coffee trees, I estimate an increase of 0.68 additional years of schooling per log unit increase in price exposure (Column 5). Second, in Column 6, I use a dummy variable for individuals who were 18 or younger in 2010, at the time of the price boom. The estimated effect of 0.47 years further suggests that those exposed to higher coffee prices during their schooling years remained in school longer. Third, these effects are robust in terms of significance and size to the district level measures of coffee trees (Table A8).

To further assess the robustness of the results, I take advantage of a unique feature in the 2015 and 2020 DHS surveys, which directly asks respondents about their migration history.[15] The surveys provide information on how long each individual has been living in their current community. I restrict the sample to individuals who have been living in their community since 2009 or earlier and have thus not migrated since the coffee price shock in 2010, ensuring that the analysis examines the effects on those who remained in their original locations throughout the period of interest[16]. In Table A9 in the appendix, I re-estimate Eq. 1.8 for this long-term residents subsample. Even though I lose about 60% of the observations, the magnitude and significance of the effects remain consistent with the original findings across all variables and models. This provides strong empirical support that migration is unlikely to be a significant driver of the results.

1.6.2.1 Placebo checks

A potential concern when studying the effects of coffee price shocks on school attendance is the possibility that the observed changes might be driven by parallel price increases in other crops grown alongside coffee, such as maize, beans, and other commodities. In particular, since many coffee farmers in Rwanda are smallholder farmers and also producers of other crops, it is possible that the changes we observe in schooling outcomes are correlated with price fluctuations in maize or beans. While this concern is somewhat unlikely due to the highly spatially disaggregated nature of my analysis, which is based on 416 municipalities with varying levels of coffee intensity, I further investigate this by conducting placebo checks using local maize and beans prices. To this end, I re-run the main models using average annual local producer prices for maize and dry beans from the Food and Agriculture Organization Statistics (FAOSTAT) database as placebo treatments. The results from these placebo checks show no significant effect on school attendance (Table A10). In fact, the estimated coefficients for maize and beans prices are negative, suggesting that price increases for these other crops are not associated with increased school attendance, and may even

[15] This was not possible prior to 2015, as the country's historical context, particularly the legacy of the Rwandan Genocide and tensions between Hutu and Tutsi populations led to the exclusion of migration questions in earlier surveys.

[16] For this purpose, I use the DHS men and women recode data for which migration patterns are collected. While this approach reduces the sample size compared to the Household recode data (which also includes children and other household members who are not the primary DHS respondent), it offers the advantage of allowing me to more precisely identify and isolate migration patterns. However, it is important to note that this sample might differ in its composition to the sample used in the main analysis.

have a slight detrimental effect on schooling outcomes by placing additional financial strain on households, leading to a reduction in resources available for education or other expenditures that support schooling.

1.6.2.2 Heterogeneous effects by proximity to Coffee Washing Station

Next, I test if CWS proximity affects the results on schooling years. Theoretically, we would expect that the proximity to CWS not only affects short-term schooling decisions but also has lasting impacts on educational attainment due to the additional income flow generated by these stations. Table 1.6 estimates the effects of proximity to CWS on schooling outcomes, using distance bands of 5, 6, 7, and 8 km, and breaks down the results by operational status (private (Panel A) vs. cooperative (Panel B)). Again, I find strong evidence that the positive effects on schooling are primarily driven by proximity to private CWS, with an estimated coefficient of about 1.2 to 1.5 for households near private CWS. Given the average log price increase in coffee between cohorts, this corresponds to an increase of half a year to two-thirds of a year of schooling, suggesting a substantial impact on educational attainment. On the other hand, I do not observe significant effects for cooperative CWS, reinforcing the earlier finding that the economic benefits of private CWS are more directly transmitted to local households.

Furthermore, I implement the ”donut” approach, where I exclude communities within the 5-8 km radius from the analysis, treating them as a potential overlap zone. In Panel C and D of Table 1.6, I exclude all clusters that fall into the ”donut area” (5-8 km) and re-estimate the models using the adjusted sample and for cluster distances ranging from 4 to 7 km to a CWS. The estimated effects on schooling years are stronger than those found in the baseline model, suggesting that the effects are concentrated among individuals living in close vicinity of the CWS. Again, I only find positive effects for children in close proximity to private coffee washing stations.

1.6.2.3 Heterogeneous effect by CWS size

Next, I test whether the size of a CWS affects the results and differences across the two ownership types. Prior research has shown that agribusiness-size can be an important driver of farm productivity (Aremu et al., 2024; Gezahegn et al., 2019; Quiroga et al., 2020; Barrett, 1996) and job quality (Fabry et al., 2022), both could plausibly affect the transmission channel of price shocks

Table 1.6: Heterogeneous estimates: years of schooling by proximity to CWS

	Completed years of schooling			
	(1)	(2)	(3)	(4)
Panel A: Proximity to Private CWS				
$\log(\bar{p}_t^{144}) \times$ Private CWS in 5km	1.21*			
	(0.655)			
$\log(\bar{p}_t^{144}) \times$ Private CWS in 6km		1.35**		
		(0.665)		
$\log(\bar{p}_t^{144}) \times$ Private CWS in 7km			1.54**	
			(0.659)	
$\log(\bar{p}_t^{144}) \times$ Private CWS in 8km				1.50**
				(0.705)
Observations	8,946	8,946	8,946	8,946
R^2	0.21744	0.21761	0.21828	0.21830
Within R^2	0.03682	0.03704	0.03786	0.03788
Panel B: Proximity to Cooperative CWS				
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 5km	0.514			
	(0.775)			
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 6km		0.267		
		(0.626)		
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 7km			0.215	
			(0.553)	
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 8km				-0.314
				(0.642)
Observations	7,679	7,679	7,679	7,679
R^2	0.17262	0.17301	0.17257	0.17256
Within R^2	0.04540	0.04584	0.04534	0.04532
Panel C: Proximity to Private CWS (donut approach)				
$\log(\bar{p}_t^{144}) \times$ Private CWS in 4km	2.00**			
	(0.854)			
$\log(\bar{p}_t^{144}) \times$ Private CWS in 5km		1.89**		
		(0.744)		
$\log(\bar{p}_t^{144}) \times$ Private CWS in 6km			1.75**	
			(0.713)	
$\log(\bar{p}_t^{144}) \times$ Private CWS in 7km				1.73**
				(0.690)
Observations	6,323	7,158	7,805	8,314
R^2	0.24366	0.23589	0.22603	0.22247
Within R^2	0.02986	0.02848	0.02822	0.03181
Panel D: Proximity to Cooperative CWS (donut approach)				
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 4km	0.866			
	(0.902)			
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 5km		0.533		
		(0.785)		
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 6km			0.307	
			(0.632)	
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 7km				0.064
				(0.596)
Observations	5,974	5,974	6,591	7,124
R^2	0.15736	0.15726	0.15676	0.16317
Within R^2	0.04385	0.04373	0.04500	0.04480
Individual controls	✓	✓	✓	✓
Community controls	✓	✓	✓	✓
CWS controls	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓
District × Year Trend	✓	✓	✓	✓

Notes: LPM estimations. Individual's completed years of schooling are based on 2015 and 2020 DHS household record data for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2x2km grid at the cluster'centroids. CWS controls include age of CWS, elevation, slope, total cherries purchased in 2012 and start year of operation. The independent variable of interest is the interaction between coffee prices P and a dummy indicator for whether the surveyed individual resides within a 5 to 8 km radius (4 - 7 km in the donut approaches (Panel C and Panel D)) of a coffee washing station (private or cooperatively organized). Prices $\bar{p}_t^{144}$ are measured as the logarithm of the average global prices for arabica coffee during birth year's 144 months (12 years) of schooling. In all estimations, standard errors are in parentheses and clustered at the municipality level. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

Table 1.7: Heterogenous estimates by CWS size

	Completed years of schooling			
	Full Sample	Donut	Full Sample	Donut
$\log(\bar{p}_t^{144}) \times$ Private CWS in 5km	1.52*	2.32**		
	(0.878)	(0.945)		
$\log(\bar{p}_t^{144}) \times$ Private CWS in 5km $\times >$ P50(Cherries purchased)	-0.719	-0.958		
	(1.30)	(1.46)		
$\log(\bar{p}_t^{144}) \times$ CoopCWS_5km			0.666	0.841
			(1.12)	(1.09)
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 5km $\times >$ P50(Cherries purchased)			0.278	-0.413
			(1.61)	(1.60)
Observations	8,946	7,158	7,679	5,974
R^2	0.21775	0.23605	0.17070	0.15862
Within R^2	0.03720	0.02867	0.04318	0.04528
Individual controls	✓	✓	✓	✓
Community controls	✓	✓	✓	✓
CWS controls	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
Birth Year fixed effects	✓	✓	✓	✓
District $\times$ Year Trend	✓	✓	✓	✓

Notes: LPM estimations. Individual's completed years of schooling are based on 2015 and 2020 DHS household record data for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2x2km grid at the cluster'centroids. CWS controls include age of CWS, elevation, slope, total cherries purchased in 2012 and start year of operation. The independent variable of interest is the interaction between coffee prices P and a dummy indicator for whether the surveyed individual resides within a 5 to 8 km radius (4 - 7 km in the donut approaches (Panel C and Panel D)) of a coffee washing station (private or cooperatively organized). Prices $\bar{p}_t^{144}$ are measured as the logarithm of the average international prices for arabica coffee during birth year's 144 months (12 years) of schooling. In all estimations, standard errors are in parentheses and clustered at the municipality level. Significance levels are indicated as $*p < 0.1; **p < 0.05; ***p < 0.01$

on the global market to local farmers. CWS differ in their installed processing capacity, affecting the extent they source coffee cherries from surrounding communities and their productivity. On the one hand, large CWS tend to have greater capacity to source coffee cherries from a wider network of farmers and may therefore operate at a more commercial scale. Their ability to process larger volumes could improve market access, enhance productivity through economies of scale, and enable them to offer higher payments in response to price shocks. On the other hand, smaller CWS might make better use of inputs, such as labor, or be more embedded in the local community, fostering closer relationships with farmers. This stronger local integration could lead to more direct and immediate price transmission from the global market to individual farmers.

To test this, I use data on a CWS' total cherries purchased per year to proxy for station size. I interact the main catchment area variable (5 km) with an additional indicator for whether the CWS processed above-median coffee cherry volumes. The results are presented in Table 1.7 and show that the schooling effects for private CWS are not driven by larger stations. Instead, for both, the full sample and the donut approach, the effects are concentrated among smaller CWS suggesting that local economic linkages—rather than the sheer scale of coffee processing—are the key drivers of increased schooling outcomes. Once again, I find significant effects only for privately organized coffee washing stations, reinforcing the idea that private CWS transmit income shocks to local farmers more effectively than their cooperative counterparts, but only if they are small.

1.6.2.4 High quality coffee washing stations

Next, I investigate the effects using the sample of only high quality producing CWS. It is plausible that private and cooperative stations may differ in the quality of the beans they produce, potentially affecting CWS' profit margins and how price changes translate into household income and schooling outcomes. The idea behind this approach is twofold: first, by controlling for homogeneous quality, I rule out that the effects are driven by differences in coffee quality. Second, theoretically, cooperatives could more often follow fixed pricing structures with long-term contracts and stable price premiums. The observed effects for private CWS in the main analysis could then be driven by the fact, that only these stations react to price fluctuations. This would imply that the reason for cooperatives for not showing positive effects, is not because they do not transmit income gains but rather, that their price stability shields them from price shocks.

To empirically test this, I use the sample of Cup of Excellence participating, specialty CWS to explore whether the effects persist in a sample where price premiums and product quality are constant across CWS ownership types and relatively high. Only specialty coffee can access high specialty premiums on the global market (Guariso et al., 2011). Due to their complex processing methods and quality standards, farmers producing high quality, specialty coffee typically operate under long-term contract including price premiums that hedge against default and provide more stable earnings. Thus in the sample of specialty CWS (independent of ownership status) these price premiums should be relatively equally distributed.

Interestingly, the results align with the overall sample: Private specialty coffee washing stations remain the main driver behind the observed increases in schooling in this subsample of specialty coffee washing stations. The effects are even larger in both panels, the full sample (Panel A of Table 1.8) and the donut approach (Panel B of Table 1.8). Again , I do not find an effect for

Table 1.8: Effects by Proximity to Specialty CWS

	Completed Years of Schooling								
	All	Coop.	Private	All	Coop	Private	All	Coop	Private
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Specialty Coffee Washing Stations									
$\log(\bar{p}_t^{144}) \times$ SpecialtyCWSin5	0.756	-0.319	1.66						
	(0.730)	(0.906)	(1.14)						
$\log(\bar{p}_t^{144}) \times$ SpecialtyCWSin6				1.18^{*}	-0.097	2.16^{**}			
				(0.672)	(0.836)	(0.990)			
$\log(\bar{p}_t^{144}) \times$ SpecialtyCWSin7							1.25^{**}	0.115	2.08^{**}
							(0.600)	(0.846)	(0.862)
Observations	10,744	4,838	5,906	10,744	4,838	5,906	10,744	4,838	5,906
R^2	0.206	0.176	0.226	0.206	0.176	0.226	0.206	0.176	0.226
Panel B: Specialty Coffee Washing Stations (donut approach)									
$\log(\bar{p}_t^{144}) \times$ SpecialtyCWSin5	0.865	-0.310	2.02^{*}						
	(0.743)	(0.932)	(1.20)						
$\log(\bar{p}_t^{144}) \times$ SpecialtyCWSin6				1.28^{**}	-0.248	2.13^{**}			
				(0.619)	(0.875)	(0.893)			
$\log(\bar{p}_t^{144}) \times$ SpecialtyCWSin7							1.21^{*}	-0.014	2.25^{**}
							(0.682)	(0.869)	(1.02)
Observations	10,247	4,697	5,550	10,247	4,697	5,550	10,247	4,697	5,550
R^2	0.213	0.184	0.230	0.213	0.183	0.233	0.212	0.180	0.236
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Community Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
CWS controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Birth Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
District × Year Trend	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: LPM estimations. Individual's completed years of schooling are based on 2015 and 2020 DHS household record data for the sample of individuals aged 18 or older. Individual controls control for respondents' gender, age, and urban status (1 if yes). Cluster controls include altitude (in meters), road density in a 2x2km and days with rainfall 2005 and 2010 grid at the cluster'centroids. The independent variable of interest is the interaction between coffee prices P and a dummy indicator for whether the surveyed individual resides within a 5 to 7 km radius to a specialty coffee washing station (private or cooperatively organized) measured by a CWS's rank in the Cup of Excellence in 2008, 2009, and 2011. Coffee Washings Stations are filtered for opening dates prior to 2010 only. CWS controls include year of operation. Prices $\bar{p}_t^{144}$ are measured as the logarithm of the average international prices for arabica coffee during birth year's 144 months (12 years) of schooling. In all estimations, standard errors are in parentheses and clustered at the municipality level. Significance levels are indicated as $*p < 0.1$; $**\,p < 0.05$; $***p < 0.01$

cooperatives, if any, the effect is negative. If price premiums or product quality were the primary mechanism behind the observed schooling effects, we would expect no significant impact in this subsample, as these farmers already benefit from stable, high prices regardless of global market movements and they produce the best cherries on the market. Thus, these findings reinforce the idea, that other farm practices, e.g. farm management and operation are key factors behind the main results and access to price premiums and product quality are not driving the results.

1.6.3 Mechanisms

1.6.3.1 Income effect

The results suggest that income effects during coffee booms play a key role in driving educational investments, outweighing potential substitution effects from children working or dropping out of school. In this section, I investigate whether the observed improvements in schooling outcomes can be attributed to increases in household wealth resulting from higher coffee prices. Ideally, this mechanism would be tested using direct measures of individual income, but unfortunately such data is not available in the DHS surveys. To approximate household income, I use the DHS wealth index, a widely adopted proxy for income. This index is a composite measure based on household assets, housing quality, and access to essential services, constructed via principal component analysis. The index accounts for asset ownership (e.g., radio, television, mobile phone, refrigerator, and vehicle), housing conditions (e.g., type of flooring and roofing materials), and access to utilities (e.g., drinking water source and sanitation facilities). The resulting score is divided into quintiles, where the first quintile represents the poorest households and the fifth quintile represents the wealthiest.

To assess whether the coffee boom translates into higher household wealth, I estimate the same model specification as in equation 1.9, using the wealth index as the dependent variable. The sample differs slightly to the one used for educational attainment, now reflecting the wealth index of the household head and not of individual household member in school. Table 1.9 presents the results for the treatment measure of coffee trees. A one standard deviation increase in global coffee prices leads to a 0.513 increases in household wealth for areas above median coffee trees. Similarly to earlier results, I only find positive effects for the subsample of private and not for cooperatively managed CWS with estimated effect sizes of the same magnitude (Table 1.10). These results provide strong evidence that increased income is a primary driver of the observed educational gains and that particularly household income near private CWS and after price booms is the key mechanism behind the results.

Table 1.9: Mechanism: The effect of coffee price booms on households' wealth

	Wealth index					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Coffee Trees in Municipality						
$>$ P50(CoffeeTrees) $\times \log(\bar{p}_t^{144})$	0.818***			0.531***		
	(0.298)			(0.191)		
$\text{CoffeeTrees}_{\text{municipality}} \times \log(\bar{p}_t^{144})$		0.319**			0.159	
		(0.145)			(0.117)	
$>$ $\text{P50CoffeeTrees}_{\text{municipality}} \times <$ 18 years of age			0.221***			0.165***
			(0.069)			(0.050)
Observations	10,003	10,003	10,003	10,003	10,003	10,003
R^2	0.03953	0.02482	0.03810	0.51785	0.51752	0.51785
Individual controls				✓	✓	✓
Community controls				✓	✓	✓
Municipality fixed effects				✓	✓	✓
Birth year fixed effects				✓	✓	✓
District $\times$ year trend				✓	✓	✓

Notes: LPM estimations. Wealth is based on 2015 and 2020 DHS household record data (hv270) for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2x2km grid at the cluster'centroids. The independent variable is the interaction between the intensity of coffee production C and coffee prices P during school going years. Coffee production is measured as the number of coffee trees per municipality scaled by municipality size or above(1)/below(0) the median number of trees. Prices P are measured as the logarithm of international arabica prices during birth year's 12 years of schooling. The variable <18 years of age is a dummy variable indicating that the surveyed individual belongs to an age cohort that was still of school age during the boom in 2010/11. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

1.6.3.2 Alternative mechanisms

Unlike staple crops such as maize or beans, coffee is not a direct contributor to household nutrition. If staple crops become expensive, poor households cannot afford them which can affect household nutrition in coffee regions and overall health with consequences on schooling (Mekasha et al., 2022; Sharma, 2022; Beshir & Maystadt, 2023). In contrast, coffee is a cash crop, meaning its price increases does not directly affect nutrition or health status. Thus, if rising coffee prices lead to higher household income, one could expect to see improvements in other indicators of household wealth, such as health-related investments. To test this, I estimate the effects on available health outcomes in the DHS using both, the full sample (Columns 1-3) and the subsample of non-movers (Columns 4-6) in Table 1.11. I use two widely used proxies for health status from the DHS: individual's health insurance coverage and the use of insecticide-treated nets (ITNs). Health insurance is measured as a binary variable indicating whether an individual is covered by health insurance. The use of insecticide-treated nets is measured by the number of nets reported to be used by the household. I find slight increases for the use of mosquito nets,

Table 1.10: Mechanism: The effect of coffee price booms on households' wealth near CWS

	Wealth index					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Proximity to Private CWS						
	Full Sample	Donut	Full Sample	Donut	Full Sample	Donut
$\log(\bar{p}_t^{144}) \times$ Private CWS in 5km	0.398	0.496*				
	(0.283)	(0.300)				
$\log(\bar{p}_t^{144}) \times$ Private CWS in 6km			0.644**	0.580**		
			(0.273)	(0.287)		
$\log(\bar{p}_t^{144}) \times$ Private CWS in 7km					0.541**	0.495*
					(0.254)	(0.261)
Observations	7,705	6,256	7,705	6,714	7,705	7,168
R^2	0.46367	0.49903	0.46415	0.48148	0.46401	0.46658
Panel C: Proximity to Cooperative CWS						
	Full Sample	Donut	Full Sample	Donut	Full Sample	Donut
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 5km	0.418	0.233				
	(0.280)	(0.346)				
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 6km			0.050	0.290		
			(0.291)	(0.313)		
$\log(\bar{p}_t^{144}) \times$ Cooperative CWS in 7km					0.288	0.305
					(0.266)	(0.279)
Observations	6,470	5,223	6,470	5,639	6,470	6,093
R^2	0.33715	0.35356	0.33728	0.34475	0.33688	0.32848
Individual controls	✓	✓	✓	✓	✓	✓
Community controls	✓	✓	✓	✓	✓	✓
CWS controls	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓	✓
District × year trend	✓	✓	✓	✓	✓	✓

Notes: LPM estimations. Wealth is based on 2015 and 2020 DHS household record data (hv270) for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2x2km grid at the cluster'centroids. CWS controls include age of CWS, elevation, slope, total cherries purchased in 2012 and start year of operation. The independent variable is the interaction between the intensity of coffee production C and coffee prices P during school going years. Coffee production is measured as the number of coffee trees per municipality scaled by municipality size or above(1)/below(0) the median number of trees. Prices P are measured as the logarithm of international arabica prices during birth year's 12 years of schooling. The variable <18 years of age is a dummy variable indicating that the surveyed individual belongs to an age cohort that was still of school age during the boom in 2010/11. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

but the effects are not very robust across specifications. Thus, these findings indicate that health outcomes are not significantly affected by the increased incomes and much of the income effect could be absorbed by schooling.

1.6.3.3 Public spending on schools

In addition to private income gains for coffee-producing individuals, rising coffee prices could also lead to higher public spending on schools, which in turn may contribute to the positive effects

Table 1.11: Mechanism: Income effects for health expenditures

	Covered by health insurance (1 if yes)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{CoffeeTrees}_{\text{municipality}} \times \times \log(\bar{p}_t^{144}))$	-0.111			-0.094		
	(0.106)			(0.129)		
$\text{CoffeeTrees}_{\text{municipality}} \times \times \log(\bar{p}_t^{144})$		0.019			0.056	
		(0.059)			(0.079)	
$\text{CoffeeTrees}_{\text{municipality}} \times <$ 18in 2010			-0.011			-0.032
			(0.026)			(0.031)
Observations	5,884	5,884	5,884	3,813	3,813	3,813
R^2	0.24717	0.24695	0.24720	0.30489	0.30496	0.30574
	Insecticide-treated nets					
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{CoffeeTrees}_{\text{municipality}} \times \times \log(\bar{p}_t^{144}))$	0.132			0.042		
	(0.100)			(0.122)		
$\text{CoffeeTrees}_{\text{municipality}} \times \times \log(\bar{p}_t^{144})$		0.101**			0.098*	
		(0.045)			(0.058)	
$\text{CoffeeTrees}_{\text{municipality}} \times <$ 18in 2010			0.041			0.025
			(0.028)			(0.032)
Observations	5,884	5,884	5,884	3,813	3,813	3,813
R^2	0.28186	0.29732	0.29616	0.32236	0.32290	0.32213
Individual controls	✓	✓	✓	✓	✓	✓
Community controls	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓	✓
District × year trend	✓	✓	✓	✓	✓	✓

Notes: LPM estimations. Full sample (Columns 1-3) and Non-movers sample (columns 4-6). Health insurance is measured as (1) if individual is covered by health insurance. Insecticide-treated nets measure the number of insecticide-treated nets used by the household. is based on 2015 and 2020 DHS men and individual record data for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2x2km grid at the cluster'centroids. CWS controls include age of CWS, elevation, slope, total cherries purchased in 2012 and start year of operation. The independent variable is the interaction between the intensity of coffee production C and coffee prices P during school going years. Coffee production is measured as the number of coffee trees per municipality scaled by municipality size or above(1)/below(0) the median number of trees. Prices P are measured as the logarithm of international arabica prices during the past 12 years from the survey month. The variable <18 years of age is a dummy variable indicating that the surveyed individual belongs to an age cohort that was still of school age during the boom in 2010/11. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

on education. To test this mechanism, I examine both the intensive margin (number of teachers) and the extensive margin (number of schools) of public spending on schools in Rwandan districts. I use administrative data from the Ministry of Education (MINEDUC) in Rwanda to explore these relationships. Specifically, I examine the number of active schools per district and the number of teachers per district as indicators of public spending on schools. These variables are provided on an annual basis from 2005 to 2020 and are measured at the district level. I include district and year fixed effects. Table A11 presents the results. Coffee booms do not have any significant effects on public school provision, suggesting that changes in public school supply are unlikely to be a key mechanism driving the positive effect of coffee price fluctuations on educational outcomes.

1.7 Conclusion

This paper studies the effects of coffee price booms on schooling in Rwanda and explores heterogeneous effects across exposure to private and cooperative coffee washing stations. The results suggest that higher global prices for Arabica coffee increase both school attendance and educational attainment in Rwanda. The results are robust to various treatment intensity measures, placebo price shocks for maize and beans and a subsample for individuals without migration history. These findings contrast with prior evidence on coffee booms in Brazil and Columbia, who found negative effects on schooling outcomes (Carrillo, 2020; Kruger, 2007). I propose a potential explanation for this pattern: Rwanda's smallholder farming structure—where coffee is cultivated as a cash crop in low quantities per farmer alongside staple crops—may reduce child labor due to lower production quantities per farmer and farm. Many farmers harvest only small amounts of cherries and sell directly to the nearest CWS, limiting work opportunities for children along the value chain. I provide suggestive evidence for this pattern by showing that the main effects are driven solely by small CWS, reinforcing the idea that small-scale farms play a key role in driving positive schooling outcomes. This aligns with prior research showing that smaller agribusinesses often have a more positive impact than larger ones (Aremu et al., 2024; Fabry et al., 2022).

Furthermore, a key finding of my study is that privately managed CWS are the primary driver of the observed education effects, whereas I do not find any effects in the vicinity of cooperative-run CWS. This finding is particularly relevant to the ongoing debate about the role of cooperatives in Rwanda (Elder et al., 2012; Ortega et al., 2019; Munir, 2022). Importantly, my results are not driven by differences in product quality or access to price premiums, suggesting that other mechanisms are at play. While I cannot pin down the exact mechanism, previous research has highlighted some challenges of coffee cooperatives in Rwanda. Cooperatives often struggle with organizational and managerial challenges. Many face issues such as financial mismanagement, lack of transparency, lack of strong managerial leadership and difficulties in fulfilling contracts on time (Boudreaux, 2009). Low trust in farm managers can further limit their effectiveness

(Elder et al., 2012). Studies also suggest that cooperatives often fill management positions with local individuals rather than hiring outsiders with specialized skills, potentially limiting their efficiency and ability to implement best practices in production and price distribution (Boudreaux, 2009). Given that research on price transmission in the coffee sector is already scarce (Tamru & Minten, 2023; Kebede, 2022) further research is needed to understand the exact differences in price transmission and to explore why cooperative CWS, despite their ability to produce high-quality coffee, do not generate the same positive income and education effects observed near privately managed CWS.

This study has some further limitations. First, the random displacement of the DHS data prevents me from studying more direct effects, and the DHS also limits my ability to measure employment and child labor on coffee farms and CWS more directly. Studying this could further help explain the insignificant findings for cooperatives. It is not implausible to assume that if mothers living near cooperatives have higher labor force participation—for example, by working on farms or selling cherries to cooperative CWS—children could take on more domestic responsibilities, potentially reducing their time for schooling. Future research should explore the intra-household allocation of labor and occurrences of child labor in coffee production, particularly in the washing process and on smallholder farms, through targeted surveys with smallholder farmers and CWS-management for both, cooperative and private stations. Second, I have no information on a child's performance in school, e.g. test scores. Skills however, are an important driver of long-term economic growth (Gust et al., 2024). Lastly, I am unable to explain whether my results and those of prior studies can be attributed to other factors such as stronger institutions, variations in cultural norms, preferences for education, or individual differences in risk and time preferences, that could potentially influence young people's perception of schooling in times of commodity booms. Young people in Rwanda count discount future education benefits differently than young people from other context. Exploring this, presents a promising avenue for future research.

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A1 Appendix

A1.1 Definition of catchment area of Coffee Washing Station

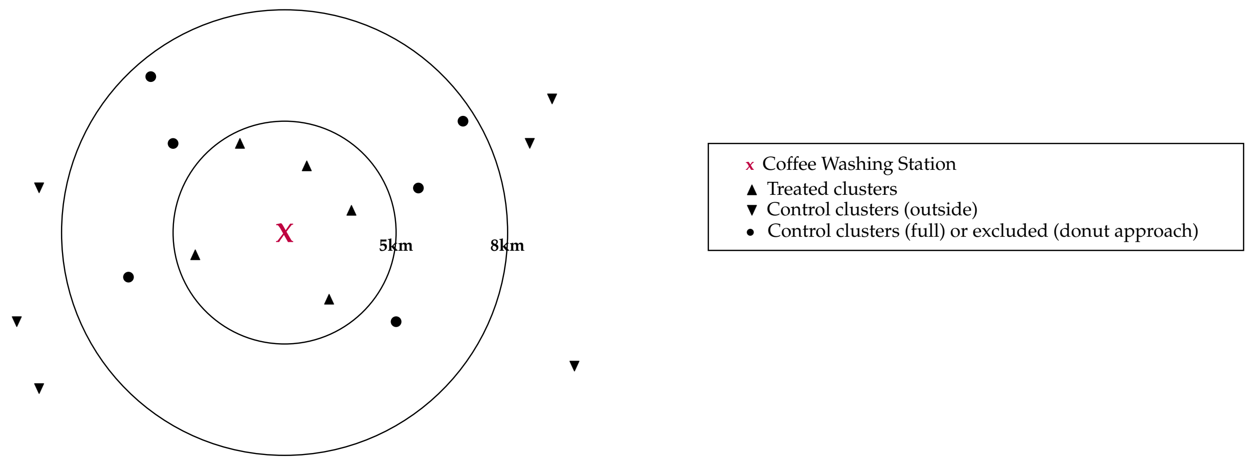

Fig A1: Graphical representation of catchment area of CWS

This figure is a graphical depiction of my definition of the vicinity of the coffee washing station. The inner circle depicts my baseline measure of the catchment area using a 5km radius around a coffee washing station. The outer circle depicts the alternative measure of the catchment area using a 8km radius around a CWS. The dots (triangles, dots, upside-down triangles) depict cluster locations while the red X depicts a CWS. Using the baseline measure of catchment area, ”triangles” represent treated locations within 5km, The dots represent treated or control locations outside the 5km radius, depending on the approach (full vs donut). The upside-down triangles represent control locations outside the vicinity of the CWS.

