

# Institutions and Immutable Causes of Human Capital

by

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# Abstract

My doctoral thesis examines the broad question of whether appropriately designed institutions and policies can address the short- and long-term consequences of determinants of human capital which are “immutable” by nature or are perceived as such. I consider three different types of immutable determinants of human capital: male versus female biology; colonization; and traditional norms of gender roles.

In Chapter 1, I examine whether, and how, change in political regime type affects excess male infant mortality. Analyzing data on more than 3 million live births from sub-Saharan African countries, I exploit within-mother variation in political regime type to find that excess male infant mortality significantly decreases following a transition to democracy. I identify competitiveness of executive recruitment, constraints on the chief executive, and political participation as the features of democracy that matter most. Examining causal mechanisms, I find that democracy fosters the provision of health inputs, including maternal education, tetanus immunization, breastfeeding, and normal birth weight, all of which have stronger health benefits for boys than for girls, despite being found to be *ex-ante* “gender-neutral” in my setting.

In Chapter 2, I examine how colonial reproductive laws interact with market incentives to shape long-term fertility behavior in Africa. Exploiting the arbitrary division of ancestral ethnic homelands and the resulting discontinuity in institutions across the British-French colonial borders, I find that women in former British areas are more likely to delay sexual debut and marriage, and have fewer children. However, these effects disappear in areas close to sea, where market access and the opportunity cost of childbearing appear to be high irrespective of the colonizer identity. This heterogeneous impact of colonial origins extends to measures of local economic development and household welfare. Examining causal mechanisms, I argue that the fertility effect of colonial origins is

directly linked to colonial population policies and reproductive laws and their impact on the use of modern methods of birth control. I find little evidence that the fertility effect of British colonization operates through education or income. While British colonization is linked to higher female education levels, this occurs mainly close to the sea while the fertility effects do not. Again, while income levels differ, the fertility gap between British and French colonies opened prior to 1980, while the income gap opened-up after 1990. This chapter highlights the heterogeneous nature of the colonial origins of comparative fertility behavior and economic development, and implies that economic incentives may overcome historical determinism.

In Chapter 3, I examine the interplay between legal origins and pre-colonial cultural norms of gender roles in determining female economic empowerment in sub-Saharan Africa. Taking advantage of the arbitrary division of ancestral ethnic homelands across countries with different legal origins, I directly compare women among the same ethnic group living in civil law countries and common law countries. I find that women in common law countries are significantly more educated, are more likely to work in the professional sector, and are less likely to marry at young age. However, these effects are either absent or significantly lower in settings where ancestral cultural norms do not promote women's rights and empowerment. In particular, I find little effect in bride price societies, patrilocal societies, and societies where women were not involved in agriculture in the past.

# Dedication

*Dedicated to:*

*My late Dad: **Arhini Byll Claude**, who unfortunately did not stay in this world long enough to see his daughter become a Doctor.*

*My Mom : **Jacqueline Mewena**, because I owe it all to you.*

# Declaration

All chapters of this thesis are self-contained research articles. I acknowledge the contribution of Roland Pongou for the research associated with the three chapters of this thesis. I also acknowledge the contribution of David Canning and Abel Brodeur for the research associated with the second and third chapters of this thesis, respectively. In all cases, their contributions are equal to my own, although the work in this thesis is in my own words.

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# General Introduction

Biology, history, and ancestral traditions are important determinants of human capital accumulation. Owing to the immutable nature of these factors, social scientists and policymakers have generally overlooked the important question of whether appropriate institutions and policies can be designed to address their short-term and long-term consequences. My thesis aims to address this broad question. I consider three different types of immutable determinants of human capital: male versus female biology; colonization; and traditional norms of gender roles. I show that these factors affect various dimensions of human capital, but that their effects can be constrained by certain social interventions and economic policies.

In the first chapter (co-authored with Roland Pongou), we examine how political regime type interacts with child biology and the preconception environment to shape male-female differences in infant mortality. Excess male mortality is observed in most societies. This male survival disadvantage is largely attributed to “genetic fatality”. Perhaps owing to this perception, this issue has been completely overlooked in the economic literature and in policy circles. Using a large individual-level dataset from sub-Saharan Africa, we exploit within-mother variation in political regime type to demonstrate that improvement in the quality of democratic institutions significantly reduces the male survival disadvantage in the first year of life. Interestingly, we find that increased constraints on the executive and increased political competition are features of democracy that matter most for the reduction of the survival disadvantage of boys. Analyzing the possible drivers of the effect of political regime type on excess male infant mortality, we show that democratic institutions significantly constrain the negative influence of male genes on male mortality. We show that this effect operates because democracy fosters the supply of public-health goods like tetanus vaccination during pregnancy, and improves maternal

human capital, which have greater health benefits for boys. Importantly, we rule out hormonal (testosterone) transfer and discrimination against girls as possible mechanisms driving our findings.

The second chapter (co-authored with David Canning and Roland Pongou) addresses the broad question of whether economic incentives can mitigate the long-term effects of historical institutions. In particular, we focus on the role of colonial reproductive laws in shaping fertility behavior in Africa. Exploiting the arbitrary division of ancestral ethnic homelands and the resulting discontinuity in institutions across the British-French colonial borders, we demonstrate that differences in reproductive behavior between African countries can be traced back to colonial institutions.

Central to this chapter, we examine whether the fertility effect of colonial institutions depends on market access. The rationale is that market access increases labor force participation, which in turn increases the opportunity cost of childbearing. It follows that if the market effect dominates the colonial effect on fertility, one should expect the latter to be smaller in areas with higher market access.

We test and validate this hypothesis. We estimate an economically significant negative effect of British colonization on marriage timing, the timing of sexual debut, and the number of children. However, these effects disappear in areas with exogenously high market access, where the opportunity cost of childbearing appears to be high irrespective of the colonizer identity. They are only present in areas with low market access, where job opportunities are scarcer. Examining the possible mechanisms, we demonstrate that the fertility effect of colonial origins in areas with low market access can be directly linked to colonial reproductive laws, a feature of colonial institutions that has received almost no attention in the literature, and their impact on the use of modern methods of birth control. Moreover, we rule out the impact of British colonization on income and women's human capital as the primary channels through which its fertility effect operates.

The third chapter (co-authored with Abel Brodeur and Roland Pongou) analyzes the interplay between traditional cultural norms and legal institutions in determining social and economic outcomes. A large literature documents persistent economic and social impacts of formal historical institutions. However, very little is known about how these institutions interact with traditional cultural norms to determine long-term outcomes.

We address this question by studying the persistent effect of legal origins on female economic empowerment in sub-Saharan Africa, and how pre-colonial cultural norms may attenuate or exacerbate this effect. Taking advantage of the arbitrary division of ancestral ethnic homelands across countries with different legal origins, we implement a Regression Discontinuity Design that directly compares women among the same ethnic group living in civil law countries and common law countries. Our findings show that women in common law countries are significantly more educated, are more likely to work in the professional sector, and are less likely to marry at young age. However, these effects are either absent or significantly lower in settings where ancestral cultural norms do not promote women's rights and empowerment. In particular, we find little effect in bride price societies, patrilocal societies, and societies where women were not involved in agriculture in the past. One conclusion from this chapter is that traditional cultural norms that constrain women's rights negatively affect their human capital and economic empowerment, but these negative effects are attenuated by better formal institutions.

The findings from my thesis have significant policy relevance. The key message from its three chapters is that one should not have a fatalistic view of the determinants of human capital that are immutable or perceived as such. Indeed, the different analyses imply that appropriate policies and social interventions can mitigate genetically induced excess male mortality, overcome the long-term impacts of bad historical shocks such as colonization, and attenuate the negative effect of certain ancestral traditions on women's outcomes.

# Chapter 1

## Democracy, Genes, and the Male Survival Disadvantage

### 1.1 Introduction

At each age of life, more men die than women, such that despite the sex ratio at birth being around 105 males for every 100 females, by age 65, there are only 78 males for every 100 females globally. Beyond posing a fundamental health-equity problem, excess male mortality and the resulting scarcity of male adults in most societies lead to grave social consequences, including lower marriage prospects for women, decreased bargaining power for wives, and an increased risk of sexually transmitted diseases for women due to greater male infidelity, among others. In most societies, the male survival disadvantage arises very early in life. Indeed, among children who died before their fifth birthday between 2000 and 2015 globally, boys outnumbered girls by over 34 million (UN IGME (2018), World Bank (2019)). During the same period, the risk of infant death was about 1.15 times higher for boys than for girls, which by far exceeds the male-to-female ratio at birth. Excess male mortality is largely attributed to “genetic fatality”. Perhaps owing to this perception, this issue has been completely overlooked in the economic literature and in policy circles.<sup>1</sup> What is more, policy actions are constrained by the limited knowledge of its fundamental causes. A pressing question that remains unexplored, therefore, is whether appropriate social interventions can mitigate this male survival disadvantage.

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<sup>1</sup>Comparatively, there is a large and fascinating literature on the female survival disadvantage observed in some Asian societies, inspired by the pioneering research of Sen (1990) on missing women.

In this chapter, we address this question by providing the first causal evidence on the effect of political regime type on sex differences in infant mortality. We also document the possible mechanisms driving this effect, and identify easy-to-implement interventions that can reduce excess male mortality in early ages.

Excess male infant mortality is known to be driven by genetic and preconception environmental differences between the sexes.<sup>2</sup> At the same time, this male survival disadvantage varies a great deal across countries, even in the absence of gender discrimination in the allocation of resources that affect child survival. This fact indeed suggests a possible role for the institutional and technological environment (e.g., [Drevenstedt et al. \(2008\)](#), [Pongou et al. \(2017\)](#), [Goldin and Lleras-Muney \(2019\)](#)).<sup>3</sup> However, the literature documenting a possible interaction between the institutional environment and biological factors in the determination of health outcomes is still very preliminary. This chapter contributes to this nascent research agenda by documenting the causal effect of democracy on the male disadvantage in infant mortality.

The theoretical argument for why democracy should matter for child outcomes has ambiguous implications. It is argued that the coordination problem that hinders public goods provision is less prevalent in democratic regimes ([Boix and Posner \(1998\)](#)). Moreover, leaders are constrained by competitive electoral processes to act in the interest of the people ([Lake and Baum \(2001\)](#)). According to the median voter theorem ([Black \(1948\)](#), [Downs \(1957\)](#)), the policy most preferred by the median voter will be enacted. It follows that if the median voter is poor and therefore has strong preferences for redistribution, elected governments have an increased incentive to provide more public goods, including public health goods and education ([Meltzer and Richard \(1981\)](#), [Przeworski et al. \(2000\)](#), [Lake and Baum \(2001\)](#), [De Mesquita et al. \(2005\)](#)). It is however argued that these theories do not generalize to all societies, in particular those organized around fragmented groups ([Pongou and Tondji \(2019\)](#)), or those in which policies are selected under supermajority rules ([Mahajan et al. \(2019\)](#)). In such societies, multiple equilibria may arise,

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<sup>2</sup>See Section 1.2 for the relevant literature.

<sup>3</sup>Examining the rise and fall of excess male infant mortality during the 20th century, [Drevenstedt et al. \(2008\)](#) attribute the amelioration of male survival observed after 1970 to improved obstetric practices and neonatal care. In a recent study, [Pongou et al. \(2017\)](#) use data on twins in combination with a natural experiment on the development of African institutions to show that the effects of biology and preconception environment on the excess infant mortality of male children are important only in poor institutions. [Goldin and Lleras-Muney \(2019\)](#) focus on late childhood, and show that progress in medical technology had a greater effect on female than on male life expectancy in the United States.

where a particular equilibrium, if selected, might work against redistribution. Consistent with these conflicting theoretical predictions, the empirical literature has found contradictory results. While some empirical studies have shown that democracy reduces infant mortality (see e.g., [Lake and Baum \(2001\)](#), [De Mesquita et al. \(2005\)](#), [Besley and Kudamatsu \(2006\)](#), [Kudamatsu \(2012\)](#)), others did not find a strong relationship ([McGuire \(2006\)](#), [Ross \(2006\)](#), [Gerring et al. \(2012\)](#)).

It follows from these literatures that democracy will matter for the sex gap in infant mortality only to the extent that it constrains or affects genetic and preconception influences on this outcome. Given the higher vulnerability of male children to several leading causes of infant death ([Zeitlin et al. \(2002\)](#), [Mage and Donner \(2004\)](#)), democracy will benefit boys more than girls if it translates into medical progress and public health interventions that lead to a better prevention and treatment of health conditions (e.g., neonatal tetanus) that mostly affect the former. But democracy could also lead to a better prevention of autoimmune diseases (such as measles, tuberculosis, and pneumonia) to which girls are more vulnerable than boys (e.g., [Preston \(1976\)](#), [Garenne \(1994\)](#), [Bouman et al. \(2005\)](#)).<sup>4</sup> These diverging arguments imply that the question of whether democracy reduces or exacerbates excess male infant mortality is best answered empirically.

This chapter focuses on sub-Saharan Africa, an ideal setting because this region has experienced unprecedented institutional changes over the last several decades. Indicators like political participation, competition in executive recruitment, and constraints on executive power have improved substantially in recent years in many African countries. Consistent with economic theory, these positive developments have fostered a better supply of public goods, especially those that benefit the poor. As a result, several health indicators have improved. In particular, the probability of dying within the first year of life has fallen from 107.8 out of every 1,000 births in 1990 to 54.9 out of every 1,000 births in 2015 ([World Bank \(2019\)](#)). However, it remains unclear if these institutional improvements have also reduced the sex gap in infant mortality.

Using micro-panel data from 141 Demographic and Health Surveys, we examine the fertility history of 978,223 mothers residing in 40 sub-Saharan African countries and extract records of more than 3.8 million births over the period 1960-2016. Each live

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<sup>4</sup>[Goldin and Lleras-Muney \(2019\)](#) show that progress in the fight against certain infectious diseases in the United States benefited females more than males.

birth is matched with information on the level of democracy in the country and year of birth. We measure regime type using Polity IV score, which is a variable coded on a scale ranging from full autocracy ( $-10$ ) to full democracy ( $+10$ ). We analyze this variable both as a continuous variable and as a discrete variable, where a regime type categorization is generated distinguishing between an autocratic regime (score ranges from  $-10$  to less than  $-5$ ), anocratic regime (from  $-5$  to  $+5$ ), and democratic regime ( $+6$  and above).<sup>5</sup>

Taking into account the possible endogeneity of transition to democracy, we exploit within-mother variation in regime type, therefore controlling for the confounding effects of unobserved time-invariant characteristics at the mother, household, and country level, in combination with two main empirical strategies to uncover the causal impact of democracy on excess male infant mortality. Our first identification strategy, which is a difference-in-differences (DID) methodology, compares the male-female difference in infant mortality within opposite-sex siblings born before and after democratization in each country and across countries that have transitioned to democracy and countries that have not. The second identification strategy is a regression discontinuity design (RDD), which compares cohorts born close enough to the year of democratization. The assumption is that children born around the time of democratization are identical except for their exposure to democracy. In addition to these two main identification methods, we conduct a number of identification checks, including an event study analysis, to rule out alternative explanations for our main findings.

Our findings are quite consistent across all these strategies. The central finding of this chapter is that democracy significantly reduces excess male infant mortality. The DID estimates show that moving from autocracy to democracy reduces the male survival disadvantage by more than 20% of the average male-female difference in infant mortality. Importantly, the finding is robust to controlling for a range of potentially confounding factors in addition to mother fixed effect, including country specific time trends in infant mortality, year of survey fixed effects, twin status, and birth order. Birth order in particular controls for selection into high fertility, and it has been shown to affect child health outcomes (Jayachandran and Pande (2017)). The result is also robust to specifications that control for important correlates of regime type such as national income and devel-

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<sup>5</sup>This categorization is proposed by Polity IV (Marshall et al. (2010)) and has been used in several studies examining the impact of democracy (see, for example, Epstein et al. (2006)).

opment assistance. Moreover, we show that the negative impact of democracy on excess male infant mortality is consistent when we use five other measures of democracy. The RDD estimation, which also controls for the aforementioned variables, confirms the main finding of the chapter, showing a significant drop in excess male infant mortality in the years following democratization.

Next, we attempt to understand which features of democracy have the greatest significance for the sex gap in infant mortality. This analysis is important because democracy is multifaceted and its various dimensions do not necessarily evolve at the same pace over time or across countries. In addition, uncovering the components of democracy that are most critical for the sex gap in infant mortality is likely to inspire the design of effective policies aiming at addressing this issue. Polity IV score is a composite variable constructed using three main variables (executive recruitment, constraints on the executive, and political competition). Each of these three variables is also constructed using more basic variables. We find that three basic features of democracy matter most for the improvement of the infant mortality sex gap. These features are constraints on the executive, competitiveness of executive recruitment, and political participation.

We then analyze the social and public health mechanisms through which democracy affects the male survival disadvantage in early ages. We consider several determinants of child mortality: (a) those associated with health in utero (maternal tetanus immunization, prenatal care services, and birth weight); (b) those associated with health at birth and after birth (professional assistance during delivery and breastfeeding practices); and (c) determinants associated with health both in utero and after birth (access to improved sources of water and improved sanitation, and maternal education).<sup>6</sup> We find that democracy positively affects most of these health inputs. In addition, we find that these health inputs have stronger health benefits for boys than for girls. This is despite the fact that they are *ex-ante* "gender-neutral".<sup>7</sup>

We also consider the biological mechanism through which democracy reduces the male survival disadvantage. Two main hypotheses explain the sex gap in infant mortality in

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<sup>6</sup>Several of these determinants of child health have been considered in the literature (Jayachandran and Pande (2017), Jayachandran and Kuziemko (2011)).

<sup>7</sup>For instance, as democracy improves, mothers who are pregnant of boys are equally as likely as those who are pregnant of girls to be immunized against tetanus. However, maternal immunization against tetanus has a greater negative impact on male infant mortality.

societies where there is little gender bias in the allocation of resources that affect child health and survival.<sup>8</sup> These hypotheses are: the *biological hypothesis* and the *preconception origins hypothesis*. According to the biological hypothesis, males are biologically weaker than females and are therefore more susceptible to several diseases and premature death (Naeye et al. (1971)). The preconception origins hypothesis holds that some of the preconception environmental factors that determine the offspring sex ratio also affect health in utero and after birth, and consequently affect sex differences in mortality (Pongou (2013)). Following Pongou et al. (2017), we test these two hypotheses using a large sample of twins extracted from our dataset. We find that democratic regimes significantly constrain the negative influence of male genes on male mortality. Improved democratic institutions also constrain the contribution of preconception environmental factors to excess male infant mortality. Interestingly, we consider if our findings could be explained by the hormonal (testosterone) transfer theory. In the context of our study, this theory would imply that the reduced sex gap in infant mortality among opposite sex twins, as a response to improved democratic institutions, is explained by an increase in the likelihood of death among girls due to in utero exposure to testosterone. Indeed, we find no statistical difference in infant mortality between twin girls with a female co-twin and twin girls with a male co-twin, regardless of the regime type. This rules out the hormonal transfer theory as a possible explanation for our findings.

This study is the first to analyze the causal effect of regime type on excess male infant mortality, and to identify the possible mechanisms through which this effect operates. It contributes to the aforementioned literature that studies the effect of democracy on multiple dimensions of human development. This literature has not examined the effect of democracy on sex differences in health outcomes. In a cross-regional analysis of sub-Saharan African countries, Pongou et al. (2017) exploit variation in the historical development of institutions across African countries to demonstrate that improvement in the quality of institutions is associated with a fall in the male survival disadvantage. Our study is related to this work, but differs in important respects. Unlike Pongou et al. (2017), we focus on democracy and its subcomponents, and show that they causally reduce the male survival disadvantage in early ages. Moreover, we provide novel insights on

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<sup>8</sup>Sen (1990) argues that such bias does not exist in sub-Saharan Africa. Our analysis confirms this argument, as we do not find gender discrimination in the allocation of the health inputs mentioned earlier, regardless of the type of political regime.

the possible channels through which democracy operates, and identify easy-to-implement interventions likely to reduce excess male infant mortality. Our findings are consistent with the small literature that examines how public health interventions affect genetic influences on health, and that argues that health policies should account for genetic factors (Boardman et al. (2012), Boardman and Fletcher (2015)). Indeed, results from our study have practical policy implications in that they are likely to inform policymakers on the extent to which gender-neutral investments in certain health inputs can reduce the sex gap in survival.

The rest of this chapter is laid out as follows. In Section 1.2, we present the conceptual framework for our study. Section 1.3 describes the datasets used for the analysis and provides summary statistics. Section 1.4 presents the methodology and the empirical strategy. The empirical findings are presented in Section 1.5 and Section 1.6. Section 1.7 discusses the mechanisms through which democratic institutions affect the male survival disadvantage in early ages. Finally, we conclude in Section 1.8.

## 1.2 Conceptual Framework

In order to lay out the relationship between democratic institutions and the male-female gap in infant mortality, one needs to understand the basic or proximate causes of excess male mortality. For this reason, we will start by reviewing the literature on the main drivers of the sex difference in child mortality. Then we will describe how democratic institutions shape these determinants to affect the survival disadvantage of boys.

### 1.2.1 Causes of Excess Male Mortality in Early Ages

Two major hypotheses have been advanced to explain sex mortality differences in early childhood, when behavioral differences should be minimal. The first hypothesis attributes the male-female difference in infant mortality to biological and genetic differences between the sexes (Naeye et al. (1971), Waldron (1983)). According to this hypothesis, males have a weaker immune system and are more susceptible to X-linked recessive disorders. As a consequence, they have an inherently greater vulnerability to most leading causes of child

morbidity and mortality such as perinatal conditions (including prematurity and respiratory distress), congenital anomalies and infectious diseases (Waldron (1998), Sawyer (2012)). This greater vulnerability translates into excess male mortality, especially in societies where there is little gender bias in the allocation of food and of health resources. Crediting the biological explanation of the sex gap in infant mortality, many empirical studies have examined the impact of male sex on infant mortality in both developed and developing countries (see Drevenstedt et al. (2008), Pongou (2013), Boco (2014) for instance). Consistent with the theory, they have effectively reported a higher risk of death among male infants.

However, the biological hypothesis has some limitations that have been acknowledged in the literature (Pongou (2013), Pongou (2015)). Studies have pointed out several diseases to which females are more vulnerable than males. Analyzing sex differences in mortality by causes of death, Preston (1976) and Garenne and Lafon (1998) show that females are more likely to die from certain infectious and autoimmune diseases, especially in late childhood and early adulthood. In the same vein, in a study that focuses on the United States, Goldin and Lleras-Muney (2019) use a historical dataset containing information on the causes of death in Massachusetts to show that infectious diseases had a disproportionate effect on females between the ages of 5 and 25. These studies seem to suggest that the biological explanation of excess male mortality in early years is not conclusive.

Other studies have stressed the fact that biological differences are unlikely to account for the cross-country and temporal variations in the magnitude of the male disadvantage in infant mortality, as documented, for example, in Drevenstedt et al. (2008), Boco (2014), and UN IGME (2015). Analyzing the surprising rise and fall in excess male mortality experienced by many industrialized countries over the 20th century, Drevenstedt et al. (2008) suggest that the male survival disadvantage in early-age is the outcome of a complex interplay between biology and both the medical-technical and epidemiological context, rather than the result of solely biological susceptibility to diseases. Indeed, they show that as causes of infant death shifted from infectious diseases to perinatal conditions due to improvement in living conditions in the 1970s, the male disadvantage in infant mortality worsened because males have a greater vulnerability to mortality for perinatal conditions. The subsequent medical-technical advancements which minimized

health complications from perinatal conditions have disproportionately benefited males, therefore reducing their disadvantage in infant mortality.

An additional limit of the biological hypothesis can be traced into studies that provide evidence of environmental causes common to both sex ratio and reproductive impairment (Pongou (2013) provides a review of these studies). For instance, Pongou (2015) explained the concurrent increase in American female births and their relative mortality after the Second World War by the exposure of females to environmental hazards that are likely to increase the probability of female conception and decrease the female survival advantage. Despite this suggestive evidence, the biological hypothesis and most of the empirical work that has tested this hypothesis implicitly assumes that the sex of the child is randomly determined. One implication of this assumption is that most studies has failed to consider the endogeneity of the sex of the child and therefore has provided only a partial understanding of the determinants of the sex gap in infant mortality.

In order to address these limitations of the biological hypothesis, Pongou (2013) formulates the preconception origins hypothesis to explain the sex gap in infant mortality. This hypothesis holds that parental circumstances around the time of conception such as diet, health conditions, or exposure to environmental hazards, which have been shown to determine both the sex of a child and the health of a child in utero and after birth, also affect the sex gap in infant mortality. This hypothesis generalizes the biological hypothesis and provides a broader framework for understanding temporal and spatial variation in the sex gap in mortality, and how these variations relate to variation in the sex ratio at birth. Both the biological hypothesis and the preconception origin hypothesis are tested in a unified framework in Pongou (2013). This study proposes a decomposition methodology based on twins to estimate the distinct contributions of child biology and preconception environment to the male disadvantage in infant mortality.<sup>9</sup> Applying this methodology to a large sample of data on twins from sub-Saharan Africa, Pongou (2013) finds that both child biology and preconception environmental factors significantly contribute to excess male infant mortality.

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<sup>9</sup>In order to understand the channels through which improved democratic institutions could affect the infant mortality sex gap, the distinction between child biology and the preconception environment is particularly important because, unlike biology, the preconception environment can be modified if we identify the specific components in this environment that are likely to affect the survival chances of babies. In this sense, the preconception origins hypothesis lays the groundwork for health policies aiming to reduce this early-life health inequality between boys and girls.

## 1.2.2 Why Does Democracy Matter for Sex Differences in Infant Mortality?

It follows from Section 1.2.1 that democracy will matter for the sex gap in infant mortality only to the extent that it constrains or affects genetic and preconception influences on this outcome. In this section we will highlight possible channels through which democracy might affect excess male infant mortality positively or negatively. The argument essentially relies on the assumption that democracy induces the provision of certain public goods that affect survival.

### 1.2.2.1 Democracy and the Provision of Public Goods

Political economy models provide several arguments for why democratic governments outperform nondemocratic governments in the provision of public goods. One mechanism emphasizes the role of accountability structures. It is argued that democracies are constrained by the electoral process with contested elections and universal suffrage to prioritize the provision of public goods rather than private goods, while nondemocracies or less democratic societies face no such constraint. In fact, democratic leaders who are competing for votes are more likely to provide higher levels of public goods since they are likely to be evaluated on their ability to provide basic benefits to their constituents (Lake and Baum (2001)). This argument is related to the median voter theorem which implies that a majoritarian democracy should have more incentives to provide public goods as long as these policies are the most favoured by the median voter and hence by a majority of the population. Therefore, in settings where the poor face the greatest health challenges and constitute the majority of the population, the provision of public health goods become more cost effective than private transfer to win support (De Mesquita et al. (2005)).

Another argument developed in the literature focuses on the power of citizen coordination. This literature contends that the coordination problem that hinders public goods provision is minimized when citizens share the same preferences or when social norms significantly constrain defectors and free-riders. Since such coordination is more likely to be present in democratic systems due to the existence of civil society groups (Boix and

Posner (1998)), local public goods provision is also likely to be higher.

Notwithstanding the above-mentioned literature, empirical studies analyzing the link between regime type and public goods provision provide conflicting evidence. Some evidence show that public goods provision and income redistribution are higher under democratic systems (Sen (1981), Meltzer and Richard (1981), Przeworski et al. (2000), Lake and Baum (2001), De Mesquita et al. (2005), Deacon (2009), Acemoglu et al. (2015), Acemoglu et al. (2019)). In particular, Lake and Baum (2001) and De Mesquita et al. (2005) find evidence for higher provision of public education (literacy rates, enrollement rates) and public health goods (immunization, access to clean water) in more democratic regime. However, Mulligan et al. (2004) find no association between public spending for education and the level of democracy. Focusing on infant mortality<sup>10</sup>, cross-country analyses such as Przeworski et al. (2000), Lake and Baum (2001) and Besley and Kudamatsu (2006) find that democracy reduces infant mortality, though Ross (2006) estimates an insignificant effect using within-country variation. In a recent work, Kudamatsu (2012) produces more consistent estimates by using micro panel data and taking into account several flaws from previous empirical studies. He finds that democracy significantly reduces infant mortality. Analyzing the mechanism, he also provides evidence, though not conclusive, that the negative effect of democracy on infant mortality is mediated by improved access to sanitation, clean water, immunization and higher global health spending after democratization.

Figure 1.7 corroborates this argument. This figure shows a positive correlation between the level of democracy, proxied by polity IV score<sup>11</sup>, and the prevalence of selected public health conditions associated with child survival (maternal tetanus immunization, prenatal visits, clean water and sanitation, low birth weight, breastfeeding during at least six months, and maternal education).

### 1.2.2.2 Democracy and the Sex Gap in Infant Mortality

Compounding the arguments above, we hypothesize that improvement in the quality of democratic institutions will reduce the male disadvantage in infant mortality to the extent

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<sup>10</sup>The literature assessing the link between democracy and infant mortality has not examined the effect of democracy on the male-female difference in this outcome.

<sup>11</sup>See Section 1.3 for a detailed description of this index.

that better democracy promotes the provision of health inputs effective in reducing either the exposure to causes of child death for which boys have the highest vulnerability or the likelihood that these causes lead to death. More precisely, there are at least two channels through which improvement in democratic institutions could reduce the sex gap in infant mortality. First, democracy can constrain biological or genetic expression by addressing the social and environmental causes of diseases and death.<sup>12</sup> In the context of the sex gap in infant mortality, such causes could include a lack of immunity against certain diseases, a lack of proper prenatal care, the absence of breastfeeding, and poor maternal conditions resulting, for instance, in low birth weight. Second, democracy can either affect or constrain preconception influences on excess male mortality. These hypotheses are summarized in Figure 1.1.

As an illustration of this mechanism, assume that improved democracy translates into higher coverage of tetanus immunization during pregnancy. Because boys are more likely to be affected by neonatal tetanus (Garenne and Lafon (1998), Sawyer (2012)), they will benefit more from democracy than girls. However, democracy could also lead to a better prevention of autoimmune diseases, to which girls are more vulnerable than boys (Ahmed et al. (1985), Chao (1996), Bouman et al. (2005)). Indeed, excess female mortality for certain diseases including measles, tuberculosis, influenza-pneumonia-bronchitis and certain infectious and parasitic diseases at young ages is well documented (Preston (1976), Garenne (1994), Goldin and Lleras-Muney (2019)). If democracy improves the prevention of these diseases, its benefits will accrue more to girls than to boys. These differing arguments imply that the question of how democracy affects the sex gap in infant mortality is best answered empirically.

