THE GRADUATE INSTITUTE GENEVA

INSTITUT DE HAUTES ÉTUDES INTERNATIONALES ET DU DÉVELOPPEMENT

GRADUATE INSTITUTE OF INTERNATIONAL AND DEVELOPMENT STUDIES

# Heterogeneous Impacts of Pro-Poor Policies and Programmes in Southern and Eastern Africa

**THESIS**

Submitted at the Graduate Institute
in fulfilment of the requirements of the
PhD degree in Development Economics

by

**Roxana Elena MANEA**

**Thesis N° 1382**

**Geneva**

**2021**

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Sur le préavis de M. Timothy SWANSON, professeur à l'Institut et co-directeur de thèse, de Mme Martina VIARENGO, professeur adjoint à l'Institut et co-directeur de thèse, de M. Yi HUANG, professeur adjoint à l'Institut et membre interne du jury, et de Mme Christelle DUMAS, professeur, département d'économie politique, Université de Fribourg, la directrice de l'Institut de hautes études internationales et du développement autorise l'impression de la présente thèse sans exprimer par là d'opinion sur son contenu.

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# Résumé/Abstract

Hétérogénéité des impacts des politiques et programmes contre la pauvreté en Afrique de l'Est et du Sud / Heterogeneous impacts of pro-poor policies and programmes in Southern and Eastern Africa

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In this dissertation, I estimate the impacts of three pro-poor interventions implemented in Southern and Eastern Africa. I assess the heterogeneity of these policies, their intended and unintended effects. In so doing, I compile evidence on their strengths and limitations. In Chapter 1, I assess the role of a subsidy programme that aims to improve access to formal housing in South Africa. Against the background of inequality being associated with higher crime rates, I show that housing subsidies have not only reduced inequality, but they also mitigated violent crimes. The impact on property crimes, however, is limited. Moreover, I evaluate the impact of school feeding programmes on education in Malawi in Chapter 2. I show that significant results are limited to food-insecure areas. In addition, I find that the effects of school meals are also limited to attracting children into school for the first time, while the impacts on dropout rates are less robust. Finally in Chapter 3, I estimate the impacts of the elimination of primary school fees in Tanzania. I compile evidence that the policy has increased enrolment rates, but it has had a limited impact on student retention. I show that the elimination of school fees has narrowed the gender gap in education, and it has especially benefited girls from districts with historically higher investments in school infrastructure. Overall, I argue that policy-makers must be aware of reform heterogeneities so as to exploit the strengths of educational policies and address their limitations by adopting complementary measures. Finally, I bring evidence that the impacts of pro-poor policies, such as housing subsidies, are not limited to individual gains, but they can also improve outcomes that are of importance to society as a whole, such as crime.

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Dans cette thèse, j'évalue les impacts de trois mécanismes d'intervention publique contre la pauvreté mis en œuvre en Afrique de l'Est et du Sud. J'évalue l'hétérogénéité de ces politiques publiques, leurs effets voulus mais aussi involontaires, négatifs ou positifs. Dans cette perspective, je compile des preuves sur les points forts et les limites de ces politiques. Ainsi, dans le 1er chapitre, j'évalue le rôle d'un programme de subventions qui vise à améliorer l'accès au logements formels en Afrique du Sud. Compte tenu du fait que les inégalités sont associées à des taux de criminalité plus élevés, je montre que les subventions au logement ont non seulement réduit les inégalités, mais aussi atténué les crimes violents. Cependant, l'impact sur les crimes contre les biens reste limité. De plus, dans le 2ème chapitre, j'évalue l'impact des programmes des repas scolaires sur l'éducation au Malawi. Je montre que les résultats statistiquement significatifs sont limités aux zones d'insécurité alimentaire. Par ailleurs, j'en arrive également à la conclusion que les repas scolaires sont efficaces pour attirer les enfants à l'école pour la première fois, alors que les impacts sur les taux d'abandon sont moins forts. Enfin, dans le 3ème chapitre, j'évalue les effets de l'élimination des frais de scolarité à l'école primaire en Tanzanie. Comme dans le chapitre précédent, je montre que cette politique éducationnelle a augmenté les taux d'inscription à l'école. En revanche, elle a eu un impact limité sur le maintien à l'école des élèves. En outre, il apparaît que l'élimination des frais de scolarité a réduit l'écart éducationnel entre les sexes et a particulièrement profité aux filles qui habitent dans des circonscriptions administratives où les investissements dans l'infrastructure scolaires ont été historiquement plus élevés. Dans l'ensemble de cette thèse, je soutiens que les décideurs politiques doivent être conscients des hétérogénéités potentielles entretenues par les réformes afin d'exploiter les atouts des politiques éducationnelles et de surmonter leurs limites en adoptant des mesures complémentaires. Finalement, je montre également que les impacts des politiques en faveur des pauvres, telles que les subventions au logement, ne se limitent pas aux bénéfices individuels, mais peuvent également diminuer la criminalité, ce qui sert l'intérêt collectif de toute la société.

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# Contents

# Introduction

The central theme of my dissertation concerns the evaluation of a sample of pro-poor programmes and policies in sub-Saharan Africa. I estimate the impacts of three interventions: housing subsidies in South Africa, school feeding programmes in Malawi and free primary education in Tanzania. The outcomes of interest range from inequality in housing conditions and crime in the case of South Africa, to enrolment and retention rates in Malawi, and finally, educational achievement and gender gaps for the Tanzanian case study. In this order, each of these case studies is explored in a separate chapter.

To give context to my case studies, I briefly describe background developments in sub-Saharan Africa and then introduce the specificities of each sampled country. For instance, improvements in access to basic amenities in sub-Saharan Africa have been modest. In 1990, 48 percent of the population had access to an improved water source. In 2012, the ratio had only risen to 61 percent.[1,2] Similarly, only 26 percent of the population on the sub-continent had access to improved sanitation facilities in 1990. The percentage was 31 in 2010.[3] Finally, with respect to access to electricity, the improvement has been more substantial. In 2000, 26 percent of the population in a sample of 35 sub-Saharan countries had access to electricity. In 2018, electricity reached 50 percent of these populations.[4]

[1]The World Bank. DataBank. Database: Africa Development Indicators. Improved water source (% of population with access). Indicator SH.H2O.SAFE.ZS. Definition: Percentage of the population with reasonable access to an adequate amount of water from an improved source, such as a household connection, public standpipe, borehole, rainwater collection and protected well or spring. Unimproved sources include vendors, tanker trucks, and unprotected wells and springs. Reasonable access is defined as the availability of at least 20 litres a person a day from a source within one kilometre of the dwelling. Source organisations: World Health Organization and United Nations Children's Fund, Joint Measurement Programme. www.wssinfo.org.

[2]According to a more stringent classification, only 18 percent of the sub-Saharan population had access to a safely managed source of water in 2000. In order to meet the criteria for a safely managed drinking water service, people must use an improved source that meets three criteria: (i) it should be accessible on premises, (ii) water should be available when needed, and (iii) the water supplied should be free from contamination. The fraction increased to 24 percent in 2015 (World Health Organization and the United Nations Children's Fund, 2017).

[3]The World Bank. DataBank. Database: Africa Development Indicators. Improved sanitation facilities (% of population with access). Indicator SH.STA.ACSN. Definition: Percentage of the population with at least adequate access to excreta disposal facilities that can effectively prevent human, animal and insect contact with excreta. Improved facilities range from simple but protected pit latrines to flush toilets with a sewerage connection. To be effective, facilities must be correctly constructed and properly maintained. Source organisations: World Health Organization and United Nations Children's Fund, Joint Measurement Programme. www.wssinfo.org.

[4]The World Bank. DataBank. Database: Africa Development Indicators. Access to electricity (% of

While improvements in terms of access to basic amenities have been sluggish on average in sub-Saharan Africa, South Africa, however, has performed particularly well. According to the 1996 census, 59 percent of households had access to piped water in their dwelling or on site, 40 percent had access to flush toilets and 54 percent had electricity. In 2018, these percentages had increased to 75, 66 and 85, respectively (Statistics South Africa, 2019). This progress is also linked to the fact that the percentage of South African households living in a formal dwelling has increased from 65 percent in 1996 to 81 percent in 2018 (Statistics South Africa, 2012, 2019). Nevertheless, despite these improvements in averages, the distribution of adequate housing remains highly unequal in South Africa.

In Chapter 1, I document the deleterious effects of inequality in terms of access to quality housing and basic amenities. I find evidence that housing inequality explains a significant share of the variation in both property and violent crimes in South Africa. Additionally, I find that the implementation of a programme granting housing subsidies to low-income South Africans has reduced inequality in terms of housing conditions. Moreover, the programme has also had the unexpected impact of reducing violent crimes. I argue that access to satisfactory living conditions has reduced the level of strain felt by South Africans. Strain is generally fuelled by the inability of individuals to attain positively-valued goals and is theoretically linked to criminal behaviour. This chapter shows that the impacts of pro-poor policies are not limited to individual gains but can also improve outcomes of importance to society as a whole.

Compared to the improvements in basic amenities, the advancements in education have been substantially stronger, with some African countries performing remarkably well. In 1975, only 56 percent of children of primary school age were in school in sub-Saharan Africa. By 2018, the ratio of out-of-school children had been lowered to 20 percent.[5] In Tanzania, for instance, the net enrolment rate for primary education was only 50 percent in 1999. Therefore, the out-of-school population registered a sharp decrease following the elimination of primary school fees in 2002. Around 2008, Tanzania had almost achieved universal primary education. However, net enrolment rates began to deteriorate in the following decade, and the proportion of out-of-school children increased to 18 percent in 2018.[6] Unlike Tanzania, Malawi has higher enrolment rates. Between 2010 and 2016, net enrolment rose from 84 to 90 percent (Malawi National Statistical Office, 2017). Nevertheless, Malawi suffers from low retention rates. The survival rate to the last grade of primary education was 50 percent in 2011.[7] In Tanzania, this rate was 81 percent in 2009 and 85 percent in 2018.[8]

population). Indicator EG.ELC.ACCS.ZS. Electrification data are collected from industry, national surveys and international sources. Source organisations: World Bank, Sustainable Energy for All (SE4ALL) database from the SE4ALL Global Tracking Framework led jointly by the World Bank, International Energy Agency and the Energy Sector Management Assistance Programme.

[5]The World Bank. DataBank. Children out of school (% of primary school age). Indicator SE.PRM.UNER.ZS. UNESCO Institute for Statistics. uis.unesco.org. Accessed 27 September, 2020.

[6]Tanzanian statistics are sourced from The World Bank. See footnote 5.

[7]The most recent, non-estimated data point. UNESCO Institute for Statistics. Survival rate to the last grade of primary education. data.uis.unesco.org. Accessed September 27, 2020.

[8]UNESCO Institute for Statistics. data.uis.unesco.org. Survival rate to the last grade of primary

Against this background, I investigate the educational impacts of school feeding programmes in Malawi and the elimination of primary school fees in Tanzania in Chapters 2 and 3, respectively. I find that school feeding has had a strong positive effect on net enrolment rates, but the impact on retention rates has been more localised. The greatest beneficiaries have been the children of households constrained financially and facing food insecurity. Moreover, I show that the elimination of school fees in Tanzania has also reduced the proportion of individuals who have never enrolled in school. Importantly, I document evidence that the elimination of fees has impacted girls relatively more, which has led to a narrowing of gender-based inequalities in terms of primary school enrolment.

It is safe to say that the world has put inequality under the magnifying glass in recent years. It is also important to note that African countries rank unfavourably in comparison to other developing nations in terms of economic inequality, and a few among them are exceedingly unequal. This is unfortunate because highly unequal societies grow more slowly in the long run than countries with low inequality (Halter et al., 2014; Berg et al., 2018). This relationship is particularly strong if income inequality is coupled with inequality of opportunity (Aiyar & Ebeke, 2020). Inequality can breed crime (Kelly, 2000; Demombynes & Özler, 2005; Metz & Burdina, 2018), impact social cohesion and increase mistrust in institutions (Alesina & La Ferrara, 2002; Gustavsson & Jordahl, 2008; Goubin, 2018). Adding more complexity to the problem is the fact that inequality tends to be stubbornly persistent across generations (Lindahl et al., 2015; Celhay & Gallegos, 2015).

Inequality, however, is not limited to income and wealth. For instance, the discussion about poverty has already shifted from monetary poverty to other dimensions, such as nutrition, health, education, information, water, sanitation and housing. The study of inequality is slowly but steadily following suit (United Nations, 2020). The chapters of this dissertation are precisely contributing to this trend, as I am studying inequality in terms of access to education, adequate housing, sanitation, safe water and electricity. My dissertation provides evidence that pro-poor policies can mitigate the inequality of opportunities between the poor and the rich, females and males. Housing subsidies reduce housing inequality, and their impacts are further extended to the mitigation of violent crimes. School meals and the elimination of user fees in education have improved access to education for the poor and for females.

Importantly, policy-makers must be aware of all the ways in which pro-poor policies can stray away from their objectives. The Tanzanian case study shows how the elimination of school fees can perpetuate historical patterns. Average improvements are registered, but the disadvantaged, i.e., females residing in districts with historically low investments in education, continue to benefit less compared to females from other districts. Moreover, school feeding in Malawi underscores the importance of having this type of intervention complemented by other programmes, as there is only so much one intervention can do in isolation. I find that the impacts of school meals on education are highly reliant on the prevailing food security situation. Unless there is a food constraint to relax, the

education. Accessed September 27, 2020.

programme will have limited impact. Thus, school feeding should be targeted at the most vulnerable of households. However, if communities oscillate between food security and insecurity, then casting a wider protection net can prepare communities for periods when food insecurity is widespread.

The case studies on Malawi and Tanzania point to a further limitation of educational policies and programmes in sub-Saharan Africa. Despite the fact that school feeding and user fee elimination have both been very successful at attracting children into primary education for the first time, their impact on retention rates has been limited. In fact, dropout rates have increased post-reform for grades 4–7 in Tanzania. In Malawi, there is the added problem of early enrolment due to the implementation of school feeding. To relax budget constraints, parents enrol their children in Grade 1 before the recommended age of six. Or this is not necessarily a welcome development as five-year-olds will likely not have the capacity of a six- or seven-year-old to absorb information. Governments can design and implement their educational policies and programmes such that these limitations are addressed, e.g., promoting complementary educational policies and offering pre-school opportunities. Providing policy-makers with evidence is the first step.

Inequality of opportunity is a societal ill that requires serious mitigation. Not only is the unfair and unequal distribution of opportunities detrimental to individuals, it also negatively affects societies, social cohesion and the advancement of development goals. The timing of this dissertation overlaps with the unfolding crisis triggered by the 2020 pandemic, which has had a disproportionate impact on the poor. Moreover, the young are likely to suffer significantly in terms of missed education due to school closures. Children who were relying on school meals will see this safety net taken away from them. The gains in school performance due to better nutrition may be reversed. Furthermore, anecdotal evidence indicates that during this period, girls are facing an increasing risk of early marriage, unwanted pregnancies and female initiation rites such as genital mutilation. The role of education as a stepping stone in achieving social mobility has been seriously hindered by the pandemic. Inequalities along various other dimensions will likely also increase. Therefore, well-designed and well-informed pro-poor policies are ever more relevant and important in the current context, as inequalities are increasing and the progress made in the past decades is now being slowly reversed due to the extraordinary shock that the 2020 pandemic has inflicted on developing and industrialised economies and societies alike.

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# Chapter 1

# Crime, Inequality and Subsidised Housing in South Africa

*Co-authored with* **Patrizio Piraino** *and* **Martina Viarengo**

## Abstract

We study the relationship between housing inequality and crime in South Africa. We create a novel panel dataset combining information on crimes at the police station level with census data. We find that housing inequality explains a significant share of the variation in both property and violent crimes, net of spillover effects, time and district fixed effects. An increase of one standard deviation in housing inequality explains between 9 and 13 percent of crime increases. Additionally, we show that a prominent post-apartheid housing programme for low-income South Africans led to a reduction in inequality and a decline in violent crimes. These findings suggest the important role that equality in housing conditions can play in reducing crime in the context of an emerging economy.

**JEL classification**: D63, I38, R58.

## 1.1 Introduction

South Africa is the most unequal country in the world. It has the highest Gini coefficient in global cross-country comparisons (World Bank, 2020). Besides high inequality, South Africa also exhibits exceedingly high crime rates. According to the most recent worldwide homicide ranking, South Africa has a homicide rate which is 6 times larger than the world average (Global Burden of Disease Collaborative Network, 2018; South African Police Service, 2020).

Consequently, South Africa provides a unique context to analyse the role of socio-economic inequalities and their impact on crime. To date, the vast majority of studies on inequality and crime have focused on income inequality (Kelly, 2000; Enamorado et al., 2016; Kang, 2016; Metz & Burdina, 2018) while a few of them have also looked into consumption-based and land inequality (Demombynes & Özler, 2005; Buonanno & Vargas, 2019). Nevertheless, it has become increasingly apparent that economic inequality is just one dimension of a broader phenomenon. Specifically, housing inequality is particularly important for countries where a significant percentage of households do not enjoy formal living arrangements. In post-apartheid South Africa, only 65 percent of households lived in a formal dwelling (Statistics South Africa, 2012).

We aim to provide the first study concerning the relationship between inequality in housing conditions and various types of violent and property crimes in South Africa over 1996–2011. We also examine the role of a major post-apartheid housing programme that was introduced by the South African Government to reduce inequality in living conditions. We evaluate the effect of this large-scale housing programme on inequality and crime in one large province, the Western Cape. According to our estimates based on data from the Department of Environmental Affairs and Development Planning (2014) and Franklin (2020), the mean stock of housing projects in the Western Cape Province has evolved from 0 to roughly 2.08 and 4.22 thousand housing units per 100,000 people at the beginning of 1995, 2000 and 2010, respectively. This is the equivalent of approx. 54,250 and 178,500 housing units delivered by the beginning of the years 2000 and 2010, respectively. The scale of this government housing scheme is remarkable—both compared to other African countries or emerging economies.[1]

[1] According to the Department of Human Settlements, 3 million houses had been delivered by the end of 2017 in South Africa. On the continent, Algeria comes close, but the context is vastly different as the State provides 95 percent of all Algerian housing (Centre for Affordable Housing Finance in Africa, 2019). Between 1999 and 2018, 1,176,000 housing units were labelled for the underprivileged, i.e., logement public locatif, with most of these housing units being delivered after 2010 (Algérie Presse Service, 2018). Algeria's population rose from 36 to 42 million between 2010 and 2018. Another prominent housing intervention on the continent is the Integrated Housing Development Programme (IHDP) in Ethiopia. However, its scale pales compared to South Africa. Since its inception in 2006, the IHDP has only delivered 250,000 flats (The Economist, 2017). Ethiopia's population rose from 78 to 106 million between 2006 and 2017. Morocco has also embarked on a mission to eliminate informal housing arrangements under the Villes Sans Bidonvilles Programme, which started in 2004 and delivered 277,583 housing units—according to the Government of Morocco (www.mhpv.gov.ma, accessed September 6, 2020). In addition, 24,629 additional units have been delivered under a different low-cost housing scheme, logement à faible valeur immobilière totale. Morocco's population rose from 30 to 36 million between 2004 and 2018. If anything, the provision of government housing in South Africa is comparable to post-war reconstruction programmes in Europe.

We draw upon a unique panel dataset. We merge data on crime at the police station level with socio-economic data from the South African census to form a spatial panel. We collect information from the universe of police stations in the country on both violent offences (aggravated assaults, murders and rapes) and property crimes (thefts out of vehicles and residential burglaries), and we use census data to construct an index describing housing conditions across South Africa's former magisterial districts. In addition, we exploit the spatial nature of the data to identify the magnitude of spillover effects across districts. Finally, we also merge data on the location of housing projects that were approved by the Department of Human Settlements in the Western Cape Province. We use this data to investigate the impact that improved access to adequate housing has on crime.

We find that housing inequality is positively related to the prevalence of all types of crime we investigate, except for murders. For most crimes, an increase of one standard deviation in housing inequality can explain between 9 and 13 percent of increases in criminal offences. The inequality-crime association is stronger for thefts out of vehicles, where a standard deviation increase in housing inequality explains 41 percent of the variation in the number of theft incidents per 100,000 individuals. Spillover effects between districts are significant and stand at approximately 30 percent of a district's own crime levels. Moreover, we show that an increase of 1,000 housing units per 100,000 people (approximately 0.45 standard deviations) reduces housing inequality by roughly 0.04–0.16 standard deviations. In terms of impacts on crime, we find that an increase of 1,000 housing units per 100,000 people has triggered a 5 to 6 percent reduction in the rate of violent crimes. To our knowledge, this is the first study to show that inequality in housing conditions leads to crime. This finding is consistent with the predictions of strain theory in criminology. Significant strain can be associated with inequality in housing conditions because of people's failure to achieve the fundamental life goal of decent housing. Agnew (2001) argues that strains that are most likely to cause an offending behaviour are usually high in magnitude, i.e., they are intense or lengthy and important to the individual, they are perceived as unjust and happen against the background of low social control.

Our study makes three main contributions to the literature. First, we provide evidence regarding the role of a neglected dimension of inequality, housing inequality, and its relationship with crime. Second, our analysis relies on panel data, whereas the literature on crime and inequality in South Africa has been limited to cross-sectional analyses. Moreover, we also factor in the spatial autocorrelation of crime and unobservables across districts. Such a high-quality dataset is rare in a developing country context. Third, we show that a large-scale post-apartheid housing programme, which is able to reduce housing inequality, has also demonstrated potential in the mitigation of violent crimes. Even at the level of industrialised economies with good data availability, there is insufficient evidence related to housing interventions (Collinson et al., 2015). For developing countries, the evidence is even scarcer.

For instance, Britain built approx. 3 million units of social housing in 1950–70 (The Economist, 2020). Britain's population rose from 50 to 60 million between 1950 and 1970.

## 1.2 Literature

### 1.2.1 Rational Criminals

Sociology and psychology were the first disciplines to study crime. They were followed by criminology in the post-war period and economics in the late 1960s, when Gary Becker published his influential work on the economics of crime and punishment (Cook et al., 2013). Before the economic theory of crime, the central view was that society or culture were the chief creators of crime, which meant that there was limited room for policy to intervene and contain crime since societal changes are arguably long-term processes that cannot be easily altered (Cook et al., 2013). In contrast, the economics model introduced the idea that crime is a rational individual choice, whereby potential offenders compare the costs and benefits of criminal acts to decide whether illegal actions are worthwhile. In this context, governments can intervene to either reduce the attractiveness of criminal participation relative to legitimate living, or increase the costs of crime by making detection easier or punishment harsher. In criminology, the equivalent of the economics approach to studying crime became known as the "*rational choice theory*", which was introduced in the 1980s (Akers, 1990). At first, criminology and economics worked separately. In recent years, however, economists have become more inclined to draw on the work of criminologists. This is evocative of a more generalised trend promoting interdisciplinary work between economics and other disciplines (Cook et al., 2013).

Evidence regarding the pillars of the economic theory of crime is extensive. First, there is research examining the factors that speak to the attractiveness (or lack thereof) of legal earning opportunities. For instance, these are studies that focus on education as a crime-limiting factor (Lochner & Moretti, 2004; Machin et al., 2011; Chalfin & Raphael, 2011; Anderson, 2014; Hjalmarsson et al., 2015; Bell et al., 2016) or investigate low wages and unemployment as an inducement to a life of crime (Raphael & Winter-Ebmer, 2001; Gould et al., 2002; Machin & Meghir, 2004; Fougère et al., 2009; Bell et al., 2018; Khanna et al., 2019; Hémet, 2020). Second, researchers have also sought evidence related to the effectiveness of deterrents such as the size and intensity of police activities (Levitt, 2002; Di Tella & Schargrodsky, 2004; Evans & Owens, 2007; Lin, 2009; Draca et al., 2011; DeAngelo & Hansen, 2014; Chalfin & McCrary, 2018) or the magnitude and swiftness of sanctions (Liedka et al., 2006; Drago et al., 2009; Hawken & Kleiman, 2009; Johnson & Raphael, 2012). Overall, the literature suggests that increases in education are a powerful crime mitigator, while unemployment is not a strong predictor of crime. Similarly, improvements in law enforcement systems are systematically linked to reductions in crime; however, sanctions appear to be a relatively weak deterrent.

### 1.2.2 Inequality, Property and Violent Crimes

#### 1.2.2.1 Theory

Economic theory has examined the relationship between inequality and property crimes. Apart from education and labour market characteristics, which have received significant

attention in testing the validity of the first pillar of the economic theory of crime, researchers have also explored additional factors that proxy for the relative attractiveness of illegal activities with respect to lawful employment. Inequality is a prominent example.

Chiu & Madden (1998) build on Becker (1968) and suggest that an increase in average income—which happens against the background of higher incomes becoming higher and lower incomes becoming lower—will increase the potential proceeds from illegal activities as well as the appeal of property crimes. This framework also implies that property offences will disproportionately happen in the relatively richer neighbourhoods unless the adoption of defence technologies becomes widespread. Similar theoretical insights have been put forward by Freeman (1999), Wu & Wu (2012) and Costantini et al. (2018).

The aforementioned studies raise crucial insights. For instance, due to its underlying cost-benefit framework, the economics approach to studying crime is best suited to discussing property offences, while the framework is limited in terms of explaining violent crimes (Kelly, 2000; Wu & Wu, 2012; Draca & Machin, 2015). This is because property and violent crimes are perpetrated for different reasons. Property offences are carried out for material gain, which makes them amenable to a Becker-type cost-benefit analysis, whereas violent crimes are not (Kelly, 2000; Demombynes & Özler, 2005).

Criminological theories have examined the relationship between inequality and violent crimes. While inequality is regarded as a deciding factor in assessing the magnitude of illegal benefits in a cost-benefit analysis of property offences, the same phenomenon can also be interpreted as a source of strain leading to anger and impulsiveness, which in return make violent crimes more likely. The latter argument is based on the theory of strain in criminology, which hypothesises that criminal behaviour is the result of the strain individuals and societies feel, whereby strain is brought on by the failure to achieve positively valued goals (Agnew, 1992, 1999, 2001). The strains most likely to cause offending behaviour are generally high in magnitude, intense or lengthy and important to individuals, and they are perceived as unjust (Agnew, 2001). Negative emotions, such as anger, frustration and despair, are the hypothesised channels that connect strain to criminal activities (Agnew, 1992, 1999, 2001; Brezina, 2017). Among these, anger is central to using strain theory to explain violent crimes (Aseltine et al., 2000; Piquero & Sealock, 2000; Mazerolle et al., 2003).

#### 1.2.2.2 Empirics

Empirical research supports the theoretical insights of the economic theory of crime. For instance, Demombynes & Özler (2005) find a positive correlation between inequality and property crimes in South Africa. The authors show that the incidence of property offences is higher in police precincts that are relatively wealthier than their immediate neighbours. Moreover, Metz & Burdina (2018) document similar results for a sample of urban centres in the United States. Finally, Bourguignon et al. (2003) also investigate the link between inequality and property crimes and conclude that the leftmost part of the income distribution is responsible for property crimes in Colombia. In this case, a

change in income among individuals above a specified threshold would have no impact on mitigating crime. The same type of insight is also posited by Machin & Meghir (2004).

Regarding strain theory, the evidence comes exclusively from criminology. To test the theory, Rebellon et al. (2009) and Mahler et al. (2017) use individual-level data. Macro-level evidence is also common. For instance, Brezina et al. (2001), Hoffmann & Ireland (2004) and de Beeck et al. (2012) take schools as their unit of observation. Others look at neighbourhoods, as do Warner & Fowler (2003) and Hoffmann (2003). They all find suggestive evidence that strain leads to violence and criminal behaviour.

Strain theory has not been tested explicitly by economists, yet it has been invoked to explain the relationship between inequality and violent offences. One notable example is Kelly (2000) who studies crime in metropolitan areas of the United States. Although not using strain theory to interpret their results, Enamorado et al. (2016) bring further evidence that inequality leads to higher homicide rates in Mexico, and Buonanno & Vargas (2019) reach the same conclusion for the case of violent crimes in Colombia. Kang (2016) further refines the link between inequality and crime and argues that it is a specific type of inequality, i.e., segregation and poverty concentration, that drives violent crimes in the United States.

### 1.2.3 Crime, Housing Inequality and Related Interventions

The literature has mainly focused on the relationship between crime and economic inequality. Therefore, data on consumption, expenditure, land ownership and income has been commonly used. To our knowledge, there is no significant study that investigates the impacts of housing inequality on crime. In this context, a limited strand of the literature has evaluated interventions which target housing inequality, e.g., giving ownership titles to informal dwellers, providing infrastructure equitably or introducing housing subsidies.

For instance, Field (2004, 2005a,b, 2007) finds that an urban titling initiative in Peru has significantly increased household labour supply, household investments, renovations and reduced fertility. Galiani & Schargrodsky (2010) document similar evidence for Buenos Aires, Argentina. In contrast, some studies show no impact or even negative effects regarding housing subsidies or rent vouchers on individual outcomes such as labour force participation, earnings and the health of beneficiaries in the United States in the short to medium terms (Susin, 2005; Newman et al., 2009; Jacob & Ludwig, 2012; Jacob et al., 2015) as well as in India (Barnhardt et al., 2017).

Regarding outcomes that are of societal importance, such as crime, the literature is relatively smaller, and most of the evidence is based on information from industrialised countries. For instance, Santiago et al. (2003) study public housing in Denver, Colorado and argue that the programme has not impacted property crimes or violent offences. In contrast, Freedman & Owens (2011) and Woo & Joh (2015) do find that the Low-Income Housing Tax Credit programme has reduced crime in the United States, and in Austin, Texas, respectively. Freedman & Owens (2011) further argue that the programme has mitigated violent crimes, but it has had no effects on property crimes. Unlike the findings

of Freedman & Owens (2011), Disney et al. (2020) show that both violent and property offences have decreased as a result of the Right to Buy scheme in the United Kingdom, which enabled the tenants of public housing to buy their dwellings at subsidised prices.

### 1.2.4 Crime, Inequality and Housing Policies in South Africa

To date, no study has examined the effects of government housing on crime in South Africa, but several studies have investigated the determinants of crime. The most prominent study is Demombynes & Özler (2005), who study consumption-based inequality and crime using the 1996 census data. The authors find that inequality is associated with increases in both property and violent crimes, although the evidence is stronger for property offences. Similarly, Bhorat et al. (2017) study income-based inequality and crime, and they, too, use a cross-sectional dataset, namely the 2011 census. The authors find a positive link between inequality and property crimes but not violent crimes. Other studies have examined correlates of crime, such as education (Jonck et al., 2015), changes in ethnic composition around the time of democratization (Amodio & Chiovelli, 2018), the weather (Bruederle et al., 2017) and social capital in the former apartheid-time resettlement camps (Abel, 2019a).

Regarding the study of subsidised housing in South Africa, Franklin (2020) uses government housing data for metropolitan Cape Town and shows that low-cost housing developments have had a significant and positive effect on household earnings, particularly those of women. In contrast, Picarelli (2019) and Lall et al. (2012) argue that there is no impact on labour force participation, but that the education of children has improved. However, the latter papers do not focus on Cape Town. Instead, they investigate all metropolitan areas and employ vastly different identification strategies compared to Franklin (2020).

## 1.3 Background

### 1.3.1 Inequality and Crime

Inequality and crime are pressing issues in South Africa. The country has the highest Gini index in the world (World Bank, 2020)[2], and an estimated 65 percent of the pre-tax national income was captured by the top 10 percent of its earners during the past decade (World Inequality Database, 2020).[3] Moreover, the income share of the top 1 percent has increased from 10 to 21 percent between 1993 and 2014 (Alvaredo et al., 2018). In terms of crime, the homicide rate in South Africa is significantly above the global average and can be regarded as a symptom of wider crime-related problems. The homicide rate is of 36 reported cases per 100,000 individuals (South African Police Service, 2018) while the

[2]Consumption data from 2014–15 is used to compute the Gini index of South Africa in this cross-country classification. The data can be retrieved from https://databank.worldbank.org. Accessed December 20, 2020.

[3]Only São Tomé and Príncipe has a higher rate of 68.9 percent, which is a significant increase from an estimated 60 percent in 2015. The data can be retrieved from https://wid.world/data. Accessed December 20, 2020.

global average is of 6.1 homicides per 100,000 people (United Nations Office on Drugs and Crime, 2019).[4] Murder rates have remained extremely high, and this reflects the extraordinary level of violence that exists in South Africa.[5]

Moreover, property-related crimes are also a serious issue.[6] In 2018, 1 in every 24 households on a suburban block was burgled (Statistics South Africa, 2018). Given the sizeable magnitude of crime, it is not surprising that this threat is reflected in the way South Africans go about their daily lives. Approximately 32 percent of individuals reported avoiding open spaces due to fear of crime, 17 percent were keeping their children from playing in their neighbourhoods and 14 percent were fearful of walking in their own town or using public transportation (Statistics South Africa, 2018). In addition, about 52 percent of South African households took significant measures to protect their homes (Statistics South Africa, 2018). Given the high levels of poverty in the country, this also implies that a large percentage of households allocate part of their limited resources to home protection.

South Africa has been suffering from widespread crime and high inequality for a long time. For instance, the concept of separate and unequal resource allocation was embedded into South Africa's apartheid legislation on general facilities, education and employment (Byrnes, 1996). Although apartheid was formally introduced in 1948, the ideology had been in place long before (Wilkinson, 1998). This has had long-term consequences for the socio-economic development of South Africa. In 1991, the legislative pillars of apartheid were repelled: The Land Act of 1913 and 1936, the Group Areas Act of 1950 and the Population Registration Act of 1950 (Byrnes, 1996). The new democratic government came to power in 1994. Several reforms followed.

As a result of concerted development efforts and reforms—including several safety-net programmes, public pension schemes, housing subsidies, school feeding programmes, the elimination of school fees for the poor and free healthcare for children, the elderly and other vulnerable groups—the poverty rate fell from 34 percent in 1996 to 19 percent in 2015 (African National Congress, 1994; World Bank, 2018).[7] Moreover, the proportion of the population with access to basic amenities has continued to increase (World Bank, 2018). In 1994, 62 percent of individuals had access to electricity. In 2015, the percentage increased to 86. Regarding access to improved water and sanitation facilities, the ratios went up from 83 to 93 percent and from 53 to 66 percent, respectively (World Bank, 2018). Furthermore, South Africa is close to achieving universal primary and secondary education, which are key to promoting socio-economic mobility. School attendance among children aged 6 to 18 has increased from 85 percent in 1996 (Census, 1996)[8] to 96 percent

[4]2017 statistics are used for comparability. 20,277 murders between April 2017 and March 2018 (South African Police Service, 2018) for an estimated population of 56,52 million (Statistics South Africa, 2017).

[5]For the 2019–20 financial year, the rate was still at 36 (South African Police Service, 2020).

[6]There are some differences across countries with respect to definitions and propensities to report such incidents that make it more difficult to compare South Africa with other countries.

[7]The World Bank uses international poverty lines in this assessment. Namely, US $1.9 at 2011 purchasing power parity exchange rates. Based on locally measured poverty thresholds, i.e., R758 per month in terms of 2017 prices, the poverty rate was 40 percent in 2015 (World Bank, 2018).

[8]According to the 1996 census. Obtained as the division between full- and part-time students on the

in 2017 (Statistics South Africa, 2019). However, despite these improvements in average wellbeing, crime has remained high, and South Africa continues to be exceedingly unequal (Alvaredo et al., 2018; World Bank, 2020; World Inequality Database, 2020).

Using 2015 data, the World Bank (2018) shows that most of the richest decile in South Africa was connected to the electricity grid and had access to an improved water source: 98 and 97 percent, respectively.[9] In contrast, about 78 and 54 percent of the poorest decile enjoyed these amenities. Similarly, roughly 65 percent of the poor had access to improved sanitation, while the richest decile was nearing universal access (World Bank, 2018). Lastly, only 2–3 percent of the richest decile were living in overcrowded housing in 2015. The rate among the poorest decile was 68 percentage points higher (World Bank, 2018).[10]

### 1.3.2 Housing Policy

Housing policy in South Africa started off as a racialised scheme around the 1920s. The 1918–20 influenza pandemic likely precipitated the writing of policies and the establishment of institutions intent on segregating South Africans based on race (Wilkinson, 1998). Between the 1930s and the mid-1970s, housing policy in South Africa served the segregationist agenda of the apartheid government. This agenda ultimately promoted the concept of separate development which implied total apartheid (Wilkinson, 1998). Starting with the mid-1970s, however, pressure started mounting against the apartheid government. The urbanization of black South Africans was increasingly regarded as inevitable (Goodlad, 1996). It was no longer feasible to keep black South Africans away from white urban centres. In this context, due to a long history of injustices, people in townships started to revolt (Wilkinson, 1998). Consequently, the government began to slowly relax its vision of total apartheid, and some measures were taken to increase the housing infrastructure catering to African households. Nevertheless, these efforts lacked ambition. For instance, they included the provision of minimally serviced sites on which African households could build a formal structure at their own expense (Goodlad, 1996; Wilkinson, 1998). Some of these sites were so poorly located that they remained empty (Goodlad, 1996). They were referred to as "*toilets in the veld*" (Tomlinson, 1998).

With the fall of apartheid in 1994, the provision of adequate housing became a prominent tool to rebuilding South Africa. In fact, housing policies became part of the overarching Reconstruction and Development Programme (RDP). The RDP was the master framework of that time, an integrated and coherent programme of socio-economic transformation that was designed to enable South Africa to overcome the legacies of apartheid and transition to a racially inclusive democracy. The Ministry in the Office of the President (1995) has eloquently summarised the programme, which prominently aimed at reducing

one hand, and the 6-to-18 population less the institutionalised and the "*unspecified*" category, on the other.

[9] Calculations of the World Bank based on 2014–15 data.

[10] The World Bank uses the number of people per bedroom to measure overcrowding. Two persons per bedroom is the standard applied to determine whether a household is overcrowded.

inequality, among other objectives.

> "*The [..] RDP is our response to the serious social and economic problems of South Africa: mass poverty, gross inequality, a stagnant economy and enormous backlogs.*"

In the same document, the RDP is set against a context of inequality, poverty and crime.