A1.2 Arabica prices and Rwandan exports of green coffee beans from 2005 to 2020

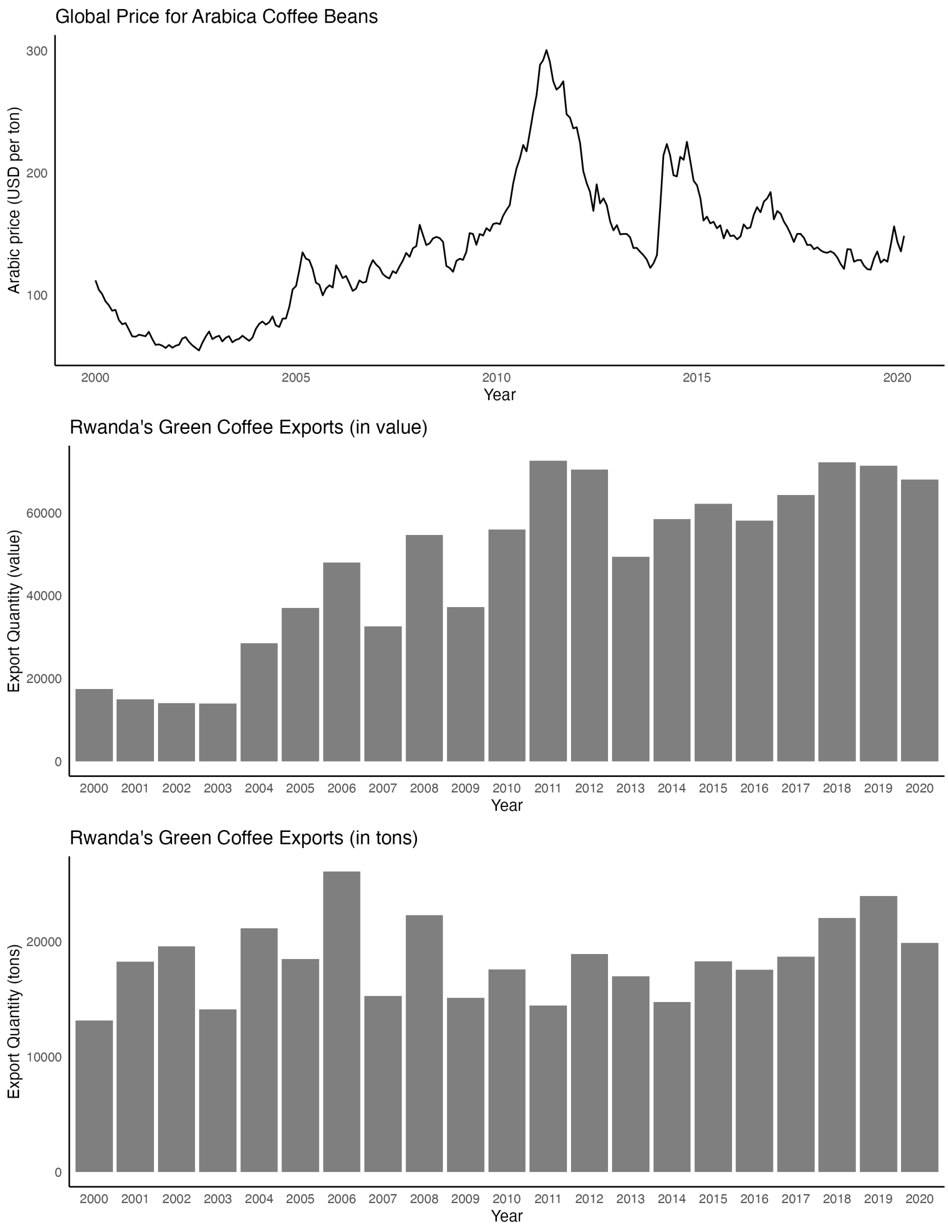

Fig A2: Evolution of prices for Arabica beans (top) and country level exports of green coffee beans in value (middle) and tons (bottom) between the survey period of 2005-2020. Sources: IMF (coffee prices) and FAOSTAT (export data). Author's calculations.

A1.3 Robustness check I: Aggregation at district level (school attendance)

Table A3: Robustness check: Aggregation at the district level

	School attendance					
	(1)	(2)	(3)	(4)	(5)	(6)
$>$ P50Coffee Trees$_{\text{district}} \times \log(\bar{p}_t{}^{12})$	0.046***		0.040***		0.035***	
	(0.008)		(0.003)		(0.003)	
CoffeeTrees$_{\text{district}} \times \log(\bar{p}_t{}^{12})$		0.013***		0.015***		0.014***
		(0.004)		(0.002)		(0.002)
Observations	22,175	22,175	22,175	22,175	22,175	22,175
R^2	0.01054	0.00957	0.27746	0.27729	0.27777	0.27771
Within R^2			0.26548	0.26531	0.26580	0.26574
Individual controls			✓	✓	✓	✓
Community controls			✓	✓	✓	✓
District fixed effects			✓	✓	✓	✓
Survey Months fixed effects			✓	✓	✓	✓
Province X Year Trend					✓	✓

Notes: LPM estimations. School attendance is based on 2005, 2010 DHS household record data. Individual controls control for respondents' age, age squared, age of the household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters), road density in a 2x2km grid at the cluster's centroid and days with rainfall in 2005 and 2010. The independent variable of interest is the interaction between the intensity of coffee production C and coffee prices P. Coffee production is measured as above (1) or below (0) the median number of coffee trees per district scaled by district size or the standardized measure of coffee trees per district. Prices $\bar{P}_t$ are measured as the logarithm of international prices for arabica coffee during the past 12 months from the survey month. In all estimations, standard errors are in parentheses and clustered at the district level. Significance levels are indicated as $*p < 0.1$; $**\,p < 0.05$; $***p < 0.01$.

A1.4 Robustness check II: varying price exposure measures

Table A4: Robustness check: Various price exposure measures

	School attendance					
	(1)	(2)	(3)	(4)	(5)	(6)
$>$ P50Coffee $\text{Trees}_{\text{municipality}} \times \log(\bar{p}_t^6)$	0.066***					
	(0.023)					
$>$ P50CoffeeTrees$_{\text{municipality}} \times \log(\bar{p}_t^3)$		0.066***				
		(0.023)				
$>$ P50Coffee $\text{Trees}_{\text{municipality}} \times \log(p_t)$			0.059***			
			(0.021)			
CoffeeTrees$_{\text{municipality}} \times \log(\bar{p}_t^6)$				0.031**		
				(0.014)		
CoffeeTrees$_{\text{municipality}} \times \log(\bar{p}_t^3)$					0.033**	
					(0.014)	
CoffeeTrees$_{\text{municipality}} \times \log(p_t)$						0.033**
						(0.013)
Observations	22,175	22,175	22,175	22,175	22,175	22,175
R^2	0.29165	0.29163	0.29161	0.29151	0.29153	0.29159
Within R^2	0.26875	0.26873	0.26871	0.26861	0.26863	0.26869
Individual controls	✓	✓	✓	✓	✓	✓
Community controls	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Survey Months fixed effects	✓	✓	✓	✓	✓	✓
District X Year Trend	✓	✓	✓	✓	✓	✓

Notes: LPM estimations. School attendance is based on 2005, 2010 DHS household record data. Individual controls control for respondents' age, age of household head, years of education of the household head, urban status (1 if yes). Community controls include altitude (in meters) and road density in a 2x2km grid at the cluster centroids. The independent variable is the interaction between the intensity of coffee production C and coffee prices P. Coffee production is measured as above (1) or below (0) the median number of coffee trees per municipality scaled by municipality size or the standardized measure of coffee trees per municipality. Prices P are measured as the logarithm of international arabica prices during the past 6 months, 3 months from and the current price during the survey month. In all estimations, standard errors are in parentheses and clustered at the municipality level. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

A1.5 The effect of coffee price booms by age groups

Table A5: The effect of coffee price booms by age groups

	School attendance					
Age Group	Primary	Lo. Secondary	Up. Secondary	Primary	Lo. Secondary	Up. Secondary
	(1)	(2)	(3)	(4)	(5)	(6)
$>$ P50(CoffeeTrees) $\times \log(\bar{p}_t^{12})$	0.024*	0.082**	0.159***			
	(0.012)	(0.039)	(0.059)			
ln(CoffeeTrees) $\times \log(\bar{p}_t^{12})$				0.005	0.022	0.102**
				(0.006)	(0.022)	(0.044)
Observations	11,943	5,464	4,768	11,943	5,464	4,768
R^2	0.07282	0.12883	0.18451	0.07248	0.12796	0.18561
Within R^2	0.01595	0.04525	0.06280	0.01559	0.04430	0.06407
Individual controls	✓	✓	✓	✓	✓	✓
Community controls	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Survey Months fixed effects	✓	✓	✓	✓	✓	✓
District X Year Trend	✓	✓	✓	✓	✓	✓

Notes: LPM estimations. School attendance is based on 2005, 2010 DHS household record data. Individual controls control for respondents' age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters), road density in a 2x2km grid at the cluster centroids. The independent variable is the interaction between the intensity of coffee production C and coffee prices P. Coffee production is measured as above (1) or below (0) the median number of coffee trees per municipality scaled by municipality size or the standardized measure of coffee trees per municipality. Prices P are measured as the logarithm of international arabica prices during the past 12 months from the survey month. Primary school children are aged 6–12, lower secondary school children 13–15, and upper secondary school children 16–18 years. In all estimations, standard errors are in parentheses and clustered at the municipality level. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

A1.6 Addressing short term migration patterns

Table A6: Alternative sample to account for migration (school attendance)

	School attendance					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Excluding Kigali districts						
$>$ P50($\text{CoffeeTrees}_{\text{municipality}}$) $\times \log(\bar{p}_t^{12})$	0.071***		0.068***		0.067***	
	(0.023)		(0.021)		(0.023)	
$\text{CoffeeTrees}_{\text{municipality}} \times \log(\bar{p}_t^{12})$		0.023**		0.030**		0.031**
		(0.011)		(0.012)		(0.015)
Observations	22,175	22,175	22,175	22,175	22,175	22,175
R^2	0.01074	0.01080	0.29144	0.29126	0.29185	0.29170
Within R^2			0.26853	0.26835	0.26896	0.26880
Panel B: Excluding Urban DHS clusters						
$>$ P50($\text{CoffeeTrees}_{\text{municipality}}$) $\times \log(\bar{p}_t^{12})$	0.065***		0.057**		0.056**	
	(0.024)		(0.022)		(0.024)	
$\text{CoffeeTrees}_{\text{municipality}} \times \log(\bar{p}_t^{12})$		0.020*		0.031**		0.036**
		(0.012)		(0.013)		(0.016)
		(0.010)		(0.011)		(0.013)
Observations	19,902	19,902	19,902	19,902	19,902	19,902
R^2	0.01117	0.01117	0.28938	0.28918	0.28984	0.28966
Within R^2			0.26591	0.26570	0.26638	0.26620
Individual controls			✓	✓	✓	✓
Community controls			✓	✓	✓	✓
Municipality fixed effects			✓	✓	✓	✓
Survey Months fixed effects			✓	✓	✓	✓
District $\times$ Year Trend					✓	✓

Notes: LPM estimations. Panel A estimates the main model without the three districts of Kigali, Gasabo, Kicukiro, Nyarugenge. Panel B excludes all urban districts (n=4115) from the analysis. School attendance is based on 2005, 2010 DHS household record data. Individual controls control for respondents' age, age squared, age of the household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters), road density in a 2x2km grid at the cluster's centroid and days with rainfall in 2005 and 2010. The independent variable of interest is the interaction between the intensity of coffee production C and coffee prices P. Coffee production is measured as above (1) or below (0) the median number of coffee trees per municipality scaled by municipality size or the standardized measure of coffee trees per municipality. Prices $\bar{p}_t^{12}$ are measured as the logarithm of international prices for arabica coffee during the past 12 months from the survey month. In all estimations, standard errors are in parentheses and clustered at the municipality level. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

A1.7 Difference in school years relative to coffee prices

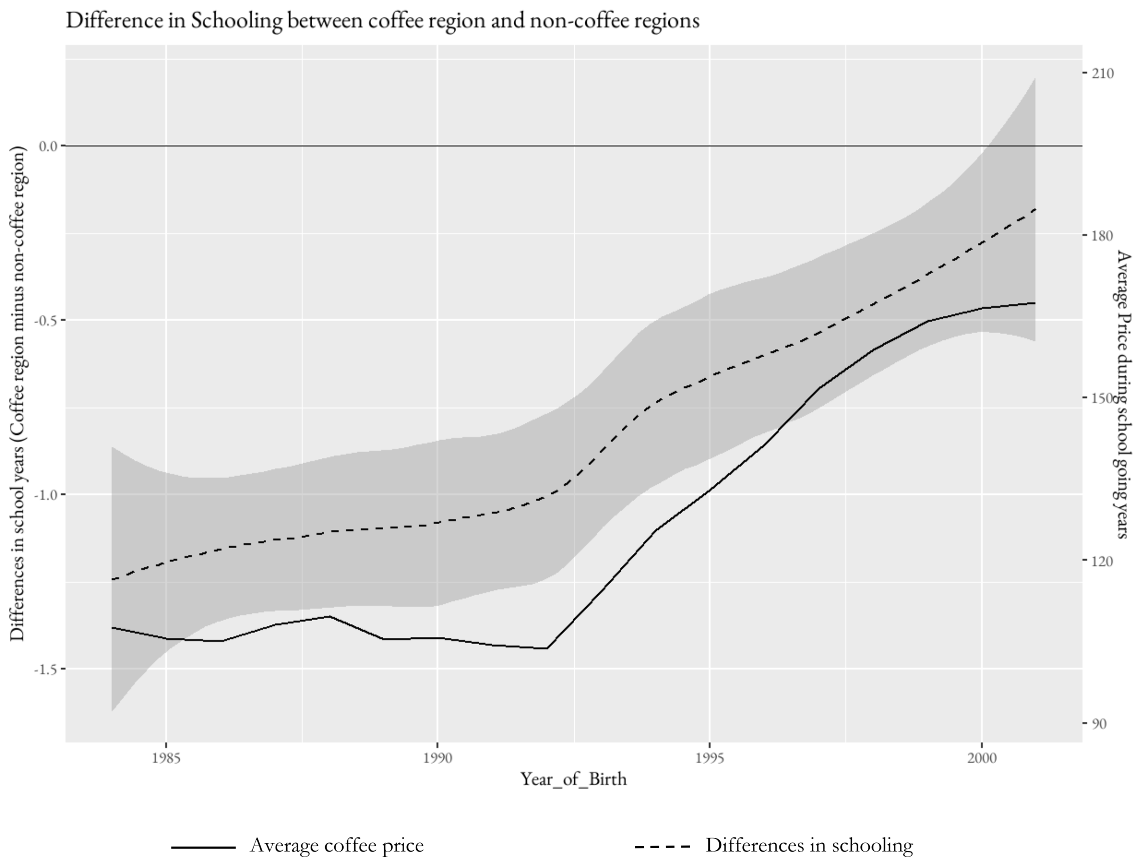

Fig A7: Average difference in years of schooling by regions

This figure illustrates the average coffee price faced by cohorts during their school years (over a 12 years period) and the difference in years of schooling between individuals born in municipalities with a high coffee intensity (above median number of trees) and those born in municipalities with a low cultivation intensity (below median number of trees). The figure shows that coffee booms are associated with increased years of schooling as the difference between the two regions decreases with increasing coffee prices. This idea is generalized to a regression framework that exploits greater variation across municipalities and cohorts in Eq. 1.9.

A1.8 Robustness check III: Aggregation at district level (years of schooling)

Table A8: Robustness check: Aggregation at the district level

	hv108					
	(1)	(2)	(3)	(4)	(5)	(6)
$>$ P50(Coffee Trees$_{\text{district}}$) $\times \log(\bar{p}_t^{144})$	2.49***			2.01***		
	(0.659)			(0.548)		
Coffee Trees$_{\text{district}}$ $\times \log(\bar{p}_t^{144})$		1.16***			0.911***	
		(0.296)			(0.250)	
$>$ P50 Coffee Trees$_{\text{district}}$ $\times <$ 18in 2010			0.482***			0.366**
			(0.175)			(0.162)
Observations	11,510	11,510	11,510	11,510	11,510	11,510
R^2	0.01593	0.01329	0.01705	0.20773	0.20745	0.20292
Within R^2				0.03574	0.03540	0.02989
Individual controls				✓	✓	✓
Community controls				✓	✓	✓
District fixed effects				✓	✓	✓
Survey Months fixed effects				✓	✓	✓
Province $\times$ Year Trend				✓	✓	✓

Notes: LPM estimations. Years of schooling are based on 2015 and 2020 DHS household record data for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2x2km grid at the cluster'centroids. The independent variable is the interaction between the intensity of coffee production C and coffee prices P during school going years. Coffee production is measured as the number of coffee trees per district scaled by district size or above(1)/below(0) the median number of trees. Prices $\bar{p}_t^{12}$ are measured as the logarithm of international arabica prices during birth year's 12 years of schooling. The variable <18 years of age is a dummy variable indicating that the surveyed individual belongs to an age cohort that was still of school age during the boom in 2010/11. Significance levels are indicated as $*p < 0.1$; $**\, p < 0.05$; $***p < 0.01$

A1.9 Robustness check IV: Sample of long-time residents

Table A9: The effect of coffee price booms on years of schooling (non-moving sample)

	hv108		
	(1)	(2)	(3)
$> \text{P50(Coffee Trees}_{\text{municipality}}) \times \log(\bar{p}_t^{144})$	1.58*		
	(0.896)		
$\text{Coffee Trees}_{\text{municipality}} \times \log(\bar{p}_t^{144})$		0.805**	
		(0.367)	
$>$ P50 Coffee $\text{Trees}_{\text{municipality}} \times <$ 18in 2010			0.452*
			(0.248)
Observations	4,428	4,428	4,428
R^2	0.25372	0.25382	0.25215
Within R^2	0.03863	0.03876	0.03661
Individual controls	✓	✓	✓
Community controls	✓	✓	✓
Municipality fixed effects	✓	✓	✓
Survey Months fixed effects	✓	✓	✓
District × Year Trend	✓	✓	✓

Notes: LPM estimations. Years of schooling are based on 2015 and 2020 DHS household record data for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2×2 km grid at the cluster'centroids. The independent variable is the interaction between the intensity of coffee production C and coffee prices P during school going years. Coffee production is measured as the number of coffee trees per municipality scaled by municipality size or above(1)/below(0) the median number of trees. Prices $\bar{p}_t^{12}$ are measured as the logarithm of international arabica prices during birth year's 12 years of schooling. The variable <18 years of age is a dummy variable indicating that the surveyed individual belongs to an age cohort that was still of school age during the boom in 2010/11. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

A1.10 Placebo tests: Maize and beans prices

Table A10: Placebo tests: The effect of maize and beans prices

	Completed Years of Schooling							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Coffee Trees at Municipality Level								
$> \text{P50}(\text{CoffeeTrees}_{\text{municipality}}) \times \log(\bar{p}_t^{\text{Maize, 144}})$	-1.44							
	(0.942)							
$> \text{P50}(\text{CoffeeTrees}_{\text{municipality}}) \times \log(\bar{p}_t^{\text{Beans, 144}})$		0.279						
		(0.903)						
$\text{CoffeeTrees}_{\text{municipality}} \times \log(\bar{p}_t^{\text{Maize, 144}})$			-0.072					
			(0.411)					
$\text{CoffeeTrees}_{\text{municipality}} \times \log(\bar{p}_t^{\text{Beans, 144}})$				0.451				
				(0.373)				
Panel B: Proximity to CWS (full approach)								
$\text{PrivCWS\_5km} \times \log(\bar{p}_t^{\text{Maize, 144}})$					-0.561			
					(0.962)			
$\text{PrivCWS\_5km} \times \log(\bar{p}_t^{\text{Beans, 144}})$						-0.167		
						(0.856)		
$\text{CoopCWS\_5km} \times \log(\bar{p}_t^{\text{Maize, 144}})$							-0.947	
							(1.10)	
$\text{CoopCWS\_5km} \times \log(\bar{p}_t^{\text{Beans, 144}})$								-0.034
								(1.34)
Observations	7,380	9,597	7,380	9,597	7,380	9,597	7,380	9,597
R^2	0.19585	0.20285	0.19564	0.20435	0.19573	0.20394	0.19607	0.20254
Within R^2	0.02713	0.03288	0.02687	0.03471	0.02698	0.03420	0.02740	0.03251
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓
Community controls	✓	✓	✓	✓	✓	✓	✓	✓
CWS controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Survey Months fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
District × Year Trend	✓	✓	✓	✓	✓	✓	✓	✓

Notes: LPM estimations. Years of schooling are based on 2015 and 2020 DHS household record data for the sample of individuals ages 18 or older. Individual controls control for respondents age, age of household head, years of education of the household head, urban status (1 if yes). Cluster controls include altitude (in meters) and road density in a 2x2km grid at the cluster'centroids. The independent variable is the interaction between the intensity of coffee production C and prices for maize and dry beans during school going years of individual i. Coffee production is measured as the number of coffee trees per municipality scaled by municipality size or above (1) / below (0) the median number of trees. Prices P are measured as the 12 year average logarithm of local producer prices for maize and beans taken from FAOSTAT (2024). Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$ Source: FAOSTAT, Author's calculations.

A1.11 Public spending on schools

Table A11: Public spending on schooling

	log(Number of Schools) (1)	log(Number of teachers) (2)
$> \text{P50}(\text{CoffeeTrees}_{\text{district}}) \times \log(\bar{p}_t^{12})$	-0.069 (0.263)	0.748 (1.636)
Observations	330	330
R^2	0.97483	0.92483
District fixed effects	✓	✓
Year fixed effects	✓	✓

Notes: Data provided by the Ministry of Education for a panel of all Rwandan districts from 2005 to 2020. Coffee region is measured as above (=1) or below (=0) median number of coffee trees per district scaled by district size. Prices $\bar{P}_t$ are measured as the average logarithm of international prices for arabica coffee during the year of observation. District and year fixed effects are included. In all estimations, standard errors are in parentheses and clustered at the district level. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

CHAPTER II

Genocide, Women's Empowerment and Intergenerational Transmission of Violent Attitudes

Abstract

In this paper, we explore how mass violence shapes attitudes on violence against children, and we examine how these attitudes are transmitted across generations in the context of the Rwandan genocide. Leveraging a natural experiment, we find that local genocide violence causes younger women from regions more affected by genocide to hold less violent attitudes compared to older women from the same regions, and to women in the same age cohorts from less genocide-affected regions. Using an instrumental variable approach to estimate the transmission effect, we also show that descendants of these younger women from genocide-affected regions are similarly less likely to adopt violent attitudes. We provide evidence that genocide-induced empowerment is the underlying mechanism. As such, our findings underscore previous evidence on the conflict–prosociality link and demonstrate persistent effects across generations, but call for a more detailed investigation of the underlying mechanisms for the second generation.[1]

Keywords: Women's Empowerment, Mass Violence, Attitudes, Intergenerational Transmission
JEL codes: J13, D74, D79

[1]This chapter is based on joint work with Dr. Alina Greiner-Filsinger (Postdoctoral Researcher, University of Mannheim). We are grateful to Jule Beck, Tilman Brück, Sabine Carey, Maximilian Filsinger, Anke Hoeffler, Kelly Kadera, Marie Kammer, Romaine Loubes, Marie Meye, Wolfgang Stojetz and participants at the European Political Science Association Annual Conference 2023, Jan Tinbergen European Peace Science Annual Conference 2023, AFK Empirical Peace and Conflict Research Workshop 2023 for helpful feedback and comments. We thank Dusan Ristic and Jacob Westerholt for their excellent research assistance.

2.1 Introduction

The protection of children from all forms of violence is a fundamental human right (United Nations, 1989) and a target of the UN's agenda for sustainable development. However, violence against children (VAC) is widespread (Hoeffler, 2017; Cuartas, 2021).[2] Prior research has shown that VAC causes harmful psychological and physiological responses (Baker-Henningham et al., 2009; Rockmore & Barrett, 2022; Doerr et al., 2023), which has far-reaching implications for economic outcomes, such as human capital formation (Guariso & Verpoorten, 2019; Brück et al., 2019; La Mattina, 2018), children's health (Akresh et al., 2012; Tapsoba, 2023), child work (Chin et al., 2023), or the formation of preferences (Cavatorta et al., 2023; Castillo, 2020; Jennings & Sanchez-Pages, 2017) and political attitudes (Hazlett, 2020; Blattman, 2009). Yet, factors that cause individuals to support VAC remain poorly understood.

In this paper, we explore the role of mass violence in shaping VAC attitudes and we examine how these attitudes are transmitted across generations. We focus on the 1994 Rwandan genocide, which created a unique natural experiment for studying these effects. Drastic changes in the aftermath of the genocide, both, on a micro- and a macro-level, led to an unprecedented process of women's mobilization and empowerment (Rogall & Zarate-Barrera, 2021). For example, as the majority of victims, perpetrators, and prisoners were men, areas most affected by violence saw dramatic shifts in sex ratios (La Mattina, 2017). As a result, women were pushed into new roles, both within the home and in society, giving them greater autonomy and influence.[3] Young women, in particular, experienced this phase of empowerment during their formative years, when core beliefs and attitudes are being shaped. They observed and internalized examples of women's empowerment, including their mothers and other role models, taking on new roles, challenging traditional gender norms. Previous research has shown that this genocide-induced women's empowerment led to higher public good provision, less domestic violence against women, and

[2] The most common form of VAC is corporal punishment: the use of physical violence to discipline (United Nations Children's Fund, 2014). While states are legally obliged to enshrine the prohibition of children's corporal punishment in domestic law, in many countries – including Rwanda – corporal punishment is lawful in the home.

[3] Additionally, the post-genocide government implemented laws to strengthen women's rights, such as securing land and inheritance rights, and ensuring that women occupy at least 30% of decision-making positions (Abbott et al., 2018).

set examples for more prosocial and less violent behaviors (Wolbrecht & Campbell, 2007; Rogall & Zarate-Barrera, 2021).

Building on these findings and insights from socialization theory, we propose and test a new channel linking mass violence to VAC attitudes. We test whether mass violence affects VAC attitudes and we examine how these attitudes are transmitted across generations through the channel of mother's exposure to genocide-induced empowerment conditions. Our population of interest are first- and second-generation individuals (mothers and their children) who live within the same household in post-genocide Rwanda. To empirically test our proposed mechanism, we first couple spatially fine-grained data on genocide intensity from the *gacaca* court records with three waves of the Demographic and Health Survey (DHS). Our identification strategy explores two sources of variation: spatial variation, i.e. mothers residing in communes with varying genocide intensity, and the variation in mother's year of birth to identify young women in their formative years at the time of the genocide. We measure post-genocide empowerment conditions by interacting the two variables and estimate the effect on mothers' VAC attitudes using a factorial difference-in-differences with a continuous treatment approach (Xu et al., 2024; Callaway et al., 2024). The intuition is that young women ($\leq$ 18 years of age at the time of the genocide) from high genocide regions were socialized under different conditions than older women from the same regions and, importantly, also than their same age cohort counterparts from less-genocide regions (i.e. young women from communes that are less affected by genocide violence). Second, we leverage this quasi-natural experiment as instrumental variable for mothers' VAC attitudes to estimate the transmission effect, i.e. the causal relationship between mothers VAC attitudes and those of their children, the second generation.

We focus on self-reported VAC attitudes by women and their children for three main reasons. First, we measure attitudes to mitigate concerns about measurement error. Disclosing attitudes toward violence is less stigmatized than reporting violent behaviors against children or personal experiences of violence. Prior research has shown that in Rwanda, reporting actual experiences of violence often involves substantial measurement error, primarily driven by social desirability bias (Cullen, 2022). Second, VAC attitudes represent one of the few measures of violence-related norms consistently collected in the DHS across all respondents, regardless of gender, age, or relationship status.[4] This is particularly valuable for obtaining a representative picture of the effect of genocide violence on VAC attitudes of all women across the country. Third, measuring VAC attitudes provides a unique opportunity to examine intergenerational dynamics, as both mothers and their children are asked identical questions in the DHS.