### 1.3 Data and Descriptive Statistics

To estimate the causal effect of democratic institutions on excess male infant mortality, this study links country-level data on democracy with micro-data on the survival of children. In this section, we first describe Demographic and Health Surveys (DHS) from

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<sup>12</sup>Note that democratic institutions cannot change biology, so they cannot change genetic differences between boys and girls. This is why most studies assume explicitly in the literature that biological or genetic differences between the sexes do not vary across countries (Drevenstedt et al. (2008), Sen (2003), Pongou et al. (2017)). Democracy may only constrain the expression of genes.

which we obtain individual-level data on infant mortality. Second, we present how we measure democracy in this project. Then, we briefly present some descriptive statistics.

### **1.3.1 Data on Infant Mortality**

Our outcome of interest is infant mortality, defined as the risk of death within the first year after birth. Primary data on this variable is obtained from Demographic and Health Surveys (DHS). DHS surveys are representative at the national and subnational level and are comparable across years and across countries. In each DHS, a standardized individual questionnaire is administered to a representative sample of women of reproductive age (between 15 and 49 years old). To construct the dataset, we merge 136 Birth Recode files from DHS surveys conducted between 1986 and 2017 in 40 sub-Saharan African countries (see Appendix Table [A1.8](#)). Each file contains all the live births reported by interviewed mothers. For the analysis, we consider only children that were fully exposed to mortality risk in their first year and so we drop children who were less than one year old at the survey.

Selected women in each DHS were asked to provide information about their fertility history including the date of birth of each child, whether the child is still alive, and, if the child has died, their age at death. We use this information to define the dependent variable as a dummy that equals one if the child dies before the age of one and zero otherwise. DHS also gather detailed information on a host of demographic and socio-economic characteristics of mothers and their children. Based on this information, we define the following indicators used in the empirical analysis below: a dummy for whether the child is a boy, a set of birth order dummies, an indicator for whether the child is a twin, and an indicator for the age of the mother at delivery. In addition, DHS collect information on many health inputs including prenatal, antenatal, and postnatal health care services. The latter information is used in Section [1.7](#) to investigate the mechanism underlying our results.

### **1.3.2 Measures of Democracy**

Democracy is a multidimensional concept that is hard to summarize. It is common in the literature to emphasize the procedural aspects of democracy (elections and universal

suffrage, for instance) as well as the substantive aspects of democracy (civil liberties). There are myriad measures of democracy in the literature that integrate these different dimensions. In the main results of this chapter, we rely on a widely used measure of democracy: Polity IV score. Polity IV is the latest version of the polity data series, which assesses a state's level of democracy based on (1) an evaluation of the state's elections for competitiveness and openness, (2) the degree of political competition, and (3) the extent of legislative and judicial constraints on the executive authority (Marshall et al. (2010)). By combining these three features, Polity IV measures the extent to which democratic or authoritarian rules are institutionalized in a given state or country.

The Polity IV dataset contains coded annual information on the level of democracy of over 100 countries around the world including the 40 sub-Saharan African countries for which DHS are available. Among the numerous continuous measures of democracy, polity IV is the only measure with broad historical coverage. In fact, it covers the period spanning from 1800 to 2015. In practice, a "Polity score" is assigned to each country for every year in the period. This score is a composite index derived from the coded values of six components of the authority characteristics including regulation of chief executive recruitment (XREGG), competitiveness of executive recruitment (XRCOMP), openness of executive recruitment (XROPEN), constraint on chief executive (XCONST), competitiveness of political participation (PARCOMP), and regulation of participation (PARREG).<sup>13</sup> The polity score, therefore, provides a regime authority score that ranges from  $-10$  (full autocracy) to  $+10$  (full democracy), with  $-10$  through  $-6$  corresponding to autocracies,  $-5$  through  $+5$  corresponding to anocracies, and  $+6$  through  $+10$  corresponding to democracies (Marshall et al. (2010)).

While in the empirical analysis below the continuous polity IV measure of democracy is emphasized in the main results and the mechanism section, we also report results using dichotomous and trichotomous indices of democracy, mainly for robustness checks. In fact, a common practice in research using a continuous measure of democracy is to set cut-points or threshold values that define regime types. For the purpose of this study, we will use three different regime type categorizations. First, based on the polity score, we define two dummies for the level of democracy such that a country is democratic if polity score is strictly greater than the median value of Polity IV which is strictly

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<sup>13</sup>See Section 1.5 for a detailed explanation of these components.

greater than +4. Second, following the literature (Marshall et al. (2010)), we also define a trichotomous index of democracy such that any authority regime at or below  $-6$  is considered autocratic, while any authority regime at or above  $+6$  is labelled democratic. Authority regime in between these cut points is labelled anocratic.

In addition, to ensure that our results are not driven by the components and methodology used to define the Polity IV score, we implement several sensitivity analyses using binary indicators of democracy based on alternative measures of regime authority including Political Risk Services (PRS), Freedom House (FH), a democracy measure defined by Boix (2014) (BMR), a measure of democracy defined by Kudamatsu (2012), and an index of political regimes and transition constructed by Papaioannou and Siourounis (2008) (PS). Similar to Polity IV, each of these measures of democracy is a composite index of several dimensions of the regime authority.

### 1.3.3 Descriptive Statistics

Using information on the country and year of birth of the child, we merged the two datasets described above to obtain a final dataset containing information on a sample of 3,792,650 children born between 1960 and 2015 from 978,223 mothers residing in 40 sub-Saharan African countries. Table 1.1 reports summary statistics for the main variables used in the empirical analysis below. This table reveals that the average child is a boy born in 1995 to a mother aged 25 years old at the time of delivery. This boy is on average 1.18 times more likely to die within the first year of life compared with a girl.

Figure 1.2 provides a graphical illustration of the relationship between excess male infant mortality and the level of democracy. Each dot on this graph represents the average male-female difference in infant death for each value of Polity IV. Using the cut-offs ( $-5$  and  $5$ ) for the three-part regime categorization (autocracy, anocracy, and democracy) suggested by the authors of Polity IV, we draw two vertical lines at the aforementioned cut-offs and overlay on the scatter plot regression lines fitted separately for values falling within each institutional setting. Whereas Figure 1.2 shows no difference in excess male infant mortality when we move from an autocratic to an anocratic regime, we observe a drop-off in the excess male infant mortality when we move to a democratic regime. This graphical analysis, while imprecise, suggests a role for democratic institutions in

shaping the male-female difference in infant mortality. In the next sections, we discuss our identification strategy and provide further evidence on this relationship.

## 1.4 Identification Strategies

Since children born before and after a transition to democracy might differ in several aspects (other than their institutional environment) relevant for their survival, using a first difference in comparing them might not necessarily measure the influence of democratization on child survival and therefore on male-female difference in child survival. To identify the causal effect of democracy on the male-female difference in infant mortality, we use two empirical strategies. First, we implement a difference-in-differences approach with mother fixed effects. Specifically, we compare the male-female difference in infant mortality across siblings born before and after democratization. Second, we exploit the timing of democratization as a cut-off to assess the change in the male-female difference in infant mortality across the cohorts born immediately before and after the year of democratization. The latter approach is a Regression Discontinuity Design where the running variable is measured by the difference between the child’s birth year and the year of democratization.

### 1.4.1 Difference-in-Differences with Mother Fixed Effects

We exploit temporal and spatial variation<sup>14</sup> in regime type to assess the causal impact of democracy on excess male infant mortality. Our first identification strategy is a difference-in-differences analysis formalized by the following linear probability model:

$$\begin{aligned}
 M_{icmt} = & \lambda_0 + \lambda_1 \text{Male}_i + \lambda_2 \text{Demo}_{ct} + \lambda_3 \text{Male}_i \times \text{Demo}_{ct} + \theta_c \text{TREND}_{ct} \\
 & + X'_{icmt} \pi + \alpha_m + \delta_t + \varepsilon_{icmt}
 \end{aligned}
 \tag{1}$$

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<sup>14</sup>Similar identification strategies have been adopted in studies focusing on developing countries (e.g., [Duflo \(2001\)](#), [Bleakley \(2010\)](#), [Martinez-Bravo \(2017\)](#), [Larreguy and Marshall \(2017\)](#)).

where  $M_{icmt}$  is the outcome of interest which corresponds to a dummy that equals one if a baby  $i$  born in year  $t$  to mother  $m$  in country  $c$  dies before their first birthday. The variable  $\text{Male}_i$  is a dummy that takes value one if child  $i$  is a boy and zero otherwise.  $\text{Demo}_{ct}$  refers to the level of democracy in each country-year. It measures the level of democracy of the country in the year of birth of child  $i$ .<sup>15</sup> As we show later, we analyze democracy both as a continuous variable and as a discrete variable. The next regressor  $\text{Male}_i \times \text{Demo}_{ct}$  is an interaction term between the sex of the child  $i$  and the level of democracy.  $\alpha_m$  and  $\delta_t$  refer to mother fixed effects and child’s year of birth fixed effects, respectively.  $\theta_c \text{TREND}_{ct}$  is a linear time trend specific to country  $c$ . It is a set of dummy variables, where each dummy is an interaction between a country of birth and a binary indicator for the year of birth. Finally,  $X'_{icmt}$  is a vector of exogenous covariates including an indicator for twin birth, a set of birth order dummies, and year-of-survey fixed effects.

A number of features of this empirical specification are worth highlighting. First, by controlling for mother fixed effects, our identification strategy only accounts for within-mother variation in the level of democracy. In this case, the empirical exercise consists in the comparison of survival chances across opposite-sex siblings, which ensures that we rule out time-invariant unobserved characteristics of mothers as potential confounders. In addition, mother fixed effects indirectly control for time-invariant confounders at the household, community, and country-level.<sup>16</sup> Second, another attractive feature of this specification is that it controls for year of birth fixed effects. This is particularly helpful since it has been shown that although infant mortality rates are decreasing on a yearly basis globally, the pace of reduction varies significantly across countries.<sup>17</sup> Adding year of birth fixed effects in addition to mother (and hence country) fixed effects in the specification accounts for this fact by ensuring that any yearly global trend in infant mortality is not driving our results. Third, in order to ensure that our results do not capture more subtle trends in the probability of death in early ages we enrich our specification by allowing each country to have its own linear trend in infant mortality. Further, controlling for child characteristics known to affect child survival, such as twin status and birth order, allow us to increase the precision of our estimates. In addition, since birth order

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<sup>15</sup>As described in Section 1.3, we present our results using Polity IV score. But in the Appendix, we also consider other existing measures of democracy.

<sup>16</sup>We also estimate a variant of equation (1) that only controls for country fixed effects.

<sup>17</sup>See for example Figure A.5 in Kudamatsu (2012) showing heterogeneity in the decreasing trends in infant mortality rates between democratic and non-democratic countries in sub-Saharan Africa.

measures selection into high fertility (Jayachandran and Pande (2017)), it can be view as controlling for the effect of democracy on fertility in our context.

In equation (1),  $\lambda_1$  describes the male-female difference in infant mortality when the level of democracy is zero, and our parameter of interest  $\lambda_3$  describes the marginal effect of democracy on this difference. We expect to obtain estimates such that  $\lambda_1 > 0$  because excess male infant mortality has been documented in sub-Saharan African countries, and such that  $\lambda_3 < 0$ , meaning that countries with a high level of democracy should experience a lower survival disadvantage of boys within the first year of life.

Our empirical approach in equation (1) is, therefore, equivalent to a difference-in-differences identification strategy where, we compare male-female difference in infant mortality within opposite sex siblings born before and after democratization in each country and across countries that have already transitioned to democracy and countries that had not.

**Democracy as a Continuous Treatment.** We first estimate equation (1), using democracy as a continuous variable. Here, democracy is measured by the Polity IV index which takes any integer value between  $-10$  and  $+10$ . This approach therefore exploits marginal changes in the quality of democracy to estimate the causal effect of democracy on excess male infant mortality. Indeed, this estimation approach takes advantage of the fact that most countries are likely to experience small changes in the quality of their political institutions within short time periods. It follows that most mothers are likely to have experienced such small changes during their childbearing period, even if they did not experience a transition to full democracy during this period. Our identification strategy which compares opposite-sex siblings is therefore using most of our observations.

**Democracy as a Discrete Treatment.** In a second approach, we estimate equation (1) using categorical measures of democracy. Following the literature, we define three discrete variables for democracy based on different cut-off points. The first variable is a binary indicator equal to one if the polity score is strictly greater than  $-4$  (the median value of polity in our sample), and zero otherwise. The second variable is a binary variable equal to one if the polity score is strictly greater than  $+4$ . The third variable is a trichotomous indicator based on the three-part regime categories suggested by the Polity

IV project. The three categories of this variable are: autocracy (polity score from  $-10$  through  $-6$ ), anocracy (from  $-5$  through  $+5$ ), and democracy (from  $+6$  through  $+10$ ). Contrary to the continuous treatment case, the discrete model implicitly requires that the fertility period of women (or the period during which a woman has children) be sufficiently long for her to have children both before and after democratization. Identification is therefore based on women whose childbearing period began before a regime transition and ended after the transition.

#### 1.4.1.1 Validity of the Difference-in-Differences

A consistent estimation of  $\lambda_3$  in equation (1), requires that the interaction term between the sex of a child and democracy be as good as randomly assigned, conditional on the controls. That is, it must be uncorrelated with the error term.

One way to validate our identification strategy is to assess whether the decline in excess male infant mortality started with democratization. This is easily observable in an event study where we estimate year-wise changes in excess male infant mortality before and after democratization.<sup>18</sup> Empirically, we estimate the following equation:

$$\begin{aligned}
 M_{icmt} = & \lambda_0 + \lambda_1 \text{Male}_i + \sum_{t=x}^n \lambda_{2t} \text{Demo}_{ct} + \sum_{t=x}^n \lambda_{3t} \text{Male}_i \text{Demo}_{ct} \\
 & + \theta_c \text{TREND}_{ct} + X'_{imct} \pi + \alpha_m + \delta_t + \varepsilon_{icmt}
 \end{aligned}
 \tag{2}$$

This equation is similar to our main specification in equation (1), but where the variables  $\text{Demo}_{ct}$  are dummies indicating the number of years before and after democratization. For example,  $\text{Demo}_{c-5}$  (resp.  $\text{Demo}_{c5}$ ) is a binary variable equal one if a child was born in country  $c$  five years before (resp. after) democratization. For the purpose of this analysis, we follow the Polity IV project, in defining a country as a democracy whenever the Polity IV score assigned to this country is greater than or equal to 6. In order to estimate the year-wise dummies before and after democratization, two restrictions

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<sup>18</sup>A similar approach has been implemented in [Kudamatsu \(2012\)](#).

on the sample are necessary. First, we drop from the sample countries that experienced a collapse of democracy the years following their transition from autocracy or anocracy to democracy.<sup>19</sup> This restriction is necessary to avoid using interrupted democracy as a control group since this could lead to a downward bias of the effect of democracy on excess male infant mortality. Second, we drop from the sample children born within one year before democratization in some specifications and include them in others. One justification for dropping these children is that they are likely to be partially treated by democracy. For example, a child born in December 1989 in a country that transitioned into democracy in January 1990 and who died at the age of five months was partially treated. In general, children born within the year following democratization were likely to be treated because their first birthday would have occurred after democratization. Dropping those children ensures that our year-wise estimates are not biased downward.<sup>20</sup>

#### 1.4.1.2 Identification Checks

**Additional Controls.** Despite the use of mother fixed effects in the main specification, a remaining potential threat to identification stems from the fact that democracy might not be totally exogenous (Lipset (1959)). To address this potential issue, we control for two important time-varying country-specific factors that correlate with both the level of democracy and child survival (see for example Kudamatsu (2012)). These controls are: income per capita and the level of development assistance. Note that all the time invariant correlates of democracy have already been controlled through mother fixed effects. As we will see later, these additional controls change neither the magnitude nor the significance of the estimate of the effect of democracy on excess male infant mortality, strongly suggesting that our findings are not driven by selection on unobserved variables.

**Alternative Definitions of Democracy.** Another robustness check is to assess whether the results obtained from the aforementioned identification strategies are robust to other definitions of democracy. As mentioned in Section 1.3, there are many other measures of

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<sup>19</sup>These countries are: Lesotho, Madagascar, Malawi, Mali, and Zambia.

<sup>20</sup>Several other studies drop partially treated observations from the analysis (e.g., Duflo (2001), Martinez-Bravo (2017), Larreguy and Marshall (2017)). For example, in a study examining the impact of free primary education on educational outcomes in developing countries, Duflo (2001) drops children that are partially treated by the reform.

democracy that exist in the literature. We collect data on five other measures of democracy and assess the effect of democracy on excess male infant mortality using these alternatives measures. The first measure is the democratic accountability index drawn from Political Risk Services database. This index defines democracy based on: (1) whether there are free, fair and competitive elections; (2) whether there is an executive power turnover; (3) the level of civil rights and personal liberties protection; and (4) how responsive the government is to its people.<sup>21</sup> The second measure of democracy is a measure of political and civil liberties provided by Freedom House. The third measure of democracy is defined by Kudamatsu (2012). The latter defines democracy as a political system where (1) the chief executive of the national government has been elected in multiparty elections with universal suffrage, without subsequently banning opposition parties; and (2) a new chief executive has assumed office by winning multiparty elections. The fourth measure of democracy is drawn from a dataset of permanent democratic transitions during the Third Wave of Democratization (Papaioannou and Siourounis (2008)). The fifth measure is a dichotomous indicator of sovereignty/independence defined by Boix (2014).

## 1.4.2 Regression Discontinuity Design

In this section, we examine the causal impact of democracy on the male-female difference in infant mortality using a Regression Discontinuity Design with mother fixed effects. Assuming that the cohort born around the time of democratization are identical except for their exposure to democracy, we exploit cross-cohort variation in exposure to democracy within the same mother, to identify the causal effect of democracy on excess male infant mortality.

### 1.4.2.1 Baseline RDD Strategy

To implement our RDD strategy, we focus on the subset of sub-Saharan African countries that transitioned to democracy without subsequently returning to a non-democratic regime. Following the trichotomous index of democracy described in section 1.3, we define a country as democratic when its Polity IV score is greater than or equal to +6.

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<sup>21</sup>The PRS methodology is available at the following link: <https://www.prsgroup.com/wp-content/uploads/2014/08/icrgmethodology.pdf>.

Below this threshold, the country is thus considered as non-democratic. Based on this definition, we identify 14 countries that transitioned to democracy, but only 9 countries that did not experience a collapse of democracy in the subsequent years.<sup>22</sup> Our local RDD estimation therefore focuses on these 9 countries. We refer to the cohort born after the year of democratic transition as *fully treated*, while those born before the year of democratization serve as the *control group*.

In practice, our baseline RDD analysis is given by the following equation:

$$\begin{aligned}
 M_{icmt} = & \beta_0 + \beta_1 \text{Male}_i + \beta_2 \text{Demo}_{ct} + \beta_3 \text{Male}_i \times \text{Demo}_{ct} + f(\text{BD}_{icmt}) \\
 & + \alpha_m + \delta_t + \varepsilon_{icmt}
 \end{aligned}
 \tag{3}$$

Similar to equation (1), the dependent variable  $M_{icmt}$  is a dummy that takes value one if a baby  $i$  born in year  $t$  from mother  $m$  in country  $c$  dies before its first birthday. The variable  $\text{Male}_i$  is a dummy that takes value one if child  $i$  is a boy and zero otherwise.  $\text{Demo}_{ct}$  is a dummy indicator for the level of democracy in each country-year. As mentioned above, for a given country  $c$  and year  $t$ , this variable takes value one if the Polity IV score is greater or equal to +6 and zero otherwise. The next regressor  $\text{Male}_i \times \text{Demo}_{ct}$  is an interaction term between the sex of the child  $i$  and the level of democracy.  $\alpha_m$  and  $\delta_t$  refer to mother fixed effects and child's year of birth fixed effects, respectively.

In addition, equation (3) controls for a second-order RD polynomial given by  $f(\text{BD}_{icmt})$ . For any given child, this is a polynomial function of the absolute difference between the year of birth and the year of democratization in the country of birth of this child.

Similar to our difference-in-differences strategy, controlling for mother fixed effects allows us to get rid of some time invariant characteristics of mothers, households, communities, and countries as potential confounders. This is particularly helpful in our RDD

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<sup>22</sup>Among the countries for which DHS surveys are available, the following countries transitioned to democracy in the 1990s but experienced a collapse of their democratic system in the following years: Lesotho (1993), Madagascar (1992), Malawi (1994), Mali (1992), and Zambia (1991).

The following countries have experienced a sustain democratic system after their transition: Benin (1991), Burundi (2005), Comoros (2004), Ghana (2001), Kenya (2002), Liberia (2006), Senegal (2000), Sierra Leone (2007), and South Africa (1992).

setting which requires that around the cutoff (the year of democratization) children should only differ in their exposure to democracy. Indeed, controlling for mother fixed effects ensures that cohorts born around the eligibility year cut-off are balanced across most of the core time invariant determinants of child survival. In the next section, we provide evidence that cohort born around the year of democratization have similar trends in several pre-treatments characteristics that are relevant for child survival.

**Partially Treated Children.** Since, in any given country, children born in the year before the transition to democracy have necessarily spent part of their first year of life under the democratic regime<sup>23</sup>, they are thus more likely to be *partially treated*. As a result, not taking into account partially treated children is likely to bias our estimates. We address this issue by estimating equation (3) over a sample that excludes children partially exposed to democracy. That is, children born the year preceding democratic transition.

#### 1.4.2.2 Validity of the RDD

The central identifying assumption in any RDD specification requires that observations are close-as-randomly assigned into treatment and control groups. That is, relevant determinants of the outcome of interest, besides the treatment, should vary smoothly at the cut-off. In our setting that exploits within mother variation in the quality of democracy and compares male-female difference in infant mortality before and after democratization, the random assignment assumption would imply that within the same mother, cohorts born around the time of democratization are identical except for their exposure to democracy.

Despite the fact that controlling for mother fixed effects ensures that we compare observations with similar time-invariant characteristics that are likely to explain our results, a remaining potential threat to the identifying assumption is the fact that children are likely to differ along unobserved time-varying characteristics that are relevant for their survival. To address such concerns, we restrict our analysis to cohorts with a birth year close to the year of democratization. Specifically, in our main RDD analysis we focus on a bandwidth of ten cohorts on either side of the year of democratization.

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<sup>23</sup>Except for those children who died before democratization.

Figure 1.5 provides evidence that our control group only differs from the treated one with respect to their exposure to democracy. In this figure, we examine the trends before and after democratization for selected factors known to determine child survival. These factors include the sex of the child, twin status, birth order, age of mother at birth, and the number of children at each birth. For each factor, we draw a scatter plot where each gray dot represents the average of the corresponding outcome for a given cohort. Overlaid on each scatter plot is a local polynomial fitted separately for cohorts born before and after democratization. It appears from Figure 1.5-*b* that there is no difference in the male-female composition of children before and after democratization. That is, the flat distribution of dots around democratization suggests that cohorts born before and after democratization have the same sex-distribution of children. Similarly, despite a positive trend in the proportion of twins, the average birth order, the age of mother at birth, and the number of children at birth, there is no jump at the cut-off for each of these variables (see Figure 1.5-*c* - Figure 1.5-*f*).<sup>24</sup> The scatter plots showing the distribution of the male-female difference in these variables (see Figure 1.5-*g* - Figure 1.5-*i*) assure that there are no significant differences in the composition of cohorts born before and after democratization. Overall, Figure 1.5 indicates that cohorts on either side of the year of democratization are balanced across treatment groups with respect to most of the determinants of child survival, except for the male-female difference in infant mortality. Indeed, we observe a clear jump around the year of democratization in excess male infant mortality only (see Figure 1.5-*a*).

## 1.5 Effect of Democracy on Excess Male Infant Mortality

### 1.5.1 Difference-in-Differences Estimates

In this section, we present our main empirical results using the difference-in-differences methodology. First, taking advantage of marginal changes in the quality of democratic institutions, we present estimates of equation (1) using a continuous treatment. Second,

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<sup>24</sup>Note that the positive trend in these variables is expected because each of them increases with mother's age and many mothers in our sample had a birth before and after democratization.

we re-estimate equation (1) but with democracy as a discrete variable. Third, we test the common trends assumption using an event study approach to provide evidence of no pre-treatment trends. Fourth, to further assess the validity of our identification strategy, we do different sensitivity checks. We present results where we augment our main specification to include controls for other covariates of democracy. In addition, we implement our empirical strategy using alternative definitions of democracy.

### 1.5.1.1 Democracy as a Continuous Treatment

The baseline estimates showing the effect of democracy on excess male infant mortality are reported in Table 1.2. In this table, each column controls for year of birth fixed effects, survey year fixed effects, and child characteristics including twin status and birth order. Unlike column (1), column (2) controls for country fixed effects, and columns (3)-(5) control for mother fixed effects, unlike the first two columns. In addition, column (4) controls for country-specific linear trend in infant mortality. Robust standard errors adjusted for clustering at the country level are displayed in parenthesis.

Regardless of the specification, estimates in Table 1.2 show that, consistent with the existing literature, being a boy significantly increases the risk of death within the first year of life. In fact, excess male infant mortality is around 13 per thousand live births across all specifications. Put differently, the risk of infant death is higher by 1.31 per thousand points for boys compared to girls. Interestingly, we also find that, regardless of the specification, democracy has a highly significant and negative impact on excess male infant mortality. However, the magnitude of this effect slightly varies across specifications. Indeed, compared to column (1), adding controls for country fixed effects (column (2)) and mother fixed effects (column (3)), decreases the point estimate on the interaction term from  $-0.26$  to  $-0.24$  to  $-0.19$  per thousand, respectively. Moreover, compared to column (3), controlling simultaneously for mother fixed effects and for differential trend in infant mortality across countries (column (4)) leave the effect of democracy on excess male infant mortality unchanged.

### 1.5.1.2 Democracy as a Discrete Treatment

In Table 1.3 we report estimates from a regression analysis of the effect of democracy on excess male infant mortality with democracy as a discrete variable. For this analysis we use the dichotomous and trichotomous measures of democracy defined in Section 1.4. Similar to our preferred specification in column (4) of Table 1.2, each columns of Table 1.3 controls for year of birth fixed effects, survey year fixed effects, child characteristics including twin status and birth order, and country-specific trend. Unlike even-numbered columns, odd-numbered columns in Table 1.3 also control for country-specific characteristics known to correlates with democracy.<sup>25</sup> For comparison, columns (1) and (2) reproduce the estimates of columns (4) and (5) of Table 1.2. Estimates for our three discrete measures of democracy are presented in columns (3)-(8).

Overall, our results are strongly consistent regardless of the specification. Using democracy as a discrete treatment, we still find that boys are significantly more likely to die within the first year of life compared to girls, but this disadvantage decreases significantly with the level of democracy. Interestingly, compared to column (1), estimates in column (3), (5), and (7) in Table 1.3 suggest that the effect of democracy on the male survival disadvantage becomes economically larger as the cut-off point for the country to be democratic increases. For example, using the median value of Polity IV score in our sample as the threshold for democratization, column (3) shows that moving from autocracy to democracy reduces excess male infant mortality by 0.17 percentage points, which is about 11% of the average male-female difference in infant mortality estimated from the sample. This effect doubles when we increase the threshold for democratization to +6. Indeed, estimates in column (7), show that moving from autocracy to democracy (with a country being democratic when its Polity IV score is greater than or equal to one) reduces the male disadvantage in infant mortality by 0.31 percentage points, which is about 20% of the average male-female difference in infant mortality.

### 1.5.1.3 Validity of the Identification Strategy: Event Study Approach

As discussed in section 1.4, to validate our identifying assumption, we can examine the dynamic of excess male infant mortality before and after democratization to provide evi-

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<sup>25</sup>We will show below that these additional controls do not change our baseline findings.

dence of no pre-treatment trends. Using equation (2), we estimate the year-wise changes in excess male infant mortality before and after democratization. Results are summarized in Figures 1.4 and 1.3 where we report on the Y- and X-axes the change in excess male infant mortality and the number of years since democratization, respectively. On each of these figures we plot the year-specific estimates of the effect of the change in the political regime and show the 95% level confidence intervals from an estimation of equation (2). The solid line on these figures shows the dynamic of excess male infant mortality over time.<sup>26</sup>

Whereas in Figure 1.3 we use a 2-year interval to define the years before and after democratization, Figure 1.4 uses an interval of one year. The dots in Figure 1.4 are therefore interpreted as changes in excess male infant mortality relative to nondemocratized countries, compared to the five-year period immediately before democratization. Similarly, we interpret the point estimates in Figure 1.3 as changes in excess male infant mortality relative to nondemocratized countries, compared to the nine-year period immediately before democratization. Overall, these figures show that there is no decreasing pre-trend in excess male infant mortality before democratization. Instead, both of these graphs clearly show that the decline in excess male infant mortality starts with democratization, with a statistically significant decline starting 3 years after democracy.

#### 1.5.1.4 Identification Checks

**Additional Controls.** Controlling for mother fixed effects might not be sufficient to ensure that the results described above are totally unbiased. That is, our findings might spuriously attribute the decrease in the male disadvantage in infant mortality to improved democratic institutions. In fact, whereas mother fixed effects implicitly control for time-invariant unobserved mother and country-level characteristics, our empirical specification does not totally account for some of the time-varying correlates of democracy also known to affect child survival. Even if year of birth fixed effects indirectly control for several time-varying correlates of democracy, it is not sufficient to rule out the possibility that other time-varying factors that determine child survival and correlate with democracy might bias our observed effect. As discussed in Section 1.4, we improve from this issue by controlling for two additional determinants of democracy, namely income per capita and

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<sup>26</sup>The regression results can be found in Appendix Tables A1.2 and A1.3.

Official Development Assistance.<sup>27</sup> Income per capita and Official Development Assistance are relevant controls if they foster investment in health inputs that better prevent or treat diseases to which male children are more vulnerable.