> "*Wealthy suburbs with excellent infrastructure exist side by side with run down townships, squatter camps and city areas. These are characterised by overcrowding, poverty and unemployment, poor services, inadequate facilities, collapsing infrastructure and general decay. Crime and desperation have resulted.*"

During the first year of implementation, 1994–95, the budget allocated to the RDP was of R2.5 billion out of a R148 billion state budget. For 1995–96, the budget was increased to 5 billion (Ministry in the Office of the President, 1995). Out of the RDP budget, the government planned an allocation of R1.4 billion and R1.8 billion to housing in 1994–95 and 1995–96, respectively (Goodlad, 1996; Tomlinson, 1998). The relative size of the housing allocation speaks to the importance of this component within the wider RDP strategy.[11] The housing programme was also considered a catalyst for the building, steel and furniture sectors, an employer for local communities and an engine for the economy (Ministry in the Office of the President, 1995). The RDP goal was to provide decent, well-located and affordable housing to all by 2003 (African National Congress, 1994).

Housing for all was reflective of the vision of the African National Congress. To operationalise this vision, the RDP's medium-term ambition was to build 1 million houses in the first 5 years (African National Congress, 1994; Ministry in the Office of the President, 1995). According to the Department of Human Settlements, only 870,629 houses had been built by the beginning of the year 2000, which meant that roughly 87 percent of the target was attained. Although the target was objectively missed and demand far outstripped the housing units on offer, the achievement was nevertheless significant in the eyes of South Africans and internationally. On December 24, 1999, the New York Times ran an article titled "*Small Houses a Big Step for South African Pride*".[12] It underlined the fact that these "*[. . . ] houses are the most tangible symbol of the post-apartheid government's commitment to redressing this country's stark inequalities*". The subsidised housing infrastructure that was available at the end of 1999 housed 3 million low-income South Africans in one-room houses at the cost of US $2 billion—the equivalent of approximately R12.3 billion in 1999 (New York Times, 1999). An additional 3 million people remained on the waiting list. A South African male interviewed by the New York Times said that

[11] Other important destinations were health, education and infrastructure. Regarding health, the RDP allocated R472.8 and R500 million to the Free Health Care Programme in 1994–95 and 1995–96, respectively. To improve education, R100 million were allocated in 1994–95. With the purpose of improving municipal services and infrastructure, the RDP further allocated R830 million over a two-year period starting 1994. Local communities also had access to R250 million for urgent needs in 1994–95. Numbers are sourced from the report of the Ministry in the Office of the President (1995).

[12] https:/www.nytimes.com/1999/12/24/world/small-houses-a-big-step-for-south-african-pride.html

three years of house ownership made him "*feel like a man*", although his life and that of his family still faced a myriad of other problems.

The unmet five-year target was due to the construction of housing units starting off slowly. Out of the 1994–95 budget allocation, only R42 million were used in 1994–95. Further underspending during the financial year 1995–96 led to a budget of R2.2 billion for 1996–97 (Goodlad, 1996).[13] Progressively, the capacity of the government to deliver did increase. For instance, between 1994 and 2014, 3.7 million houses and serviced sites were provided at the cost of R125 billion[14] (Department of Human Settlements, 2014). By the end of 2017, an additional 0.4 million housing opportunities had been delivered. This was the equivalent of a total of approximately 3 million houses and 1.1 million serviced sites.[15]

The Reconstruction and Development Programme has evolved as it encountered difficulties and had to adapt. It was followed by the Growth, Employment and Redistribution Strategy in 1996, the Accelerated and Shared Growth Initiative for South Africa in 2005, the New Growth Path in 2010 and the National Development Plan in 2013 (Adelzadeh, 1996; Gelb, 2006; Naidoo & Maré, 2015). In parallel, the housing dimension of the original Reconstruction and Development Programme has also evolved throughout the years, along with the institutions it created and the legislation it inspired. References to RDP have been relatively more persistent in the context of housing policy. Nevertheless, housing schemes, too, have often changed name, along with their strategy or implementation design. Further details on these changes are discussed in Section 1.6.

In 1994, there were 2.6 million formal housing units in South Africa, 1.7 million shacks on un-serviced sites and 0.6 million shacks on serviced sites. 1.5 million households were roofless (Goodlad, 1996). A quarter of the population did not have access to piped water, and over 40 percent did not have electricity or proper sanitation (Goodlad, 1996). According to the most recent census data, which were collected in 2011, South Africa had 14.4 million households. Of these, 10.6 million lived in adequate housing (74 percent) and 2.5 million lived in an informal dwelling. The remainder lived in traditional structures. To put these numbers into context and facilitate comparisons, note that there were 9 million households in South Africa in 1996. Finally, 2018 estimations have put the proportion of households living in a formal dwelling at 81 percent (Statistics South Africa, 2019).

Significant progress has been registered since 1994. Nevertheless, average improvements hide the fact that some of apartheid's legacies have not been truly addressed. The RDP has been criticised because of its supply-side approach to housing development and the perpetuation of spatial segregation, as the dwellers of public housing find themselves physically distant from economic opportunities (Wilkinson, 1998). In return, socio-economic spatial segregation breeds crime (Kang, 2016). In a more recent iteration of the housing

[13] R1 was US $0.22 in January 1997. US $1 in 1997 is worth roughly US $1.6 in 2020.

[14] Expressed in 2010 prices. R1 was US $0.15 in December 2010. US $1 in 2010 is worth roughly US $1.19 in 2020. R1 in 2014 was approx. R0.82 in 2010. As a reference, South Africa's GDP in 2014 was R3,800 billion (R3,115 billion in 2010 prices). Statistics South Africa. Accessed December 11, 2020. http://www.statssa.gov.za/?p=4184.

[15] Department of Human Settlements. Housing Delivery Statistics. Accessed December 11, 2020. http://www.dhs.gov.za/sites/default/files/u16/HSDG%20to%20Dec%202017.pdf.

policy, the government does state that it is committed to "*combating crime, promoting social cohesion and improving quality of life for the poor*" within its broader vision of achieving "*sustainable human settlements and quality housing*" (Department of Human Settlements, 2004).

## 1.4 Data and Summary Statistics

### 1.4.1 Data

We use three sources of data. First, we rely on census data released by Statistics South Africa. This includes the 1996, 2001 and 2011 waves. 2011 is the most recent wave that is available. Second, we obtained crime data for the financial years of 1996–97, 2001–02 and 2011–12 from the Crime Statistics and Research Unit of the South African Police Service (SAPS). The police data includes the universe of crimes that were either reported by the community or recorded as a result of police action. The dataset includes detailed information about the type of crime, including numerous types of violent and property crimes. The SAPS dataset has three dimensions: year, police station and type of crime. In this study, we use references to police districts and police stations interchangeably to define the lowest level at which SAPS aggregates crime indicators. Our third source of data includes the GPS coordinates of government housing projects in the Western Cape Province, their date of registration and their planned or approved size. This dataset was obtained from the Department of Environmental Affairs and Development Planning in the Western Cape, and it was initially published in a technical report (Department of Environmental Affairs and Development Planning, 2014).

We constructed the dataset as follows. To begin with, we reconcile the census waves. We start with the 2011 census which was compiled at the lowest possible level of aggregation, roughly 85,000 small area layers that contain the universe of households and individuals with aggregated characteristics. Since we have the coordinates of the centroids of these small area layers, we assign them to the polygons of the administrative units that stand as the common denominator between the 1996 and 2001 waves. South Africa's 354 former Magisterial Districts (MDs) fulfil this role. MD-level aggregation thus allows us to append the 1996, 2001 and 2011 census years into a panel of MD-year observations.[16]

Then, we merge the census panel with the SAPS panel. SAPS data were provided at the level of police stations. There are roughly 1,130 police stations, with their number changing only slightly over time. Some police districts have an irregular shape whose centroid can easily fall outside district boundaries. This problem can be solved by generating a random point within the boundaries of police districts instead of using the centroid for merger. In 35 percent of cases, a police station shares substantial surface with more than one magisterial district. In order to address this issue, we adopt the following procedure.

[16]While the use of weights for the 2011 data was not necessary because the database includes the universe of households and individuals, we do use weight variables to compute our indicators of interest at the level of MDs in 1996 and 2001.

We generate multiple random points per police station to better distribute the incidence of crime between the magisterial districts feeding the crime statistics of the police station in question. For stations that cross the borders of multiple MDs, we count the number of points that fall in each district and assign crimes proportionally to each concerned MD.[17]

Lastly, we merge the housing information point-to-polygon with the MD-year spatial panel, where the points are the project GPS coordinates and the polygons are the MDs.

The result is a panel of 354 MDs observed 3 times, in 1996, 2001 and 2011. This represents a total of 1,062 observations. Census data are representative at the level of MDs, and we use these data to compute inequality indices and average socio-economic characteristics. Each MD is populated with crime data, which is sourced from the universe of crime incidents reported to police stations across the country. Regarding the housing data, the sample is limited to the universe of housing projects in the Western Cape Province. The Western Cape sample has 42 districts observed 3 times, which amounts to 126 observations.

This dataset is the first to allow the analysis of South African census and police information in a panel setup. It is also the first dataset to include project information on government housing for the Western Cape Province. Franklin (2020) also uses project-level data, but his focus is limited to metropolitan Cape Town. Finally, this is a spatial panel. Thus, we can grasp the magnitude of crime spillover effects between administrative units and model the spatial interdependence of MD-level unobservables.

### 1.4.2 Measurement of Crime and Inequality

We measure crime as the natural logarithm of crime incidence per 100,000 people to normalise the distribution of the variable. The data we use consist of crimes reported to the police. However, not all crimes are reported to the police. This does not represent a significant shortcoming as it has been shown that "*the statistician who chooses to ignore the under-recording problem completely would not be misled to any important degree*" (Pudney et al., 2000; Rufrancos et al., 2013).

In order to construct the housing inequality index, we rely on six variables that are indicative of the quality of the housing infrastructure: type of dwelling (permanent, traditional or informal), access to water (tap water inside the building, in the yard, on a community stand or no access to piped water), type of toilet facilities (flush/chemical toilet, pit latrine, bucket latrine or no toilet facilities), type of fuel for cooking and heating (electricity/solar/gas, paraffin/coal or wood/animal dung), and type of fuel for lighting (electricity/solar, paraffin/gas or candles). These are the defining aspects of adequate housing.[18]

[17] This assumes that crimes are homogeneously distributed within the boundaries of any police station. We believe this assumption is unlikely to have significant consequences as the size of police districts is proportional to the size of their population and cities have several police districts. If cities were part of larger police districts, crimes would be concentrated in and around the city, while the outside territory would be less afflicted. Police stations with large territories are usually sparsely populated. See Appendix A.1 for a presentation of the merger between MDs and stations.

[18] Regarding the measurement of housing inequality in the general literature, some researchers rely on

These are also the housing dimensions that the intervention we evaluate, the subsidised housing programme, has targeted in terms of outcomes—whether directly (dwelling type, access to water, type of toilet facilities and electricity) or indirectly (type fuel for cooking and heating via the availability of electricity and the ability to safely store and accumulate assets). We start by computing inequality in terms of each of these six variables. Then, based on the standardised values of the inequality measures, we use factor analysis to reduce the number of variables and summarise their information into one index, i.e., the latent variable that describes inequality in terms of housing conditions. Appendix A.2 shows the factor analysis. There is only one factor with an eigenvalue greater than 1, and the Kaiser-Meyer-Olkin measure of sampling adequacy is encouraging and stands at 0.83. To demonstrate the robustness of our results, we show in the appendix that our findings hold for other combinations of the variables that serve the computation of the inequality index.

$$Inequality = -\sum_{i=1}^{K} \left[ p_i \times ln\big(\sum_{j=1}^{i} p_j\big) \right] \tag{1.1}$$

To accommodate the fact that the variables describing housing conditions are sets of ordered categories, we rely on Cowell & Flachaire (2017) to compute inequality. See Equation 1.1, where $K$ stands for the number of categories, which are ordered starting from the best one and moving down to the worst. That is, $j = 1$ corresponds to houses made from permanent materials, the availability of tap water inside the household's dwelling, the availability of flush or chemical toilets, and the use of electricity or solar energy for lighting, cooking and heating purposes. The larger the number of people who move into the top category, the greater the decline in inequality will be. $p_i$ is the probability of an individual belonging to category $j = i$.

### 1.4.3 Summary Statistics

Table 1.1 reports the summary statistics that describe the 1,062 MD-year observations in the analytical sample.[19] Table 1.1 shows that crime incidence has increased slightly between 1996 and 2001, and it has thereafter decreased sharply. Murders have been the exception. They have decreased across all waves. The inequality index points to a gentle decrease between 1996 and 2001, and a sharp decrease between 2001 and 2011. The percentage of unemployed and discouraged individuals has followed the same pattern. Moreover, average education has improved significantly between 1996 and 2011, and so have most variables proxying for household socio-economic status. Lastly, there is also an indication of post-apartheid rearrangements of the population, as the percentage of people on the move was particularly large in the case of the 1996 wave.

variables such as surface size, number of rooms or selling value to measure housing inequality (Tan et al., 2016; Tunstall, 2015). Others take a multidimensional approach to quantifying access to adequate housing, which is sometimes confounded with an asset index. These studies use variables that describe the quality of wall, floor or roof materials, the presence of basic amenities, asset ownership, access to roads, schools, hospitals, etc. (Filmer & Pritchett, 2001; McKenzie, 2005; Van Phan & O'Brien, 2019).

[19] In computing these summary statistics, MDs are assigned the same weight regardless of their population or surface.

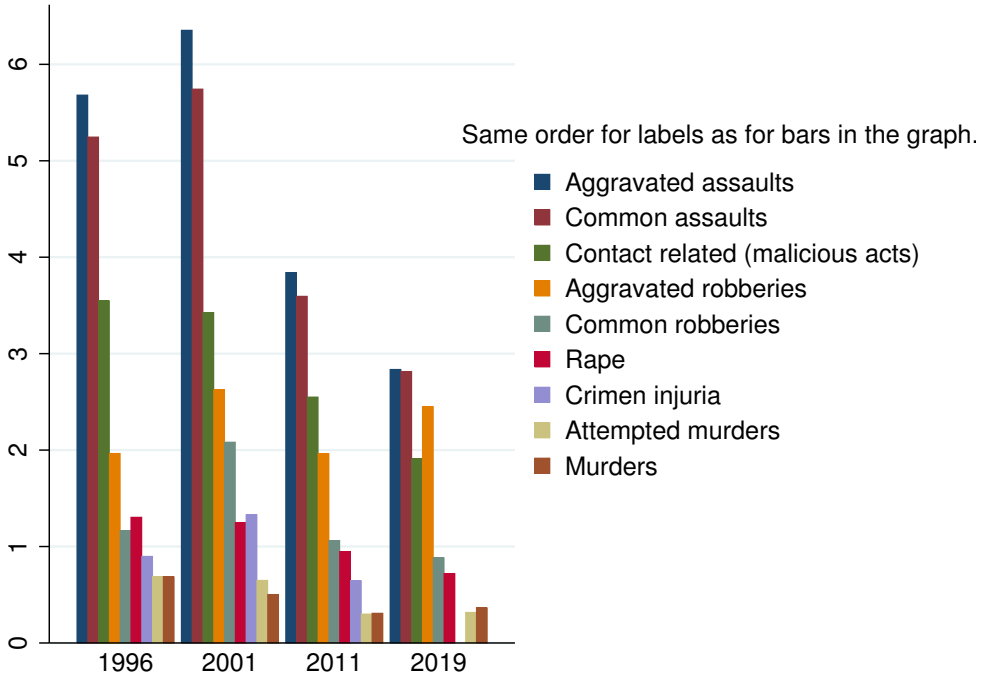
Table 1.1: Descriptive Statistics

| | Per Year, Across MDs | | | Across Years and MDs | | | |
|---|---|---|---|---|---|---|---|
| | Mean values | | | Mean | $\sigma$ | Min | Max |
| | 1996 | 2001 | 2011 | 1996–2011 | | | |
| **Crimes declared at police stations** | | | | | | | |
| All violent crimes per 100,000 people | 2,528 | 2,816 | 1,700 | 2,348 | 1,882 | 296 | 28,428 |
| Aggravated assaults per 100,000 people | 828 | 896 | 548 | 758 | 640 | 74 | 6,400 |
| Murders per 100,000 people | 71 | 52 | 35 | 53 | 40 | 3 | 395 |
| Rapes per 100,000 people | 140 | 141 | 116 | 132 | 86 | 0 | 1,429 |
| All property crimes per 100,000 people | 1,694 | 1,796 | 1,066 | 1,519 | 1,384 | 117 | 16,812 |
| Thefts out of vehicles per 100,000 people | 321 | 350 | 174 | 282 | 500 | 0 | 5,064 |
| Residential burglaries per 100,000 people | 639 | 748 | 503 | 630 | 573 | 42 | 8,426 |
| **Factor analysis of Cowell-Flachaire measures** | | | | | | | |
| Inequality index housing conditions | 0.35 | 0.25 | -0.60 | 0.00 | 0.96 | -2.49 | 1.76 |
| Inequality index housing conditions, standardised | 0.36 | 0.26 | -0.62 | 0.00 | 1.00 | -2.61 | 1.84 |
| **MD-averaged individual characteristics** | | | | | | | |
| Average years of education | 5.55 | 5.92 | 7.18 | 6.22 | 1.34 | 2.21 | 10.00 |
| Percentage Black | 71.43 | 73.93 | 75.47 | 73.61 | 31.26 | 0.00 | 100.00 |
| Percentage Coloured | 16.81 | 16.35 | 15.69 | 16.29 | 26.75 | 0.00 | 91.77 |
| Percentage Asian | 1.06 | 0.97 | 1.06 | 1.03 | 4.95 | 0.00 | 90.75 |
| Percentage unemployed | 17.96 | 22.39 | 14.83 | 18.39 | 6.61 | 2.68 | 44.70 |
| Percentage discouraged | 4.84 | 12.15 | 6.58 | 7.86 | 5.06 | 0.63 | 29.94 |
| Percentage moved in past 10 years | 35.77 | 37.93 | 15.41 | 29.70 | 20.06 | 0.99 | 95.41 |
| **MD-averaged household characteristics** | | | | | | | |
| Average household size | 4.15 | 3.94 | 3.37 | 3.82 | 0.69 | 2.25 | 6.52 |
| Average number rooms per household | 3.75 | 3.83 | 4.09 | 3.89 | 0.50 | 2.11 | 5.54 |
| Percentage living informal dwelling | 13.24 | 11.66 | 8.74 | 11.21 | 11.02 | 0.00 | 67.49 |
| Percentage access to water on premises | 58.55 | 59.78 | 71.65 | 63.33 | 28.67 | 0.77 | 98.79 |
| Percentage access flush or chemical toilet | 39.70 | 44.87 | 57.36 | 47.31 | 30.36 | 0.19 | 98.09 |
| Percentage access to electricity | 53.52 | 66.35 | 83.01 | 67.63 | 23.90 | 1.04 | 99.01 |
| Percentage authority removes rubbish | 47.04 | 49.09 | 54.84 | 50.32 | 30.91 | 0.04 | 99.21 |
| Percentage owns dwelling of residence | 72.62 | 53.71 | 54.47 | 60.27 | 16.55 | 7.68 | 98.53 |
| **MD specific** | | | | | | | |
| Density (1,000 people per $km^2$) | 0.24 | 0.25 | 0.30 | 0.27 | 0.95 | 0.00 | 9.19 |
| **Sample size** | 354 | 354 | 354 | 1,062 | 1,062 | 1,062 | 1,062 |

Average education is for individuals aged 5 or above. The inequality index is based on a factor analysis of 6 variables denoting inequality in terms of housing conditions: type of dwelling, access to piped water, type of toilet facilities and type of fuel for lighting, cooking and heating. Whites are the reference group. Percentage unemployed counts individuals who do not have a job and are looking for employment. Percentage discouraged includes the unemployed who are not looking for a job and those individuals who choose not to work. The denominator consists of the population aged between 15 and 64 years old.

Figures 1.1a and 1.1b present the raw crime categories that are reported by the South African Police Service for violent and property crimes, respectively. These figures include the analytical sample, 1996, 2001 and 2011, and the out-of-sample year of 2019 to show the current situation. Figures 1.1a and 1.1b confirm the insights of Table 1.1. Crime incidence has first increased between 1996 and 2001 and then decreased. Exceptions exist. The incidence of rapes and thefts out of vehicles has generally decreased between 1996 and 2019, although the decrease between 1996 and 2001 was less pronounced. Moreover, murders have decreased between 1996 and 2011, but then exhibited an uptick in 2019.

Figure 1.1a: Violent Crimes (Incidence per 1,000 People Nation-Wide)



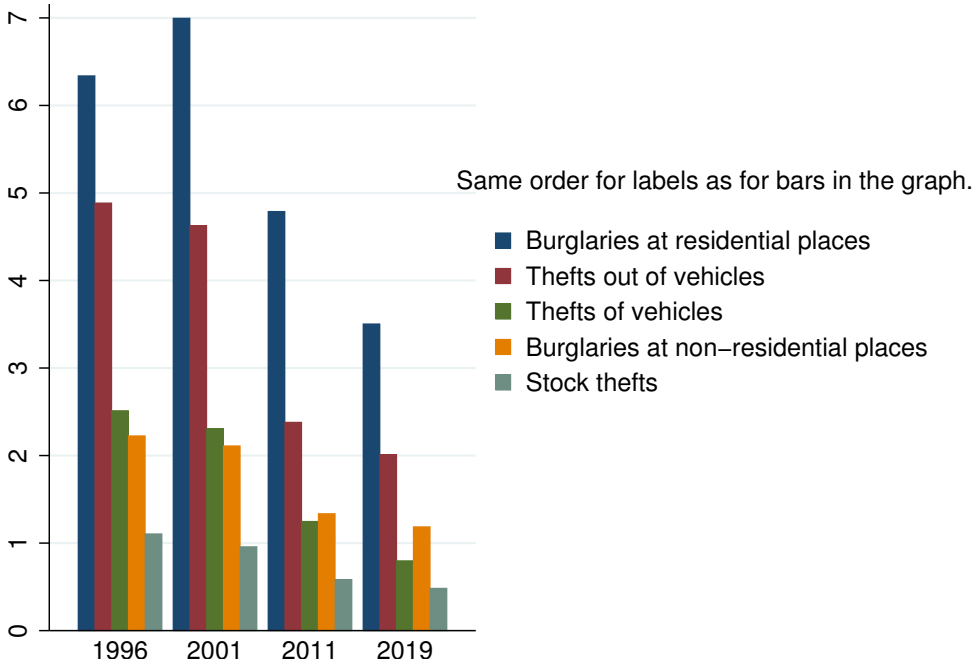

Violent crimes include contact and contact-related crimes. These would have been the universe of violent crimes, but due to the imperfect overlap between crime categories across the years, we had to drop some sexual offences. Nevertheless, due to their relatively small magnitude (as far as reporting to the police is regarded), this omission does not impact the picture that this graph is meant to give.

Figures 1.1a and 1.1b have also motivated our choice of dependent variables. First, we see that aggravated assaults are the most commonly reported type of violent crime. Moreover, since the literature shows that murders are less likely to suffer from misreporting, we decided to include them in our analysis. Furthermore, we also explore the evolution of rapes, as crimes against women have been at the forefront of debates in South Africa. Additionally, we also focus on the two most reported types of property crimes, which consist of burglaries at residential places and thefts out of vehicles. These are also the crime categories that are most often analysed in the literature (Kelly, 2000; Lochner & Moretti, 2004; Demombynes & Özler, 2005; Kang, 2016).

Finally, Figure 1.2 shows the spatial distribution of crime and housing inequality across South Africa's MDs and averaged out across the three waves of data. We see that violent crimes cluster in the Western, Northern and Eastern Cape Provinces, while property crimes are predominant in and around metropolitan areas. Finally, inequality shows a spatial pattern whereby higher values cluster in the Eastern half of the country, particularly in and around the areas which were designated by the apartheid regime for occupation by black populations to serve segregationist agendas.

The summary statistics presented in this section suggest a positive relationship between crime and housing inequality. Inequality has only slightly improved between 1996 and 2001, while it has registered a substantial decrease between 2001 and 2011. In parallel,

Figure 1.1b: Property Crimes (Incidence per 1,000 People Nation-Wide)




This is the universe of property crimes in South Africa.

crime has followed a similar path. Between 1996 and 2001, progress was minimal. Some types of crime have even registered an increase in their incidence. Starting 2001, however, crime incidence has been declining. These trends are supportive of our hypothesis, per which improvements in inequality have decreased the incidence of crime. We explore this link in the following sections by conducting a spatial fixed effects analysis. Moreover, we evaluate the impact of a large-scale housing programme on inequality and crime.

## 1.5 Empirical Analysis

### 1.5.1 Housing Inequality and Crime

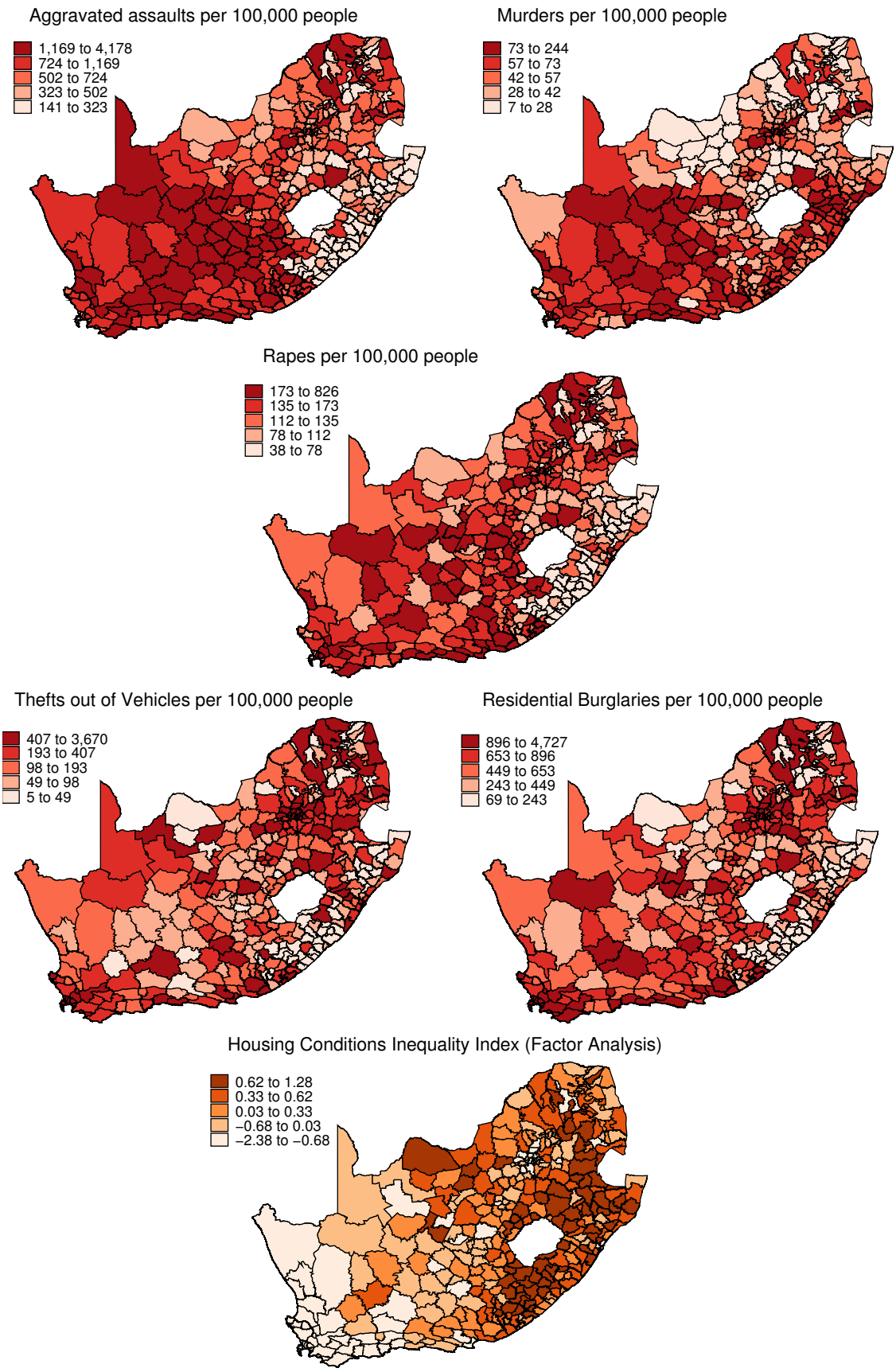
To estimate the relationship between housing inequality and crime, we take advantage of variation over time and across space by using a balanced panel of magisterial districts. We begin our estimations with a fixed effects model as presented in Equation 1.2.

$$C_{nt} = \alpha_n + \lambda_t \iota_n + \beta H_{nt} + X_{nt}\gamma + \epsilon_{nt} \tag{1.2}$$

$C_{nt} = (C_{1t}, C_{2t}, \ldots, C_{Nt})^T$ is the natural log of crime incidence per 100,000 people, where $n$ represents the magisterial district and $t$ represents the time period. As discussed above, we have $N = 354$ magisterial districts and $T = 3$ waves of data, 1996, 2001 and 2011. $H_{nt}$ is the housing inequality index. Equation 1.2 includes both the district fixed effects $\alpha$ and the time fixed effect $\lambda$.[20] $X_{nt}$ includes covariates such as time-varying individual

[20] $\tau$ is a vector of 1s.

Figure 1.2: Spatial Distribution of Variables (1996–2011 Means)



and household characteristics averaged at the magisterial district level, and population density—as presented in Table 1.1.

Table 1.2 shows the coefficient estimates deriving from the fixed effects model. We estimate the model separately for different types of violent and property crimes. The results show a positive association between housing inequality and crime rates. This is true for all types of crimes, with the exception of murders and residential burglaries, where we do not find a statistically significant coefficient.

Table 1.2: Fixed Effects Estimates

| | **VIOLENT CRIMES** Log of crime per 100,000 people | | | | **PROPERTY CRIMES** Log of crime per 100,000 people | | |
|---|---|---|---|---|---|---|---|
| | **All Violent Crimes** | **Aggravated Assaults** | **Murders** | **Rapes** | **All Property Crimes** | **Thefts out of Vehicles** | **Residential Burglaries** |
| **Explanatory variables** | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Housing inequality index | 0.108*** | 0.129*** | -0.064 | 0.131*** | 0.085*** | 0.449*** | 0.059 |
| | (0.030) | (0.033) | (0.050) | (0.045) | (0.032) | (0.076) | (0.037) |
| Avg. education (years) | -0.138 | -0.108 | -0.191* | -0.115 | -0.244** | -0.503*** | -0.235** |
| | (0.098) | (0.092) | (0.104) | (0.095) | (0.102) | (0.123) | (0.105) |
| Perc. unemployed | -0.004 | -0.005 | -0.002 | -0.005 | -0.002 | -0.017*** | -0.001 |
| | (0.003) | (0.003) | (0.005) | (0.005) | (0.003) | (0.006) | (0.004) |
| Perc. discouraged | -0.006 | 0.001 | -0.020*** | -0.004 | -0.009 | -0.009 | -0.008 |
| | (0.005) | (0.006) | (0.008) | (0.006) | (0.005) | (0.010) | (0.006) |
| Within $R^2$ | 0.52 | 0.47 | 0.47 | 0.25 | 0.54 | 0.44 | 0.38 |
| F | 44 | 32 | 34 | 13 | 52 | 39 | 32 |
| Other controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 1,062 | 1,062 | 1,062 | 1,062 | 1,062 | 1,062 | 1,062 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust errors are reported in parenthesis. The inequality index is based on a factor analysis of inequality in terms of water access, type of dwelling, type of toilet and type of fuel/energy for lighting, heating and cooking. All specifications include the following covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage Black, Coloured and Asian) and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet, percentage who have their rubbish removed by authorities, percentage with access to electricity and finally, percentage of dwelling owners.

Controlling for a large set of confounding factors, Table 1.2 shows that a standard deviation increase in the housing inequality index is associated with an approximate increase of 0.108 and 0.085 log points, i.e., 11 and 9 percent, in overall violent and property crimes, respectively. When looking at specific types of crimes, we notice that thefts out of vehicles appear to be particularly sensitive to housing inequality, as the coefficient reaches 0.449 log points. Table 1.2 also reports the coefficient estimates for some of the control variables included in the model. In particular, we note a generally negative correlation between average education and property crimes.

As a robustness check, we use a non-linear specification to account for the fact that the distribution of the untransformed crime variables resembles a Poisson. The estimated coefficients are reported in Appendix A.4. The coefficients on housing inequality are very similar in the two specifications.

### 1.5.2 Accounting for Spillover Effects

The estimation above does not take into account the potential spillover effects across magisterial districts. In order to measure spillover effects, we can use a spatial model. This amounts to including a spatial lag on the dependent variable as follows:

$$C_{nt} = \alpha_n + \lambda_t \iota_n + \rho W_n C_{nt} + \beta H_{nt} + X_{nt}\gamma + \eta_{nt} \tag{1.3}$$

where $\eta_{nt} = \phi M_n \eta_{nt} + \varepsilon_{nt}$, and $W$ and $M$ are square matrices that describe the spatial dependency between magisterial districts. $W$ applies the same positive weight for contiguous spatial units and a zero weight for all other units, while $M$ is the inverse distance weighting matrix. $\rho$ and $\phi$ are scalars.[21]

The specification above implies that any change to an explanatory variable in a given magisterial district can affect the dependent variable in that district's neighbours via increases in own crime, which spills into other districts provided that $\rho$ is different from zero. This allows us to estimate the spatial autoregressive combined (SAC) model with individual and time fixed effects in Equation 1.4 (LeSage & Pace, 2009; Lee & Yu, 2010).

$$C_{nt} = (I_n - \rho W_n)^{-1}(\alpha_n + \lambda_t \iota_n + \beta H_{nt} + X_{nt}\gamma) + (I_n - \rho W_n)^{-1}(I_n - \phi M_n)^{-1}\varepsilon_{nt} \tag{1.4}$$

The defining characteristics of a SAC model are the inclusion of a spatial lag on the dependent variable and the spatial interdependence of the disturbance terms. The SAC specification controls not only for time-invariant omitted variables, but also controls (at least partially) for time-varying omitted variables or unobserved latent influences that explain the spatial clustering of the dependent variable (LeSage & Pace, 2009). It is well established that crime incidence exhibits spatial dependency, e.g., crime hotspots (Chainey et al., 2008; Ratcliffe, 2010). The use of spatial models is in fact relatively common in studies of crime as they help mitigate the omitted variables bias.

It is important to note that the spatial dependency of district observations makes the estimated coefficients of interest not as easily interpretable, as the derivative of the de-

[21] We maintain that individuals are unlikely to travel across multiple districts to perpetrate crimes. Thus, the $W$ matrix is appropriate. However, we are less restrictive about the spatially correlated unobservables, which we assume can be similar beyond first-order neighbours, hence the $M$ matrix. If the scalars are not statistically different from zero, then the use of spatial lags is not necessary for the dependent variable or the error term, respectively.

pendent variable with respect to the explanatory variables is no longer simply $\beta$ or $\gamma$.[22] Instead, average direct and total effects can be computed (LeSage & Pace, 2009).

For a given magisterial district $n$, the average direct impact measures the effect of a change in $X_n$ on crime in district $n$ inclusive of feedback loops, whereby observation $n$ affects its neighbours, and these neighbours will, in return, loop back and affect $n$.[23] The average total impact for district $n$ includes the own derivative (direct impact) and all of the cross derivatives (the indirect impact or spillover effects) (LeSage & Pace, 2009).

While spillover effects motivate the inclusion of spatial lags in the dependent variable, the spatial lags on the error term are motivated by the assumption of spatial heterogeneity (LeSage & Pace, 2009). Thus, in addition to the individual heterogeneity modelled by the fixed effects framework, we are now allowing some of the unobserved characteristics of any given district to be similar to those of its neighbours. The intensity of this similarity is decreasing the further away districts are from each other. Importantly, we assume that these unobserved characteristics are unrelated to the observed covariates.[24]

Table 1.3 shows the results from estimating Equation 1.4. Spillover effects, as estimated by $\rho$, appear to be present in all cases. In most cases, spillover effects from neighbouring districts are roughly one third of a district's own crime incidence. $\phi$ is also generally significant, which points to the existence of spatial heterogeneity in the error terms. If the SAC model identifies the data generating process correctly, then the estimates in Table 1.3 will suffer less from omitted variables compared to the simple fixed effects model. This allows us to get as close as possible (given the available data) to a causal interpretation of the effects of housing inequality on crime.

For violent crimes, the estimated direct effect of housing inequality is 0.085 log points (approximately 8.9 percent), while the total effect, which accounts for spillover effects from neighbouring districts, is about 0.109 log points. The direct impact of housing inequality on aggravated assaults and rape is 0.094 and 0.124 log points, respectively. In the same order, the total impacts are 0.124 and 0.135 log points. Lastly, the coefficient on education is negative across the board—districts that are more educated have less crime. The direct impact of a one-year increase in average education varies between a reduction of 0.130 log points in the case of rapes and 0.204 for murders.

Moving to property crimes, we note that spillover effects are slightly smaller than those for violent crimes, although there is one notable exception: thefts out of vehicles. Moreover, compared to the fixed effects estimates, the spatial estimates display a more consistent pattern across the various types of property crime.

[22]This is because the derivative of $C_{nt}$ with respect to $X^k_{mt}$, where $m$ can be different from $n$, is potentially non-zero. $k$ denotes the $k$th variable in the $X$ matrix.

[23]District $n$ will thus suffer from the crime it fuels in neighbouring districts due to an increase in own crime incidence.

[24]We also assume that spillover effects at the borders between South Africa and its neighbouring countries are negligible. Since these are national borders, with strict controls, it seems a plausible assumption.