We find strong empirical support for our proposed channels. Women who were 18 or younger at the time of genocide and live in areas with greater levels of former genocide violence are significantly less likely to perceive VAC as acceptable compared to older women from the same regions, and to women in the same age cohorts from less genocide-affected regions. A one standard deviation increase in local genocide intensity decreases younger women's VAC attitudes by 7.7 percentage points. Furthermore, we show that due to the shift in their mothers' attitudes, children from these younger women in genocide affected regions are also significantly less likely to justify VAC. As such, we find strong support for an causal intergenerational transmission of non-violent attitudes. Our findings are robust across various instrument choices, such as alternative measurements of genocide intensity, and hold for different specifications of our violent attitudes variables. We further show that the effects remain stable in significance and size when we restrict our sample to children born after 1994 which supports our claim that the effect on children is not driven by direct exposure to the Rwandan genocide, but works through mother-level differences in genocide exposure. Additionally, the results are not driven by an arbitrary age cutoff of 18 years that splits our sample into younger and older women. Instead, our results remain robust across various plausible age cutoff specifications for identifying women in their formative years.

Finally, we present tentative evidence suggesting that the results are driven by our proposed mechanism of young women's empowerment. We find positive effects on several proxies for women's empowerment. Young women who live in former high genocide regions are significantly more likely to currently work in formal or informal employment. Importantly, these findings are not driven by selection effects, e.g., these young women could be less likely to live with a partner. Instead, we show that the results remain similar in size and magnitude for partnered and unpartnered women. In contrast to previous studies, e.g. La Mattina (2017), we provide

[4] Unlike the DHS domestic violence module which is only included for a subset of women, and an even smaller share of men. Questions about VAC attitudes are asked of all respondents in the DHS, independent of gender and relationship status.

evidence that younger women in high-genocide regions are less likely to be a victim of physical domestic violence. This suggests that these women might choose partners who hold similar attitudes opposing intra-family violence, including VAC. We test this by examining the subsample of couples (where both wife and husband data are available) and find evidence that the partners of younger women from high genocide regions are indeed less likely to accept VAC.

This study brings value to several strands of the literature. First, we add to empirical contributions that show how attitude formation of children is influenced by external factors (Cavatorta et al., 2023) and in large part shaped by the attitudes of parents (Gay, 2023; Dohmen et al., 2012; Alan et al., 2017; Black et al., 2005). For instance, Dohmen et al. (2012) show that the transmission of risk and trust preferences from parents to children follows a pro-cyclical pattern and that socialization is an important driver in the transmission process. Similar to these studies, we find a pro-cyclical pattern of VAC attitudes between first and second generation victims of mass violence, which has not been studied by the literature. Second, we contribute to the literature that investigates persistent effects of historical shocks for subsequent generations (Gay, 2023; Lupu & Peisakhin, 2017; Rozenas et al., 2017; Caruso, 2017; Charnysh & Peisakhin, 2022) and the mass violence–domestic violence link (Stojetz & Brück, 2023; La Mattina, 2017; Rutayisire & Richters, 2014; Saile et al., 2014; Cesur & Sabia, 2016; Østby et al., 2019). Similar to Campante & Yanagizawa-Drott (2015), we demonstrate the need to investigate the transmission of violent attitudes in post-war contexts on children. While it has been established that children who grow up in violent homes likely become perpetrators or victims of domestic violence as adults (Ireland & Smith, 2009), there is a lack of studies investigating mitigating factors that can help end this violent cycle. Our findings help to explain why exposure to violence is associated with increasing prosocial behavior and attitudes. As such, our study also adds to the ongoing debate to what extent prosocial norms can emerge after conflict (Blattman, 2009; Blattman & Annan, 2010; Bauer et al., 2016; Cecchi & Duchoslav, 2018; Mironova & Whitt, 2016; Erten & Keskin, 2020; Bulte & Horan, 2011; Dhar et al., 2019). Third, we add value to the growing women's empowerment literature (Bochenkova et al., 2023; Harris & van der Windt, 2023) by studying the cascading effects of empowerment for the second generation. In contrast to previous studies, we look beyond immediate benefits for children's health or education (Annan et al., 2021; Duflo, 2012). Instead, we investigate how empowerment is linked to the formation of attitudes beneficial for subsequent generations, and analyze the causal relationship between attitudes on VAC of women and their children. As such we are most closely related to the study by Rogall & Zarate-Barrera (2021) and demonstrate that witnessing women's empowerment leaves persistent effects for younger women *and* their descendants. This also sets us apart from socialization studies e.g. Dinas (2014), which use pre-post comparisons, treating all individuals socialized before an event as the control group and those after as the treatment group. In contrast, we adopt a difference-in-differences approach in that we identify individuals as treated only if they meet two conditions: (i) they live in areas with high levels of violence exposure and (ii) they were socialized during their formative years. This aligns closely with the approach by Rogall & Zarate-Barrera (2021) and allows us to capture more nuanced dynamics and account for potential treatment heterogeneity.

Lastly, we aim to further contribute to understanding the lasting consequences of the Rwandan genocide (Bonnier et al., 2020; Rogall & Zarate-Barrera, 2021; Rogall, 2021; Yanagizawa-Drott, 2014; Guariso & Verpoorten, 2019). While persistent effects for child outcomes have been studied, most focus on education (Bundervoet & Fransen, 2018; Chin et al., 2023; Guariso & Verpoorten, 2019; La Mattina, 2018). Our research offers insights into how the second generation's attitudes evolve after violent conflict, suggesting that women's empowerment may be an important mechanism driving anti-violent (or pro-social) attitude formation of the second generation. Thus, we showcase an alternative mechanism through which mass violence can strengthen pro-social norms, namely through the formation of emancipative values among younger women with important and positive implications for their children and possibly subsequent generations.

The rest of this paper is organized as follows: Section 2 introduces a brief theoretical discussion of how the genocide leads to empowerment that then impacts the second generation. Section 3 provides background information on the history of the Rwandan genocide. Section 4 and 5 describe the data and empirical strategy respectively before in section 6, the main results and key mechanism behind our results are presented. Section 7 discusses the results and concludes.

2.2 The lasting impact of violent conflict across generations

How does violent conflict shape attitudes on violence across generations? We synthesize arguments from several literatures to shed light on the persistent effects of mass violence. Our objective is to add to the debate how and when violent cycles can not only be broken, but potentially transformed into a virtuous cycle. We argue that women who were exposed to greater levels of mass violence when they were 18 or younger (i.e., in their formative years when core beliefs and attitudes develop) are more likely to condemn violence within their families.[5] Our argument builds on two core assumptions. The first is that areas with higher levels of mass violence were marked by greater levels of women's mobilization and participation (Rogall & Zarate-Barrera, 2021). The second is that witnessing violence-induced women's empowerment acts as role model mechanism for younger women who are more likely to internalize these changes and form attitudes opposing violence. As such, we expect greater women's empowerment socialization effects in areas that were more affected by genocide violence. Furthermore, we argue that the positive, empowering effect on younger women exposed to higher levels of mass violence has cascading effects on their children. Empowered women who are socialized into condemning violence transmit these attitudes to their children. In sum, we propose that mass violence indirectly reduces the second generation's proclivity for violence through their mothers' socialization into condemning violence.[6]

2.2.1 Mass violence and women's empowerment

The first part of our argument is that higher levels of mass violence lead to women's empowerment, which leads to role model effects for younger women. Recent research has illustrated how mass violence can lead to processes that are beneficial to women's rights and status (see e.g., Rogall & Zarate-Barrera, 2021; Berry, 2015; Hughes & Tripp, 2015; Tripp, 2015; Bakken & Buhaug, 2021; Webster et al., 2019). While this idea might seem counterintuitive at first, the underlying logic is straightforward. Mass violence is inherently gendered with the majority of perpetrators, victims, and prisoners being men (Goldstein, 2001). Women fill the vacuum resulting

[5]There exists no clear consensus in the literature how to define the beginning and end of the *formative-years*, *coming-of-age* or *socialization* period. However, the period of adolescence is widely recognized as formative stage of human development (see discussion in Dinas, 2014). The World Health Organization (2025) defines adolescence as "the phase of life between childhood and adulthood, from ages 10 to 19". We define adolescents as 18 or younger (i.e., born in 1976 or later) for the purpose of our study, but use different year-of-birth thresholds in a sensitivity analysis.

[6]We want to emphasize that our argument does not rest on the assumption that genocide or any form of mass violence is beneficial for women, or for subsequent generations, but that women's outward and inward response to mass violence can lead to positive cascading effects. We also acknowledge that not all forms of violence create conditions for women's empowerment. Especially conflicts that involve gender-based violence and one-sided violence – both characteristics of the Rwandan genocide – are thought to lead to more women's mobilization and in turn more women's empowerment (Savun et al., 2024).

from these demographic imbalances by taking on new roles and responsibilities. This leads to a disruption of traditional gender hierarchies, as women gain more economic and political power.

In addition to entering new spaces that have opened up due to men's absence, women also act against collective gendered threats posed by mass violence (Kreft, 2019; Berry, 2015; Savun et al., 2024). Through their participation in grassroots movements and campaigns, women bring unprecedented visibility to issues of gender inequality and gender-based violence (Berry, 2015). Only a small proportion of women activists and leaders end up in positions of political power post-conflict, yet those who successfully make it to parliaments or governments act as a powerful symbol of empowerment for girls and women who witness their achievements (Arvate et al., 2021; Campbell & Wolbrecht, 2006; Wolbrecht & Campbell, 2007). In line with our argument, the role model effect has been found to be especially pronounced for adolescents and lessens with age.[7] Even when women's mobilization does not translate into political representation or personal gain, witnessing women organize around issues of gender inequality and act as agents of change – likely for the first time in their lives – can be highly influential during adolescents' formative years.[8]

Political events experienced during formative years can serve as exogenous catalysts that give issues unprecedented visibility and provide young individuals with extensive information required for attitude formation, hence "nonattitudes" become "real attitudes" (Sears & Valentino, 1997). Crucially, this holds true only for attitude objects that are rendered salient by the respective political event (Sears & Valentino, 1997).[9] In the context of the Rwandan genocide, attitudes on gender and violence gained unprecedented salience. As such, we expect women socialized in an era characterized by violence-induced women's empowerment to form attitudes rejecting violence as a means to demonstrate authority and enforce discipline.[10]

[7] For instance, Wolbrecht & Campbell (2007) show that the political activity of younger women is impacted to a significantly greater degree by the presence of female members of parliament than that of older women.

[8] Experiencing new social or political situations earlier in life has the power to mold attitudes that are in younger age still in development, whereas the same event would be less impactful when experienced later in life when stable attitudes have already been formed (Mannheim, 1952, 296).

[9] Although existing research focuses on socialization induced by non-violent political events, we expect to observe similar socialization patterns for adolescents who come of age during a historical era characterized by violence-induced women's empowerment.

[10] We do not dispute that exposure to mass violence can lead to a normalization of violence, with violence shifting from the theater of war into homes. However, we expect that under the conditions that we investigate, a post-war culture of violence can be counteracted by witnessing women's empowerment and the subsequent formation of non-violent (or pro-social) attitudes.

Therefore, we expect younger women who witnessed women's empowerment to be less likely to accept VAC. Patriarchal norms justify the use of violence, such as the physical discipline of children. Patriarchal systems are characterized by a clear hierarchy, with men at the top and women and children beneath them. If subordinates (women or children) 'misbehave', someone higher in the hierarchy (men or parents) must correct their behavior by means of violence (Namy et al., 2017). As such, VAC is rooted in norms of gender inequality (Namy et al., 2017). Consequently, we argue that women who grew up watching older women challenge a patriarchal system are socialized into more progressive gender attitudes and are thus more likely to condemn violence within their families. This is supported by evidence that empowered women report less domestic violence and corporal punishment which might set off a virtuous cycle (Rogall & Zarate-Barrera, 2021).

2.2.2 Intergenerational transmission of attitudes on violence

In line with the virtuous cycle hypothesis, we expect that the positive, empowering effect on younger women exposed to higher levels of mass violence has cascading effects for their children. As mothers, this generation of women socialized into rejecting violence transmit these attitudes to their children. Our argument here is twofold: on the one hand, women who are less accepting of violence want their children to adopt attitudes identical to theirs and teach this core belief that violence is not acceptable to enforce discipline to their children. On the other hand, following from positive assortative mating, women choose like-minded partners with similar preferences (Dohmen et al., 2012), i.e., who too are more likely to condemn violence within their families. In sum, we expect second generation individuals to be less accepting of VAC if their mothers were in their formative years and exposed to higher levels of violence during the genocide.

Women's empowerment has been linked to immediate positive outcomes for (young) children, e.g., health or education (Annan et al., 2021; Duflo, 2012). In contrast to previous studies, we study the causal effect of mothers' attitudes on self-reported attitudes by their children and investigate if the second generation benefits from genocide-induced attitude formation of their mothers. The intra-family transmission of attitudes and preferences from parent to child is well established in the literature, to which we add significantly with our focus on VAC attitudes. Investigating VAC attitudes in the context of intergenerational transmission is particularly relevant. Children who grow up in families that tolerate violence are at risk to adopt the same beliefs, and to become violent or end up with violent partners as adults (Ireland & Smith, 2009). Empowered women can break this violent cycle, with important implications for future generations. If the second generation is more likely to condemn VAC due to their mothers' more progressive VAC attitudes, this might give us an indication of how they will fare as parents. Following the intergenerational transmission argument, the second generation would be less likely to teach their children (i.e., the third generation) that the use of violence is a normal or adequate response.

2.3 The Rwandan genocide

The origin of the 1994 Genocide in Rwanda against the Tutsi is complex and rooted in decades of colonial history and discrimination.[11] The commonly identified trigger event was the shooting down and fatal crash of the plane of president Habyarimana on April 6, 1994. Within hours, members of the military, the administration, and the government-sponsored militia group as well as civilians began to kill Tutsi, but also moderate Hutu and Hutu leaders of the opposition party. While conclusive figures of the death toll among Hutu are not available, many Hutu were killed trying to protect Tutsi, suspected of helping Tutsi, or mistaken for Tutsi (Straus, 2019). The 1994 genocide lasted for 100 days between April–July and around 1 million people were killed, the majority of them men (Republic of Rwanda, 2002).[12]

Approximately one million perpetrators are estimated to have participated in the genocide which corresponds to around 14 % of Rwanda's total population (based on the 1991 census

[11] It would be impossible to do the complex history justice within the scope of this paper. Comprehensive and detailed summaries can be found in e.g., Heldring (2021); Hintjens (1999); Bonnier et al. (2020); Straus (2019); Uvin (1997); Verwimp (2006); Des Forges (1999); Prunier (2014); Straus (2006). Important to note is that an "ethnic conscience" did not exist in the early pre-colonial period in Rwanda, but started to emerge in the 18th century when the heads of the newly established permanent army identified themselves as Tutsi who were to have greater privileges than clan leaders (Ntezimana, 1987; Republic of Rwanda, 2002). As such, the ethnic identities Hutu and Tutsi predate the colonial rule, but the strict distinction between and the salience of belonging to either of the two groups was a product of discriminatory colonial policies which stipulated that the Tutsi were to be the sole group with power to administer.

[12] However, it is worth noting that the Rwandan government speaks about genocide and massacres between October 1990 and December 1994 (Republic of Rwanda, 2002). Verpoorten (2005) identifies the first attacks of the (predominantly Tutsi) rebel group Rwandan Patriotic Front (RPF) in October 1990 as starting point for civil unrest in Rwanda. The RPF's objective was to remove president Habyarimana from power and enable the return of hundred of thousands exiled Rwandans. Increasingly threatened by the RPF's success, and faced with decreasing levels of popular support, Habyarimana and his allies launched a campaign designed to incite hatred and fear of the Tutsi, portraying them as collaborators of the RPF. As such, the genocide was not an isolated sudden outbreak of violence, but intertwined with the ongoing civil war between the Rwandan government and the RPF (Des Forges, 1999).

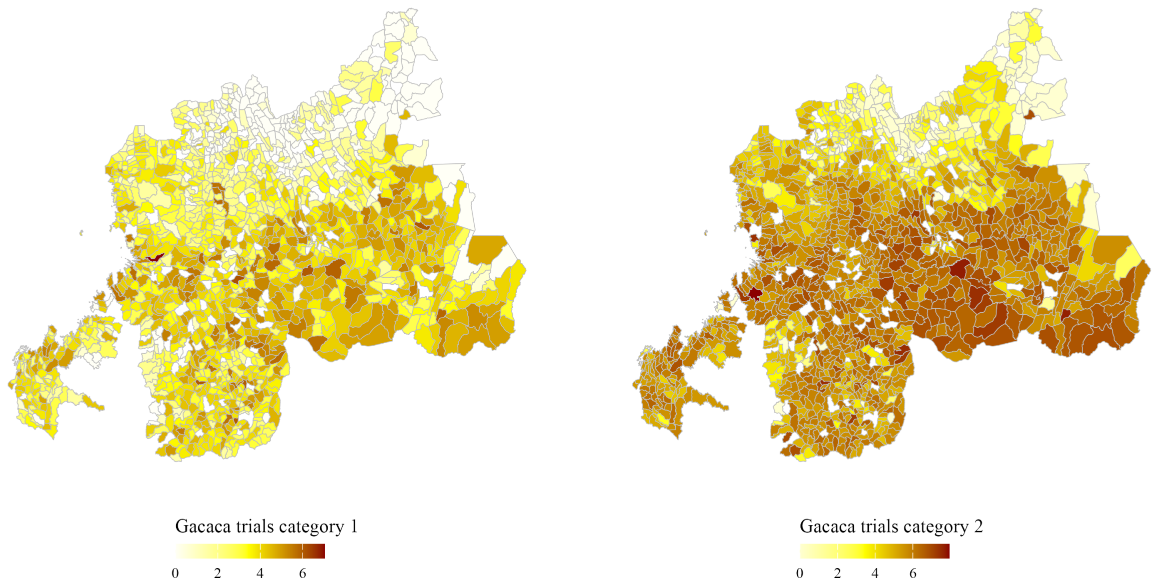

Fig 2.3.1: Distribution of *gacaca* trials across Rwanda. The map displays the number of category 1 (left) and category 2 (right) gacaca trials per sector, scaled by sector population in 1991.

population count, Nyseth Brehm et al., 2014).[13] As the Rwandan justice system was overwhelmed with bringing the massive number of perpetrators to justice, the government tasked local *gacaca* community courts with conducting trials for perpetrators of genocide and crimes against humanity across the country. Perpetrators were classified into three categories: *category 1* if they planned or organized the genocide or committed rape and sexual torture, *category 2* if they committed or were accomplices to torture, murder or desecration of corpses, and *category 3* if they committed property crimes or looting (Nyseth Brehm et al., 2014).[14][15] The intensity and frequency of genocide events varied across the country (see Fig. 2.3.1). According to perpetrator information in the *gacaca* court records, the median age of participants was 34 and around 88 % of all participants (95 % for violent offenses) were men (Nyseth Nzitatira et al., 2023).

Men made up the majority of victims, perpetrators, and as such the imprisoned population. As a consequence, the genocide drastically altered the sex ratio in Rwanda, particularly in areas with high genocide violence. (Rutayisire & Richters, 2014; Eichelsheim et al., 2019; La Mattina,

[13]Estimates on participation vary. The official numbers released by the Rwandan government range between 1–2 million participants.

[14]See Appendix A.10 for detailed information on the different categories

[15]Between 847,233 and 888,307 individuals were found guilty of genocide crimes across all three *gacaca* categories (Nyseth Nzitatira et al., 2023). The *gacaca* courts and records are described in more detail in the next section. A detailed description of the categories provided in Nyseth Brehm et al. (2014) can be found in the appendix.

2017). This, in turn, had strong impacts on gender roles and relations. Historically, Rwandan women faced widespread discrimination and had limited opportunities. They were barred from owning land, received less education compared to men, and were often confined to low-paying jobs, leaving them largely dependent on male authority figures (Rogall & Zarate-Barrera, 2021; Kraehnert et al., 2019). However, the 1994 genocide marked a turning point. With men making up the majority of the victims and perpetrators, women stepped up as heads of families, community leaders, and active participants in political life. Additionally, the post-genocide government implemented laws to strengthen women's rights, such as securing land and inheritance rights, and ensuring that women occupy at least 30% of decision-making positions (Abbott et al., 2018). By the time of the first post-genocide election in 2003, women secured 48% of the seats in parliament. Young women, in particular, experienced this phase of transition during their formative years, when core beliefs and attitudes are being shaped. They observed and internalized examples of women, including their mothers and other role models, taking on new roles and challenging traditional norms (Wolbrecht & Campbell, 2007; Rogall & Zarate-Barrera, 2021). Important to note is that changes in sex ratios, and as such the subsequent empowerment mechanisms (as outlined in the previous chapter), also varied locally with genocide intensity (Rogall & Zarate-Barrera, 2021). As such, this context allows us to use the variation in both spatial genocide intensity and temporal variation in socialization of first generation women to test the transmission of genocide violence. The Rwandan genocide is an ideal case to investigate our research question based on the following factors that we can exploit to empirically test our arguments: i) The strongly gendered participation in genocide, ii) the locally varying intensity of genocide violence, iii) which together led to an unprecedented process of women's mobilization and participation, iv) with temporarily varying implications for women's attitude formation, and v) the availability of fine-grained individual and spatial data to capture all this.

2.4 Data

We obtain data from several sources - the 2010/2011, 2014/2015 and 2019/2020 Demographic and Health Surveys (DHS), the records of the Rwandan *gacaca* courts and the 1991 Rwandan census.

2.4.1 Demographic and Health Surveys

Our population of interest are first- and second-generation individuals (mothers and their children) who live within the same household in post-genocide Rwanda. We obtain geo-coded survey data from three waves of Demographic and Health Surveys (DHS) fielded in Rwanda in 2010/2011, 2014/2015, and 2019/2020 (DHS Rwanda, 2012, 2016, 2021). The DHS are nationally representative, cross-sectional household surveys that are conducted every few years in low- and middle-income countries.[16] The DHS offers several key advantages for our analysis: First, the survey data are geo-coded, allowing us to spatially match them to fine-grained information about local genocide intensity.

Second, the DHS interviews all eligible household members which enables us to pursue a two-generational design and allows us to examine identical survey questions across generations.[17] Third, this feature of the DHS also reduces measurement error studying the transmission of attitudes within households, as all family members are interviewed by the same interviewer. As such, the richness of the DHS survey data combined with its multi-generational structure allows us to measure our outcome of interest – attitudes on VAC – for mothers and their young adult children within households. To the best of our knowledge, we are the first to construct such a dataset where each child-level entry is linked to their mother's information to measure within-family-transmission of attitudes.

We measure attitudes on VAC with the following question: "In your opinion, is a parent justified in hitting or beating his/her children for the following reasons: i) if he/she disobeys, ii) if he/she is impolite, iii) if he/she embarrassed the family?". Our main outcome variable VAC_any takes the value of 1 if the respondent agrees to any of the three statements, and 0 otherwise. Our alternative outcome variable VAC_sum is the sum score of affirmative items, standardized to

[16] In Rwanda, the DHS were implemented by the National Institute of Statistics of Rwanda (NISR). The DHS program is funded by the U.S. Agency for International Development.

[17] The target group of the DHS are primarily women, but also men of reproductive age (defined as 15–49 for women, and 15–59 for men). Eligible household members are regular residents who fall within said age group. The DHS surveys consist of three questionnaires: the household questionnaire, the women's questionnaire, and the men's questionnaire. The household questionnaire includes basic demographic information about all household members (including age, sex, marital status, education, and relationship to the head of the household) and household characteristics. In all sampled households, all eligible women are interviewed using the women's questionnaire. In half of the households, all eligible men are interviewed using the men's questionnaire. In addition, a sub-sample is interviewed for the domestic violence module. In 2010, 37% of women and 0 % of men; in 2014, 39 % of women and 34 % of men; and in 2019, 19 % of women and 33 % of men were selected and interviewed for the domestic violence module.

mean zero and standard deviation one.[18] In Table 2.4.1 we present summary statistics for our main variables. We restrict our sample to children (respondents who identify as daughter or son of the household head) and pair them with their mothers' (partnered women who identify as household head or wife of the household head) data entries to create matched child-mother pairs.[19] We restrict our sample to mothers who are currently in a partnership. We do so to ensure that the family composition is constant across our sample.[20] Given that it is impossible to link children's DHS data to that of their mothers if they do not live within the same household, our data might not perfectly reflect the universe of child-mother-pairs in Rwanda. Instead, we specifically focus on those cases where mothers and children live in the same household. Still, we argue that our sample represents our population of interest (i.e., children and young adults of the second generation) very well. By looking into the full population of respondents in the age group interviewed by the DHS, we show that the overwhelming majority live in a joint household with their parent, which suggests that our sample closely mirrors the broader population. Please see Fig. B.1 in the appendix for more details. Therefore, we argue that the benefits of our approach outweigh the costs: while we lose the small share of interviewed children who do not live in the same household as their parents, we gain the ability to draw inferences about intra-family transmission and can strengthen the precision of our results through the inclusion of child-level respectively mother-level controls.

[18] Although this is a standard question in both the women's and the men's questionnaire, the items on whether child-beating is justified were unfortunately not asked in the 2019 men's questionnaire. For that reason and because of the DHS strategy to oversample women, daughters are overrepresented in our analyses

[19] More precisely, we use the DHS individual recode to identify female respondents, and the DHS men's recode to identify male respondents. We then restrict these samples to respondents who identify as daughters respectively sons of the household head and join them together to construct our children data set. To construct the sample of mothers, we again use the DHS individual recode to identify female respondents, but this time restrict the sample to respondents who identify as either the wife of the household head or the household head and who state that they are currently partnered/married. We then match our children sample with our mothers sample based on household ID and survey wave. We only retain matched observations, i.e., we drop observations of children for whom there is no corresponding mother's data within the same household.

[20] Our reasoning here includes several aspects. First, the majority of mothers are partnered, i.e., our selection strategy reflects the most common form of family composition. Second, as part of our additional analyses we investigate a subset of our sample of mother-child-pairs where the father has also been interviewed to account for the impact of father's VAC attitudes. If we were to sample unpartnered women in our main analysis, this comparison would not be meaningful. Third, women who leave relationships and women who are widowed might differ in important aspects from partnered women. The former typically have more resources and outside options to be able to leave a relationship, but also often strong reasons to leave a relationship such as abuse and other forms of intra-family violence. The latter often have fewer resources, and might be widowed as a result of the genocide. These specific conditions might correlate with genocide intensity and age, and might thus bias our estimations. Nevertheless, in a robustness test, we also include mothers who do not currently live in partnership to show that our results are not driven by sample selection.

Table 2.4.1: Summary Statistics: Household Variables from the 2010, 2015, and 2020 DHS

Variable	Mean	SD	Min	Max	N
Panel A: Child Variables from the DHS (Women's and Men's recode)					
Male	0.23	0.42	0	1	4967
Year of Birth	1996	4.82	1978	2005	4967
Years of Education	6.13	2.82	0	16	4966
Literate	0.86	0.35	0	1	4957
Child asserts child beating is justified in at least 1 situation	0.78	0.42	0	1	4967
Number of items child asserts child beating is justified	2.06	1.25	0	3	4967
Child asserts wife beating is justified in at least 1 situation	0.43	0.50	0	1	4959
Number of items child asserts wife beating is justified	1.17	1.67	0	5	4959
Panel B: Mother Variables from the DHS (Women's recode)					
Year of Birth	1971	5.85	1960	1990	5834
Young Mother	0.27	0.45	0	1	5834
Years of Education	4.09	3.54	0	20	5834
Literate	0.59	0.49	0	1	5834
Wealth Index	3.28	1.31	1	5	5834
Woman asserts child beating is justified in at least 1 situation	0.68	0.47	0	1	5834
Number of items women asserts child beating is justified	1.76	1.35	0	3	5834
Physical violence by partner in the last 12 months	0.47	0.50	0	1	1215
Number of items women asserts wife beating is justified	1.39	2.01	0	9	1215
Woman asserts wife beating is justified in at least 1 situation	0.49	0.50	0	1	4966
Women is currently working	0.07	0.25	0	1	4965
Women receives cash income	0.22	0.42	0	1	4541
Any decision	0.52	0.50	0	1	3582
Visits	0.21	0.41	0	1	4932
Purchases	0.14	0.34	0	1	4930
Healthcare	0.27	0.45	0	1	4932
Earnings	0.25	0.43	0	1	2997
Panel C: Father Variables from the DHS (Men's recode)					
Year of Birth	1966	7.87	1946	1990	4036
Years of Education	4.28	3.54	0	20	4036
Father asserts child beating is justified in at least 1 situation	0.59	0.49	0	1	1889
Number of items father asserts child beating is justified	1.51	1.37	0	3	1889

2.4.2 Gacaca records

We couple the individual-level survey data with fine-grained information on local genocide intensity. These violence measures are spatially merged with the DHS files. As direct measures of individual genocide exposure are not available, we proxy for genocide intensity by the prosecution rates for crimes committed during the genocide as in previous studies (Yanagizawa-Drott, 2014; Bonnier et al., 2020; Rogall, 2021; Friedman, 2013). The data come from a nationwide dataset provided by the "National Service of Gacaca Jurisdiction" by the Rwandan government that encompasses the locations of nearly 12,000 local *gacaca* courts established across the country to prosecute individuals accused of perpetrating genocidal violence. *Gacaca* refers to a traditional Rwandan dispute resolution mechanism, in which village elders and community members gather to discuss local disputes.[21] In response to the vast number of prisoners whose cases remained untried in the aftermath of the genocide, the Rwandan government officially launched the *gacaca* court system in 2002 to try and judge those who wish to confess or have been accused of participating in the genocide (Corey & Joireman, 2004). The courts were active until 2012, during which a total of 1.6 million *gacaca* trials were held in 12,000 courts across the country (Nyseth Nzitatira et al., 2023).[22]

Importantly, the courts operated based on the location of the alleged crimes, allowing prosecutions to take place even in the absence of the accused individuals (Bonnier et al., 2020; Nyseth Nzitatira et al., 2023). As in previous studies, we aggregate the *gacaca* data at the 1991 commune level (n=144) for two main reasons.[23] First, since we merge this data with data from the DHS, in which observations are placed in specific spatial clusters and coordinates that are randomly shifted by 2 to 5 km (depending on whether the area is urban or rural) to protect respondent confidentiality, finer spatial aggregation would introduce measurement error. Second, the commune-level is the smallest administrative unit for which genocide violence can still be accurately identified (Straus, 2006) and category 1 and 2 offenders were tried in courts operational at the sector level (Nyseth Brehm et al., 2014) Finally, as of 1990, residents required government permission to move between communes, making the commune level a stable and suitable unit of analysis (Nyseth Nzitatira et al., 2024).