In column (5) of Table 1.2 and columns (2), (4), (6), and (8) of Table 1.3, we display results from an estimation of equation (1) where we add controls for the log of GDP per capita and the level of Official Development Assistance. In each of these specifications, we also control for more complex trend in infant mortality at the country level by adding country-specific trends. Compared to our preferred specification, the point estimates from the regressions with controls of covariates of democracy remain highly stable. These additional controls do not change the negative effect of democracy on excess male infant mortality, which further reassures that our findings are not driven by selection on unobserved variables.

**Robustness to Alternative Definitions of Democracy.** In Appendix Table A1.5, we report results from estimating equation (1) using the five alternative measures of democracy discussed in section 1.4. Similar to column (6) in Table each specification in Appendix Table A1.5 we include all the controls: year of birth fixed effects, survey year fixed effects, child characteristics including twin status and birth order, country-specific trend, and covariates of democracy. Whereas in columns (1) and (2), we measure democracy as a continuous treatment using the democratic accountability index from Political Risk Services and the Freedom House index, respectively. In columns (3) and (4) we measure democracy as a dichotomous treatment using indices from Papaioannou and Siourounis (2008) and Boix (2014) respectively. This table shows that our finding that excess male infant mortality decreases with democracy is robust to different definitions of democracy. In column (5), we further examine the sensitivity of our results. We replicate our baseline empirical analysis using both the definition of democracy<sup>28</sup> and sample of children used in Kudamatsu (2012). This additional check do not invalidate our main results. Indeed, the findings are qualitatively similar to those uncovered so far.

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<sup>27</sup>It has been argued that donors generally favor democratized countries. Therefore, in order to receive more foreign aid assistance, countries have more incentives to move toward a democratic regime. This selection into democracy implies that if democratic countries are also countries receiving a larger share of foreign aid assistance, the level of this assistance might be a confounding factor for the effect of democracy on sex differences in infant mortality.

<sup>28</sup>See Section 1.4 for a description of this measure.

## 1.5.2 Regression Discontinuity Design Estimates

In the previous section, we discussed the main findings from our first empirical strategy that uses difference-in-differences approach. In this section, we present estimates of the causal impact of democracy on excess male infant mortality using our second identification strategy, which is the RDD design. Before we present the main regression results from the RDD specification, we start by providing a visual illustration of the effect of democratization on the male disadvantage in infant mortality.

### 1.5.2.1 Graphical Illustration

Figure 1.6 provides a visual illustration of the RD design where the running variable is given by the absolute difference between the child's birth year and the year of democratization in its country of birth. The vertical red line on this graph marks the eligibility year of democratization cut-off. Each gray dot on this graph represents the estimated excess male infant mortality averaged within groups of 2 cohorts. The two-dimensional curve overlaid on the scatter plot is a local polynomial function of the running variable, fitted separately using data from the cohort born before democratization (where the running variable takes on negative values) and the cohort born after democratization (where the running variable takes on positive values). Figure 1.6 reveals a systematic jump at the eligible year cut-off for the outcome of interest. Indeed, we observe an important decrease in excess male infant mortality at time zero (the year of democratization).

### 1.5.2.2 RDD Results

The baseline results from an estimation of equation (3) are summarized in Table 1.4. In each column of this table, we control for child's year of birth fixed effects and mother fixed effects. For inference purposes, standard errors clustered at the country level are reported in parenthesis. Following our empirical strategy, we report estimates both with (Panel A) and without (Panel B) children born within the year before democratization. In the first column we begin by reporting within-mother Ordinary Least Square (OLS) estimates. Consistent with the prediction as well as the results from the difference-in-differences analysis, the OLS estimates show that boys are more likely to die within the

first year of life compared to girls, but this excess male infant mortality is significantly reduced following a transition to democracy.

As mentioned above, the reduced form estimates from the OLS results is likely to be explained by time-varying determinants of child survival that differ across cohort born before and after democratization. To address this concern, we estimate the RDD specification in equation (3) using five different bandwidths. Specifically, we estimate equation (3) on the subsets of cohorts born within 4, 6, 8, 10, and 12 years (in absolute term) from the year of democratization. Results are displayed in columns (2)-(6) of Table 1.4. As opposed to the first column, the specification in these columns include a control for the second-order RD polynomial in the absolute difference between the child's year of birth and the year of democratization. The RDD estimates reveal that excess male infant mortality significantly decreases after democratization. The effect ranges from  $-3.32$  to  $-4.99$ , representing a decline in excess male infant mortality of 23.71 to 32.24% of the average excess male infant mortality. This effect is quite stable even when we exclude from the sample children born within the year before democratization (see Panel B of Table 1.4). Moreover, we find that these results are robust to specifications that include controls of observed factors known to affect child survival (twin status, birth order), correlates of democratization (income and official development assistance), and country-specific trends in infant mortality (see Table A1.4).

## 1.6 Unbundling Democracy: Which Features of Democracy Matter Most?

Democracy is a multifaceted concept and its various dimensions do not necessarily evolve at the same pace over time and across countries. It is, therefore, important to disaggregate this multidimensional concept in order to assess which components are critical for the survival of children. As mentioned in Section 1.3, the Polity IV score is a composite index coding the salient norms of authority patterns within a state. These authority patterns are defined by Eckstein (1973) as the nature of relations among superordinate and subordinate political entities at the national level. Specifically, the Polity IV dataset characterizes political systems using a set of six indicators that are grouped to form variables

describing system along three structural features of the regime authority. The first structural characteristic is the patterns of executive recruitment which is evaluated through three indicators: (1) the regulation of the executive recruitment which assesses the extent to which there are institutionalized procedures for transferring executive power; (2) the degree of competitiveness of executive recruitment; and (3) the openness of executive recruitment. These three indicators describe the process through which social superordinates acquired their positions of political authority (Eckstein (1973)). In a democratic system, this process is generally captured by the existence of regularly scheduled competitive and open elections of the political representatives.

The second authority dimension is the responsiveness of the executive authority, which refers to the extent to which superordinates must consider the preferences of subordinates in decision making. Polity IV measures this authority trait by assessing the extent of institutionalized constraints on the executives' actions. In modern democracies, the chief executive is usually constrained by the legislative and judicial branches of government.

The third authority trait of polities captured by the polity IV score is the pattern of political participation. This structural component of regime authority is evaluated through two operational indicators measuring the extent of regime control of political activity in the political arena. These indicators are: (1) regulation of political participation, which refers to the extent to which there are binding rules on when, whether, and how political preferences are expressed, and (2) competitiveness of political participation, which measures the extent to which alternative preferences for policy and leadership can be freely expressed in the political arena. Combining these two indicators, Polity IV characterizes political activity in a democratic system as having multiple political groups that regularly compete for political influence with no use of coercion. It therefore appears that Polity IV places more emphasis on the extent to which political power is freely contested by political parties or any other organized groups in the political sphere.

The six variables described above are coded using an ordinal system. Each variable is coded on a Likert scale, with the highest value of the scale varying from one variable to another. For each variable, higher values reflect higher quality. We estimate the causal impact of each of those six variables on excess male infant mortality using the same specification as in equation (1), but replacing democracy with each of the variable. Results from these regressions are displayed in Table 1.5. The coefficient on the interaction

term between each of these institutional variables and child sex is negative, implying that these institutions reduce excess male infant mortality. However, features of democracy that matter more include competitiveness of the executive recruitment, constraints on chief executive, regulation and competitiveness of political participation. Regulation and openness of executive recruitment matter less. It is possible that those features of democracy which matter more have a greater effect on the provision of public goods than feature that matter less. These findings are consistent with the view that political accountability, participation, and competitiveness affect the provision of basic public goods. The next section will show that those goods have stronger health benefits for boys, even if their provision is ex-ante gender neutral.

## 1.7 Mechanisms

We have shown that improved democratic institutions significantly reduce the male disadvantage in infant mortality. In this section, we investigate the pathways through which better democratic institutions reduce this male disadvantage. Following our conceptual framework (see Figure 1.1), we first examine whether democracy is associated with more public health goods. Second, we investigate whether these public health goods have stronger health benefits for boys. Third, we consider the possibility that girls are discriminated against in the allocation of health resources as democracy improves. Lastly, using twins, we investigate whether better democratic institutions mitigate biological and preconception influences on the male disadvantage in infant mortality.

### 1.7.1 Democratic Institutions, Health Inputs and the Male Survival Disadvantage

There are many public health interventions that have been shown to be effective in reducing infant mortality.<sup>29</sup> In the analysis below, we will focus on health inputs associated with different dimensions of child health. The first set of indicators is related to child health conditions in utero and includes: (1) maternal tetanus toxoid immunization, which prevents death from neonatal tetanus; (2) prenatal care services necessary to prevent death

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<sup>29</sup>See for example Figure 1 in Jones et al. (2003).

from conditions related to pre-term delivery; and (3) normal birth weight (birth weight  $\geq 2.5\text{kg}$ ), known to increase child survival. The second set of health indicators refer to child health conditions at birth and after birth, and includes: (4) breastfeeding practices, acknowledged to prevent deaths from diarrhea, pneumonia, and neonatal sepsis (Jones et al. (2003)); (5) professional assistance during delivery, acknowledged to increase the survival chance of babies. A third set of indicators refer to health inputs known to improve child health conditions both in utero and after birth and includes: (6) household access to improved water; (7) access to improved sanitation; and (8) maternal education. The latter health inputs have been emphasized in the literature linking parental human capital to child health in early ages (see for instance Breierova and Duflo (2004), Lindeboom et al. (2009), McCrary and Royer (2011), Keats (2018)).

#### 1.7.1.1 Democracy and Health Inputs

We look at the association between democracy and the above-mentioned health inputs. Except for maternal education and access to improved water and sanitation, DHS collect information on these health inputs only for children born within the five years preceding the survey. This restriction prevents us from using DHS data to analyze the causal impact of democracy on these variables that are available only for recent births. In fact, a five-year period is certainly not enough to observe major variation in regime type. Fortunately, country-level data drawn from the World Bank Indicators database allow a cross-country assessment of the association between the quality of democracy and access to several health inputs effective at reducing infant mortality including those considered in this chapter.

Figure 1.7 displays a scatter plot and a regression line showing the relationship between the level of democracy measured by the Polity IV score and the average level of each of the aforementioned health inputs. Graphs (a)-(d) show a positive correlation between polity score and the percentage of children who received a tetanus injection before one year of age; the prevalence of exclusive breastfeeding within the first six months of life; access to prenatal care services; and the percentage of deliveries occurring in hospitals, respectively. Similar correlations are observed with female education, access to water, and access to sanitation (Figure 1.7 Graphs (f), (g), and (h), respectively). Concerning low birth weight, which is a health condition at birth that increases the risk of infant

death, Graph (e) in Figure 1.7 shows a lower percentage of children with low birth weight as Polity IV score increases. Overall, Figure 1.7 suggests that democracy fosters the provision health conditions associated with increased child survival in early ages. This relationship is supported by a number of empirical studies (Lake and Baum (2001), Besley and Kudamatsu (2006), Kudamatsu (2012)).

### 1.7.1.2 Health Inputs and Infant Mortality by Sex

In this section, we estimate the impact of each of the aforementioned health inputs on excess male infant mortality. For this purpose, we estimate the following linear probability model:

$$M_{icmt} = \lambda_0 + \lambda_1 \text{Male}_i + \lambda_2 H_{icmt} + \lambda_3 \text{Male}_i \times H_{icmt} + X'_{imt} \pi + \alpha_c + \theta_t + \varepsilon_{icmt} \quad (3)$$

This equation is similar to our main empirical specification in equation (1), except that the explanatory variable of interest is the interaction between child sex and a health input. In equation (3),  $M_{icmt}$ , is a binary variable equal one if the child  $i$  born to mother  $m$  in country  $c$  at time  $t$  dies within the first year of life.  $\text{Male}_i$  is a binary indicator for whether or not the child is a boy.  $H_{icmt}$  is a dummy variable equal to one if the child  $i$  born to mother  $m$  in country  $c$  at time  $t$  receives the health input.  $X'_{imt}$  is a vector that controls for multiple birth, birth order dummies, and a set of time variant country-specific characteristics. Finally,  $\alpha_c$  and  $\theta_t$  stand for country and year of birth fixed effects, respectively.

Table 1.6 reports results from estimating equation (3) separately for each of the eight health inputs analyzed in this chapter. Two main results emerge from this table. First, each health input has a significant negative impact on infant mortality, implying that these inputs reduce significantly the risk of infant death. Second, the negative coefficient on the interaction term between each health input and the sex of the child shows that each input significantly reduces excess male infant mortality. Health inputs such as maternal education, tetanus immunization, breastfeeding, and birth weight matter more than prenatal visits, professional assistance during delivery, access to improved water and sanitation. Combined with Figure 1.7, results from Table 1.6 indicate that the negative relationship between the level of democracy and the size of excess male infant mortality

is driven by the positive effect of democracy on tetanus immunization, breastfeeding practices, normal birth weight, and maternal education, all of which are associated with stronger benefits for the survival of boys in early ages.

### 1.7.1.3 Are Girls Discriminated Against when Democracy Improves?

In this section, we consider the possibility that discrimination against girls in the allocation of health resources increases with the level of democracy, explaining the negative effect of democracy on excess male infant mortality. As a country moves toward better governance, it is possible that technological progress in the health sector makes it possible for parents to discover the sex of their child before birth. In a context where parents have strong preferences for boys, access to such technology might affect parental decisions in the allocation of prenatal health resources, such as prenatal visits or maternal tetanus injection. If so, improved democratic institutions will increase female mortality, therefore, decreasing the male-female gap in infant mortality.

We empirically investigate this alternative explanation by using an empirical specification where we estimate sex differences in the exposure to health inputs and how these differences are affected by the quality of democracy. Results are presented in Appendix Table A1.7 and illustrated in Figure 1.8. In this figure, we plot the relationship between the level of democracy and the estimated male-female difference in the exposure of the child to each health input considered in this chapter. Overall, Figure 1.8 shows that boys and girls are equally likely to receive each health input, with the exception of birth weight, which is known to be greater for boys. However, as for other health inputs, the male-female difference in birth weight does not change with the level of democracy. These results are consistent with the fact that the interaction term between democracy and child sex on each of these health inputs is zero. These results show that there is no discrimination against girls in the allocation of health resources no matter the quality of democracy.

Jointly, results discussed in this section suggest that improved democratic institutions reduce the male disadvantage in infant mortality by fostering the provision of *ex-ante gender-neutral* health inputs, including tetanus immunization, breastfeeding practices, normal birth weight, and maternal education, all of which are associated with stronger

benefits for the survival of boys.

### 1.7.2 Democracy, Biology, and Preconception Environment

The literature on the causes of sex differences in infant mortality shows that child biology and preconception environmental factors are the key proximate determinants of the male survival disadvantage. Hence, to understand the mechanism underlying the relationship between democracy and the male survival disadvantage in early ages, it is important to assess how democracy affects the contribution of biological and preconception factors to this disadvantage. For this purpose, we follow the twin-based decomposition methodology developed by Pongou (2013) (see also Pongou et al. (2017)) to separate the distinct contributions of biology and preconception environmental factors to excess male infant mortality, and we assess whether improvement in the quality of democratic institutions affects these contributions.

The twin decomposition methodology relies on the assumption that co-twins share the same preconception environment. Therefore, if there are no gender preferences in the allocation of resources that affect child survival, any observed male-female difference in the likelihood of death within the first year of life within an opposite-sex twin pair can be solely attributed to biological differences between the sexes. Specifically, we estimate the separate contribution of biology and the preconception environment to the male survival disadvantage using the following equations:

$$M_{icmt} = \lambda_0 + \lambda_1 \text{Male}_i + X'_{imt} \pi + \sigma_c + \theta_t + \varepsilon_{icmt} \quad (4.1)$$

$$M_{icmt} = \beta_0 + \beta_1 \text{Male}_i + X'_{it} \pi + \alpha_{twin} + \theta_t + \varepsilon_{icmt} \quad (4.2)$$

In equation (4.1), we estimate the effect of the sex of the child on infant mortality, controlling for country fixed effects and other exogenous covariates. The parameter  $\lambda_1$  measures the additive effect of biology and the preconception environment to excess male infant mortality. In equation (4.2), we control for twin fixed effects, therefore only exploiting variation within a pair of opposite-sex twins to estimate the effect of the sex of the child on the risk of infant death. In this latter equation, our coefficient of interest

$\beta_1$  reflects the unique contribution of biological differences between boys and girls to excess male infant mortality. Finally, by taking the difference between the point estimates from these two regressions ( $\lambda_1 - \beta_1$ ), we obtain the contribution of preconception environmental factors to excess male infant mortality.

We restrict our sample to live twin births<sup>30</sup>, and using the three regime categories suggested by Polity IV we divide our sample into three groups: twins born in autocratic, anocratic, and democratic years. We then estimate equations (4.1) and (4.2) in each of these institutional settings. This allows us to quantify the distinct contributions of biology and preconception environmental factors in each institution, and to determine how they are affected by the level of democracy. Results are summarized in Figure 1.9<sup>31</sup>, where the solid line represents the estimated contributions of biological or preconception environmental factors by regime type, and the dotted lines give the 95% confidence interval. Figure 1.9 shows that moving from autocracy to anocracy slightly increases the contribution of biology to excess male infant mortality; however, moving from anocracy to democracy reduces substantially the contribution of biology to this outcome. The contribution of preconception environmental factors decreases substantially from autocracy to anocracy and decreases slightly from anocracy to democracy. It follows from this analysis that democracy significantly constrains preconception and genetic influences on excess male infant mortality.

### 1.7.3 Hormonal Transfer Theory

In the twin analysis above, it is possible that our hypothesized effect of democracy on the influence of biology on excess male infant mortality is in fact explained by the hormonal transfer theory. According to this theory, the testosterone hormone synthesized by a male fetus is likely to be transferred to its cotwin within the womb. It is hypothesized that such a transfer induce a partial masculinization of several aspects of the female co-twin (e.g., [McFadden \(1993\)](#), [Loehlin and Martin \(1998, 2000\)](#), and [Cronqvist et al. \(2016\)](#)), perhaps including the likelihood of death.

If this theory is valid in our context, it is possible that the reduction in the male survival disadvantage that we found previously is at least partially driven by an increased

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<sup>30</sup>Summary statistics for this subsample can be found in Appendix Table [A1.1](#).

<sup>31</sup>Estimated coefficients presented in this figure are available in Appendix Table [A1.7](#).

risk of death among females with a male co-twin following a higher exposure to testosterone in the womb. To show that our results are not driven by the hormonal transfer theory, we estimate the effect of having a male co-twin by comparing the probability of death between a female who shares the womb with a female co-twin, i.e. the control group, and a female who shares the womb with a male co-twin, i.e. the treated group. Results are reported in Table 1.7. Overall, the estimated coefficients show that having a male co-twin does not significantly increase the probability of death of the female, suggesting that differential exposure to the testosterone hormone is less likely to explain sex differences in infant mortality. In addition, this finding is not affected by regime type.

## 1.8 Conclusion

Excess male mortality is largely attributed to genetic fatality. Owing to this perception, this issue has been completely overlooked in the economic literature and in policy circles, and little is known about whether appropriate social interventions can constrain male genetic expression. In this chapter, we provided the first causal evidence on the effect of democracy on the male-female difference in infant mortality. Using individual-level data from sub-Saharan African countries, our main result demonstrates that improvement in the quality of democratic institutions significantly reduces the male survival disadvantage in the first year of life. Moving from autocracy to democracy reduces excess male infant mortality by 20 – 30 percent.

Analyzing the mechanism, we used twins to find that better democratic institutions constrain preconception and genetic influences on the male survival disadvantage. Moreover, we found that improvement in the quality of democratic institutions increases the provision of public health goods such as maternal education, tetanus immunization, breastfeeding practices, and normal birth weight. Despite the fact that these health inputs are shown to *beex-ante gender neutral* in our context, we found them to have stronger benefits to the survival of boys. Taken together, the findings, therefore, suggest that the supply of these health inputs strongly constrain genetic influences on male infant mortality. Importantly, we ruled out the hypothesis that hormonal (testosterone) transfer or discrimination against girls could be driving our findings.

The findings of this study have important implications for the design of policies that

can mitigate excess male mortality. Policies range from strengthening key aspects of democracy, to improving the supply of certain public health goods, to improving maternal education. Interestingly, some of these policies can be implemented in less than perfect democracies.

## 1.9 Figures

Figure 1.1: Conceptual framework

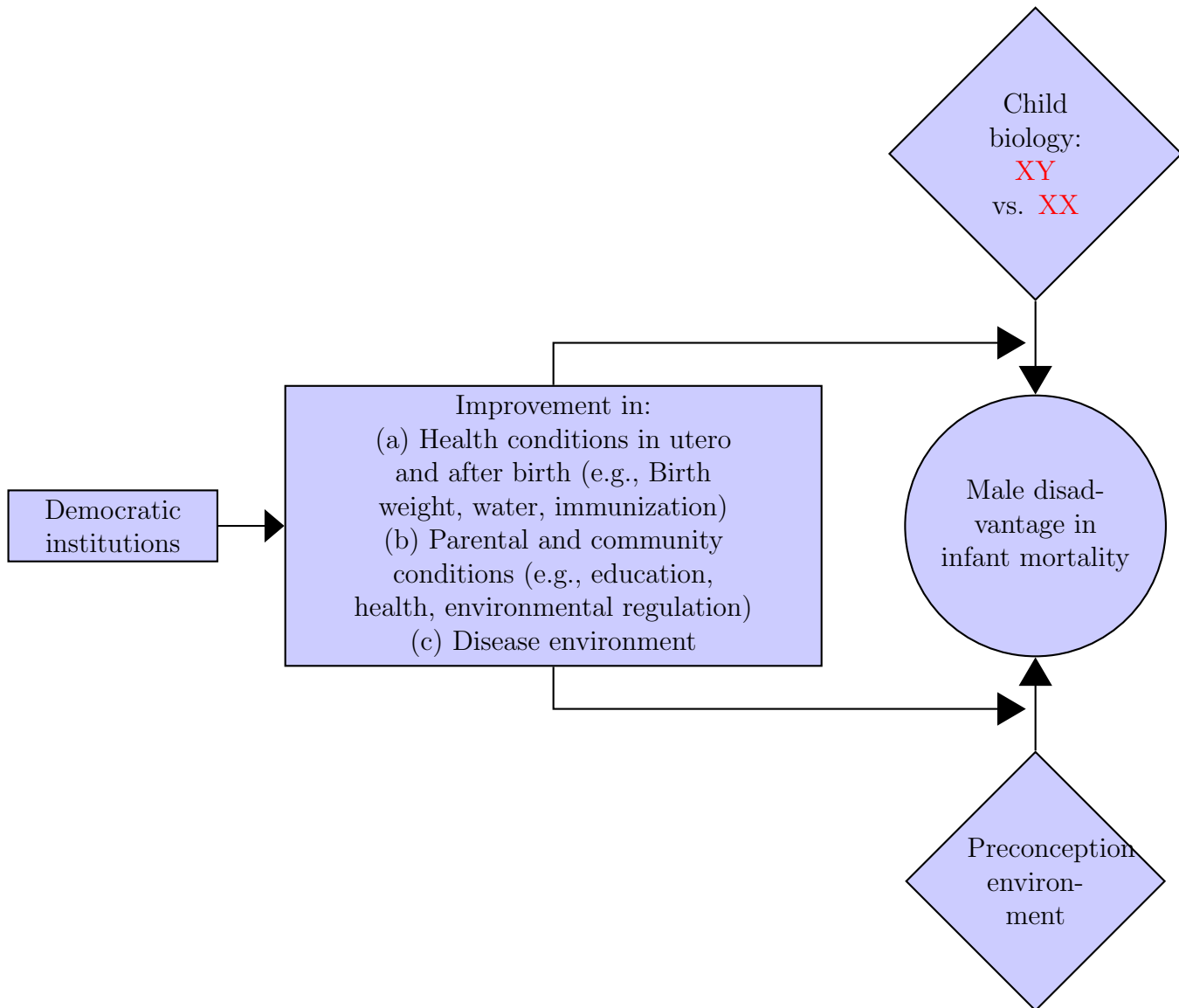
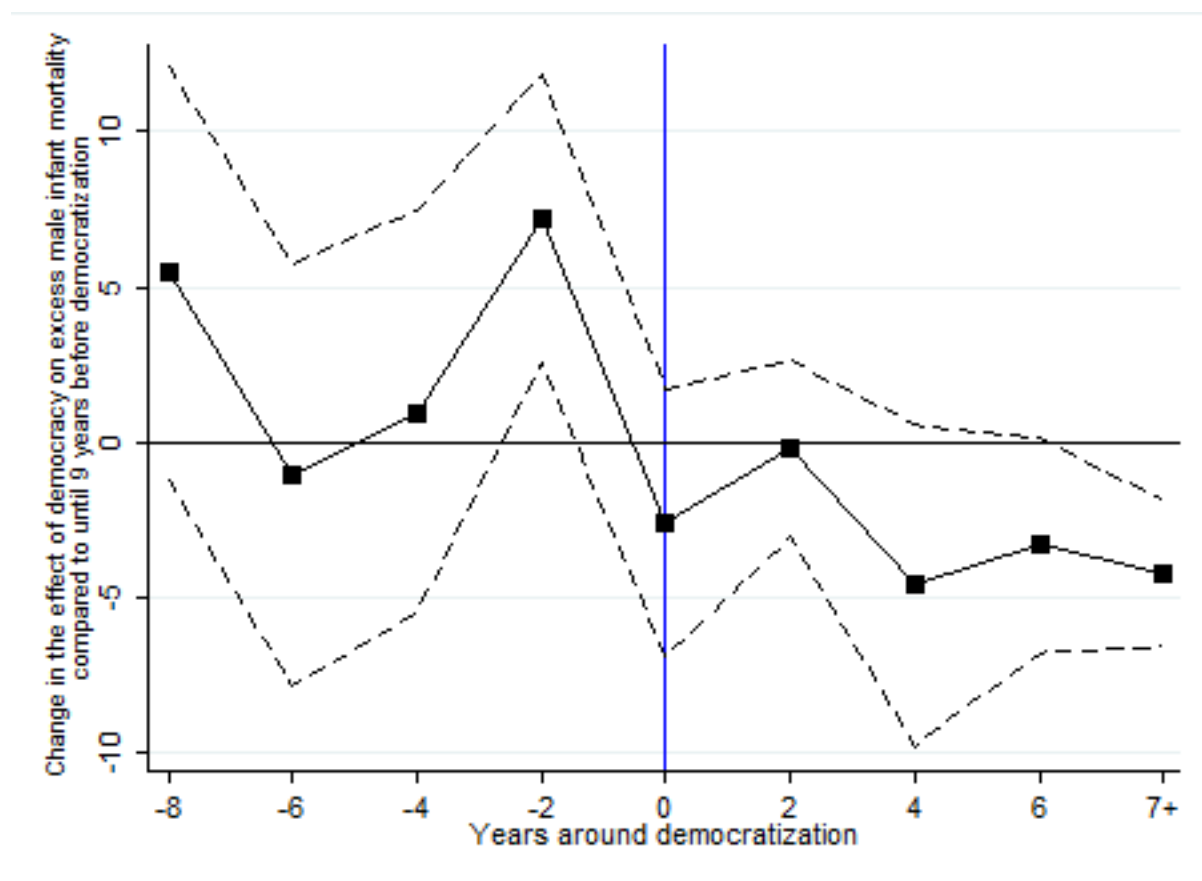


Figure 1.2: Regime type and male-female difference in infant mortality



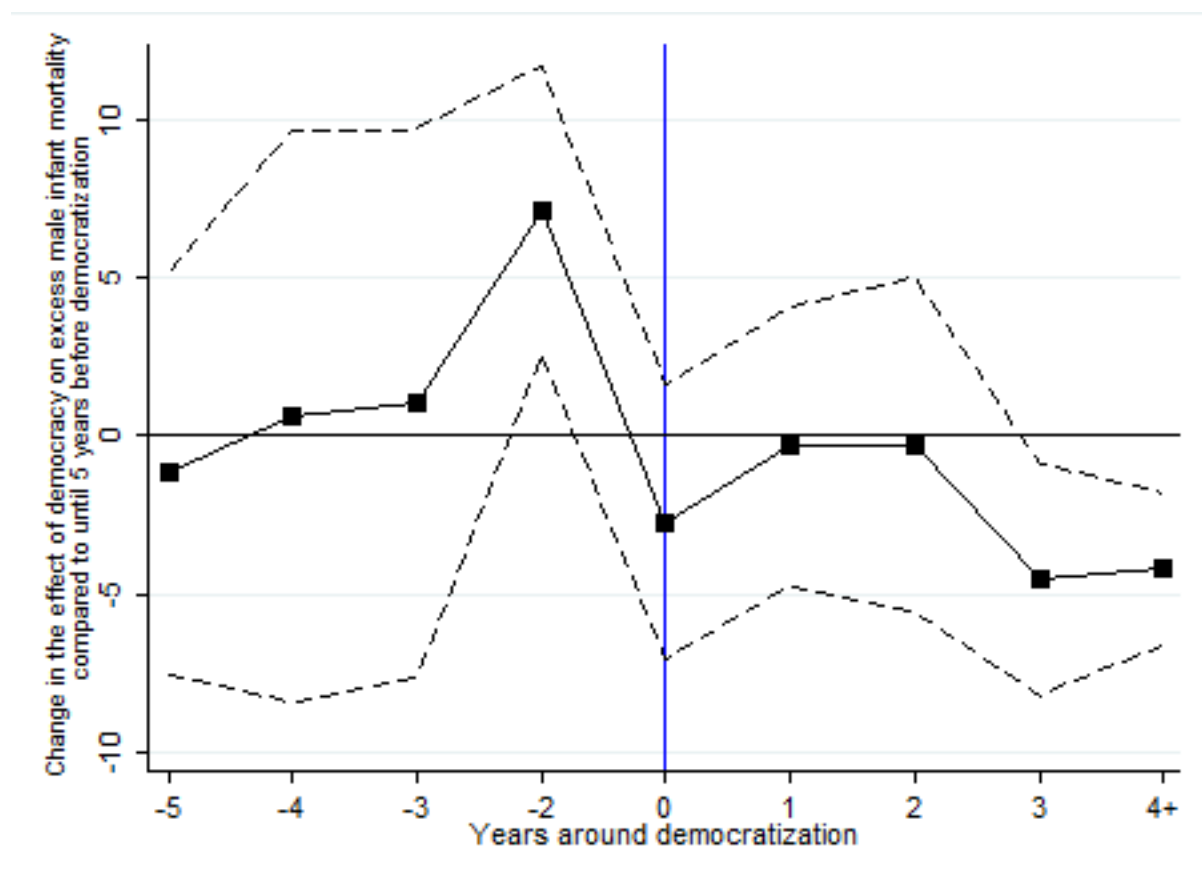
Note: The scatter plot represents the average male-female difference in infant mortality for each Polity IV score. For each regime type, the line passing through the dots is a fitted line representing the average male-female difference in infant mortality.