Table 1.3: Spatial Autoregressive Combined, Fixed Effects Estimates

| | VIOLENT CRIMES Log of crime per 100,000 people | | | | | | | | PROPERTY CRIMES Log of crime per 100,000 people | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **All Violent Crimes** | | **Aggravated Assaults** | | **Murders** | | **Rapes** | | **All Property Crimes** | | **Thefts out of Vehicles** | | **Residential Burglaries** | |
| | (1) | | (2) | | (3) | | (4) | | (5) | | (6) | | (7) | |
| **Explanatory variables** | Direct | Total | Direct | Total | Direct | Total | Direct | Total | Direct | Total | Direct | Total | Direct | Total |
| Housing inequality index | 0.085*** | 0.109*** | 0.094*** | 0.124*** | -0.030 | -0.038 | 0.124*** | 0.135*** | 0.099*** | 0.124*** | 0.345*** | 0.522*** | 0.086** | 0.105** |
| | (0.029) | (0.037) | (0.031) | (0.041) | (0.044) | (0.055) | (0.037) | (0.040) | (0.030) | (0.038) | (0.058) | (0.093) | (0.035) | (0.043) |
| Avg. education (years) | -0.181*** | -0.233*** | -0.153*** | -0.201*** | -0.204*** | -0.256*** | -0.130** | -0.142** | -0.250*** | -0.311*** | -0.448*** | -0.677*** | -0.231*** | -0.281*** |
| | (0.047) | (0.062) | (0.050) | (0.067) | (0.071) | (0.090) | (0.060) | (0.066) | (0.049) | (0.063) | (0.095) | (0.155) | (0.056) | (0.070) |
| Perc. unemployed | 0.002 | 0.003 | 0.003 | 0.004 | 0.005 | 0.006 | -0.002 | -0.002 | 0.001 | 0.001 | -0.014** | -0.020** | 0.003 | 0.004 |
| | (0.003) | (0.004) | (0.004) | (0.005) | (0.005) | (0.006) | (0.004) | (0.005) | (0.003) | (0.004) | (0.007) | (0.010) | (0.004) | (0.005) |
| Perc. discouraged | -0.002 | -0.003 | 0.005 | 0.007 | -0.010 | -0.012 | -0.004 | -0.004 | -0.006 | -0.007 | -0.009 | -0.013 | -0.005 | -0.006 |
| | (0.005) | (0.006) | (0.005) | (0.006) | (0.007) | (0.009) | (0.006) | (0.006) | (0.005) | (0.006) | (0.009) | (0.014) | (0.005) | (0.007) |
| $\rho$ | 0.314*** | | 0.340*** | | 0.291*** | | 0.121 | | 0.280*** | | 0.479*** | | 0.257*** | |
| | (0.000) | | (0.000) | | (0.000) | | (0.148) | | (0.000) | | (0.000) | | (0.000) | |
| $\phi$ | 0.932*** | | 0.938*** | | 0.904*** | | 0.753*** | | 0.906*** | | 0.500 | | 0.893*** | |
| | (0.000) | | (0.000) | | (0.000) | | (0.001) | | (0.000) | | (0.204) | | (0.000) | |
| Pseudo $R^2$ | 0.14 | | 0.22 | | 0.09 | | 0.06 | | 0.14 | | 0.01 | | 0.12 | |
| $\chi^2$ | 294 | | 270 | | 142 | | 145 | | 307 | | 386 | | 203 | |
| Other controls | Yes | | Yes | | Yes | | Yes | | Yes | | Yes | | Yes | |
| N | 1,062 | | 1,062 | | 1,062 | | 1,062 | | 1,062 | | 1,062 | | 1,062 | |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are reported in parenthesis. Crime is assumed to be spatially correlated with the level of crime in a district's first-order neighbours. As for the errors, they are assumed to be correlated with those of all other neighbours, albeit inversely proportional to the distance between districts. The inequality index is based on a factor analysis of inequality in terms of water access, type of dwelling, type of toilet and type of fuel/energy for lighting, heating and cooking. All specifications include the following covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage Black, Coloured and Asian) and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet, percentage who have their rubbish removed by authorities, percentage with access to electricity and finally, percentage of households who own their dwelling.

The impact of housing inequality on residential burglaries is positive and significant. The direct and total effects are 0.086 and 0.105 log points, respectively. Similarly, an increase of one standard deviation in housing inequality triggers a 0.099 log points direct impact on all property crimes—the equivalent of 10.4 percent. In the case of thefts out of vehicles, the direct impact is 0.345 log points. The total impacts are 0.124 and 0.522 log points, respectively.[25] Education is also negatively related to property crimes across the board. Appendix A.4 shows that the results in Table 1.3 are robust to the choice of variables that are used to build the housing inequality index as well as to the use of a principal component analysis instead of factor analysis.[26]

In summary, the empirical results in this section show that higher inequality in terms of housing conditions is associated with higher crime, whether violent or property-related. While these are not estimations of the causal impact of housing inequality on crime, they do account for spillover effects across space as well as time and magisterial district fixed effects. This finding contributes to the literature on the relationship between inequality and crime by providing first evidence of this unexplored dimension of inequality, i.e., housing conditions. This is of high relevance for contexts where, like South Africa, access to adequate housing is limited and very unequally distributed.

## 1.6 Subsidised Housing, Inequality and Crime

Building on the observation of a positive relationship between housing inequality and crime, as documented in the previous section, we examine here the implications of housing subsidies for both inequality and crime. Since 1994, the government has introduced several subsidy schemes, and these subsidies have often changed nature and name.

At the outset, there were four schemes: project-linked, individual, consolidation and institutional subsidies (Department of Housing, 1997). Among these, the project-linked subsidy scheme has been the most prevalent. As of 2010, according to the database provided by the Western Cape Department of Environmental Affairs and Development Planning (2014), 72 percent of all government subsidy schemes were project-linked. As the purpose of different subsidies can be diverse, we focus on project-linked subsidies in our empirical analysis. This is because we know with certainty that project-linked subsidies were aimed to provide fully serviced housing units to people who had not benefited from other subsidy schemes before the RDP policy was put in place.[27]

[25] The relatively higher responsiveness of thefts out of vehicles to inequality may be explained by this crime's more opportunistic nature and ease of perpetration.

[26] In the order in which the indices are presented in Appendix A.4, the Kaiser-Meyer-Olkin measures of sampling adequacy are 0.83, 0.83, 0.76 and 0.68. Each analysis recommends only one factor or principal component. All factor loadings are positive.

[27] Project-linked subsidies were targeted to individuals who did not benefit from any previous housing subsidies. The final beneficiaries were assigned to housing units based on a waiting list. A serviced site is defined by the following minimum requirements: (i) a piped water supply with at least one stand pipe per 25 households; (ii) a properly functioning sanitation system for each household; and (iii) suitable access to each property and a storm water drainage system (Department of Human Settlements, 2009).

Initially, project-linked subsidies only consisted of projects implemented by external developers whose proposals were approved by the government. Developers were required to meet specific milestones to draw down on the contract value. This procedure explains why these projects were called "*Progress Payment Housing Projects*". Housing beneficiaries were granted subsidies as one-off grants based on their level of income. For the poorest, the subsidy would cover the cost of the house entirely, with the fully subsidised houses colloquially referred to as "*RDP houses*" (Tomlinson, 1999).

In 1998, a new subsidy scheme was introduced: the People's Housing Process (PHP). Under this scheme, beneficiaries would build, or manage the construction, of their own houses (Public Service Commission, 2003). In parallel, the scheme of the Progress Payment Housing Projects underwent important changes from 2001, with the provincial and municipal governments progressively taking over the role of developers (Department of Human Settlements, 2016). In 2007, the existing schemes were abolished (Gordon et al., 2011) and further reforms followed. From 2009, the provision of government housing was done under the new Integrated Residential Development Programme (IRDP).

To simplify the discussion, we will group the IRDP and its predecessor (the Progress Payment Housing Projects) under the same label "*IRDP*", while the PHP projects will remain a separate category. When we reference government housing, we mean projects under both the IRDP and PHP labels. Regardless of their name, the objective of these schemes has been the same: provide low-income South Africans with fully serviced housing units. Against a backdrop of large informal settlements, government houses are made from permanent materials, endowed with access to tap water, flush toilet facilities and electricity. Norms and standards for the construction of government housing were issued and revised over time (Tissington, 2011).

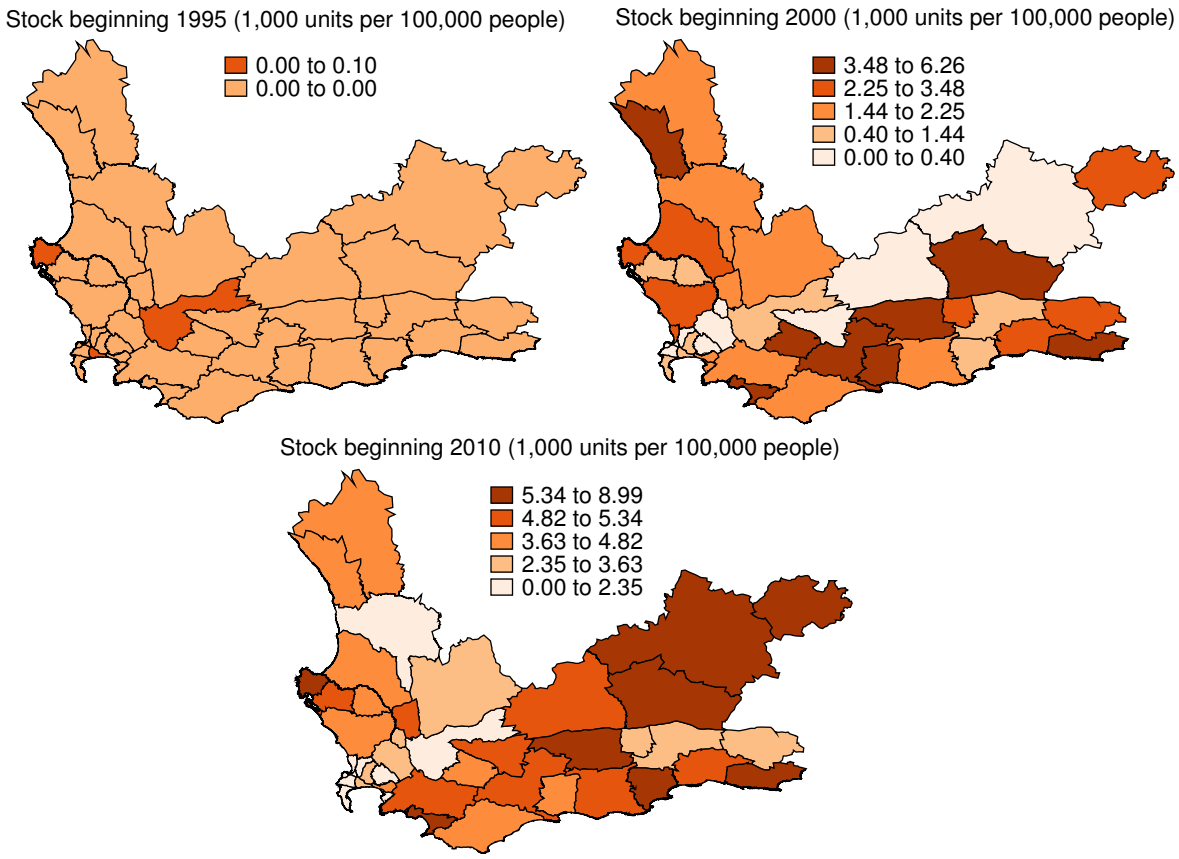
### 1.6.1 Government Housing and Inequality

In this section, we investigate whether the introduction and expansion of government subsidised housing has affected housing inequality. Due to data demands, the empirical analysis that follows will be limited to one South African province, the Western Cape.[28] We rely on a list of housing projects provided by the provincial Department of Environmental Affairs and Development Planning. The list is comprised of the geo-referenced location of housing projects, the year in which they were registered by the provincial Housing (Development) Board and their approved size.[29] This dataset covers projects that were registered between 1994 and 2009. We then compute a yearly average rate of housing execution per project between 1995 and 2008. We use data from Franklin (2020) to obtain the execution rate in Cape Town over this period and apply the same execution rates to the entire Western Cape Province. This gives us an estimated distribution of housing units across the provincial territory over the period of interest.

[28] This province includes one of the largest metropolitan areas in the country, Cape Town, and represents 11 and 12 percent of the country's surface and population, respectively.

[29] For 78 of 558 projects, we rely on the planned as opposed to approved size due missing information.

Figure 1.3: Stock of Government Housing in the Western Cape Province




Stock numbers include the units delivered by the following subsidy schemes: the Integrated Reconstruction and Development Programme (IRDP, which includes its predecessor project-linked subsidies) and the People's Housing Process (PHP). There are 42 magisterial districts in the Western Cape Province.

In order to link the housing data to the census years (1996, 2001 and 2011), our analysis relies on the stock of housing units at the beginning of 1995, 2000 and 2010. The mean stock of housing projects has increased from 0 to 2.08 thousand units per 100,000 people between 1995 and 2000, and it reached 4.22 thousand units per 100,000 people at the beginning of 2010. Stock numbers include the units delivered by IRDP, which includes its predecessor project-linked subsidies, and PHP. Figure 1.3 shows the estimated distribution of housing units across the magisterial districts of the Western Cape Province over the period of interest.

The data we use in the analysis implies three main caveats for the evaluation of the subsidy programme. First, as we limit the focus to the Western Cape Province, we only have 42 magisterial districts observed in three different years. As the resulting sample is small, it will reduce the precision of our estimates considerably. Second, the use of imputed rather than exact stocks of government housing introduces measurement error in the key independent variable and, as a consequence, a risk of attenuation bias under classical assumptions. Third, we cannot assume the absence of crime spillover effects between districts at either side of provincial borders. Therefore, we can no longer use the spatial model in our regression analysis and thus revert to the fixed effects estimation.

Table 1.4: Government Housing and Inequality

| | HOUSING INEQUALITY INDEX $\mu = 0$ and $\sigma = 1$ | | | | | |
|---|---|---|---|---|---|---|
| **Explanatory variables** | (1) | (2) | (3) | (4) | (5) | (6) |
| Lag 1 IRDP and PHP | -0.114*** | -0.040** | | | | |
| | (0.037) | (0.018) | | | | |
| Lag 2 IRDP and PHP | | | -0.127*** | -0.043** | | |
| | | | (0.037) | (0.018) | | |
| Lag 3 IRDP and PHP | | | | | -0.151*** | -0.050** |
| | | | | | (0.035) | (0.020) |
| Within $R^2$ | 0.85 | 0.97 | 0.85 | 0.97 | 0.86 | 0.97 |
| F | 47 | 129 | 45 | 129 | 58 | 132 |
| Other controls | I | II | I | II | I | II |
| N | 126 | 126 | 126 | 126 | 126 | 126 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Two-way fixed effects. Robust errors are reported in parenthesis. The reported explanatory variables are measured as 1,000 units per 100,000 people. The dependent variable is standardised. The inequality index is based on a factor analysis of MD-level inequality in terms of: water access, type of dwelling and toilet, type of fuel for cooking, heating and lighting. Integrated Reconstruction and Development Programme (IRDP) and the People's Housing Process (PHP). Group I includes the following set of covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage Black, Coloured and Asian), average education, percentage unemployed, percentage discouraged and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage who have their rubbish removed by authorities and percentage of households who own their dwelling of residence. In addition to Group-I variables, Group II further includes: percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet and percentage with access to electricity.

The benefits of housing projects may take time to materialise. Therefore, we estimate separate models for the housing stocks at different lags. Table 1.4 reports the coefficient estimates from regressing the housing inequality index on the lagged stock of subsidised units. It shows that an increase of 1,000 units per 100,000 people (roughly 0.45 of a standard deviation) is associated with a decrease in the housing inequality index between 0.114 and 0.151 standard deviations—columns (1), (3) and (5). When we include controls for the separate components of the inequality index, as we did in the previous section, the estimated coefficients fall to values between $-0.04$ and $-0.05$. We suggest that the magnitude of the true effect of the housing projects on inequality lies between the values estimated in the two alternative specifications, as some of the variation in the variables defining the index may not be related to government subsidies.

### 1.6.2 Government Housing and Crime

In this section, we estimate the effect of the housing projects on crime rates net of district and time fixed effects, as well as controlling for a variety of observable variables that are related to the development of government housing, e.g., prevalence of informal housing,

limited access to public amenities, etc.[30] Table 1.5a and 1.5b report the estimated effects on violent and property crimes, respectively.

Table 1.5a shows that government housing is negatively related to violent crimes.[31] An increase of 1,000 housing units per 100,000 people is associated with a decrease of 0.053 to 0.062 log points in the rate of overall violent crimes—columns (2), (4) and (6). This is the equivalent of a 5–6 percent decrease. The coefficients are larger in magnitude for higher lags of the housing stock. This provides suggestive evidence that it may take time for the benefits of housing subsidies to materialise.

The results in Table 1.5a also show that when we include both housing inequality and government housing in the estimation—columns (3), (5) and (7)—, the coefficients on the stock of housing projects decrease in magnitude. While the magnitude change is not significant, the coefficients do lose significance. As discussed, this may be due to the lower precision of the estimates as a consequence of the much smaller sample size for the Western Cape Province. However, this finding is consistent with our hypothesis that improvements in the housing stock affect crime through a decrease in housing inequality. This is also visible when comparing the effect of housing inequality on crime with and without controlling for subsidised housing, as shown in the top row of Table 1.5a—column (1) versus columns (3), (5) and (7).[32]

In contrast to violent crimes, we do not find a significant link between the IRDP-PHP projects and property crimes in Table 1.5b. In addition, the coefficients on housing inequality do not vary as much between column (1) and the other columns of Table 1.5b.

### 1.6.3 Discussion

Strands of the economic and sociological literatures suggest that social interventions can limit strain, either directly through the achievement of positively valued goals or indirectly via reduced inequality. This literature also suggests that reduced inequality and strain are associated with lower levels of crime, particularly violent crimes (Shaw & McKay, 1942; Agnew, 1992, 1999, 2001).

The empirical results presented in this study are consistent with this literature. In particular, we add evidence regarding an understudied policy intervention: housing subsidies. We showed that government housing is associated with lower housing inequality (Table 1.4) and suggested that this lower inequality helps mitigate crime (Table 1.5a), consistent with strain theory. Meth & Charlton (2017) report that male beneficiaries of government housing in South Africa were enabled to achieve their aspirations, which are not limited to tangible assets, i.e., a house, but also in terms of masculinity, social status, self-esteem and respect, all of which are correlated with the individual's transitioning from the state

[30] Since housing projects are more likely to be developed in areas of increased crime over time, it is possible that our specification underestimates the impact of government housing on crime.

[31] To increase power, we pool all violent crimes, but note that the effect is strongest on aggravated assaults.

[32] We can reject the null of coefficient equality between column (1) and columns (3) and (5) at the 10 percent level and we just about fail to reject the null for specification (7), as the p-value is 0.11.

Table 1.5a: Government Housing and Violent Crimes

| | ALL VIOLENT CRIMES Log of crime per 100,000 people | | | | | | |
|---|---|---|---|---|---|---|---|
| **Explanatory variables** | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Housing inequality index | 0.323* | | 0.236 | | 0.228 | | 0.207 |
| | (0.192) | | (0.200) | | (0.204) | | (0.218) |
| Lag 1 RDP and PHP | | -0.053** | -0.044 | | | | |
| | | (0.026) | (0.028) | | | | |
| Lag 2 RDP and PHP | | | | -0.056* | -0.046 | | |
| | | | | (0.029) | (0.031) | | |
| Lag 3 RDP and PHP | | | | | | -0.062** | -0.051 |
| | | | | | | (0.029) | (0.033) |
| Avg. education (years) | -0.685** | -0.686** | -0.667** | -0.680** | -0.663** | -0.659** | -0.647** |
| | (0.277) | (0.284) | (0.274) | (0.282) | (0.273) | (0.281) | (0.274) |
| Perc. unemployed | -0.001 | 0.002 | -0.000 | 0.002 | 0.001 | 0.002 | 0.001 |
| | (0.009) | (0.008) | (0.009) | (0.009) | (0.009) | (0.009) | (0.009) |
| Perc. discouraged | -0.008 | 0.000 | 0.007 | -0.006 | 0.001 | -0.017 | -0.008 |
| | (0.026) | (0.027) | (0.028) | (0.026) | (0.027) | (0.026) | (0.027) |
| Within $R^2$ | 0.72 | 0.72 | 0.72 | 0.72 | 0.73 | 0.72 | 0.73 |
| F | 54 | 53 | 46 | 52 | 47 | 56 | 54 |
| Other controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 126 | 126 | 126 | 126 | 126 | 126 | 126 |
| *Inequality index* | | | *(3) - (1)* | | *(5) - (1)* | | *(7) - (1)* |
| Wald test† $\chi^2$ | | | 2.68 | | 2.65 | | 2.51 |
| P-value | | | 0.10 | | 0.10 | | 0.11 |
| *IRDP-PHP* | | | *(3) - (2)* | | *(5) - (4)* | | *(7) - (6)* |
| Wald test‡ $\chi^2$ | | | 1.21 | | 1.19 | | 0.99 |
| P-value | | | 0.27 | | 0.27 | | 0.32 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Two-way fixed effects. Robust errors are reported in parenthesis. The inequality index is based on a factor analysis of MD-level inequality in terms of: water access, type of dwelling and toilet, type of fuel for cooking, heating and lighting. Integrated Reconstruction and Development Programme (IRDP) and the People's Housing Process (PHP). The housing stock is measured as 1,000 units per 100,000 people. All specifications include the following covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage black, Coloured and Asian) and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet, percentage who have their rubbish removed by authorities, percentage with access to electricity and finally, percentage of households who own their dwelling.

†The first *Wald* test checks whether the housing index is statistically the same with or without controlling for the IRDP-PHP stock. For instance, inequality in (3), (5) and (7) is tested against inequality in (1). H0: coefficients are the same across the different regressions. H1: coefficients are different.

‡The second *Wald* test checks whether the impact of the IRDP-PHP stock is statistically the same with or without the inclusion of the inequality index. For instance, the IRDP-PHP stock in (3), (5) and (7) is tested against inequality in (2), (4) and (6). The tests are done using seemingly unrelated Least Square Dummy Regressions with errors clustered at MD level.

Table 1.5b: Government Housing and Property Crimes

| | ALL PROPERTY CRIMES Log of crime per 100,000 people | | | | | | |
|---|---|---|---|---|---|---|---|
| **Explanatory variables** | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Housing inequality index | 0.228 | | 0.224 | | 0.217 | | 0.196 |
| | (0.169) | | (0.174) | | (0.174) | | (0.186) |
| Lag 1 RDP and PHP | | -0.011 | -0.002 | | | | |
| | | (0.027) | (0.028) | | | | |
| Lag 2 RDP and PHP | | | | -0.015 | -0.006 | | |
| | | | | (0.028) | (0.029) | | |
| Lag 3 RDP and PHP | | | | | | -0.024 | -0.014 |
| | | | | | | (0.027) | (0.030) |
| Avg. education (years) | -0.574** | -0.590** | -0.573** | -0.586** | -0.571** | -0.574** | -0.563** |
| | (0.271) | (0.282) | (0.272) | (0.281) | (0.272) | (0.280) | (0.274) |
| Perc. unemployed | -0.003 | -0.002 | -0.003 | -0.002 | -0.003 | -0.001 | -0.003 |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) |
| Perc. discouraged | -0.005 | -0.011 | -0.005 | -0.012 | -0.004 | -0.014 | -0.005 |
| | (0.028) | (0.025) | (0.026) | (0.025) | (0.027) | (0.027) | (0.028) |
| Within $R^2$ | 0.70 | 0.70 | 0.70 | 0.70 | 0.70 | 0.70 | 0.71 |
| F | 26 | 26 | 24 | 25 | 24 | 26 | 25 |
| Other controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 126 | 126 | 126 | 126 | 126 | 126 | 126 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Two-way fixed effects. Robust errors are reported in parenthesis. The inequality index is based on a factor analysis of MD-level inequality in terms of: water access, type of dwelling and toilet, type of fuel for cooking, heating and lighting. The housing stock is measured as 1,000 units per 100,000 people. All specifications include the following covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage black, Coloured and Asian) and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet, percentage who have their rubbish removed by authorities, percentage with access to electricity and finally, percentage of households who own their dwelling.

of informal dweller to that of homeowner. Since men are the main perpetrators of violent crimes (Centre for the Study of Violence and Reconciliation, 2007), their capacity to achieve standard goals or socially constructed aspirations may mitigate collective strain, which can reduce frustrations and limit impulsive, anger-led outbursts of violence. Strain theory may also help explain the observed differential effects of subsidised housing on violent crimes versus property crimes (Table 1.5a versus Table 1.5b). In contrast to violent crimes, it is less likely that reductions in strain, anger and frustrations will play a key role in the mitigation of financially motivated crimes. Anger is central in strain theory's explanation of crime (Agnew, 2001), and there is evidence from criminology that anger chiefly impacts violent crimes (Aseltine et al., 2000; Mazerolle et al., 2003; Piquero & Sealock, 2000).

While government housing can affect crime indirectly via reduced inequality and strain, we also acknowledge that direct links are possible. A dwelling made from permanent materials provides enhanced protection against intruders, and access to in-house tap water and toilet facilities reduces the probability of victimization, as the need to leave the household to fetch water or use shared toilet facilities is eliminated. However, the existing theoretical literature as well as the results of this section lead us to suggest that inequality plays a substantial role in explaining the link between government housing and crime.

### 1.6.4 Falsification Test

To probe the plausibility of the empirical mechanism we suggested above, we explore a different policy intervention whose timing overlapped with that of the housing subsidies. This provides us with a simple falsification test. The policy intervention is the old-age pension introduced by the Social Assistance Act of 1992. Both the housing and pension schemes have been implemented in post-apartheid South Africa with an explicit attempt to mend the inequalities created by the racialised policies of the previous regime.

Pension eligibility is based on age and income. In 1996 and 2001, women aged 60 or above and men aged 65 or above were eligible to receive the pension if they were below a specified income threshold. In 2008, the law was amended to equalise age eligibility across genders. In 2011, both women and men were eligible to receive pensions as of their 60th birthday conditional on meeting the income criterion. The South African old-age pension scheme has been studied extensively. Studies in the literature have shown that the programme had a variety of effects on the most disadvantaged groups in the population.[33] Black and Coloured populations have been the main beneficiaries of the policy, with few Whites earning below the income threshold (Case & Deaton, 1998). That is, similar to subsidised housing, the old-age pension has reached a large number of low-income beneficiaries against the background of apartheid-inherited socio-economic inequalities.

[33]Duflo (2000, 2003) shows a positive effect on anthropometric indicators for girls. Other studies have estimated the impact on the labour supply of both the elderly and their working-age household co-residents (Ranchhod, 2006; Abel, 2019b). Finally, there is also evidence that the pension scheme had an effect on household composition and decision-making processes within households (Hamoudi & Thomas, 2014; Ambler, 2016).

We obtain variation in the intensity of the policy across districts by using the percentage of age-eligible individuals.[34] The results in Table 1.6 show that the proportion of people eligible for the pension does not have a significant effect on the housing inequality index.

Table 1.6: Old-Age Pension Eligibility and Housing Inequality

| | **HOUSING INEQUALITY INDEX** | |
|---|---|---|
| | $\mu = 0$ and $\sigma = 1$ | |
| **Explanatory variable** | (1) | (2) |
| Perc. eligible pension | -0.042 | -0.035 |
| | (0.060) | (0.036) |
| Within $R^2$ | 0.82 | 0.97 |
| F | 31 | 122 |
| Other controls | I | II |
| N | 126 | 126 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Two-way fixed effects. Robust errors are reported in parenthesis. The dependent variable is standardised. The inequality index is based on a factor analysis of MD-level inequality in terms of: water access, type of dwelling and toilet, type of fuel for cooking, heating and lighting. The percentage eligible for old-age pensions is computed considering the Black and Coloured populations that meet the age criterion. Group I includes the following set of covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage Black, Coloured and Asian), average education, percentage unemployed, percentage discouraged and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage who have their rubbish removed by authorities and percentage of households who own their dwelling of residence. In addition to Group-I variables, Group II further includes: percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet and percentage with access to electricity.

In addition, we show in Table 1.7 that our proxy for pension eligibility does not show any evidence of a negative effect on crime either. If anything, the point estimate suggests a positive correlation between the prevalence of pension-eligible individuals and crime. Overall, this simple falsification test supports our interpretation of the main results in this study. That is, the negative relationship between subsidised housing projects and crime is not spurious and is suggestive of an inequality-mediated effect of housing subsidies.

## 1.7 Conclusion

This study adds to the literature on the link between inequality and crime. While most existing studies focus on inequality in income or consumption, we explore disparities in terms of housing conditions. We show that variations in housing conditions in a highly

[34] In Tables 1.6 and 1.7, we use the district-level percentage of age-eligible African Blacks and Coloured. On average, approximately 5 percent of individuals in a magisterial district are eligible for the pension. We also run sensitivity regressions using (i) African Blacks, Coloureds and Asians, (ii) African Blacks only, (iii) Coloureds only, (iv) all population groups. The implications of the test remain unchanged.

Table 1.7: Old-Age Pension Eligibility and Crime

| | **ALL VIOLENT CRIMES Log of crime per 100,000 people** | | **ALL PROPERTY CRIMES Log of crime per 100,000 people** | |
|---|---|---|---|---|
| **Explanatory variables** | (1) | (2) | (3) | (4) |
| Housing inequality index | | 0.378* | | 0.259 |
| | | (0.201) | | (0.198) |
| Perc. eligible pension | 0.075 | 0.088 | 0.040 | 0.049 |
| | (0.066) | (0.065) | (0.061) | (0.063) |
| Avg. education (years) | -0.697** | -0.655** | -0.586** | -0.557** |
| | (0.277) | (0.256) | (0.269) | (0.254) |
| Perc. unemployed | -0.001 | -0.003 | -0.003 | -0.005 |
| | (0.009) | (0.009) | (0.010) | (0.010) |
| Perc. discouraged | -0.005 | 0.016 | -0.006 | 0.008 |
| | (0.027) | (0.033) | (0.029) | (0.034) |
| Within $R^2$ | 0.72 | 0.73 | 0.70 | 0.71 |
| F | 74 | 69 | 29 | 25 |
| Other controls | Yes | Yes | Yes | Yes |
| N | 126 | 126 | 126 | 126 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Two-way fixed effects. Robust errors are reported in parenthesis. The inequality index is based on a factor analysis of inequality in terms of: water access, type of dwelling and toilet, type of fuel for cooking, heating and lighting. The percentage eligible for old-age pensions is computed considering the Black and Coloured populations that meet the age criterion. All specifications include the following covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage Black, Coloured and Asian) and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet, percentage who have their rubbish removed by authorities, percentage with access to electricity and finally, percentage of households who own their dwelling.

unequal, emerging economy explain a significant share of most types of crime. An increase of one standard deviation in housing inequality explains around 12 percent of crime increases.

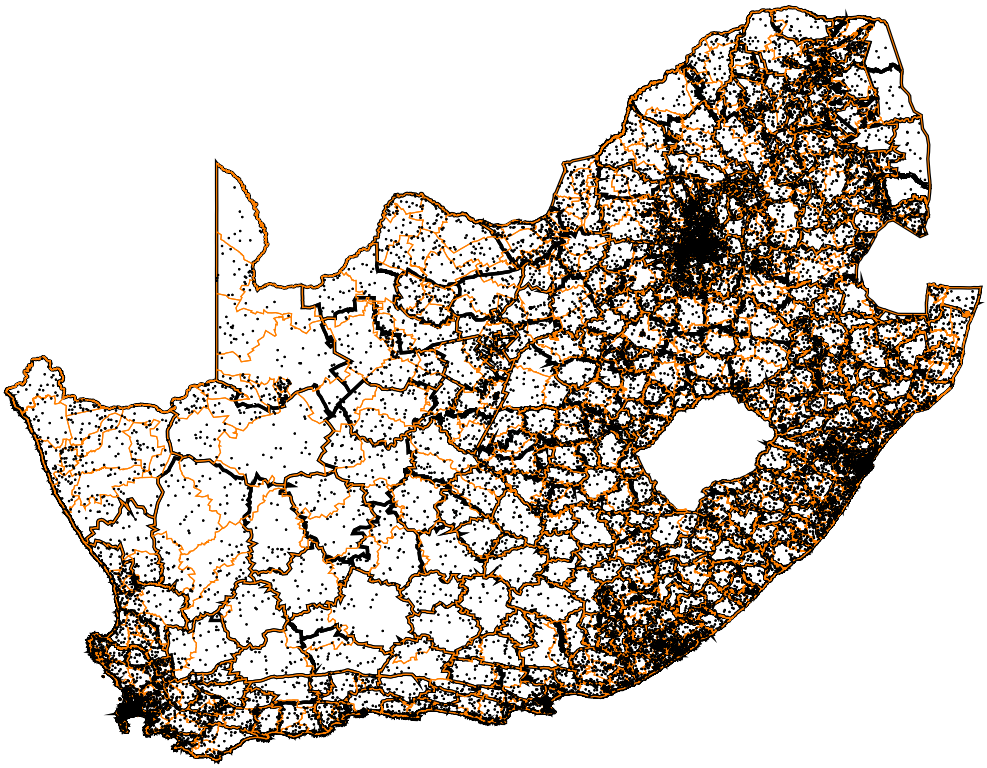
Different dimensions of inequality may matter differently in developing countries as compared to high-income economies (Demombynes & Özler, 2005; Enamorado et al., 2016). Our findings expand the limited available evidence for developing countries. In particular, we collect and merge data from different sources in South Africa, which allows us to account for key confounding factors as well as quantify the magnitude of spillover effects, which amount to 25–35 percent of a district's own crime levels.

Crime is a prominent issue in South Africa. Successive governments since the democratic transition have tried different policy measures—both direct and indirect—to reduce it. We show that a housing programme aimed at fast-tracking socio-economic development may have had the indirect effect of mitigating violent crimes by partially reducing inequality. As argued by Kelly (2000), different economic and sociological theories may explain different types of crime. In particular, property crimes may be best explained by cost-benefit analyses, while violent crimes are better understood from a strain or social disorganization perspective. We argue that strain theory can help interpret our results on the relationship between subsidised housing, housing inequality and the incidence of violent crimes. We suggest that housing subsidies may reduce violent crimes by alleviating the levels of strain that have historically been conducive to violence.

Finally, our study contributes to the wider literature investigating the relationship between social protection policies, e.g., educational policies, labour market interventions, government housing, and crime. Most of these programmes are meant to lift people out of poverty and ensure a more equal distribution of socio-economic means and opportunities. These measures are likely to be most effective at reducing crime. It may be superfluous to note that these types of policies are not incompatible with actions aimed at improving the criminal justice and law enforcement systems. In fact, it is reasonable to assume that a larger set of complementary policies may be necessary to obtain long-lasting reductions in crime rates.

# A Appendix

## A.1 Merger Between Magisterial Districts and Police Districts



The black, thick contour is that of the magisterial districts (MDs). The orange, thin contour describes the police districts. Each police district is populated with several randomly generated points. For police districts that cross several MDs, of the total number of points, we count the points that fall in each MD and we distribute the crimes of that police district to its respective MDs proportionally.

## A.2 Housing Inequality, Factor Analysis

| Inequality in terms of: | Factor Loadings | Uniqueness |
|---|---|---|
| Type of dwelling | 0.65 | 0.46 |
| Access to water | 0.72 | 0.44 |
| Type of toilet | 0.78 | 0.39 |
| Type of energy for lighting | 0.75 | 0.31 |
| Type of fuel for cooking | 0.91 | 0.15 |
| Type of fuel for heating | 0.76 | 0.30 |

The analysis is done based on the full sample of 1,062 observations and standardised inequality measures. Vector 1 is the only factor with an eigenvalue greater than 1, namely 3.52. The Kaiser-Meyer-Olkin measure of sampling adequacy is 0.83. This value is very encouraging and recommends the use of factor analysis to construct an index to describe inequality in terms of housing conditions.

## A.3 Negative Binomial Estimates

| | VIOLENT CRIMES Crime incidence per 100,000 people | | | | PROPERTY CRIMES Crime incidence per 100,000 people | | |
|---|---|---|---|---|---|---|---|
| | **All Violent Crimes** | **Aggravated Assaults** | **Murders** | **Rapes** | **All Property Crimes** | **Thefts out of Vehicles** | **Residential Burglaries** |
| **Explanatory variables** | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Housing inequality index | 0.107*** | 0.125*** | -0.035 | 0.145*** | 0.091*** | 0.324*** | 0.054* |
| | (0.026) | (0.029) | (0.040) | (0.030) | (0.028) | (0.049) | (0.031) |
| Avg. education (years) | -0.034 | -0.013 | -0.010 | 0.016 | -0.128*** | -0.089 | -0.090** |
| | (0.038) | (0.040) | (0.053) | (0.043) | (0.038) | (0.058) | (0.042) |
| Perc. unemployed | -0.005* | -0.005 | -0.003 | -0.004 | -0.004 | -0.017*** | -0.002 |
| | (0.003) | (0.003) | (0.005) | (0.003) | (0.003) | (0.006) | (0.004) |
| Perc. discouraged | -0.002 | 0.004 | -0.014** | 0.000 | -0.004 | -0.006 | -0.003 |
| | (0.004) | (0.005) | (0.007) | (0.005) | (0.004) | (0.007) | (0.005) |
| $\chi^2$ | 778 | 565 | 651 | 304 | 910 | 686 | 469 |
| Other controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 1,062 | 1,062 | 1,062 | 1,062 | 1,062 | 1,062 | 1,062 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Negative binomial two-way fixed effects estimates. Standard errors are reported in parenthesis. The dependent variable is the incidence of crime at the level of MDs per 100,000 people. The coefficients have not been exponentiated. All specifications include the following covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage black, Coloured and Asian) and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet, percentage who have their rubbish removed by authorities, percentage with access to electricity and finally, percentage of households who own their dwelling.