We proxy for genocide intensity with the number of category 1 and category 2 *gacaca* trials per commune (relative to the respective commune's population). We focus on these categories for two main reasons: first, both encompass violent genocide crimes and are thus more comparable, whereas property destruction and looting (i.e., category 3 crimes) might be seen as a distinct type of offense in our setting. Second, the *gacaca* trials data were found to measure category 3 crimes with severe measurement error, whereas they give an accurate representation of category 1 and

[21] The word itself translates to "justice on the grass" in Kinyarwanda, symbolizing the open-air spaces where the courts were held. See Corey & Joireman (2004) for a detailed description.

[22] Note, however, that not every genocidal crime was tried before *gacaca* courts. Those individuals deemed primarily responsible for the genocide were tried before the International Criminal Tribunal for Rwanda, for instance. Nevertheless, *gacaca* courts constitute the best source of available data on civilians who committed genocidal violence (Nyseth Nzitatira et al., 2023).

[23] Before 2002, Rwanda was organized into 1,484 sectors, 145 communes, and 11 prefectures. In 2002, a reform changed the communes into 104 districts, and the prefectures became 12 provinces. In 2006, the country was reorganized again into 30 districts and 5 regions. Today, Rwanda is divided into the following administrative units: provinces (4 + city of Kigali), districts (30), sectors (416), cells (2,148), and villages (14,837). In 1991, each commune had an average population of 51,235, and the average size of the area was 153.8 square meters (La Mattina, 2017).

2 guilty verdicts (Nyseth Nzitatira et al., 2024). Nyseth Nzitatira et al. (2023) were able to obtain information on i) the outcome of each verdict, i.e., whether defendants were found guilty or not guilty in the *gacaca* courts, and ii) individual characteristics of the defendants, allowing them to account for repeat participants and duplicate entries. As such, their *gacaca* verdicts data are a more precise approximation of the number of people who stood trial and were found guilty, whereas our data rather reflects the number of trials. However, the verdicts data is only available on the district-level and thus lack in spacial precision. In Fig. B.3 we also show that trials and verdicts are highly correlated. Due to their much higher spatial precision, we rely on the sector-level trials data for category 1 and category 2 crimes and aggregate this data on the 1991 commune level in our main models. [24]

Lastly, for our robustness checks we use two alternative measures of genocide intensity. First, we proxy for genocide exposure with the number of days during which a commune was under RPF (Rwandan Patriotic Front) control. The RPF, primarily composed of Ugandan-based Tutsi living in exile, launched an infiltration into Rwanda from the northeast boarder to Uganda in April 1994 with the aim of putting an end to the massacres. As they advanced from the northeast, their forces swiftly captured territory, saving countless Tutsis and moderate Hutus from ongoing massacres and finally toppling the Hutu government in July. Their infiltration resulted in a pattern where the genocide lasted longer the farther one got from the Ugandan border. The number of days a commune had been under RPF control correlates with a reduction in genocide violence, as their presence helped to limit further massacres. We use this variation in genocide intensity as robustness check to estimate our effect of interest. We measure the exact number of days that a commune was under RPF control between the start and the end of the 1994 RPF advancement (a 109 day period from April, 1, 1994 to July, 19, 1994).[25] However, we would like to note this measure is not without limitations. While the RPF saved the lives of numerous Tutsis and moderate Hutus as they advanced toward the south and west, they also killed thousands of militia members and Hutu civilians, especially women and children (Corey & Joireman, 2004; Des Forges, 1999). These

[24] In contrast to Rogall & Zarate-Barrera (2021), we do not differentiate between local and external genocide violence and might therefore pick up a weighted average of the two (similar to La Mattina (2017)). We do not see this as an issue for our proposed channel, but rather understand our approach as more conservative in that it makes it harder to pick up a positive effect by considering both dimensions of genocide violence.

[25] See Verpoorten (2012) for a detailed description. While some communes in the North close to the Ugandan border were under control between 79 and 109 days, other sectors that only came under control by the RPF in July, were only under RPF control between 1 and 19 days.

Table 2.4.2: Summary Statistics: Genocide and Other Commune-Level Variables

Variable	Mean	SD	Min	Max	N
Panel A: Data from records of the gacaca courts					
Trials Category 1	3.10	1.19	0.15	5.07	144
Trials Category 2	4.94	1.34	0.62	6.84	144
Distance to mass grave (in km)	11.38	8.87	1.97	56.62	144
Days under RPF control	40.93	41.11	0.00	109.00	144
Panel B: Data from 1991 Census of the Rwandan population					
Literacy Rate Male Population	0.43	0.05	0.31	0.65	144
Literacy Rate Female Population	0.34	0.07	0.19	0.56	144
Labor Force Participation Male Population	0.44	0.04	0.29	0.51	144
Labor Force Participation Female Population	0.46	0.03	0.28	0.52	144

Notes: All variables are aggregated at the commune level. Trials category 1 and 2 are scaled by commune population size in 1991.

killings were seen as retaliation for the ongoing genocide taking place elsewhere in the country and could thus be also considered as different measure of violence (Rogall, 2021). Second, we use data on the location of mass graves, based on satellite maps from the Yale Genocide Studies Program to measure the intensity of genocide violence. This data is retrieved from Verpoorten (2012) and aims to measure the proximity to a large-scale massacre. We calculate the distance between the location of the DHS cluster to the nearest location of the 71 mass graves in Rwanda, aggregate this variable at the commune level and scale it to mean 0 and standard deviation of 1.

2.4.3 Rwandan Census 1991

Finally, to assess local pre-genocide conditions we use the second population census that was conducted in August 1991 by the National Institute of Statistics in Rwanda. The census data are available at the commune level in pre-2002 borders. We obtain commune level population data for 1991 as well as literacy and labor force participation rates separately for the female and male population to account for pre-genocide conditions.

2.5 Identification strategy

This paper examines the intergenerational transmission of violent attitudes from mothers to their descendants. Our identification strategy proceeds in several steps: First, we estimate the causal effect of local genocide violence on mothers' VAC attitudes. We then exploit this exogenous variation in maternal attitudes as an instrumental variable to estimate the transmission effect, i.e. the effect of maternal attitudes on those of the second generation.

The identification strategy relies on the unique historical context following the Rwandan genocide. Due to the gendered nature of the genocide, where men comprised the majority of victims, perpetrators, and prisoners, the sex ratio in areas with high levels of violence was significantly altered, potentially leading to our suggested mechanism of young women's empowerment in regions with high genocide intensity. By leveraging the interaction between geographic variation in genocide intensity across 144 communes and women's year of birth, we construct an instrumental variable for mothers' violent attitudes. This enables us, in a second step, to causally identify the effect of mothers' VAC attitudes on those of the second generation.

2.5.1 First stage: Genocide violence and women's violent attitudes

We start by estimating the causal effect of local genocide violence on mother's attitudes toward VAC. To do so, we estimate a factorial difference-in-differences design (Xu et al., 2024) in which we exploit two sources of variation: spatial (women living in communes with varying degree of genocide intensity) and temporal (women's year of birth). The idea is that young women ($\leq$18 years of age at the time of the genocide) from high genocide regions were socialized under different conditions than older women from the same regions and, importantly, also than their same age cohort counterparts from less-genocide regions (i.e. young women from communes less affected by genocide violence). Fig. 2.5.1 displays the main idea behind our difference-in-differences estimator. We observe a common downward trend in attitudes toward VAC over time for both high-genocide and less-genocide communes. However, for the cohorts that were 18 years of age or younger at the time of the genocide in 1994, we observe a strong divergence in attitudes across these communes for both of our outcome measures, the binary (VAC_any) as well as the scale (VAC_sum) variable. Younger women from above median level genocide communes tend to have less violent attitudes than their counterparts from below median violent communes and this is not due to the fact that genocide regions per se differ in violent attitudes. This idea can be generalized to a regression framework that exploits greater variation across communes and birth years employing the following baseline specification that is estimated by OLS:

$$\begin{aligned} VACmother_{icgt} = \alpha + \beta(Genocide_c \times Young\_mother_g) + \gamma_c + \sigma_g + \tau_t \\ + \delta X_{it} + \delta X_{ct} + \kappa \mathbf{T}_{ct} + \xi_{icgt} \end{aligned} \tag{2.10}$$

Here, the dependent variable *VACmother* indicates whether mother of child i, of birth year g, living in commune c, surveyed at the time of survey year t, perceives VAC as justified. The main variable of interest, our instrument, is given by the interaction between the time-invariant measure of genocide intensity $Genocide_c$ in commune c and a binary indicator $Young\_mother_g$ that takes the value of one if the mother is born after 1976. This condition ensures that these women were 18 years of age or younger at the end of the genocide in August 1994. For ease of interpretation, all violence measures at the commune level are normalized with mean zero and standard deviation equal to one. β_1 captures the causal effect of a one standard deviation increase in genocide intensity on the probability that a mother who was born after 1976 perceives VAC as justified.

The identifying assumption for a causal interpretation on mothers' VAC attitudes is that β_1 and the error term are not correlated, conditional on control variables. The set of control variables we include is flexible across specifications (see Table 2.6.1). First, all specifications include survey year fixed effects (τ_t) to account for overall time trends in outcomes. Second, we add household characteristics, X_{it}, such as mother's religion, her child's age, her years of schooling and the DHS wealth index[26] as well as pre-genocide commune controls such as population size, male and female labor force participation and male and female literacy rates in 1991. Third, we include a commune (γ_c) and a year of birth fixed effect (σ_g) to account for any cross-sectional differences in outcomes across communes and common differences over birth years. We are careful with including additional variables that control for the repeated cross sectional nature of the DHS at the cluster level such as proxies for economic activity, as these variables might be "bad controls" (Angrist & Pischke, 2009). However, we include province-specific linear time trends (survey year $\times$ province), $\mathbf{T}_{jt}$ to account for possible long-run dynamics in socioeconomic and other characteristics across provinces that might affect our outcome of interest. Throughout the analysis,

[26]The DHS wealth index is a measure of relative household wealth (based on assets, type of housing, and access to water and sanitation). Each interviewed household is placed into five quintiles of wealth (from poorest to richest).

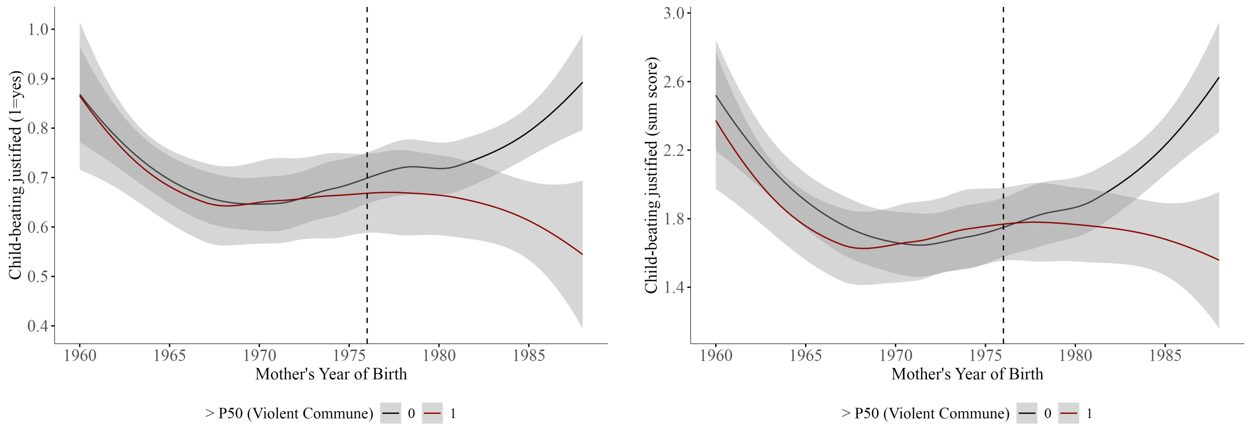

Fig 2.5.1: First stage mechanism. The figure shows local polynomial regressions on the moving average of VAC attitudes across violent and non-violent communes over mother's year of birth. The binary variable $> P50(Violent\ Commune)$ is equal to 1 if the number of category 1 and category 2 *gacaca* court trials records in a commune exceeds the 50th percentile of the distribution. The vertical bar represents the cutoff by which we divide the group of mothers in those old enough to be socialized in a pre-genocide environment (born 1975 or earlier) and those young enough to be socialized in a (post-)genocide environment (born 1976 or later).

we use robust standard errors that are clustered at the commune level to account for potential correlation within spatial clusters.

2.5.2 Second stage: Transmission of attitudes

2.5.2.1 OLS

We then proceed and estimate the transmission effect, i.e. the relationship between mother's VAC attitudes and those of her descendant. We start by estimating the following reduced-form specification that is estimated by OLS:

$$\begin{aligned} VACchild_{icgt} &= \alpha + \beta_1 VACmother_{ig} + \gamma_c + \sigma_g + \tau_t \\ &+ \delta X_{it} + \delta X_{ct} + \kappa \mathbf{T}_{ct} + \xi_{icgt} \end{aligned} \tag{2.11}$$

Here, the dependent variable *VACchild* indicates whether child i living in commune c, with a mother of birth year cohort g, surveyed at the time of survey year t, perceives VAC as justified. We are interested in the effect of mothers' attitudes on the attitudes of their descendants β_1. We include the same control variables and fixed effects as in Eq. (1). Importantly, our household control vector X_{it} controls for child age, child education and a dummy on the child's gender.

We control for child age and the child's education as children from younger women also tend to be younger and have more education. The variable on the child's gender is included in order to account for a potential gendered component in the transmission of attitudes within households, e.g. previous studies have highlighted the importance of same gender role models in the transmission of attitudes (Campante & Yanagizawa-Drott, 2015). All regressions use robust standard errors that are clustered at the commune level.

2.5.2.2 IV

Although this OLS specification controls for numerous and the most important confounding variables that jointly affect the mother's and her child's attitudes at the same time, we may not be able to control for all individual factors that are associated with child and mother VAC attitudes. Hence, the effect of the OLS estimation might be biased, and we rely on IV estimates to mitigate these concerns. We therefore employ a two-stage least squares (2SLS) approach, using the interaction of genocide violence and our binary indicator on whether the mother's socialization was shaped by the genocide (i.e., younger mothers) or not (i.e., older mothers) from Eq. (1) as instrumental variable for mother's VAC attitudes. Apart from the intra-household transmission mechanism we propose, there is no obvious reason why children from younger mothers and from genocide communes should have different attitudes than their counterparts, i.e. children from younger mothers in lower-genocide regions and children from older mothers in the same higher genocide regions, controlling for commune and birth year fixed effects. Thus, under the exclusion restriction that *genocide intensity* $\times$ *young mother* only influences the likelihood of the child's VAC attitudes through its effect on mother's attitudes, β_1 captures the transmission parameter of interest. We discuss threats to this exclusion restriction below.

2.5.2.3 Exclusion restriction

Our identification strategy makes the counterfactual assumption that, absent mother's VAC attitudes, genocide violence interacted with the young mother variable has no effect on VAC attitudes of the child. This is only true under further precautions. The instrument is composed of genocide violence, which is probably correlated with individual household characteristics, such as education or income. These characteristics are likely to affect children's VAC attitudes, as reasons to have less VAC attitudes might be driven by education (Erten & Keskin, 2020). On the other hand, the instrument is composed of mother's birth year and children from younger women tend to have more education, simply by being younger than children from older mothers.

However, we make use of the advantage of interacting both variables to create our instrument. Econometrically, we allow the instruments to have direct effects, but we control for these effects with our difference-in-differences estimator. Genocide violence and mothers' year of birth per se enter the 'second-stage' of the IV-estimation as control variables. Thus, we allow them to have individually differential effects on VAC attitudes, but the identification of mother's VAC attitudes on those of her child only stems from the interaction term, which is arguably exogenous and should only affect mother's VAC attitudes. We cannot think of any obvious reason why children from young mothers and from high genocide communes should have different attitudes than their counterparts, i.e. children from from older mothers in the same communes, and, importantly, younger mothers' children from lower-genocide regions, apart from our proposed mechanism (Table 2.5.1). In order to add additional credibility to our results, we also control for varying factors, such as the general living conditions of individuals, by including the mother's and children's years of schooling and their family wealth in the second-stage. Thus, this strategy allows us to estimate the unconfounded relationship between mother's VAC attitudes on those of her child.

2.6 Results

2.6.1 First stage: Genocide violence and mother's VAC attitudes

Table 2.6.1 reports the results from our first stage regression models. In column 1 we estimate the cross-sectional relationship between genocide intensity and mother's VAC attitudes. The cross-sectional estimate indicates no association between genocide intensity and attitudes on VAC for all women when date of birth is not considered: the coefficient is positive, small and statistically insignificant. We conclude that the genocide itself did not directly influence VAC attitudes. Column 2 presents an estimate of our effect of interest from the most parsimonious model. Column 3 includes community controls, individual controls as well as a survey round fixed effect. We observe a negative and statistically significant impact of *genocide intensity* $\times$

young mother on attitudes toward VAC. We find that in communes with high levels of genocide violence, younger mothers display significantly lower attitudes toward VAC compared to both younger women in other regions or older women within the same commune. Columns 4 and 5 show that the positive relationship is robust in terms of magnitude and statistical significance, when we include commune fixed effects and year of birth fixed effects. The results from our preferred specification (column 5) thus suggest that for a one standard deviation increase in genocide violence younger women are 7.7 percentage points less likely to justify VAC. Compared to the overall sample mean of 0.68, this results in a reduction of around 11.3%. In column 6, we use the same specification and estimate the effect separately for the scale outcome variable (VAC mother sum) and confirm the negative association. Overall, columns 2 to 6 demonstrate that the instruments are highly significant and relevant with F-statistics close to the critical value of 10. To address remaining concerns about weak instruments, we apply weak-instrument robust inference by using robust standard errors clustered at the commune level across all specifications in our second stage.

2.6.2 Second stage: The intergenerational transmission of violent attitudes

We now turn to our second stage and estimate the transmission effect. Column 1 from Table 2.6.2 reports results from a simple single linear regression model without controls. Column 2 estimates the same model as 2SLS model using our instrument (*genocide intensity* × *young mother*) as first stage. We also include estimations using a single instrumental variable (column 3). Single instruments can be favorable in terms of bias as they are approximately median-unbiased (Hahn & Hausman, 2003; Stojetz & Brück, 2023). For the single instrument, we interact the young mother dummy with a binary indicator *median violent sector*, that equals one if the commune is equal or above the median number of *gacaca* court trials scaled by population size in 1991. Columns 4, 5 and 6 add our most important control variables and columns 7 to 9 include the province time trends.

Across all model specifications, we observe a positive and statistically significant effect of a mother's attitudes on those of her child. Specifically, we find a pro-cyclical transmission effect: a child is more likely to approve VAC if their mother holds similar views, and, most importantly, the likelihood decreases if the mother disapproves of such violence. Given that our first-stage

Table 2.6.1: The effect of the Rwandan genocide on mothers' childbeating attitudes

	VAC mother any					VAC mother sum
	(1)	(2)	(3)	(4)	(5)	(6)
Genocide Intensity	0.015	0.011	0.030**			
	(0.012)	(0.013)	(0.013)			
Young Mother		0.070***	0.058***			
		(0.020)	(0.020)			
Genocide Intensity × Young Mother		-0.045**	-0.045**	-0.036*	-0.077***	-0.183***
		(0.017)	(0.021)	(0.021)	(0.025)	(0.070)
Observations	4965	4965	4965	4965	4965	4965
R^2	0.047	0.027	0.050	0.114	0.163	0.188
F-test, stat.	13.604	9.6496	13.205	9.5980	8.5849	7.4142
Individual controls	✓		✓	✓	✓	✓
Community controls	✓		✓			
Survey round FE	✓		✓	✓	✓	✓
Commune FE				✓	✓	✓
Year of birth FE				✓	✓	✓
Province specific linear trend					✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the mother agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother sum*" is the sum score of affirmative items, standardized to mean zero and standard deviation one. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. Commune controls are obtained from the 1991 Census and include population density, literacy rate, and literacy rate for men and women. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

results demonstrate that exposure to genocide significantly reduces young mothers' approval of VAC, our findings indicate a prosocial transmission effect from genocide exposure to subsequent generations; specifically, younger mothers in genocide-affected regions are less likely to endorse VAC, which, in turn, also meaningfully reduces the likelihood of her child to endorse violence. The effects of the IV models are larger than the ones estimated by OLS, suggesting that the OLS likely underestimates the transmission effect. In our preferred IV estimation, the linear instrument returns sizable and stable point estimates around 0.38 pp. In relation to the sample mean of 78% of children agreeing to at least one VAC measure, this results in a reduction of around 51%. Column 9 presents IV estimates using the binary instrument and the full set of controls. The results remain qualitatively and quantitatively consistent across different instrument choices. This stability of the IV coefficient across all model and instrumental variable specifications further strengthens confidence in our findings. Furthermore, the results remain similar if instead of the binary outcome variable we use the child's normalized sum score of affirmative items (Table

Table 2.6.2: The effect of mothers' VAC attitudes on children's VAC attitudes

	VAC child any								
	OLS	IV/2SLS		OLS	IV/2SLS		OLS	IV/2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VAC mother any	0.197***	0.441***	0.475***	0.170***	0.399***	0.342**	0.151***	0.386***	0.356***
	(0.018)	(0.122)	(0.122)	(0.017)	(0.132)	(0.134)	(0.016)	(0.126)	(0.136)
Observations	4965	4965	4965	4965	4965	4,965	4965	4965	4965
R^2	0.048	-0.026	-0.049	0.091	0.026	0.028	0.130	0.060	0.063
Individual Controls				✓	✓	✓	✓	✓	✓
Survey Round FE				✓	✓	✓	✓	✓	✓
Year of Birth FE				✓	✓	✓	✓	✓	✓
Province time trend							✓	✓	✓
IV-variable		Linear	Binary		Linear	Binary		Linear	Binary

Note: Instrumental variable at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal or above the median violent commune (dummy), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC mother any*" and "*VAC child any*" are measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using OLS/2SLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. Commune controls are obtained from the 1991 Census and include population density, literacy rate, and literacy rate for men and women. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

B.5). Similarly, the effect remains consistent if instead of the mother's binary variable we use the mother's sum of affirmative items as main explanatory variable (Table B.6).

Additionally, as a further robustness check, in Table B.7 we limit our sample to children born in 1995 or later, excluding those old enough to have been directly exposed to the genocide. While we argue that our effect of interest is primarily driven by differences in maternal exposure to genocide-related violence and is largely independent of the child's year of birth, this approach addresses concerns that including children born before 1994 might bias our findings. The effects remain stable in terms of both significance and size and support our claim that the effect on children is not driven by direct exposure, but works through mother-level differences in genocide exposure (B.7). Moreover, to address potential concerns about the arbitrariness of our "formative years" definition, we show that the observed effects are not driven by the chosen age cutoff. In our main specification, we define formative years as completed by the age of 18, classifying all women 18 or younger as "young mothers", i.e as those still in their formative years at the time of the genocide. In Table B.4, we re-estimate Eq. (1) using alternative age cutoffs. As expected, the effects remain significant at conventional levels for age cutoffs close to 18, yet, and importantly for our argument, the effect gradually diminishes when the cutoff exceeds 18. Additionally, for all cutoffs below 18, the point estimates are negative, suggesting that our main argument holds even when we consider the formative years to be concluded in younger ages, although some of these estimates lack statistical significance, likely due to the smaller sample size. The effects fade out with young and older age, which is consistent with our argument of formative years during adolescence and suggests that 18 years is a reasonable choice for defining the age cutoff for formative years.

As other studies on consequences of armed conflict, we address concerns due to selective migration or survivor bias. Migration patterns in Rwanda are highly complex (Yonekawa, 2020), and we cannot test these patterns directly as respondents in the DHS are not asked about previous places of residence. However, we consider these biases to be less of a concern in our setting as they likely lead us to underestimate the true effect. For instance, genocide violence was generally weaker in villages with greater levels of education in general and higher literacy rates in particular (Yanagizawa-Drott, 2014). Higher educated people are also more likely to be able to migrate and are thus less likely to currently live in areas that were strongly affected by genocide violence. Those individuals are also the ones for whom we would expect less VAC attitudes, so if anything, we might underestimate the effect in our sample. However, to further support this claim, we control for pre-genocide literacy and education levels (at the commune level) as well as individuals' education post-genocide (measures are taken from the DHS). With respect to survivor bias, it is plausible that women who survived the genocide and had children were exposed to less severe violence than those who did not survive. Again, we thus assume that our estimates represent lower bounds of the true effect. Similarly, we focus on a specific subset of the population, namely (young adult) children who are living together with their mothers, who survived the genocide and are not imprisoned at the time of the surveys (although they might have been in the past). Fig. B.1 in the Appendix, however, shows that this specific sample closely mirrors the general population at that age.

2.6.3 Alternative genocide measures

Our results remain further consistent when employing alternative first-stage specifications. While the *gacaca* trial records serve as a robust and widely recognized measure of genocide violence, aggregating this data at the commune level might introduce potential measurement error, which

Table 2.6.3: Impact of genocide on mothers' childbeating attitudes (alternative instruments)

	VAC mother any			
	(1)	(2)	(3)	(4)
Category 1 Trials × Young Mother	-0.082***			
	(0.027)			
Category 2 Trials × Young Mother		-0.075***		
		(0.025)		
Distance to Mass Grave × Young Mother			0.050***	
			(0.016)	
Days under RPF control × Young Mother				0.039*
				(0.020)
Observations	4965	4965	4965	4965
R^2	0.16380	0.16376	0.16288	0.16222
F-stat	6.4978	6.4929	6.3969	6.3254
Individual controls	✓	✓	✓	✓
Survey round FE	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓
Province specific linear trend	✓	✓	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. Genocide variables are measured as continuous index with mean zero and standard deviation aggregated at the commune level. "Category 1 Trials" and "Category 2 Trials" measure the number of gacaca trials per category and are scaled by commune population size in 1991. "Distance to Mass Grave" measures the average distance to a mass grave per commune and "Days under RPF control" measures the average days a commune has been under RPF control in 1994. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the mother agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. Commune controls are obtained from the 1991 Census and include population density, literacy rate, and literacy rate for men and women. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

could bias our results. To address these concerns, we estimate alternative instrumental variable specifications to underscore the robustness of our results to variations in the classification of violence. In Table 2.6.3, we estimate the impact of genocide violence on younger mothers' attitudes separately for category 1 (Column 1) and category 2 (Column 2) trials. The estimated coefficients remain consistent in terms of size and significance across both trial categories, suggesting that our primary findings are not solely attributable to either category of *gacaca* court trials. Instead, our main instrumental variable reflects a broader measure of genocide violence exposure.

In Column 3 we use the mean distance to a mass grave, averaged across all sectors within a commune and standardized to mean 0 and standard deviation of 1, as alternative instrument. We find that for a one standard deviation increase in distance to a mass grave — indicating greater distance from sites of concentrated genocide violence — is associated with an increase in young mothers' likelihood of endorsing VAC attitudes, which is again in line with our main findings.

Table 2.6.4: The effect of mothers' VAC on children's VAC (alternative first stage instruments)

	VAC child any			
	(1)	(2)	(3)	(4)
VAC mother any	0.396***	0.376***	0.490***	0.459***
	(0.128)	(0.120)	(0.142)	(0.144)
Observations	4,965	4,965	4,965	4,965
R^2	0.055	0.065	0.049	0.037
Individual Controls	✓	✓	✓	✓
Survey Round FE	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓
Year of Birth FE	✓	✓	✓	✓
Province specific linear trend	✓	✓	✓	✓
IV-variable (genocide measure)	Cat1 Trials	Cat2 Trials	Mass grave dist.	RPF days

Note: Instrumental variable at the first stage are (1) the number of category 1 trials, (2) the number of category 2 trials, (3) distance to a mass grave and (4) the average number of days a commune had been under RPD control in 1994, all interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC mother any*" and "*VAC child any*" are measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using 2SLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Similarly, Column 4 estimates the effect of the average number of days a commune was under RPF control during the genocide on mothers' VAC attitudes. in line with our theoretical predictions that more days under RPF control - a measure for less genocide violence - should increase attitudes toward violence for younger women, our findings indicate that a one standard deviation increase in days under RPF control corresponds to a higher likelihood of younger mothers endorsing VAC. Again, these findings reinforce our primary conclusion: younger mothers from areas with greater exposure to genocide violence exhibit lower approval of VAC.

We then turn to estimating our second stage using this alternative set of instrumental variables. Table 2.6.4 reports the results from our preferred IV estimation using category 1 trials (column 1), category 2 trials (column 2), massgrave distance (column 3), and RPF control (column 4) as alternative measures for genocide intensity in Eq. (1). Across all specifications, the effects remain close to the estimated main effect from Table 2.6.2 of 0.38. Thus, we conclude that our primary instrument serves as an appropriate and reliable proxy for measuring genocide violence, and that our main results are robust to alternative measures of genocide violence.

Overall, our results provide strong and robust evidence of a causal, long-term link between exposure to genocide and transmission of violent attitudes within households and across generations. We find a strong prosocial effect of younger mother's exposure to genocide violence and her approval of VAC. This prosocial effect further transmits to the second generation, resulting in less violent attitudes for children who grew up with mothers socialized in times of genocide-induced empowerment.