Figure 1.3: Dynamic of excess male infant mortality (in per thousand) before and after democratization



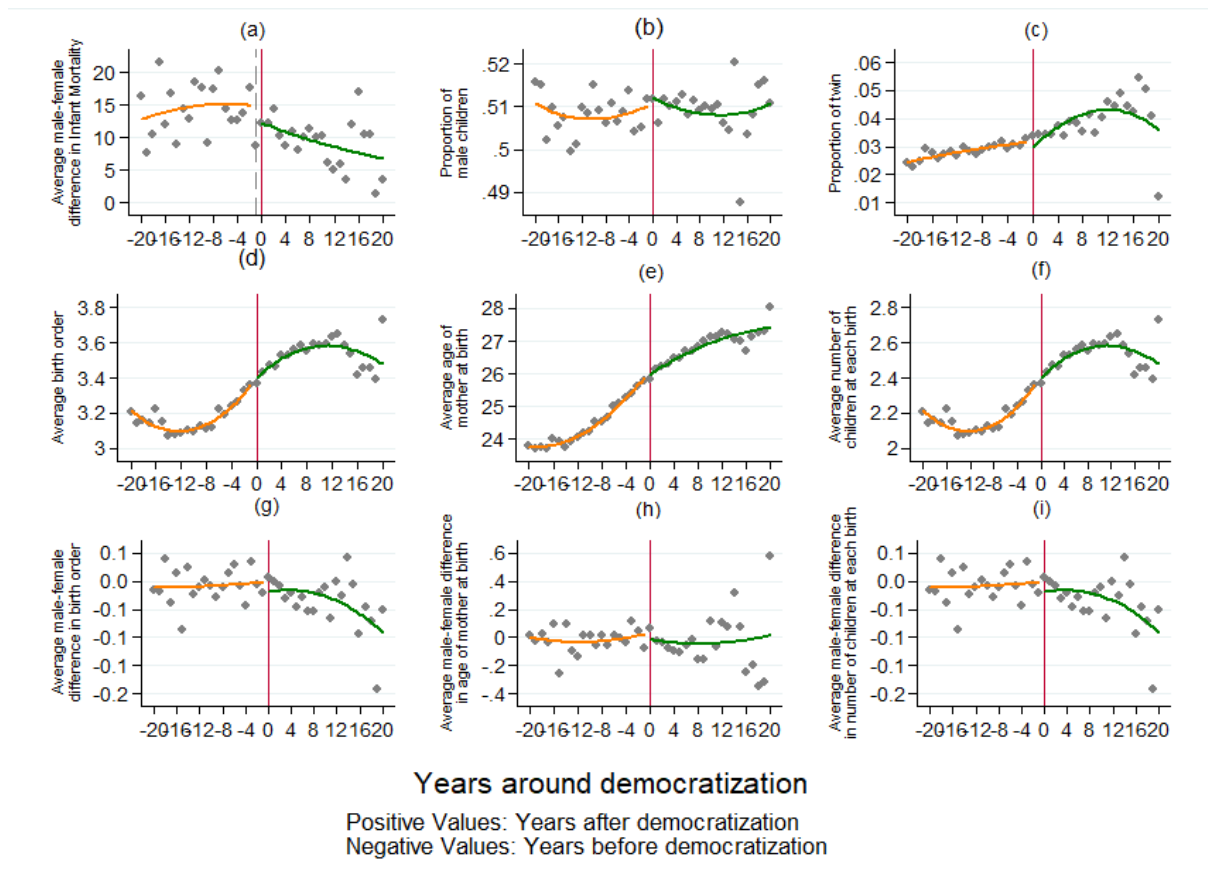
*Note:* The figure plots the trends in excess male infant mortality around democratization. Time (in years) relative to the year of democratization runs on the horizontal axis. We exclude from the sample countries that experienced a collapse of democracy. We also exclude from the sample partially treated children (children born during the year before democratization). Each dot represents an average of the estimated change in excess male infant mortality for observations within a 2-year bandwidth

Figure 1.4: Dynamic of excess male infant mortality (in per thousand) before and after democratization



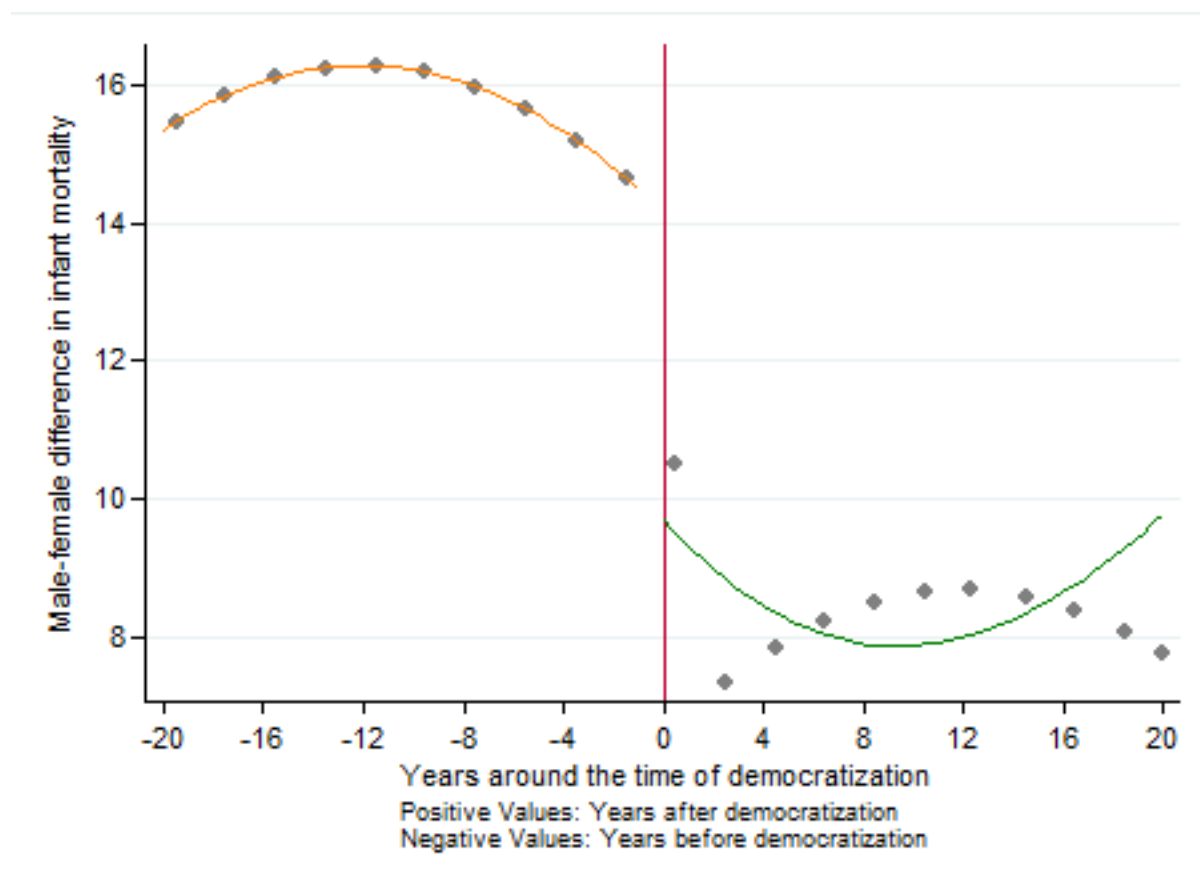
*Note: The figure plots the trends in excess male infant mortality around democratization. Time (in years) relative to the year of democratization runs on the horizontal axis. We exclude from the sample countries that experienced a collapse of democracy. We also exclude from the sample partially treated children (children born during the year before democratization). Each dot represents an average of the estimated change in excess male infant mortality for observations within a 1-year bandwidth*

Figure 1.5: Trends in selected outcomes before and after democratization



*Note:* Each gray dot represents average of the corresponding outcome for a given cohort (birth year). The solid lines over the scatter plot are local polynomials fitted either side of the year of democratization (indicated by the vertical red line). The vertical gray dashed line indicates the year just before democratization.

Figure 1.6: Trend in excess male infant mortality (in per thousand) before and after democratization



Note: Each gray dot represents average of the corresponding outcome for a given cohort (birth year). The solid lines over the scatter plot are local polynomials fitted either side of the year of democratization (indicated by the vertical red line). The vertical gray dashed line indicates the year just before democratization.

Figure 1.7: Level of democracy and public health inputs

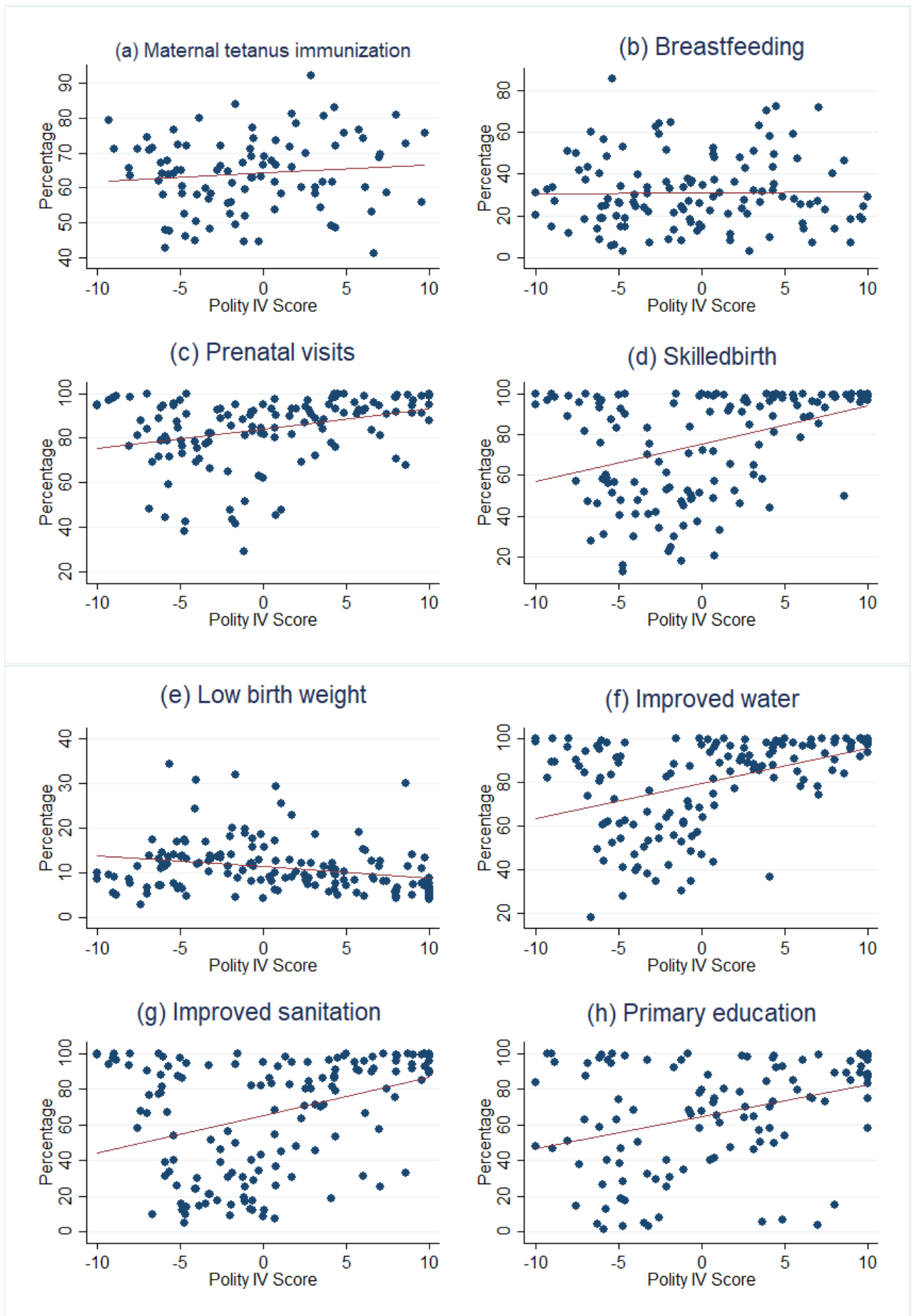


Figure 1.8: Exposure of child to health inputs is gender-neutral

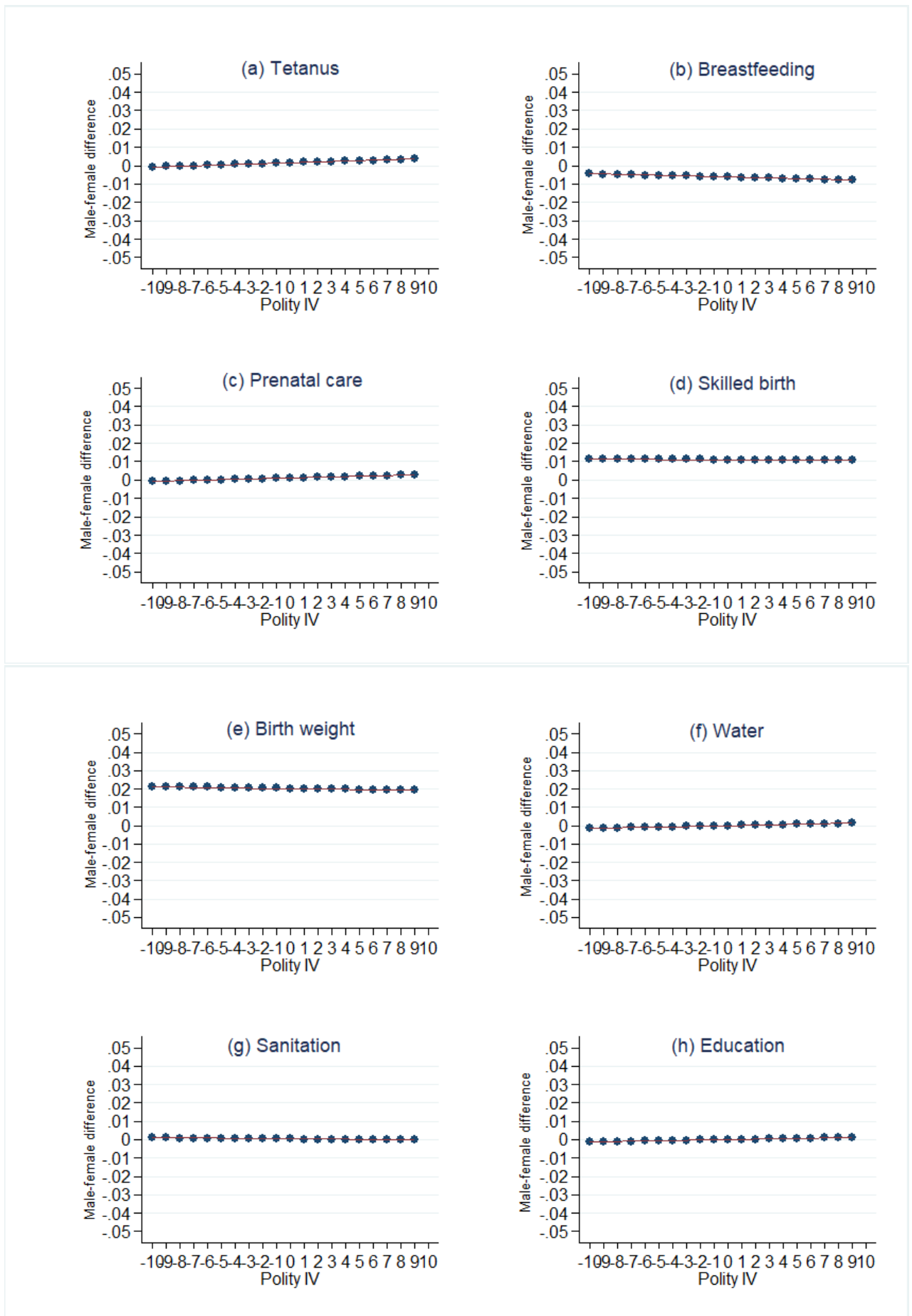
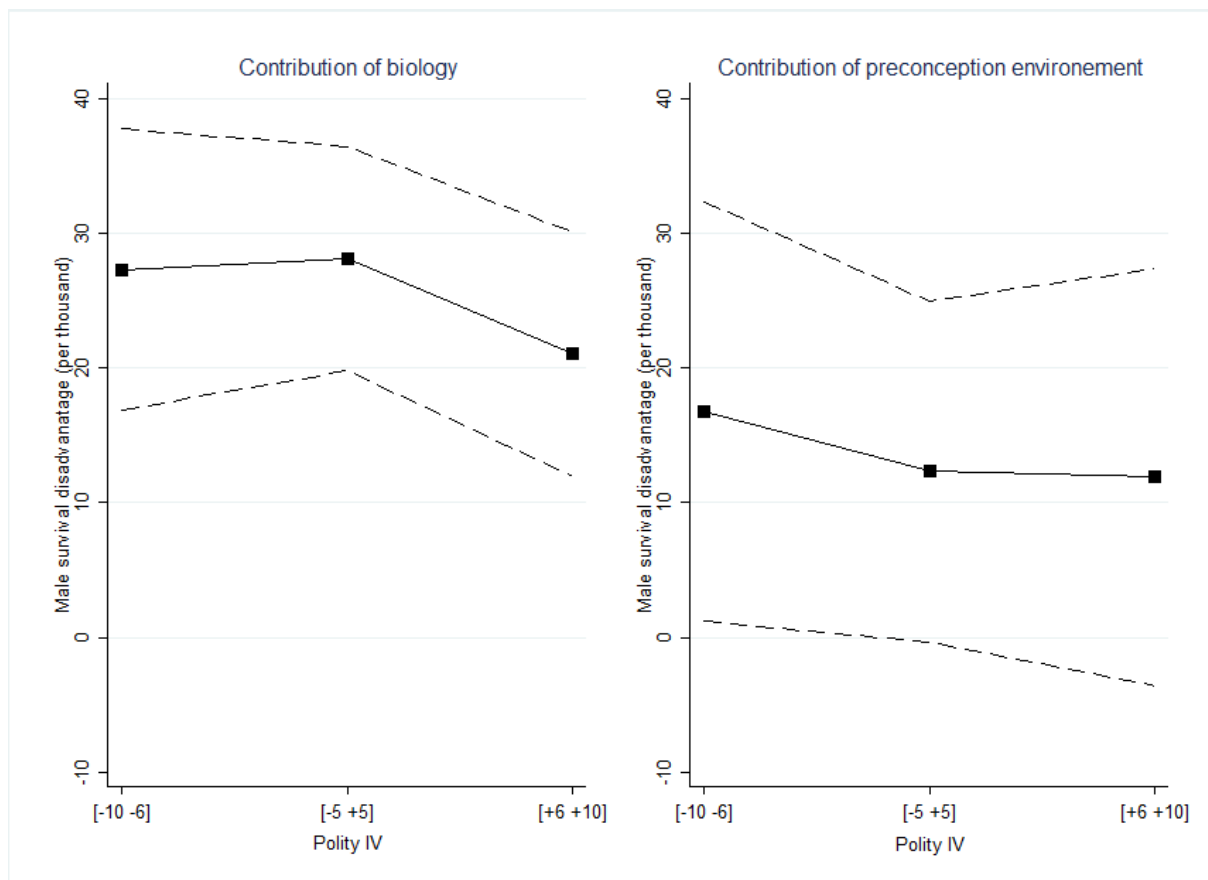


Figure 1.9: Democracy and the distinct contributions of biology and preconception environment to excess male infant mortality



Note: Plotted are estimated coefficients reported in Appendix Table A1.7. Dotted lines represent 95 percent level confidence intervals of the estimated coefficients

## 1.10 Tables

Table 1.1: Descriptive Statistics

	N	Mean	Std. de.	Min.	Max.
<b><i>Child-level characteristics</i></b>					
<i>Infant Mortality (IM in per thousand)</i>					
IM Male	1,932,842	90.10	286.33	0	1000
IM Female	1,871,122	76.60	265.96	0	1000
IM All children	3,803,964	83.46	276.58	0	1000
Child is a boy	3,803,964	0.51	0.50	0	1
Child is a twin	3,803,964	0.03	0.17	0	1
Birth order	3,803,964	3.38	2.28	1	21
Birth year of child	3,803,964	1994.69	10.64	1960	2016
Age mother at delivery	3,803,964	25.11	6.25	15	49
<b><i>Mother-level characteristics</i></b>					
Birth year of mother	980,256	1973.38	11.32	1936	2000
Number of children	980,256	3.88	2.56	1	19
Mother is married	980,256	0.72	0.45	0	1
Mother has no education	980,256	0.45	0.50	0	1
<i>Household wealth index</i>					
Poor	980,256	0.58	0.49	0	1
Urban residence	980,256	0.32	0.47	0	1
<b><i>Country-level characteristics</i></b>					
Number of children per country	1,809	105,066	74,767	7,158	327,261
Polity IV score	1,768	-2.68	5.60	-10	9
GDP per capita (in 2010 US)	1,809	1,230.81	1,796.93	207	10,580
ODA <sup>†</sup> as a percentage of GDP	1,809	0.11	0.07	0	0

Note: <sup>†</sup> Official Development Assistance

Table 1.2: Democracy and the male disadvantage in infant mortality - Difference-in-differences estimates

	(1)	(2)	(3)	(4)	(5)
<b><i>Dependent variable: Risk of infant death (per thousand)</i></b>					
Boy	13.13*** (0.643)	13.05*** (0.635)	13.48*** (0.633)	13.46*** (0.634)	13.48*** (0.607)
Democracy	-0.57* (0.304)	-0.53** (0.222)	-0.39** (0.181)	-0.10 (0.134)	-0.15 (0.109)
Boy × Democracy	-0.26*** (0.059)	-0.24*** (0.060)	-0.19*** (0.064)	-0.19*** (0.063)	-0.18*** (0.061)
Year of birth FE	✓	✓	✓	✓	✓
Year of survey FE	✓	✓	✓	✓	✓
Country FE		✓			
Mother FE			✓	✓	✓
Child Characteristics	✓	✓	✓	✓	✓
Country-specific trend				✓	✓
Country Characteristics					✓
N	3,792,649	3,792,649	3,792,649	3,792,649	3,652,709
Number of cluster	39	39	39	39	39

*Note:* Each column is from a separate regression. We measure the level of democracy by using Polity IV score.

Robust standard errors are in parentheses, adjusted for clustering by country. Child characteristics include an indicator for multiple birth, and a set of birth order dummies. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 1.3: Robustness checks - Difference-in-differences estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Dependent variable: Risk of infant death (per thousand)</i>								
Boy	13.46*** (0.634)	13.48*** (0.607)	14.62*** (0.900)	14.62*** (0.873)	14.39*** (0.778)	14.37*** (0.720)	15.12*** (1.061)	15.14*** (1.043)
Democracy	-0.10 (0.134)	-0.15 (0.109)						
Boy × Democracy	-0.19*** (0.063)	-0.18*** (0.061)						
Demo=Polity>median			-1.25 (1.943)	-1.82 (1.452)				
Boy × Demo=Polity>median			-1.65** (0.681)	-1.65** (0.714)				
Demo=Polity>4					-3.64*** (1.412)	-2.77* (1.442)		
Boy × Demo=Polity>4					-2.71*** (0.891)	-2.62*** (0.844)		
Demo:-5<=Polity<=5							0.08 (2.71)	-0.39 (2.308)
Demo: Polity>=6							-2.61 (2.46)	-2.61 (2.187)
Boy × Demo:-5<=Polity<=5							-1.78* (0.988)	-1.79 (1.087)
Boy × Demo: Polity>=6							-3.05*** (1.091)	-3.01*** (1.052)
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of survey FE	✓	✓	✓	✓	✓	✓	✓	✓
Country FE								
Mother FE	✓	✓	✓	✓	✓	✓	✓	✓
Child Characteristics	✓	✓	✓	✓	✓	✓	✓	✓
Country-specific trend	✓	✓	✓	✓	✓	✓	✓	✓
Country Characteristics		✓		✓		✓		✓
Number of cluster	39	39	39	39	39	39	39	39
N	3,792,649	3,652,709	3,792,649	3,652,709	3,792,649	3,652,709	3,792,649	3,652,709

*Note:* Robust standard errors are in parentheses, adjusted for clustering by country. In column (1) and (2), we define democracy using the continuous value of Polity IV. In column (3) and (4) we use an indicator of democracy equal to 1 if Polity IV is strictly greater than -4 (the median value of Polity IV). In column (5) and (6) we use an indicator of democracy equal to 1 if Polity IV is strictly greater than 4. Column (7) and (8) display estimates from a regression where we define a country as "autocratic", "anocratic" or "democratic" if Polity IV is strictly less than -5, between -5 and +5, and strictly greater than +5, respectively. Child characteristics include an indicator for multiple birth, and a set of birth order dummies. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 1.4: Democracy and the male disadvantage in infant mortality - RDD estimates

	RDD - Bandwidth					
	OLS (1)	<4 years (2)	<6 years (3)	<8 years (4)	<10 years (5)	<12 years (6)
<i>Dependent variable: Risk of infant death (per thousand)</i>						
<i>Panel A: Including partially treated children</i>						
Boy	15.05*** (1.303)	14.00*** (2.226)	14.21*** (1.590)	15.60*** (0.849)	15.35*** (0.688)	15.48*** (0.747)
Democracy	1.54 (5.671)	0.62 (1.484)	-3.02* (1.581)	1.12 (4.314)	1.85 (4.328)	1.23 (4.066)
Boy × Democracy	-4.69** (1.439)	-3.32 (2.684)	-3.55* (1.779)	-4.78** (1.449)	-4.25** (1.343)	-4.99*** (1.092)
Observations	986,431	301,618	425,004	533,693	633,879	713,811
Number of cluster	9	9	9	9	9	9
<i>Panel B: Excluding partially treated children</i>						
Boy	15.20*** (1.365)	14.68*** (1.751)	14.83*** (1.270)	15.97*** (0.550)	15.61*** (0.575)	15.80*** (0.435)
Democracy	1.02 (5.155)	-0.37 (2.008)	-3.71*** (0.833)	0.87 (4.965)	2.46 (5.042)	1.40 (4.053)
Boy × Democracy	-4.77** (1.457)	-3.42 (2.539)	-3.99*** (1.147)	-5.09*** (0.981)	-4.37*** (1.100)	-5.20*** (0.697)
Observations	953,859	269,046	392,432	501,121	601,307	681,239
Number of cluster	9	9	9	9	9	9
Year of birth FE	✓	✓	✓	✓	✓	✓
Country FE						
Mother FE	✓	✓	✓	✓	✓	✓

*Note:* Each column is from a separate regression. We measure the level of democracy by using Polity IV score. Robust standard errors are in parentheses, adjusted for clustering by country. Partially treated children are children born in the year before democratization. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 1.5: Unbundling democracy: features of democracy that matter

Component of democracy is:	Executive Recruitment			Executive Authority	Political Participation	
	Regulation (1)	Competitiveness (2)	Openness (3)	Constraints (4)	Regulation (5)	Competitiveness (6)
<i>Dependent variable: Risk of infant death (per thousand)</i>						
Boy	14.82*** (1.178)	14.30*** (0.693)	14.34*** (1.004)	15.12*** (0.910)	13.52*** (0.635)	14.18*** (0.805)
Component of democracy	1.27 (2.056)	-1.03 (1.259)	0.55 (1.836)	-0.14 (2.121)	-0.02 (1.052)	-1.15 (1.714)
Boy × Component of democracy	-1.54 (1.080)	-2.09*** (0.660)	-1.23 (1.029)	-2.55*** (0.870)	-3.23*** (0.635)	-2.33** (0.947)
Year of birth FE	✓	✓	✓	✓	✓	✓
Year of survey FE	✓	✓	✓	✓	✓	✓
Country FE						
Mother FE	✓	✓	✓	✓	✓	✓
Child Characteristics	✓	✓	✓	✓	✓	✓
Country Characteristics	✓	✓	✓	✓	✓	✓
Country-specific trend	✓	✓	✓	✓	✓	✓
Number of cluster	39	39	39	39	39	39
N	3,435,245	3,435,245	3,435,245	3,435,245	3,435,245	3,435,245

*Note: Each entry is from a separate regression. Robust standard errors are in parentheses, adjusted for clustering by country. Child characteristics include an indicator for multiple birth and a set of birth order dummies. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .*

Table 1.6: Health inputs and their effects on the relative infant mortality of boys

Health input is:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Maternal education	Maternal tetanus immunization	Breastfeeding	Birth weight	Prenatal visits	Skilled birth	Water	Sanitation
	<i>Dependent variable: Risk of infant death (per thousand)</i>							
Boy	12.99*** (0.65)	10.22*** (0.94)	57.38*** (6.44)	18.68*** (2.03)	8.32*** (1.93)	10.94*** (0.84)	12.76*** (0.61)	12.68*** (0.70)
health input	-25.55*** (1.83)	-18.03*** (2.35)	-423.68*** (25.09)	-23.58*** (3.46)	-25.20*** (4.19)	-12.65*** (1.80)	-11.26*** (1.59)	-17.68*** (2.34)
Boy × health input	-2.75*** (0.66)	-4.71*** (1.00)	-53.90*** (6.48)	-11.80*** (1.93)	-2.07 (2.07)	-2.04 (1.27)	-0.98 (0.64)	-1.21 (0.90)
Country FE	✓	✓	✓	✓	✓	✓	✓	✓
Country characteristics	✓	✓	✓	✓	✓	✓	✓	✓
Number of cluster	38	40	40	38	38	40	40	40
N	3,389,919	527,290	785,643	393,217	494,894	673,741	3,482,102	3,493,075

*Note:* Each entry is from a separate OLS regression. Robust standard errors are in parentheses, adjusted for clustering by country. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