## A.4 Robustness to Variations of the Inequality Index

| | VIOLENT CRIMES Log of crime per 100,000 people | | | | | | | | PROPERTY CRIMES Lof of crime per 100,000 people | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | All Violent Crimes | | Aggravated Assaults | | Murders | | Rapes | | All Property Crimes | | Thefts out of Vehicles | | Residential Burglaries | |
| | (1) | | (2) | | (3) | | (4) | | (5) | | (6) | | (7) | |
| **Variations index specification** | Direct | Total | Direct | Total | Direct | Total | Direct | Total | Direct | Total | Direct | Total | Direct | Total |
| **Benchmark: FA on type of dwelling, access to water, type of toilet and fuel/energy for lighting, cooking and heating** | | | | | | | | | | | | | | |
| Housing inequality index | 0.085*** | 0.109*** | 0.094*** | 0.124*** | -0.030 | -0.038 | 0.124*** | 0.135*** | 0.099*** | 0.124*** | 0.345*** | 0.522*** | 0.086** | 0.105** |
| | (0.029) | (0.037) | (0.031) | (0.041) | (0.044) | (0.055) | (0.037) | (0.040) | (0.030) | (0.038) | (0.058) | (0.093) | (0.035) | (0.043) |
| | (8) | | (9) | | (10) | | (11) | | (12) | | (13) | | (14) | |
| **PCA on type of dwelling, access to water, type of toilet and fuel/energy for lighting, cooking and heating** | | | | | | | | | | | | | | |
| Housing inequality index | 0.099*** | 0.126*** | 0.122*** | 0.158*** | -0.014 | -0.018 | 0.147*** | 0.160*** | 0.108*** | 0.133*** | 0.371*** | 0.549*** | 0.103*** | 0.125*** |
| | (0.030) | (0.039) | (0.032) | (0.042) | (0.046) | (0.057) | (0.038) | (0.041) | (0.032) | (0.039) | (0.061) | (0.092) | (0.036) | (0.044) |
| | (15) | | (16) | | (17) | | (18) | | (19) | | (20) | | (21) | |
| **FA on type of dwelling, access to water, type of toilet and fuel/energy for lighting** | | | | | | | | | | | | | | |
| Housing inequality index | 0.109*** | 0.139*** | 0.156*** | 0.200*** | -0.008 | -0.010 | 0.159*** | 0.173*** | 0.101*** | 0.124*** | 0.423*** | 0.610*** | 0.105*** | 0.127*** |
| | (0.032) | (0.040) | (0.034) | (0.043) | (0.048) | (0.060) | (0.040) | (0.043) | (0.033) | (0.041) | (0.066) | (0.095) | (0.038) | (0.045) |
| | (22) | | (23) | | (24) | | (25) | | (26) | | (27) | | (28) | |
| **FA on type of dwelling, access to water and type of toilet** | | | | | | | | | | | | | | |
| Housing inequality index | 0.098*** | 0.125*** | 0.148*** | 0.190*** | 0.024 | 0.030 | 0.125*** | 0.138*** | 0.081** | 0.101** | 0.304*** | 0.458*** | 0.084** | 0.102** |
| | (0.031) | (0.040) | (0.033) | (0.043) | (0.047) | (0.059) | (0.039) | (0.044) | (0.033) | (0.041) | (0.064) | (0.098) | (0.037) | (0.045) |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each cell is a different regression: 7 dependent variables $\times$ 4 index measurements (including the benchmark specification). Errors are reported in parenthesis. Fixed effects estimates. Crime is assumed to be spatially correlated with the level of crime in a district's first-order neighbours. As for the errors, they are assumed to be correlated with those of all other neighbours, albeit inversely proportional to the distance between districts. All specifications include the following covariates: year dummies, density (1,000 people per $km^2$), MD-level averages of individual characteristics such as population-group (percentage black, Coloured and Asian), education, percentage unemployed, percentage discouraged and percentage individuals who have moved in the past 10 years, and MD-level averages of household characteristics such as average size, average number of rooms, percentage living in an informal dwelling, percentage with access to water on the household's premises, percentage who own a flush or chemical toilet, percentage who have their rubbish removed by authorities, percentage with access to electricity and finally, percentage of households who own their dwelling.

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# Chapter 2

# School Feeding Programmes, Education and Food Security in Rural Malawi


## Abstract

Existing investigations of the impact of school feeding programmes on educational outcomes have provided mixed evidence of success. In this chapter, I investigate a potential explanation for this lack of consensus in the literature. I argue that the prevailing food security situation at the time and place of the programme's evaluation plays a major role. I study the case of rural Malawi. I use an instrumental variable approach and propensity score matching to estimate the impact of school feeding on primary school enrolment and retention rates. I focus on villages with overlapping characteristics. I estimate that school feeding has increased enrolments by 7 percentage points on average, but the impact on retention rates has been relatively limited. However, when I distinguish between food-secure and food-insecure areas, not only do I find a larger impact on enrolments in food-insecure areas, but I also uncover a significant increase of around 2 percentage points in the retention rate of students in these same areas. Across the board, impacts are not significant in food-secure areas. I conclude that school feeding programmes bear an impact on education as long as they also intervene to relax a binding food constraint.



**JEL classification**: I21, I38.

## 2.1 Introduction

Evidence concerning the impact of school feeding programmes on educational outcomes is mixed. Some researchers find positive and significant effects for at least *some* indicators (Jacoby et al., 1998; Powell et al., 1998; Ravallion & Wodon, 2000; Kremer & Vermeersch, 2005; Kazianga et al., 2009; Meng & Ryan, 2010; Alderman et al., 2012; Kazianga et al., 2012; Nikiema, 2019), while others find no impact at all or very modest effects (Tan et al., 1999; Buttenheim et al., 2011; Adrogue & Orlicki, 2013; McEwan, 2013; Azomahou et al., 2019). I investigate how one dimension of within-country variation across time and space can impact the outcomes of school feeding evaluations and can thus help explain the lack of consensus in the literature.

Specifically, I assess the magnitude and heterogeneity of school feeding impacts on the extensive and intensive margins of education, i.e., enrolment and retention rates. My hypothesis is that the prevailing food security situation at the time of the evaluation, in the areas where school feeding is implemented can predict whether school feeding will boost or leave enrolment and retention rates unaffected. Singh et al. (2014) have put forward a similar hypothesis with respect to the impact of school feeding on nutrition. However, the authors look at drought from a historical perspective, and they rely on self-reported exposure to drought. In contrast, I use a contemporaneous measure of food insecurity, which is the result of institutional evaluations of food availability, to provide insights into the interactions between local food insecurity and the effectiveness of school feeding. I complement the main analysis with secondary investigations into age at enrolment and the grade-for-age gap.

I combine Malawi's Third and Fourth Integrated Household Surveys with localised information on food insecurity from the Famine Early Warning Systems Network (FEWS-NET). I implement an instrumental variable approach at the individual level to estimate the local average treatment effect of school feeding on educational outcomes. I instrument the endogenous individual-level treatment with village-level treatment, which is considered to be as good as random *conditional* on the inclusion of the covariates that inform the targeting of villages. In addition, I also run a reduced-form specification at the individual level and a propensity score matching analysis at the community level to estimate the average intention to treat effect.

Controlling for district and time effects as well as individual, household and village characteristics, I find that school feeding has chiefly attracted new enrolments. However, its impact on dropouts is less clear. School feeding has improved the extensive margin of schooling by 6–7 percentage points on average, but there is no significant impact on the intensive margin. When I distinguish between food-secure and food-insecure areas, however, not only do I find a larger impact on the extensive margin of schooling in food-insecure areas, but I also uncover a significant 2 percentage point increase in the intensive margin of schooling in food-insecure areas in a sample of communities with overlapping characteristics. I call this the *overlap sample*.

Reduced-form regressions confirm the above pattern. The likelihood of being in school is roughly 2 percentage points higher for children in treated villages than it is for children in control communities. This magnitude is further increased in food-insecure areas by 0.6–1.0 percentage points. As for the intensive margin of education, children in treated food-insecure villages are 1 percentage point more likely to remain in school when compared to control children. Moreover, propensity score matching estimations at the community level yield similar results. Had all food-insecure communities received school feeding, then the extensive margin of schooling would have been 2–3 percentage points greater than in a scenario where no community was treated. Across the board, there are no significant impacts in food-secure communities.

I conclude that school feeding improves enrolment and retention rates as long as there is a binding food constraint to relax. Moreover, increases in enrolment appear to be more sensitive to school feeding than are retention rates. This raises the question of whether children might not be enrolling in school only temporarily when food is scarce and droughts reduce the demand for their time in agriculture. I argue that school feeding is best implemented alongside a package of interventions to improve educational outcomes regardless of environmental factors. However, even if the impact of school feeding on enrolment and retention rates is occasionally limited, the programme can still improve nutrition for those children who would anyway attend school, which in turn can increase their school performance. There is evidence that school feeding is associated with improved cognitive and nutritional outcomes for children attending Grade 1 in Malawi during the academic year 2010–11, which was minimally affected by food insecurity (Nkhoma et al., 2013).

The impact heterogeneity that is documented in this study strengthens the argument that school feeding should be directed at the most vulnerable of areas in Malawi. The scaling up of school feeding to cover everyone regardless of their socio-economic situation is likely to lead to a watered-down average impact. However, if the target is a moving one, e.g., communities oscillate between food security and insecurity, then casting a wider net can ensure preparedness when food insecurity is widespread.

The findings of this article help to interpret the conflicting evidence on school feeding, enrolment and retention rates. For instance, Kazianga et al. (2012) evaluate a food-for-education experiment in Burkina Faso and estimate a 4 percentage point average intention to treat effect on enrolment rates. Similarly, Ravallion & Wodon (2000) and Meng & Ryan (2010) find a positive impact of 15–21 percentage points on school participation in Bangladesh. Finally, Alderman et al. (2012) estimate that food-for-education interventions in Uganda have chiefly led to the enrolment of children who were out of school at baseline. Among these children, those exposed to treatment were 9 percentage points more likely to be in school at endline. In contrast, Azomahou et al. (2019) find that school meals have not impacted enrolment or retention rates in Senegal. Similarly, Tan et al. (1999) and Buttenheim et al. (2011) also find no impact on enrolment or dropout rates in the case of the Philippines and Laos. This article identifies one important reason why the same programme can lead to both significant and insignificant results; namely, the prevailing food security situation at the time and place of the programme's evaluation.

## 2.2 Theory of School Feeding

School feeding programmes can be classified as an in-kind conditional welfare transfer whose aim is to incentivise households to invest in the education of their children, i.e., enrolling and keeping them in school. Thus, school feeding programmes can be ascribed to the category of demand-side interventions in education. However, if we move away from the premise that school feeding is mainly meant to increase attendance, enrolment and retention rates, and instead consider that school feeding can also improve performance, as well-nourished children are better able to learn and participate in school, then school feeding can also be regarded as a school input. This means that school feeding can be treated as a supply-side intervention. Nevertheless, since the focus of this study falls on enrolment and retention rates, then the classification of school feeding programmes as a demand-side intervention is in order.

As a demand side intervention, the impact of school feeding programmes can be analysed using a cost-benefit analysis (Becker, 1962; Machin & Stevens, 2004; Jimenez & Patrinos, 2008; Glewwe & Muralidharan, 2016), per which parents decide to invest in the education of their children if the present value of the benefits associated with better-educated children is greater than the current cost of sending children to school. Households incorporate individual benefits into their decision-making process; however, they are unlikely to also consider the social benefits of additional schooling. Thus, their decisions may not be optimal for societies as a whole (Machin & Stevens, 2004; Jimenez & Patrinos, 2008). Consequently, governments and their development partners have the incentive to intervene in this context. They can rely on two types of interventions to incentivise investments in education. They can either reduce the cost of schooling or increase the benefits of education.

Costs can be direct, such as fees, uniforms and supplies, or indirect, such as lost labour, whether paid or unremunerated chores and agricultural tasks. Benefits generally incorporate the present value of future income from skilled employment. Though smaller in magnitude, benefits can also be immediate if, for instance, regular school attendance is rewarded. School feeding is a case in point. The magnitude of the impact, however, is not the same for everyone. The provision of school meals can be invaluable to food-insecure, budget-constrained households. But it can also mean very little to households that were already effortlessly providing meals to their children. This is a consequence of the nature of the intervention, which is an in-kind transfer that implicitly assumes what the problem is and gives a one-size-fits-all type of solution. If the assumption is faulty and the household is *not* facing a binding food constraint, then an otherwise well-intended intervention can have limited impact. This is the main theoretical reason why it may be cost-effective to target only the most vulnerable of areas or schools against the background of limited public resources.

Consequently, I hypothesise that for school feeding to have an impact in terms of educational outcomes, the programme must be implemented in an area that is facing both binding food constraints *and* educational gaps. If there are no binding food constraints,

then households have no additional incentive to send their children to school following the implementation of school feeding programmes. In this case, school feeding offers a solution, i.e., complementary meals, to a non-existent problem. Therefore, the prevailing local food security situation can explain why the impact of school feeding on enrolment and attendance can be limited on occasions.

## 2.3 Context

### 2.3.1 Education, Rural Malawi

The net enrolment rate among children aged 6 to 14 years old has increased from 84 percent in 2010 to 90 percent in 2016 in Malawi (National Statistical Office, 2017). These statistics are encouraging, but there is still room for improvement—especially after seeing how enrolment rates have decreased from 90 percent in 2016 to 88 percent in 2017, in the aftermath of the flooding and drought events of 2016 (UNICEF, 2019). Moreover, educational outcomes vary substantially between urban and rural Malawi. The difference between primary school *completion* rates in urban and rural Malawi was of 34 percentage points around the time when school feeding impacts are observed and evaluated in this study (Ministry of Education, Science and Technology, 2014).

I restrict this study to rural Malawi to keep a homogeneous sample of households. Nevertheless, only a small part of the data is lost as more than 80 percent of Malawians live in rural areas (National Statistical Office, 2019). Rural areas are also the most pertinent setting to discuss educational outcomes and school feeding against the background of food insecurity that is brought on by natural causes. Malawi's rural population is overly-reliant on small-scale, rain-fed agriculture; therefore, households are vulnerable to climate variability and change. It does not help that Malawi is prone to floods and droughts or that their frequency and spread have intensified in the past decades. Finally, population growth and environmental degradation will also continue to aggravate the impacts of the aforementioned natural disasters (Government of Malawi, 2016).

### 2.3.2 School Feeding Programmes in Malawi

Malawi's experience with school feeding programmes pre-dates the efforts of the government to regulate these interventions. For instance, the Department of School Health and Nutrition, which provides guidance on health and nutrition-related interventions, was established in 2007. However, school feeding in Malawi dates back to the late 1990s, when the World Food Programme first implemented a pilot project in the country (Government of Malawi, 2009). The other important entity implementing school feeding in Malawi is Mary's Meals. Their work started in 2002.

As of 2015, Malawi counted 5,864 primary schools (UNICEF, 2018). The World Food Programme worked with 783 of these schools in 13 districts (Webb et al., 2018). Mary's Meals catered to 635 additional schools in 20 districts as of 2016 (McMahon, 2016). Malawi has 28 districts. Both implementing agencies follow the guidelines issued by the

government. The World Food Programme and Mary's Meals target communities and areas that are vulnerable and lagging behind in terms of educational outcomes.

## 2.4 Data

### 2.4.1 Household and Community Data

I use Malawi's Third and Fourth Integrated Household Surveys (2010–11; 2016–17). The result is a pooled cross-sectional dataset. These surveys have been implemented through the National Statistical Office with support from the World Bank. The analysis sample is limited to rural communities and includes all children that are of primary school age, 6 to 14 years old. On average, 16 percent of these children have received school meals at some point during the 12 months prior to the interview taking place. Moreover, 34 percent of communities were being targeted by school feeding at the time of the interview.[1] Not everyone within a targeted community was necessarily treated. As the concept of a community often overlaps with the boundaries of a village, I will use references to communities and villages interchangeably.

Children are classified as *in school* if they have replied positively to the following survey question: "Are you currently attending school or, if school is not in session now, did you attend school in the session just completed and plan to attend next session?" This variable measures the intensive margin of schooling, i.e., the proportion of children who are still in school conditional on past enrolment. I obtain the extensive margin of schooling by complementing the out-of-school sample per the above survey question with children who have never attended school but who were of primary school age at the time of the survey interview. Thus, the extensive margin measures the proportion of all children of primary school age who are in school, regardless of whether they have or have never enrolled in primary education. Besides the intensive and extensive measures of schooling, I explore two additional variables: the grade-for-age gap and age at the time of enrolment. For those who have never enrolled in school, the grade-for-age gap is given by the grade in which they should have been given their age. For early achievers, the gap is zero (Islam & Choe, 2013).

[1]Summary statistics suggest that between 2010 and 2016, the targeting of villages by school feeding has increased by more than the population of individual beneficiaries. There are two reasons that can explain and put this into context. First, the 2016–17 period has been hit hard by food insecurity due to floods and a prolonged drought. The Government of Malawi has listed the scaling up of school feeding programmes as a key strategy to limit the educational consequences of failed harvests (Government of Malawi, 2016). However, the implementation of school feeding has suffered, supplies became scarce, prices soared, and the budgets of schools became insufficient (Government of Malawi, 2016). It is likely that a significant number of the newly targeted villages had not actually started serving meals at the time of the survey, hence the seemingly smaller increase in the population of individual beneficiaries. Second, summary statistics also seem to suggest that the scaling up of school feeding has been particularly strategic during the drought year, such that the number of beneficiaries per village was smaller. Only the utmost vulnerable schools were targeted. This contrasts with the previous approach, which was one of cluster treatment to limit the migration of children from untreated to treated schools. In 2010, 53 percent of children in a targeted village were treated, while in 2016, the percentage was 35.

Table 2.1: Summary Statistics, Rural Malawi

| | 2010 | 2016 | Pooled | | | |
|---|---|---|---|---|---|---|
| **Variables** | **Mean** | **Mean** | **Mean** | $\sigma$ | **Min** | **Max** |
| *INDIVIDUAL CHARACTERISTICS* | | | | | | |
| In school, extensive margin | 0.887 | 0.918 | 0.903 | 0.297 | 0 | 1 |
| Grade-for-age gap (early achievers have zero gap) | 1.333 | 1.249 | 1.290 | 1.524 | 0 | 8 |
| Beneficiary school feeding past year | 0.157 | 0.171 | 0.164 | 0.371 | 0 | 1 |
| Age | 9.943 | 10.197 | 10.074 | 2.433 | 6 | 14 |
| Gender (girls 0, boys 1) | 0.498 | 0.491 | 0.495 | 0.500 | 0 | 1 |
| Father has PSLC | 0.104 | 0.080 | 0.092 | 0.289 | 0 | 1 |
| Father has JCE | 0.068 | 0.072 | 0.070 | 0.255 | 0 | 1 |
| Father has MSCE | 0.049 | 0.050 | 0.049 | 0.217 | 0 | 1 |
| Father has post-secondary diploma | 0.009 | 0.010 | 0.009 | 0.096 | 0 | 1 |
| Mother has PSLC | 0.061 | 0.070 | 0.065 | 0.247 | 0 | 1 |
| Mother has JCE | 0.033 | 0.040 | 0.036 | 0.187 | 0 | 1 |
| Mother has MSCE | 0.008 | 0.013 | 0.011 | 0.104 | 0 | 1 |
| Mother has post-secondary diploma | 0.001 | 0.002 | 0.002 | 0.041 | 0 | 1 |
| **Sample individuals aged 6-14** | **10,038** | **10,573** | | **20,611** | | |
| In school, intensive margin | 0.977 | 0.976 | 0.976 | 0.153 | 0 | 1 |
| **Sample 6-14 & enrolled in the past** | **9,113** | **9,943** | | **19,056** | | |
| Age when first started school | 6.340 | 6.045 | 6.186 | 0.916 | 5 | 14 |
| **Sample 6-14, incl. early start age of 5+** | **8,888** | **9,661** | | **18,549** | | |
| Age when first started school | 6.606 | 6.245 | 6.417 | 0.822 | 6 | 14 |
| **Sample 6-14, start age of 6+** | **7,418** | **8,109** | | **15,527** | | |
| *HOUSEHOLD CHARACTERISTICS* | | | | | | |
| Some stress i.t.o. food security (FEWS-NET) | 0.230 | 0.589 | 0.417 | 0.493 | 0 | 1 |
| Household size | 5.698 | 5.357 | 5.521 | 1.840 | 2 | 19 |
| Share of males between 6 and 9 | 8.554 | 8.432 | 8.490 | 11.501 | 0 | 67 |
| Share of males between 10 and 18 | 13.683 | 14.594 | 14.156 | 15.335 | 0 | 100 |
| Share of males between 19 and 40 | 10.045 | 9.279 | 9.647 | 10.803 | 0 | 67 |
| Share of males over 40 | 6.540 | 7.300 | 6.935 | 9.637 | 0 | 50 |
| Share of females between 6 and 9 | 8.878 | 8.700 | 8.786 | 11.859 | 0 | 67 |
| Share of females between 10 and 18 | 13.107 | 14.708 | 13.939 | 15.097 | 0 | 100 |
| Share of females between 19 and 40 | 13.613 | 13.721 | 13.669 | 10.700 | 0 | 67 |
| Share of females over 40 | 8.243 | 9.093 | 8.685 | 12.524 | 0 | 75 |
| Female household head | 0.274 | 0.308 | 0.292 | 0.455 | 0 | 1 |
| Head is married but spouse not present | 0.024 | 0.040 | 0.032 | 0.177 | 0 | 1 |
| Head is divorced, separated, widowed | 0.247 | 0.260 | 0.254 | 0.435 | 0 | 1 |
| Head is single | 0.005 | 0.005 | 0.005 | 0.072 | 0 | 1 |
| House made from permanent materials | 0.247 | 0.231 | 0.238 | 0.426 | 0 | 1 |
| House made from mix permanent and traditional | 0.261 | 0.402 | 0.334 | 0.472 | 0 | 1 |
| Owns basic furniture | 0.495 | 0.494 | 0.495 | 0.500 | 0 | 1 |
| Owns radio and/or TV | 0.497 | 0.394 | 0.443 | 0.497 | 0 | 1 |
| Size of garden (acres) | 1.926 | 1.542 | 1.726 | 1.683 | 0 | 40 |
| Time to water (hours) | 0.238 | 0.221 | 0.229 | 0.284 | 0 | 8 |
| **Sample households** | **5,195** | **5,619** | | **10,814** | | |
| *HOUSEHOLD SAFETY NETS* | | | | | | |
| Free maize | 0.024 | 0.260 | 0.147 | 0.354 | 0 | 1 |
| Free food (not maize) | 0.008 | 0.203 | 0.109 | 0.312 | 0 | 1 |
| Public works programme | 0.027 | 0.111 | 0.071 | 0.256 | 0 | 1 |
| Inputs for work programme | 0.002 | 0.003 | 0.003 | 0.052 | 0 | 1 |
| Likuni Phala to children and mothers | 0.005 | 0.020 | 0.013 | 0.111 | 0 | 1 |
| Feeding for malnourished children | 0.001 | 0.004 | 0.003 | 0.051 | 0 | 1 |
| Bursaries for secondary education | 0.003 | 0.005 | 0.004 | 0.066 | 0 | 1 |
| Direct cash transfers from government | 0.002 | 0.029 | 0.016 | 0.125 | 0 | 1 |
| Direct cash transfers from others | 0.005 | 0.022 | 0.014 | 0.117 | 0 | 1 |

| | 2010 | 2016 | Pooled | | | |
|---|---|---|---|---|---|---|
| **Variables** | **Mean** | **Mean** | **Mean** | $\sigma$ | **Min** | **Max** |
| *HOUSEHOLD SAFETY NETS* (continued) | | | | | | |
| Other safety nets | 0.003 | 0.063 | 0.034 | 0.182 | 0 | 1 |
| Scholarships for tertiary education | 0.001 | 0.001 | 0.001 | 0.025 | 0 | 1 |
| **Sample households** | **5,195** | **5,619** | | **10,814** | | |
| *COMMUNITY CHARACTERISTICS* | | | | | | |
| Ratio in school, extensive margin (aged 6-14) | 0.887 | 0.919 | 0.904 | 0.115 | 0.09 | 1 |
| Ratio in school, intensive margin (aged 6-14) | 0.974 | 0.975 | 0.975 | 0.050 | 0.56 | 1 |
| Comm. targeted by school feeding programmes | 0.251 | 0.433 | 0.345 | 0.476 | 0 | 1 |
| Perc. aged 24+ with at least PSLC | 19.297 | 21.395 | 20.386 | 14.419 | 0 | 83 |
| Perc. Muslim | 11.399 | 12.122 | 11.774 | 22.420 | 0 | 100 |
| Perc. Christian | 78.811 | 82.067 | 80.501 | 25.207 | 0 | 100 |
| Pop. community (thousands) | 3.673 | 4.689 | 4.200 | 6.482 | 0 | 80 |
| Most land in planned housing | 0.024 | 0.014 | 0.019 | 0.135 | 0 | 1 |
| Most land in squatter | 0.018 | 0.027 | 0.023 | 0.150 | 0 | 1 |
| Most land for industry | 0.006 | 0.005 | 0.005 | 0.073 | 0 | 1 |
| Most land for shops | 0.002 | 0.003 | 0.003 | 0.051 | 0 | 1 |
| Most land for other | 0.007 | 0.010 | 0.009 | 0.094 | 0 | 1 |
| Graded gravelled road | 0.253 | 0.195 | 0.223 | 0.416 | 0 | 1 |
| Dirt road | 0.489 | 0.514 | 0.502 | 0.500 | 0 | 1 |
| Dirt track | 0.097 | 0.151 | 0.125 | 0.331 | 0 | 1 |
| Distance gov. secondary school (km) | 29.28 | 24.629 | 26.866 | 26.438 | 0 | 200 |
| Distance health clinic (km) | 7.559 | 6.200 | 6.853 | 8.797 | 0 | 90 |
| Distance commercial bank (km) | 30.991 | 29.858 | 30.403 | 24.562 | 0 | 168 |
| Natural disaster in community 2 years before | 0.227 | 0.299 | 0.264 | 0.441 | 0 | 1 |
| Natural disaster in community 1 year before | 0.502 | 0.514 | 0.508 | 0.500 | 0 | 1 |
| Natural disaster in community current year | 0.200 | 0.440 | 0.324 | 0.468 | 0 | 1 |
| Irrigation scheme in community | 0.203 | 0.250 | 0.227 | 0.419 | 0 | 1 |
| Distance closest gov. primary school (km) | 1.817 | 2.404 | 2.121 | 3.491 | 0 | 50 |
| Classrooms are properly built | 0.652 | 0.708 | 0.681 | 0.466 | 0 | 1 |
| Electricity in primary school | 0.035 | 0.080 | 0.058 | 0.234 | 0 | 1 |
| Number private primary schools | 0.101 | 0.114 | 0.108 | 0.552 | 0 | 10 |
| Number religious primary schools | 0.463 | 0.436 | 0.449 | 1.452 | 0 | 25 |
| Pupils to teacher ratio | 106.496 | 90.366 | 98.125 | 48.689 | 8 | 425 |
| **Sample communities** | **546** | **589** | | **1,135** | | |

Reference categories: father has no education, mother has no education, share of males younger than 6, share of females younger than 6, head is married and the spouse is present in the household, the house is made from traditional materials, percentage practising traditional beliefs or other religions, most community land is destined to agriculture, and the community is endowed with asphalt road(s). Abbreviations: Primary School Leaving Certificate (PSLC), Junior Certificate of Education (JCE) and Malawi School Certificate of Education (MSCE).

Table 2.1 presents the summary statistics.[2] This table shows that the extensive margin of schooling in rural areas has slightly gone up from 89 percent in 2010 to 92 percent in 2016. As for the intensive margin of education, the situation appears to have remained stable at a high level. Note, however, that exposure to dropout is increasing with age. A thirteen-year-old will have had more chances to drop out of school than an eight-year-old. For instance, the dropout rate among fourteen-year-olds is 8.1 percent versus 2.3 percent for the pooled sample. Moreover, among the out-of-school children, 23 percent dropped out and 77 percent have never enrolled in primary education. Consequently, school feeding has more room to improve the extensive margin of schooling. Furthermore, some slight improvement has also been registered in terms of the grade-for-age gap. Similarly, the average age at enrolment has gone down and is getting closer to 6. This could mean that children are increasingly enrolling in school at age 6. However, there was also an increase in the number of children starting school earlier than the recommended age, which can also bring down the average.

### 2.4.2 Food Security Data

To assess the impact heterogeneity of school feeding with respect to the prevailing food security situation, I merge the aforementioned surveys point-to-polygon with information from the Famine Early Warning Systems Network (FEWS-NET). The time match between survey interviews and the food security information is almost to the yearly quarter.

FEWS-NET is a tool of the United States Agency for International Development, and it relies on the input of several organisations. I use their historical, non-projection data to capture the food security situation on the ground during 2010–11 and 2016–17. FEWS-NET classifies areas into five categories of food insecurity, as per the Integrated Food Security Phase Classification Version 2 (2016–17) and the FEWS-NET-defined Food Insecurity Severity Scale (2010–11). The two scales are very similar, but to avoid any misalignment, I collapse the five categories of food insecurity (minimal, stressed, crisis, emergency and famine) into two (minimal vs. some stress).

Minimal stress means that households are able to meet essential food and non-food needs without engaging in atypical and unsustainable strategies. If there is some stress, then FEWS-NET documents the presence of any of the following signals: households not being able to afford some essential non-food expenditures without engaging in stress-coping strategies, households experiencing high or above-usual acute malnutrition, or households

[2] A few outliers were present due to entry errors in the case of two variables which measure distance to the closest primary school and the number of religious primary schools present in the community. Regarding the former, the data clerk has inputted kilometres where metres must have been the correct metric. Thus, I made the correction, but only after I checked that the amended numbers were similar to those of neighbouring communities. Moreover, in the case of religious primary schools, there where only two communities with very large numbers. All other communities report numbers smaller than 25. However, these two rural communities reported 350 and 401. I have set them to 3 and 4, respectively. Furthermore, there were additional outliers in terms of the pupils-to-teacher variable. In this case, the reason behind the outliers was not clear. I have excluded these observations from the analysis. Very few individuals were dropped. Lastly, population numbers were off for 3 communities. They exceeded the population of the districts to which they belonged. I cut two zeroes to bring the numbers down.

only meeting their minimum food needs by depleting or liquidating essential livelihood assets or through crisis-coping strategies. The FEWS-NET variable shows that 2016 was a drought year. In 2010, 23 percent of households lived in areas classified as food insecure, while as many as 59 percent of households lived in food-insecure areas in 2016. It is clear that 2016 was affected by food insecurity, while 2010 was a rather typical year.

## 2.5 Empirical Strategy

The literature studying interventions that offer food in exchange for school attendance is generally concerned that treatment at the individual or school level is endogenous. For instance, the agencies implementing school feeding use school-selection criteria that draw heavily on local knowledge. Researchers cannot claim to observe these criteria. Moreover, further bias is likely. For an individual to have received school feeding at some point in the past year, which is the treatment variable, s/he must have been in school to begin with. Thus, it is more likely that s/he is still in school at the time of the interview. This may lead to a positive bias. Lastly, a negative bias due to omitted variables is also possible because school feeding programmes target the most vulnerable of schools, which are also likely to have relatively lower educational performance.

While the implementation design of the programme is a source of endogeneity, it also puts forward the solution. The intervention in Malawi has followed a two-step targeting of beneficiaries. The implementing agencies consult with the government and with regional and district administration departments to decide on which areas to target. These decisions must be based on information that is observable to the relevant authorities, and thus, it is argued, observable to researchers as well. Thereafter, once these areas are set, the agencies use their local knowledge to target schools (Government of Malawi, 2009). The latter step is the source of endogeneity, while the former is the identification solution.

I argue that treatment at the community level can be used as an instrument for treatment at the individual level provided that I control for the observables that inform treatment at the community level, e.g., the decision-making factors used during the first phase of the implementation process. Therefore, I defend the exogeneity of the instrument in the context of selection on observables. Similar strategies have been implemented by Ravallion & Wodon (2000), Sparrow (2007) and Islam & Choe (2013).

### 2.5.1 Individual-Level Analysis

Equations 2.1 and 2.2 describe the instrumental variable approach. These equations are presented as stacked individual observations. $Y$ denotes the outcomes of interest, e.g., whether a child is in school, his or her grade-for-age gap and age at enrolment. $D$ and $Z$ represent treatment at the individual and village level, respectively. $X$ is a set of covariates that vary at the individual and household levels, and $V$ is a set of controls that define community characteristics. $Q$ consists of district dummies. Finally, $T$ is simply a dummy which equals 1 if the year of data collection is 2016.

$$Y = \gamma_0 \iota_N + \gamma_1 \widehat{D} + X\gamma_2 + V\gamma_3 + Q\gamma_4 + \gamma_5 T + \varepsilon \tag{2.1}$$

$$D = \alpha_0 \iota_N + \alpha_1 Z + X\alpha_2 + V\alpha_3 + Q\alpha_4 + \alpha_5 T + \nu \tag{2.2}$$

The coefficient of interest is $\gamma_1$. Equation 2.3 shows that it represents the ratio between the effect of $Z$ on $Y$ and the effect of $Z$ on $D$ (Angrist & Pischke, 2008). Where $i = \overline{1...N}$ and $N$ is the number of individuals. $\gamma_1$ identifies the impact of school feeding if several assumptions are met (Imbens & Angrist, 1994; Imbens, 2004; Angrist & Pischke, 2008).

$$\hat{\gamma}_1 = LATE = \frac{cov(Z_i, Y_i \mid X, V, Q, T)}{cov(Z_i, D_i \mid X, V, Q, T)}$$

$$= \frac{E(Y_i \mid Z_i = 1, X, V, Q, T) - E(Y_i \mid Z_i = 0, X, V, Q, T)}{E(D_i \mid Z_i = 1, X, V, Q, T) - E(D_i \mid Z_i = 0, X, V, Q, T)} \tag{2.3}$$

The first assumption is *conditional mean independence*. Treatment at the community level can be considered as good as random *if* the factors affecting selection into treatment are included: $E(Y_i^1 \mid Z, X, V, Q, T) = E(Y_i^1 \mid X, V, Q, T)$ and $E(Y_i^0 \mid Z, X, V, Q, T) = E(Y_i^0 \mid X, V, Q, T)$. I do my best to include a variety of such factors; however, I cannot claim that all relevant variables have been included. I do argue, though, that I have mitigated the risk to a satisfactory extent.[3] The assumption of conditional mean independence leads to $cov(\varepsilon, \nu) = 0$. That is, the *instrument is exogenous*, which is a necessary condition for the identification of $\gamma_1$.

The second assumption relates to the *relevance of the instrument*. The instrument $Z$ must be strongly correlated with the endogenous variable. This is intuitively the case, as schools and individuals are treated if their village was targeted. First-stage regressions are indeed strong and can be consulted in the appendix.

The third assumption is *sufficient overlap*. For each village or community $v$, the condition for identification is that $0 < p(Z_v = 1 \mid X, V, Q, T) < 1$, i.e., there must be both treated and untreated villages among those with the same set of attributes. I compare and contrast results that are based on the full sample of villages as well as on a sample that is limited to communities with similar, overlapping characteristics, which I call the *overlap sample*.[4] Buttenheim et al. (2011) adopt a similar strategy, per which the authors keep the units of observation that have overlapping characteristics.

[3] In a community-level linear regression of $Z$ on $Q$, $T$, $V$ and aggregated values of $X$, the *F-stat* is 21 and $R^2$ is 0.36. In a non-linear specification, $\chi^2$ is 442 and the pseudo-$R^2$ is 0.31.

[4] The graphs plotting the extent of overlap between communities are in Appendices B.2 and B.6. The overlap is limited in absence of corrections to the sample. After corrections, the overlap improves. However, ideally, the mass around the lowest and the highest values of the treatment propensity scores would have been further reduced. Nevertheless, since one expects that a more dissimilar pool of villages would negatively bias estimates, as treatment is targeted at the more vulnerable of villages with lower educational performance, then a sample with a better overlap will increase my chances of finding the hypothesised positive link between school feeding and educational outcomes. Thus, the amount of post-correction overlap is conservative and thus acceptable.

Fourth and last, I assume the *instrument is monotonous*, i.e., the sample is assumed only to consist of compliers. In this case, if defiers were numerous, then the estimates would be biased downwards and make my hypothesis easier to reject. Because of the assumption of monotonicity, the estimation is of a local average treatment effect (LATE) (Imbens & Angrist, 1994; Angrist & Pischke, 2008).

Finally, I employ the reduced-form specification presented in Equation 2.4. This is a classical problem of selection on observables.

$$Y = \beta_0 \iota_N + \beta_1 Z + X\beta_2 + V\beta_3 + Q\beta_4 + \beta_5 T + \epsilon \tag{2.4}$$

$$\hat{\beta}_1 = AIT = E(Y_i \mid Z_i = 1, X, V, Q, T) - E(Y_i \mid Z_i = 0, X, V, Q, T) \tag{2.5}$$

In contrast to the two-stage least squares estimator, which identified LATE, $\beta_1$ identifies an average intention to treat effect (AIT) in Equation 2.5. The identification assumptions are those of conditional mean independence and sufficient overlap. This is an intention to treat effect because not everyone in a targeted village is treated.

### 2.5.2 Village-Level Analysis

I also run a village-level analysis by collapsing the data accordingly. Equation 2.4 applies, but $N$ denotes villages as opposed to individuals. I use propensity score matching. In this context, the treatment effect is the average of the difference between the observed and imputed potential outcomes computed for each community. See Equation 2.6. Identification is achieved under conditional mean independence and sufficient overlap.

$$AIT = E(\hat{Y}_i \mid Z_i = 1, X, V, Q, T) - E(\hat{Y}_i \mid Z_i = 0, X, V, Q, T) \tag{2.6}$$

$\hat{Y}_{i,Z=0}$ must be imputed for treated communities, and so does $\hat{Y}_{i,Z=1}$ for control villages. Meanwhile, the following are observed: $\hat{Y}_{i,Z=1} = Y_i$ for treated communities and $\hat{Y}_{i,Z=0} = Y_i$ for control communities. The imputation is done based on matching with the most similar village of the opposite treatment status. Similarity is ascertained based on treatment propensity scores.

## 2.6 Individual-Level Results

### 2.6.1 Local Average Treatment Effect

Table 2.2 shows that the impact of school feeding has chiefly consisted of attracting out-of-school children into primary education for the first time. The IV coefficient puts the magnitude of the impact on the extensive margin at roughly 5.8 percentage points in the pooled 2010–16 sample. In contrast, the impact for the intensive margin of schooling is not significant. This may suggest that children enrol in school during times of need, but that school feeding is not sufficient to dissuade dropout.[5]

[5]First stage regressions are strong. They are reported in Appendix B.1.