2.6.4 Potential mechanisms

Our findings reveal a significant pro-social (less-violent) link between the intensity of younger mothers' genocide experience and violent attitudes that are subsequently transmitted to their descendants. The mechanism we propose in this paper is genocide-induced empowerment for younger women. In this section, we present tentative evidence to illustrate this channel using various indicators. Given the complex and multifaceted nature of empowerment, we draw on a set of complementary measures that collectively illustrate our proposed mechanism.

2.6.4.1 Labor force participation

First, we proxy for economic empowerment by the mother's labor force participation. Due to the diminished presence of men, women might be more likely to participate in the labor force as they take on new economic responsibilities and roles traditionally held by men. Prior studies have shown that employment and economic contributions enhance a woman's bargaining power within her household and community and thus serves as a source of empowerment (Duflo, 2012). Moreover, increased economic participation might also strengthen a mother's sense of agency, potentially reshaping her attitudes toward violence.

We estimate the impact of genocide on younger mothers' labor force participation using two variables from the DHS: a binary indicator of current employment equal to 1 if the respondent is currently working, and a binary indicator of cash income equal to 1 if the respondent is paid in cash for her work. We proxy for cash income because it is associated with formal and thus more stable employment, reflecting a constant monetary contribution to the household. Table 2.6.5 shows that the labor force participation rate is higher among younger mothers in communes with higher genocide intensity. A one standard deviation increase in genocide violence is associated with an increased labor force participation of 2.9 percentage points for mothers 18 or younger at

Table 2.6.5: Potential Mechanisms: Young Mother's Empowerment

	Labor Force Participation		Bargaining Power	Domestic Violence	
	Currently Working (1)	Cash Income (2)	(3)	Physical Violence (4)	Norms (5)
Panel A: Partnered Women					
Genocide Intensity × Young Mother	0.029*	0.008	0.018	-0.159***	0.008
	(0.015)	(0.022)	(0.025)	(0.061)	(0.025)
Observations	4,965	4,539	3,581	1,214	4,964
R^2	0.20511	0.27776	0.19379	0.29476	0.22588
Panel B: Partnered and Unpartnered Women					
Genocide Intensity × Young Mother	0.027**	0.018	0.018	-0.113**	0.022
	(0.013)	(0.021)	(0.025)	(0.052)	(0.024)
Observations	6,552	6,019	3,583	1,599	6,551
R^2	0.19695	0.26066	0.19351	0.26391	0.19920
Individual Controls	✓	✓	✓	✓	✓
Survey round FE	✓	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓	✓
Year of Birth FE	✓	✓	✓	✓	✓
Province linear trend	✓	✓	✓	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "Currently Working" is measured using a binary variable that takes the value of 1 if the mother is currently in formal or informal employment. "Cash Income" is a binary variable taking the value of 1 if the respondent is pain in cash or kind for their work. "Bargaining Power" is an index that measures the sum of affirmative items to questions on the mother's involvement in the final say of the family on i) her own health care, ii) making large household purchases, iii) making household purchases for daily needs, iv) visits to family or relatives and v) the food to be cooked each day. "Physical Violence" is a binary variable that takes the value of 1 if the respondent has experienced physical violence by their partner in the past 12 months. "Norms" on domestic violence are measured using the sum score of affirmative items, on the respondent's opinion that a husband is justified in hitting or beating his wife when i) she goes out without telling him, ii) she neglects the children, iii) she argues with him, iv) she refuses to have sex with him or v) she burns the food. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

the time of genocide. We do not find an effect on cash income. The results remain similar for an alternative sample that includes mothers who are not living in partnership (Panel B).

2.6.4.2 Bargaining power

Second, we create a bargaining power index using the DHS indicators on women's involvement in the household decision-making process. The idea behind the bargaining index follows the widely accepted notion that empowerment means having the ability to live the life one chooses (Laszlo et al., 2020). We consider all the questions in the DHS that are related to women being involved in the final say on issues such as i) her own health care, ii) making large household purchases, iii) making household purchases for daily needs, iv) visits to family or relatives, and v) the food to be cooked each day. For example, women having a say over household assets is closely related to their ability to expand their resource base, and thus empowerment (Annan et al., 2021; Duflo, 2012). In order to mitigate concerns about cherrypicking outcome variables, we consider all relevant questions and combine them into a single bargaining power index. Our estimates point toward a positive relationship but we do not find a significant effect.

2.6.4.3 Domestic violence

Third, we test whether younger mothers are less likely to be victim of physical domestic violence. Previous evidence on the empowerment-domestic violence link is mixed. Some studies find that indicators of empowerment, such as employment, decision-making, and education, are associated with a reduction in intimate partner violence as women with greater outside options — due to financial independence or social support — are less likely to experience or remain in violent partnerships (Annan et al., 2021; Duflo, 2012). However, other studies find the opposite, showing that shifts in power dynamics that challenge traditional male dominance can lead to increased violence as a form of male backlash (Laszlo et al., 2020). We test this channel by using the DHS domestic violence module. We measure ”Physical Violence” as binary variable that takes the value of 1 if the respondent has experienced physical violence by their partner in the past 12 months. Attitudes on domestic violence are defined as the sum score of affirmative items on the respondent’s opinion that a husband is justified in hitting or beating his wife.

The results from column 4 suggest that younger mothers from high genocide regions are less likely to experience physical domestic violence by 15.9 percentage points. Relative to the overall sample mean of 0.47 this is a reduction of meaningful 33% compared to their counterparts from non-genocide regions and older mothers. This finding introduces an important dimension of heterogeneity to consider, expanding upon earlier studies such as those by La Mattina (2017), who finds that the genocide-induced shift in sex ratios led to an unfavorable marriage market for women that increased the likelihood to end up with a violent partner. Her argument is related to the classical marriage market argument introduced by Becker (1993). However, our findings do not support this argument for our sample of women.[27]

Moreover, this effect is not driven by selection into relationships, as we observe the same effect for the sample of unpartnered women (see Table B.8). Importantly in our context, we do not assume that local empowerment disrupts the power balance in young women's relationships. Instead, young women have formed attitudes condemning violence during their adolescence —- most likely prior to entering long-term partnerships. As such, we would expect that the most likely mechanism explaining our finding is that women partner with men who share their attitudes (in line with the assortative mating argument;(Dohmen et al., 2012))

2.6.4.4 Father's violent attitudes

To test this, we analyze a subsample of couples for which information is available on both mothers' and fathers' perceptions of VAC.[28] Fig.B.2 in the Appendix plots the difference between father's and mother's VAC attitudes, aggregated over mother's birth year and between violent and non violent communes. Across all birth years, the difference between a father's and a mother's VAC attitudes is close to zero but for regions affected by genocide violence, this difference is negative, indicating women end up partnering with men that have lower VAC attitudes than them. The opposite is true for non-violent regions. In Table B.9 we split the sample of our first stage into father's with VAC attitudes (1 if any, 0 otherwise) and show that our main effects are driven by women who partner with a husband who has the same non-violent attitudes. Although this is a different sample, namely only couples where both wife and husband have been interviewed, the result provides tentative evidence that in households where a women has low VAC attitudes, violence generally plays a minor role as women tend to end up with a non-violent partner.

[27] La Mattina (2017) focuses on partnered women surveyed in 2005 and 2010, whereas our sample includes women-children-pairs across three DHS waves (adding the 2015 wave). The distinction is not as much in our focus on women who are mothers (which most partnered women in Rwanda are), but rather the additional constraint that we introduce in sampling women whose (young adult) children have been interviewed by the DHS as well. While this could explain why our finding differs, it needs to be noted that Rogall & Zarate-Barrera (2021) also obtain the opposite result from La Mattina (2017) for their sample of partnered women.

[28] Please note that for these data on couples, the DHS only collects data on male-female relationships (Balian et al., 2024).

2.7 Conclusion

The aim of this paper was to shed light on the long-term effects of mass violence on violent attitudes across generations. We present empirical evidence that young women who were exposed to higher levels of genocide violence during a formative period when their belief systems and attitudes were still malleable to be significantly less likely to perceive VAC as justified compared to older women from the same regions and to young women from regions with less genocide violence. Across model specifications, our difference-in-differences estimates yield a decrease in VAC attitudes by around 7% percentage points when genocide intensity increases by one standard deviation for these younger women. Using an instrumental variable approach to estimate the transmission effect, we also show that children raised by these mothers experience a significant causal reduction in their likelihood of adopting VAC attitudes even decades after the genocide. We estimate transmission parameters of around 0.38 percentage points, suggesting that these second generation attitudes are in large parts shaped by the attitudes and experiences of their parents. We provide evidence that these effects are driven by a lasting increase in women's empowerment following the 1994 genocide. These younger women experience less domestic violence, are more likely to be employed, and, in a subsample of couples, are found to partner with men who also hold less violent attitudes. As such, our findings underscore and add to previous evidence on the conflict–prosociality link by demonstrating that prosocial (less-violent) effects are persistent across generations. We tie in with previous studies, such as Blattman & Annan (2010), who demonstrate that resilience, rather than debilitating psychological trauma, tends to be the norm following conflict. Our findings further highlight the importance of paying attention to both local and temporal dynamics of conflicts to understand variation in micro-level outcomes. Our results also demonstrate, in contrast to previous studies, that beyond immediate benefits for children's health and education, women's empowerment also has a positive causal impact on shaping the attitudes of subsequent generations. In investigating the cascading effects of women's empowerment, we shed light on an important mechanism that might help explain how violent cycles can be transformed into virtuous cycles. While we study young women's empowerment within a unique context of mass violence, our findings might have important implications for reducing the risk of intra-family violence, and safeguarding children's fundamental right to grow up without violence more generally. Studies that can further advance our understanding of the mechanisms and conditions that lead to children adopting their parents' beliefs and attitudes, specifically on violence, are therefore of crucial relevance.

Our paper is not without limitations. We rely on self-reported attitudinal measures, and can therefore not make claims about actual violent behavior. Future research might benefit from experimentally measuring (proclivity to) violent behavior in experiments with young adults and their parents, who, akin to our design, come from different age groups and have survived an event of mass violence. With respect to survivor bias, it is plausible to assume that those families who survived the genocide and had children were exposed to less severe violence than those who did not survive. Similarly – in light of our research question – we focus on a specific subset of the population, namely families where both parents survived the genocide, neither is imprisoned at the time of the surveys (although they might have been in the past), who are still living together and have children. This excludes a relevant group in Rwanda (and any post-conflict society), namely single-parent households, widows, and widowers. Extending the analysis to that group might be a fruitful avenue for future research.

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B Appendix

B.1 Age distribution of sample and children in the Rwandan population

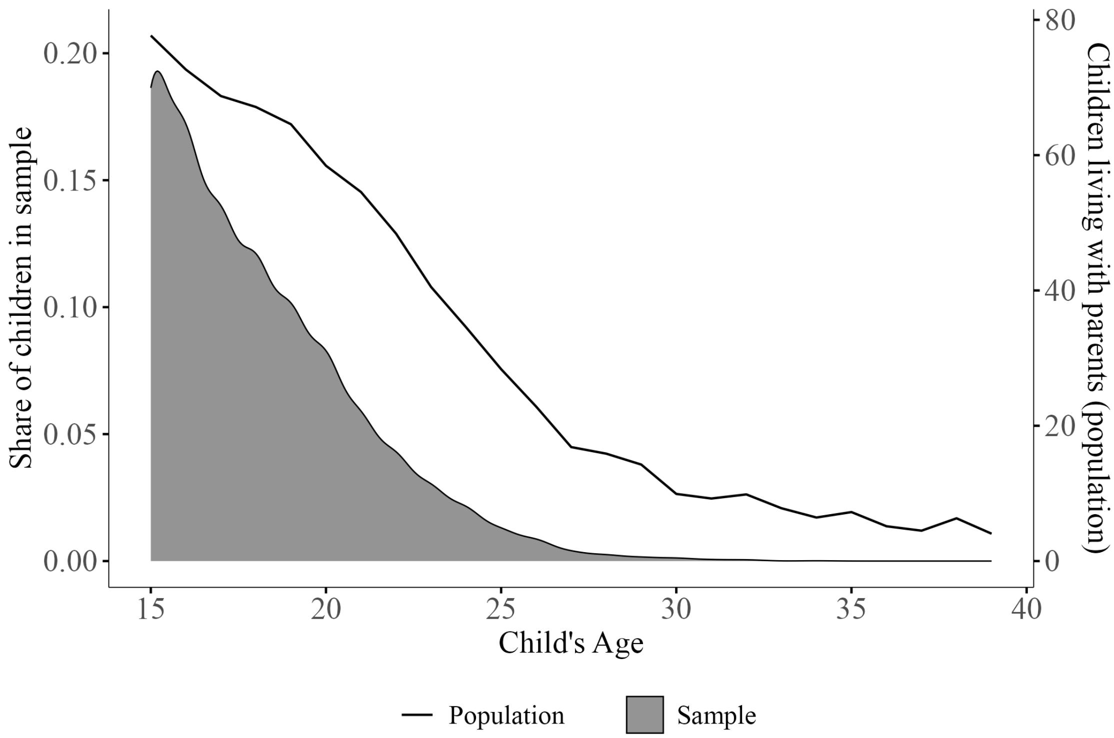

Fig B.1: The figure shows the share of second generation individuals in our sample, hence (young) adults that live in the same household as their mother, and the share of individuals who do live in the same household as their mother (representative of the general Rwandan population).

B.2 Relationship between fathers' and mothers' VAC attitudes

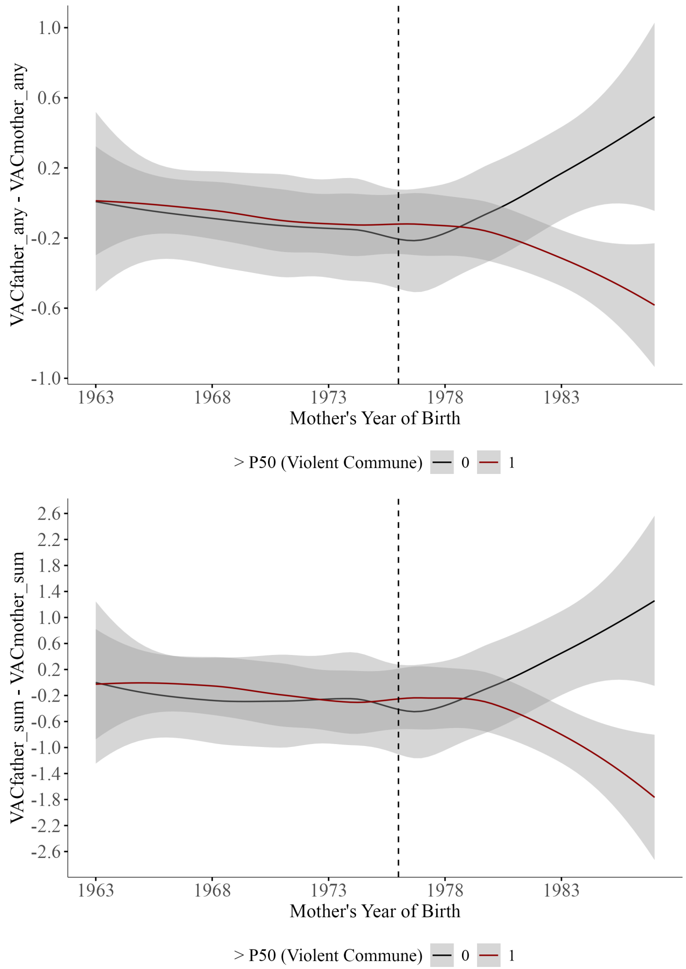

Fig B.2: Subset of main data restricted to couples. The figures display local polynomial regressions of the difference in VAC attitudes between fathers and mothers across violent and non-violent communes, aggregated over the mother's year of birth. The binary variable $>$ *P50(Violent Commune)* is equal to 1 if the number of category 1 and category 2 *gacaca* court trials records in a commune exceeds the 50th percentile of the distribution. The vertical bar represents the cutoff by which we divide the group of mothers in those old enough to be socialized in a pre-genocide environment (born 1976 or earlier) and those young enough to be socialized in a post-genocide environment (born 1977 or later).

B.3 Correlation of Gacaca trials and Gacaca verdicts

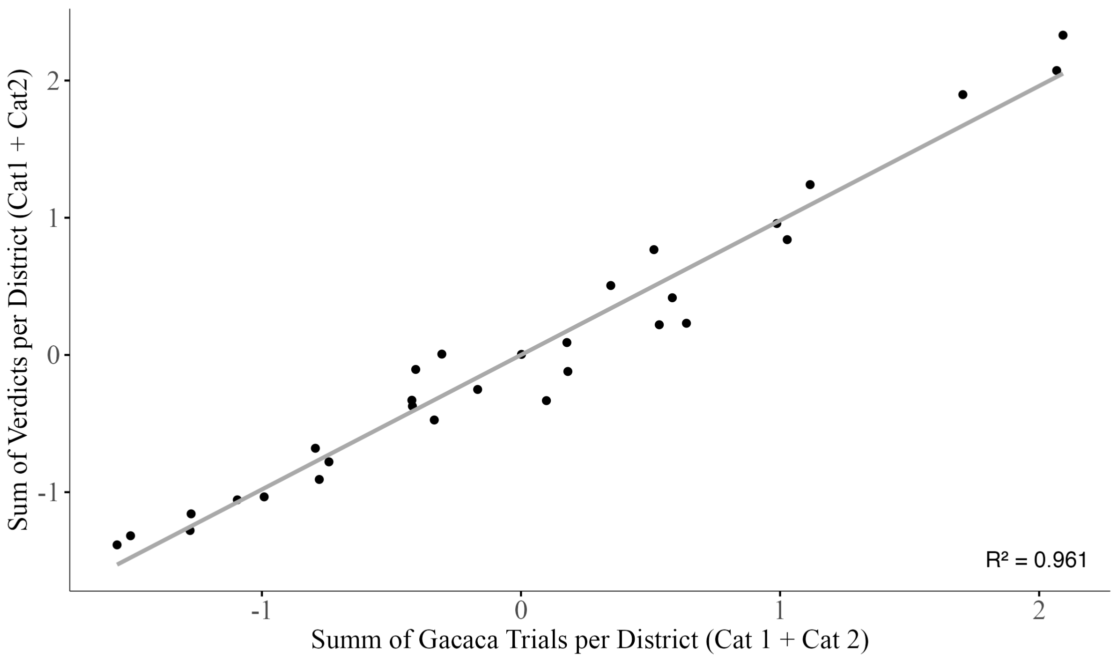

Fig B.3: Correlation between *gacaca* trials and verdicts, both aggregated at the district level (n=30)

B.4 First stage effect at different Age Cutoff Points

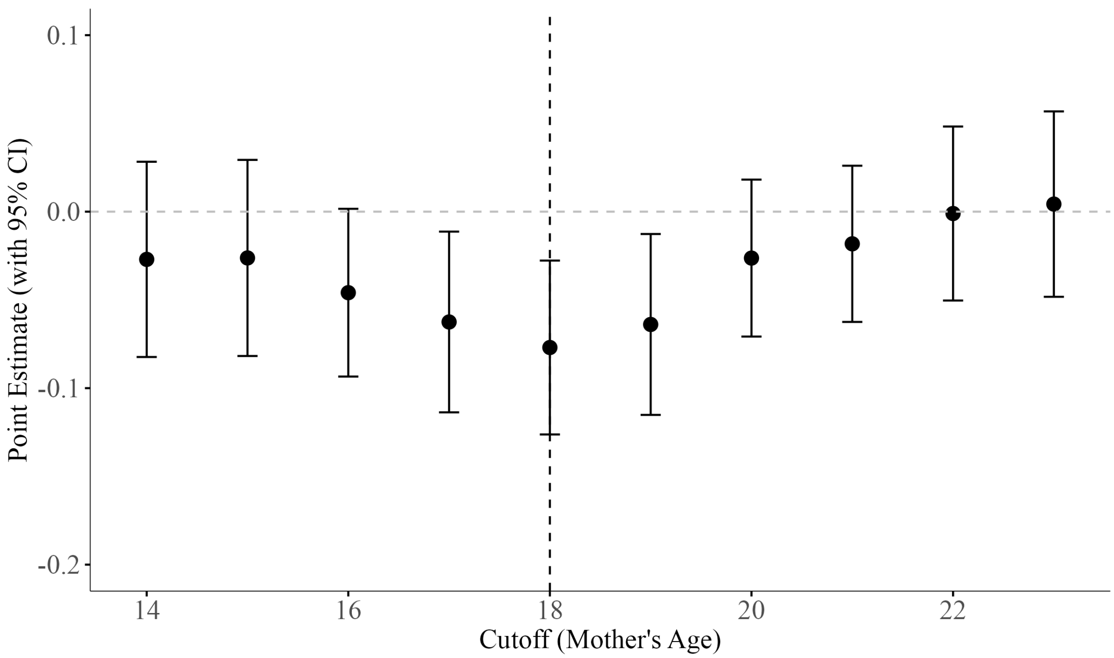

Fig B.4: The figure shows fully specified first-stage estimations using different age-cutoffs to determine mothers in their formative years (young mother). 95% confidence intervals are based on clustered standard errors at the commune level.

B.5 Different outcomes specification VAC child sum

Table B.5: The effect of mothers' VAC on children's VAC (child sum)

	VAC child sum								
	OLS	IV		OLS	IV		OLS	IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VAC mother any	0.615***	1.64***	1.74***	0.503***	1.23***	1.19***	0.435***	1.01***	1.03**
	(0.051)	(0.376)	(0.389)	(0.048)	(0.404)	(0.409)	(0.046)	(0.351)	(0.397)
Observations	4,967	4,967	4,967	4,965	4,965	4,965	4,965	4,965	4,965
R^2	0.0528	-0.0955	-0.1241	0.1106	0.0343	0.0419	0.1501	0.1029	0.1000
Individual Controls				✓	✓	✓	✓	✓	✓
Survey Round FE				✓	✓	✓	✓	✓	✓
Commune FE				✓	✓	✓	✓	✓	✓
Year of Birth FE				✓	✓	✓	✓	✓	✓
Province specific linear trend							✓	✓	✓
IV-variable (genocide measure)		Linear	Dummy		Linear	Dummy		Linear	Dummy

Note: Instrumental variable at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal or above the median violent commune (dummy), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC child any*" is measured as the sum of affirmative items. Regressions are estimated using OLS/2SLS with linear and dummy instruments at the first stage respectively. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

B.6 Different endogenous variable specification VAC child sum

Table B.6: The effect of mothers' VAC (mother sum) on children's VAC

	VAC child any								
	OLS	IV		OLS	IV		OLS	IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VAC_mother_sum	0.068***	0.121***	0.129***	0.057***	0.107*	0.102*	0.051***	0.121**	0.109**
	(0.006)	(0.043)	(0.043)	(0.006)	(0.056)	(0.057)	(0.006)	(0.047)	(0.050)
Observations	4,967	4,967	4,967	4,965	4,965	4,965	4,965	4,965	4,965
R^2	0.0482	0.0194	0.0104	0.0886	0.0549	0.0597	0.1278	0.0744	0.0884
Individual Controls				✓	✓	✓	✓	✓	✓
Survey Round FE				✓	✓	✓	✓	✓	✓
Commune FE				✓	✓	✓	✓	✓	✓
Year of Birth FE				✓	✓	✓	✓	✓	✓
Province specific linear trend							✓	✓	✓
IV-variable (genocide measure)		Linear	Binary		Linear	Binary		Linear	Binary

Note: Instrumental variable at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal or above the median violent commune (binary), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC child any*" is measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother any*" is measured as the sum of affirmative items. Regressions are estimated using OLS/2SLS with linear and binary instruments at the first stage respectively. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

B.7 Post 1994 born only

Table B.7: Transmission of Violent Attitudes (post 1994-born only)

	VAC child any								
	OLS	IV		OLS	IV		OLS	IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VAC_mother_any	0.198***	0.507***	0.545***	0.176***	0.385**	0.395**	0.152***	0.405***	0.396**
	(0.021)	(0.130)	(0.133)	(0.021)	(0.158)	(0.165)	(0.020)	(0.145)	(0.154)
Observations	3,295	3,295	3,295	3,293	3,293	3,293	3,293	3,293	3,293
R^2	0.049	-0.070	-0.102	0.089	0.026	0.021	0.139	0.057	0.062
Individual Controls				✓	✓	✓	✓	✓	✓
Community Controls				✓	✓	✓	✓	✓	✓
Survey Round FE				✓	✓	✓	✓	✓	✓
Commune FE				✓	✓	✓	✓	✓	✓
Year of Birth FE				✓	✓	✓	✓	✓	✓
Province specific linear trend							✓	✓	✓
IV-variable (genocide measure)		Linear	Dummy		Linear	Dummy		Linear	Dummy

Note: Subsample of main sample including only post 1994 born children. Instrumental variable at the first stage are (1) the number of category 1 and 2 trials per commune (linear) or (2) whether the commune is equal or above the median violent commune (dummy), both interacted with our "young mother" variable. "Young mother" is defined as a mother from a birth year cohort > 1976. "*VAC mother any*" and "*VAC child any*" are measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. Regressions are estimated using OLS/2SLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. Commune controls are obtained from the 1991 Census and include population density, literacy rate, and literacy rate for men and women. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

B.8 Different sample: Including women not in partnership

Table B.8: Impact of genocide on mothers' childbeating attitudes

	VAC mother any (1)	VAC mother sum (2)
Genocide Intensity × Young Mother	-0.061***	-0.151**
	(0.023)	(0.064)
Observations	7,591	7,591
R^2	0.14750	0.17160
F-test	8.2804	9.4216
Individual controls	✓	✓
Survey round FE	✓	✓
Commune FE	✓	✓
Year of birth FE	✓	✓
Province specific linear trend	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. "*VAC mother any*" is measured using a binary variable that takes the value of 1 if the mother agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother sum*" is the sum score of affirmative items, standardized to mean zero and standard deviation one. Regressions are estimated using OLS. Individual controls include mother's education years, a wealth index, as well as the child's gender, birth year and years of education. Commune controls are obtained from the 1991 Census and include population density, literacy rate, and literacy rate for men and women. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

B.9 First stage (split couples sample)

Table B.9: Impact of genocide on mothers' childbeating attitudes

	VAC mother any		VAC mother sum		VAC mother any		VAC mother sum	
VAC father any	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Genocide Intensity × Young Mother	-0.112*	0.069	-0.240	0.200				
	(0.062)	(0.076)	(0.162)	(0.225)				
median_violent_cell × Young Mother					-0.401***	-0.007	-0.870**	-0.221
					(0.126)	(0.113)	(0.344)	(0.365)
Observations	784	1,150	784	1,150	784	1,150	784	1,150
R^2	0.47	0.40	0.50	0.43	0.48	0.40	0.50	0.43
Survey Round FE	✓	✓	✓	✓	✓	✓	✓	✓
Commune FE	✓	✓	✓	✓	✓	✓	✓	✓
Mother Year of Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
District Level Trend	✓	✓	✓	✓	✓	✓	✓	✓

Note: "Young mother" is defined as a mother from a birth year cohort > 1976. "Genocide intensity" is measured as a continuous index with mean zero and standard deviation one of the number of category 1 and category 2 gacaca trials aggregated at the commune level and scaled by commune population size in 1991. Median violent cell is equal to 1 if the number 1 and 2 trails at the commune level are above the median violent commune (dummy). "*VAC mother any*" and "*VAC father any*" is measured using a binary variable that takes the value of 1 if the respondent agrees to any of the three statements on violence against children, and 0 otherwise. "*VAC mother sum*" is the sum score of affirmative items, standardized to mean zero and standard deviation one. Regressions are estimated using OLS. Individual controls include mother's and father's education years, father's birth year, a family wealth index, as well as the child's gender, birth year and years of education. In all estimations, standard errors controlling for spatial correlation are in parentheses and clustered at the commune level. Significance levels are indicated as *p<0.1; **p<0.05; ***p<0.01.

B.10 *Gacaca* categories

Please note that this overview is taken from Nyseth Brehm et al. (2014, 347f.). The initial categorization classified acts of participation into four categories, but was later adapted to the three categories below (Republic of Rwanda, 1996; Corey & Joireman, 2004).

Category 1

1. Any person who committed or was an accomplice in the commission of an offense that puts him or her in the category of planners or organizers of the genocide or crimes against humanity;
2. Any person who was at a national leadership level or that of prefecture (state) level—including those serving in public administration, political parties, army, gendarmerie, religious denominations, or a militia—who committed crimes of genocide or crimes against humanity or encouraged others to participate in such crimes, together with his or her accomplice;
3. Any person who committed or was an accomplice in the commission of offense that puts him or her among the category of people who incited, supervised, and were ringleaders of genocide or crimes against humanity;
4. Any person who was at the leadership level at the sub-prefecture and commune (municipality)— including those serving in public administration, political parties, army, gendarmerie, communal police, religious denominations, or a militia—who committed any crimes of genocide or other crimes against humanity or encouraged others to commit similar offenses, together with his or her accomplice; and
5. Any person who committed the offense of rape or sexual torture, together with his or her accomplice.

Category 2

1. A notorious murderer who distinguished himself or herself in his or her location or wherever he or she passed due to the zeal and cruelty used, together with his or her accomplice;
2. Any person who tortured another even though such torture did not result into death, together with his or her accomplice;
3. Any person who committed a dehumanizing act on a dead body, together with his or her accomplice;
4. Any person who committed or was an accomplice in the commission of an offense that puts him or her on the list of people who killed or attacked others resulting in death, together with his or her accomplice;
5. Any person who injured or attacked another with the intention to kill but such intention was not fulfilled, together with his or her accomplice; and
6. Any person who committed or aided another to commit an offense against another without an intention to kill, together with his or her accomplice

Category 3

1. A person who only committed an offense related to property. However, when the offender and the victim came to a settlement and settled the matter before authorities or witnesses before commencement of the law, the offender was not prosecuted.