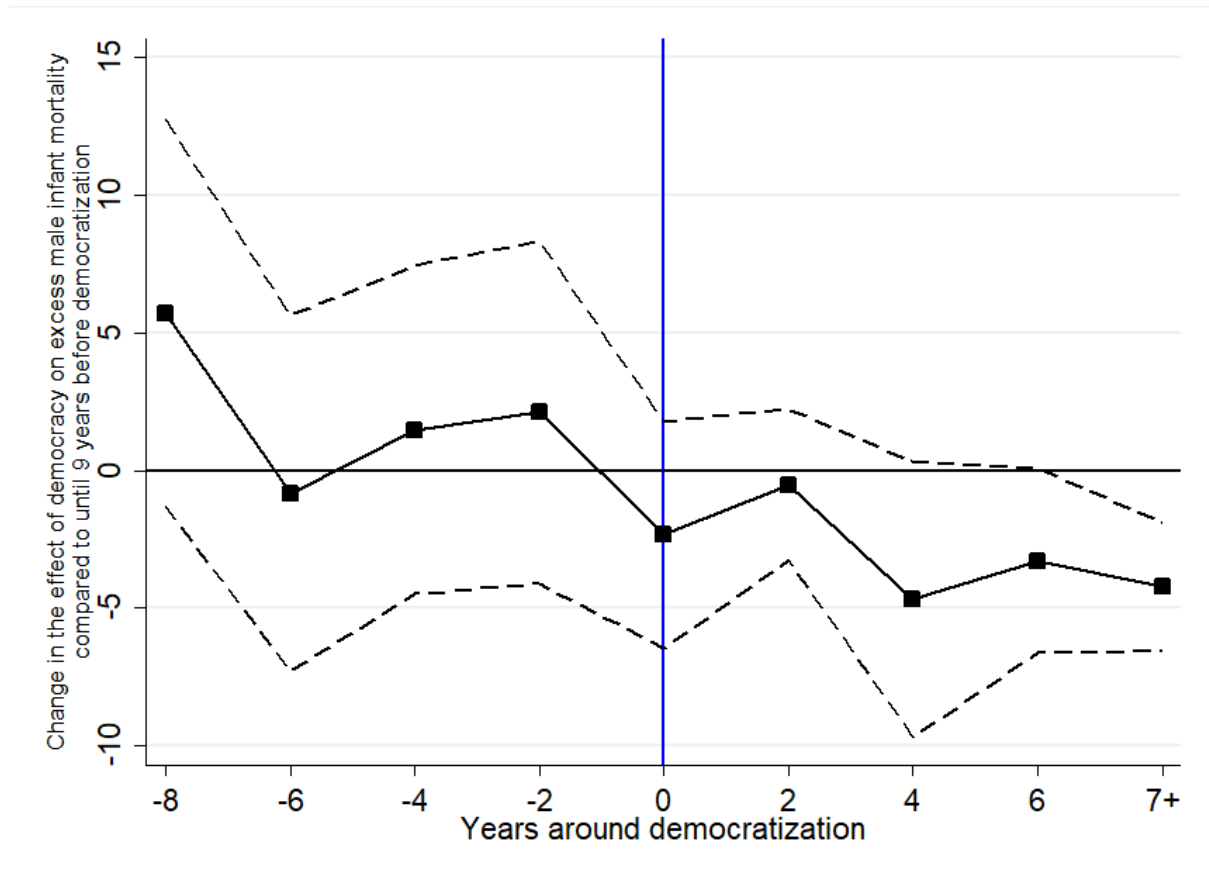
Table 1.7: Testing the Hormonal Transfer Theory in each regime type

	Whole sample (1)	Autocracy (2)	Anocracy (3)	Democracy (4)
<b><i>Dependent variable: Risk of infant death (per thousand)</i></b>				
Female with a Male co-twin (vs. Female with a Female co-twin)	4.65 (3.459)	3.03 (10.258)	5.84 (6.472)	6.66 (4.931)
Year of birth FE	✓	✓	✓	✓
Year of survey FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Number of cluster	40	25	27	22
N	57,813	12,904	16,141	12,707

*Note:* Each entry is from a separate regression. Robust standard errors are in parentheses, adjusted for clustering by country. Controls include the age of mother at delivery, the square of the age of mother at delivery, the mothers's number of children at the time of delivery, the level of education of the mother, the mother's marital status, and a set of birth order dummies. We define a country as "autocratic", "anocratic" or "democratic" if Polity IV is strictly less than -5, between -5 and 5 or strictly greater than 5, respectively. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

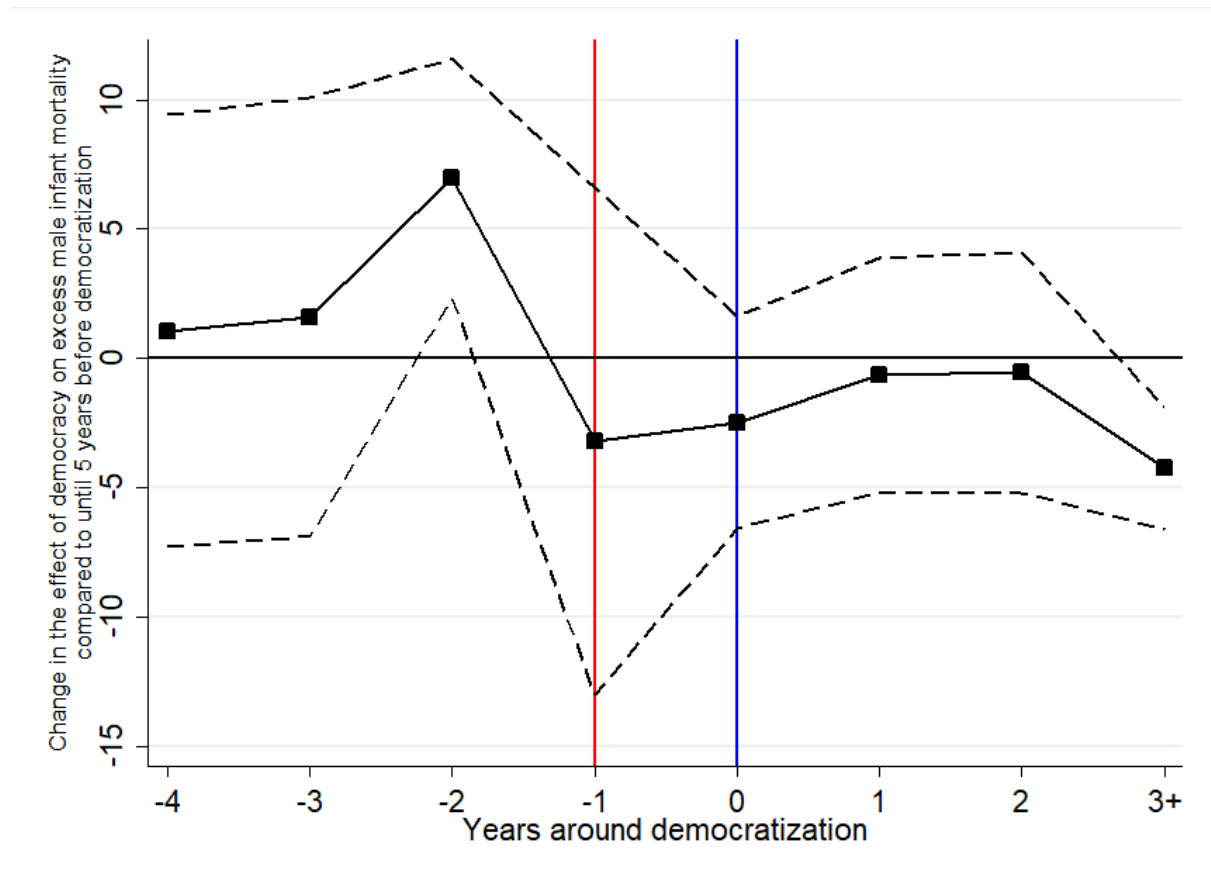
## 1.11 Appendix

Figure A1.1: Dynamic of excess male infant mortality (in per thousand) before and after democratization



*Note:* The figure plots the trends in excess male infant mortality around democratization. Time (in years) relative to the year of democratization runs on the horizontal axis. We exclude from the sample countries that experienced a collapse of democracy. We consider all children including partially treated children (children born during the year before democratization). Each dot represents an average of the estimated change in excess male infant mortality for observations within a 2-year bandwidth.

Figure A1.2: Dynamic of excess male infant mortality (in per thousand) before and after democratization



*Note:* The figure plots the trends in excess male infant mortality around democratization. Time (in years) relative to the year of democratization runs on the horizontal axis. We exclude from the sample countries that experienced a collapse of democracy. We consider all children including partially treated children (children born during the year before democratization). Each dot represents an average of the estimated change in excess male infant mortality for observations within a 1-year bandwidth.

Table A1.1: Descriptive Statistics (Twin sample)

	N	Mean	Std. de.	Min.	Max.
<b><i>Child-level characteristics</i></b>					
<i>Infant Mortality (IM per thousand)</i>					
IM Male	60,613	289.15	453.37	0	1000
IM Female	59,541	247.90	431.79	0	1000
IM All children	120,154	268.71	443.29	0	1000
Child is a boy	120,154	0.50	0.50	0	1
Birth order	120,154	4.55	2.45	1	18
Birth year of child	120,154	1995.74	10.26	1960	2016
<b><i>Mother-level characteristics</i></b>					
Age mother at delivery	120,154	27.07	6.22	15	48
Birth year of mother	54,535	1968.93	10.69	1936	1998
Number of children	54,535	2.20	0.69	2	10
Mother is married	54,535	0.77	0.42	0	1
Mother has no education	54,535	0.51	0.50	0	1
<i>Household wealth index</i>					
Poor	54,535	0.60	0.49	0	1
Urban residence	54,535	0.29	0.45	0	1

Table A1.2: Dynamic of excess male infant mortality before and after democratization

	Including children born the year before democratization (1)	Excluding children born the year before democratization (2)
Boy	13.867*** (0.868)	13.859*** (0.884)
5 years before × Boy		-1.178 (3.082)
4 years before × Boy	1.069 (4.050)	0.635 (4.377)
3 years before × Boy	1.608 (4.117)	1.024 (4.203)
2 years before × Boy	6.958*** (2.247)	7.123*** (2.214)
1 year before × Boy	-3.209 (4.761)	
Year of democratization × Boy	-2.464 (1.990)	-2.718 (2.098)
1 year after × Boy	-0.651 (2.205)	-2.145 (4.823)
2 years after × Boy	-0.552 (2.252)	-0.266 (2.563)
3 years after × Boy		-4.512** (1.780)
3+ years after × Boy	-4.249*** (1.142)	
4+ years after × Boy		-4.181*** (1.164)
Year of birth FE	✓	✓
Year of survey FE	✓	✓
Country FE		
Mother FE	✓	✓
Child Characteristics	✓	✓
Country Characteristics	✓	✓
Country-specific trend	✓	✓
Observations	3,011,906	2,979,334

*Note:* Robust standard errors are in parentheses, adjusted for clustering by country. Child characteristics include an indicator for multiple birth, and a set of birth order dummies. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A1.3: Dynamic of excess male infant mortality before and after democratization

	Including partially treated children (1)	Excluding partially treated children (2)
Boy	13.762*** (0.899)	13.753*** (0.898)
7-8 years before × Boy	5.730 (3.410)	5.461 (3.230)
5-6 years before × Boy	-0.835 (3.140)	-1.048 (3.297)
3-4 years before × Boy	1.477 (2.900)	0.964 (3.151)
1-2 year before × Boy	2.102 (3.021)	
Year of democratization × Boy	-2.345 (2.001)	-2.605 (2.090)
1-2 year after × Boy	-0.516 (1.338)	-0.200 (1.382)
3-4 years after × Boy	-4.689* (2.429)	-4.597* (2.508)
5-6 years after × Boy	-3.283* (1.632)	-3.319* (1.674)
7+ years after × Boy	-4.240*** (1.132)	-4.204*** (1.150)
Year of birth FE	✓	✓
Year of survey FE	✓	✓
Country FE		
Mother FE	✓	✓
Child Characteristics	✓	✓
Country Characteristics	✓	✓
Country-specific trend	✓	✓
Observations	3,011,906	2,979,334

*Note:* Robust standard errors are in parentheses, adjusted for clustering by country. Child characteristics include an indicator for multiple birth, and a set of birth order dummies. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. Partially treated children are children born in the year before democratization. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A1.4: Robustness Checks: Democracy and the male disadvantage in infant mortality - RDD estimates

	RDD - Bandwidth					
	OLS (1)	<4 years (2)	<6 years (3)	<8 years (4)	<10 years (5)	<12 years (6)
<b>Dependent variable: Risk of infant death (per thousand)</b>						
<b>Panel A: Including partially treated children</b>						
Boy	15.02*** (1.316)	14.15*** (2.247)	14.41*** (1.601)	15.69*** (0.871)	15.36*** (0.685)	15.46*** (0.794)
Democracy	-1.38 (3.415)	2.21 (2.635)	-1.76 (1.850)	-1.00 (2.355)	-1.81 (2.262)	-2.15 (2.272)
Boy × Democracy	-4.45** (1.371)	-3.31 (2.657)	-3.50* (1.645)	-4.68** (1.490)	-4.02** (1.433)	-4.80*** (1.162)
Observations	967,896	296,262	417,753	524,380	622,757	701,073
Number of cluster	9	9	9	9	9	9
<b>Panel B: Excluding partially treated children</b>						
Boy	15.10*** (1.376)	14.59*** (1.743)	14.92*** (1.248)	15.93*** (0.533)	15.50*** (0.491)	15.68*** (0.460)
Democracy	-2.33 (2.962)	1.91 (3.348)	-2.51 (1.470)	-1.51 (2.802)	-2.59 (2.819)	-2.63 (1.997)
Boy × Democracy	-4.45** (1.369)	-3.12 (2.572)	-3.80*** (1.026)	-4.86*** (1.034)	-4.03*** (1.167)	-4.89*** (0.771)
Observations	936,463	264,829	386,320	492,947	591,324	669,640
Number of cluster	9	9	9	9	9	9
Year of birth FE	✓	✓	✓	✓	✓	✓
Country FE						
Mother FE	✓	✓	✓	✓	✓	✓
Child Characteristics	✓	✓	✓	✓	✓	✓
Country Characteristics	✓	✓	✓	✓	✓	✓
Country-specific trend	✓	✓	✓	✓	✓	✓

*Note:* Each column is from a separate regression. We measure the level of democracy by using Polity IV score. Robust standard errors are in parentheses, adjusted for clustering by country. Child characteristics include an indicator for multiple birth, and a set of birth order dummies. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. Partially treated children are children born in the year before democratization. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A1.5: Robustness to alternative measures of democracy

	(1)	(2)	(3)	(4)	(5)
Democracy measure is:	PRS	FH	PS	BMR	KDM
Male	14.79*** (1.239)	14.44*** (0.779)	14.54*** (0.730)	14.27*** (0.727)	14.79*** (1.198)
Democracy measure	-0.37 (0.353)	0.51 (0.832)	-1.45 (1.409)	-2.64* (1.470)	-9.41* (4.483)
Male × Democracy measure	-0.52 (0.320)	-1.25* (0.707)	-2.02* (1.078)	-1.97** (0.918)	-4.78* (2.752)
Year of birth FE	✓	✓	✓	✓	✓
Year of survey FE	✓	✓	✓	✓	✓
Country FE					
Mother FE	✓	✓	✓	✓	✓
Child characteristics	✓	✓	✓	✓	✓
Country characteristics	✓	✓	✓	✓	✓
Country-specific trend	✓	✓	✓	✓	✓
N	2,637,880	3,592,352	2,745,493	3,490,216	643,846

*Note:* Each entry is from a separate regression. Robust standard errors are in parentheses, adjusted for clustering by country. Child characteristics include an indicator for multiple birth, and a set of birth order dummies. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. In columns (1)-(5) we measure democracy using: the democratic accountability index defined by Political Risk Services (PRS), the Freedom House index (FH), an index of political regimes and transition constructed by *Papaioannou and Siourounis (2008)* (PS), a dichotomous index from *Boix (2014)*, and a binary indicator of democracy constructed by *Kudamatsu (2012)* (KDM), respectively. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A1.6: The effect of democracy on public health inputs is gender-neutral

<i>Dependent Variable is:</i>	Maternal education (1)	Tetanus immunization (2)	Breastfeeding (3)	Birth weight (4)	Prenatal visits (5)	Skilled birth (6)	Water (7)	Sanitation (8)
Boy	0.000 (0.001)	0.002 (0.001)	-0.006*** (0.001)	0.020*** (0.001)	0.001 (0.001)	0.011*** (0.002)	0.000 (0.000)	0.000 (0.000)
Democracy	0.003*** (0.001)	0.005* (0.003)	-0.000 (0.001)	-0.001 (0.001)	0.003 (0.002)	0.005 (0.003)	-0.003* (0.002)	0.004** (0.002)
Boy × Democracy	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Country FE	✓	✓	✓	✓	✓	✓	✓	✓
Country characteristics	✓	✓	✓	✓	✓	✓	✓	✓
N	3,386,690	526,331	784,268	392,024	493,943	672,347	3,477,838	3,488,816
Sample mean	0.244	0.729	0.941	0.908	0.842	0.615	0.317	0.200

*Note:* Each entry is from a separate regression. Robust standard errors are in parentheses, adjusted for clustering by country. Country characteristics include the log of GDP per capita in 2010 US and the level of Official Development Assistance as a percentage of GDP. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A1.7: Democracy and the contribution of biology and preconception factors to excess male infant mortality

	Total male effect (1)	Biological effect (2)	Preconception effect (3)=(1)-(2)
<b><i>Panel A: Autocracy</i></b>			
Male	44.04*** (5.889)	27.30*** (5.324)	16.74 *** (7.938)
N	36,718	36,718	
Number of cluster	36	36	
<b><i>Panel B: Anocracy</i></b>			
Male	40.40*** (4.867)	28.10*** (4.245)	12.30 ** (6.458)
N	54,446	54,446	
Number of cluster	35	35	
<b><i>Panel C: Democracy</i></b>			
Male	32.90*** (6.393)	21.01*** (4.620)	11.89 (7.887)
N	28,614	28,614	
Number of cluster	21	21	
Year of birth FE	✓	✓	
Year of survey FE	✓	✓	
Country FE	✓		
Twin FE		✓	
Birth Order	✓	✓	

*Note:* Each entry is from a separate regression. We restrict the sample to twin births only. Robust standard errors are in parentheses, adjusted for clustering by country. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A1.8: List of countries and DHS surveys

Countries	DHS year of surveys
Angola	2011, 2015-16
Benin	1996, 2001, 2006, 2011-12
Botswana	1988
Burkina Faso	1992-93, 1998-99, 2003, 2010
Burundi	1987, 2010-11, 2016-17
Cameroon	1991, 1998, 2004, 2011
Central African Republic	1994-95
Chad	1996-97, 2004, 2014-15
Comoros	1996, 2012
Congo	2005, 2011, 2012
Cote d'Ivoire	1994, 1998-99, 2005, 2011-12
Ethiopia	2000, 2005, 2011, 2016
Gabon	2000-01, 2012
Gambia	2013
Ghana	1988, 1993-94, 1998-99, 2003, 2008, 2014
Guinea	1999, 2005, 2012
Kenya	1988-89, 1993, 1998, 2003, 2008-09, 2014
Lesotho	2004-05, 2009-10, 2014
Liberia	1986, 2006-09, 2013
Madagascar	1992, 1997, 2003-04, 2008-09
Malawi	1992, 2000, 2004-05, 2010, 2015-16
Mali	1987, 1995-96, 2001, 2006, 2012-13
Mauritania	2000-01
Mozambique	1997, 2003-04, 2011, 2015
Namibia	1992, 2000, 2006-07, 2013
Niger	1992, 1998, 2006, 2012
Nigeria	1986-87, 1990, 2003, 2008, 2010, 2013
Rwanda	1992, 2000, 2005, 2007-08, 2010-11, 2014-15
Sao Tome and Principe	2008-09
Senegal	1986, 1992-93, 1997, 2005, 2008-16
Sierra Leone	2008, 2013
South Africa	1998
Sudan	1989, 1990
Swaziland	2006-07
Tanzania	1991-92, 1996, 1999, 2004-05, 2007-12, 2015-16
Togo	1988, 1998, 2013, 2014
Uganda	1988-89, 1995, 2000-01, 2006, 2009-11 2016
Zaire	2007, 2013, 2014
Zambia	1992, 1996-97, 2001-02, 2007, 2013-14
Zimbabwe	1988-89, 1994, 1999, 2005, 2006, 2010-11, 2015

Table A1.9: Bootstrap: Democracy and the male disadvantage in infant mortality - RDD estimates

	RDD - Bandwidth					
	OLS (1)	<4 years (2)	<6 years (3)	<8 years (4)	<10 years (5)	<12 years (6)
<i>Dependent variable: Risk of infant death (per thousand)</i>						
<b>Panel A: Including partially treated children</b>						
Boy	15.05*** (1.303)	14.00*** (2.226)	14.21*** (1.590)	15.60*** (0.849)	15.35*** (0.688)	15.48*** (0.747)
Democracy	1.54 (5.671)	0.62 (1.484)	-3.02* (1.581)	1.12 (4.314)	1.85 (4.328)	1.23 (4.066)
Boy × Democracy	-4.69** (1.439)	-3.32 (2.684)	-3.55* (1.779)	-4.78** (1.449)	-4.25** (1.343)	-4.99*** (1.092)
Observations	986,431	301,618	425,004	533,693	633,879	713,811
Number of cluster	9	9	9	9	9	9
Bootstrap: WCR p-value by country	0.063	0.324	0.219	0.023	0.016	0.008
<b>Panel B: Excluding partially treated children</b>						
Boy	15.20*** (1.365)	14.68*** (1.751)	14.83*** (1.270)	15.97*** (0.550)	15.61*** (0.575)	15.80*** (0.435)
Democracy	1.02 (5.155)	-0.37 (2.008)	-3.71*** (0.833)	0.87 (4.965)	2.46 (5.042)	1.40 (4.053)
Boy × Democracy	-4.77** (1.457)	-3.42 (2.539)	-3.99*** (1.147)	-5.09*** (0.981)	-4.37*** (1.100)	-5.20*** (0.697)
Observations	953,859	269,046	392,432	501,121	601,307	681,239
Number of cluster	9	9	9	9	9	9
Bootstrap: WCR p-value by country	0.066	0.227	0.063	0.004	0.004	0.004
Year of birth FE	✓	✓	✓	✓	✓	✓
Country FE						
Mother FE	✓	✓	✓	✓	✓	✓

*Note:* Each column is from a separate regression. We measure the level of democracy by using Polity IV score. Robust standard errors are in parentheses, adjusted for clustering by country. Partially treated children are children born in the year before democratization. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ . Following Roodman et al. (2019) and Nielsen et al. (2020), the Bootstrap uses Wild Cluster bootstrap symmetric P values with clustering at the country level. We set the number of replications to 9999.

# Chapter 2

## Colonial Origins and Fertility: Can the Market Overcome History?

### 2.1 Introduction

Over the past two centuries, the world experienced a gradual breakout from a long-standing Malthusian trap, in which income growth was offset by population growth. Economic growth theory explaining this remarkable escape has emphasized the essential role of human capital in lowering fertility and inducing a transition from stagnation to sustained economic growth (Galor (2011)). At the global level, fertility has declined significantly since the 1960s, from five children per woman to fewer than half of this number in 2016 (The World Bank (2016)). However, in the developing world where fertility has also more than halved over the past half century, there is significant variation across countries in the magnitude and the timing of this decline, with fertility remaining very high in Africa, in spite of an impressive rise in female education and a steep decline in mortality rates. This latter variation has been attributed to initial differences in geographical factors, historical accidents, cultural factors, and institutional endowments (Galor (2011)). Yet, there has been no empirical exploration of the link between historical political institutions and reproductive behavior, especially the persistence of contexts of high fertility rates in Africa. What is more, we know very little about whether policy interventions can mitigate the long-term impact of history.

In this paper, we address this important gap by studying the causal effect of colonial

origins on fertility in sub-Saharan Africa and documenting the nature of the mechanisms governing this effect. Central to our study is the analysis of important heterogeneity in this effect by market access, where by market access, we mean proximity to the sea. We show that proximity to the sea is inked to increased female labor force participation, which increases the opportunity cost of childbearing. If the market effect dominates the colonial effect on fertility, one should expect the latter effect to be smaller in areas with higher market access. This analysis directly addresses a concern raised in policy circles about the importance of the research agenda documenting the long-term economic impact of historical events for policymaking. The argument usually advanced is that, since history cannot be changed, such research is unlikely to inspire the design of policies.<sup>1</sup> Departing from this paradigm, our analysis implies that economic incentives can break the bonds of historical determinism. Indeed, contrasting the two major colonial powers in Africa, namely the British and the French, we document significant average effects of colonial origins on fertility behavior and local economic development; however, we find that these effects completely disappear in areas with high market access, showing that exogenous access to economic opportunities is able to mitigate the long-term consequences of differential colonial legacies.

In our conceptual framework, differences in fertility behavior across African countries can be *directly* linked to differences in colonial population policies. In 1920, France adopted a pronatalist policy and legal framework, in part due to worries about its population size relative to its European rivals, and these policies were subsequently extended to its colonies. The laws prohibited any propaganda on contraceptive use or directed against having children, and severely repressed abortion ([Latham \(2002\)](#), [Garenne \(2017\)](#)). While these laws were revoked in France in 1967, they remained in effect in all former French colonies in Africa. The French pronatalist laws contrasted with the more liberal culture of reproductive rights in Great Britain in this period ([Oliver \(1995\)](#), [Caldwell and Sai \(2007\)](#), [Beach and Hanlon \(2019\)](#)).

British colonial population policies went through several phases. In the 1920s these policies were pronatalist, due to high death rates and a desire for a large native work force. However, in the 1930s the British Eugenics movement lobbied for population control in

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<sup>1</sup>See [Banerjee and Duflo \(2014\)](#) for a review of the literature contrasting the different views on the extent to which history is ultimately deterministic.

the colonies, and some small scale family planning initiatives were begun, mainly in Asia and the Caribbean. A major shift occurred with World War 2. Colonial policy, which historically allowed a great deal of autonomy to local administrators, was centralized in the Colonial office in London, to aid the war effort. In 1941 a colonial population control policy was adopted, as a method of promoting economic development in the colonies. This population control policy meant that access to family planning was encouraged, and the British colonies were the first in Africa to implement family planning programs (Oliver (1995), Caldwell and Sai (2007)).

Our framework also acknowledges a possible *indirect* role of colonial rules in shaping fertility, through its effects on economic development. The bulk of the literature contrasting the British and the French colonial rules argues that the British governance style outperformed the French approach in fostering human capital accumulation and the protection of local political structures and property rights (La Porta et al. (1998); Acemoglu et al. (2001); Lee and Schultz (2012); Dupraz (2019)). Consequently, British colonization was more conducive to economic development, thought to lower fertility.<sup>2</sup> By contrast, recent studies focusing on the legal systems governing marital property rights demonstrate that the common law system inherited from the British colonial power exhibits less secure marital property rights, therefore decreasing female bargaining power within the household, when compared to the French civil law system (Anderson (2018)). To the extent that female bargaining power negatively affects fertility (Doepke and Tertilt (2018)), this argument seems to imply that fertility should be higher in former British colonies. It follows from these conflicting arguments that the impact of British (vs. French) colonization on fertility behavior is theoretically ambiguous. This question is therefore best answered empirically. We address this question and show, in addition, how colonial origins interact with market incentives to shape long-term fertility outcomes in sub-Saharan Africa.

Sub-Saharan Africa is the only region of the world where the demographic transition is still in its early stages. Despite an impressive increase in female education and a significant decline in child mortality, fertility in this region remains very high, with an average rate of 4.8 children per woman in 2016, which is twice as high as the world average of 2.4 children

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<sup>2</sup>The relationship between economic development and lower fertility is difficult to determine since there is two-way causality. The literature documenting the positive effect of lower fertility (induced by contraceptive use) on female economic empowerment and child quality (see Ananat and Hungerman (2012), Myers (2017)) suggests that female human capital and labor participation may be partly the result of population policies directly affecting fertility.

per woman ([World Bank \(2016\)](#)). This high level of fertility, however, masks significant variation across and within African countries, as the demographic transition process is much more advanced in certain parts of the continent than in others.<sup>3</sup> The increasing divergence in fertility rates between former British and French colonies in recent decades is evidence of this fact. These countries experienced growing fertility rates during their transition to independence and in parts of the 1970s (see [Figure 2.1-a](#)). During those years, fertility rates were slightly lower in former French colonies. However, by the end of the 1970s, the gap had reversed and fertility rates started to decrease more rapidly in former British colonies. Indeed, the fertility gap between former British and French colonies more than tripled between 1975 and 2016 ([Figure 2.1-c](#)). Quite remarkably, there is a parallel between these differing trends in fertility and trends in income per capita. [Figure 2.1-b](#) shows that former French and British colonies had comparable level of economic development during the independence years, but former British colonies experienced a radical take-off in the mid 80s and have grown much faster than former French colonies. The latter have basically stagnated since 1960 (see [Bergh and Fink \(2018\)](#)). Importantly, as [Figure 2.1-c](#) shows, the reversal of the fertility gap between former British and French colonies started more than two decades before the per capita income gap became visible. This difference in timing implies that the income gap is unlikely to explain the fertility gap.<sup>4</sup>

The aforementioned trends obviously suggest a relationship between colonial institutions, fertility, and income. However, the extent to which these correlations can be considered causal is not at all clear, as one cannot rule out the possibility that unobserved factors are driving these relationships. It is possible that the British and the French colonized countries that were initially very different in terms of their natural and cultural endowments, and that these initial differences subsequently translated into differences in fertility behavior. In investigating how colonial origins affect fertility, we give this endogeneity issue a serious consideration. We address this concern by exploiting the natural experiment that led to the arbitrary division of historical ethnic homelands across

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<sup>3</sup>For example, in 2015, the fertility rate was 2.4 children per woman in South Africa, 3.2 in Lesotho, and 7.2 in Niger ([World Development Indicator, 2019](#)).

<sup>4</sup>This difference in timing therefore implies that the effect of colonial origins on fertility is not mediated by economic development. We also demonstrate this when analyzing the micro-level mechanism underlying this effect. It follows that our documents a new mechanism through which colonial origins affect economic development in Africa.

colonial borders during the “Scramble for Africa”. The arbitrary nature of the placement of borders in Africa means we can separate out the effects of policies which differ across countries and culture factors, such as language, ethnicity, and ancestral religion which are similar on each side of the border.