Table 2.2: Impact of School Feeding on the Extensive and Intensive Margins of Schooling

| | IN SCHOOL, EXTENSIVE MARGIN | | | | | | IN SCHOOL, INTENSIVE MARGIN | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Pooled sample | | 2010 typical year | | 2016 drought year | | Pooled sample | | 2010 typical year | | 2016 drought year | |
| **Explanatory variable** | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| ***Panel A: Full sample*** | | | | | | | | | | | | |
| Beneficiary school feeding | 0.124 | 0.058 | 0.159 | -0.002 | 0.098 | 0.084 | 0.032 | 0.007 | 0.035 | -0.011 | 0.032 | 0.037 |
| | (0.005)*** | (0.018)*** | (0.008)*** | (0.024) | (0.007)*** | (0.033)*** | (0.003)*** | (0.009) | (0.005)*** | (0.012) | (0.004)*** | (0.018)** |
| | [0.008]*** | [0.026]** | [0.016]*** | [0.038] | [0.009]*** | [0.038]** | [0.004]*** | [0.011] | [0.006]*** | [0.014] | [0.006]*** | [0.020]* |
| $R^2$ | 0.11 | 0.10 | 0.14 | 0.11 | 0.10 | 0.10 | 0.04 | 0.04 | 0.05 | 0.04 | 0.04 | 0.04 |
| F | 20 | - | 14 | - | 9 | - | 4 | - | 2 | - | 2 | - |
| $\chi^2$ | - | 1599 | - | 1084 | - | 737 | - | 414 | - | 211 | - | 223 |
| Robust score test | - | 13.75 | - | 48.09 | - | 0.17 | - | 6.24 | - | 13.81 | - | 0.05 |
| P-value test | - | 0.00 | - | 0.00 | - | 0.92 | - | 0.04 | - | 0.00 | - | 0.97 |
| N | 20,611 | 20,611 | 10,038 | 10,038 | 10,573 | 10,573 | 19,056 | 19,056 | 9,113 | 9,113 | 9,943 | 9,943 |
| ***Panel B: Overlap sample*** | | | | | | | | | | | | |
| Beneficiary school feeding | 0.128 | 0.072 | 0.169 | 0.006 | 0.082 | 0.084 | 0.030 | 0.015 | 0.040 | -0.008 | 0.027 | 0.048 |
| | (0.005)*** | (0.018)*** | (0.009)*** | (0.025) | (0.008)*** | (0.034)** | (0.003)*** | (0.009) | (0.006)*** | (0.013) | (0.005)*** | (0.018)*** |
| | [0.010]*** | [0.027]*** | [0.018]*** | [0.036] | [0.012]*** | [0.039]** | [0.004]*** | [0.010] | [0.008]*** | [0.015] | [0.007]*** | [0.019]** |
| $R^2$ | 0.11 | 0.11 | 0.14 | 0.10 | 0.10 | 0.10 | 0.04 | 0.04 | 0.07 | 0.05 | 0.05 | 0.05 |
| F | 14 | - | 7 | - | 6 | - | 3 | - | 1 | - | 2 | - |
| $\chi^2$ | - | 1062 | - | 451 | - | 467 | - | 270 | - | 102 | - | 149 |
| Robust score test | - | 9.53 | - | 44.95 | - | 0.00 | - | 2.35 | - | 12.79 | - | 1.24 |
| P-value test | - | 0.01 | - | 0.00 | - | 1.00 | - | 0.31 | - | 0.00 | - | 0.54 |
| N | 13,353 | 13,353 | 4,493 | 4,493 | 6,524 | 6,524 | 12,315 | 12,315 | 4,098 | 4,098 | 6,147 | 6,147 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. Errors are clustered at the level of communities in square brackets. F, $\chi^2$ and the exogeneity test statistics correspond to the specification with robust errors. Panel A includes all observations. Panel B only includes individuals from communities with treatment propensity scores $\in (0.1, 0.9)$, where propensity scores are predicted based on logit regressions at the community level. Other covariates include: *individual-level variables* (age, age squared, gender, father's education: no education vs. primary/ lower/ upper secondary and post-secondary, and mother's education), *household-level variables* (household size, household gender-age composition, whether the household benefits from safety nets other than school feeding and as enumerated in Table 2.1, female-headed household, marital status: married with spouse present vs. head is married but spouse is not present/ head is divorced, separated or widowed/ head is single, wall materials: traditional dwelling vs. walls made from permanent materials/ walls made from a mix of permanent and traditional materials, household owns basic furniture, household owns a radio or TV-set, average garden size, average time to water source, prevailing food security situation in the household's area: some stress vs. minimal stress), *village-level characteristics* (population, perc. Muslims, perc. Christians, main land use: agriculture vs. planned housing/ squatter/ industry/ shops and other, main access road: asphalt vs. gravelled road/ dirt road and dirt track, presence of irrigation schemes, whether there were any natural disasters during the current year, one or two years before, distances to chipatala, banks, secondary and primary government schools, whether schools are mainly made from permanent materials, whether they are electrified, the number of private primary schools, the number of religious primary schools, and the ratio of pupils to teachers), district and year dummies.

Moreover, I separate the pooled sample into its component years to tentatively investigate the heterogeneity of school feeding in terms of the prevailing food security situation. 2010 was a year of satisfactory agricultural output and minimal food insecurity. In contrast, 2016 was marked by food insecurity. I find no impact on the extensive or intensive margins of schooling in 2010, but the results for 2016 suggest a positive and significant increase of 8–9 percentage points in the extensive margin of schooling and 3–4 percentage points for the intensive margin.[6] For 2016, the $\chi^2$ statistic of the robust score test fails to reject the null of exogeneity. Thus, the OLS coefficient can take precedence over the IV estimate.[7]

Furthermore, I limit the sample to similar communities to mitigate the risk of confounding the impact of school feeding with that of unobservable factors that set the treated communities apart from the control communities. This is done by compiling an *overlap sample* which excludes the communities whose treatment propensity scores are lower than 0.10 or greater than 0.90.[8] I find that previous results are robust to the overlap sample. Panels A and B of Table 2.2 lead to the same conclusions. However, the robust score test fails to reject the null of exogeneity for the case of retention rates in the pooled sample in Panel B. Thus, the more efficient OLS coefficient, which is significant with a magnitude of 3 percentage points, can be considered instead of the IV estimate.

Finally, I run a falsification test on the dependent variable. I replace the education of children with that of their parents. I measure the education of parents as a dummy variable taking the value of 1 if at least one parent has achieved at least primary education. In Appendix B.3, I show that school feeding does not impact the false dependent variable. Thus, my analysis survives the falsification test.

### 2.6.2 Impact Heterogeneity

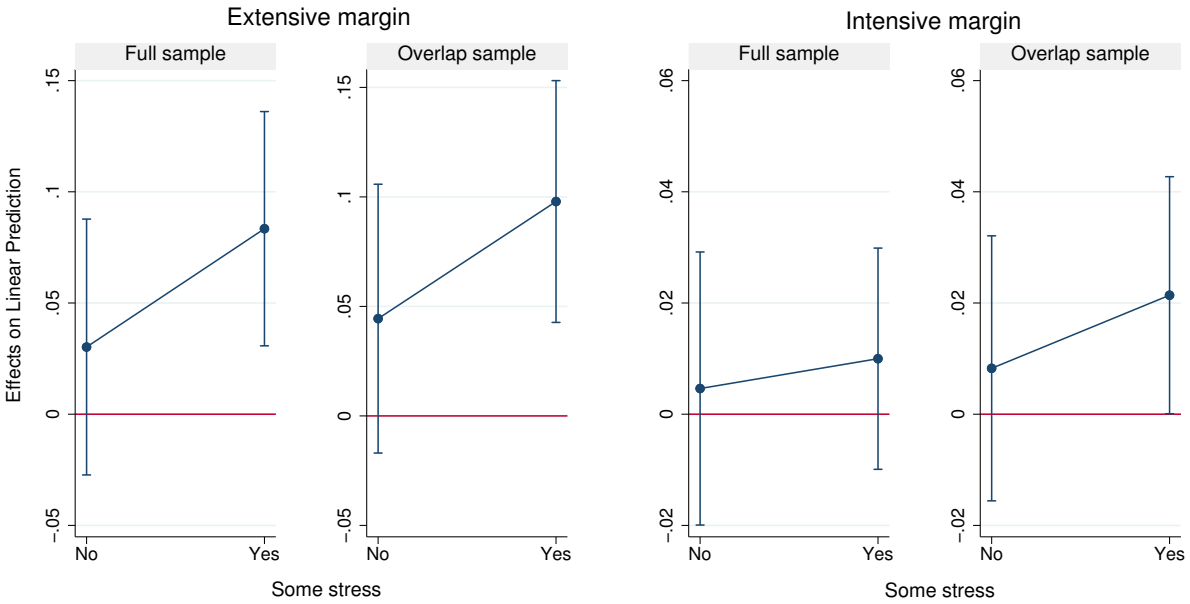
In this section, I explore a further avenue to assessing the impact heterogeneity of school feeding in terms of the prevailing food security situation. This is based on the observation that even during times of satisfactory harvests, pockets of food insecurity still exist. Similarly, even if the situation is one of generalised drought, some areas will, nevertheless, be minimally affected by food insecurity.

[6] I have also explored the heterogeneity of results in terms of gender. Differences between boys and girls are limited. School feeding has not impacted the education of either gender in 2010, and the drought sample of 2016 shows very limited heterogeneity.

[7] The 2010 and 2016 samples offer a snapshot of Malawi during two very different socio-economic contexts. This can explain why the robust score tests, which examine the presence of endogeneity, have led to different conclusions in the case of the 2010 and 2016 samples. On account of the drought experienced during the 2015–16 agricultural season, the implementation strategy of school feeding programmes was modified to mitigate the consequences of drought. The agencies implementing school feeding were asked to expand the coverage of school feeding (Government of Malawi, 2016). As a consequence of this expansion, the criteria that the implementing agencies were previously using to decide which schools to target have probably lost part of their relevance, as a wider and more diverse array of schools and students were now being targeted. These criteria were the source of endogeneity. Thus, the bias that was previously created by my inability to observe the school-selection criteria has likely been reduced in the aftermath of the 2016 drought and against the background of the amended programme implementation strategy.

[8] Appendix B.2 shows graphically how this sample restriction improves the amount of overlap between the control and treated villages.

Figure 2.1: Average Marginal Effects of School Feeding with 90% CIs and Varying Food Insecurity




IV coefficients, confidence intervals are based on clustered errors. The covariates from Table 2.2 apply, plus an interaction term between school feeding and the prevailing food security situation.

Figure 2.1 presents the marginal effects of school feeding on the extensive and intensive margins of schooling given two levels of localised food insecurity: minimal versus some food-related stress or crisis.[9] This figure confirms that the impact of school feeding is heterogeneous with respect to the prevailing food security situation. The impact of school feeding would not be significant if all children lived in food-secure areas. In contrast, if food insecurity were to prevail, then the impact on enrolment rates would become significant at 8.3 and 10 percentage points in the full and overlap sample, respectively. Similarly, retention rates would be 1–2 percentage points higher, although significance is only attained in the overlap sample.

### 2.6.3 Average Intention to Treat Effect

To investigate the strength of my findings further, I estimate a reduced-form specification. I use the methodology proposed by Altonji et al. (2005) and Oster (2019) to test the robustness of the estimates to the bias created by unobservables. The AIT is a conservative estimate compared to LATE. Therefore, the robustness of the reduced-form results with respect to unobservables will be even more encouraging.

Altonji et al. (2005) and Oster (2019) argue that one can tentatively evaluate the robustness of results to the omitted variable bias by observing coefficient and $R^2$ movements before and after the inclusion of controls. First, this method can give the bias-adjusted $\beta$ coefficient conditional on two parameter inputs: the relative degree of selection on observed and unobserved variables ($\delta$), and the $R^2$ value that one assumes corresponds to a

[9] First stage regressions are presented in Appendix B.4.

scenario whereby all relevant variables are observed and included in the regression ($R_{max}$). Second, one can compute the maximum level of $\delta$ such that the $\beta$ coefficient is brought down to zero and $R^2$ is set to $R_{max}$. Lastly, the method can also help identify the maximum $R^2$ for which the $\beta$ coefficient is still positive and $\delta$ is of a set value. Oster (2019) recommends $\delta = \pm 1$, which means that unobservables are as important as observables, and $R_{max} = 1.3\hat{R}^2$, where $\hat{R}^2$ is the estimated $R^2$ when observables are included in the estimation.

Table 2.3 shows that the impact of school feeding on the extensive and intensive margins of education is positive and significant, and that the effect is driven by the programme's effectiveness in food-insecure areas. Moreover, results are generally robust to omitted variables. The coefficients of interest are all keeping their sign after they are corrected for the omitted variable bias, where $R_{max}$ is set to $1.3\hat{R}^2$ and unobservables are assumed as important as observables, i.e., $\delta = \pm 1$. Most results are robust to even more stringent specifications than the one recommended by Oster (2019). This allows me to state with some level of confidence that school feeding has had a positive and significant effect on educational outcomes. The effect ranges from 1.6 to 3.1 percentage points in the case of the extensive margin of schooling, while for the the intensive margin, the magnitude is of 1 percentage point in food-insecure areas.

### 2.6.4 Mechanisms

In this subsection, I look at age at enrolment conditional on enrolment and children's grade-for-age gap to discuss the mechanisms that connect school feeding to enrolment and retention rates. I assume that exposure to treatment has been lengthy, i.e., villages have been treated for several years before they were interviewed at the time of the Integrated Household Surveys. In this case, a food security analysis is no longer appropriate, as I am eliciting the impact of school feeding over longer periods of time and the food security data is punctual not historical.

Primary school starts at age 6 in Malawi, but late- or early-age enrolments are not uncommon. Among five-year-olds in rural Malawi at the time of the Integrated Household Surveys, 4 percent reported being in pre-school, 28 percent reported being in primary education, and the remainder had not yet enrolled in any form of education. Moreover, among the ever-enrolled sample of children aged 6 to 14 years old, 23 percent have reportedly enrolled at age 7 or above.

Table 2.4 suggests that school feeding has reduced the average age at enrolment, and that it has done so by attracting children into school earlier than the advice of the government. This statement is based on the observation that the impact of school feeding is weak for the sample of children whose age at enrolment is 6 or above, but it is negative and strongly significant when early enrollees are also allowed in the sample. The magnitude of the local average treatment effect is of 2–3 months. First stage regressions can be consulted in Appendix B.5.

Table 2.3: Reduced-Form Regressions

| | IN SCHOOL, EXTENSIVE MARGIN | | | | | | IN SCHOOL, INTENSIVE MARGIN | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | ***Full sample*** | | | ***Overlap sample*** | | | ***Full sample*** | | | ***Overlap sample*** | | |
| **Explanatory variable** | Pooled | Food insecure | Food secure | Pooled | Food insecure | Food secure | Pooled | Food insecure | Food secure | Pooled | Food insecure | Food secure |
| Community targeted | 0.016 | 0.021 | 0.007 | 0.022 | 0.031 | 0.006 | 0.002 | 0.008 | -0.002 | 0.005 | 0.010 | -0.002 |
| by school feeding | (0.005)*** | (0.008)** | (0.007) | (0.006)*** | (0.009)*** | (0.008) | (0.003) | (0.004)* | (0.004) | (0.003) | (0.005)** | (0.004) |
| | [0.007]** | [0.011]* | [0.010] | [0.008]*** | [0.011]*** | [0.011] | [0.003] | [0.005]* | [0.005] | [0.003] | [0.005]* | [0.004] |
| Bias-adjusted $\beta$ for: | | | | | | | | | | | | |
| $R_{max} = 1.3\hat{R}^2$ and $\delta = \pm 1$ | 0.015 | 0.010 | - | 0.020 | 0.028 | - | - | 0.008 | - | - | 0.010 | - |
| $R_{max} = 0.15$ and $\delta = \pm 1$ | 0.015 | negative | - | 0.018 | 0.022 | - | - | 0.006 | - | - | 0.009 | - |
| $R_{max} = 0.50$ and $\delta = \pm 1$ | 0.012 | negative | - | 0.011 | negative | - | - | 0.006 | - | - | 0.009 | - |
| $\delta$ for: | | | | | | | | | | | | |
| $R_{max} = 1.3\hat{R}^2$ and $\beta = 0$ | 9.862 | 1.613 | - | 72.575 | 3.686 | - | - | 6.977 | - | - | 5.892 | - |
| Max $R_{max}$ for: | | | | | | | | | | | | |
| $\beta > 0$ and $\delta = \pm 1$ | 1.000 | 0.130 | - | 1.000 | 0.190 | - | - | 1.000 | - | - | 1.000 | - |
| $R^2$ | 0.09 | 0.09 | 0.11 | 0.09 | 0.09 | 0.12 | 0.04 | 0.04 | 0.04 | 0.04 | 0.05 | 0.04 |
| F | 17 | 8 | 11 | 11 | 7 | 7 | 4 | 2 | 3 | 3 | 2 | 2 |
| N | 20,611 | 8,431 | 12,180 | 13,353 | 6,650 | 6,703 | 19,056 | 7,706 | 11,350 | 12,315 | 6,086 | 6,229 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. Errors are clustered at the level of communities and presented in square brackets. The covariates from Table 2.2 apply. The F statistic corresponds to the specification with robust errors. The overlap sample only includes individuals from communities with a treatment propensity score between 0.1 and 0.9. Propensity scores are predicted based on logit regressions at the community level.

Table 2.4: Impact of School Feeding on Age at Enrolment and Grade-for-Age Gaps

| | **AGE AT ENROLMENT** | | | | | | | | **GRADE-FOR-AGE GAP** | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Children enrolled at 6 or above | | | | Children enrolled at 5 or above | | | | All children 6–14 | | | | In-school children | | | |
| **Explanatory** | ***Full sample*** | | ***Overlap sample*** | | ***Full sample*** | | ***Overlap sample*** | | ***Full sample*** | | ***Overlap sample*** | | ***Full sample*** | | ***Overlap sample*** | |
| **variable** | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| Beneficiary school feeding | 0.003 | -0.088 | 0.004 | -0.098 | -0.009 | -0.227 | -0.018 | -0.241 | -0.194 | -0.202 | -0.215 | -0.224 | 0.020 | -0.096 | 0.012 | -0.098 |
| | (0.018) | (0.050)* | (0.021) | (0.052)* | (0.019) | (0.053)*** | (0.021) | (0.054)*** | (0.023)*** | (0.073)*** | (0.025)*** | (0.074)*** | (0.021) | (0.062) | (0.023) | (0.062) |
| | [0.025] | [0.073] | [0.027] | [0.070] | [0.026] | [0.077]*** | [0.029] | [0.073]*** | [0.030]*** | [0.104]* | [0.034]*** | [0.101]** | [0.026] | [0.081] | [0.029] | [0.079] |
| $R^2$ | 0.15 | 0.15 | 0.13 | 0.13 | 0.14 | 0.14 | 0.13 | 0.12 | 0.45 | 0.45 | 0.45 | 0.45 | 0.47 | 0.47 | 0.47 | 0.47 |
| F | 22 | - | 12 | - | 26 | - | 16 | - | 176 | - | 120 | - | 180 | - | 124 | - |
| $\chi^2$ | - | 2122 | - | 1144 | - | 2521 | - | 1452 | - | 16816 | - | 10988 | - | 17208 | - | 11329 |
| Rob. sc. test | - | 4.02 | - | 4.81 | - | 19.92 | - | 21.04 | - | 0.01 | - | 0.01 | - | 4.08 | - | 3.66 |
| P-value test | - | 0.13 | - | 0.09 | - | 0.00 | - | 0.00 | - | 0.99 | - | 0.99 | - | 0.13 | - | 0.16 |
| N | 15,527 | 15,527 | 10,069 | 10,069 | 18,549 | 18,549 | 11,979 | 11,979 | 20,611 | 20,611 | 13,353 | 13,353 | 18,602 | 18,602 | 12,020 | 12,020 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. Errors are clustered at the level of communities and presented in square brackets. The covariates from Table 2.2 apply. F, $\chi^2$ and the exogeneity statistics correspond to the specification with robust errors. Age at enrolment is recalled for each interviewee. Children who have never enrolled are excluded. The grade-for-age gap is determined based on age, the grade a child is in and the grade that s/he should be in given her or his age. The gap is always zero or positive. Children who are ahead in school (early achievers) are considered with a zero gap. If a child has never been to school, then the gap is given by the grade the child should have been in given her or his age.

Regarding the grade-for-age gap analysis, Table 2.4 shows once again that school feeding appears to be improving educational outcomes by attracting out-of-school children into primary education. If out-of-school children are left out of the analysis, then school feeding does not have a strong impact on the grade-for-age gap of children who are currently attending school. In contrast, if the sample includes all children in the age group 6–14, then school feeding does reduce the grade-for-age gap by 2–3 months on average. In this case, the IV and OLS estimates are not statistically different from each other.

## 2.7 Community-Level Results

I use propensity score matching to estimate the average intention to treat effect of school feeding on community-level averages of the extensive and intensive margins of schooling.

Table 2.5: Impact of Community-Level Targeting on Average Schooling

| | **AVG. IN SCHOOL, EXTENSIVE MARGIN** | | | | | |
|---|---|---|---|---|---|---|
| | # matches | | Level of tolerance overlap assumption | | | |
| **Explanatory variable** | # 1 | # 2 | # 0.01 | # 0.05 | # 0.10 | # 0.20 |
| ***Pooled sample*** | | | | | | |
| Community targeted | 0.018 | 0.021 | 0.019 | 0.021 | 0.020 | 0.009 |
| by school feeding | (0.006)*** | (0.007)*** | (0.006)*** | (0.009)** | (0.009)** | (0.008) |
| | **AVG. IN SCHOOL, INTENSIVE MARGIN** | | | | | |
| | # matches | | Level of tolerance overlap assumption | | | |
| **Explanatory variable** | # 1 | # 2 | # 0.01 | # 0.05 | # 0.10 | # 0.20 |
| ***Pooled sample*** | | | | | | |
| Community targeted | 0.005 | 0.002 | 0.005 | 0.002 | -0.001 | 0.007 |
| by school feeding | (0.004) | (0.006) | (0.004) | (0.004) | (0.004) | (0.002)*** |
| Treated | 384 | 384 | 381 | 360 | 326 | 240 |
| Untreated | 715 | 715 | 692 | 547 | 428 | 280 |
| # matches | 1 | 2 | 1 | 1 | 1 | 1 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors. Other covariates include: individual- and household-level variables aggregated at the level of communities (household size, perc. households who benefit from safety nets other than school feeding, perc. female-headed households, perc. households whose head is married but spouse is not present, perc. households whose head is divorced, separated or widowed, perc. households whose head is single, perc. households with walls made from permanent materials, perc. households with walls made from a mix of permanent and traditional materials, perc. households who own basic furniture, perc. households who own a radio/TV-set, perc. Muslims, perc. Christians, average education of individuals aged 24+, average garden size, average time to collect water from source and food insecurity prevalence) and village-level characteristics (population, main land use: agriculture vs. planned housing/ squatter/ industry/ shops and other, main access road: asphalt vs. gravelled road/ dirt road and dirt track, presence of irrigation schemes, history of natural disasters, distances to chipatala, banks, secondary and primary schools, whether schools are mainly made from permanent materials, whether they are electrified, the number of private primary schools, the number of religious primary schools and the pupils-to-teacher ratio), as well as district and year dummies.

Table 2.5 presents the results for the pooled sample, which includes both food-secure and food-insecure communities. If all communities were targeted by school feeding, then they would all see a 2 percentage point increase in enrolments compared to a no-treatment

scenario. The coefficient is robust to various samples defined by the overlap tolerance parameter. Significance is lost only in the most stringent of specifications. In contrast, retention rates do not seem to respond to school feeding programmes in the pooled sample.

To explore the impact heterogeneity of school feeding, I divide the sample into food-secure and food-insecure communities. Table 2.6 confirms that results are heterogeneous in terms of the prevailing food security situation. School feeding has no impact on the extensive margin of schooling if communities face minimal food insecurity. However, school feeding does increase enrolment by 2–4 percentage points in food-insecure communities. Results are strongly significant and robust to various levels of the tolerance parameter. As for the intensive margin of education, the impact of school feeding in food-insecure communities is positive, but the magnitude is small and treatment is only occasionally significant.[10]

Lastly, I run a falsification test. The extensive margin of primary schooling is swapped for the extensive margin of secondary education. The latter is defined as the percentage of children aged 15–18 who are in secondary education. General equilibrium effects could impact this analysis insofar as parents' decision to enrol children in primary education is linked to their older siblings' enrolment in secondary school, or if school feeding would have allowed more children to graduate from primary school and thus be eligible for secondary education. Results do not suggest any clear link between school feeding and the falsified outcome. My analysis survives the falsification test. See Appendix B.8.

## 2.8 Conclusion

In this chapter, I have used an instrumental variable approach and propensity score matching to estimate the impact of school feeding on educational outcomes in rural Malawi. The evidence is causal to the extent that I manage to control for the factors that explain both village-level treatment and educational outcomes. My results and conclusions are shown to hold even after running a number of robustness checks.

I provide evidence that the significance and magnitude of the impact of school feeding programmes rely on the prevailing local food security situation at the time and place of the evaluation. As long as there is a food constraint to relax and an educational gap to fill, school feeding improves educational outcomes. However, school feeding appears to be more effective in attracting children into school for the first time than in keeping them in school. Enrolment rates have increased by roughly 8–10 percentage points in food-insecure areas. Targeted villages, which include both treated and control children, have experienced an increase of 2–3 percentage points in average enrolment in food-insecure areas. In contrast, the impact on retention rates is less robust. While the IV and reduced-form estimations point to a 1–2 percentage point improvement in food-insecure areas, my propensity score matching analysis fails to find a robust impact. Across the board, impacts are not significant in food-secure areas. My investigation into mechanisms further proves that the effect of school feeding has been at its strongest in enrolling out-of-school children.

[10] Overlap and balance plots are Appendices B.6 and B.7.

Table 2.6: Heterogeneous Impact of Community-Level Treatment on Average Schooling

| | AVG. IN SCHOOL, EXTENSIVE MARGIN | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | ***Minimal stress sample*** | | | | | ***Some stress sample*** | | | | |
| **Explanatory** | Level of tolerance overlap assumption | | | | | Level of tolerance overlap assumption | | | | |
| **variable** | #0 | # 0.01 | # 0.05 | # 0.10 | # 0.20 | #0 | # 0.01 | # 0.05 | # 0.10 | #0.20 |
| Community targeted | -0.001 | -0.001 | -0.005 | -0.007 | -0.009 | 0.027 | 0.026 | 0.039 | 0.021 | 0.019 |
| by school feeding | (0.024) | (0.021) | (0.005) | (0.006) | (0.008) | (0.005)*** | (0.005)*** | (0.016)** | (0.003)*** | (0.012)* |

| | AVG. IN SCHOOL, INTENSIVE MARGIN | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | ***Minimal stress sample*** | | | | | ***Some stress sample*** | | | | |
| **Explanatory** | Level of tolerance overlap assumption | | | | | Level of tolerance overlap assumption | | | | |
| **variable** | #0 | # 0.01 | # 0.05 | # 0.10 | # 0.20 | #0 | # 0.01 | # 0.05 | # 0.10 | #0.20 |
| Community targeted | -0.018 | -0.019 | -0.000 | 0.002 | 0.002 | 0.001 | 0.001 | 0.007 | 0.008 | 0.001 |
| by school feeding | (0.025) | (0.020) | (0.002) | (0.008) | (0.004) | (0.004) | (0.004) | (0.003)** | (0.002)*** | (0.010) |
| Treated | 161 | 160 | 152 | 142 | 99 | 199 | 184 | 163 | 143 | 106 |
| Untreated | 453 | 427 | 296 | 221 | 123 | 242 | 228 | 195 | 170 | 113 |
| # matches | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. The covariates from Table 2.5 apply.

My findings suggest that school feeding should be complemented by other educational programmes to improve educational outcomes in the long run. School feeding is also best targeted at the most vulnerable of schools or villages; however, if villages oscillate between food security and insecurity, then casting a wider net can be all-important when shocks to food availability are widespread. This is especially true of predominantly rural economies such as Malawi, which rely on rain-fed agriculture.

Finally, the impact heterogeneity that is documented in this chapter can help explain the often conflicting evidence with respect to the impact of school feeding on enrolment and retention rates. Therefore, this study can act as a bridge between studies such as Ravallion & Wodon (2000), Meng & Ryan (2010) and Kazianga et al. (2012), on the one hand, and articles such as Tan et al. (1999), Buttenheim et al. (2011) and Azomahou et al. (2019), on the other. Against this background, this study has also emphasised the caveats of not considering the interaction between programme design and the food security context at the time and place of programme observation, which has implications for the outcome of evaluations. Therefore, this study can provide valuable insights to inform the planning of future school feeding evaluations, as it underscores the importance of collecting the necessary information to ascertain whether food-related constraints are binding in the evaluators' samples of interest.

# B Appendix

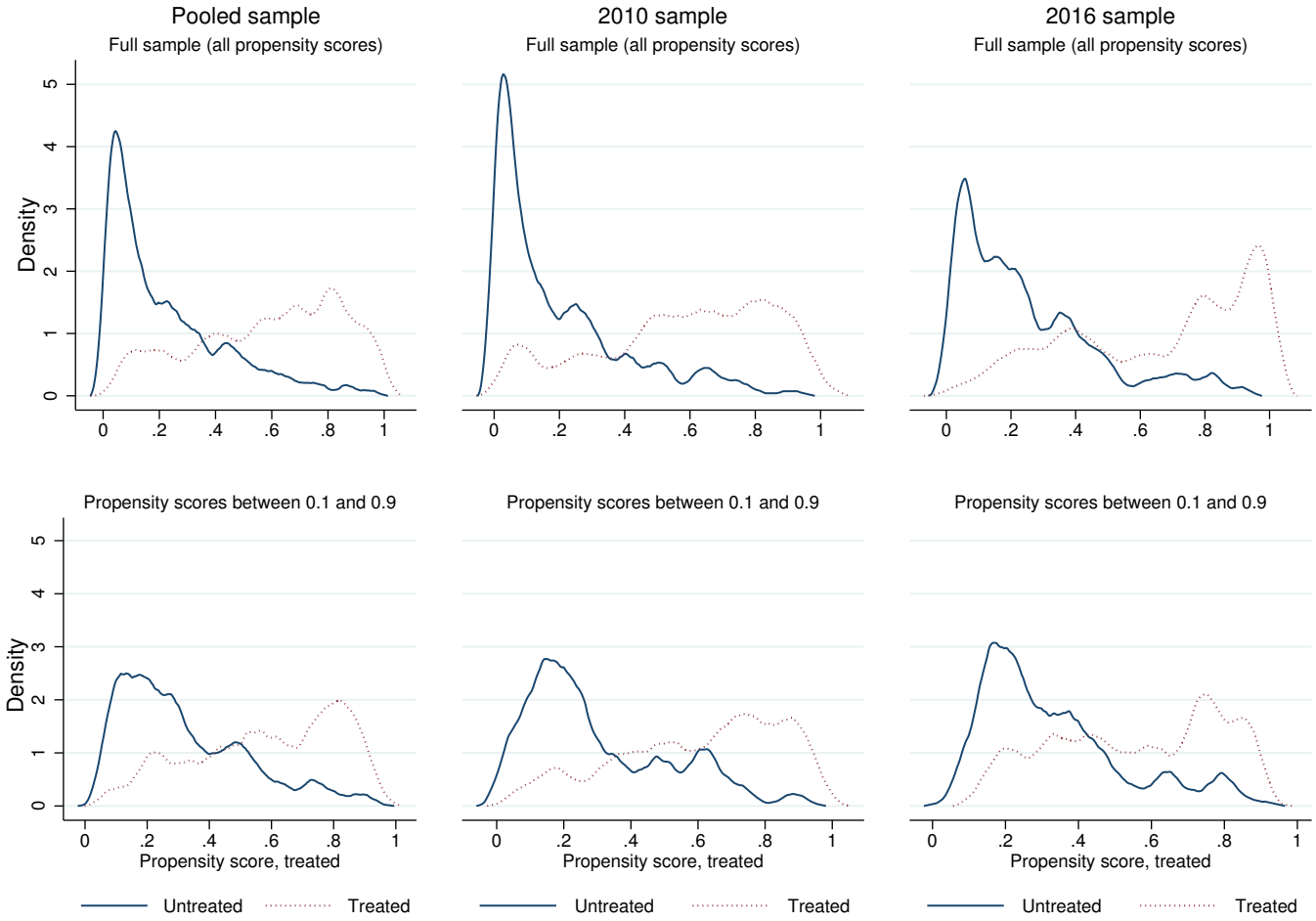
## B.1 First Stage Regressions, Main Analysis

| | BENEFICIARY SCHOOL FEEDING | | | | | |
|---|---|---|---|---|---|---|
| | Sample for extensive margin | | | Sample for intensive margin | | |
| **Explanatory variable** | Pooled | 2010 | 2016 | Pooled | 2010 | 2016 |
| ***Full sample*** | | | | | | |
| Community targeted | 0.280 | 0.365 | 0.204 | 0.297 | 0.391 | 0.215 |
| by school feeding | (0.007)*** | (0.012)*** | (0.009)*** | (0.007)*** | (0.013)*** | (0.009)*** |
| | [0.017]*** | [0.032]*** | [0.020]*** | [0.019]*** | [0.034]*** | [0.020]*** |
| $R^2$ | 0.31 | 0.39 | 0.28 | 0.33 | 0.44 | 0.30 |
| F | 66 | 44 | 36 | 77 | 57 | 40 |
| N | 20,611 | 10,038 | 10,573 | 19,056 | 9,113 | 9,943 |

| | BENEFICIARY SCHOOL FEEDING | | | | | |
|---|---|---|---|---|---|---|
| | Sample for extensive margin | | | Sample for intensive margin | | |
| **Explanatory variable** | Pooled | 2010 | 2016 | Pooled | 2010 | 2016 |
| ***Overlap sample*** | | | | | | |
| Community targeted | 0.303 | 0.398 | 0.209 | 0.322 | 0.426 | 0.221 |
| by school feeding | (0.008)*** | (0.014)*** | (0.010)*** | (0.008)*** | (0.015)*** | (0.010)*** |
| | [0.018]*** | [0.036]*** | [0.021]*** | [0.008]*** | [0.038]*** | [0.021]*** |
| $R^2$ | 0.28 | 0.32 | 0.29 | 0.31 | 0.36 | 0.31 |
| F | 51 | 28 | 25 | 61 | 38 | 28 |
| N | 13,353 | 4,493 | 6,524 | 12,315 | 4,098 | 6,147 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 2.2 apply.

## B.2 Sufficient Overlap Assumption, Wave Samples




The figure plots the overlap between treated and untreated communities based on covariates. Propensity scores are predicted based on logit regressions at the community level. Treatment at the community level is regressed on the usual aggregated individual-, household- and village-level variables plus district dummies. For the pooled sample, a time dummy is also included.

## B.3 Falsification Test, Individual-Level Analysis

| | AT LEAST ONE PARENT HAS AT LEAST PRIMARY EDUCATION | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Pooled sample | | | | 2010 typical year | | | | 2016 drought year | | | |
| **Expl.** | ***Full Sample*** | | ***Overlap Sample*** | | ***Full Sample*** | | ***Overlap Sample*** | | ***Full Sample*** | | ***Overlap Sample*** | |
| **variable** | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| Benef. | 0.000 | -0.001 | 0.000 | 0.017 | -0.012 | -0.038 | 0.007 | 0.014 | 0.011 | 0.061 | 0.023 | 0.111 |
| school | (0.009) | (0.027) | (0.010) | (0.027) | (0.013) | (0.033) | (0.015) | (0.035) | (0.012) | (0.052) | (0.015) | (0.054)** |
| feeding | [0.013] | [0.042] | [0.014] | [0.040] | [0.018] | [0.048] | [0.018] | [0.047] | [0.018] | [0.081] | [0.022] | [0.073] |
| $R^2$ | 0.14 | 0.14 | 0.15 | 0.15 | 0.15 | 0.15 | 0.13 | 0.13 | 0.16 | 0.16 | 0.18 | 0.17 |
| F | 40 | - | 29 | - | 23 | - | 12 | - | 24 | - | 21 | - |
| $\chi^2$ | - | 3499 | - | 2429 | - | 2009 | - | 900 | - | 2053 | - | 1500 |
| N | 20,611 | 20,611 | 13,353 | 13,353 | 10,038 | 10,038 | 4,493 | 4,493 | 10,573 | 10,573 | 6,524 | 6,524 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is a dummy variable. The covariates from Table 2.2 apply. The education of the mother and father, which were covariates in Table 2.2, however, are no longer included in the specification.