CHAPTER III

THE HUMAN CAPITAL COSTS OF VIOLENT TEACHERS

ABSTRACT

This paper provides evidence on the effect of violent teachers on human capital formation. I combine panel data from multiple cohorts of primary students in rural Malawi with detailed information on their teachers' violent behavior during class, including physical, emotional and sexual violence. To tackle the potential endogeneity of teacher violence, I apply a difference-in-differences design with staggered treatment and estimate a value-added model that includes teacher fixed effects and lagged student's test scores. My results suggest that violent teachers reduce grade progression by 30 percentage points and decrease math scores by 15 percentage points over one academic year. Early secondary students show the highest effects. I do not find any significant effects on English and Chichewa test scores.[1]

Keywords: Teacher Violence, Education, Test Scores, Grade Progression, Malawi
JEL codes: O13, O12, I20

[1] I am grateful to participants of the Development Research Seminar series and several doctoral seminars at the University of Konstanz for their useful comments. I gratefully acknowledge the financial support provided by the German state of Baden-Württemberg through its promotion of young scientists and artists program (Landesgraduiertenförderungsgesetz). A special thanks to the Population Council Washington for sharing the data with me. The opinions, findings and conclusions reported in this article are those of the author.

3.1 Introduction

Violence against children is not only a critical public health, social and human rights issue (Tapsoba, 2023; Cuartas, 2021; Berthelon et al., 2018; Hoeffler, 2017), it also significantly disrupts the formation of human capital (Slade & Wissow, 2007; Brown & Velásquez, 2017; Kumar et al., 2022). Much of the existing human capital literature has focused on violence against children in conflict zones (Bertoni et al., 2019; Brück et al., 2019; Callen et al., 2014), domestic violence (Gutierrez & Molina, 2021; Carrell & Hoekstra, 2010) or violence surrounding schools (Ang, 2020; Casey et al., 2018; Koppensteiner & Menezes, 2021). However, a significant share of violence against children takes place within schools (Evans et al., 2023; Ammermueller, 2012) and a main contributor to this are violent teachers (Baker-Henningham et al., 2009; Psaki et al., 2017; Deole, 2018; Kumar et al., 2022). In Malawi, for instance, approximately 40% of children experience physical violence in schools, most often at the hands of their teachers, who view it as an acceptable form of discipline (Ameli, 2017)[2]. However, despite it's high prevalence, research on the effect of teacher violence on students' human capital formation is sparse and largely underexplored by the economic literature.

In this paper, I address this gap by examining the effect of teacher violence on students' human capital formation in the context of rural Malawi. In particular, I examine how teachers' use of physical, emotional or sexual violence during class affects students' likelihood of repeating a grade and perform lower in test scores. I focus on the context of rural Malawi, where students persistently show one of the highest grade repetition rate in Sub-Saharan Africa and consistently score among the lowest in standardized math and English assessments (Hungi & Thuku, 2010). I study human capital formation at the extensive and intensive margin using student data on annual grade progression and standardized test scores in math, Chichewa and English collected by the Malawi Schooling and Adolescent Study (MSAS) between 2007 and 2013. I focus on these two channels which are fundamental to human capital accumulation, as they reflect the quantity and quality dimension of education (World Bank, 2017).

[2] Although still widely used, corporal punishment is a violation of a child's rights according to the UN Convention on the Rights of the Child

Theoretically, teacher violence may negatively affect educational quality by harming students' mental health and reducing their motivation to learn. Studies have shown that students subject to physical or emotional harm experience heightened anxiety, poorer mental health outcomes and greater difficulty with attention and concentration (Michaelsen & Salardi, 2020). This, in turn, leads to lower cognitive development and test score results (Baker-Henningham et al., 2009). At the same time, teacher violence may also lower educational quantity by increasing the likelihood of dropout or grade repetition (Kumar et al., 2022). Thus, studying both dimensions of education production is crucial for understanding the effect of teacher violence on the formation of human capital.

In the empirical analysis, I use matched student–teacher level records from longitudinal data of 1,764 students across two states in rural Malawi. I couple individual students' survey data with detailed information on their teachers' use of violence in the classroom, including measures of physical, emotional and sexual violence. To identify a causal effect on the extensive margin, I apply a staggered difference-in-differences design with multiple periods and groups, that leverages the staggered exposure to teacher violence during different grades. At the intensive margin, I use a value-added model that includes lagged test score of students and teacher fixed effects to control for all observable and unobservable characteristics of the teachers and school inputs, while identifying the effect of teacher violence on test scores. Thus, this identification strategy is based on a comparison of the value-added in test scores of students being victim of violence versus non-victim taught by the same teacher, across teachers with varying levels of violence use.

My results show that teacher violence significantly undermines human capital formation at both the extensive and intensive margins. At the extensive margin, exposure to violent teachers decreases the likelihood of getting promoted to the next grade by 30 percentage points over the survey period. To account for the varying impact of different forms of violence, I analyze three distinct measures of violence separately: (i) physical violence, including actions such as being punched, slapped or whipped by the teacher; (ii) emotional violence, involving verbal abuse; and (iii) sexual violence, characterized by unwanted or inappropriate physical contact of a sexual nature with the teacher. The results show that all types of violence significantly reduce student's grade progression and test scores, but the effects are the most pronounced for sexual violence. The results further reveal that teacher violence disproportionately affects young students and those in early secondary education, with the largest effect sizes observed among those who were exposed to violence in early secondary education (grade 9 and 10). The decline at the extensive margin couples with negative effects on test scores. The findings show that violent teachers reduce math scores by around 0.15 percentage points over the academic year. Importantly, these results are not driven by selection, as I only include students in the analysis that are attending school in both waves and for which test scores data is available. Moreover, the negative effects on math scores remain consistent and statistically significant when I control for previous lagged test scores, student's sorting and learning efforts by the rest of the class between testing rounds. I also account for earlier expose to teacher violence and find similar results. However, I do not find any significant effects on English or Chichewa test scores.

The main contributions of this paper are threefold: First, this study adds to the growing body of literature examining the effects of violence on children's educational outcomes. Existing studies have shown that exposure to violence—whether at home (Cuartas, 2021; Carrell & Hoekstra, 2010), around schools (Ang, 2020; Casey et al., 2018; Koppensteiner & Menezes, 2021) or in the community (Brown & Velásquez, 2017; Brück et al., 2019; Bertoni et al., 2019; Akresh & de Walque, 2008)—can have severe consequences for human capital formation. For instance, Cuartas (2021) finds a strong link between corporal punishment and lower cognitive outcomes in Columbia, with children exposed to corporal punishment being about 24% less likely to be developmentally on track than children who were not exposed to corporal punishment. However, much of this evidence focuses on other forms of violence against children, with little attention paid to teacher-perpetrated abuse. While a few studies have started to explore the prevalence and consequences of violence in schools, these studies are mostly of cross sectional nature (Devries et al., 2014; Ameli, 2017; Psaki et al., 2017; Baker-Henningham et al., 2009). Deole (2018), for instance, documents a negative effect of school violence on math performance in Brazil. Ammermueller (2012), using Trends in International Mathematics and Science Study (TIMSS) data for several European countries, finds that being a victim of violence in schools has negative impacts on contemporary and later student performance as well as the level of educational attainment. Kumar et al. (2022) uses cross sectional data from India and finds that corporal punishment experienced by a child in school adversely affects their cognitive test scores. Most relevant to my context, Psaki et al.

(2017) study the associations between school violence and three education outcomes, absenteeism, learning and dropout, using the MSAS data. They find that school-related sexual violence was associated with poorer education outcomes for both, males and females while physical violence was not. However, these studies do not sufficiently distinguish between abuse perpetrated by other students, school staff, and teachers or are limited in tackling the potential endogeneity of violent teachers, i.e. the need to isolate the effect of violence from general teacher ability and earlier education inputs. My study expands on this literature by providing robust causal evidence on the specific impact of teacher violence. To the best of my knowledge, this is the first study to causally assess the impact of teacher violence on human capital formation in a low-income setting, where such research is particularly scarce. By showing that the effects on the extensive and intensive margin of human capital formation are substantial, I also provide a novel perspective on the "global learning crisis" (World Bank, 2017) as teacher violence not only harms educational quality, it also decreases the likelihood of a successful grade progression. This is particularly relevant to the context of Malawi, where grade repetition is an issue for development (Hungi & Thuku, 2010). I further contribute to the literature by offering a distinct analysis of three different measures of teacher violence. The estimates consistently show negative effects across all types of violence, with slightly larger impacts for sexual violence. This aligns with the study by Evans et al. (2023), who highlight the need to measure both physical and sexual violence in schools. In line with their findings, my results also emphasize the necessity of studying all children, regardless of their schooling status. While I observe a negative effect on learning outcomes for students who remain in school, my results also show that teacher violence causes victims of violence more often to repeat a grade and drop out of school than non-victims. Thus, I add value to the literature by showing that teacher violence can have long-term consequences on human capital formation at both, the intensive and extensive margin.

Second, I contribute to the literature that highlights the role of school inputs as an important determinant in educational production. Existing research has demonstrated that better-quality schools can have profound impacts on students' academic and non-academic outcomes (Lucas & Mbiti, 2014; Clark & Del Bono, 2016), long-term economic growth (Hanushek & Woessmann, 2012; Breton, 2011) and also serve as protective spaces, shielding children, particularly girls, from external forms of violence (Behrman et al., 2017). I add to this literature by analyzing data from students in Malawi, a country with low school quality (Kaarsen, 2014; Hungi & Thuku, 2010). The literature has identified two inner-classroom dynamics as main determinants of school quality. One the one hand, studies have highlighted the critical role of peers (Duflo et al., 2011). For instance, negative peer behaviors, such as bullying or disruptions, are associated with poorer test scores and reduced performance (Brown & Taylor, 2008; Carrell et al., 2018; Comi et al., 2021; Burke & Sass, 2013; Alan et al., 2021). On the other hand, teachers are perhaps the most critical input in educational production within schools (Rothstein, 2010; Chetty et al., 2014b,a). Recent evidence shows that teacher quality significantly affects both cognitive and non-cognitive skills (Jackson, 2018; Kraft, 2019) and even marginal improvements in teacher effectiveness yield measurable academic benefits (Azam & Kingdon, 2015). A strength of my paper lies in separating out the effect of teachers' violence from several other teacher characteristics such as ability, by including teacher fixed effects. This is important, as recent studies have highlighted that teachers in many low income countries have a large variation in their subject knowledge (Metzler & Woessmann, 2012; Bold et al., 2017; Bau & Das, 2020) and experience (Papay & Kraft, 2015) that may simultaneously affect students' achievement. Additionally, by having data on the learning improvements of other students in the class room, I am able to isolate the effect of teachers' violent behavior from the effect of their varying ability, e.g. on peers' test scores. Moreover, a few studies have emerged on the importance of teachers' attitudes and teaching preferences in education production. Specifically, I add to this smaller body of studies that shows how teachers' attitudes, e.g. on ethnic groups (Alan et al., 2023) or gender norms (Lavy & Sand, 2018; Rakshit & Sahoo, 2023), can have important implications for human capital formation. The latter is particularly important in our context as girls are also more likely to experience violence by teachers (Steiner et al., 2021). I add to this literature by showing that preferences for violent teaching methods affect student's educational progress. Importantly, my findings suggest that the negative effect sizes from teacher violence are similar in size and magnitude to the positive effects from studies investigating the impact of subject knowledge (Metzler & Woessmann, 2012).

Third, this study also engages with the ongoing debate around the use of value-added models (VAM) to measure teacher impacts, particularly in low-income settings.[3] While VAM have been widely used in high-income contexts to evaluate teacher quality and effectiveness (Chetty et al.,

[3] See Koedel et al. (2015) for an overview

2014b,a; Jaschke & Keita, 2021), their applicability in low-income settings remains controversial (Guarino et al., 2015). Recent work, such as Bau & Das (2020), Crawfurd & Elks (2019), and Sass et al. (2014) however provide recent evidence that VAM can produce unbiased estimates of teacher effectiveness in low-income settings. I contribute by applying a VAM in rural Malawi to quantify the effects of teacher violence. Evaluating teacher effectiveness is particularly relevant in this context, as the limited availability of schools makes school choice more constrained than in higher-income settings (Beuermann et al., 2023). In such contexts, where students are less able to choose their schools, concerns about non-random sorting of students into classrooms—an important issue in VAM (Hayes et al., 2023; Rothstein, 2010; Sass et al., 2014)—are naturally mitigated. Nevertheless, I demonstrate that incorporating controls for student sorting and peer learning effects in VAM can significantly improve statistical precision and efficiency. This underscores the potential utility of these models for evaluating teacher effectiveness in similar low-income settings.

The rest of the paper is organized as follows. Section 2 provides an overview of the institutional background relevant to this study. Section 3 describes the dataset, main variables, and presents some descriptive analysis of teacher violence. Section 4 outlines the empirical approaches. The results of the analysis are discussed in Section 5, and Section 6 concludes.

3.2 Institutional background

3.2.1 Education system in Malawi

The education system in Malawi follows an 8-4-4 system, consisting of eight years of primary education, four years of secondary education (divided into two years of lower secondary and two years of higher secondary), and four years of tertiary education. The official age of entry into school is 6 years. In 1994, the Malawian government abolished primary school fees, leading to an increase in enrollment rates at both the primary and secondary levels (Al-Samarrai & Zaman, 2007). However, despite the free primary education, only 35% of Malawian children completed primary school in 2010. Frequent challenges, such as late entry and grade repetition, result in many students being over-aged for their grade, leading to age heterogeneity in classrooms and prolonged time to complete primary education—in some cases up to 23 years (World Bank, 2010).

Studies have shown that grade repetition is the most impactful factor in school dropout decisions, most likely due to parents' perceptions of school, as high repetition rates in primary education may discourage parents from keeping their children in school (Mingat, 2003). In 2010, Malawi had one of the highest repetition rates globally (World Bank, 2010). In primary education, annual repetition rates reached approximately 20%, the highest level in the Sub Saharan African region and evidence at both, national and international levels, indicates that high repetition rates do not improve learning outcomes but instead increase the likelihood of dropping out of school (World Bank, 2010). They also negatively impact the student-teacher ratio and raise education costs that are spent to provide primary education for repeaters (Taniguchi, 2015). This issue is most pronounced in standards 1 to 4, where repetition rates are highest in Malawi. In addition, many Malawian students who do complete primary school lack basic literacy and math skills. In 2010, Malawian students ranked among the lowest in student performance, with the weakest results in English reading and second lowest in mathematics, among 15 Southern African countries assessed by the Southern African Consortium for Monitoring Educational Quality (SACMEQ) (Hungi & Thuku, 2010). While reforms were proposed, including building more classrooms, improving teacher deployment, and reducing repetition, many issues continue to impede the goal of providing quality education to all children in Malawi and are still of relevance for the Malawian School System (Sunny et al., 2017). After completing primary school, students may advance to secondary education. The first two years culminate in the Junior Certificate of Education (JCE), while the final two years lead to the Malawi School Certificate of Education (MSCE). Secondary schools are designed to prepare students for higher education or vocational training. However, access to secondary education is often limited by high costs, competitive admission processes, and inadequate infrastructure.

3.2.2 Legal situation on violence in schools

Despite international agreements such as the UN Convention on the Rights of the Child, which guarantees the protection of children from all forms of violence, corporal punishment remains widespread in many educational settings in Malawi (Ameli, 2017; Psaki et al., 2017). The data used in this paper reflects the situation in Malawi in 2011. At that time, the legal framework regarding corporal punishment was unclear. While the 1962 Education Act was under review in 2009, and recommendations were made to explicitly prohibit corporal punishment, the 2012 Education Act, which replaced it, remained silent on the issue Psaki et al. (2017). Therefore, while more recent policies indicate a stronger stance against corporal punishment, the findings in this paper should be understood in the context of Malawi's legal and policy environment as it stood in 2011. Although the 2009 review of the Malawian Education Act recommended an explicit ban on corporal punishment, it continues to be a common practice in schools and a few studies consistently show that corporal punishment is still regularly administered in Malawian schools. For instance, Ameli (2017) report that nearly 40% of children had experienced corporal punishment at school, with many students expressing fear of attending school due to the threat of violence. This violence is not limited to physical punishment but also includes emotional and sexual, gender-based violence (Ameli, 2017).

3.3 Data and descriptive analysis

3.3.1 Data

I use data from the Malawi Schooling and Adolescent Study (MSAS) to estimate the effect of violent teachers on children's human capital formation. The MSAS is a panel study that began in 2007, following 2,649 adolescents aged 14–17 from the rural districts of Balaka and Machinga in southern Malawi (Fig.C.1). The sample consists of 1,764 students (875 girls and 889 boys) randomly selected from 59 primary schools in the two districts. These schools were selected randomly, with their chances of inclusion weighted according to their 2006 enrollment figures. At each selected school, about 30 students stratified by gender, age and attending standards 4–8 (which corresponds to the last five years of primary education) were sampled. Additionally, a separate sample of 885 out-of-school adolescents from the surrounding communities was also drawn. The data from this group are not included in the analysis presented here, as this study focuses exclusively on the subset of children who were enrolled in school during the first round of data collection in 2007. Following the initial round of data collection, yearly follow-ups were conducted from 2008 to 2011, with an additional follow-up in 2013. The overall attrition rate across rounds is very small with re-interview rates of 95% in 2008, 93% in 2009, 91% in 2010 and 2011, and 84% in 2013.

The MSAS gathered comprehensive data on adolescents, including socioeconomic and family backgrounds, detailed educational histories (recording the year, grade level, and age at which dropouts or grade repetitions occurred), classroom experiences (such as exposure to teacher violence), and labor market participation and employment status. Table 3.3.1 displays summary statistics on variables used at the student's level. In waves 2 and 3, the survey also administered age-specific assessments in mathematics, English, and Chichewa. All survey waves were conducted consistently between the same period of the year (June and early September) aligning with the end of the school year in Malawi, where the academic year typically begins in late September. This timing ensures comparability across waves and captures students' experiences and outcomes at a consistent point in their academic cycle.

Several features of the MSAS are of particularly importance for this study. First, a key feature of the MSAS is the inclusion of detailed questions on school violence, specifically capturing students' experiences of physical, emotional, and sexual violence as well as identifying the perpetrator. Physical violence includes being punched, slapped, or whipped by teachers, schoolmates or others, either at school or on the way to school. For instance, students were asked how often a teacher physically struck them during the past school year. Emotional violence encompasses experiences such as verbal abuse. Sexual violence refers to any unwanted or inappropriate physical contact of sexual nature. To the best of my knowledge, the MSAS is the only panel dataset to systematically differentiate between physical, emotional and sexual violence in the classroom and to attribute specific incidents to distinct perpetrator groups (See for instance Evans et al., 2024). This level of specificity allows a nuanced understanding of the prevalence and impact of teacher-related violence.

Second, questions on physical, emotional and sexual violence were administered via Audio Computer-Assisted Self-Interviewing (ACASI), enabling students to privately and anonymously input their responses directly into a computer or tablet. The use of ACASI is critical for mitigating measurements errors, such as social desirability bias or student's reluctance to disclose information that often accompany sensitive survey questions such as on experiences of violence. Previous studies, such as those by Kelly et al. (2013), have shown that in Malawi ACASI significantly increases the reporting rates of sensitive experiences, such as sexual violence,

Table 3.3.1: Summary statistics: Student level characteristics

Variable	Mean	SD	Min	Max	N
Panel A: Grade progression					
Male	0.50	0.50	0.0	1.00	1705
Age (wave 1)	14.77	1.10	13.0	19.00	1705
District of Balaka	0.49	0.50	0.0	1.00	1705
Ever repeated a standard	0.92	0.27	0.0	1.00	1705
Age at school start	6.85	1.53	0.0	13.00	1705
Late school entry (>8 years of age)	0.28	0.45	0.0	1.00	1705
Class of first violent incident	4.67	5.65	1.0	11.00	1705
Highest class ever attended	8.38	1.98	4.0	12.00	1705
Panel B: Test scores in Math, Chichewa and English					
Male	0.41	0.49	0	1	990
Age (wave 2)	15.78	0.88	14	19	990
District of Balaka	0.50	0.50	0	1	990
Orphan	0.06	0.23	0	1	990
Household Asset Index	0.03	0.08	-0.4	0.16	990
Yao tribe	0.39	0.49	0	1.00	990
Chewa tribe	0.20	0.40	0	1	990
Lomwe tribe	0.26	0.44	0	1	990
Other tribe	0.18	0.38	0	1	990
Mother has completed primary education	0.16	0.37	0.0	1.00	990
Father has completed primary education	0.34	0.47	0.0	1.00	990
Class in wave 2	7.33	1.30	4.0	9.00	990
Class in wave 3	8.20	1.37	4.0	10.00	990
Ever repeated a standard (Standard 1-2)	0.37	0.48	0.0	1.00	978
Age at school start	6.65	1.80	0.0	12.00	990
Late school entry (>8 years of age)	0.25	0.44	0.0	1.00	990
Endline english score (wave 3)	1.54	0.96	0.0	5.00	958
Baseline english score (wave 2)	1.59	0.97	0.0	5.00	990
Endline math score (wave 3)	7.05	1.50	1.0	10.00	958
Baseline math score (wave 2)	7.56	1.66	0.0	11.00	990
Baseline math score (wave 1)	4.52	0.96	0.0	6.00	985
Endline Chichewa score (wave 3)	4.54	0.87	0.0	6.00	958
Baseline Chichew score (wave 2)	4.04	1.07	0.0	6.00	990
Can read propperly (wave 1)	3.42	1.08	0.0	4.00	985

Notes: The number of observations used to calculate the summary statistics in Panel A corresponds to the sample used in the main regressions for grade progression. The number of observations used to calculate the summary statistics in Panel B corresponds to the sample used in the main regressions for test scores.Baseline scores are collected at the beginning of the 2007 (wave 1) and 2008 school-year (wave 2). Endline scores were collected at the end of the 2009 school-year (wave 3). Students' asset index is obtained using principal component analysis of various binary variables indicating the ownership of durable assets by the household.

compared to traditional face-to-face interviews. This is especially important in contexts where reporting violence, particularly by teachers, may be stigmatized or carry social costs.

Third, in wave 2 and 3, the MSAS also collected students' test scores in mathematics, Chichewa and English as well as detailed information about their teachers. The students' teachers completed detailed ACASI questionnaires that gathered data on their backgrounds, employment histories, teaching methods, and participation in professional training programs. This enables me to create student-teacher matched pairs, a critical component for estimating teacher value-added models and a unique feature within datasets focused on studying school violence.

3.3.1.1 Education outcomes

I measure human capital formation at the extensive and intensive margin using student data on annual grade progression and standardized test scores in math, Chichewa and English. Depending on the level of outcome, I use two distinct sub-samples of the MSAS. For the empirical analysis of grade progression, I include all baseline students enrolled in school during the first survey wave for whom educational history data is available. This results in a panel of 1,705 students, comprising 874 male and 861 female students.

I operationalize grade progression as follows: In waves 1 to 4 of the survey, students report whether they were promoted to the next grade, missed the previous school year, dropped out, or repeated a grade. In wave 1, students also provided a detailed history of their educational progression, documenting each grade they had attended, as well as instances of grade repetition or missed years. This allows me to construct a longitudinal panel that tracks students' grade progression from primary school (grades 1–8) through secondary school (grades 9–12). To quantify successful grade progression, I create a binary variable as my main outcome, assigning a value of 1 if a student progresses to the next grade and 0 if the student either repeats the grade or drops out of school. This choice is motivated by the need to address sample selection concerns. Teacher violence may have both immediate and cumulative effects, influencing two distinct groups: students who drop out quickly and those who remain in school but face grade repetition before eventually exiting. By combining these outcomes into a single measure of grade progression (1 if successful, 0 unsuccessful), I account for both dimensions of the impact.[4] This approach ensures that my estimates provide a more comprehensive view of how teacher violence affects students' grade progression over time, capturing any disruptions in their educational trajectory.

[4]To ensure accurate classification, I assign both dropouts and students who repeat grades a value of 0, as these students are not successfully progressing through the education system. This approach avoids comparing dropouts (1 if yes) with students who remain enrolled but are delayed in their grade progression (0 if no dropout). However, in Appendix C.2 (Figure C.2), the effects are displayed for dropouts using this classification.

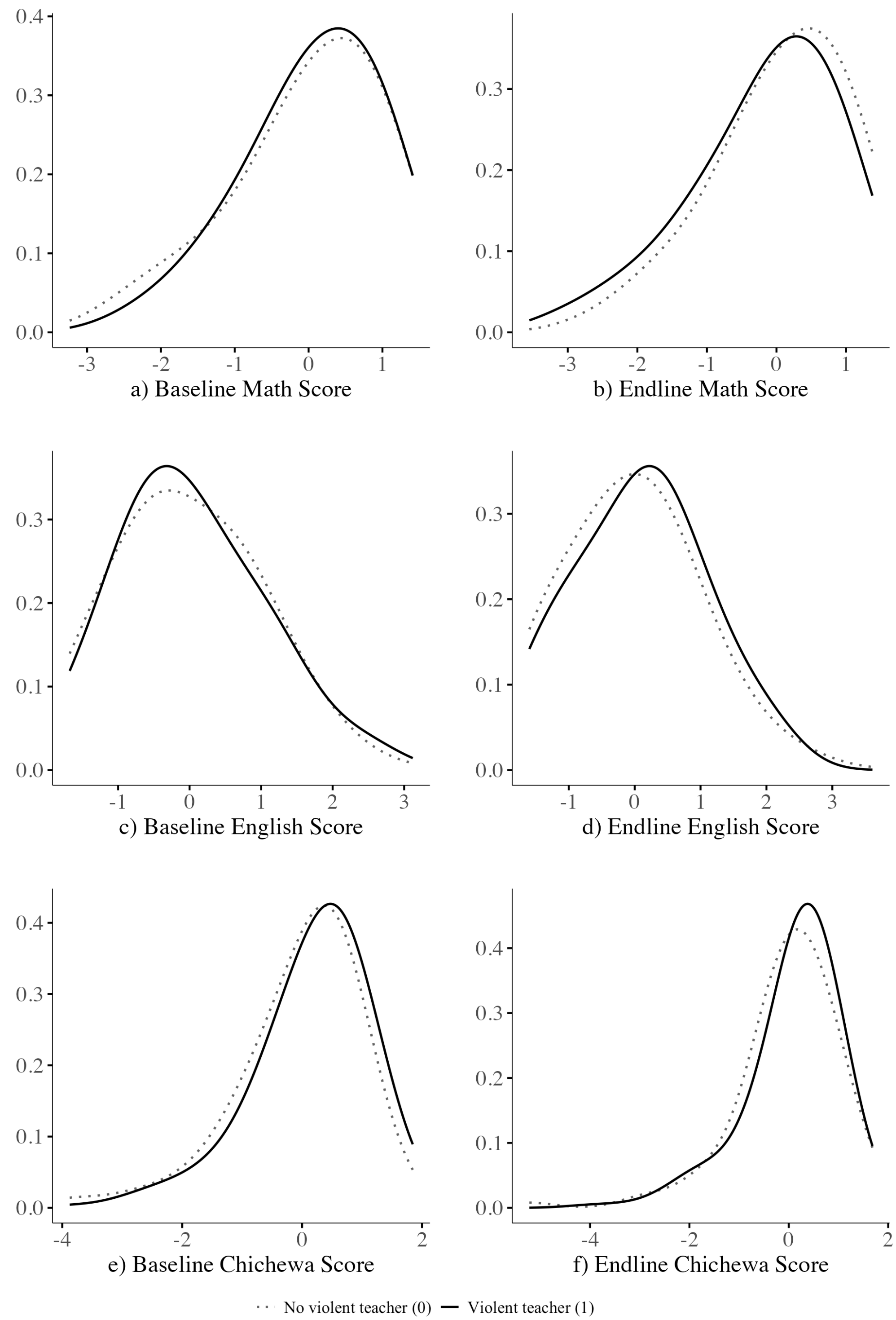

Fig 3.3.1: Distribution of test scores. Standardized math, Chichewa and English scores are plotted by experience of teacher violence (1 if experienced violence in the past academic year). Baseline scores are measured at the end of wave 2. Endline scores are measured at the end of the academic year in 2009 (wave 3). Both scores were measured for all children who remained in school in the academic year of 2009

At the intensive margin, I restrict the sample to students being enrolled in school in both waves for which with test score data is available, resulting in 990 observations. The MSAS designed these test assessments with contextual relevance to Malawi (Psaki et al., 2017). At the end of each school year in 2008 (wave 2) and 2009 (wave 3), students took tests in mathematics, as well as in English, and Chichewa. Chichewa is the official, primary language of instruction from grades 1 to 5. In the assessments of Chichewa and English, students were given one minute to read a short passage aloud, those who read at least the first three sentences were then asked six comprehension questions. In wave 3, new stories of similar length and complexity were used. Reading comprehension scores ranged from 0 (if the student failed to read the first three sentences or answered all questions incorrectly) to 6 (if they answered all six questions correctly). Mathematics scores were derived from twelve questions based on the Malawi Institute of Education's standard 3 achievement tests. These included problems on number ordering, arithmetic (addition, subtraction, multiplication, and division), and two word problems. The math test scores ranged from 0 to 12.