Combining individual-level data on women aged 20 to 49 years old from Demographic and Health Surveys (DHS) with data on historical ethnic homelands from Murdock’s Ethnographic Map of Africa and geographic data from several sources (see Section ??), we implement a spatial Regression Discontinuity Design (RDD) with ethnic homeland fixed effects to estimate the causal effect of British (versus French) colonization on reproductive behavior. This identification strategy accounts for culture and other unobserved ethnicity-related factors that may affect fertility and that could potentially bias our estimates. In addition, by only comparing observations that are close enough to the border, our strategy accounts for natural endowments, initial differences in economic development and other hard-to-account-for unobserved geographical factors. Reassuringly, we do not find a wide range of geographical factors and natural endowments (e.g. elevation, soil suitability for agriculture, and natural resources) to vary across the colonial border.

We find that, on average, women in former British colonies have significantly fewer children than their counterparts in former French colonies. In the proximate determinants framework this must be due to delays in the age of marriage and sexual activity, or avoidance of pregnancy within marriage. Part of our story is through family planning use, but we also find that women in former British colonies are more likely to initiate sexual activity at older ages and to delay childbearing, and they are less likely to engage in child marriage (that is, being married before 18 years old). Importantly, these findings are robust to controlling for religion (Catholicism, Protestantism, Islam) and all the aforementioned geographic factors and natural endowments.

Our findings are also robust to controlling for *spillover effects* at the border. This latter control is important because it takes into account the fact that certain families that were close enough to the arbitrary colonial border were themselves split across countries. Because these families are likely to maintain ties with individuals living across the border and even cross the border to reside temporarily on the other side, this is likely to attenuate the fertility effect of British colonization owing to these individuals being doubly treated. We address this issue by estimating the effect of colonial origins on two subsamples: the

sample of natives and the sample of natives living beyond 5km from the British-French border. These latter analysis show that women in former British colonies have on average 0.3 – 0.6 fewer children than their counterparts in former French colonies. In the context of fertility, this gap is large. In fact, the differential trends in fertility presented in Figure 2.1-*a* suggest that closing a gap of such magnitude might take several decades.

Central to our study is the analysis of how colonial origins interact with market access to shape fertility behavior. Indeed, we show that the average effect of British colonization masks important heterogeneity that depends on proximity to the sea coast, taken as a measure of historical and contemporaneous market access.<sup>5</sup> During the pre-industrial era, territories close to the coast were more accessible and attractive for early Europeans engaged in trade, mainly because of their proximity to European markets and the presence of natural harbors and capes amenable to docking ships. Consequently, European merchants settled in large numbers along the African coastline, quickly forming centers of commercial activity. Moreover, the potential for faster economic development generated by the existence of pre-colonial trade posts in areas close to the coast provided incentives for greater colonial investments in these areas compared to the hinterland (Huillery (2009), Okoye and Pongou (2017), Ricart-Huguet (2017)). Therefore, with greater access to global markets, and subsequently more economic opportunities, coastal areas (relative to the hinterland) were more likely to be economically developed regardless of the identity of the colonizer. This was less likely to be the case in the hinterland where economic opportunities were more likely to be endogenous to colonial institutions.

As a preliminary to our analysis, we show that these patterns have persisted to the present day. First, we show that coastal areas are much more developed today than the hinterland, and that women in those areas enjoy higher education and labor participation, implying a higher opportunity cost of childbearing. Second, we estimate the effect of British colonization on local economic development and female labor participation in the coastal areas and in the hinterland separately. In particular, we find that British colonization has a positive and significant effect on light density in areas far from the coast, but this effect disappears in coastal areas.<sup>6</sup> Reassuringly, we find similar results

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<sup>5</sup>This analysis is possible because distance to sea is orthogonal to British colonization in our sample.

<sup>6</sup>Light density has been used to measure local economic development in several papers including Min (2008), Chen and Nordhaus (2011), Henderson et al. (2012), Pinkovskiy (2013), Michalopoulos and Papaioannou (2013, 2014).

when using indicators of households welfare (child nutrition and child death) and female occupation and earnings, the latter measuring the opportunity cost of childbearing. These findings imply that across the British-French colonial border, areas closer to the coast are similarly developed. They also imply that the opportunity of having a child is not only higher in coastal areas compared to the hinterland, but it is equally high in these areas irrespective of the colonizer’s identity.

Now analyzing the heterogeneous effect of colonial origins on reproductive outcomes, we find that British colonization has little effect on these outcomes in coastal areas. The fertility effect of British colonization is only present in the hinterland. These findings imply that the fertility effect of colonial origins does not persist when the opportunity cost of having a child is sufficiently high. From a policy perspective, our analysis suggests that even if history is immutable, its long-term effects can be modified through appropriately designed policy interventions that generate economic opportunities for women.<sup>7</sup>

Finally, we test the *direct* mechanism through which colonial origins affect reproductive outcomes in Africa. We show that British colonization has a strongly positive effect on the use of modern contraceptive methods, and that this effect is much greater in areas far from the sea. We also explore the *indirect* and secondary mechanism supported by our conceptual framework. This mechanism builds on the theoretical literature on the short-term drivers of fertility and demographic transition (Becker (1960), Mincer (1963), Becker and Lewis (1973), Galor and Weil (1996), Strulik (2017), Doepke and Tertilt (2018)). Following this literature, we consider three additional channels: (a) female education; (b) female economic empowerment; and (c) child quality (measured by child mortality and anthropometric indicators). We do not find strong evidence for this indirect mechanism. First, we find that British colonization has a larger effect on female education in areas close to the sea, in contrast to its heterogeneous effect on fertility. This clearly suggests that education is not the primary channel through which colonial origins operate. Second, we argue that the effect of British colonization on female economic empowerment does not explain its effect on fertility behavior in areas far from the sea for two reasons. The first reason is that the fertility gap between these colonies preceded the income gap (Figure

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<sup>7</sup>Importantly, these findings are novel and are of independent interest to studies of the long-term effect of colonial origins on economic development (Acemoglu et al. (2001), Michalopoulos and Papaioannou (2014)). Indeed, our study is the first to highlight the heterogeneous nature of the colonial origins of comparative economic development, and to show that the long-term impact of history can vary depending on exogenous market access.

2.1-c). The second reason is that the effect of colonial origins on fertility in areas far from the sea persists even after controlling for household income (and/or female education). Finally, we find that the effect of colonial origins on measures of child quality is stronger in the hinterland, consistent with their heterogeneous effect on fertility. However, their effect on under-five mortality is weak and inconsistent. The analysis, therefore, suggests that the indirect mechanism is not the primary channel through which colonial origins affect fertility behavior in areas far from the sea. The findings are more supportive of our direct mechanism. The latter implies that the fertility effect of colonial origins is directly linked to colonial reproductive laws and their impact on the use of modern methods of birth control.

## 2.2 Contribution to the Literature

Our paper asks a new question. It explores the broad and important question of whether economic policies can mitigate the long-term impacts of history, even though history itself is immutable. We focus on colonial reproductive laws as a historical fact, but it is evident this question is generalizable to other historical facts such as the slave trade, colonial property rights institutions, and so on. Our main finding that market incentives can mitigate the long-term impacts of colonial origins implies that appropriately designed economic policies may be able to overcome the bonds of historical determinism. Obviously, this finding can also be viewed as a contribution to the recurrent and often uneasy debate over the range of policy actions that can be undertaken to repair the damages of bad historical shocks.

Besides asking a new question, our study has three other contributions. First, our work nurtures the current debate on variation in the pace of demographic transition in Africa (see [Bongaarts and Casterline \(2013\)](#) and the references therein). While the extant literature explains this phenomenon by focusing on cross-country differences in the short-term determinants of fertility (such as female labor participation, female education, and child mortality), we contribute to this debate by showing that deep-rooted political institutional factors matter, and that these factors matter primarily in areas with low market access.

Second, we focus on colonial reproductive laws, a feature of colonial institutions that

has received no attention in the literature. To the extent that fertility affects economic development (Bloom et al. (2009)), our paper can be viewed as documenting a novel mechanism through which colonial origins have had a persistent impact on economic development in Africa. In fact we do not find that the fertility effect of colonial origins is driven by economic development; instead we show that colonial origins affect fertility through the use of modern methods of contraception. In this sense, our study enriches the broad literature on the historical origins of comparative economic development (Acemoglu et al. (2001), La Porta et al. (2008), Nunn (2008), Alesina et al. (2011), Nunn and Wantchekon (2011), Okoye and Pongou (2014, 2017), Wantchekon et al. (2015), Michalopoulos and Papaioannou (2013), Alesina et al. (2013), Acemoglu et al. (2014), Cogneau and Moradi (2014), Fenske and Kala (2017), Dupraz (2019), Anderson (2018); see also Michalopoulos and Papaioannou (2018) for a comprehensive literature review). However, our paper distinguishes itself from this literature in that we answer a completely different question. In doing so, our analysis is the first to highlight the heterogeneous nature of the colonial origins of comparative fertility behavior and economic development in Africa, a finding that has significant policy relevance.

Finally, our paper contributes to the nascent literature that investigates heterogeneity in the long-term effects of history. Using data from Nigeria, Okoye et al. (2019) show that colonial railroads have short-term and long-term impacts on several measures of local economic development. They analyze heterogeneity in the effect of colonial railroads, finding that its effect is only present in areas with low pre-railway access to the coast. In the same vein, in a study that investigates the role of national institutions for subnational development, Michalopoulos and Papaioannou (2014) show that the explanatory power of national institutions on regional economic development was only visible in areas close to the capital centers. Our paper clearly differs from the aforementioned studies in its scope, analysis, and policy implications.

The rest of this paper unfolds as follows. Section 2.3 describes our conceptual framework. Section 2.4 describes the data. Section 2.5 presents our empirical strategy. Our findings are presented in Sections 2.6 and 2.7. Section 2.8 discusses the mechanisms driving the fertility effects of colonial origins, and Section 2.9 concludes.

## 2.3 Conceptual Framework

In this section, we present our conceptual framework, which highlights the possible theoretical channels through which colonial origins may affect reproductive behavior. This framework is summarized in Figure 2.2. It features two types of channels. The first and primary channel is a *direct* mechanism supported by differences in colonial population policies. The second channel is a class of *indirect* mechanisms which we view as secondary. These latter mechanisms include the protection of marital property rights, the protection of economic property rights, administrative rules, and education policies. We argue that these channels affect proximate determinants of fertility behavior such as contraceptive use (our *direct* mechanism), household income, female education and economic empowerment, and child quality. In the next section, we first recall the theoretical literature on these proximate determinants, and then show how they could be affected by the aforementioned colonial institutions.

### 2.3.1 Literature on the Drivers of Fertility

It is important to differentiate between proximate or mechanical determinants of fertility and the distal social determinants of these behaviors. The major proximate determinants of fertility in Africa are delayed age of marriage and sexual activity, use of contraception, abortion, and postpartum insusceptibility (Bongaarts (2015), Canning et al. (2015)). These proximate determinants depend in turn on fertility desires which depend on child mortality (which induces replacement and insurance fertility), women's education and labor market opportunities (which affect the opportunity cost of children), and female empowerment and social norms (which affect women's bargaining power and decision making).

Following the pioneering work of Becker (1960), the literature has also emphasized the forward looking quality-quantity tradeoff in children, as an underlying factor of fertility decisions within the household (Mincer (1963), Becker and Lewis (1973), Galor and Weil (1996), Strulik (2017), Doepke and Tertilt (2018)). Becker's framework assumes that parents derived utility from both the quantity and the quality of their children, viewed as normal goods and treated similarly as other consumption goods. A key insight from

this model is the child quantity-quality trade-off theory, whereby an increased demand for future child quality lowers the demand for child quantity. Moreover, Becker's theory implies that a high level of wages induces parents to demand fewer, higher quality children, because of an increase in the opportunity cost of raising children.<sup>8</sup>

The quantity-quality trade-off theory has been extended in several directions, uncovering new insights. An important literature emphasizes the role of female relative wages and education in explaining fertility (Mincer (1963), Schultz (1981), Galor and Weil (1996), and Galor and Weil (2000)). Galor and Weil (1996) show that a rise in the relative wage of women due to technological progress increases the opportunity cost of childbearing more than a rise in family income. This in turn enables women to substitute out of childbearing into the labor market, thereby reducing their demand for children. Similarly, Galor and Weil (2000) show that as the return to investment in education rises following technological progress, the opportunity cost of raising children rises as well, lowering fertility. Subsequent studies show that the role of female education in lowering fertility is mediated by delays in marriage and in onset of childbearing, and by a more effective use of modern methods of birth control (see Bongaarts (2010)).

There has been a debate about the relative importance of the proximate and distal determinants of family planning. Pritchett (1994) emphasizes that actual fertility is usually very close to desired fertility in most countries, and so women seem able to achieve their desired fertility even if some of the proximate mechanisms for fertility control, such as contraception and abortion, are difficult to access. However, evidence from intervention studies, and more recent work in Africa has emphasized that while desired fertility remains high there is a considerable unmet need for family planning and that access to family planning methods could have a large impact on fertility (Debpuur et al. (2002), Bongaarts and Casterline (2013)).

Recent studies explicitly incorporate contraceptive use into economic models of fertility (Bhattacharya and Chakraborty (2017), Strulik (2017)). A key insight from these models is that, as income rises, households spend more on contraceptive methods, which allow them to experience utility from sexual activity without a proportional increase in the number of children. Contraceptive use is therefore seen as another factor that medi-

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<sup>8</sup>Galor and Moav (2002) incorporate technological progress into Becker's framework, uncovering a new quantity-quality theory. In their theory, parents substitute quality for quantity in response to technological progress that increases the returns to child quality.

ates the theoretically negative relationship between income and fertility (see also [Becker \(1960\)](#)).

## 2.3.2 British vs. French Colonial Institutions

In this section, we highlight several key aspects of colonial institutions that may affect fertility. The proximate determinants are delayed age of sexual activity and marriage, and contraceptive use, while the more distal determinants of these behaviors are household income, child mortality, female education and economic empowerment, and the desire for child quality.

### 2.3.2.1 Primary Mechanism: Colonial Population Policies and Reproductive Health Laws

Following World War I, Great Britain and France adopted very different population policies that were extended to their colonies and translated into differing culture of contraceptive use in these former colonies.

France adopted a pronatalist population policy in the early 1920s. In order to raise fertility and allow the country to regain the numerical superiority that it had in the centuries before the demographic deficit caused by World War I, on July 31, 1920, France passed a law that severely repressed abortion and that prohibited any propaganda on contraceptive use or directed against having children ([Latham \(2002\)](#), [Garenne \(2017\)](#)).<sup>9</sup> As part of French civil law, the application of the 1920 pronatalist law was extended to the French colonies. Despite subsequent amendments to the 1920 law and its repeal in December 28, 1967 in the metropole (*Loi Neuwirth*), with great liberalization of reproductive health laws, this pronatalist law remained in application in all former French colonies in Africa after their independence. In fact, in many francophone African countries, it was not until the 1980s and 1990s that this law was revoked, and reforms authorizing information and awareness campaigns on family planning were gradually introduced.

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<sup>9</sup>The pronatalist law of 1920 was reinforced by a law called the “*Code de la Famille*” introduced by the French government in July 30, 1939. This law gave more entitlements to adults with children, including cash incentives to mothers who stayed at home to care for children, subsidized holidays, better maternity leaves, and a lump sum transfer to parents with a third child.

France's pronatalist laws contrasted with the liberal culture of reproductive rights in Great Britain. Indeed, the influence of Malthusianism alongside the emergence of a national conversation about family planning following the famous *Bradlaugh-Besant* trial that took place in England in 1877, democratized ideas of birth control in England and in societies with strong cultural ties to Great Britain. In fact, the *Bradlaugh-Besant* trial, named after two secular activists, Annie Besant and Charles Bradlaugh, who were prosecuted for publishing a book providing elementary contraceptive information (see Appendix Figure A2.1), brought substantial attention to a subject highly controversial in Victorian Society. The central debate during this trial focused on the contemporary widespread argument that family size should be an optimal conscious choice. Despite the guilty verdict, the publicity surrounding the trial, inside and outside England (particularly in British colonies [Beach and Hanlon \(2019\)](#)), radically increased the demand for information about contraception. Following the trial, the sales of books and pamphlets on family planning and contraception increased and in the subsequent years, England experienced a sharp decline in fertility. [Beach and Hanlon \(2019\)](#) demonstrate that the changing societal norms about family planning and contraception induced by this trial partly explained the sharp decline in fertility beginning in the 1870s in England. Moreover, they show that the consequence of this trial resonates also in countries with strong cultural ties with England. In particular they associate the fertility decline in South Africa and English Canada (two British colonies) to the shift in societal norms surrounding family planning. More practical contraceptive information appeared beginning in 1921 with the establishment of clinics promoting birth control primarily in London. In addition, after World War II, the rising awareness of the world demographic problem intensified British efforts to control population growth through the development and dissemination of modern methods of contraception.

Despite the adoption of a colonial population control policy by the Colonial Office in London during World War II, these family planning programs were not instituted in the British colonies in Africa until the late 1950s. Among African countries, Kenya was a pioneer in adopting family planning policies. Modern contraception was introduced in this country in 1957, and the first clinics offering modern methods of birth control appeared in 1960. The adoption of family planning policies in other former British colonies accelerated after their independence. For instance, in Ghana, interest in population con-

trol was materialized right after the independence with the creation in 1961 of “Family Advice Center” which consisted of specialized centers providing resources for family planning (Oliver (1995), Caldwell and Sai (2007)). During this period, as already mentioned, former French colonies were still under the 1920 pronatalist law. It was not until the World Population Conference held in Bucharest in 1974 that attitudes toward family planning began to change in these countries. This conference was a turning point essentially because it reunited representatives of 139 member states who drafted the “World Population Plan of Action”, in which principles and directives for population policy and action were formulated. Beginning in 1980, certain former French colonies revoked the 1920 law, and by 1990 almost all of these former colonies had revoked it. This policy changed again at the World Population Conference held in Cairo in 1994 which saw a turn away from population control policies, particularly from coercive policies, and the emergence of a right-based approach to access to family planning.

It follows that former British colonies introduced family planning policies much earlier than former French colonies. Moreover, studies have found even at present that the former French colonies have more restrictive reproductive health laws in terms of access to family planning and abortion than the former British colonies (Finlay and Erin (2017)). Exploiting an index of changes in reproductive health laws in sub-Saharan Africa (see Finlay et al. (2012)), Finlay and Erin (2017) show that the effect of liberalization of reproductive health laws on contraceptive use among women was much larger in former British colonies than in former French colonies.

### 2.3.2.2 Secondary Mechanism: Colonial Rules

**Legal Marital Laws.** The degree of protection of marital property rights differs markedly under the French civil law and the British common law (Anderson (2018)). Under the common law and the underlying separate marital property regime, housewives have no rights to any of the marital property upon the marriage dissolving by either divorce or death. As a result, whereas separate ownership of property might imply benefits for female entrepreneurs through the protection of their own productive assets upon divorce, this marital property law has pernicious consequences for most women, in particular for those working on farms, because it does not recognize non-monetary contributions within

the household. In contrast, the community marital property regime that characterizes the civil law system is associated with a stronger protection of marital property rights. In fact, a central feature of this marital regime is the joint ownership of marital property. It implies an equal division of property between the spouses in the case of marriage dissolution.

Some consequences of these differences have been documented in the literature. [Anderson \(2018\)](#) analyzes the effect of legal origins on HIV status in Africa. She finds that women under the common law regime are more likely to be infected with HIV than their counterparts under the civil law regime, but no effect is found among men. She argues that the community property regime (and thus the French civil law system) leads to empowerment of married women by increasing their bargaining power within the household. This translates into increasing use of protective contraception, thus lowering the risk of HIV.

**Economic Property Rights.** A number of studies focusing on the differences in the legal system inherited from colonization to explain cross-country variation in economic development have stressed the superiority of the common law system in two major legal outcomes: (i) the legal protection of private investors vis-à-vis the state; and (ii) the extent of judicial independence ([La Porta et al. \(1998\)](#), [La Porta et al. \(1999\)](#), [Beck, Demirguc-Kunt, and Levine \(2003\)](#)). In this literature it is claimed that by fostering greater independence of the judicial system and offering lighter government ownership and stronger legal protection of investors, the common law system limits the extent of expropriation and promotes contract enforcement and secured property rights. This is in sharp contrast with the French civil law system characterized by government ownership and regulation, which discourages investment and impedes economic development. Consistent with these theoretical propositions, many empirical studies show that the common law system is associated with more secure property rights, higher quality of government, greater political freedom, and better financial development in the present-day ([La Porta et al. \(1998\)](#), [Djankov et al. \(2002\)](#), [Glaeser and Shleifer \(2002\)](#)). This is consistent with Appendix Figure [A2.2](#), which compares former British and French colonies in terms of different measures of contemporary institutional quality. Indeed, we see that former British colonies significantly outperform former French colonies in terms of the protection of property rights, level of democracy, bureaucracy quality, and quality of the business environment.

**Colonial Administrative Rules and Education Policies.** Historians of European expansion in former colonies have compared the British policy of indirect rule to the French policy of direct rule, arguing most of the time that the former was more conducive to economic growth and human capital accumulation (Crowder (1964), Bertocchi and Canova (2002), Iyer (2010)). Whereas French direct rule was highly centralized and based on the idea of assimilating colonial territories, British indirect rule was much more decentralized and dedicated to preserving local traditions and practices through collaboration with traditional chiefs. This difference contributed to the empowerment and legitimization of local governments in former British (vs. French) colonies, thereby building strong local political structures more complementary to economic growth and public goods provision.

A last difference between the British and the French colonization that is likely to influence fertility through its main proximate determinants is related to educational policies. In order to satisfy the increasing demand for educated administrative workforce within former colonies, both the British and the French colonial governments developed a dual system of private and public schools, although with a different intensity. Unlike the French, the British relied heavily on mission societies to provide and diffuse education. This may have contributed to generating a British advantage in educational outcomes. This advantage was especially stronger for women in former British colonies given that Protestant missions prioritized female education and were more present in the British colonial empire, as opposed to the Catholic missions more present among the French (Nunn et al. (2014)).

The aforementioned British-French differences have persisted to the present-day, as illustrated in the descriptive analysis of Appendix Figures A2.3-a and A2.3-b. These figures show that former British colonies invest more in health and education than former French colonies. In addition, Appendix Figure A2.3-c shows that females in former French colonies are more likely to have no education compared to their counterparts in former British colonies. These figures are consistent with empirical studies showing higher level of household wealth, educational attainment, and greater provision of local public goods in former British areas (Dupraz (2019), Lee and Schultz (2012)). Further, analyzing historical archives on colonial public investments, Dupraz (2019) attributes the advantage in educational outcomes among Anglophones to higher public investments in education

in former British colonies than in former French colonies.

### **2.3.3 Linking Colonial Origins to Fertility: A Theoretically Ambiguous Relationship**

It follows that colonial origins have a theoretically ambiguous effect on reproductive outcomes. To the extent that colonial population policies have had persistent effects on present-day reproductive culture and contraceptive use, one should expect women in the former British colonies to delay marriage and initiation of sexual relationships, and to have fewer children compared to their counterparts in former French colonies. Similarly, if, as argued above, other British colonial institutions including the protection of economic property rights laws, administrative rules, and education policies positively affect the other drivers of fertility (household income, female education, female economic empowerment, and child quality) the same effect should be expected. At the same time, if the civil law system empowers women and increases their relative bargaining power in the household, one should expect the opposite effects. It appears, therefore, that the theoretical relationship between colonial origins and reproductive behavior is not unambiguous. This justifies the empirical analysis to follow.

## **2.4 Data and Descriptive Statistics**

We examine the impact of British (vs French) colonization on several reproductive outcomes including the number of children born to a woman, the onset of childbearing, marriage timing, and sexual behavior. For this purpose (see Section 2.5 below), we matched individual-level information on reproductive behavior from Demographic and Health Surveys with georeferenced data on historical ethnic homelands and geographical data in Africa. In this section, we describe these datasets and how we match them. We also describe how we measure geographic and location variables exploited in this chapter.

### 2.4.1 Individual-level Data

Information on reproductive behavior and socioeconomic and demographic characteristics is drawn from Demographic and Health Surveys (DHS). These surveys have been conducted every five years since 1986 in most African countries. Each DHS survey<sup>10</sup>, interviews a nationally representative sample of women aged 15 to 49 years old and men aged 15 to 59 years old. DHS surveys gather detailed information on a host of demographic, health, and socio-economic characteristics of women and men. For the purpose of the analysis below, we match observations from each country with information about the identity of the colonizer who administered the country the longest during the colonial era.

In this chapter, we only analyze DHS surveys collected from sub-Saharan African countries colonized by either Great Britain or France. Among these countries, due to our identification strategy (described in Section 2.5), we select only former British (French) colonies that share border with a former French (British) colony. We further restrict the analysis to DHS surveys with GPS information and to women aged 20-49 years old, whose migration status is known. This leaves us with a sample of 34,405 women living in 10 countries, among which 7 countries are former French colonies and 3 countries are former British colonies.<sup>11</sup> The first panel of Appendix Table A2.1 provides summary statistics on the main outcomes of interest as well as on individual socioeconomic and demographic variables used in the empirical analysis.

### 2.4.2 Ethnicity-level Data

To effectively circumscribe the causal impact of British (vs French) colonization on fertility behavior, we control for ethnic homeland fixed effects in our empirical strategy. Ethnic homeland fixed effects ensure that our estimated effect is not biased by ethnic-specific characteristics such as culture or norms surrounding gender and fertility. In fact, it is

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<sup>10</sup>Information from DHS surveys are generally recorded at different levels. The analysis in this chapter relies mainly on the Individual Recode (IR) files, the Household Recode (HR) files, and the Child Recode (CR) files.

<sup>11</sup>Former French colonies are: Benin, Burkina Faso, Cameroon, Ivory Coast, Guinea, Niger, and Togo. Former British colonies are: Ghana, Nigeria, and Sierra Leone. In a robustness analysis we exclude Cameroon and Togo, whose first colonizer was Germany; this does not affect our main conclusions.

possible that former British and French colonies have different pre-colonial characteristics that also determine reproductive behavior. By controlling for ethnic homeland fixed effects, we also address this potential source of endogeneity. We also control for religious affiliation, given the fact that Protestant missions were more likely to be present in British colonies whereas French colonies had more Catholic missions (Nunn et al. (2014)). Controlling for religion is also important because Islam, which predates European missionary activities in Africa, is more prevalent in certain countries than in others.

We collect data on the location coordinates of historical ethnic homelands by relying on George Peter Murdock’s Ethnographic Map of Africa (1959). This map portrays the spatial distribution of 826 ethnic areas across Africa at the time of colonization. Following a similar approach as in Michalopoulos and Papaioannou (2013, 2014, 2016), we overlay contemporary national boundaries of Africa on Murdock’s map to identify historical ethnic homelands that are split across former colonies (Figure 2.3-a). Figure 2.3-b shows the same map but only retains the subset of former British and French colonies for which both DHS and geographic data are available.

### 2.4.3 Geographic Variables

Using GPS information from the DHS, we augment the individual-level data with geographic information measured at a very fine level. Following Michalopoulos and Papaioannou (2013), we divide Africa into pixel units of 12kmx12km. For each pixel, we rely on various sources (see Section 2.12) to collect information on the following measures of geographic and natural endowments: elevation, soil suitability for agriculture, area under water (rivers, lakes, streams), natural resources, distance from the centroid of a pixel to the sea coast, and distance from the centroid of a pixel to the national border. Panel B in Appendix Table A2.1 shows some descriptive statistics for these variables. To implement the spatial RDD analysis (see Section 2.5), we define the running variable as the nearest distance from the centroid of a pixel to the British-French border. In Section 2.5, we show that these geographic variables do not vary across the British-French border, which is reassuring as it implies that within each ethnic homeland, areas that were colonized by the British are comparable to areas that were colonized by the French with respect to measures of local economic development in the precolonial era. Nevertheless, we also

show regression results that control for these variables. Importantly, controlling for these variables in addition to ethnic homeland fixed effects, largely account for pre-colonial events such as the slave trade, given that the number of slaves exported from each area was primarily a function of the distance to the coastline and some of the aforementioned geographic variables.

## 2.5 Identification Strategy: Regression Discontinuity Design

We estimate the causal effect of colonial origins on reproductive outcomes using a spatial Regression Discontinuity Design (RDD) with ethnic homeland fixed effects. A central feature of this identification strategy is to exploit within-ethnic homeland variation across individuals residing close to the British-French border. We have a baseline strategy that consists of applying this methodology to the whole sample. However, this strategy is likely to suffer from spillover effects at the border, leading to an attenuation bias. We address this concern using a second strategy that consist of restricting the analysis either to natives or to natives that live at least 5km to the border on each side. Throughout, results from these identifications strategies are contrasted with those obtained from the Ordinary Least Squares regressions.

### 2.5.1 Baseline Strategy

Our baseline strategy is expressed as follows:

$$Y_{ipcet} = \alpha + \beta \text{British}_c + f(\text{BD}_{pce}) + \delta_e + \gamma_t + \sigma_a + \theta_r + Z'_{pce}\mu + \varepsilon_{ipcet} \quad (1)$$

where  $Y_{ipcet}$  is the outcome of interest for an individual  $i$  born at time  $t$ , living in country  $c$ , ethnic homeland  $e$ , and pixel  $p$ . The variable  $\text{British}_c$  is equal to one if the country was colonized by Great Britain and zero if it was colonized by France. Our coefficient of interest here is  $\beta$ . This coefficient gives the local average effect of British colonization on reproductive behavior.  $\delta_e$ ,  $\gamma_t$ ,  $\sigma_a$ , and  $\theta_r$  are ethnic homeland fixed effects, year of birth fixed effects, age fixed effects, and religion fixed effects, respectively.

$Z'_{pce}$  is a vector of location controls (distance from the centroid of pixel  $p$  to the sea coast and distance from the centroid of pixel  $p$  to the nearest national border) and geographic controls (area under water, elevation, soil suitability for agriculture, pixel area, and natural resources), all measured at the pixel level.

Lastly, the function  $f(BD_{pce})$  represents a second-order RD polynomial of the distance from the centroid of each pixel to the British-French border. Yet, for robustness checks and to account for the multidimensional nature of the cut-off in a spatial RDD (Dell (2010)), we also show that our results are robust to two other specifications where the RD polynomial  $f(BD_{cep})$  is defined as: (i) a third-order polynomial of the distance from the centroid of each pixel to the British-French border; and (ii) a cubic polynomial in latitude and longitude of a pixel.