## B.4 First Stage Regressions, Interaction Model

| | BENEFICIARY SCHOOL FEEDING | | BENEFICIARY SCHOOL FEEDING× SOME STRESS | |
|---|---|---|---|---|
| **Explanatory variables** | Sample for ext. margin | Sample for int. margin | Sample for ext. margin | Sample for int. margin |
| ***Full sample*** | | | | |
| Community targeted | 0.259 | 0.276 | -0.023 | -0.021 |
| by school feeding | (0.009)*** | (0.010)*** | (0.003)*** | (0.003)*** |
| | [0.024]*** | [0.025]*** | [0.007]*** | [0.007]*** |
| Community targeted | 0.046 | 0.046 | 0.355 | 0.373 |
| × Some stress | (0.013)*** | (0.013)*** | (0.009)*** | (0.010)*** |
| | [0.029] | [0.031] | [0.021]*** | [0.022]*** |
| $R^2$ | 0.31 | 0.33 | 0.40 | 0.44 |
| F | 65 | 76 | 47 | 57 |
| N | 20,611 | 19,056 | 20,611 | 19,056 |
| ***Overlap sample*** | | | | |
| Community targeted | 0.276 | 0.294 | -0.023 | -0.023 |
| by school feeding | (0.010)*** | (0.011)*** | (0.003)*** | (0.003)*** |
| | [0.026]*** | [0.028]*** | [0.008]*** | [0.009]*** |
| Community targeted | 0.056 | 0.055 | 0.360 | 0.378 |
| × Some stress | (0.014)*** | (0.015)*** | (0.010)*** | (0.011)*** |
| | [0.033]* | [0.036] | [0.024]*** | [0.025]*** |
| $R^2$ | 0.28 | 0.31 | 0.36 | 0.40 |
| F | 50 | 61 | 14 | 16 |
| N | 13,353 | 12,315 | 13,353 | 12,315 |

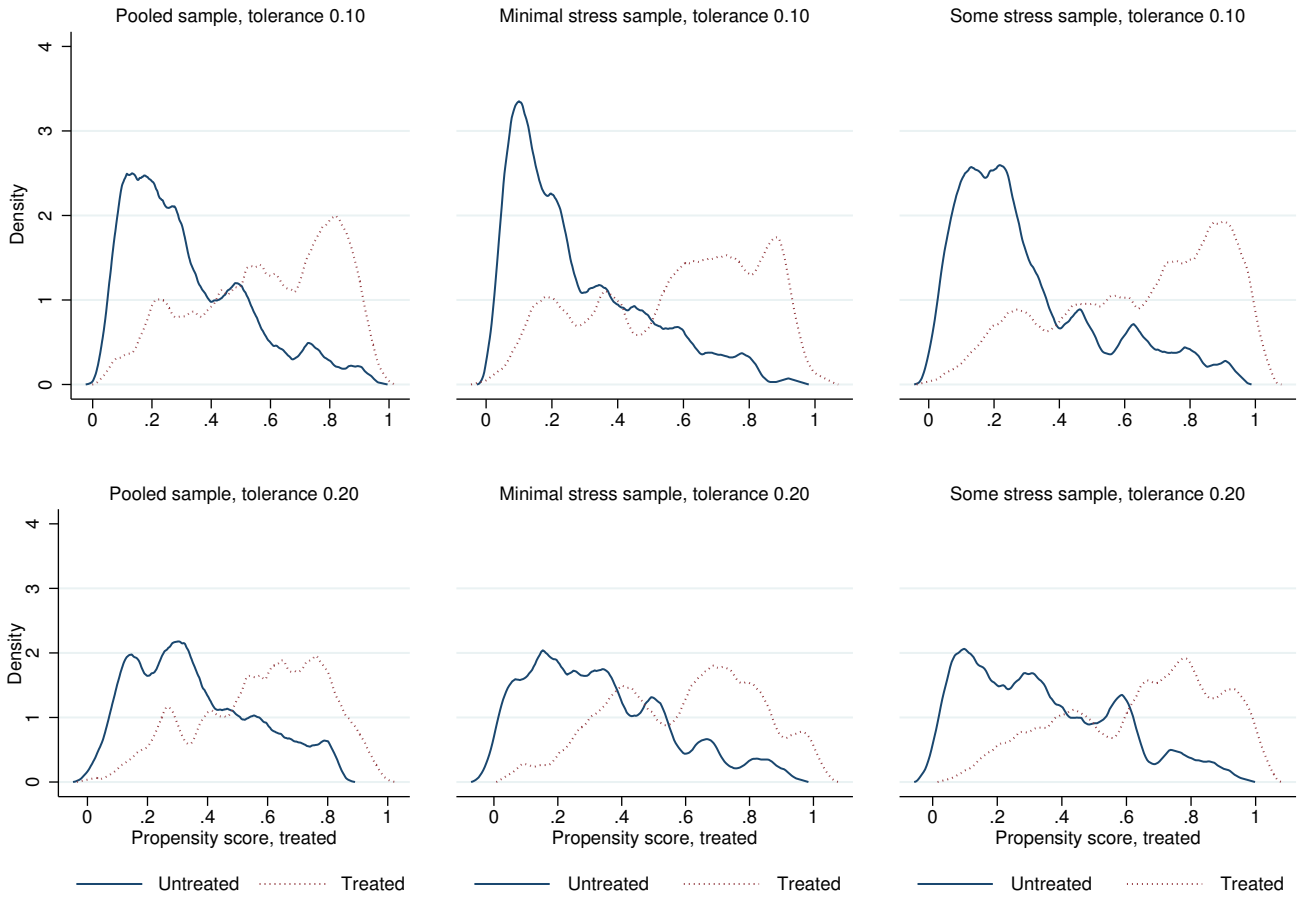
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 2.2 apply, plus the interaction term: instrument × food security.

## B.5 First Stage Regressions, Mechanisms

| | BENEFICIARY SCHOOL FEEDING | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 6+ sample | | 5+ sample | | Sample all children 6–14 | | Sample in-school children | |
| **Explanatory variable** | ***Full sample*** | ***Overlap sample*** | ***Full sample*** | ***Overlap sample*** | ***Full sample*** | ***Overlap sample*** | ***Full sample*** | ***Overlap sample*** |
| Community targeted by school feeding | 0.303 | 0.325 | 0.297 | 0.322 | 0.280 | 0.303 | 0.303 | 0.328 |
| | (0.008)*** | (0.009)*** | (0.007)*** | (0.008)*** | (0.007)*** | (0.008)*** | (0.007)*** | (0.008)*** |
| | [0.019]*** | [0.020]*** | [0.019]*** | [0.020]*** | [0.017]*** | [0.018]*** | [0.019]*** | [0.020]*** |
| $R^2$ | 0.34 | 0.32 | 0.33 | 0.31 | 0.31 | 0.28 | 0.34 | 0.32 |
| F | 66 | 51 | 74 | 59 | 66 | 51 | 82 | 65 |
| N | 15,527 | 10,069 | 18,549 | 11,979 | 20,611 | 13,353 | 18,602 | 12,020 |

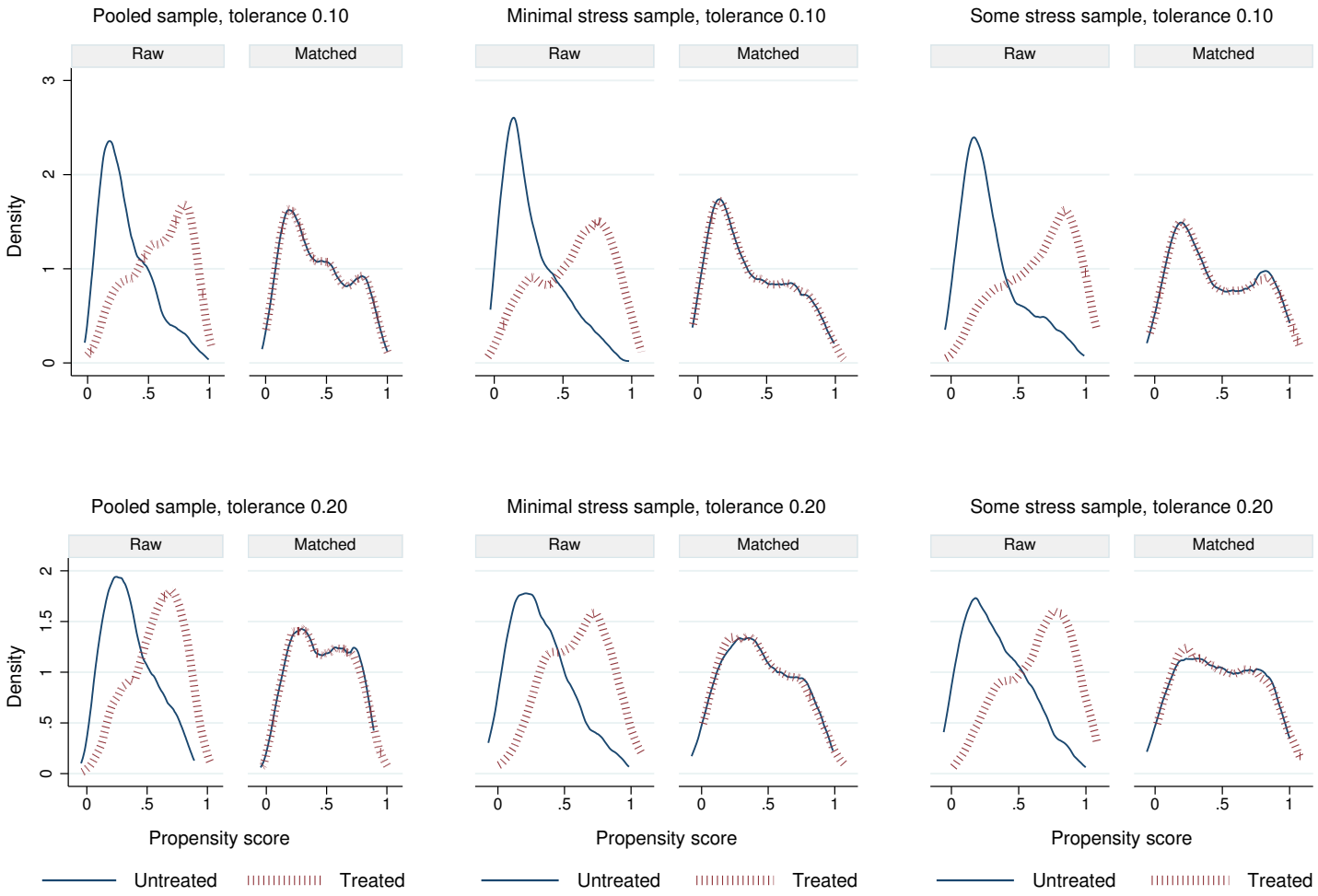
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 2.4 apply.

## B.6 Sufficient Overlap Assumption, Food Security Samples




The figure plots the overlap between treated and untreated communities based on covariates. The top row includes communities with a propensity score between 0.1 and 0.9. The bottom row only includes communities with a propensity score between 0.2 and 0.8. Note samples: pooled, food secure and food insecure.

## B.7 Balance Plots, Food Security Samples



## B.8 Falsification Test, Community-Level Analysis

| | AVG. IN SECONDARY SCHOOL EXTENSIVE MARGIN | | | | | |
|---|---|---|---|---|---|---|
| | # matches | | Level of tolerance overlap assumption | | | |
| **Explanatory variable** | # 1 | # 2 | # 0.01 | # 0.05 | # 0.10 | # 0.20 |
| ***Panel A: Pooled sample*** | | | | | | |
| Community targeted | -0.004 | -0.006 | -0.004 | -0.005 | -0.010 | -0.068 |
| by school feeding | (0.013) | (0.011) | (0.014) | (0.014) | (0.011) | (0.020)*** |
| Treated | 382 | 382 | 379 | 358 | 324 | 238 |
| Untreated | 710 | 710 | 688 | 543 | 426 | 278 |
| # matches | 1 | 2 | 1 | 1 | 1 | 1 |

| | | Level of tolerance overlap assumption | | | |
|---|---|---|---|---|---|
| | #0 | # 0.01 | # 0.05 | # 0.10 | # 0.20 |
| ***Panel B: Minimal stress sample*** | | | | | |
| Community targeted | -0.024 | -0.027 | 0.023 | 0.026 | -0.064 |
| by school feeding | (0.030) | (0.031) | (0.042) | (0.015)* | (0.020)*** |
| Treated | 160 | 159 | 151 | 141 | 99 |
| Untreated | 448 | 423 | 292 | 219 | 122 |

| | | Level of tolerance overlap assumption | | | |
|---|---|---|---|---|---|
| | #0 | # 0.01 | # 0.05 | # 0.10 | # 0.20 |
| ***Panel C: Some stress sample*** | | | | | |
| Community targeted | -0.031 | -0.038 | 0.015 | 0.030 | -0.003 |
| by school feeding | (0.023) | (0.014)*** | (0.040) | (0.034) | (0.058) |
| Treated | 198 | 183 | 162 | 142 | 105 |
| Untreated | 242 | 228 | 195 | 170 | 113 |
| # matches | 1 | 1 | 1 | 1 | 1 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 2.5 apply.

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# Chapter 3

# Heterogeneous Impacts of School Fee Elimination in Tanzania: Gender and Colonial Infrastructure

*Co-authored with* **Pedro Naso**

## Abstract

In this study, we investigate the impacts of the 2002 elimination of primary school fees in Mainland Tanzania. We explore how the magnitude of these effects depends on gender and the size of early investments in the educational infrastructure of Tanganyika. We use the 2002 and 2012 census waves as well as historical information on the location of schools in the late 1940s, and conduct a difference-in-differences analysis. We find that exposure to an average of 1.7 years of free primary education has reduced the proportion of people who have never attended primary education by 6.8 percentage points. The benefits of fee removal have been significantly larger for females compared to males, and females from districts where the size of investments in education was relatively larger during colonial rule have been the greatest beneficiaries.

**JEL classification**: I28, J16, N37.

## 3.1 Introduction

The evidence regarding the persistence of historical events and their impacts on current socio-economic outcomes is well documented (Nunn, 2009, 2014b). It is argued that culture (Nunn & Wantchekon, 2011; Alesina et al., 2013), the existence of multiple equilibria (Redding et al., 2011; Bleakley & Lin, 2012) and the shaping of domestic institutions (Acemoglu et al., 2005; Dell, 2010; Vogler, 2019) facilitate the persistence of the impact of historical events. In contrast, evidence regarding how the persistence of historical institutions and events affect the outcomes of contemporary reforms is limited. Governments implement reforms with the objective of improving outcomes on average as well as to even out the distribution of outcome values across individuals. Disparities exist for a variety of reasons. For instance, historical institutions and infrastructure have been linked to the uneven distribution of resources and socio-economic outcomes—whether by favouring one group over the general population (Becker & Woessmann, 2008; Huillery, 2009; Calvi & Mantovanelli, 2018) or by holding back some populations (Frankema, 2010; Bruhn & Gallego, 2012; Naritomi et al., 2012). Thus, we ask these questions: How do current reforms interact with the legacies of historical events and institutions? Do they mitigate or perpetuate historical disparities?

We show that current development policies may feed the disparities created by history as opposed to eliminating them. We evaluate the impacts of the 2002 elimination of primary school fees in Tanzania. Then, we explore the interaction of these impacts with the early colonial and missionary infrastructure of Tanganyika, which has created a pattern whereby the gender gap in education is smaller in districts that have benefited from relatively larger investments during colonial rule.

We find that the elimination of fees has improved educational outcomes on average. We estimate a 6.8 percentage point reduction in the ratio of people who have never enrolled in school and a 6-month improvement in time spent in primary education.[1,2] Moreover, the reform has also reduced the educational gender gap. Furthermore, females from districts with a stronger history of investments in education have been the greatest beneficiaries of the reform even though they were already better off compared to other females. For instance, a standard deviation increase in the number of Protestant or Catholic schools scaled per 100,000 students leads to a further reduction of 1.7 percentage points in the ratio of females who have never enrolled in primary education. Females from districts with historically low investments in education would have required the largest boost. Nevertheless, they were held back as their female counterparts from districts with denser school coverage in the 1940s benefited disproportionately. Finally, we compile evidence that one of the mechanisms at play is the persistence of investments. Districts with a larger educational infrastructure in the past continue to have a larger infrastructure today.

[1]Estimated for an average reform intensity of 1.7 additional years of free education.

[2]These effects apply to cohorts that were of school age at the time of the reform or shortly after. The persistence of these impacts should be studied again when more recent data becomes available. We caution that this is not the first time Tanzania removed fees in a bid to raise enrolment rates (World Bank & UNICEF, 2009; Somerset, 2009).

We employ a difference-in-differences estimation strategy with multiple layers of fixed effects. We use a panel of gender-age-district cohorts observed in 2002 and 2012. This panel is merged with historical data on the educational infrastructure of Tanganyika around the late 1940s. For this purpose, we have geo-referenced a map documented by Buchert (1991), which includes information on the type of school infrastructure: Catholic, Protestant or village authority. To estimate the magnitude and heterogeneity of reform impacts, we had to overcome a serious obstacle posed by the simultaneous, country-wide implementation of the reform. We have exploited the fact that some individuals are treated while others are not, depending on one's year of birth. Additionally, for those who are treated, the intensity of their exposure to the reform depends on the pre-reform educational performance of their district. For instance, in districts with low pre-reform performance, exposure to the benefits of free primary education will be at its highest. This identification strategy has already been used to evaluate the impacts of fee elimination in Ethiopia (Chicoine, 2019, 2020), Kenya (Lucas & Mbiti, 2012a,b) and Malawi (Zenebe Gebre, 2019). We elicit reform heterogeneities by interacting the reform with variables denoting gender and the colonial school infrastructure of Tanganyika. The parallel trends assumption is validated and strongly supportive of our difference-in-differences strategy.

The contribution of our study is threefold. First, we bring additional evidence to the literature discussing the implementation of free primary education. Our results are similar to those estimated for other African countries, although the magnitude of our estimates is slightly smaller than in the case of Ethiopia (Chicoine, 2019) and less than half of the effects found by Zenebe Gebre (2019) for Malawi. Deininger (2003), Nishimura et al. (2008) and Grogan (2009) also confirm our findings. The authors evaluate the 1997 fee reform of Uganda and show that girls have experienced larger improvements relative to boys. In contrast, evaluations of the Kenyan reform find a higher impact on the graduation rates of boys compared to girls, thus contributing to a widening of the gender gap in favour of boys (Lucas & Mbiti, 2012b). Regarding evidence on Tanzania, Hoogeveen & Rossi (2013) have already attempted to evaluate the elimination of fees. They exploit the staggered absorption of out-of-school children as an identification strategy. In line with our own findings, they show that the elimination of fees has increased the probability of children being in school at age 7. Unlike our study, they are unable to assess the effects of the reform on completed education due to data limitations. We further refine their findings by using a different identification methodology and exploring the heterogeneity of impacts with respect to the historical infrastructure of Tanzania.

Second, we contribute to the literature assessing the long-term impacts of historical events on education. The study closest to ours is the cross-country analysis of Nunn (2014a). Similar to his findings, we argue that the investments of Protestant missions have had positive impacts on the educational outcomes of females relative to males. Becker & Woessmann (2008) have also put forward this hypothesis. However, unlike Nunn (2014a) who does not find any significant effects of Catholic schools on female education, we do find evidence that these schools are associated with smaller educational gaps between females and males in Tanzania. Finally, Montgomery (2017) also evaluates the impacts of the

educational infrastructure erected during colonial rule in Tanzania. Whereas Montgomery (2017) argues that the infrastructure of German East Africa is linked to higher gender-based educational gaps in present-day Tanzania, we find that the British and missionary infrastructure of the 1940s is associated with smaller gaps in the long run.

Third and last, we contribute to a relatively limited strand of the literature which shows that historical legacies inform the success of reforms down the line (Pop-Eleches, 2007). Similarly, the patterns created by other phenomena that are characterised by persistence, i.e., traditions, have also been shown to endure in the aftermath of reforms (Ashraf et al., 2020). For instance, girls raised in the bride price tradition are better educated, and educational policies have benefited them more relative to girls from other traditions. The hypothesised channel is the marriage market (Ashraf et al., 2020). In this paper, we show that Tanganyika's historical infrastructure speaks to the distribution of current reform impacts because of the spatial persistence of investments in educational infrastructure.

## 3.2 Context

### 3.2.1 Educational Infrastructure during Colonial Rule

Modern-day Mainland Tanzania was German East Africa between 1891 and the end of World War I. Thereafter, Britain gained control over the area, which became known as Tanganyika. Although present since the 1840s, mission schools were initially few and far between (Buchert, 1991). Teaching was conducted in local languages (Tabetah, 1982), and the main focus was to "*civilise*" and convert the local populations to Christianity (Tabetah, 1982; Buchert, 1991). Mission schools expanded geographically during the German colonial rule (Gillette, 1977; Buchert, 1991), and while proselytisation remained their most important goal, they also incorporated secular teachings (Buchert, 1991). Mission schools were open to both boys and girls (Gillette, 1977).

Besides the missionary infrastructure, the government also set up its own schools. Its concerns were non-religious and focused on vocational, civic and general education (Gillette, 1977). However, the government infrastructure was limited, clustered around the coast and only served the male children of chiefs (Gillette, 1977). As of 1913, there were roughly 115,000 children in school and 95 percent of them were educated in missionary schools (Buchert, 1991). Nevertheless, most of this educational groundwork was substantially damaged during World War I (Cameron & Dodd, 1970; Chachage, 1988; Buchert, 1991).

During the interwar period, the British administration promoted a laissez-faire philosophy and avoided assuming any active role in the educational sector (Cameron, 1967; Buchert, 1991). The colonial state adopted policies that favoured adaptation so as to "*preserve traditional societies*" (Buchert, 1991). This translated into a vocational curriculum, which relied on the use of local languages and the implementation of agricultural and practical activities, with the ultimate purpose of preparing and training Africans to accept and serve the economic and political goals of the colony (Buchert, 1991).

This policy of non-interference applied to government-assisted schools; however, these were in limited supply during the interwar period (Cameron, 1967). The overwhelming majority of the educational infrastructure was still made up of mission schools that were *not* assisted by the state and which chiefly brought literacy skills to converts as a result of religious teachings (Buchert, 1991). In 1924, 21 percent of children were in school: 5,000 attended 72 public schools and 162,000 children studied in mission schools (Siwale & Sefu, 1977).

As a result of Britain's system of indirect rule, which relied on local leaders to govern the territory, revenues from taxation provided traditional authorities with an independent source of finance to develop their local infrastructure, e.g., schools (Chachage, 1988; Buchert, 1991). Consequently, village- or "*native*-" authority schools, were set up in addition to government and mission schools. The local-authority schools had similar aims as government schools. They did not have any religious goals but focused on educating local chiefs, headmen and their children (Cameron & Dodd, 1970). Ultimately, these schools came under government control (Gillette, 1977), although there were a few exceptions, such as the schools set up by the Chagga people who continued to use revenues from their cash crops to fund and supervise their schools (Cameron & Dodd, 1970).

World War II spared Tanganyika in ways that World War I did not. World War II caused a rise in the prices of primary goods, some of which were produced by Tanganyika. Thus, state revenues increased (Cameron & Dodd, 1970). Moreover, the role of education started to become widely recognised, as the colonial state required skilled labour to meet the needs of a modern economy which relied on the production of cash crops (Buchert, 1991). Furthermore, international pressure for social development mounted (Cameron, 1967; Siwale & Sefu, 1977). Consequently, the British colonial state adopted a new, interventionist philosophy as a provider of financial resources and inspector of school activities (Cameron, 1967; Cameron & Dodd, 1970; Buchert, 1991). The active involvement of the British administration in the educational sector has ultimately favoured the country-wide progressive migration from a vocational to an academic curriculum. Mission schools had to conform to the centrally-determined curriculum, which reduced the importance of religious teachings, so as to be eligible for financial assistance (Cameron, 1967; Buchert, 1991). In fact, unassisted mission schools saw their numbers decrease (Buchert, 1991). In 1945, they represented roughly 60 percent of all mission schools (Cameron & Dodd, 1970). Instruction in local languages was virtually replaced by Swahili in all types of schools. The basic, four-year primary school cycle included classes on arithmetic, reading, writing, religion, health and hygiene, general knowledge, physical education, singing, agriculture and handwork (Cameron & Dodd, 1970).

Throughout colonial rule, school teachings have been gender-specific regardless of school type, with girls usually being disadvantaged both in quantitative as well as qualitative terms (Mbilinyi et al., 1991; Olekambaine, 1991). At the same time, however, the colonial state was not particularly invested in the education of African boys either. The gender ratio was roughly 1 girl to 3 boys among primary school students in 1956 (Gillette, 1977; Siwale & Sefu, 1977). Missionaries favoured the numbers, "*the production of sincere, edu-*

*cated Christians, of whom the more the merrier*" and only offered rudimentary education (Cameron & Dodd, 1970), "*enough to understand the Bible but not so much as to result in pupils turning away from the church*" (Gillette, 1977). In contrast, government schools were fewer, had fewer students as well, and were more preoccupied with the creation of individuals ready to take on administrative tasks in the government apparatus.

### 3.2.2 Post-independence Educational Developments

In 1958, three years before independence, only 24 percent of children aged 5–14 were in school in Mainland Tanzania (Omari et al., 1983). Despite this situation, the post-independence government was slow to expand the primary education sector during the 1960s (Omari et al., 1983; Oketch & Rolleston, 2007). As of the late 1960s, girls continued to be under-represented among primary school students. In 1968, they represented 39 percent of an enrolled population of 750,000 children (Gillette, 1977; Siwale & Sefu, 1977).

The Arusha Declaration (1967) was the first step toward the concept of Education for All in a bid to build a Tanzanian national identity (Jerve, 2006). Then, the Musoma Resolution of 1974 declared Universal Primary Education a national priority. Other reforms included the first attempt to remove primary school fees in 1973-74 (Galabawa, 1990; World Bank & UNICEF, 2009), the building of schools[3] and the introduction of compulsory schooling laws in 1978. The educational reforms of the 1970s had finally allowed schools to reach full parity around 1984–85 (Olekambaine, 1991). The net and gross primary school enrolment rates in 1981 were 69.7 and 98.3 percent, respectively (Ishumi, 2014).

The educational outcomes of the early 1980s were encouraging; however, the expansionary efforts of the government also exacted a toll on the quality side of education. To meet demand and stay within budget, most of the newly deployed teachers were insufficiently qualified (Omari et al., 1983; Galabawa, 1990). Despite their increasing numbers, classrooms were also often insufficient to absorb new entrants. Lastly, because of high population growth rates and the adverse economic conditions of the 1980s[4], the quality of the educational system deteriorated further (Galabawa, 1990; Jerve, 2006). Fees were reinstated and education-related costs increased (Jerve, 2006). By 1990, the net and gross enrolment rates had decreased to 59.6 and 80.7 percent, respectively (Ishumi, 2014).

### 3.2.3 Free Primary Education

Following the deteriorating educational situation of the late 1980s and 1990s, the Government of Tanzania adopted the Primary Education Development Plan (PEDP) (Government of Tanzania, 2001). Per this plan, primary school fees and all other mandatory

[3]There were 3,238 primary schools in 1961, 4,070 in 1970 and 9,931 in 1980 (Ishumi, 2014).

[4]These adverse conditions amounted to the oil crises of the 1970s, Tanzania's war with Uganda and agricultural stagnation (Buchert, 1991; Jerve, 2006; Vavrus & Moshi, 2009).

parental contributions were removed as of January 2002.[5,6] To compensate schools for the loss in income, the PEDP introduced a capitation grant of US $10 (TSh9,000) per child per year as well as an investment grant to build the necessary classrooms, sanitation facilities and teachers' accommodation. The capitation grant was increased to TSh10,000 in 2006 (Government of Tanzania, 2006) and has since remained constant (Mbiti et al., 2019).

Table 3.1: Pre- and Post-reform Infrastructure

| | | **PRE-REFORM** | | **POST-REFORM** | |
|---|---|---|---|---|---|
| | | **1999** | **2001** | **2002** | **2005** |
| (1) | Population of 7–13 children[a] | 5,427,156 | 5,679,676 | 5,810,309 | 6,220,512 |
| (2) | Gross enrolment rate | 77.2%[b] | 84%[c] | 99%[c] | 105.41%[d] |
| (3) = (1) × (2) | Children in school | 4,189,764 | 4,770,928 | 5,752,206 | 6,557,042 |
| (4) | Stock of classrooms start of year | 57,367[e] | est. 60,000[e] | est. 60,000[e] | 89,875[f] |
| (5) | Stock of teachers start of year | 103,966[g] | 102,313[h] | est. 109,665[i] | 134,638[j] |
| (6) = (3) ÷ (4) | Student-to-classroom ratio | 73:1 | 80:1 | 96:1 | 73:1 |
| (7) = (3) ÷ (5) | Student-to-teacher ratio | 40:1 | 47:1 | 52:1 | 49:1 |

[a]World Bank (2001), Annex H, Table 3 and PEDP, Annex 3, Table 1. [b]World Bank (2001), Annex H, Table 3. [c]World Bank (2005), Chapter 4, Table 1. [d]World Bank DataBank and UNESCO Institute for Statistics, ID: SE.PRM.ENRR. [e]World Bank (2001), Annex H, Table 5. [f]Stock of 1999 plus the project-declared output of 29,922 classrooms and plus the output of 2,586 classrooms built under a related World Bank Project (World Bank, 2005). [g]World Bank (2005), Annex H, Table 4. [h]Based on the 1999 stock less attrition at 1.59%. Teacher hirings were frozen (World Bank, 2001). [i]Considering the 1999 stock and attrition rates of approx. 1.59% per year between 1999 and 2000, the stock would have been of 100,665 at the end of 2001 (World Bank, 2001). However, the government planned to hire approx. 9,000 teachers by 2002. This stock of unemployed teachers is likely to have existed because of a prior freeze on teacher hirings (World Bank, 2001). The target would have been missed only if deployment had failed. In a bid not to underestimate the 2002 teacher capacity, we assume all 9,000 teachers were recruited. [j]Stock of 2001 plus the project-declared output of 32,325 teachers (World Bank, 2005). The strategy of temporarily employing double-shift teaching has underperformed. (World Bank, 2005).

The expected surge in enrolment due to the elimination of fees has motivated the government to adopt several measures to avoid overwhelming the educational system, e.g., implementing a staggered absorption of children and expanding the educational infrastructure. The government built 29,922 classrooms and hired 32,325 teachers during 2002–04. Although impressive, these efforts barely managed to maintain educational services at pre-reform levels. See Table 3.1. While the increase in enrolments at the start of 2002 was notable and a considerable break from previous trends, the change in infrastructure was minimal in 2002. Relative to the number of students, the infrastructure shrank.

[5]Zanzibar is an autonomous administrative region and has followed a different reform schedule. The timing of the census data does not allow for the study of the reform in Zanzibar.

[6]Before their elimination, school fees were estimated to have been roughly US $4.6 per child per academic year (Valente, 2019). Monthly food and non-food expenditure per capita was estimated at TSh10,120 (US $12.5) for Mainland Tanzania in 2000–01 (National Bureau of Statistics, 2002). This included the monetary equivalent of the food grown by the household. Moreover, Sumra (2017) documents that the overall education-related costs, fees and parental contributions included, were US $8 to 16 per year per child—the equivalent of one to two months' worth of agricultural wages.

## 3.3 Data and Summary Statistics

We use two sources of data to estimate the impact of the elimination of fees on educational outcomes. First, we rely on the 2002 and 2012 Tanzanian census data which were made available by the Tanzanian Bureau of Statistics and distributed by the Minnesota Population Center (2018). Second, we have geo-referenced the map in Figure 3.1, which was originally included in a 1947 report by His Majesty's Government on the administration of Tanganyika and documented by Buchert (1991).

### 3.3.1 Census Data

The census data are collapsed such that the units of observation consist of gender-age-district-year groups. The administrative borders of districts in Tanzania have changed between census years. In order to present the 2002 and 2012 data in terms of the same administrative demarcations, we use the 1988 district borders, as both the 2002 and 2012 administrative units can be traced back to their more encompassing 1988 polygons. Consequently, we have a panel where gender-age-district groups are observed twice.

Table 3.2: Summary Statistics and T-tests: Census Data

| | **Mean** | $\sigma$ | **Min** | **Max** | **Mean Fem.** | **Mean Males** | **Diff.** | **t-stat** | **p-val** |
|---|---|---|---|---|---|---|---|---|---|
| Ratio without any education | 0.164 | 0.123 | 0.000 | 0.816 | 0.188 | 0.141 | 0.047 | 16.33 | 0.00 |
| Years primary educ. (all individuals) | 5.471 | 0.939 | 1.236 | 6.979 | 5.358 | 5.585 | -0.227 | -10.19 | 0.00 |
| Years primary educ. (only enrollees) | 6.530 | 0.360 | 4.091 | 6.989 | 6.577 | 6.482 | 0.095 | 11.11 | 0.00 |
| Gender-age-district-period groups | 7,004 | 7,004 | 7,004 | 7,004 | 3,502 | 3,502 | - | - | - |

Sample weights have been used to compute all variables. 103 Mainland districts as per the 1988 demarcations $\times$ 2 genders $\times$ 2 periods (2002 and 2012) $\times$ 17 age groups (14 to 30 years old) gives 7,004 observations. For the t-test, $H1$ is Difference $\neq 0$.

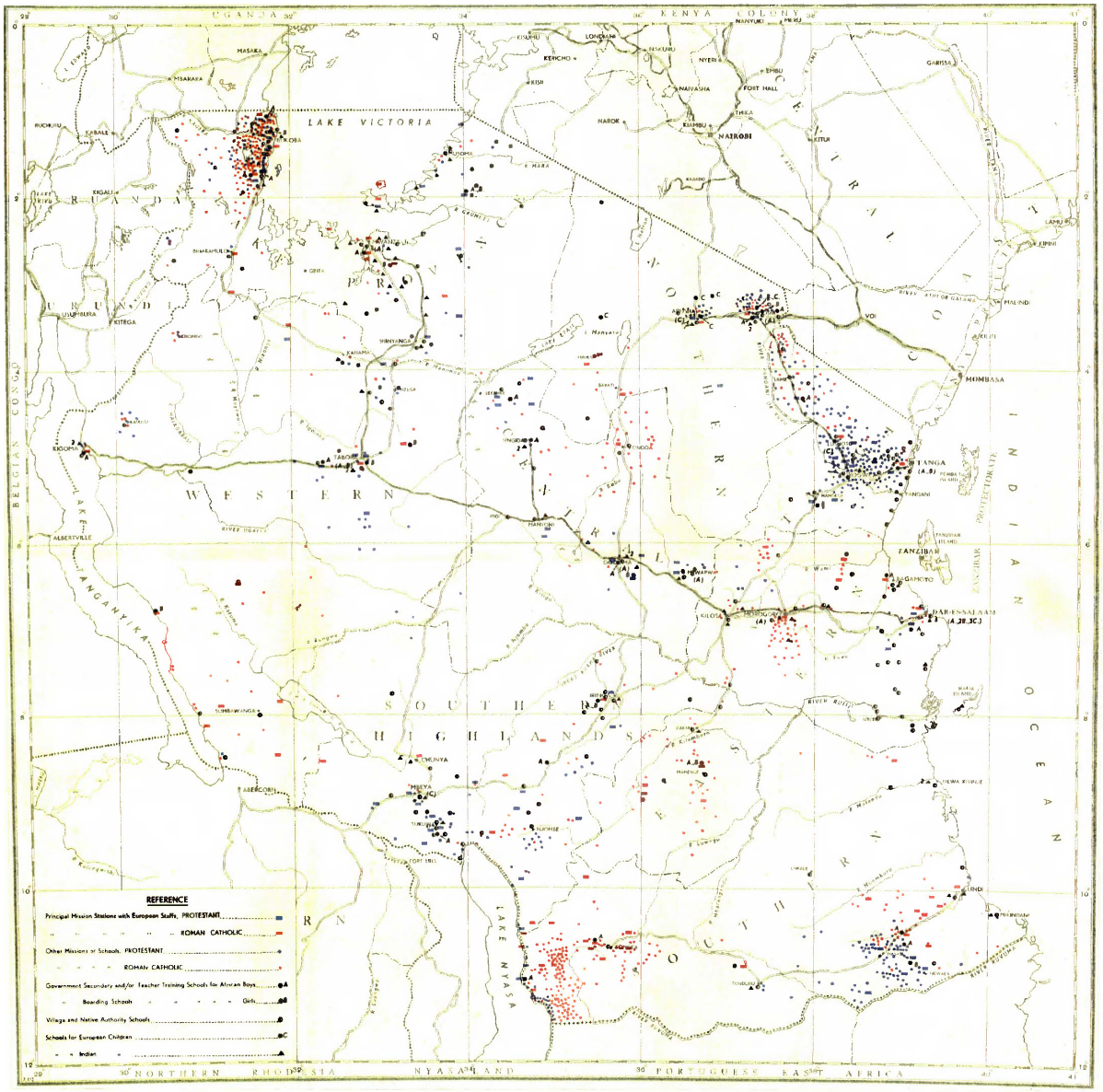
The summary statistics presented in Table 3.2 show that there is a statistically significant difference between women and men in terms of their school attendance, with 19 percent of women never having attended school in the 2002 and 2012 census waves, while only 14 percent of men experienced the same situation. Furthermore, once in school, females seem to acquire more years of education than their male counterparts do. The difference is slight but significant. However, because girls are more likely to be out of school than boys, the overall girl-boy educational gap is significant, with girls performing worse.

### 3.3.2 Colonial Infrastructure Data

Map 3.1 presents the educational school infrastructure of Tanganyika around the late 1940s. We have geo-referenced 1,291 schools of which 177 are government/village-authority schools, 634 are Catholic mission schools and 480 are registered as Protestant. The map suggests that while there are denominational clusters, e.g., Catholic schools in and around the Ruvuma region and Protestant schools in Kilimanjaro, there are also areas with a fair

amount of mixing, such as Kagera. Government schools accompany mission schools, but they also cover remote areas which were otherwise poorly served by mission schools. The map includes additional information that we do not use. For instance, we exclude secondary and teacher training schools from the analysis as well as the schools for European and Asian children. They represent a very small fraction of the total number of schools.

Figure 3.1: Educational Facilities in Tanganyika Cca. 1947




*Source:* Report by His Majesty's Government on the administration of Tanganyika, 1947. Documented by Buchert (1991), PhD Dissertation. International and Comparative Education, University of London.

In order to account for the fact that population clusters will be accompanied by a larger number of schools, we scale the number of schools to district population. For lack of data regarding population numbers in the 1940s, we use the 2012 population of individuals aged 7–13. The assumption is that the distribution of the 1940s population among dis-

tricts is correlated with that of 2012.[7] Huillery (2009) also uses population numbers to scale historical institutions, but she does have access to historical population records. We opine that the use of populations as opposed to surface, i.e., Nunn (2014a), is advised for the scaling of variables in the case of Tanzania, as large swaths of land are uninhabited and make districts appear large without cause. Table 3.3 shows that an average district in 1947 was endowed with 17 schools per 100,000 children aged 7–13 in 2012. At the same time, some districts had no access to schools, while some had upwards of 100 schools.

Table 3.3: Summary Statistics: Colonial Infrastructure

| **Variables** | **Mean Sample** | % | $\sigma$ | **Min** | **Max** |
|---|---|---|---|---|---|
| All | 17 | 100 | 23 | 0 | 136 |
| Catholic | 8 | 47 | 16 | 0 | 115 |
| Protestant | 6 | 35 | 12 | 0 | 86 |
| Village authority | 3 | 18 | 4 | 0 | 31 |
| Districts | 103 | 103 | 103 | 103 | 103 |

Number of schools scaled per 100,000 individuals aged 7–13 in 2012.