My primary outcome variables of interest at the intensive margin are the math and reading test scores of students, measured by the number of correct answers. All test scores were standardized to have a mean of 0 and a standard deviation of 1. Figure 3.3.1 presents the distribution of standardized test scores for students with violent and non-violent teachers (which is measured at wave 3, see section 3.1.2), both at baseline and endline. At the endline, I find a statistically significant gap in mean math scores, whereas no significant differences are observed in English and Chichewa scores. A Kolmogorov-Smirnov test reveals that the distributions for math are significantly different (p-value < 0.01). Students with violent teachers perform worse on math tests at the endline, with a mean gap of 0.19 standard deviations, compared to a smaller and insignificant gap of 0.08 standard deviations at baseline. No statistically significant differences are found in the mean English and Chichewa scores at either baseline or endline.

3.3.1.2 Measuring teacher violence

The main variable capturing teacher violence is generated using information from the students' ACASI interviews. During waves 1–4, adolescents were asked to report their experiences with

Table 3.3.2: Students' exposure to teacher violence

	Never (0)	Once or Twice (1)	A few times (2)	Many times (3)	Almost every day (4)	Any (1-4)	N
Wave 1							
Physical Violence	79.70	11.80	5.20	2.30	1.10	20.30	1725
Emotional Violence	85.40	5.60	3.10	4.20	1.70	14.60	1725
Sexual Violence	92.60	2.10	1.30	0.60	0.80	4.80	1725
Wave 2							
Physical Violence	86.10	8.10	4.30	1.00	0.50	13.90	1725
Emotional Violence	88.20	4.30	3.50	3.40	0.60	11.80	1725
Sexual Violence	98.70	0.50	0.40	0.20	0.20	1.30	1725
Wave 3							
Physical Violence	91.90	4.60	2.20	0.90	0.40	8.10	1725
Emotional Violence	89.30	4.10	2.70	2.80	1.00	10.70	1725
Sexual Violence	98.60	0.70	0.60	0.10	0.10	1.40	1725
Ever victim of violence (waves 1-3)							
Physical Violence	64.80	20.33	11.20	3.49	0.90	35.20	1725
Emotional Violence	68.40	11.62	7.71	8.63	2.20	31.60	1725
Sexual Violence	91.10	2.73	1.90	0.74	0.9	7.90	1725

Notes: The cells represent row percentages of reported experiences of violence, as indicated by students through ACASI interviews conducted during waves 1–3. Column 7 shows the percentage of students reporting any experience of violence, regardless of intensity (1–4), representing the primary treatment variable from the main analysis. Raw percentages sum to 100. The sample consists of all children enrolled in school during wave 1.

teacher violence during the previous school year[5]. For the analysis, I create a binary variable that captures any instance of violence, regardless of frequency and type of violence. This represents my main "treatment" variable (see section 4.1) and I provide robustness check on alternative specifications. Physical violence is measured as instances in which a student reports being hit, beaten or punched by a teacher at least once during the previous school year. Emotional violence is characterized by instances of a teacher making sexual comments or shouting at the students during the school year. Sexual violence is captured by instances in which a student reports being inappropriately touched or pinched by a teacher. Table 3.3.2 presents the frequency of incidents where students experienced violence in schools, along with the binary variables that were calculated based on the raw figures. Similar to the findings of Ameli (2017), I find that approximately 36% of Malawian students experience physical violence in school, 31.6% experience emotional violence, and around 8% experience sexual violence. The prevalence of

[5]The exact survey questions read as follows: *Physical violence:* "In this school year, how often have teachers punched, slapped or whipped you at school?" With possible answers options, 0=Never, 1=Once or Twice, 2=A few times, 3=Many Times, 4=Almost everyday. *Emotional Violence:* In this school year, how often have teachers made sexual comments or shout at you at school? *Sexual Violence:* "In this school year, how often have teachers touched or pinched your breasts, buttocks or genitalia at school?"

Table 3.3.3: Summary statistics: Teacher level characteristics

Variable	Teacher (wave 2) Mean	Teacher (wave 2) SD	Teacher (wave 3) Mean	Teacher (wave 3) SD
Year of Birth	1970	8.81	1971	8.89
Female	0.21	0.41	0.20	0.40
Number of Children	3.10	2.05	3.36	6.19
District of Balaka	0.48	0.50	0.47	0.50
Tribe: Chewa	0.13	0.34	0.14	0.35
Religion: Catholic	0.35	0.48	0.33	0.47
Prayers daily	0.60	0.49	0.63	0.48
Asset index	0.00	1.89	0.00	1.94
Housing provided by Ministry of Education	0.40	0.49	0.43	0.50
Head teacher	0.28	0.45	0.33	0.47
Travel distance to school (in minutes)	18.15	20.96	18.92	31.92
Teaching experience	4.66	4.34	4.94	4.67
Permanent contract	0.88	0.32	0.90	0.31
Ever received Job Promotion	0.15	0.36	0.09	0.29
Speaks English	0.90	0.30	0.93	0.25
Uses physical punishment to discipline	0.15	0.36	0.12	0.33
Received training in gender-based violence	0.41	0.49	0.35	0.48
Received training in Safe School Toolkit	0.18	0.39	0.08	0.28

Notes: Teacher variables are obtained from the teacher questionnaires. The number of observations is n=275 in wave 2 and n=252 in wave 3.

violence decreases with increased age, suggesting that younger students are particularly exposed to teacher violence.

3.3.2 Correlates of teacher violence

In wave 2 and 3 of the MSAS, I obtain detailed information about a child's teacher. Summary statistics of these teacher variables are presented in Table 3.3.3. The teacher information is linked to the students data to create student-teacher matched pairs. To explore the factors associated with teacher violence, I regress the dependent variable, teacher violence, on various observable teacher characteristics. These characteristics come from the teacher questionnaires of wave 2 and 3. I use two alternative dependent variables for this analysis. The first is the student-reported indicator of violence exposure from above, which is coded as 1 if any student of a particular teacher reports experiencing violence. Since each teacher is associated with multiple students, this variable aggregates reports from all students in the class. On average, 55% of teachers have used physical violence on at least one student in wave 2, and 44% in wave 3. The second outcome is based on a teacher-reported survey question from the teacher survey asking whether the teacher uses physical violence as a disciplinary method during class, which is coded as 1 if the teacher affirms using physical violence.[6] On average, this variable is smaller that the ones from the student questionnaire, with 15% of teachers in wave 2 and 12% in wave 3 reporting using physical punishment in class.

The estimates are presented in Table 3.3.4. Columns (1) and (2) use the student-reported indicator of violence exposure as the dependent variable, while in columns (3) and (4), I use the teacher reported variable. The results from columns (1) and (2) reveal no statistically significant associations between teachers' use of violence and observable characteristics such as ethnicity, religious practices, having children, socioeconomic status, years of teaching experience, employment status (permanent vs. temporary contract), English proficiency, or the distance teachers travel to school. In wave 2, older teachers and those who had recently received a promotion were less likely to engage in any form of violence against students, while head teachers were more likely to commit violence. Yet, these results do not hold for the sample of teachers from wave 3, where younger teachers as well as female teachers were less likely to commit violence. This suggests that the patterns related to teacher's characteristics may not reflect a general trend but are rather arbitrary artefacts of the data.

Likewise, I do not find any significant correlations between teacher characteristics and the use of physical violence in class for the teacher-reported outcome variables, except for two measures related to teacher training, i.e. teachers who had received training on the "Safe School Toolkit" or on "Gender-Based Violence" were less likely to report using physical violence as a disciplinary method. Broadly, these results are in line with other studies finding a positive correlation between teacher's use of violence in class and inadequate training that may lead to teachers lacking knowledge about non-violent discipline methods (Scharpf et al., 2021; Nkuba et al., 2018).

Overall, the evidence suggests that teacher violence is not randomly distributed, and the factors associated with it are likely correlated with teacher ability, which may confound the relationship between teacher violence and student learning outcomes. Therefore, it is important to account for teacher characteristics when identifying the effect of teacher violence on students' learning. This motivates the use of teacher fixed effects in our main models of test scores. Since primary schools in Malawi typically have a single teacher per class, the fixed effects capture any

[6] Unfortunately, the teacher survey does not include questions on emotional or sexual violence.

Table 3.3.4: Correlates of teachers' use of violence

	Teacher's Use of Violence			
	Student reported (wave 2)	Student reported (wave 3)	Teacher reported (wave 2)	Teacher reported (wave 3)
Year of Birth	-0.007*	0.003*	0.004	0.005
	(0.0009)	(0.0004)	(0.002)	(0.006)
Female Teacher	-0.093	-0.131*	-0.002	-0.055
	(0.045)	(0.015)	(0.008)	(0.018)
Number of Children	-0.023	-0.003	-0.017	-0.003
	(0.004)	(0.003)	(0.007)	(0.0006)
Housing by Ministry	-0.046	0.037	0.051	-0.003
	(0.037)	(0.037)	(0.030)	(0.160)
Chewa Ethnicity	-0.145	0.113	0.014	-0.002
	(0.100)	(0.109)	(0.061)	(0.138)
Daily Prayers	0.050	0.0006	-0.074	0.047
	(0.047)	(0.090)	(0.114)	(0.045)
Teacher Asset Index	-0.005	0.005	0.010	0.003
	(0.005)	(0.013)	(0.004)	(0.007)
Head Teacher	0.136**	0.044	0.018	0.002
	(0.007)	(0.086)	(0.070)	(0.081)
Teaching Experience	0.012	0.0001	0.002	-0.0006
	(0.004)	(0.004)	(0.002)	(0.005)
Permanent Position	0.217	0.194	0.002	0.111
	(0.187)	(0.088)	(0.111)	(0.030)
Proper English Usage	0.012	0.019	-0.010	-0.033
	(0.020)	(0.075)	(0.070)	(0.052)
Gender-Based Violence Training	0.022	-0.119	-0.066	-0.075**
	(0.098)	(0.045)	(0.026)	(0.001)
Save Schools Training	0.024	-0.108	-0.113*	0.068
	(0.148)	(0.150)	(0.014)	(0.094)
Time of commute (in min.)	-0.001	0.0002	0.0009	0.0005
	(0.001)	(0.0006)	(0.0005)	(0.0003)
Ever Received Job Promotion	-0.311*	-0.032	-0.015	0.065
	(0.047)	(0.054)	(0.025)	(0.011)
Observations	275	252	275	252
R^2	0.13	0.07	0.08	0.05
Within R^2	0.13	0.07	0.07	0.05
Balaka fixed effects	✓	✓	✓	✓

Notes: Teacher violence measures are regressed on various teacher characteristics. Student reported outcomes are dummy variables coming from the students' questionnaire and measure whether the teacher has used any form of violence during the prior school year on any student (1 if yes). Balaka district dummies are controlled. Robust standard errors in parentheses. Significance levels are indicated as $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

relevant observable and unobservable time-invariant teacher characteristics and also subsume broader school or class level inputs.

3.4 Identification strategy

This paper aims to estimate the causal effect of teacher violence on children's grade progression and test scores. To identify the effects of interest, I employ two main methodological approaches. First, I estimate the effect of teacher violence on grade progression using a difference-in-differences approach with multiple periods and groups. Second, for the subsample of children who are still in school in wave 3 and with available test scores data and teacher information, I estimate the impact of teacher violence on test scores using a teacher value-added model.

3.4.1 Grade progression

The first objective of this paper is to estimate the causal effect of teacher violence on grade progression. A simple correlation between teacher violence and successful grade progression may suffer from several endogeneity issues, preventing a credible causal interpretation. For instance, students with lower academic performance may be more likely to experience violence from teachers who use punitive disciplinary practices while these students may also be more likely to repeat a grade, which could confound the relationship between teacher violence and grade repetition. To obtain estimates that can be more credibly interpreted as causal, I exploit the panel structure of the data and compare trends in education outcomes between victims and non-victims of teacher violence in a difference-in-differences design. The panel nature of the data allows me to account for any invariant students characteristics such as cognitive ability or prior academic achievement and any time invariant effects, such as trends in grade progression with increasing age, that both mitigate the most central concerns about omitted variables bias and reverse causality.

For the empirical analysis, I create a binary variable for each student and each grade, that takes the value of 1 if the student has successfully promoted to the next grade (i.e., the student has not missed the year or repeated this particular grade), and a value of 0 indicating that the student has either repeated the grade or missed the past year of schooling. I use this variable to analyze students' grade progression over time, capturing any disruptions in their educational trajectory from primary through secondary education, in a difference-in-differences design. In its traditional form, difference-in-differences estimators compare two groups and two time periods, e.g. one group is the group of students that were victims of violence by their teacher and the other group is the ”untreated” group throughout, who did not experience violence by their teachers. However, in my context, the empirical setting is more complex. Instead of two time periods, the data allows me to explore multiple time periods and treatment to be staggered across individuals, with students becoming victim of violence at different times, i.e. at different ages and grades. In such cases, recent methodological contributions have raised concerns about the reliability of the traditional two-way fixed effects (TWFE) approach to estimate treatment effects, as the TWFE can yield biased estimates when treatment effects are heterogeneous across groups or over time (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021). To address these challenges, I use the estimator by Callaway & Sant'Anna (2021) (hereafter CSDID) that provides a more reliable basis for causal inference in the presence of staggered treatment and heterogeneous effects.

The main treatment variable is generated as follows: During waves 1–4, adolescents were asked to report their experiences with teacher violence during the previous school year. I assign treatment as any instance in which a student reports being victim of either physical, emotional, or sexual violence during that school year and I record the grade at which the incident occurred. Robustness tests using alternative treatment definitions for each types of violence are provided in the appendix. Once a student is classified as treated, they remain in the treated group for the duration of the entire survey period which spans up to the time of the last survey in 2011 (wave 5). This allows me to examine whether exposure to teacher violence not only has an immediate effect but also a sustained impact on their grade progression.[7]

I provide point estimates for the effect of each year following the first violent incidence. This allows me to study the effects depending on the length of exposure. In other words, I observe potential adjustment patterns in the point estimates over time. To achieve this, I estimate the following dynamic event study model

$$\pi_{it} = \alpha_i + \alpha_t + \sum_{k=-C}^{C} \delta_k \cdot D_{it}^k + \epsilon_{it} \quad \text{where } C < 0 \text{ and } C > 0 \tag{3.12}$$

[7] For this approach, I assume that children who did not report being victims of teacher violence were unaffected by it (SUTVA). This is a strong assumption, as it overlooks potential spillover effects; for example, children who witness violence against their peers may still be affected by the resulting environment of fear and insecurity. Such an environment could influence their cognitive development, potentially biasing the estimates (Carrell et al., 2018). However, this bias would likely attenuate the results toward zero, suggesting that the estimates presented below represent a lower bound of the true effect of teacher violence on children's outcomes.

where ${D^k}_{it}$ is a binary variable that takes the value of 1 for unit i k periods before or after treatment and 0 otherwise. The coefficients δ_k represent the estimated effects of teacher violence at different time periods relative to the event, covering an event window of $[C = -6; C = 6]$. α_i and α_t are individual fixed and time fixed effects. I estimate three coefficients: first, the yearly pre- and post-teacher violence point estimates. These estimates allow me to study how the treatment effect changes over time and allow me to visually inspect whether pre trends in outcomes are parallel. Second, to ease interpretation, I aggregate all average treatment effects on the treated (ATTs) and estimate an overall effect over the entire survey period. Third, I estimate individual ATT parameters by grade (of first exposure to teacher violence).

3.4.2 Test scores

The second objective of this paper is to estimate the causal effect of teacher violence on test scores for those students, who remain in school during wave 2 and 3 and with test score data available. Math, English and Chichewa scores are analyzed separately. There are three possible threats to identification. First, as in any analysis of how teachers or teachers' characteristics affect student outcomes, one concern is the non-random sorting of students and teachers into particular schools and classrooms (Bau & Das, 2020). Second, teachers' background may correlate with other teacher or classroom characteristics that influence test scores, such as teachers' ability. Third, students' abilities may simultaneously influence both teachers' use of violence and student outcomes, thereby confounding the relationship between violent teachers and test scores. I account for these concerns by using the endline (wave 3) score as the dependent variable, while controlling for the baseline (wave 2) score and teacher fixed effects within a value-added framework, similar to previous studies (Rakshit & Sahoo, 2023). While the teacher fixed effects captures all observable and unobservable time-invariant teacher characteristics, the lagged test score is supposed to capture the effect of all previous inputs and unobservable shocks relevant to the education production (Todd & Wolpin, 2007). Note that the value-added model, by including the baseline test score, also controls for individual-level variation in the quality of students and thus alleviates the concern of reverse causality where teachers may endogenously use violence by observing the performance of the students. In a robustness test in section 5.2.1, I additionally show that sorting is less of an issue. Thus, I estimate the following specification that is estimated by dynamic OLS:

$$Y_{igjt} = \beta_0 + \beta_1 Y_{it-1} + \beta_2 TV_{it} + \beta_2 X_{it} + T_j + \gamma_g + \epsilon igjt \tag{3.13}$$

Y_{igjt} is a test score for student i, in grade g, with teacher j, in year t. Y_{it-1} is a vector of prior-year ("lagged") test score in the same subject. In a robustness check, I allow this vector for expansions to include lagged scores extending further back than 1 year, and also include lagged scores in the other subjects. TV_{it} is the treatment indicator of teacher violence. X_{it} is a vector of student characteristics, such as orphan status, late school entry, gender, age, and an household asset index. γ_g is a grade fixed effect and T_j is a teacher fixed effect. $\epsilon igjt$ is the idiosyncratic error term. This value-added approach, isolates the effect of teacher violence by examining within-student and within-teacher variation.

3.5 Results

3.5.1 Grade progression

3.5.1.1 Event study type plots

Fig. 3.5.1 presents event-study type plots from the dynamic CSDID estimator. The figure shows that students, who experienced violent teacher are less likely to progress to the next grade. Even one year after experiencing violence, students are 0.12pp less likely to be promoted to the next grade. Importantly, the estimates are consistent with the parallel trends assumption as the coefficients on time periods (grades) prior to the first experience of teacher violence are all close to zero and exhibit no discernible pre-trends. The figure also sheds light on the dynamics of the treatment effects, showing that treatment effects increase in magnitude over time in the post treatment periods. The observed increase in treatment effects over time could likely be explained by the gradual nature of the effect: First, when a teacher engages in violent behavior, the immediate impact may prompt some students to drop out quickly, while others endure for a longer period before eventually leaving school. Additionally, teacher violence may lower test scores due to the aforementioned psychological stress-threat mechanism, which in turn increases the likelihood of grade repetition or later dropout (Deole, 2018). The effects on dropouts are presented in Appendix C.2. The effects are similar.

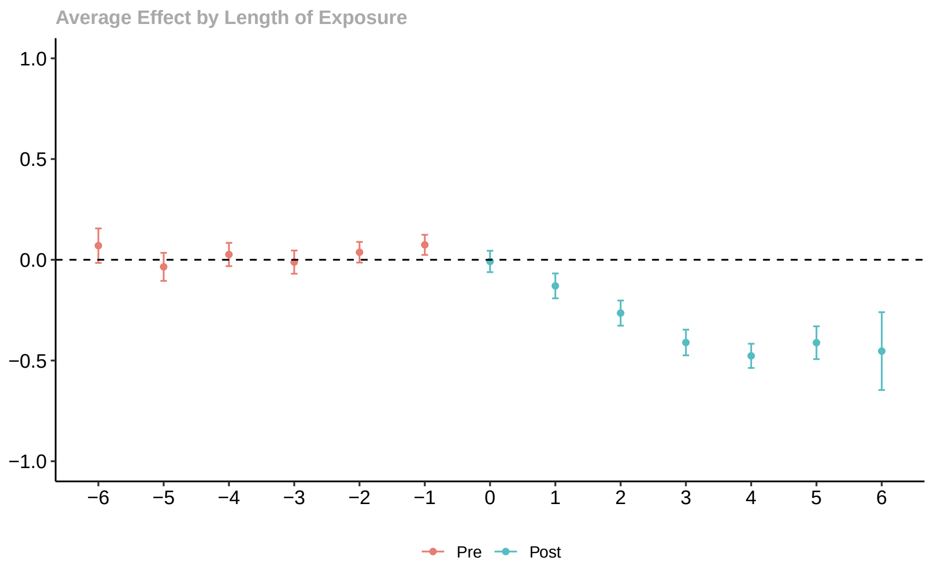

Fig 3.5.1: Event study plots on the effect of teacher violence on student's grade progression. The figure shows event study type plots using the dynamic version of the TWFE model by (Callaway & Sant'Anna, 2021). The outcome variable is grade progression (1 if promoted to the next grade, 0 otherwise). The time variable is the student's grade and the treatment group variable is given by the grade in which the student first experienced any type of teacher violence (physical, sexual and emotional) during the past school year. I estimate six post-periods since estimating more post-periods dramatically increases the standard errors in the post-period Borusyak et al. (2024). Similarly, the maximum number of pre-periods that can be estimated in my panel is six.

The cumulative effects of teacher violence become more pronounced over time, as it impacts both groups—those who drop out early and those who persist for a few years, repeat grades, before exiting. This interplay between students dropping out and those performing poorly in class, leading to grade repetition, underscores the rationale behind my choice of outcome variable: successful grade progression. By accounting for both dropout rates and grade repetition, I mitigate concerns about selection bias in treatment and control groups and ensure more accurate estimates of the true treatment effects.

3.5.1.2 Aggregated average treatment effects

Table 3.5.1 presents point estimates, standard errors and 95% confidence bands for the average ATTs of the effect of teacher violence on grade progression over the event study window. The overall ATT averages the average treatment effects across all lengths of exposure to the treatment. This parameter should be interpreted as a comprehensive summary effect of becoming a victim of violence on grade progression. It represents the average effect of being exposed to violence, taking

Table 3.5.1: Average treatment effects of teacher violence on grade progression

	Any Violence (1)	Physical (2)	Emotional (3)	Sexual (4)
ATT	-0.308***	-0.283***	-0.292***	-0.343***
Std. err.	0.018	0.024	0.026	0.043
95%-CI	[-0.344; -0.272]	[-0.330; -0.237]	[-0.343; -0.241]	[-0.427; -0.260]
N	1705	1705	1705	1705

Notes: The table reports overall summaries of the average treatment effects point estimates (ATT), clustered standard errors at the teachers' level and 95% confidence intervals in brackets for the effect of teacher violence on grade progression. Point estimates are based on dynamic event study aggregation. Estimates are based on the non-parametric DiD approach by Callaway and Sant'Anna (2021). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

into account all individuals who experienced the violence at any point in time. In this sense, it is conceptually similar to the ATT in the canonical TWFE model, where there are exactly two periods and two groups. The estimation is implemented based on Equation (3.12) under the parallel trend assumption. The results yield two main findings. First, on average, teacher violence significantly reduces grade progression by 30 percentage points over the event study window. This effect is both highly significant and substantial in size, highlighting the severe impact of teacher violence on students' grade progression in Malawi. Second, the negative effects are consistent across all types of violence, including physical (column 2), emotional (column 3), and sexual (column 4) violence. The persistence of these effects across different forms of violence underscores the broad and detrimental consequences of teacher-perpetrated abuse on students' academic trajectories.

3.5.1.3 Effects by first exposure and gender

Fig.C.3 provides estimates of grade-specific effects. The idea here is to look at average effects specific to each grade at which first exposure to violence occurred. To do so, I aggregate the group-time average treatment effects from CSDID into group-specific average treatment effects for each grade. Additionally, I divide the sample by gender to estimate the effects of first exposure to violence separately for female and male students. The figure plots point estimates of the effects and the corresponding bootstrapped standard errors. I find negative and statistically significant effects across all grade levels, indicating that teacher violence negatively impacts grade progression at each level of education. The average effect of being exposed to teacher violence is largest at grade level 10 for both male and female students, and also at grade level 9 for female students. This is likely due to the fact that the impact of teacher violence is more pronounced for secondary school children, as they face higher costs associated with schooling, such as school fees, and thus greater opportunity costs of schooling. This could make students more likely to drop out of school during this stage. Again, these results hold for all three measures of violence (see Appendix C.4 - C.6).

3.5.2 Test scores

I now turn to the effects of teacher violence on test scores. The results of the main regression analysis are presented in Table 3.5.2, which shows separate estimates for math (columns 1–2), English (columns 3–4), and Chichewa (columns 5–6) scores. Columns (1), (3), and (5) present results without student controls, while columns (2), (4), and (6) include the full specification from Equation (3.13). I find a significant positive effect of the baseline score in math, English, and Chichewa on students' respective endline scores, highlighting the importance of including lagged test scores in value-added models to account for prior inputs into the production function. Importantly, I find a significant decrease of 14.9 percentage points in standardized math scores for students who reported being victim of teacher violence in the academic year 2008-2009. The effects on English and Chichewa scores are statistically insignificant. The negative impact on math scores remains consistent across different model specifications but varies depending on the type of violence (Table A6). Specifically, the effect in Table A6 is significant for sexual and emotional violence, suggesting that these forms of abuse may have a more profound and lasting impact on cognitive and academic outcomes compared to physical violence. Again, I do not find an effect on English or Chichewa.

These findings suggest that teacher violence not only reduces grade progression but also lowers academic performance for those who remain in school, particularly in math. To put the effect size in broader perspective, I compare it with the distribution of effect sizes found in various educational interventions in the literature (Rakshit & Sahoo, 2023). Research by Kraft (2020) for high-income countries and by Evans & Mendez Acosta (2021) for low- and middle-income countries show that the median effect size for math outcomes in educational interventions is 0.07 standard deviations. In comparison, my estimated effect of teacher violence on math outcome is higher than the median effect size reported in these papers synthesizing the existing evidence. This underscores the substantial impact of teacher violence on student performance and highlights

Table 3.5.2: The effect of teacher violence on test scores

	Endline Test Score (Math)		Endline Test Score (Chichewa)		Endline Test Score (English)	
	(1)	(2)	(3)	(4)	(5)	(6)
Teacher Violence	-0.119*	-0.149**	-0.041	-0.005	0.063	0.011
	(0.059)	(0.061)	(0.065)	(0.064)	(0.064)	(0.064)
Baseline Math Score (wave 2)	0.438***	0.427***				
	(0.033)	(0.032)				
Baseline Chichewa Score (wave 2)			0.380***	0.384***		
			(0.051)	(0.051)		
Baseline English Score (wave 2)					0.361***	0.324***
					(0.041)	(0.041)
Observations	990	990	990	990	990	990
R^2	0.4123	0.4215	0.3008	0.3160	0.3143	0.3529
Within R^2	0.1782	0.1911	0.1303	0.1494	0.1199	0.1694
Grade fixed effects	✓	✓	✓	✓	✓	✓
Teacher fixed effects	✓	✓	✓	✓	✓	✓
Student controls		✓		✓		✓

Notes: VAM models estimated by dynamic OLS. Endline test scores in math, English and Chichewa are measured at the end of the academic year in 2009 (wave 3). Baseline scores are measured at the end of the academic year 2008 (wave 2). Both scores were measured for all children who remained in school in the academic year of 2009. Teacher violence is measured as any (=1) instance of violence, regardless of frequency and type during the past school year. Grade and teacher fixed effects are included in all estimations. Student controls include orphan status, late school entry, child's gender, age, and an household asset index. Class learning is calculated as the average test score gain of peers (fellow students in the same class under the same teacher in the respective subject) between waves 2 and 3. Class baseline test score are calculated as average peers test scores in the respective subject at baseline. In all estimations, standard errors are in parentheses and clustered at the school level. Significance levels are indicated as *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

the critical importance of addressing teacher violence as part of efforts to tackle the global learning crisis (World Bank, 2017).

3.5.2.1 Robustness analysis

In analyzing the causal effect of teacher violence on students' test scores, three potential threats to identification must be addressed. First, the estimated effect may suffer from omitted variable bias if there are unobserved characteristics correlated with both teacher violence and student outcome. Our model addresses this issue by including teacher fixed effects that control for all teacher level unobserved heterogeneity. Yet, the effect of teacher violence on test scores may still be biased if teacher characteristics have a differential effect on victims' and non-victims' learning outcomes and are not captured by the fixed effect. I address this concern by including a measure of class learning to allow for differential impact of teachers on the individual surveyed student and the rest of the class. Specifically, I estimate the full specification while incorporating a measure of class learning, calculated as the average test score gain of peers (fellow students in the same class under the same teacher) between waves 2 and 3, excluding the individual student surveyed. The results are presented in column (1) for math, column (4) for Chichewa, and column (7) for English of Table 3.5.3. Interestingly, class learning has a negative impact throughout, indicating that higher peer learning gains are associated with lower test scores for individual students. Importantly, the results for the estimated effects of teacher violence remain similar with my main analysis, with the effect sizes on math scores now estimated at 13 percentage points over the academic year. Additionally, there is no observable change in the estimated effect of the lagged test score, suggesting that the effect of class learning operates through mechanisms unrelated to the students' initial academic performance.

Second, another challenge arises from the potentially non-random sorting of students to teachers. For instance, better quality teachers may be assigned to teach better quality students, or parents of high-performing students may select schools that have better quality teachers. However, as discussed earlier, sorting is unlikely in our context for two key reasons. First, the average travel distance to schools for students in the two districts is 52 minutes, which significantly limits students' ability to choose schools freely. Second, most primary schools in Malawi typically have a single teacher per grade, making it implausible that, within schools, students sort themselves into classrooms with different teachers. To further support this claim, I provide evidence showing that including baseline class characteristics does not systematically change the results. I include average baseline test score of the class in order to account for peer quality at the class level (Kraft, 2019). Thus, this approach allows individual students and their peers to have different learning trajectories based on their baseline score. I find no significant effects of baseline class scores on individual endline test scores and I do not observe any changes in the estimated effects of individuals baseline scores. This reinforces the robustness of my preferred specification by showing that the estimated effect of teacher violence on student performance is not driven by peer effects or sorting at the class level. Controlling for teacher fixed effects along with these class controls are likely to alleviate the concern of sorting.