For inference purposes, we follow the method of Cameron et al. (2011) and cluster standard errors along both the country and ethnic-family dimensions.<sup>12</sup> As pointed by Cameron et al. (2011) the double-clustering enables to account for spatial correlation and other arbitrary correlation within each dimension.

We exploit the empirical specification in equation (1) to estimate the average effect of British (vs. French) colonization on reproductive behavior (Section 2.6). In addition, in Section 2.7 we use the same specification to estimate the heterogeneous impact of British colonization by market access.

## 2.5.2 Addressing Spillover Effects

A potential threat to our identification strategy comes from the fact that *spillover effects* induced by migration from other regions and by migration across the British-French border could possibly bias our results. Indeed, using the current place of residence to identify the ethnic homeland of an individual, as we do in the baseline model, has potential flaws. First, if areas close to the border attract individuals from other regions and cultures of the country, then including those individuals in the analysis could bias our baseline estimates, as they do not properly control for culture. We address this issue by restricting the analysis to natives only. The latter are individuals who never lived elsewhere than

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<sup>12</sup>Murdock (1959) identifies more than 800 pre-colonial ethnic groups across Africa and assigns them into 96 ethnolinguistic families.

their place of birth. They have therefore only been subjected to the cultural influence of the ethnic homeland in which they were born.

Second, in a context where ineffective border controls have been shown to drive irregular and circular migrations (Lohrmann (1989), Adepoju (2008)), it is most likely that, at a certain point in their lifetime, some people might have migrated from the British side to the French side of the border and vice-versa. Indeed, due to the arbitrary division of ethnic homelands across colonial borders, many families that were very close to the border were split between different countries. These individuals who maintain ties across the border are likely to cross the border to reside temporarily on the other side. They are therefore likely to be treated doubly, by both the British and the French colonial legacies, leading to an attenuation bias. To assuage this migration issue at the border, we perform the analysis on the subset of natives, but excluding individuals inside pixels that fall within 5km of each side of the border. This approach is similar to an RDD estimation where we assume that the British-French border that divides the ethnic homeland is 10 kilometer thick. Michalopoulos and Papaioannou (2013) follow a similar approach in a study of the role of pre-colonial ethnic institutions on contemporary African development. This approach addresses the spillover issues in the baseline model.

### 2.5.3 Validity of the Identification Strategy

There are many challenges associated with spatial RDD. In our setup that compares individuals' reproductive behavior within the homeland of the same ethnicity in adjacent countries with different colonial origins, the validity of the identification strategy requires that observations are close-to-randomly assigned into treatment and control groups across the border. That is, the border between former British and French colonies within the homeland of the same ethnicity should not be influenced by local circumstances such as existing political or institutional factors that themselves are potentially important determinants of reproductive behavior. Indeed, historians and social scientists concerned with the European expansion and colonization in Africa provide ample evidence of the randomness of African borders. They argue that at the time of the scramble for Africa in the 1880s, Europeans had in most cases, drawn the African borders without or with extremely limited knowledge of local conditions (see for instance Michalopoulos and Pa-

paioannou (2015, 2013) for a review of historical arguments supporting the arbitrary drawing of African borders). Thus, the incidental nature of this historical event ensures that the border between countries with different colonial origins is locally random.

Another key identification assumption in the RDD setup requires that the relevant determinants of the outcome of interest, besides the treatment, should vary smoothly at the border. It implies, therefore, in our specific case, that ethnic areas across the border should be similar across all relevant factors of reproductive behavior, with the exception of the identity of the colonizer. We assess the plausibility of this assumption by examining the relationship between colonial origins and a set of observable characteristics that may independently affect reproductive behavior. Using an RDD approach similar to our main specification (equation (1)), we estimate the effect of British colonization on various geographic, ecological, and natural resource variables (dependent variables) that are likely to shape reproductive outcomes. Results are displayed in Table 2.1. We find no effect of British colonization on any of these outcomes. Since these outcome variables are also important determinants of initial economic development our results also imply that, within ethnic homelands, areas colonized by the British were similar to those colonized by the French in terms of precolonial economic development.

## 2.6 Average Effect of Colonial Origins

We now examine empirically the causal relationship between colonial origins and reproductive outcomes by exploiting variation within ethnic groups partitioned between former British and French colonies. Before turning to the main empirical findings, we first provide a graphical illustration of the RD estimates of the British effect on fertility and other reproductive outcomes.

### 2.6.1 Graphical Illustration

Figures 2.4 and 2.5 provide a visual illustration of the RD design, where the running variable is defined as the geodesic distance (in kilometers) from the centroid of each pixel to the nearest British-French border. The vertical line in these graphs marks the British-French border (the cut-off at zero). Each graph plots, for individuals within 2km bins,

the average value of the outcome of interest, conditional on religion fixed effects. The two-dimensional curve overlaid on each scatter plot shows the predicted outcome for a regression that includes a linear polynomial in the running variable, fitted separately using raw data from former British colonies (where distance takes on positive values) and raw data from former French colonies (where distance takes on negative values). Following our preferred empirical identification strategy which accounts for *spillover* issues at the border, we plot the RD graphs using the sample of natives.

In Figure 2.4 we focus on the number of children ever born. The RD graphs for the other outcomes are displayed in Figure 2.5. Figure 2.4-a shows that at the border, women in former British colonies have slightly fewer children than their counterparts in former French colonies. A similar jump at the border is observed when we consider the other outcomes of interest in this chapter. Indeed, in Figures 2.5-a and 2.5-g, we observe a lower probability of onset of childbearing before age 18 and child marriage (that is marriage before age 18), respectively, amongst females living on the British side of the border. Figure 2.5-d shows that women in former British colonies are slightly more likely to delay initiation of sexual activity. We observe a similar pattern on the RD graphs where we consider the whole sample containing both natives and non natives (Figures A2.4 and A2.5).

## 2.6.2 Fertility Behavior

In this section, we discuss the RD estimates of the average effect of British (vs. French) colonization on the number of children born to a woman. We start by presenting the baseline results from an estimation of equation (1), where  $Y_{i\text{cept}}$  is the total number of children ever born for a randomly selected woman  $i$ . Then, we present results that address *spillover effects* at the border.

### 2.6.2.1 Baseline Results

The baseline estimates of the effect of British colonization on fertility measured by the number of children ever born are presented in *Panels A* and *B* of Table 2.2. In each column of this table, we control for ethnic homeland fixed effects, age fixed effects, year of birth fixed effects and religion fixed effects. Below the estimates, we report robust

standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level. We begin by reporting in the first column within-ethnicity Ordinary Least Squares (OLS) estimates which account for idiosyncratic country- and ethnicity-specific characteristics that are likely to confound the results. We find that the within-ethnic homeland estimate is negative and statistically significant. Females in former British colonies have on average 0.4 fewer children.

In order to address the concern that some hard-to-account-for or unobservable factors such as geographic and ecological factors, are likely to drive the OLS results, we implement an RDD approach using four different bandwidths. Specifically, we restrict the sample to pixels within 60km, 100km, 150km, and 200km from each side of the British-French border.<sup>13</sup> Results are displayed in columns (2)-(5). The empirical specification in each of these columns includes a second-order RD polynomial in distance from the centroid of a pixel to the nearest British-French border. The RD estimates reveal that women in former British colonies have significantly fewer children than their counterparts in former French colonies. The fertility effect of British colonization is not only significant, but it is also economically meaningful. The British effect on fertility ranges from  $-0.33$  to  $-0.45$ , representing a decline in fertility of about 8 to 11% of the average number of children per woman in the sample.

The British impact on fertility barely changes (*Panel B*) when we augment the specification with a rich set of geographic and location controls including measures of area under water, elevation, soil suitability for agriculture, area of the pixel, natural resources, and distance to the national border. The coefficient on British colonization remains precisely estimated in all specifications. Moreover, the size of each coefficient is similar to the estimate obtained where we do not add control for geographical factors, which reflect the fact that there is no discontinuity in these factors at the border. The next section presents estimates that address the issue of *spillover* at the border.

### 2.6.2.2 Addressing Spillover Effects at the Border

In Section 2.5, we argued that spillover effects induced by migration could bias our estimates of the true effect of British colonization on reproductive behavior. Following our

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<sup>13</sup>In Figure 2.6 we show results for a larger set of bandwidths.

identification strategy, we address this issue in two different ways.

First, we estimate equation 1 on the subset of natives. Natives are individuals who never lived elsewhere than their place of birth since they were born. They have therefore been subjected only to the culture of their ancestral ethnic homeland. The conditional and unconditional estimates are displayed in *Panels C* and *D* of Table 2.2, respectively. Compared to the baseline results in *Panels A* and *B*, the negative effects of British colonization on fertility reported in *Panels C* and *D* are quantitatively larger and more precisely estimated. In the most restrictive specification in column (2) of *Panel C*, when we limit our attention to areas within 60km of the British-French border, the coefficient on the dummy for British colonization is negative ( $-0.39$ ) and statistically significant. The coefficient increases in absolute term (up to  $-0.52$ ) and retains its statistical significance as we increase the bandwidth. Turning to *Panel D*, when we control for geographic characteristics, we see that the coefficient on the British colonization dummy is in the range  $-0.38$  -  $-0.44$ , quite similar to the estimates in *Panel C*. Across all specifications, the results show that native women in former British colonies have significantly fewer children than their counterparts in former French colonies.

Second, in *Panels E* and *F* of Table 2.2, we perform estimation on the subset of natives, but excluding individuals in pixels that fall within 5km of each side of the border. Individuals within 5km of the border are more likely to cross the border and maintain (family) links on the other side.<sup>14</sup> As we already argued, they are therefore likely to be doubly treated, and including them in the sample is likely to bias our estimates downward. Excluding these individuals strengthens our findings that women in former British colonies have significantly fewer children than their counterparts in former French colonies. Indeed, the British effect in all specifications is larger (in the range  $-0.46$  -  $-0.61$ ) and significant at the conventional level, while the standard errors become tighter. Here also, controlling for geographic characteristics does not affect the results.

Our analysis is robust to a set of other specifications. Following the empirical literature on RDD, we investigate the sensitivity of our results to alternative specifications including higher-order RD polynomial and semiparametric RD approaches (Dell (2010)). Results are reported in Appendix tables A2.2-A2.5. These tables show that the uncov-

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<sup>14</sup>In results not shown, we exclude individuals within higher distances (e.g., 15km) of the border, and we find qualitatively similar results.

ered negative link between British colonization and fertility remains globally intact. We also show that our results are robust to a specification that excludes Cameroon and Togo whose first colonizer was Germany (Table A2.7).<sup>15</sup>

### 2.6.3 Other Reproductive Outcomes

In this section, we analyze the effect of British colonization on other reproductive outcomes. These outcomes include the likelihood of childbearing, the timing of becoming sexually active, and age of marriage. In addition to be interesting in their own rights, these variables are viewed as proximate determinants of fertility. It follows, therefore, that analyzing them reveals the proximate mechanism through which colonial origins affect fertility. Results from this analysis are presented in Table 2.3. The first three panels show estimates from a regression where we consider the whole sample of women (baseline specification). Estimates from other specifications where we restrict the analysis to natives and natives not living in pixels within 5km of the border are displayed in *Panels D - I*. Each panel of this table reports results from estimating our baseline equation (1), where  $Y_{\text{icpt}}$  is (1) the probability that a randomly selected woman has had her first child before age 18 (*Panels A, D, and G*); (2) a woman’s age at first sexual intercourse (*Panel B, E, and H*); and (3) the probability to marry before age 18 (*Panel C, F, and I*). In each column of Table 2.3 we control for the same set of variables as in Table 2.2 in addition of controlling for geographic and location variables.

Analyzing the RDD results in columns (2)-(5) of Table 2.3, we find that on average, women in former British colonies are significantly less likely to have their first child before age 18 compared to their counterparts in former French colonies (*Panel A*). Controlling for spillover effects at the border (*Panels D and G*), slightly raises the coefficient on British colonization. In *Panel B* of Table 2.3, we focus on the British impact on the timing of first sexual intercourse. This impact is positive and statistically significant at the conventional level. The results show that women in former British areas are more likely to delay initiation of sexual activity compared to women in former French areas. Turning to the impact of British colonization on marriage timing, we find that the risk of child marriage (that is marriage before age 18) is significantly lower for female in former

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<sup>15</sup>In analyses not reported here, we find similar results when we exclude Cameroon and Togo separately.

British colonies relative to women in former French colonies (see *Panel C* in Table 2.3). These findings are quite consistent across specifications that use different bandwidths, or control for several confounders including spillover at the border. Moreover, all these results are robust to controlling for a higher-order RD polynomial and an RD polynomial in latitude and longitude of the pixel (Appendix tables A2.2-A2.5).

## 2.7 Heterogeneous Effects of Colonial Origins by Market Access

The findings uncovered so far demonstrate that deep-rooted political institutions have persistent effects on fertility and other reproductive outcomes. However, the extent to which such institutions interact with exogenous economic forces to determine outcomes has not been widely studied. In this section, we address this gap by studying how the effect of British colonization varies with market access. By generating income-earning opportunities outside of the household, market access is likely to increase the opportunity cost of having a child. If the prevalence of such opportunities is sufficiently high, it is likely to attenuate the fertility effect of colonial origins. If this hypothesis is validated empirically it will have important policy implications because it shows that appropriately designed economic interventions can break the bounds of historical determinism.

In what follows, market access is measured by proximity to the coast. We will show that coastal areas are more developed than the hinterland irrespective of colonial origins, and that these areas are similarly developed across the British-French colonial borders. We will then study the heterogeneous effect of British colonization, by estimating this effect separately in areas close to the coast and in areas farther inland. We find that this effect is only present in the latter areas, which shows that in coastal areas the high opportunity cost of having a child nullifies the effect of colonial origins on fertility outcomes.

### 2.7.1 Proximity to the Sea: An Exogenous Determinant of Market Access

Studies examining the historical origins of the contemporary divergence in economic development across countries and regions in Africa have sometimes compared coastal areas to the hinterland, with the former being economically wealthier than the latter. This literature provides two main explanations for the persistent economic preeminence of coastal areas relative to the hinterland in former African colonies. The first explanation is the initial geographical endowment of coastal areas. In a pre-industrial context where mobility and economic activity are largely influenced by geographical conditions (Diamond and Ford (2000)), early Europeans engaged in the trade mainly landed in Africa where coastal geography was favorable. That is, where coastal areas featured the presence of natural harbors<sup>16</sup> and capes amenable to docking ships (Ricart-Huguet (2017), Huillery (2009)). This geographical advantage drove massive European settlements in territories close to the coast which, therefore, became centers of transatlantic trade activities during the pre-colonial era, at the expense of the hinterland. This spatial concentration of economic activity is consistent with the literature suggesting that due to low transportation costs and an extended scope of the market, industrialization is expected to almost always proceed first upon the coast before extending to the hinterland of a country (Smith and Stewart (1963)).

In addition, in a study theorizing the creation of industrial hubs, Krugman (1991) emphasizes the role of transportation costs in the location decision of manufacturing firms in order to explain the coexistence of an “industrialized core” and a “agricultural periphery” within a country. One could argue that the commercial activities along the Western and Eastern coast of Africa during the transatlantic trade therefore contributed to these areas offering more economic opportunities and becoming richer. The empirical results in Table 2.4 support these theoretical arguments. Indeed, using a fixed effects model, we find a highly significant negative association between distance to the sea and several measures of local economic development, including light density, an indicator for whether

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<sup>16</sup>For instance, in the East and Western African coast, the Portuguese and later the French first established trade ports in the natural harbor of the Senegal River and the Cape which later became the cities of Saint-Louis and Dakar in Senegal. Similarly, the British landed in the natural harbor of Tagrin Bay in Freetown and Cape Coast in Ghana.

the respondent is engaged in activities requiring high skills, an indicator for whether the respondent receives cash earnings, and an index for asset holdings.<sup>17</sup> To the extent that the latter measures constitute good proxies for the costs of childbearing, results from Table 2.4 imply that the opportunity cost of having a child effectively decreases with distance to sea. Put another way, the opportunity cost of having a child is higher close to the sea compared to in the hinterland.

A second explanation for the persistence of the economic advantage of coastal areas emphasized the role of colonial investments for current development outcomes. A growing literature demonstrates the importance of colonial investments in explaining contemporary regional inequalities in development (Ricart-Huguet (2017) and Huillery (2009)). Exploiting large historical datasets, Ricart-Huguet (2017) provides empirical evidence on the unequal spatial distribution of colonial investments (in infrastructure, health, and education) in both the British and French empires. Analyzing investment inequality within colonial states, this study shows that districts closer to the sea received more colonial investments than those farther away. A conclusion that one could draw from this study is that all colonial powers prioritized coastal areas in their allocation of colonial resources, therefore inducing more economic development opportunities in these areas.

It follows from these arguments that coastal areas were more likely to be similarly developed regardless of the identity of the colonizer. This is also the case because European colonizers mostly settled and ruled their colonies from areas (generally colonial capitals) that were close to the sea (Michalopoulos and Papaioannou (2014)). However, the situation was different in the hinterland, primarily due to differences in governance style. For instance, unlike French colonization, British colonization was more effective at promoting collaboration with local chiefs. To the extent that such a decentralized form of governance is conducive to local economic development (Crowder (1964), Bertocchi and Canova (2002), Iyer (2010), Lee and Schultz (2012)), this implies that the British advantage in economic development should be stronger in areas far from the sea. Therefore, if the opportunity cost of having a child is sufficiently high, so as to dominate the direct effect of colonial origins on reproductive outcomes, we should not expect to see any effect of British colonization on these outcomes in coastal areas. This effect should be

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<sup>17</sup>The asset index takes value 1 if the respondent is living in a household holding at least one of the following assets: radio, television, refrigerator, bicycle, motorcycle, and car.

more pronounced in areas far from the sea, where market access is lower and economic opportunities scarcer.

## 2.7.2 Colonial Origins, Proximity to the Sea, and Light Density

In this section, we test the hypothesis that the British advantage in terms of local economic opportunities is less pronounced in coastal areas compared to areas that are far from the coast. Following the literature on the lasting impact of colonization, we used the median distance to the sea as a cut-off that splits the sample into pixels falling above and below this median value.<sup>18</sup> Then, in each subsample we compare local economic opportunities, as proxied by light density, across former British and French colonies within the same ethnic homeland.

Formally, we use the following RDD specification separately for observations above and below the median distance from the coast:

$$Y_{pce} = \alpha + \beta \text{British}_c + f(\text{BD}_{pce}) + \delta_e + Z'_{pce} \mu + \varepsilon_{pce} \quad (2)$$

Equation (2) is similar to our main specification in equation (1), except for the following changes. First, our unit of observation is the pixel. Second, our outcome of interest is now either a dummy for whether the pixel is illuminated or not;<sup>19</sup> or a variable equal to  $\ln(0.01 + \text{Mean of light density in the pixel})$ .<sup>20</sup> Results are reported in Table 2.5. Similar to our main specification, we present the results for different bandwidths of the spatial RDD. Even-numbered columns report results for the subsample of pixels close to the sea (below the median distance to the coast), while odd-numbered columns display estimates for pixels that are far from the sea (above the median distance to the coast).

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<sup>18</sup>For instance, [Michalopoulos and Papaioannou \(2014\)](#) exploit the median distance to the capital city to examine heterogeneity in the relationship between national institutions and subnational development in Africa. Similarly, [Okoye and Pongou \(2017\)](#) use the median distance to ports to assess the heterogeneous impacts of colonial railways in Nigeria.

<sup>19</sup>This variable takes the value 1 if the average light density in the pixel is strictly greater than zero and takes the value 0 otherwise.

<sup>20</sup>Because light density is equal to zero in more than 50% of the sample we used a logarithmic transformation of the continuous measure of light density, following the same approach as in [Michalopoulos and Papaioannou \(2013\)](#).

Regardless of how we define the outcome variable (*Panels A and B*), we find significant and large differences in light density between former British and French colonies only in areas far from the sea. In these areas, British colonization has a positive effect on light density, and thus local economic opportunities. Close to the sea, however, the effect of British colonization on light density is economically small and is not statistically significant. The finding is qualitatively the same when we consider different bandwidths of the spatial RDD or when we augment the specification with a rich set of geographic and location controls (*Panels C and D*). Results from Table 2.5 are thus consistent with our hypothesis, as they imply that coastal areas are similarly developed across colonial borders and areas far from the sea are more developed on the British side compared to the French side. This evidence is also strengthened by the non-significant association between the identity of the colonizer and the proximity to the sea coast (column (7) in Table 2.1), which ensures that our measure for economic opportunities (distance to the sea) is truly exogenous to the identity of the colonizer. Moreover, using other measures of economic development and household welfare such as child mortality and nutrition we show that the British advantage in these outcomes only appears in areas far from the coast (see Table 2.10).

### 2.7.3 Heterogeneous Effects of British Colonization on Reproductive Behavior

Having established that in areas closer to the sea, there are no systematic differences in economic opportunities across former British and French colonies, we can now examine the heterogeneous effects of British colonization on reproductive behavior by proximity to the sea, our measure of market access. To do so, we implement the RDD specification in equation (2), separately for observations falling in the pixels that are close and far from the sea. Results are displayed in Tables 2.6 and 2.7. As in the previous section, even-numbered columns show estimates for the subsample of pixels close to the sea, whereas in odd-numbered columns we report the effect of British colonization on reproductive behavior for pixels far from the coast. Figures 2.4-b and 2.4-c illustrate graphically the results from this analysis when we focus on the number of children ever born. In Figure 2.4-b, we restrict the analysis to observations close to the coast, while in Figure 2.4-c, we

restrict the analysis to observations that are far from the sea. The graphs clearly show that the jump at the border is larger and only present when we consider areas far from the coast. A similar pattern is observed for the other outcomes (second and third rows in Figure 2.5).

**Fertility.** Table 2.6 shows estimates of the heterogeneous impact of British colonization on the total number of children born to a woman. Each panel of this table presents estimates from three regressions that differ on whether and how they address the issue of spillover effects at the border. Consistent with the RD graphs and the theory, women in former British colonies have significantly fewer children than their counterparts in former French colonies only in areas far from the sea coast. In areas close to the coast, the effect is economically small and not significant. These results are consistent across different bandwidths of the spatial RDD. In *Panel A* when we consider the whole sample, the estimated British-French gap in fertility observable in areas far from the sea ranges from  $-0.45$  to  $-0.51$ . The effect changes little when we augment the model by controlling for geographic and location controls (*Panel B*). The estimated fertility gap in non-coastal areas is even larger when we account for spillover at the border. In fact, the estimated fertility gap ranges from  $-0.50$  to  $-0.59$  when we restrict the analysis to the sample of natives only (*Panels C* and *D*), and from  $-0.51$  to  $-0.64$  when observations in pixels within 5km of the border are dropped in addition to only focusing on natives (*Panels E* and *F*). In areas close to the coast, the effect of British colonization remains small and is not statistically different from zero.

**Other reproductive outcomes.** Table 2.7 shows the heterogeneous impact of British colonization on the other reproductive outcomes analyzed in this chapter: the likelihood of childbearing (*panel A*), age at first sexual intercourse (*panel B*), and the likelihood of marriage before 18 (*panel C*). Overall, for each outcome, the effect of British colonization is qualitatively similar to that found for the number of children. In absolute value, this effect is larger and globally significant in areas far from the sea.

Some results are worth highlighting. Whereas the “average” effect of British colonization on the likelihood of childbearing estimated over the whole sample is negative but not statistically significant (Table 2.3, *panel A*), this effect is now larger and significantly different from zero in areas far from the sea. Concerning the effect of British colonization on the probability of getting married before age 18, it is negative and statistically

significant in areas far from the coast. In areas close to the coast, this effect is much smaller and not significant. This result implies that women in former British colonies are more likely to delay marriage, and that this effect is only visible in areas far from the coast. Turning to the timing of first sexual intercourse, even though the coefficient on the British colonization dummy is positive and statistically significant in both areas far and close to the sea, the magnitude of the effect is larger in areas far from the sea. The findings are qualitatively similar when we account for spillover effects at the border (Table 2.3, panel D - I).

Overall, the results show that the effect of British colonization on reproductive outcomes is significantly larger in the hinterland as compared to coastal areas. These findings remarkably mirror the fact that the effect of British colonization on local economic development is only present in areas far from the coast. We can therefore conclude that colonial origins have little effect on reproductive outcomes when the opportunity cost of having a child is sufficiently high. In the next section, we document micro-founded mechanisms of the fertility impact of British colonization, which are mechanisms operating at the individual level.

## 2.8 Mechanisms

In our conceptual framework, we highlight two types of channels that may link colonial origins to fertility behavior in the African context. The first is a *direct* channel supported by differences in colonial population policies and reproductive health laws. This is our *primary* channel and we expect that through this channel, colonial origins will have a direct impact on the use of modern methods of birth control. The second type is a class of *indirect* and *secondary* channels including: (1) female education; (2) female economic empowerment; and (3) child quality. These indirect channels proceed from the theoretical literature on the drivers of fertility (Mincer (1963), Becker and Lewis (1973), Galor and Weil (1996), Strulik (2017), Doepke and Tertilt (2018)). However, they may be theoretically less important than our primary mechanism because a large empirical literature shows that contraceptive use (our primary mechanism) has a positive effect on women's human capital, labor participation and child quality (health and education) through a reduction in fertility (Ananat and Hungerman (2012), Myers (2017)). Taking advantage

of the rich set of information provided in the DHS, we examine each of these channels by investigating the heterogeneous impact of British colonization by market access on their corresponding variables. Specifically, we implement an empirical analysis similar to the heterogeneous analysis conducted in Section 2.7.<sup>21</sup> If any of the aforementioned variables mediates the heterogeneous effect of British colonization on fertility outcomes, then British colonization should have a greater effect on this mediating variable in areas far from the coast.

**Contraceptive Use.** Our primary and *direct* mechanism implies that the use of modern (vs. traditional) methods of birth control should be greater in former British areas compared to former French areas. Moreover, the effect of British colonization on contraceptive use should be larger in the hinterland compared to coastal areas, consistent with our findings in Section 2.7 showing that the opportunity cost of having a child is high in the latter areas regardless of the colonizer’s identity. We explore this channel in Table 2.8. The outcome variable in this table is current use of modern methods of birth control. We define this variable as a binary indicator, which is equal to one if the respondent is using a modern method of birth control, and zero if the respondent is using a traditional method. Traditional methods of birth control include: periodic abstinence, abstinence, withdrawal, standard days method, and other methods. Modern methods include: pill, IUD, injections, diaphragm, condom, female sterilization, male sterilization, lactational amenorrhea, implants/norplant, female condom, and foam/jelly. Table 2.8 shows that women in former British colonies are significantly more likely to use a modern method of birth control in both coastal areas and the hinterland, and consistent with our predictions, this positive effect of British colonization is economically much larger in the hinterland. This corroborates the notion that economic opportunities induced by proximity to global markets raise the opportunity cost of childbearing, thus triggering a demand of more effective methods of birth controls regardless of colonial origins.<sup>22</sup>

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<sup>21</sup>We choose to present the heterogeneous effects of colonial origins on these variables because they provide a more precise and complete view of the possible mechanisms underlying our main results.

<sup>22</sup>Interestingly, Anderson (2018) finds that women in the British common law system are less likely to use methods of contraception that reduce their likelihood of contracting HIV than their counterparts in the French civil law system. It is important to note that our findings do not contradict the findings from this latter study, as in reality, our results are not directly comparable. *Protective* methods of contraception overlap but do not coincide with modern methods of birth control. For example, IUD, injections, diaphragm, condom, female sterilization, male sterilization, implants/norplant, and foam/jelly are modern methods of birth control but they do not protect against sexually transmitted diseases. Anderson (2018) acknowledges that women in the British common law system are more likely to use

Now we examine the *indirect* channel.

**Female Education.** Economic growth theory argues that an exogenous increase in the returns to investment in education raises the opportunity cost of childbearing, thereby lowering fertility demand. To test this channel, we define female education as the total number of years of education for each woman in our sample. We use this measure as our dependent variable in equation (2) and report results in Table 2.9 (*panels A, D and G*). We find that British colonization positively affects female education both in areas close to the sea and areas that are far from the sea. Interestingly, the effect is larger in coastal areas. Note that this is in contrast to the fact that the effect of British colonization on fertility behavior is much stronger in areas far from the coast. These findings clearly imply that education is not the main channel through which British colonization affects fertility in Africa. Moreover, consistent with the literature on the effect of lower fertility induced by an increased use of modern contraception on female economic empowerment, the stronger effect of British colonization on education in areas close to the sea can be seen as a by-product of the *direct* impact of British colonization on fertility in these areas.

Remark that one could argue that the effect of colonial origins on contraceptive use is mediated by female education. This is unlikely to be the case in our context because if this were the case, British colonization would have a greater effect on contraceptive use in coastal areas compared to the hinterland, mirroring its heterogeneous effect on female education. But this is not what we find. Unlike its effect on education, British colonization has a greater effect on contraceptive use in the hinterland compared to coastal areas, which is consistent with the notion that the latter effect is more direct.

**Female Economic Empowerment.** Female economic empowerment is another factor likely to mediate the effect of colonial origins on fertility behavior. We define female economic empowerment based on participation of women in the labor market. Taking advantage of the fact that DHS collect information on the respondent's type of occupation, we construct an indicator for female labor participation based on whether the respondent is engaged in activities requiring low skills. Unskilled workers include agricultural and domestic workers. We also construct an alternative indicator of female labor participation based on whether she receives cash earnings for her work. Using each

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some of these methods than their counterparts in the civil law system.

of these variables as a dependent variable in equation (2), we find that women in former British colonies are less likely to participate in unskilled jobs (*panels B, E, and H* in Table 2.9), and that they are more likely to work in a paid job (*panels C, F, and I* in Table 2.9). Moreover, we find that these effects are larger in areas far from the sea. In areas close to the sea, the effect of British colonization on the aforementioned measures of female economic empowerment is very small and it is not statistically significant.

Despite the fact that the heterogeneous effect of British colonization on female labor participation is consistent with its heterogeneous effect on fertility, this does not mean that female labor participation mediates the latter effect. This is for two reasons. The first reason is that the fertility gap between former British and French colonies preceded the income gap (Figure 2.1-c), which directly implies that the latter cannot explain the former. Second following a similar approach as in Anderson (2018), we directly control for female education and income (household assets and light density) when estimating the effect of colonial origins on fertility in areas far from the sea. We find that this effect is strongly robust to these controls (Table A2.6)). Taken together, these findings clearly show that female economic empowerment is not a primary channel through which colonial origins affects fertility. On the contrary, they suggest that higher income in former British colonies is partly a result of their lower fertility, which is consistent with the patterns shown in Figure 2.1-c.