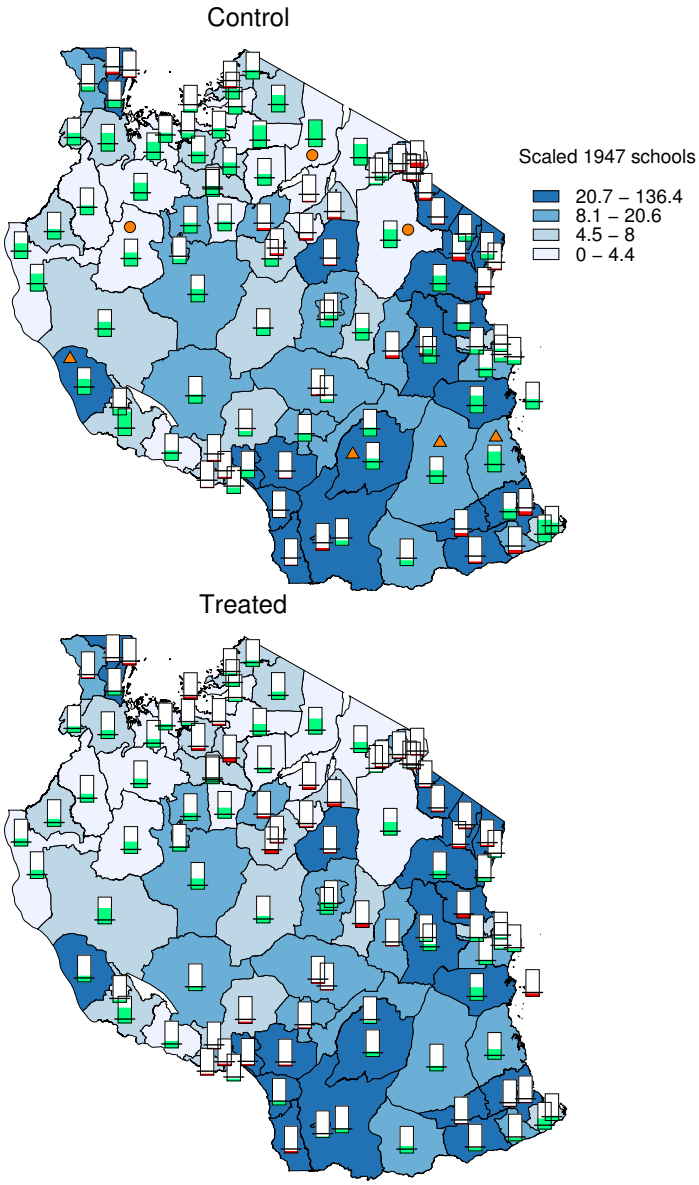
### 3.3.3 Gender Gap and Colonial Infrastructure

In Figure 3.2, we map the intensity of the colonial school infrastructure. The rectangle shapes denote the gap in enrolment between males and females. Green is for male advantage and red is for female advantage. The extent to which a rectangle is filled is indicative of the magnitude of the gap. The min and max values of the rectangle shapes are all the same across the maps to allow comparisons, i.e., 0 and 17 percent. The black horizontal lines that cut the rectangle shapes are the mean values for the control and treated groups, computed separately.

Several insights emerge from Figure 3.2. Enrolment gaps are substantially larger among the control cohorts than they are among the treated cohorts. We also notice a clustering of larger enrolment gaps in areas that had a lower density of schools at the end of the 1940s. We further note that for the case of districts with a denser school infrastructure in the 1940s, such as those marked with an orange triangle, the gap was substantially reduced for the treated cohorts. In contrast, for districts with low infrastructure, such as those marked with an orange circle, the mitigation of the enrolment gap was relatively modest. In the following sections, we explore these relationships using a regression analysis based on a two-period panel of gender-age-district cohorts.

[7]By consulting Figure 1b on page 141 of Trewartha & Zelinsky (1954) and Map 2 on page 7 of the Report on the 2012 Population and Housing Census (National Bureau of Statistics & Office of Chief Government Statistician, 2013), we acquire suggestive evidence that the population clusters of the early 1950s have largely stayed in place and overlap with those of 2012.

Figure 3.2: Never Enrolled Female to Male Differences and Colonial Infrastructure




The *rectangular diagrams* plot the difference between the ratio of females and males who have never enrolled in school. Green means a male advantage. Red is for female advantage. The *bottom and top lines of the rectangular* diagrams are the min and the max of the gap taken across both years. Therefore, the gaps plotted on the two maps are comparable. The *horizontal, black line* which crosses the rectangle shapes is the mean computed for the control and treated groups, respectively. Only the 14–23 age cohorts are considered. In 2002, they were not yet treated, but in 2012, they are all either partially or fully treated.

## 3.4 Identification Strategy

### 3.4.1 Reform Package

The elimination of school fees was part of a wider package of interventions that were put forward by the PEDP (Government of Tanzania, 2001), among which were the building of classrooms and the hiring of teachers. Consequently, we risk confounding the impact of school fee elimination with that of the infrastructure development. However, due to the

timing mismatch between the enrolment surge, the construction of classrooms and the hiring of teachers, which are documented in Table 3.1, it becomes apparent that a larger number of parents decided to send their children to school as a result of the removal of fees rather than the infrastructure developments. The jump in enrolments was immediate, as was the elimination of fees, while infrastructure developments took time to materialise.

Therefore, we argue that it was the removal of fees rather than these infrastructure developments which paved the way for improvements in education post-reform. Hoogeveen & Rossi (2013) adopt the same approach for their case study on Tanzania, and so do Deininger (2003), Grogan (2009) and Nishimura et al. (2008) in their studies of the Ugandan fee reform. Per this argument, infrastructure improvements have chiefly enabled the absorption of new enrolments, which were in return driven by the removal of fees.

### 3.4.2 Reform Intensity

An additional identification problem is posed by the fact that school fees were eliminated simultaneously across the territory of Mainland Tanzania. This complicates our identification mission because the reform may have overlapped with other country-wide developments which risk confounding the impacts of the reform. To address this issue, we employ a methodology that was initially used to evaluate the impacts of anti-malaria interventions on education and fertility in Sri Lanka and Paraguay (Lucas, 2010, 2013). Ultimately, this methodology was also applied to evaluate educational reforms that were implemented country-wide at the same time (Lucas & Mbiti, 2012a,b; Chicoine, 2019, 2020). The strategy relies on the fact that some individuals are treated while others are not, depending on one's year of birth and consequent age at the time when fees were eliminated. Moreover, for those who are treated, their potential response to the reform depends on their district's pre-reform educational performance. That is, the reform has had various degrees of geographical intensity. Districts which were performing poorly pre-reform will have a higher potential to improve following the elimination of school fees, while those with already satisfactory performance will have relatively less room for improvement, which means the intensity of the reform will be lower for the latter districts.

The reform intensity variable takes value zero for all age groups that were 14 or older in 2002 (born in or before 1988) and non-zero increasing values for cohorts aged 8–13 in 2002 (born between 1989 and 1994), as they were in school already when primary school fees were eliminated. Seven-year-olds and younger age groups (born in or after 1995) have been fully exposed to the reform and are assigned the highest values of reform intensity.[8] At the same time, intensity varies across districts and genders, as pre-reform educational attainment is averaged for each gender in each district. For the benchmark indicator, the pre-reform gender-district educational attainment is computed using the 2002 census, namely data from 14 cohorts born between 1970 and 1983, 19–32 years old in 2002. As a robustness check, we have also computed the intensity indicator based on (i) the 1970–76 cohorts and (ii) the 1977–1983 cohorts only. See Appendices C.1 and C.4.

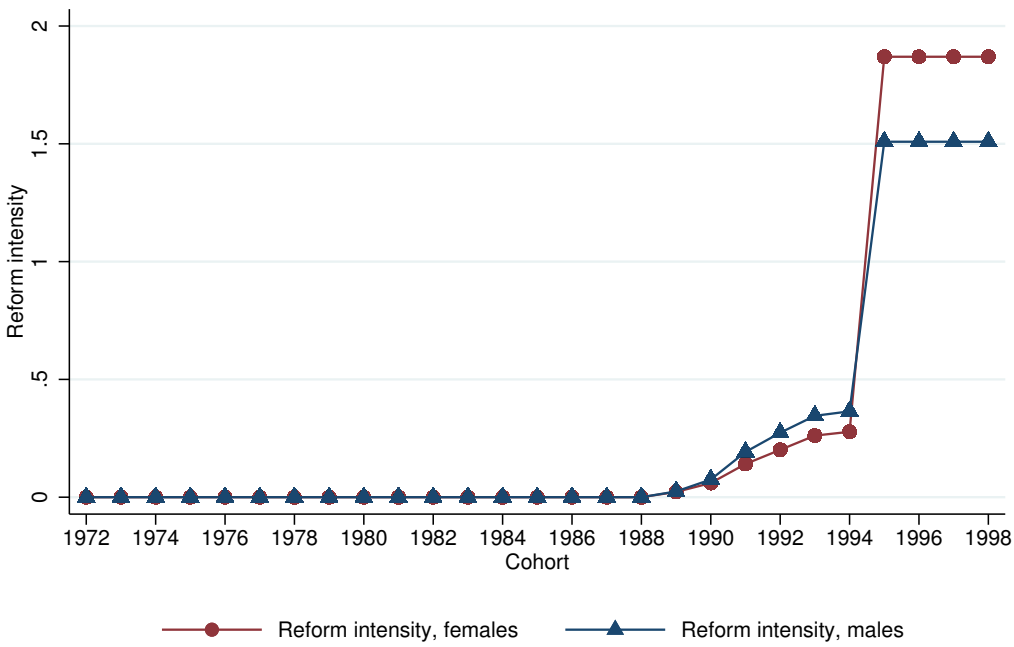
[8] To the extent that children start school at later ages, we are underestimating the impact of the reform, as some treated individuals might be classified as control and *vice versa*.

Following Chicoine (2019), the equations in System 3.1 summarise the reform intensity variable in district $d_n$ for gender $s_i$; $n \in \{1, ..., 103\}$. Reform intensity can be interpreted as the maximum number of additional years of schooling for each gender in district $d$ that can ensue from the elimination of primary school fees. Thus, the theoretical magnitude ranges from 0 to 7 and is inversely related to pre-reform educational performance.

$$Intensity_{c,d_n,s_i} = \begin{cases} \sum_{g=0}^{7}(7-g) \times F_{g,d,s} & \text{if } c \geq 1995 \\ \sum_{g=1995-c}^{7}(7-g) \times F_{g,d,s} & \text{if } 1989 \leq c \leq 1994 \\ 0 & \text{if } c \leq 1988 \end{cases} \tag{3.1}$$

where $c$ is for year of birth and $g$ is for years of primary education (i.e., 0 to 7). $F_{g,d_n,s_i}$ is defined as in Chicoine (2019). In each district $d_n$, some pre-reform gendered fraction $F$ of individuals has never attended school: $F_{0,d_n,s_i}$. Intuitively, these individuals would have benefited the most from the elimination of fees, as they had a seven-year gap to remedy, wherefrom the pre-multiplication of $F$ by $7 - g = 7$. Moreover, some fraction of individuals of gender $s_i$ in district $d_n$ have only completed 1 year of primary education: $F_{1,d_n,s_i}$. Analogously, $F_{7,d_n,s_i}$ denotes the fraction of people of gender $s_i$ who have completed primary education in district $d_n$. This fraction of the population would have had nothing to gain from the removal of fees, and this is reflected by the $7 - g$ pre-multiplication, whereby $g$ is 7 in this case. Figure 3.3 graphs the national average of the reform intensity variable per gender and year-of-birth cohort for the analysis sample.

Figure 3.3: Reform Intensity by Gender and Birth Cohort




Census 2002 data is used to gauge the potential impact (reform intensity) of free primary education. Reform intensity is computed based on the educational performance of individuals aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. We take means over districts per gender and birth cohort. The analysis sample is comprised of the graphed cohorts. 1989 is the first partially treated cohort and 1995 is the first fully treated cohort.

### 3.4.3 Model Specification and Assumptions

The design of the intensity variable leads to a difference-in-differences framework whereby age groups are the units of observation and the year-of-birth cohort, gender and the district of residence inform the intensity of treatment.[9] All age groups are control units in 2002. In 2012, some remain control (ages 24 to 30, born 1982–88), others become partially (ages 18 to 23, born 1989-94) or fully treated (ages 14 to 17, born 1995–98).[10]

Moreover, because the analysis relies on age groups, year-of-birth cohorts and period information, the literature that deals specifically with age-period-cohort (APC) analyses further informs the setup of our empirical model. APC analyses have to overcome a specific identification issue that is due to the fact that any one variable among the age, cohort and period effects can be determined as a linear combination of the remaining two. The age, period and cohort controls are all meant to act as proxies for variables that are relevant to the empirical model but which are not observable. These underlying variables are not themselves linearly dependent (Heckman & Robb, 1985).

Heckman & Robb (1985) mention that the simplest solution is to assume that one of the age, period or cohort effects is zero. This strategy comes at an important cost if the assumption is wrong. However, due to the peculiarities of this study, we argue that this risk is acceptable. Unlike APC studies, we are not concerned with the exact magnitude of the age, cohort or period effects. We do not interpret these effects or base any conclusions on their magnitude. Instead, our interest lies with the impact of the primary school reform. Therefore, we run specifications that confront all possible specifications: *(1) cohort effects are zero, (2) age effects are zero, and (3) year effects are zero.* Namely, Equations 3.2, 3.3 and 3.4, respectively, to which district and gender effects are also added.

$$Education_{a,p,d,s} = \beta_1 Intensity_{a,p,d,s} + \gamma_{1,a} + \tau_{1,p} + \delta_{1,d} + \eta_{1,s} + \epsilon_{1,a,p,d,s} \tag{3.2}$$

$$Education_{c,p,d,s} = \beta_2 Intensity_{c,d,s} + \rho_{2,c} + \tau_{2,p} + \delta_{2,d} + \eta_{2,s} + \epsilon_{2,c,p,d,s} \tag{3.3}$$

$$Education_{a,c,d,s} = \beta_3 Intensity_{c,d,s} + \gamma_{3,a} + \rho_{3,c} + \delta_{3,d} + \eta_{3,s} + \epsilon_{3,a,c,d,s} \tag{3.4}$$

where $Intensity$ is the interaction variable of a typical difference-in-differences framework. $a$ stands for age (14 to 30 year olds), $c$ is for cohort (1972 to 1998), $p$ is for period (2002, 2012), $d$ denotes the district of observation (103 districts), and $s$ is for gender. $Education$ can be any outcome of interest: enrolment rates or average years of primary education.

We expand the above specifications with additional level effects in the form of interactions. Since the age and period variables define the panel, we focus on these and assume cohort effects are not significant. Then, the strategy is to include district-period, age-period and age-district effects progressively for specifications *(4), (5)* and *(6)*, which are summarised in Equation 3.5. The objective is to account for trends and further heterogeneities in the data. The resulting framework is a high-dimensional fixed effects model.

[9] The census dataset does not include a variable documenting the district of birth of individuals. We conduct a sensitivity analysis in this regard. See Appendix C.5.

[10] See Appendix C.3 for an age-cohort-period table.

$$Education_{a,p,d,s}=\beta_4 Intensity_{a,p,d,s}+\gamma_{4,a}+\tau_{4,p}+\delta_{4,d}+\eta_{4,s}+\pi_{4,a,p}+\omega_{4,a,d}+\theta_{4,p,d}+\epsilon_{4,a,p,d,s} \tag{3.5}$$

As the treatment intensity variable is computed at district level, i.e., clusters of units within a district are assigned to a given treatment value, then clustering at the district level is advised (Abadie et al., 2017). Even though treatment is also gender-specific, gender values will be correlated within districts. Moreover, higher levels of aggregation are also preferred for clustering because they are more conservative. Lucas & Mbiti (2012b,a) and Chicoine (2019) use the same level of clustering.

The identification assumption per which cohort effects are zero, as in Equation 3.5, has been commonly employed in the literature. In the case of Krueger & Pischke (1992), the authors argue that since their cohorts of interest are close together, they must be similar, and thus cohort effects are assumed zero. Their study is of a pension reform and its impacts on labour force participation. Similarly, Machin et al. (2011) also explore an age-year panel in their study of the impact of compulsory school reforms on crime. They control for age and year effects, and assume cohort effects are zero.

In addition, we assume all relevant time-varying covariates are captured by the *Period* dummy and the interaction terms $Age \times Period$ and $District \times Period$. Unobserved time-fixed variables are eliminated by the employment of the various layers of fixed effects that capture age, district, age-district and gender-specific characteristics.

The treatment effect is described in Equation 3.6. The second term of this equation, i.e., the difference between the pre- and post-reform outcomes of the control group had it been treated, is the counterfactual that is not observed.

$$\hat{\beta}=E\big(Education^{Treated}_{a,2012,d,s}-Education^{Treated}_{a,2002,d,s}\ \mid\ Intensity_{a,2012,d,s}>0,\gamma,\delta,\eta,\pi,\omega,\theta\big)-$$

$$-E\big(Education^{Control}_{a,2012,d,s}-Education^{Control}_{a,2002,d,s}\ \mid\ Intensity_{a,2012,d,s}>0,\gamma,\delta,\eta,\pi,\omega,\theta\big) \tag{3.6}$$

The difference-in-differences estimation method assumes that the counterfactual is equal to the difference between the pre- and post-reform outcomes of the control group in the absence of treatment. This is the parallel trends assumption, presented in Equation 3.7.

$$E\big(Education^{Control}_{a,2012,d,s}-Education^{Control}_{a,2002,d,s}\ \mid\ Intensity_{a,2012,d,s}>0,\gamma,\delta,\eta,\pi,\omega,\theta\big)=$$

$$=E\big(Education^{Control}_{a,2012,d,s}-Education^{Control}_{a,2002,d,s}\ \mid\ Intensity_{a,2012,d,s}=0,\gamma,\delta,\eta,\pi,\omega,\theta\big) \tag{3.7}$$

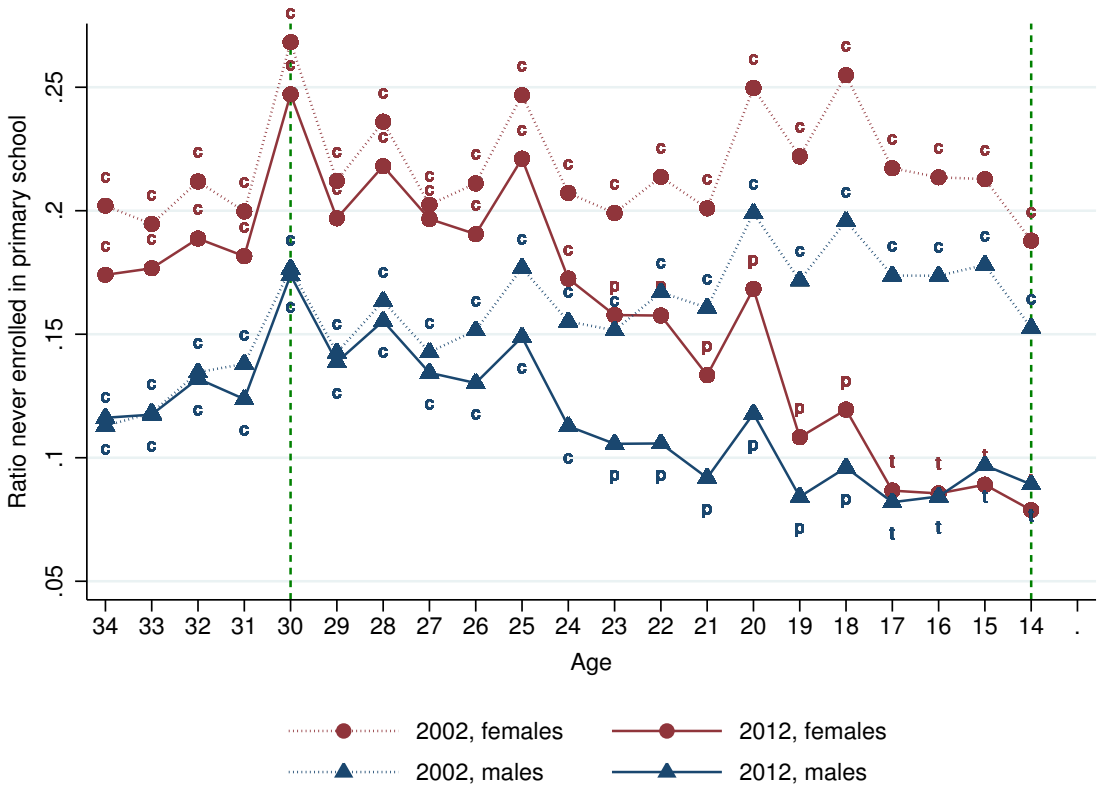
If all identification assumptions are satisfied, then the difference-in-differences estimation method identifies the *average treatment on the treated effect* presented in Equation 3.8.

$$\hat{\beta}_{DiD}=E\big(Education^{Treated}_{a,2012,d,s}-Education^{Treated}_{a,2002,d,s}\ \mid\ Intensity_{a,2012,d,s}>0,\gamma,\delta,\eta,\pi,\omega,\theta\big)-$$

$$-E\big(Education^{Control}_{a,2012,d,s}-Education^{Control}_{a,2002,d,s}\ \mid\ Intensity_{a,2012,d,s}=0,\gamma,\delta,\eta,\pi,\omega,\theta\big) \tag{3.8}$$

Figure 3.4 brings strong evidence in support of the parallel trends assumption. First, it is visible that among the control units, there is no significant difference between the out-of-school gendered ratios of 2002 and 2012, i.e., the dotted and solid lines either overlap or are parallel for each gender if the line marker is *c*. Second, starting with ages 23–24, when cohorts are classified as partially treated in 2012, it becomes apparent that the 2012 outcomes diverge from their 2002 counterparts by following a downward path.

Figure 3.4: Parallel Trends Assumption, Never-Enrolled Population Ratios




The vertical, dotted, green lines show the first and last age cohorts to be included in the analysis sample, 14 and 30 years of age, respectively. *c* is control. *p* and *t* stand for partial and full treatment, respectively.

Moreover, Figure 3.4 brings suggestive evidence of the impact of school fee elimination on the percentage of individuals who have never attended school; solid lines are notably below the dotted lines for each gender starting with the partially treated cohorts. Additionally, the graph also shows the gender imbalance regarding educational outcomes. A higher share of control women have never attended school compared to males; red lines are substantially above the blue lines for control cohorts. The graph suggests, however, that the difference between treated females and males has been eliminated following the reform; solid lines converge for the treated age cohorts.

### 3.4.4 Impact Heterogeneities

To assess the heterogeneity of reform impacts regarding gender and colonial infrastructure, we interact the reform variable with a variable capturing gender and the standardised number of schools per 100,000 children aged 7–13 in 2012. See Equations 3.9 and 3.10.

$$Education_{a,p,d,s} = \beta_5 Intensity_{a,p,d,s} + \alpha_5 Intensity_{a,p,d,s} \times Female +$$
$$\gamma_{5,a} + \tau_{5,p} + \delta_{5,d} + \eta_{5,s} + \pi_{5,a,p} + \omega_{5,a,d} + \theta_{5,p,d} + \epsilon_{5,a,p,d,s} \tag{3.9}$$
$$Education_{a,p,d,s} = \beta_6 Intensity_{a,p,d,s} + \alpha_6 Intensity_{a,p,d,s} \times Female +$$
$$+ \sum_{i=1}^{3} \phi_i Intensity_{a,p,d,s} \times Schools_{i,d} + \sum_{i=1}^{3} \zeta_i Intensity_{a,p,d,s} \times Schools_{i,d} \times Female +$$
$$+ \gamma_{6,a} + \tau_{6,p} + \delta_{6,d} + \eta_{6,s} + \pi_{6,a,p} + \omega_{6,a,d} + \theta_{6,p,d} + \epsilon_{6,a,p,d,s} \tag{3.10}$$

Where $i \in \{1, 2, 3\}$ stands for Catholic, Protestant and village-authority schools.

We do not suspect the variable measuring colonial infrastructure to be endogenous because the fixed effects that we employ take care of the factors that explain the location of colonial schools, which might also be correlated with present-day outcomes. These variables can be altitude, the weather and climate, disease prevalence, access to water and roads at the time of the missionary influx. (Nunn, 2014a; Huillery, 2009; Montgomery, 2017).

## 3.5 Results

### 3.5.1 Impact of School Fee Elimination

Among the fully treated cohorts, reform intensity is 1.7 years on average and the standard deviation is 0.9 years. The minimum value is 0.2 and the maximum is 5.1 years. We find that the implementation of the reform has improved average educational outcomes. See Table 3.4. A one-year increase in reform intensity triggers a reduction of 4 percentage points in the ratio of individuals who never enrol in education and a 0.29 increase in average years of primary education, i.e., 3.5 months. At the average intensity of 1.7 years of free education, the effect is a decrease of 6.8 percentage points in the never-enrolled population and an increase of 6 months in average primary school achievement. These are the results put forward for each block of dependent variables in column (6), which is the most comprehensive and our preferred specification. The other specifications also point to significant coefficients: a 4–5 percentage-point decrease in the ratio of individuals who have never enrolled in school, and a 0.26–0.44 years increase in average primary education for one additional year of free education.

We note, however, that if educational outcomes are only averaged over populations who *have* enrolled in primary education—third block of variables in Table 3.4—, then the aforementioned positive effects are no longer strong or consistent. These results suggest that the reform has mainly improved enrolment rates, which in return have boosted average educational levels. As the percentage of people who have never enrolled in primary education is reduced, the average education of the concerned population will automatically increase, and the magnitude of this increase would have been amplified by the efficacy of the reform in improving the educational outcomes of those enrolled.

Table 3.4: Impacts of School Fee Elimination on Educational Outcomes

| Explanatory variable | RATIO OF NEVER-ENROLLED INDIVIDUALS - All individuals - | | | | | | YEARS OF PRIMARY EDUCATION - All individuals - | | | | | | YEARS OF PRIMARY EDUCATION - Only enrollees - | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (1) | (2) | (3) | (4) | (5) | (6) | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform intensity | -0.05*** | -0.05*** | -0.05*** | -0.04*** | -0.04*** | -0.04*** | 0.44*** | 0.34*** | 0.34*** | 0.41*** | 0.26*** | 0.29*** | 0.17*** | -0.01 | -0.01 | 0.18*** | -0.03*** | 0.07*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.02) | (0.03) | (0.03) | (0.02) | (0.02) | (0.02) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| **Fixed Effects** | | | | | | | | | | | | | | | | | | |
| District | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Gender | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Birth cohort | - | Yes | Yes | - | - | - | - | Yes | Yes | - | - | - | - | Yes | Yes | - | - | - |
| Age | Yes | - | Yes | Yes | Yes | Yes | Yes | - | Yes | Yes | Yes | Yes | Yes | - | Yes | Yes | Yes | Yes |
| Period | Yes | Yes | - | Yes | Yes | Yes | Yes | Yes | - | Yes | Yes | Yes | Yes | Yes | - | Yes | Yes | Yes |
| District × Period | - | - | - | Yes | Yes | Yes | - | - | - | Yes | Yes | Yes | - | - | - | Yes | Yes | Yes |
| Age × Period | - | - | - | - | Yes | Yes | - | - | - | - | Yes | Yes | - | - | - | - | Yes | Yes |
| Age × District | - | - | - | - | - | Yes | - | - | - | - | - | Yes | - | - | - | - | - | Yes |
| Within $R^2$ | 0.19 | 0.13 | 0.13 | 0.17 | 0.09 | 0.07 | 0.26 | 0.08 | 0.09 | 0.26 | 0.06 | 0.05 | 0.17 | 0.00 | 0.00 | 0.18 | 0.00 | 0.02 |
| F | 516 | 263 | 263 | 548 | 294 | 199 | 621 | 165 | 165 | 615 | 169 | 174 | 151 | 2 | 2 | 154 | 12 | 22 |
| Nr. clusters | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 |
| N | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–94 cohorts have been partially treated, and the 1995–97 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. The sample only includes the districts of Mainland Tanzania as of 1988.

We tentatively conclude that fees have been an important obstacle in the achievement of full enrolment; however, fees do not appear to have been the main problem behind dropouts for those who do enrol in primary education. An alternative explanation is that the average educational achievement of those who would have enrolled in school regardless of the reform may have been watered down by the performance of individuals who enrol as a consequence of the removal of fees but for whom the elimination of fees is insufficient to allow them to complete primary education, and thus they drop out. Consequently, the latter group of students cancels any improvement in grade achievement for those who would have enrolled anyway, fees or no fees.

### 3.5.2 Impact Heterogeneity, Gender, Colonial Infrastructure

In Table 3.5, we assess the heterogeneity of the reform by estimating Equations 3.9 and 3.10. There is strong evidence that reform impacts are heterogeneous in terms of gender and the colonial infrastructure of the 1940s. The impact for females was at least double the effect for males. Specification (1) in Table 3.5 shows that while the impact of the reform on the never-enrolled ratio of males was 2 percentage points on average, the magnitude for females stood at 4 percentage points for one additional year of free primary education. As pointed out in Section 3.3, this may reflect the fact that there are more females who have never attended school than there are males. Thus, there is more room for females to improve relative to male cohorts. Similarly, the effect is also carried over when average educational achievement is the dependent variable. For males, the reform triggered a 0.10 increase for one additional year of free education. For females, the effect was 0.17 of a year higher. Finally, Table 3.5 also suggests that the reform has improved the educational achievement of enrolled females. Although the effect is small, it is statistically significant. We test if the sum of the coefficients in column (1) is statistically different from zero. The F-test rejects the null of non-significance with a p-value of 0. In contrast, the result for males is not robust. Overall, the results of specification (1) support the hypothesis that the elimination of fees has reduced the educational gender gap.

Table 3.5 also brings evidence that the reform has further reduced the educational gender gap in favour of females residing in districts that have benefited from stronger investments in missionary or local-authority schools during colonial rule. In contrast, these historical institutions do not appear to suggest any reform heterogeneity for males. For a standard deviation increase in the Catholic school infrastructure, the elimination of school fees further improves the enrolment of females by 1 percentage point. The same applies to the Protestant infrastructure. Similarly, the impact of fee removal on the average number of years of primary education is increased by 0.05 and 0.03 for one standard deviation increase in the scaled number of Catholic and Protestant schools, respectively. Specification (5) shows that for a deviation increase in the number of both Protestant and Catholic schools per 100,000 children, the baseline reform impacts for females, i.e., 5 percentage points higher enrolments and 0.28 better education for one additional year of free education, are further increased to 7 percentage points and 0.37 years of education.

Table 3.5: Heterogeneous Reform Impacts, Gender and Colonial Infrastructure

| **Explanatory variables** | **RATIO OF NEVER-ENROLLED INDIVIDUALS** - All individuals - | | | | | **YEARS OF PRIMARY EDUCATION** - All individuals - | | | | | **YEARS OF PRIMARY EDUCATION** - Only enrollees - | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (1) | (2) | (3) | (4) | (5) | (1) | (2) | (3) | (4) | (5) |
| Reform intensity (years) | -0.02*** | -0.02*** | -0.03*** | -0.02*** | -0.03*** | 0.10** | 0.11*** | 0.10*** | 0.10*** | 0.11*** | 0.02 | 0.03 | 0.02 | 0.02 | 0.03 |
| | (0.01) | (0.00) | (0.01) | (0.00) | (0.00) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| Reform × Female | -0.02*** | -0.02*** | -0.02*** | -0.02*** | -0.02*** | 0.17*** | 0.18*** | 0.18*** | 0.18*** | 0.18*** | 0.04*** | 0.04*** | 0.04*** | 0.04*** | 0.04*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Reform × Catholic | | 0.00 | | | 0.00 | | 0.00 | | | 0.01 | | 0.02 | | | 0.02 |
| | | (0.00) | | | (0.00) | | (0.02) | | | (0.02) | | (0.01) | | | (0.01) |
| Reform × Female× Catholic | | -0.01*** | | | -0.01*** | | 0.05*** | | | 0.05*** | | -0.00 | | | -0.00 |
| | | (0.00) | | | (0.00) | | (0.02) | | | (0.02) | | (0.00) | | | (0.00) |
| Reform × Protestant | | | 0.00 | | 0.00 | | | -0.01 | | -0.01 | | | 0.00 | | -0.00 |
| | | | (0.00) | | (0.00) | | | (0.03) | | (0.03) | | | (0.01) | | (0.01) |
| Reform × Female × Protestant | | | -0.01*** | | -0.01*** | | | 0.04*** | | 0.03** | | | 0.00 | | 0.00 |
| | | | (0.00) | | (0.00) | | | (0.01) | | (0.01) | | | (0.00) | | (0.00) |
| Reform × Village authority | | | | -0.00 | -0.00 | | | | 0.01 | 0.01 | | | | -0.01 | -0.01 |
| | | | | (0.00) | (0.00) | | | | (0.01) | (0.01) | | | | (0.01) | (0.01) |
| Reform × Female × Village | | | | -0.00 | -0.00 | | | | 0.02 | 0.01 | | | | 0.01*** | 0.01*** |
| | | | | (0.00) | (0.00) | | | | (0.02) | (0.01) | | | | (0.00) | (0.00) |
| Adjusted within $R^2$ | 0.09 | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 |
| F | 258 | 134 | 118 | 147 | 69 | 233 | 136 | 113 | 132 | 68 | 59 | 36 | 31 | 33 | 19 |
| Nr. clusters | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 |
| N | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. All regressions are based on the preferred specification, which includes the following fixed effects: District, Gender, Age, Period, District × Period, Age × Period, Age × District. The Catholic, Protestant and village-authority variables are standardised values of the normalised number of schools per 100,000 children aged 7–13 in 2012; $\mu = 1$ and $\sigma = 1$. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–94 cohorts have been partially treated, and the 1995–97 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (born 1970–83). The sample includes the districts of Mainland Tanzania as of 1988.

If average education is computed among enrollees only, then there is no heterogeneity in terms of the missionary infrastructure, but there is some heterogeneity regarding village-authority schools, as they favour larger reform impacts for females.

These findings agree with the historical facts presented in Section 3.2, per which the missionary infrastructure was mainly interested in increasing the number of religious converts and less concerned about the quality of the education they offered. Consequently, their infrastructure was larger relative to government schools, and their teaching less rigorous. It appears that this legacy has persisted to this day, as districts with historically larger missionary investments perform better in terms of the female-male enrolment gaps—presumably because they have more schools and thus are better able to relax the time constraints that limit the educational opportunities of girls.

Table 3.6: Educational Gender Gap and Colonial Infrastrucutre

| | **RATIO NEVER ENROLLED** | **AVERAGE EDUCATION** - All individuals - | **AVERAGE EDUCATION** - Only enrollees - |
|---|---|---|---|
| | (1) | (2) | (3) |
| Female | 0.05*** | -0.23*** | 0.09*** |
| | (0.00) | (0.02) | (0.01) |
| Female × Catholic | -0.01*** | 0.04** | -0.00 |
| | (0.00) | (0.01) | (0.00) |
| Female × Protestant | -0.01*** | 0.07*** | 0.01** |
| | (0.00) | (0.02) | (0.00) |
| Female × Village authority | -0.00 | 0.02 | 0.01*** |
| | (0.00) | (0.02) | (0.00) |
| Adjusted within $R^2$ | 0.35 | 0.20 | 0.18 |
| F | 46 | 24 | 138 |
| Nr. clusters | 103 | 103 | 103 |
| N | 7,004 | 7,004 | 7,004 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts. All regressions include the following fixed effects: District, Gender, Age, Period, District × Period, Age × Period, Age × District. The Catholic, Protestant and village authority variables are standardised values of the normalised number of schools per 100,000 children aged 7–13 in 2012; $\mu = 1$ and $\sigma = 1$.

Columns (1) and (2) of Table 3.6 show that, on average, there is a 5 percent gender gap in enrolment and a 0.23 gap in primary educational achievement, i.e., 3 months, with females lagging behind. However, once enrolled, females accumulate more education. The interaction terms bring additional information. We learn that the gap is smaller in districts where early investments in education were larger. For instance, the enrolment gap is reduced to 3 percent in districts endowed with an additional standard deviation in the density of Catholic and Protestant schools. Similarly, instead of a 3-month gap, in these districts, the gap would be of 1.4 months. Furthermore, column (3) of Table 3.6 shows that among the enrolled, females stay in school one month longer than males on average. Their advantage is further increased in areas with colonial-time Protestant and government investments. This heterogeneity is similar to that presented in Table

3.5. In addition, column (3) also vindicates the findings of Nunn (2014a) and Becker & Woessmann (2008), who argue that Protestant missions were more preoccupied to promote literate girls relative to Catholic schools.

Tables 3.5 and 3.6 suggest that gender gaps were already smaller in districts with larger colonial infrastructure, and the reform has also been more effective at reducing the educational gender gap in these districts. The reform continues to add to the advantages created by early investments in education and perpetuates historical legacies. All groups have received a boost, especially females. However, the females that were already better off have benefited more than the females impacted by poor historical investments in education.

### 3.5.3 Mechanism

Inspired by the work of Huillery (2009), we investigate whether the persistence of investments in infrastructure can explain why current reforms perpetuate the legacies of the past. We suspect that districts where investments in education took place relatively early continue to invest more than other districts.