Third, there is a risk of miss-specification of the value-added model. If lagged test scores do not fully account for prior learning inputs, residual confounding may distort the results. As mentioned earlier, the flexible approach of value-added models allows me to include lagged test scores extending further back than one year. To implement this, I use a measure of math knowledge from wave 1 for columns (3) of Table 3.5.3.[8] Additionally, I include a reading score from wave 1, which indicates whether the child was able to read five comprehension-based sentences in the language of the interview (either English or Chichewa, depending on the main interview language of the student) in columns (6) and (9). By including these wave 1 baseline measures, the effect of the wave 2 baseline measure slightly decreases but remains highly significant. Importantly, the inclusion of these additional baselines does not substantially alter the estimated effect of teacher violence, reinforcing the robustness of the findings. I find an effect size of 16 percentage points, which is close to the original estimate.

Furthermore, I get to the same conclusion when I include prior exposure to violence in my analysis. Given the data is limited in the availability of test scores, which are only measured in waves 2 and 3, it is important to acknowledge the possibility that students might have already been exposed to violence before the academic year of 2009. And since teacher violence may take time to fully manifest in its effects, as shown in the analysis on grade progression, it is crucial to account for any earlier exposure to violence. To address this, I include a measure of prior exposure to violence from wave 2. This variable is included in Columns 1, 3, and 5 of Table 3.5.4. The effects remain similar with an estimated effect of teacher violence of 16 percentage points. Additionally, I incorporate wave 1 baseline test scores to account for t-2 lagged scores to follow the value-added idea of controlling for lagged inputs, when including t-1 violence exposure. Again, the effects remain similar in magnitude and significance (column 2 of Table 3.5.4)

3.6 Concluding remarks

This study evaluates the effect of teacher violence on student's human capital formation in the context of rural Malawi. Employing a staggered difference-in-differences approach and a value-added model, I show that teacher violence reduces grade progression by around 30 percentage points over the event study window. I also show that it reduces student's math performance of around 15 percentage points for those, who remain in school in the academic year of 2008-2009, which is higher than the median effect size found for various educational interventions in the literature. I find that the estimated effects are significant only for math test scores, as I am unable

[8] The wave 1 tests differ from the more comprehensive measures in waves 2 and 3. Math test in wave 1 consists of five basic arithmetic and subtraction questions.

Table 3.5.3: Robustness Analysis

	Endline Score (Math)			Endline Score (Chichewa)			Endline Score (English)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Teacher Violence	-0.13**	-0.15**	-0.16***	-0.008	-0.005	-0.01	0.03	0.01	-0.003
	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)
Baseline Math Score (wave 2)	0.43***	0.43***	0.37***						
	(0.03)	(0.03)	(0.03)						
Class learning Math (wave 3 - 2)	-0.05*								
	(0.03)								
Class baseline Math (wave 2)		0.03							
		(0.03)							
Baseline Math Score (wave 1)			0.18***						
			(0.03)						
Baseline Chichewa Score (wave 2)				0.37***	0.38***	0.32***			
				(0.05)	(0.05)	(0.04)			
Class learning Chichewa (wave 3 - 2)				-0.12***					
				(0.04)					
Class baseline Chichewa (wave 2)					0.07*				
					(0.04)				
Baseline Reading Score (wave 1)						0.24***			0.13***
						(0.04)			(0.03)
Baseline English Score (wave 2)							0.34***	0.32***	0.31***
							(0.04)	(0.04)	(0.04)
Class learning English (wave 3 - 2)							-0.09**		
							(0.04)		
Class baseline English (wave 2)								0.07	
								(0.04)	
Observations	958	990	990	958	990	985	958	990	985
R^2	0.40	0.42	0.44	0.32	0.31	0.35	0.35	0.35	0.36
Within R^2	0.19	0.19	0.21	0.15	0.14	0.19	0.17	0.16	0.18
Grade fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Teacher fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Student controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: VAM models estimated by dynamic OLS. Endline test scores in math, English and Chichewa are measured at the end of the academic year in 2009 (wave 3). Baseline scores are measured at the end of the academic year 2008 (wave 2) and 2007 (wave 1). Both scores were measured for all children who remained in school in the academic year of 2009. Teacher violence is measured as any (=1) instance of violence, regardless of frequency and type during the past school year. Grade and teacher fixed effects are included in all estimations. Student controls include orphan status, late school entry, child's gender, age, and an household asset index. In all estimations, standard errors are in parentheses and clustered at the school level. Significance levels are indicated as $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

to detect any effects on English and Chichewa scores. This is consistent with the literature showing a significant effect on cognitive skill formation of violence (Ammermueller, 2012).

A few limitations should be considered when interpreting the results. First, I can not measure the severity of the violence experienced by students with the data available, which limits my ability to analyze how varying levels of violence may differentially affect students' academic performance. Second, survey questions may have been interpreted differently by respondents. This variation in interpretation could systematically affect the reported prevalence and, potentially, the outcomes associated with teacher violence. Third, although I use information

Table 3.5.4: Lagged violence

	Endline Test Score (Math)		Endline Test Score (Chichewa)		Endline Test Score (English)	
	(1)	(2)	(3)	(4)	(5)	(6)
Teacher Violence (wave 3)	-0.1680**	-0.1756**	-0.0179		0.0253	0.0160
	(0.0688)	(0.0663)	(0.0625)		(0.0653)	(0.0655)
Teacher Violence (wave 2)	0.0861	0.0661	0.0774	0.0575	-0.0580	-0.0651
	(0.0673)	(0.0666)	(0.0615)	(0.0583)	(0.0607)	(0.0620)
Baseline Math Score (wave 2)	0.4303***	0.3733***				
	(0.0323)	(0.0319)				
Baseline Math Score (wave 1)		0.1766***				
		(0.0326)				
Baseline Chichewa Score (wave 2)			0.3868***	0.3129***		
			(0.0503)	(0.0426)		
Baseline Reading Score (wave 1)				0.2349***		0.1344***
				(0.0373)		(0.0280)
Baseline English Score (wave 2)					0.3293***	0.3094***
					(0.0412)	(0.0417)
Observations	990	990	990	985	990	985
R^2	0.42158	0.44269	0.31451	0.35252	0.35145	0.36312
Within R^2	0.19119	0.22071	0.14746	0.19362	0.16761	0.18232
Grade fixed effects	✓	✓	✓	✓	✓	✓
Teacher fixed effects	✓	✓	✓	✓	✓	✓
Student controls	✓	✓	✓	✓	✓	✓

Notes: VAM models estimated by dynamic OLS. Endline test scores in math, English and Chichewa are measured at the end of the academic year in 2009 (wave 3). Baseline scores are measured at the end of the academic year 2008 (wave 2) and 2007 (wave 1). All scores were measured for all children who remained in school in the academic year of 2009. Teacher violence is measured as any (=1) instance of violence, regardless of frequency and type during the past school year. Grade and teacher fixed effects are included in all estimations. Student controls include orphan status, late school entry, child's gender, age, and an household asset index. In all estimations, standard errors are in parentheses and clustered at the school level. Significance levels are indicated as *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

from the ACASI interviews, the reliance on self-reported data may still suffer from response bias. Students may under-report their experiences of violence due to, lack of awareness, social desirability biases or the fear of social costs. These limitations call for better data collection and validation of self-reported data with administrative or experimental data. This claim has also been made by previous studies. For instance, Evans et al. (2024) show that most countries lack data to answer simple questions that policymakers might ask to take action against school-related violence, to understand the consequences of violence, or to monitor progress on reducing violence.

Despite these limitations, the findings of this study provide first valuable insights into the relationship between teacher violence and human capital formation, and highlight areas for future research. First, the findings must be understood within the broader socio-cultural context of Malawi. Cultural norms related to violence and discipline shape adolescents' experiences and responses to violence. Further research, particularly that which can establish causal links of teacher violence on students' outcomes, is needed from different contexts to fully understand the long-term consequences of teacher violence. Second, while this study provides robust evidence of the detrimental effects of teacher violence on grade progression and academic performance, the long-term consequences of such violence, e.g. on labor market outcomes or transition into adulthood remain underexplored. Expanding this research to long term outcomes is crucial in understanding how teacher violence shapes young adults' economic trajectories. Third, teacher violence could have further, far-reaching implications on human capital development. Prior research suggests that experiences of violence can alter preferences and aspirations regarding education (Castillo, 2020; Callen et al., 2014). Aspirations, however, are crucial in shaping students' investment decisions in human capital (Ross, 2019). Future research should explore how teacher violence impacts students' preferences and attitudes toward education. Such studies could provide valuable insights into whether exposure to violence shifts students' long-term goals, diminishes their motivation to invest in education, or alters their perceptions of the benefits associated with schooling. These shifts could also play an important role in intergenerational conflicts on schooling decision (Bursztyn & Coffman, 2012) and violence-induced changes in preferences toward education could persist across generations, potentially creating an intergenerational poverty trap (Black et al., 2005).

Lastly, this paper offers three key suggestions for policymaking. First, my results underscore the importance of considering teacher violence as a factor when assessing teacher quality, as overlooking such behaviors may underestimate their detrimental impact on student outcomes (Rothstein, 2010; Azam & Kingdon, 2015). Second, while schools are generally seen as protective spaces, the evidence presented here suggests that violence within them can reinforce cycles of stress, diminish aspirations, and reduce academic performance, similar to other studies (Michaelsen & Salardi, 2020). Thus, they highlight the urgent need for interventions to reduce teacher violence, such as enhanced teacher training programs, stricter enforcement of anti-violence policies, and stronger mechanisms for reporting and accountability (Scharpf et al., 2021; Nkuba et al., 2018; Ortúzar et al., 2021; Baker-Henningham et al., 2021). Teacher violence, particularly in the form of disruptions of skills, may also have lasting consequences for future labor market outcomes (Gust et al., 2024). My findings call for a more detailed analysis on the potential effect of violence on long-term economic growth.

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C Appendix

C.1 Study sites

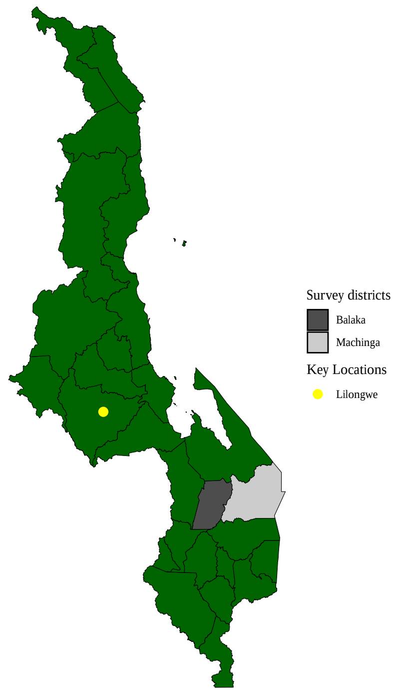

Fig C.1: Map of study sites. The figure displays the two districts of Malawi where the survey was conducted, Balaka and Machinga. Balaka covers an area of 2,140 km^2 and has a population of 438,379. Machinga covers an area of 3,771 km^2 and has a population of 369,614.

C.2 The effects on dropout

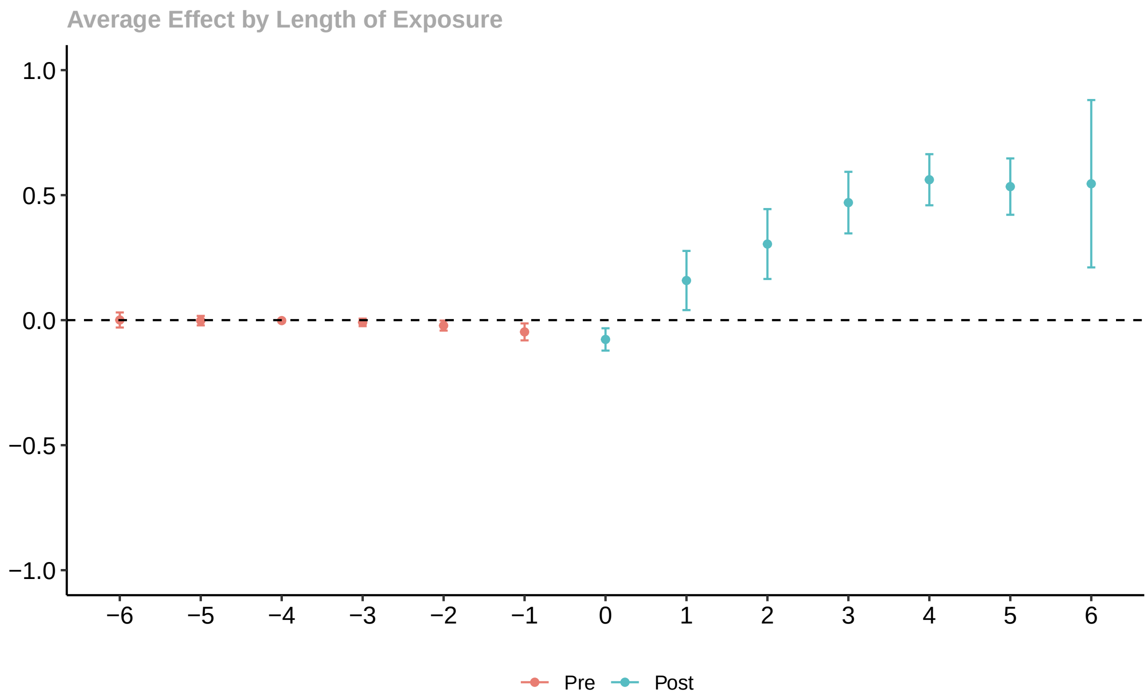

Fig C.2: Event study plots on dropout. The figure shows event study type plots using the dynamic version of the TWFE model by (Callaway & Sant'Anna, 2021) estimated using OLS. The outcome variable is school dropout (1 if dropped out of school). The time variable is the student's grade and the treatment group variable is given by the grade in which the student first experienced any type of teacher violence (physical, sexual and emotional) during the past year. I estimate six post-periods since estimating more post-periods dramatically increases the standard errors in the post-period (Borusyak et al., 2024). Similarly, the maximum number of pre-periods that can be estimated in my panel is six.

C.3 Teacher violence by class and gender

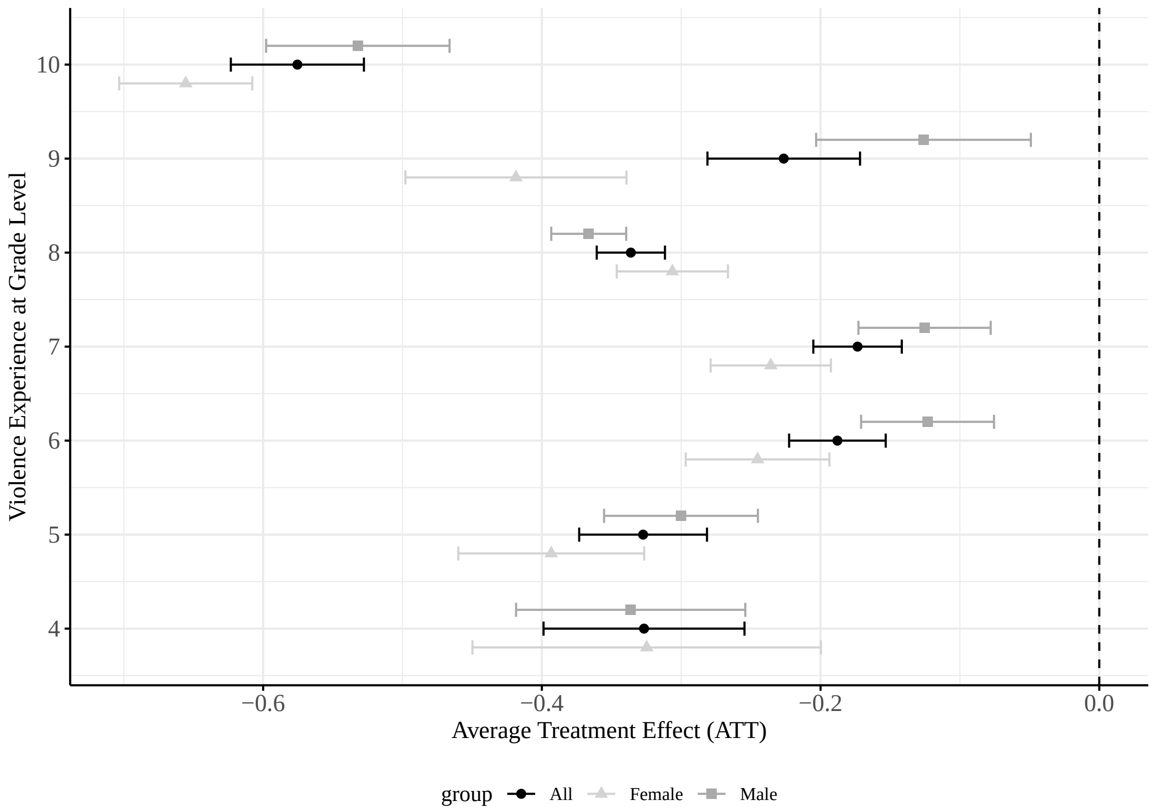

Fig C.3: Average treatment effects by grade and gender (first exposure). The figure shows the effects of violence by class/grade. The y-axis is categorized by grade. The x-axis provides estimates of the average effect of being victim of violence for units in each grade averaged across all time periods after that group becomes treated.

C.4 Teacher violence (physical violence) by class and gender

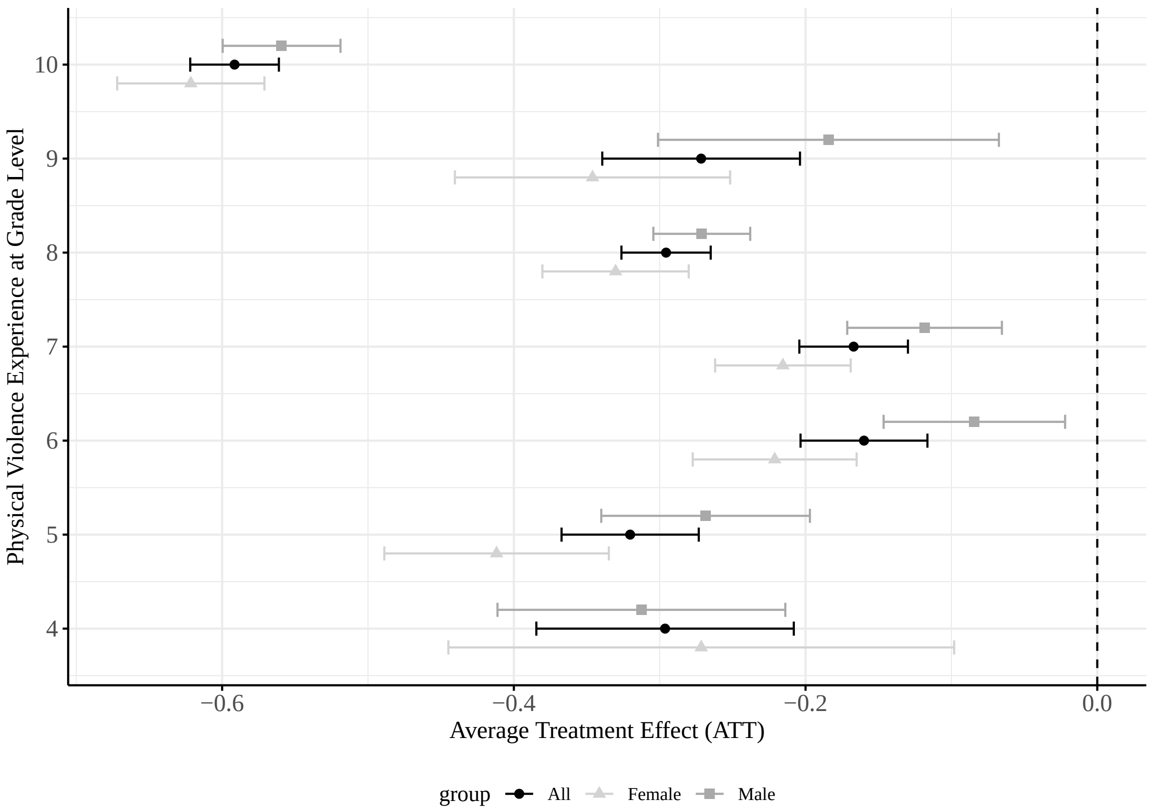

Fig C.4: Average treatment effects by grade and gender (physical violence). The figure shows the effects of violence by class/grade. The y-axis is categorized by grade. The x-axis provides estimates of the average effect of being victim of violence for units in each grade averaged across all time periods after that group becomes treated.

C.5 Teacher violence (emotional violence) by class and gender

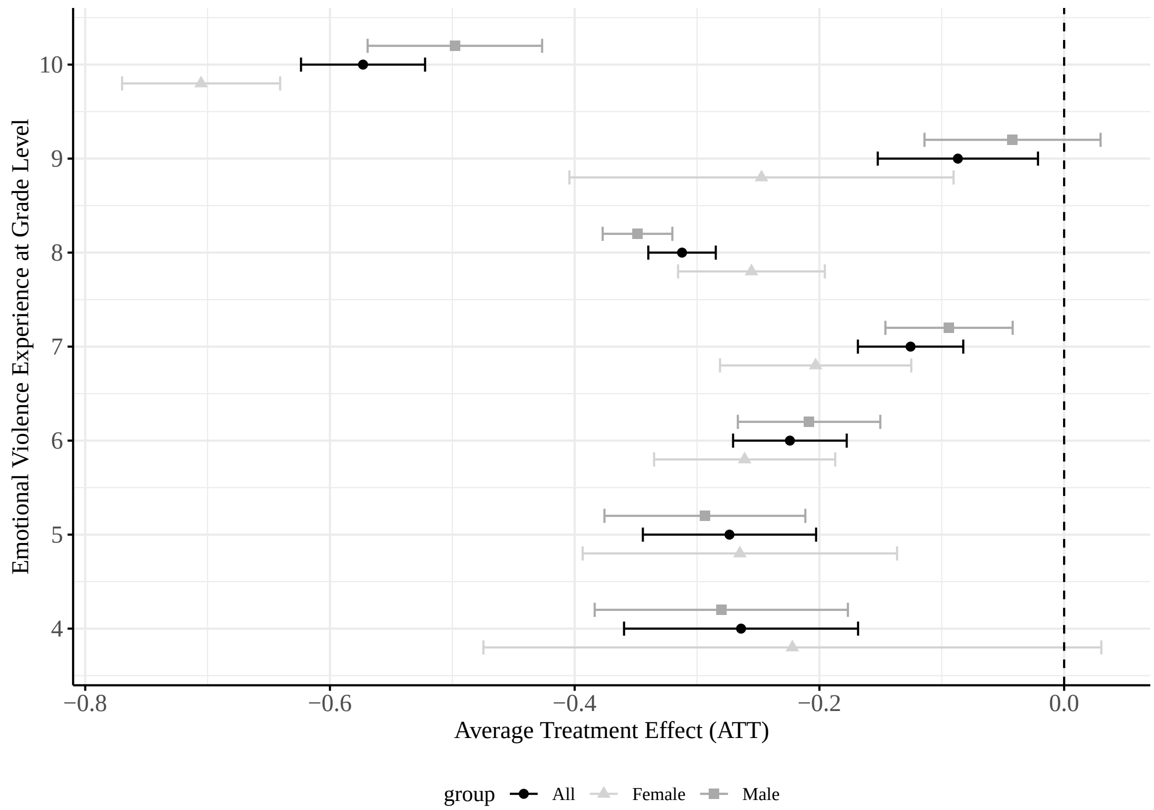

Fig C.5: Average treatment effects by grade and gender (emotional violence). The figure shows the effects of violence by class/grade. The y-axis is categorized by grade. The x-axis provides estimates of the average effect of being victim of violence for units in each grade averaged across all time periods after that group becomes treated.

C.6 Teacher violence (sexual violence)by class and gender

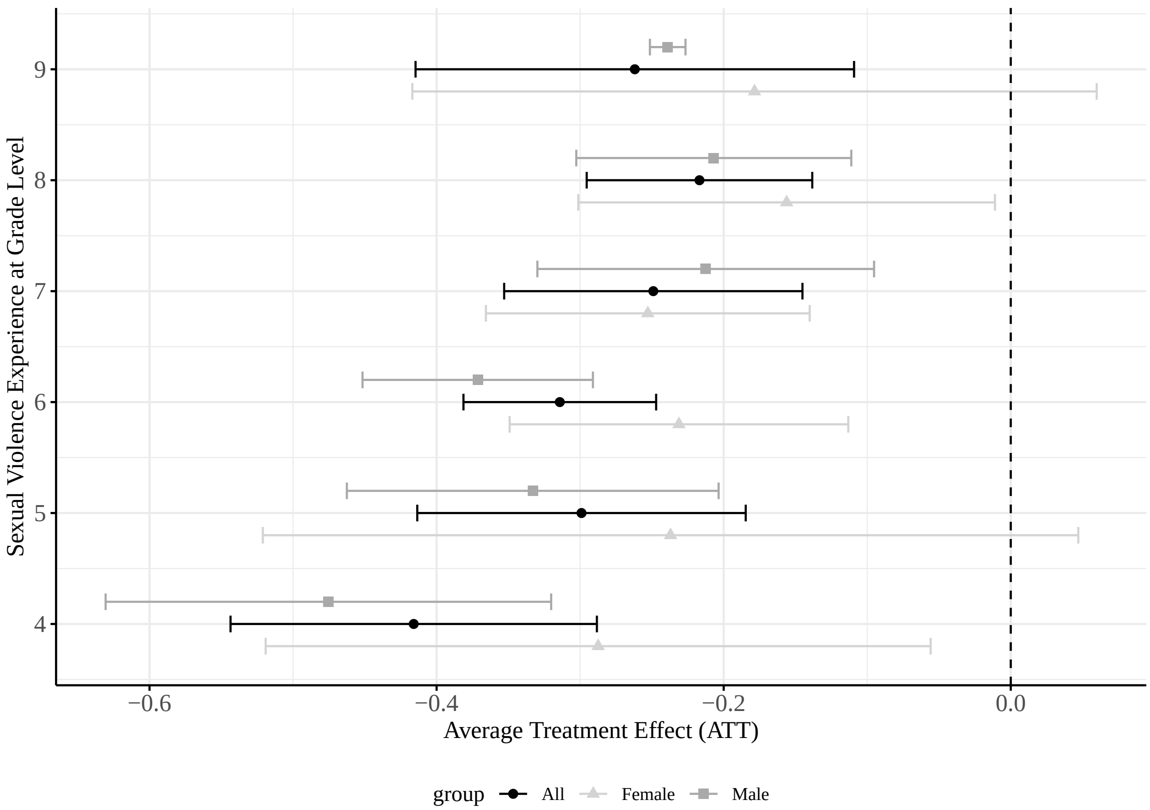

Fig C.6: Average treatment effects by grade and gender (sexual violence). The figure shows the effects of violence by class/grade. The y-axis is categorized by grade. The x-axis provides estimates of the average effect of being victim of violence for units in each grade averaged across all time periods after that group becomes treated.

C.7 Intensive margin: The effect by type of violence

Table A6: Effect by type of violence

	Endline Score (Math)			Endline Score (Chichewa)			Endline Score (English)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Physical Violence	-0.05			0.03			0.03		
	(0.08)			(0.07)			(0.09)		
Emotional Violence		-0.16**			-0.06			0.01	
		(0.08)			(0.07)			(0.08)	
Sexual Violence			-0.57***			0.03			-0.02
			(0.15)			(0.19)			(0.15)
Baseline Math Score (wave 2)	0.43***	0.43***	0.43***						
	(0.03)	(0.03)	(0.03)						
Baseline Chichewa Score (wave 2)				0.38***	0.38***	0.38***			
				(0.05)	(0.05)	(0.05)			
Baseline English Score (wave 2)							0.32***	0.32***	0.32***
							(0.04)	(0.04)	(0.04)
Observations	990	990	990	990	990	990	990	990	990
R^2	0.418	0.421	0.426	0.316	0.316	0.316	0.352	0.352	0.352
Within R^2	0.186	0.190	0.198	0.150	0.149	0.149	0.170	0.169	0.169
Grade fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Teacher fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Student controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: VAM models estimated by dynamic OLS. Endline test scores in math, English and Chichewa are measured at the end of the academic year in 2009 (wave 3). Baseline scores are measured at the end of the academic year 2008 (wave 2). All scores were measured for all children who remained in school in the academic year of 2009. Teacher violence is measured as any (=1) instance of violence, regardless of frequency for the three types of violence measured by the survey (physical, emotional, sexual) during the past school year. Grade and teacher fixed effects are included in all estimations. Student controls include orphan status, late school entry, child's gender, age, and an household asset index. In all estimations, standard errors are in parentheses and clustered at the school level. Significance levels are indicated as $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

AUTHOR’S CONTRIBUTION

The first chapter, ”Coffee Booms and Schooling: Evidence from Rwanda” is single-authored. I wrote this chapter independently and without using any resources other than those specified.

The second chapter, ”Genocide, Women’s Empowerment and Intergenerational Transmission of Violent Attitudes” is co-authored with Dr. Alina Greiner-Filsingner (Postdoctoral Researcher at the University of Mannheim). My individual contribution to this chapter amounts to 85%. Dr. Alina Greiner-Filsinger supported me in the conceptualization, the writing of section 2 of the manuscript and in reviewing and editing the manuscript.

The third chapter, ”Human Capital Costs of Violent Teachers” is single-authored. I wrote this chapter independently and without using any resources other than those specified.

Lastly, all the other parts of this dissertation are written independently and without using any resources other than those specified.

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