Table 3.7: Educational Gender Gap and Current Infrastrucutre

| | **RATIO NEVER ENROLLED** | **AVERAGE EDUCATION** - All individuals - | **AVERAGE EDUCATION** - Only enrollees - |
|---|---|---|---|
| | (1) | (2) | (3) |
| Female | 0.05*** | -0.23*** | 0.09*** |
| | (0.00) | (0.02) | (0.01) |
| Female × Current infrastructure | -0.01*** | 0.08*** | 0.01* |
| | (0.00) | (0.02) | (0.01) |
| Adjusted within $R^2$ | 0.35 | 0.20 | 0.18 |
| F | 89 | 48 | 124 |
| Nr. clusters | 103 | 103 | 103 |
| N | 7,004 | 7,004 | 7,004 |

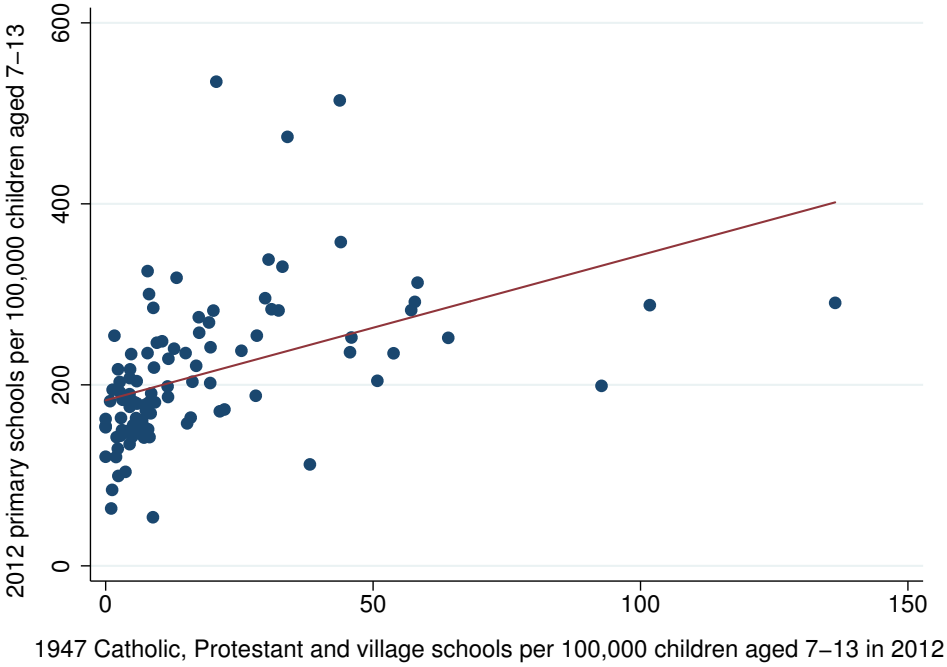
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts. All regressions includes the following fixed effects: District, Gender, Age, Period, District × Period, Age × Period, Age × District. Current infrastructure consists of the 2012 number of school per district scaled to the 2012 population of children aged 7–13 and standardised such that $\mu = 1$ and $\sigma = 1$.

Against the background of the initiative "*Big Results Now in Education*", the government has committed to sharing information about the performance of schools to promote accountability (Cilliers et al., 2020). We use the output of this initiative, i.e., a publicly available list of the universe of schools, to investigate the persistence of investments in schools. This list includes the GPS coordinates of schools and performance indicators.

Table 3.7 shows that a larger school infrastructure nowadays is also associated with a smaller gender gap. A more readily available infrastructure is arguably capable of relaxing binding household constraints, which generally keep girls away from school. Distance

to school is a serious deterrent of school attendance in developing countries in general (Muralidharan & Prakash, 2017) and in Tanzania, especially for girls and for children in rural areas (Al-Samarrai & Reilly, 2000; Kondylis & Manacorda, 2012). Moreover, the same objective constraint regarding school availability may matter more for Tanzanian girls than for boys (Lihwa et al., 2019), as the demands for female and male time are different and thus, so are their opportunity costs. Mason & Khandker (1996) argue that the time opportunity cost for girls of primary school age in Tanzania is larger than for boys. Consequently, the remoteness of schools is more costly for girls than it is for boys.

Figure 3.5: Persistence of Investments in Education




The 1947 schools are added together, i.e., Catholic, Protestant and village-authority schools. Both axes are scaled to the population of children aged 7–13 in 2012.

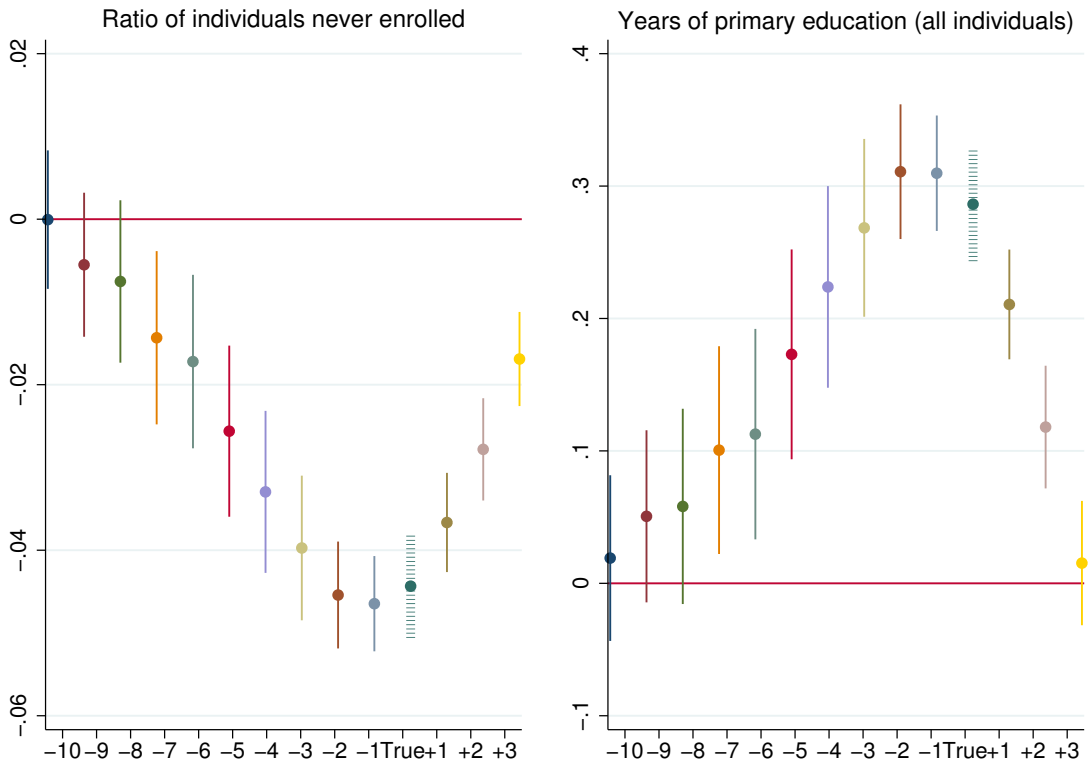
Figure 3.5 brings suggestive evidence that past and current infrastructure are related. The 1947 school infrastructure per 100,000 students aged 7–13 is highly correlated with the infrastructure in 2012. The correlation coefficient is 0.45. This evidence does not reflect a causal relationship, and the reader should treat the information accordingly. Table 3.7 and Figure 3.5 bring suggestive evidence that the mechanism which has enabled the elimination of school fees to disproportionately reach girls in districts with a stronger history of educational investments is the inherited proclivity of these districts to invest more in their educational infrastructure. Consequently, districts with above-average school densities are readier to absorb the increased demand for education in the aftermath of reforms. If schools are not sufficiently available, then the impacts of school fee elimination may be dampened by binding constraints such as limited infrastructure.

## 3.6 Robustness Checks

### 3.6.1 Falsification Test: Timing of the Reform

To scrutinise the results of Table 3.4, we falsify the treatment variable by intentionally misplacing the timing of the reform. This is the same approach adopted by Chicoine (2019). The true timing is 2002, however, we run iterations whereby the reform is assumed to have been implemented in each of the years between 1992 and 2005.[11]

Figure 3.6: Falsification Analysis




Point estimates are accompanied by 95% confidence intervals. The reform impact coefficient is plotted for 14 regressions. Control and treatment cohorts are pushed forward or backward each time by 1 year.

The results of the falsified regressions are plotted in Figure 3.6. All regressions are based on the preferred specification, which includes the following fixed effects: District, Gender, Age, Period, District × Period, Age × Period, Age × District. This figure shows how both the magnitude and significance of the estimated impact are strengthened the closer the falsified timing of the reform is to 2002, the true implementation year. The fact that the magnitude is slightly bigger in $T-1$ may be due to the fact that the elimination of fees was first announced in 2001 and then implemented in 2002. Arguably, this has allowed parents to send their children to school earlier, in an attempt to avoid enrolment refusals once educational facilities became overcrowded in the aftermath of the reform. Moreover, late enrolment is a chronic problem in Tanzania (Mason & Khandker, 1996). We may be slightly underestimating the impact of the reform because we assume that all children enrol at seven years of age. If a large number of students start school at age

[11] Appendix C.3 shows which cohorts are considered for each falsification test.

eight, then this can show up in the data as a stronger reform impact at $T-1$. Overall, we are confident that the falsification test is supportive of our analysis.

### 3.6.2 Reform Measurement

The benchmark reform intensity is gauged based on the educational performance of all individuals born between 1970 and 1983, aged 19–32 at the time of the reform in 2002.[12] To explore the robustness of our findings, we use two different reform variables. Instead of focusing on all 14 cohorts between 1970 and 1983, we take the 7 most recent cohorts, 1977–83, and then the 7 oldest cohorts, 1970–76.[13] Appendix C.4 shows that the benchmark results are robust to these alternative specifications of reform intensity. The magnitude of coefficients and the qualitative implications of Table 3.4 are maintained.

### 3.6.3 Analysis Sample

In the benchmark analysis, we have relied on an individual's district of residence, as opposed to district of birth, to compute the district-aggregated educational outcomes. Therefore, the dependent variable is likely to suffer from measurement error. Importantly, this measurement error may be non-random if there is a tendency for some districts to attract individuals that are better-educated. Unfortunately, we do not have data on district of birth. To mitigate some of the measurement error, we exclude from the computation of the aggregated outcomes of interest those individuals for whom the region of residence does not match their region of birth. Admittedly, this solution is inferior to the one whereby district mismatches are removed as district data is more granular.[14] Nevertheless, districts within regions are similar and share commonalities. Thus, we argue this correction meets its intended purpose. Consequently, we remove 18 percent of the individual-level sample, as these individuals have moved regions since birth. The aggregated variables are then re-computed. The new regression results support our previous findings. See Appendix C.5. Differences in magnitude between Tables 3.5 and Appendix C.5 are negligible, although we do gain in precision for some of the estimates if the variable of interest is the education of enrollees. The impact of the reform on males is occasionally significant and more so in districts with historically larger Catholic missionary presence.

[12] We start at age 19 because we want to avoid any issues created by late enrolments and their ensuing late graduation. This could be a problem because the scope of the reform intensity variable is to describe pre-reform performance. If cohorts include individuals who might have been impacted by the reform, then this would affect our identification strategy. We do not go beyond the 1970 cohort because, against the background of rapid and important changes in Tanzania post-independence, older cohorts are too detached from Tanzania's educational situation after 1980.

[13] Appendix C.1 compares these three cohort-based measurements, 1970–83, 1970–76 and 1977–83. The intuition is that pre-reform educational achievement and reform intensity are inversely related. While the educational situation of females has not changed significantly across the 1970–83 cohorts, for males, however, the reform intensity variable suggests a marked improvement. For instance, the variable is lower if the 1977–83 cohorts are considered, which is a consequence of these cohorts' superior educational results relative to their older counterparts. Similarly, the variable reaches its highest magnitude if the 1970–76 cohorts are employed, as they had more room for improvement, and reform intensity is thus estimated to be stronger. Benchmark intensity sits midway between the aforementioned specifications.

[14] There are 103 districts in the sample but only 18 regions.

### 3.6.4 Falsification Test: Railway Infrastructure

Finally, we check the robustness of the results presented in Section 3.5.2 by falsifying the type of colonial infrastructure that we consider. Instead of the educational infrastructure of 1947, we explore the 1950 railway infrastructure of Tanganyika. This type of falsification test has also been employed by Nunn (2014a). For this purpose, we have georeferenced a map compiled by the Department of Lands and Surveys of Tanganyika around the same time when the school infrastructure map was also compiled.[15] See Appendix C.6.

Table 3.8: Falsification Analysis: Railway Infrastructure

| | **RATIO NEVER ENROLLED** | | **AVERAGE EDUCATION** - All individuals - | | **AVERAGE EDUCATION** - Only enrollees - | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (1) | (2) | (1) | (2) |
| Reform intensity (years) | -0.02*** | -0.03*** | 0.10** | 0.12*** | 0.02 | 0.03 |
| | (0.01) | (0.01) | (0.05) | (0.04) | (0.02) | (0.02) |
| Reform × Female | -0.02*** | -0.02*** | 0.17*** | 0.16*** | 0.04*** | 0.04*** |
| | (0.00) | (0.00) | (0.03) | (0.02) | (0.01) | (0.01) |
| Reform × Railway goes through district | -0.00 | | -0.00 | | -0.01 | |
| | (0.01) | | (0.04) | | (0.02) | |
| Reform × Female × Railway goes through district | -0.00 | | 0.01 | | 0.01 | |
| | (0.01) | | (0.04) | | (0.01) | |
| Reform × Railway is close | | 0.01 | | -0.05 | | -0.02 |
| | | (0.01) | | (0.04) | | (0.02) |
| Reform × Female × Railway is close | | -0.01 | | 0.05 | | 0.01 |
| | | (0.01) | | (0.04) | | (0.01) |
| Adjusted within $R^2$ | 0.09 | 0.09 | 0.10 | 0.10 | 0.03 | 0.03 |
| F | 137 | 127 | 121 | 117 | 30 | 31 |
| Nr. clusters | 103 | 103 | 103 | 103 | 103 | 103 |
| N | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. All regressions include the following fixed effects: District, Gender, Age, Period, District × Period, Age × Period, Age × District. The railway variables are dummies that take value 1 if the railway line intersects with the district polygon—regardless whether the intersection is notable or slight—and if the centroid of the district is within 50 km from the railway line, respectively.

Nunn (2014a) uses a dummy variable to denote whether a village or ethnic group was accessed via a railway in the early 1900s. We employ the same idea and use two dummy variables to establish whether a railway crossed the territory of any one district and whether the district centroid is within 50 kilometres from the closest railway. Table 3.8 shows that the school fee reform does not exhibit any heterogeneity regarding the railway infrastructure. This is reassuring and suggests that our main results are not spurious due to unobservable factors which made certain districts more attractive to colonial settlers. Our analysis thus survives the falsification test.

[15] The map is hosted by the Princeton University Library, https://catalog.princeton.edu/catalog/10159264.

## 3.7 Conclusion

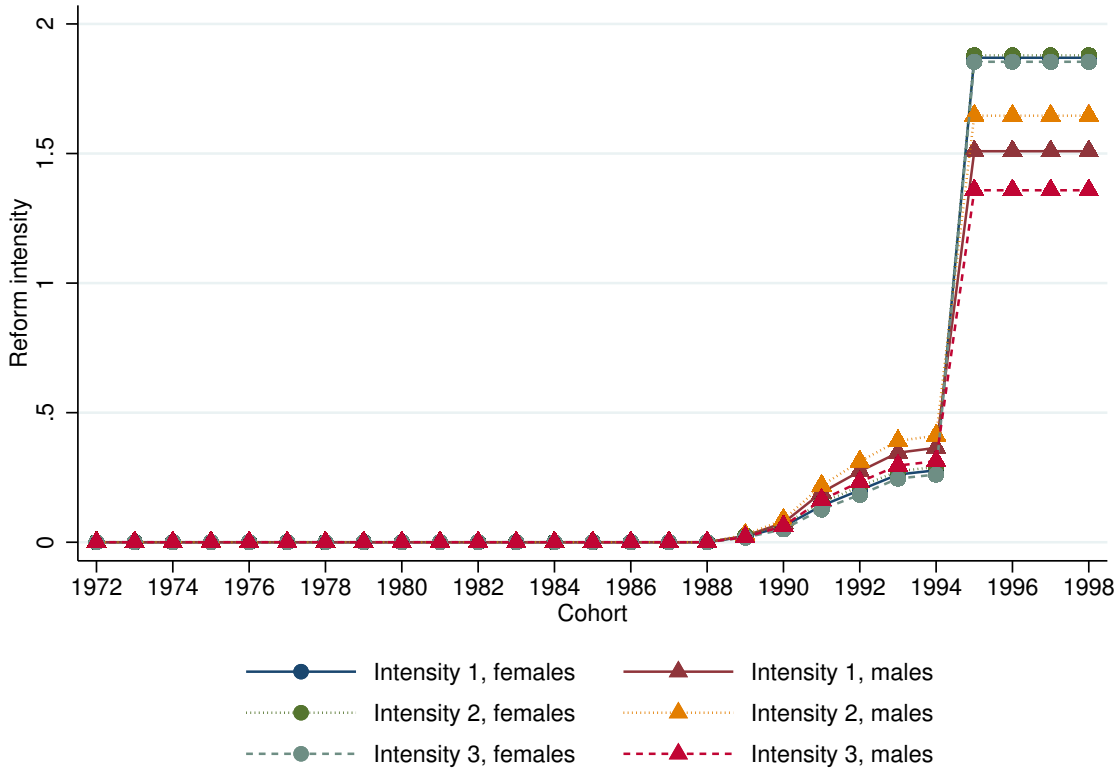
In this study, we have compiled evidence that the elimination of primary school fees in Mainland Tanzania has improved educational outcomes on average. The fraction of individuals who have never attended school has decreased by 4 percentage points and average education has increased by 3.5 months as a result of being exposed to one additional year of free primary education. Female students, who were falling behind their male counterparts in terms of enrolment before the reform, have benefited from the elimination of school fees twice as much compared to male students. This finding is in agreement with evidence from the Ugandan fee elimination (Deininger, 2003; Grogan, 2009) but stands in contrast to the Kenyan reform (Lucas & Mbiti, 2012b). The latter reports a widening of the gender gap.

We estimate that the greatest beneficiaries of the reform have been females who reside in districts where investments in education have been relatively stronger during colonial rule. This means that females who have been disadvantaged by historically poor investments in their districts have continued to benefit less from educational reforms relative to females residing in districts with a stronger legacy of colonial schools. While we find that Protestant schools have had a stronger impact on the education of females compared to Catholic schools, we fail to reject that Catholic schools have also supported female education. Consequently, our study agrees with the findings of Nunn (2014a) and Becker & Woessmann (2008) regarding the role of Protestant schools; however, we differ from Nunn (2014a) as we argue that Catholic schools, too, have been a noteworthy vehicle in reducing gender gaps in enrolment. Lastly, our study complements the work of Montgomery (2017). The author argues that the school infrastructure of German East Africa is associated with a larger educational gender gap in present-day Tanzania. In contrast, this study brings evidence that the school infrastructure erected during the British colonial rule of Tanganyika has had the opposite effect, one whereby it has facilitated a smaller gender gap in education.

The literature has documented strong evidence that the impacts of historical events and institutions are long-lasting. We conclude that current reforms may also be perpetuating colonial legacies instead of eliminating historical disparities. Policy-makers should engage in concerted efforts to identify and address such patterns. Reforms should address historical legacies to allow the convergence of outcomes across areas with differing degrees of early investments in education. For instance, infrastructure expansions, which usually accompany the removal of school fees, should give disproportionate attention to districts that have been historically disadvantaged as a consequence of the unequal allocation of resources. Otherwise, disparities will persist, although average improvements can be registered.

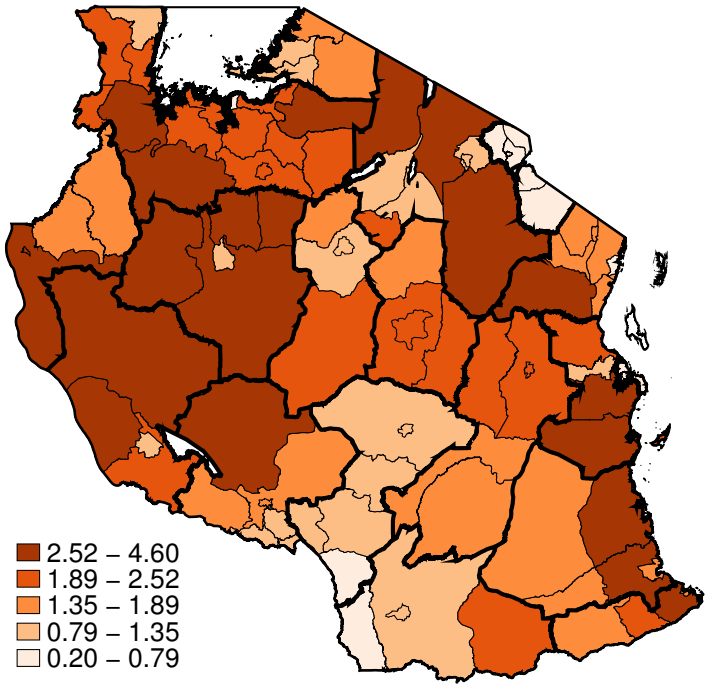
# C Appendix

## C.1 Reform Intensity Variations




Reform intensity is computed based on the educational performance of: (**Intensity 1**) individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83); (**Intensity 2**) individuals who were aged 19–25 at the time of the reform (7 birth-year cohorts, 1977–83); and (**Intensity 3**) individuals who were aged 26–32 at the time of the reform (7 birth-year cohorts, 1970-76). Reform intensity is district-gender-birth-cohort specific. The graph takes means over districts per gender and birth cohort. 1989 is the first partially treated cohort and 1995 is the first fully treated cohort.

## C.2 District and Regional Distribution of Reform Intensity




The reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (14 birth-year cohorts, 1970–83). Reform intensity is district-gender-birth-cohort specific. Only the intensity for the fully treated cohorts is mapped. We take averages over genders. 103 Mainland districts (thin contours) and 18 regions (thick contours).

## C.3 Age-Period-Cohort Table

| | AGE OR BIRTH COHORT | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Age in 2002** | **11** | **12** | **13** | **14** | **15** | **16** | **17** | **18** | **19** | **20** | **21** | **22** | **23** | **24** | **25** | **26** | **27** | **28** | **29** | **30** | **31** | **32** | **33** | **34** | **35** | **36** | **37** | **38** | **39** | **40** |
| *Birth cohort (year)* | 91 | 90 | 89 | 88 | 87 | 86 | 85 | 84 | 83 | 82 | 81 | 80 | 79 | 78 | 77 | 76 | 75 | 74 | 73 | 72 | 71 | 70 | 69 | 68 | 67 | 66 | 65 | 64 | 63 | 62 |
| True treatment status | p | p | p | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c |
| **Age in 2012** | **11** | **12** | **13** | **14** | **15** | **16** | **17** | **18** | **19** | **20** | **21** | **22** | **23** | **24** | **25** | **26** | **27** | **28** | **29** | **30** | **31** | **32** | **33** | **34** | **35** | **36** | **37** | **38** | **39** | **40** |
| *Birth cohort (year)* | 01 | 00 | 99 | 98 | 97 | 96 | 95 | 94 | 93 | 92 | 91 | 90 | 89 | 88 | 87 | 86 | 85 | 84 | 83 | 82 | 81 | 80 | 79 | 78 | 77 | 76 | 75 | 74 | 73 | 72 |
| True treatment status | t | t | t | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c |
| False, +3, 2002 status | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | | | | | | | | | | |
| False, +3, 2012 status | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | | | | | | | | | | |
| False, +2, 2002 status | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | | | | | | | | | |
| False, +2, 2012 status | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | | | | | | | | | |
| False, +1, 2002 status | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | | | | | | | | |
| False, +1, 2012 status | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | | | | | | | | |
| False, -1, 2002 status | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | | | | | | |
| False, -1, 2012 status | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | | | | | | |
| False, -2, 2002 status | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | | | | | |
| False, -2, 2012 status | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | | | | | |
| False, -3, 2002 status | | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | | | | |
| False, -3, 2012 status | | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | | | | |
| False, -4, 2002 status | | | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | | | |
| False, -4, 2012 status | | | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | | | |
| False, -5, 2002 status | | | | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | | |
| False, -5, 2012 status | | | | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | | |
| False, -6, 2002 status | | | | | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | | |
| False, -6, 2012 status | | | | | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | | |
| False, -7, 2002 status | | | | | | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | | |
| False, -7, 2012 status | | | | | | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | | |
| False, -8, 2002 status | | | | | | | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | | |
| False, -8, 2012 status | | | | | | | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | | |
| False, -9, 2002 status | | | | | | | | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | |
| False, -9, 2012 status | | | | | | | | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c | |
| False, -10, 2002 status | | | | | | | | | | | | | | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c | c |
| False, -10, 2012 status | | | | | | | | | | | | | | t | t | t | t | p | p | p | p | p | p | c | c | c | c | c | c | c |

Where $t$ denotes the treated, $p$ is for partial treatment and $c$ is for control.

## C.4 Robustness to the Measurement of Reform Intensity

| | REFORM INTENSITY† | | | REFORM INTENSITY‡ | | |
|---|---|---|---|---|---|---|
| | **Ratio Never Enrolled** | **Average Education** - All - | **Average Education** - Enrollees - | **Ratio Never Enrolled** | **Average Education** - All - | **Average Education** - Enrollees - |
| | (1) | (2) | (3) | (1) | (2) | (3) |
| Reform intensity (years) | -0.02*** | 0.11*** | 0.03* | -0.03*** | 0.11** | 0.02 |
| | (0.00) | (0.03) | (0.02) | (0.01) | (0.04) | (0.02) |
| Reform × Female | -0.02*** | 0.19*** | 0.04*** | -0.02*** | 0.18*** | 0.04*** |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.02) | (0.01) |
| Reform × Catholic | 0.00 | 0.00 | 0.02* | 0.00 | 0.01 | 0.02 |
| | (0.00) | (0.02) | (0.01) | (0.00) | (0.02) | (0.01) |
| Reform × Female× Catholic | -0.01*** | 0.04*** | -0.00 | -0.01*** | 0.05*** | -0.00 |
| | (0.00) | (0.02) | (0.00) | (0.00) | (0.02) | (0.01) |
| Reform × Protestant | 0.00 | -0.01 | -0.00 | 0.00 | -0.01 | -0.00 |
| | (0.00) | (0.03) | (0.01) | (0.01) | (0.03) | (0.01) |
| Reform × Female × Protestant | -0.0048*** | 0.03*** | 0.00 | -0.01** | 0.04** | 0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.02) | (0.00) |
| Reform × Village authority | -0.00 | 0.00 | -0.01 | -0.00 | 0.01 | -0.01 |
| | (0.00) | (0.01) | (0.01) | (0.00) | (0.02) | (0.01) |
| Reform × Female × Village | -0.00 | 0.01 | 0.01** | -0.00 | 0.01 | 0.01** |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Adjusted within $R^2$ | 0.10 | 0.10 | 0.03 | 0.10 | 0.10 | 0.03 |
| F | 74 | 68 | 18 | 59 | 65 | 19 |
| Nr. clusters | 103 | 103 | 103 | 103 | 103 | 103 |
| N | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. All regressions are based on the preferred specification, which includes the following fixed effects: District, Gender, Age, Period, District × Period, Age × Period, Age × District. The Catholic, Protestant and village-authority variables are standardised values of the normalised number of schools per 100,000 children aged 7–13 in 2012; $\mu = 1$ and $\sigma = 1$. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–94 cohorts have been partially treated, and the 1995–97 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity.

† The intensity variable is computed based on the educational performance of individuals who were aged 19–25 at the time of the reform (7 birth-year cohorts, 1977–83).

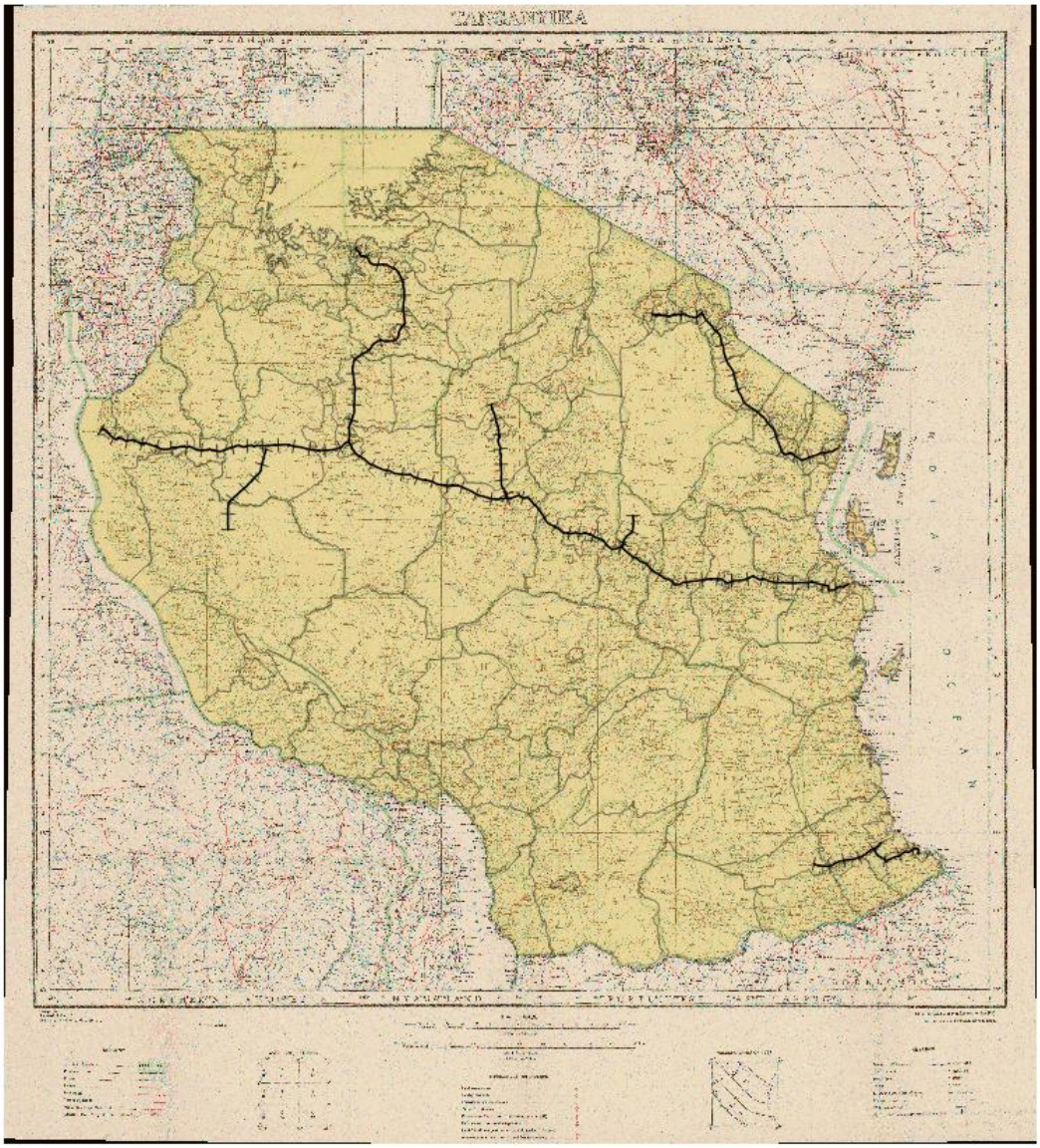
‡ The intensity variable is computed based on the educational performance of individuals who were aged 26–32 at the time of the reform (7 birth-year cohorts, 1970–76). Reform intensity is district-gender-birth-cohort specific. The sample only includes the districts of Mainland Tanzania as of 1988.

## C.5 Robustness to the Removal of Individuals as Region of Residence is Different from Region of Birth

| Explanatory variables | RATIO OF NEVER-ENROLLED INDIVIDUALS - All individuals - | | | | | YEARS OF PRIMARY EDUCATION - All individuals - | | | | | YEARS OF PRIMARY EDUCATION - Only enrollees - | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (1) | (2) | (3) | (4) | (5) | (1) | (2) | (3) | (4) | (5) |
| Reform intensity (years) | -0.03*** | -0.03*** | -0.03*** | -0.03*** | -0.03*** | 0.12*** | 0.13*** | 0.13*** | 0.13*** | 0.14*** | 0.03 | 0.03* | 0.03 | 0.03 | 0.04* |
| | (0.01) | (0.00) | (0.01) | (0.01) | (0.01) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| Reform × Female | -0.02*** | -0.02*** | -0.02*** | -0.02*** | -0.02*** | 0.17*** | 0.18*** | 0.17*** | 0.17*** | 0.18*** | 0.04*** | 0.04*** | 0.04*** | 0.04*** | 0.04*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Reform × Catholic | | 0.00 | | | 0.00 | | 0.00 | | | 0.01 | | 0.02 | | | 0.02* |
| | | (0.00) | | | (0.00) | | (0.02) | | | (0.02) | | (0.01) | | | (0.01) |
| Reform × Female× Catholic | | -0.01*** | | | -0.01*** | | 0.06*** | | | 0.05*** | | -0.00 | | | -0.00 |
| | | (0.00) | | | (0.00) | | (0.02) | | | (0.02) | | (0.00) | | | (0.00) |
| Reform × Protestant | | | 0.00 | | 0.00 | | | -0.01 | | -0.01 | | | 0.00 | | 0.00 |
| | | | (0.00) | | (0.00) | | | (0.03) | | (0.03) | | | (0.01) | | (0.01) |
| Reform × Female × Protestant | | | -0.01*** | | -0.01*** | | | 0.04*** | | 0.04*** | | | 0.00 | | 0.00 |
| | | | (0.00) | | (0.00) | | | (0.02) | | (0.01) | | | (0.00) | | (0.00) |
| Reform × Village authority | | | | -0.00 | -0.00 | | | | 0.00 | 0.00 | | | | -0.01 | -0.01 |
| | | | | (0.00) | (0.00) | | | | (0.01) | (0.01) | | | | (0.01) | (0.01) |
| Reform × Female × Village | | | | -0.00 | -0.00 | | | | 0.02 | 0.02 | | | | 0.01** | 0.01** |
| | | | | (0.00) | (0.00) | | | | (0.02) | (0.01) | | | | (0.00) | (0.00) |
| Adjusted within $R^2$ | 0.09 | 0.10 | 0.09 | 0.09 | 0.10 | 0.09 | 0.09 | 0.09 | 0.09 | 0.10 | 0.03 | 0.03 | 0.02 | 0.03 | 0.03 |
| F | 259 | 135 | 120 | 152 | 71 | 223 | 134 | 109 | 131 | 70 | 49 | 29 | 25 | 26 | 15 |
| Nr. clusters | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 |
| N | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 | 7,004 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors are clustered at the level of districts and are presented in parenthesis. Dependent variables have been computed based on the sample of individuals who have *not* changed their region of residence since birth. All regressions are based on the preferred specification, which includes the following fixed effects: District, Gender, Age, Period, District × Period, Age × Period, Age × District. The Catholic, Protestant and village-authority variables are standardised values of the normalised number of schools per 100,000 children aged 7–13 in 2012; $\mu = 1$ and $\sigma = 1$. All regressions include the 14–30 age groups of 2002 and 2012. In terms of birth cohorts, this means: 1972–98. The 1989–94 cohorts have been partially treated, and the 1995–97 cohorts have been fully treated. Census 2002 data is used to gauge reform intensity. Reform intensity is computed based on the educational performance of individuals who were aged 19–32 at the time of the reform (born 1970–83). The sample includes the districts of Mainland Tanzania as of 1988.

## C.6 Railway Infrastructure in Tanganyika Cca. 1950




*Source*: Tanganyika. Department of Lands and Surveys. Hosted by the Princeton University Library and available online at https://catalog.princeton.edu/catalog/10159264.

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# Conclusion

In this dissertation I have evaluated the magnitude and heterogeneity of impacts for three pro-poor policies and programmes implemented in Southern and Eastern Africa. I have shown that housing subsidies in South Africa mitigate inequality in terms of access to adequate housing and reduce the incidence of violent crimes. However, they do not seem to impact property-related crimes. Moreover, my evaluation of school feeding programmes in Malawi has argued that this intervention bears a significant impact on educational outcomes if it also intervenes to relax a binding food constraint. In addition, school meals have also been markedly more effective at attracting children into school for the first time than in keeping them in school. Finally, I have compiled evidence that the elimination of school fees in Tanzania has followed a similar pattern, whereby the policy has improved enrolment but left retention rates largely unaffected. In the latter case study, I have documented strong gender heterogeneities. Females have benefited disproportionately, and those residing in districts with a history of relatively larger investments in education have been the greatest beneficiaries.

The insights of Chapters 1–3 can be summarised in terms of the following policy recommendations and observations. I show pro-poor policies are indeed capable of achieving their intended goals; they are an effective tool to reduce inequalities regarding access to public services, such as basic amenities and education, between the rich and the poor, males and females. However, policy-makers must be aware of potential impact heterogeneities so as to increase the effectiveness of interventions and know when, where and for whom these interventions work best. Complementary measures may be required to address the limitations imposed by the implementation design of policy tools, historical legacies or environmental factors, to cite only a few instances. Policy-makers must be careful about unintended impacts too. These can be positive. For instance, housing subsidies being able to mitigate crime. Nevertheless, they can also be negative, such as the incentive created by school meals to enrol underage children in primary education, or the tendency of reforms to perpetuate historical legacies instead of eliminating the disparities that history may have created. Solutions exist. These can take the form of pre-school facilities or targeted infrastructure developments.

To date, research on African countries has been limited for several reasons. At the very least, data is becoming more readily available, though it continues to be unequally distributed across countries. I trust, nevertheless, that increasing attention will shift toward the continent. This attention is greatly deserved. In my field, data permitting, more

interest will be placed on the quality side of education. Policy-makers know how to attract children into school, but improvements in grade progression have been slow and students are hindered in their acquisition of the required skills. Education is crucial for socio-economic mobility, and access to educational opportunities should be equally distributed—both from a quantitative and qualitative point of view. In this regard, rural areas are disadvantaged relative to urban centres. However, urban centres are not in a universally better position. They have their own set of challenges to overcome. Unplanned, rapid urbanization in Africa is undoubtedly creating a number of problems. As the housing infrastructure grows insufficient, informality takes root, the transportation system is overwhelmed, strain grows, social disorganization ensues, and crime can follow. On account of the continent's structural transformation, an increasing amount of research will be dedicated to addressing the challenges of rapid urbanisation in sub-Saharan Africa.

Against this background, understanding historical legacies is of the essence. Moreover, culture, traditions and customs have been an undeniable factor in explaining policy outcomes. The old and the new, traditional as well as modern elements of society coexist seamlessly, and in so doing, they create unique environments that require special attention. These environments can explain some heterogeneities or unintended policy outcomes. Mastering such knowledge, along with the application of economic principles, can enable targeted policy tools that are better able to address development issues.