**Child Quality.** The divergence in reproductive behavior across former British and French colonies could also be the result of the demand for child quality being higher in former British colonies. We test this channel by assessing the long-term impact of colonial origins on a set of variables measuring child quality. These variables are the number of dead children born to a woman, a binary indicator for whether a child died before the age of five years old, and a binary indicator for whether a child is mildly wasted.<sup>23</sup> These variables have been used to measure both child quality and household welfare in a number of studies (Millimet and Wang (2011), Liu (2014), Bhattacharjee and Dasgupta (2016)). Results are reported in Table 2.10. We find that Anglophone women have significantly fewer dead children relative to their Francophone counterparts, and that this effect is only present in areas far from the sea (*panels A, D, and G*).

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<sup>23</sup>Mild wasting equals one if the child's weight-for-height is below zero standard deviation from the median weight-for-height of the reference population.

Similarly, we find that the likelihood of mild wasting is significantly lower for children residing in former British colonies only in areas far from the coast (*panels B, E, and H* of Table 2.10). Similar effects are found for under-five mortality, but in this latter case, the effects are generally not statistically significant (*panels C, F, and I* of Table 2.10). These findings are consistent with the fact that the British advantage in local economic development is only visible in areas far from the coast. Consistent with the theoretical literature on the drivers of fertility, they suggest that child quality is perhaps an indirect channel through which British colonization affects fertility in the hinterland. However, following the literature on the effect of contraceptive use on child quality (Ananat and Hungerman (2012)), higher child quality in former British colonies may also be the result of increased use of modern contraception and lower fertility in these colonies.

Overall, the results presented in this section are consistent with the idea that the negative effect of British colonization on fertility outcomes in areas far from the sea operates primarily through its direct effect on modern methods of birth control. The analysis provides little support for the indirect mechanism.

## 2.9 Conclusion

The large literature that documents the long-term economic effects of history has overlooked the question of whether these effects can be mitigated by appropriately designed policies. In this chapter, we address this important question by studying the long-term effect of colonial reproductive laws on fertility behavior in Africa. Central to our study is the analysis of how this effect varies by exogenous market access, a proxy for the opportunity cost of childbearing.

Implementing a spatial Regression Discontinuity Design with ethnic homeland fixed effects, we find that women in former British colonies have significantly fewer children than their counterparts in former French colonies. They are also more likely to delay sexual debut and marriage. However, these effects disappear in areas with high market access, and are only present in areas with low market access. The analysis therefore suggests that market incentives are likely to mitigate the long-term impact of history.

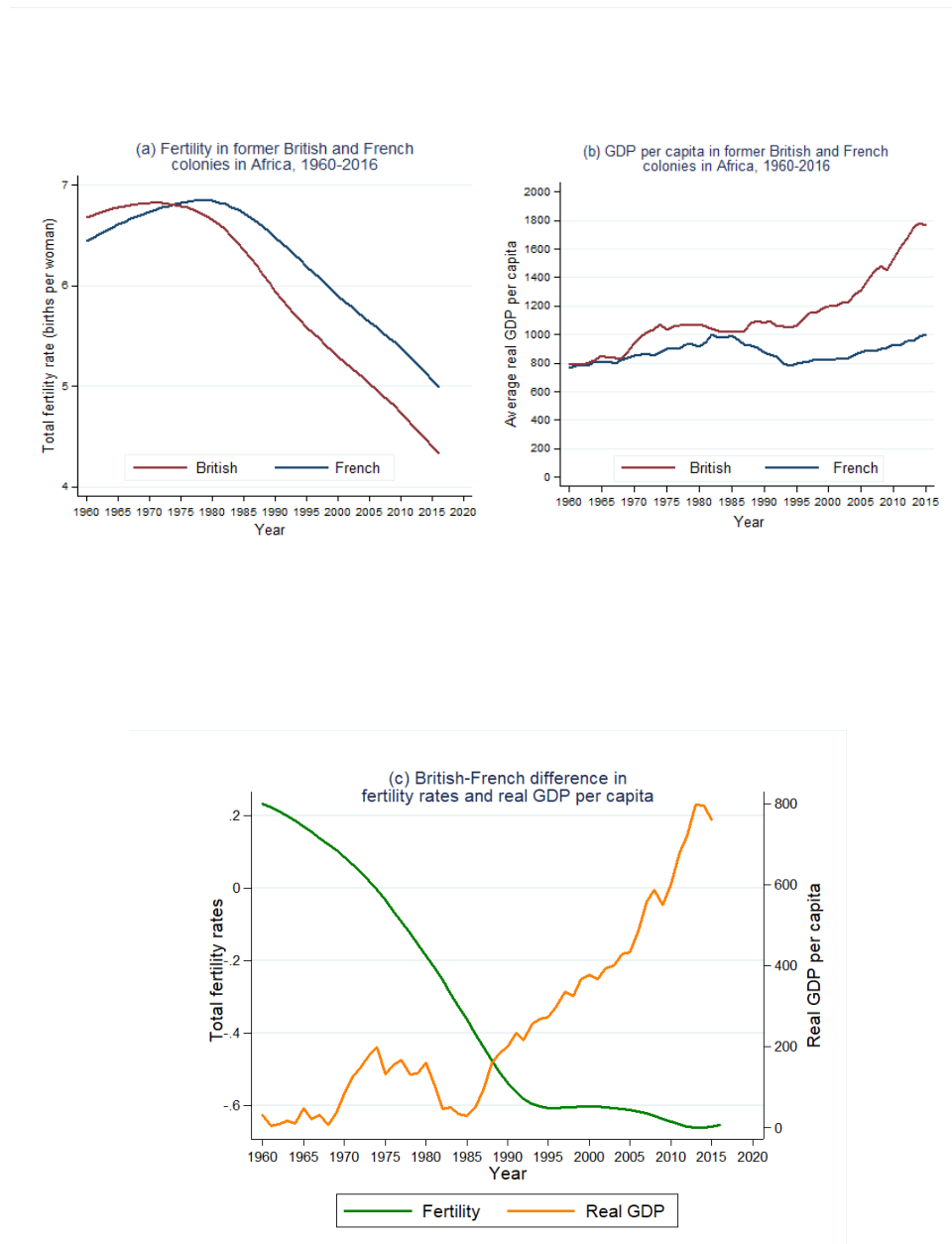
Examining causal mechanisms, we find that the fertility effect of colonial origins can

be directly linked to colonial reproductive laws and their impact on the use of modern methods of birth control. Importantly, we rule out the impact of colonial origins on income and women's human capital as the primary channels through which their fertility effect operates.

Besides uncovering novel findings about the heterogeneous nature of the colonial origins of comparative fertility behavior and economic development in Africa, our study contributes directly to the debate about the nature of policy actions that can be undertaken to repair the damages of bad historical shocks. Taken together, our findings imply that appropriately designed economic incentives can overcome the bonds of historical determinism, even though history itself cannot be changed.

## 2.10 Figures

Figure 2.1: British-French Gaps in Fertility and GDP in Africa, 1960-2016



Source: World Development Indicators and [Bergh and Fink \(2018\)](#)

Figure 2.2: Conceptual Framework

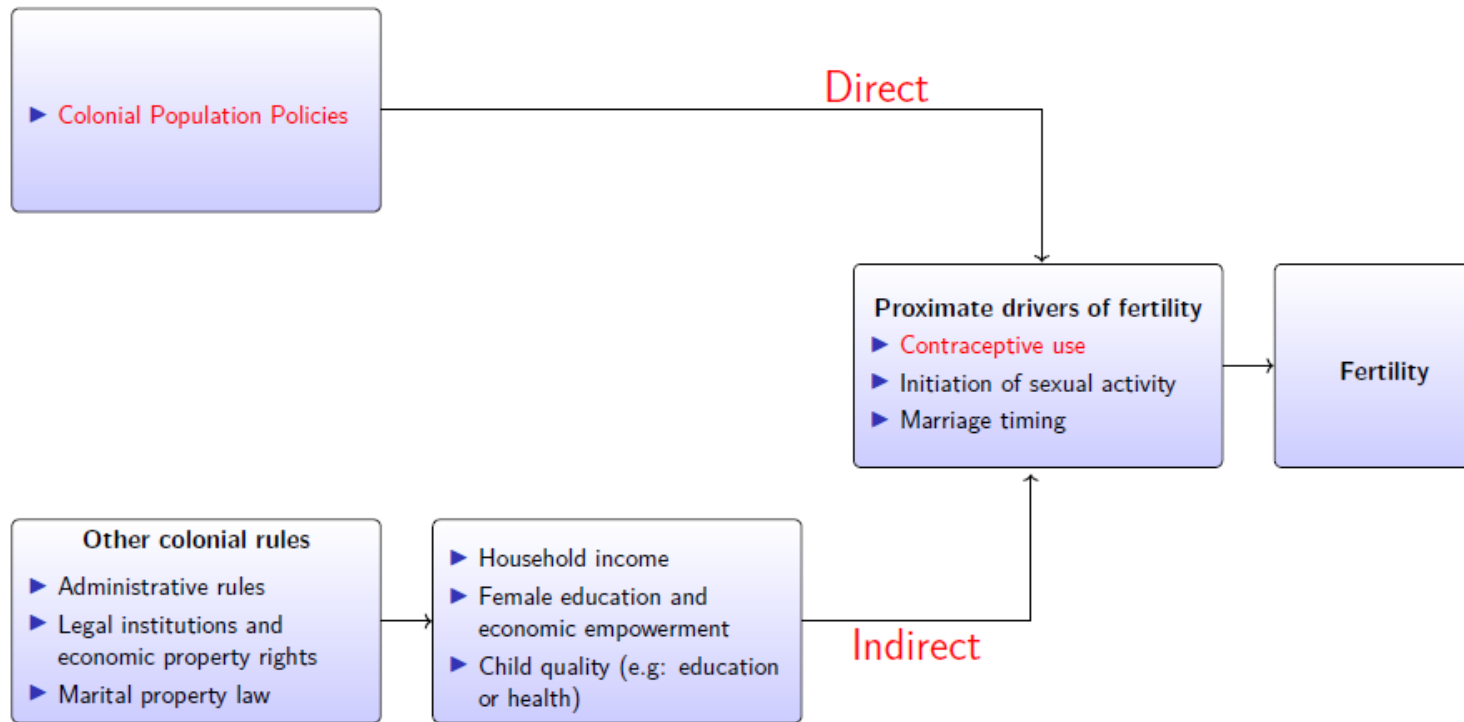


Figure 2.3: Ancestral ethnic homelands and contemporary national boundaries in Africa

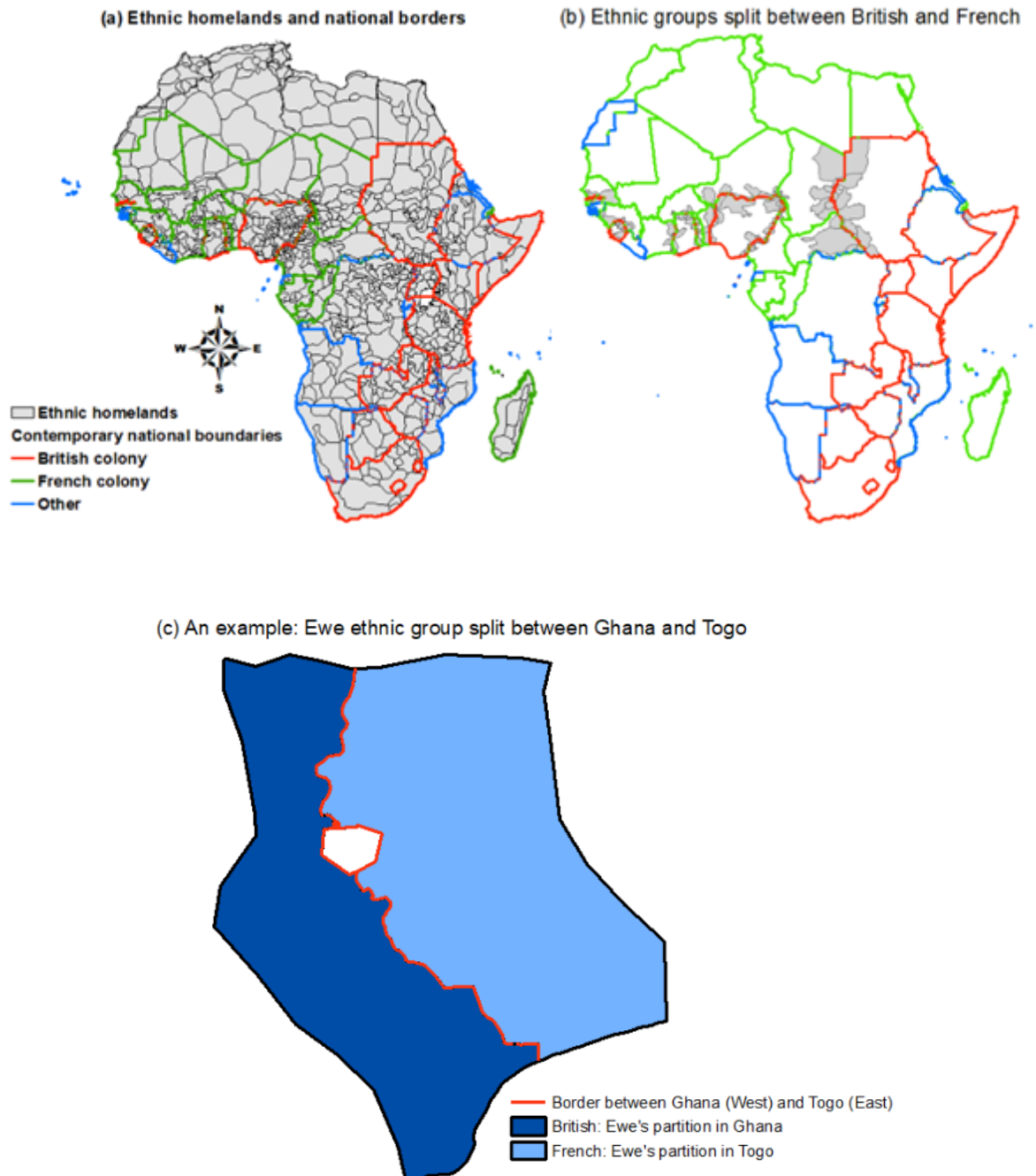


Figure 2.4: Colonial origins and fertility

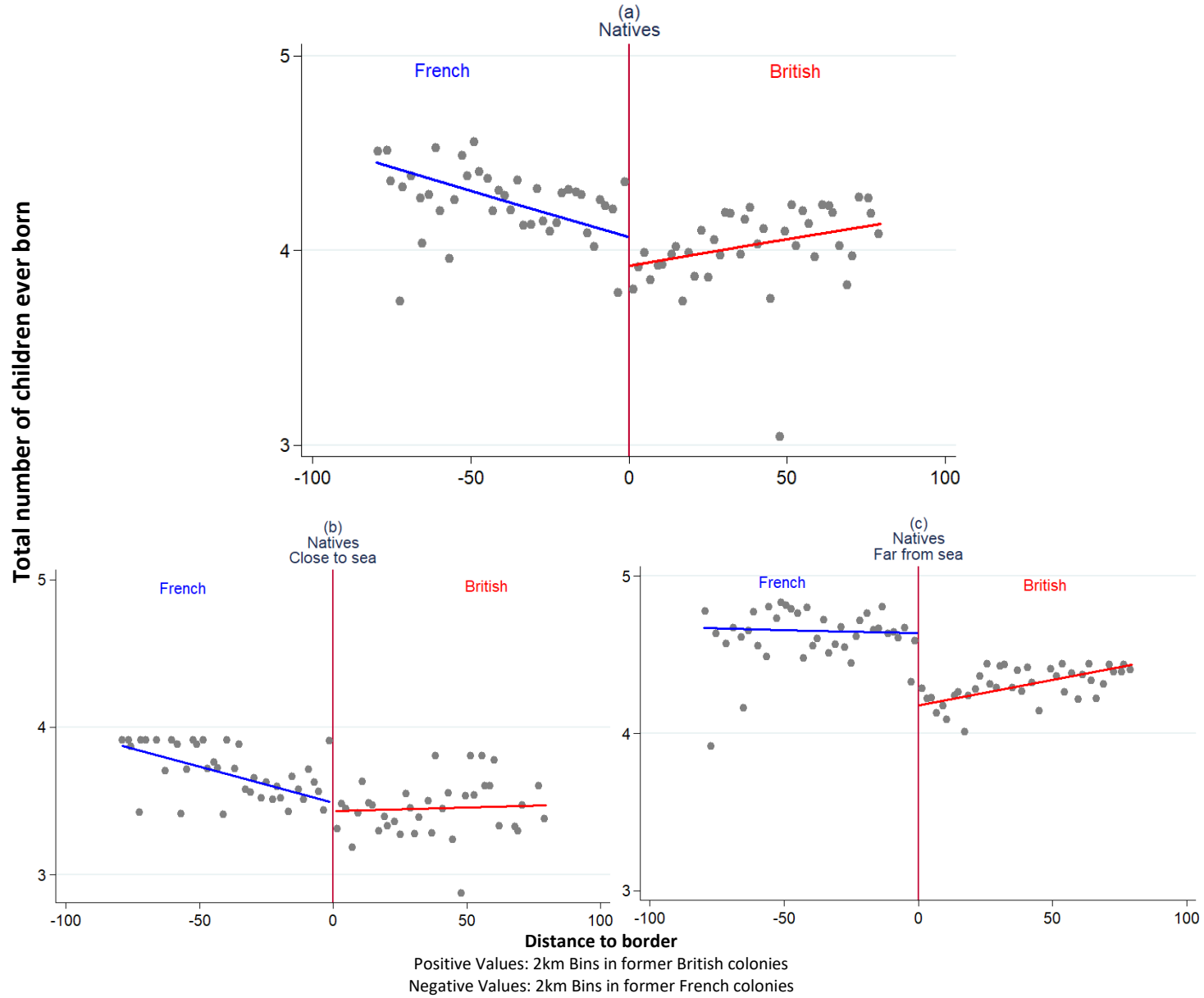
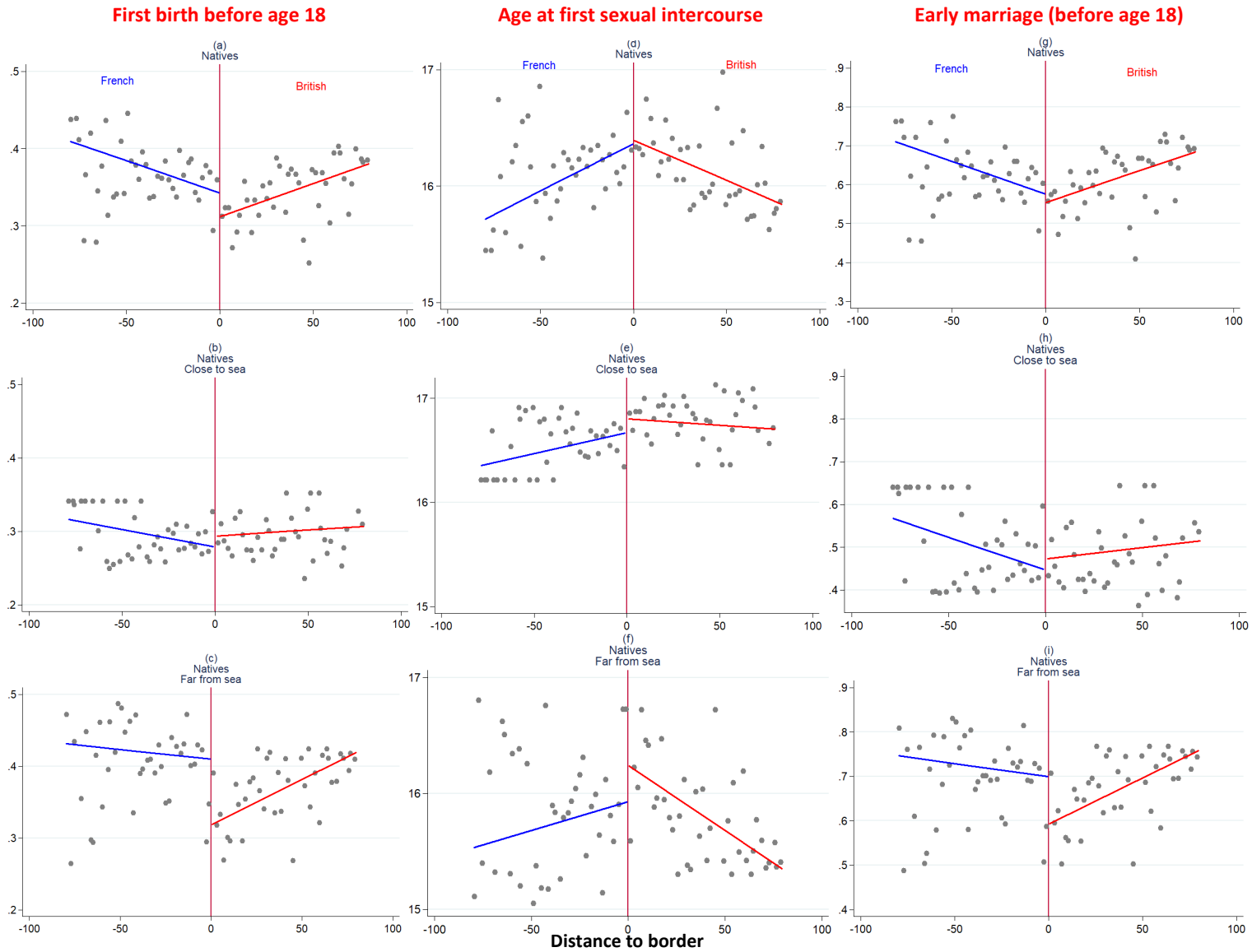
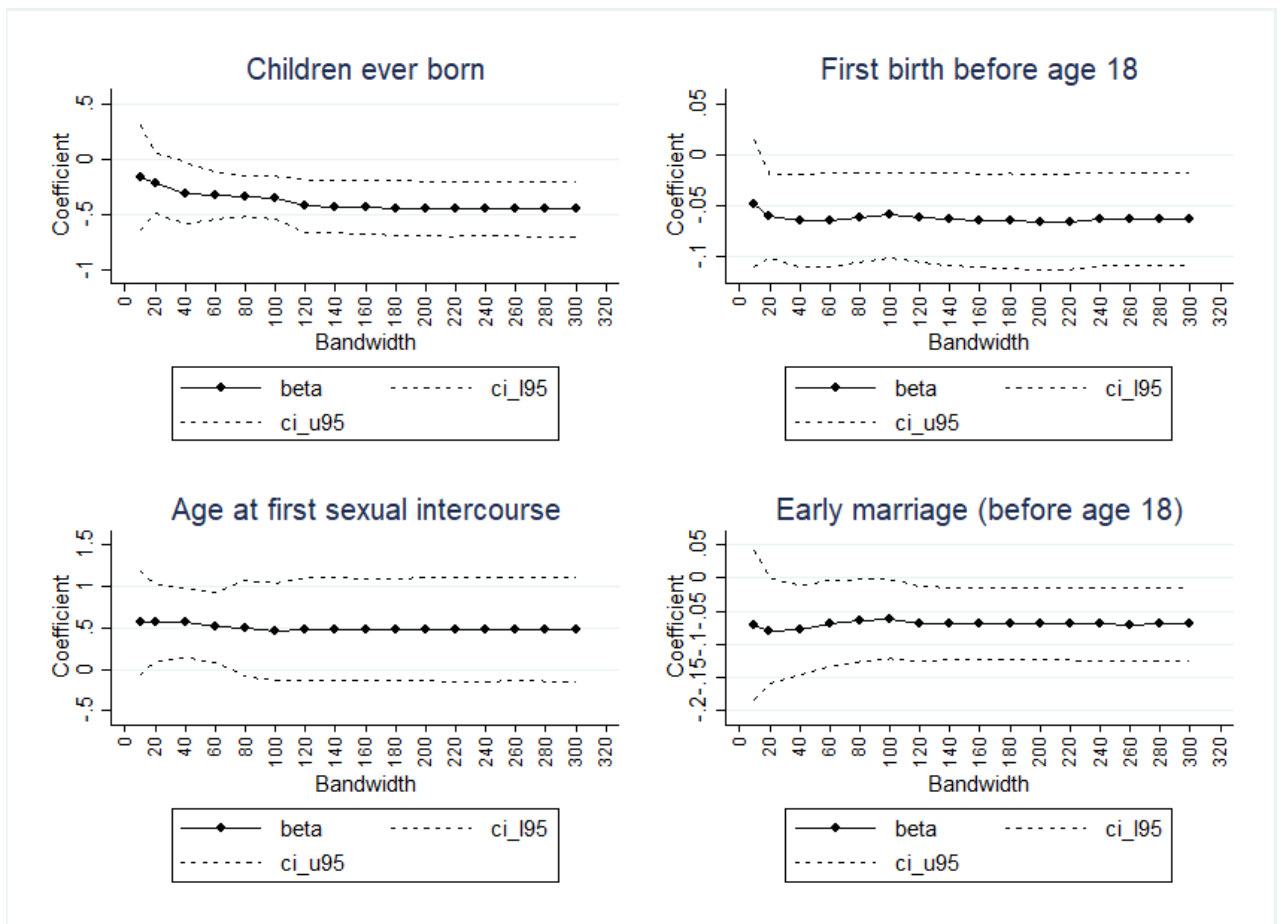


Figure 2.5: Colonial origins and other outcomes



Positive Values: 2km Bins in former British colonies  
Negative Values: 2km Bins in former French colonies

Figure 2.6: Colonial origins and reproductive behavior: RDD estimates for different bandwidths



## 2.11 Tables

Table 2.1: Validity of identification design at the pixel level

	Dependent variable is:						
	ln(Mean elevation) (1)	ln(Soil suitability) (2)	ln(Area under water) (3)	ln(Pixel area) (4)	ln(Diamonds) (5)	ln(Dist. to the border) (6)	ln(Dist. to sea) (7)
British (vs. French)	-10.62 (14.076)	0.03 (0.021)	0.38 (0.333)	-0.03 (0.057)	0.00 (0.004)	-0.03 (0.038)	-0.10 (0.065)
N	998	980	208	998	998	998	998
Countries	10	10	10	10	10	10	10
Ethno-linguistic families	23	23	23	23	23	23	23
Ethnicity F.E	✓	✓	✓	✓	✓	✓	✓

*Note: In each specification, the unit of observation is the pixel. The table reports RDD estimates associating various location and geographical characteristics at the pixel level with the colonial origin. In each specification we restrict the analysis to observations within 60km of the border and we control for a second-order polynomial in the distance from the centroid of each pixel to the ethnic border. Below the estimates, we report in parentheses standard errors clustered both at the country and the ethno-linguistic family levels. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .*

Table 2.2: Average effect - Colonial origins and fertility

	RDD - Bandwidth				
	OLS (1)	<60 km of bound. (2)	<100 km of bound. (3)	<150 km of bound. (4)	<200 km of bound. (5)
<b>Dependent variable: Total number of children ever born</b>					
<b>Panel A: Baseline specification</b>					
British (vs. French)	-0.44*** (0.112)	-0.33*** (0.107)	-0.35*** (0.097)	-0.43*** (0.118)	-0.45*** (0.123)
Observations	34,328	21,617	28,570	31,971	33,231
Countries	10	10	10	10	10
Ethno-linguistic families	23	23	23	23	23
<b>Panel B: Geographic controls</b>					
British (vs. French)	-0.41*** (0.077)	-0.34*** (0.109)	-0.35*** (0.081)	-0.40*** (0.078)	-0.41*** (0.081)
Observations	33,665	21,155	27,907	31,308	32,568
Countries	10	10	10	10	10
Ethno-linguistic families	23	23	23	23	23
<b>Panel C: Natives</b>					
British (vs. French)	-0.51*** (0.155)	-0.39** (0.156)	-0.44*** (0.168)	-0.51*** (0.174)	-0.52*** (0.177)
Observations	13,919	8,398	11,313	12,795	13,353
Countries	10	10	10	10	10
Ethno-linguistic families	22	22	22	22	22
<b>Panel D: Natives + Geographic controls</b>					
British (vs. French)	-0.45*** (0.141)	-0.38** (0.166)	-0.40*** (0.154)	-0.42*** (0.135)	-0.44*** (0.140)
Observations	13,687	8,215	11,081	12,563	13,121
Countries	10	10	10	10	10
Ethno-linguistic families	22	22	22	22	22
<b>Panel E: Natives + Thick border</b>					
British (vs. French)	-0.59*** (0.102)	-0.46*** (0.116)	-0.52*** (0.128)	-0.59*** (0.119)	-0.61*** (0.120)
Observations	12,600	7,079	9,994	11,476	12,034
Countries	10	10	10	10	10
Ethno-linguistic families	22	22	22	22	22
<b>Panel F: Natives + Thick border + Geographic controls</b>					
British (vs. French)	-0.53*** (0.104)	-0.49*** (0.130)	-0.48*** (0.123)	-0.51*** (0.104)	-0.52*** (0.117)
Observations	12,390	6,918	9,784	11,266	11,824
Countries	10	10	10	10	10
Ethno-linguistic families	22	22	22	22	22
Ethnic homeland FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓

Note: Specifications in columns (2)-(5) control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2.3: Average effect - Colonial origins and other reproductive outcomes

	RDD - Bandwidth				
	OLS (1)	<60 km of bound. (2)	<100 km of bound. (3)	<150 km of bound. (4)	<200 km of bound. (5)
<b>Baseline specification</b>					
<b>Panel A: First birth before age 18</b>					
British (vs. French)	-0.05*** (0.020)	-0.06*** (0.023)	-0.06*** (0.020)	-0.06*** (0.021)	-0.06*** (0.021)
Observations	30,477	19,098	25,161	28,284	29,474
<b>Panel B: Age at first sexual intercourse</b>					
British (vs. French)	0.47 (0.299)	0.53*** (0.199)	0.46* (0.280)	0.48* (0.280)	0.48* (0.291)
Observations	28,008	17,066	23,002	25,912	27,088
<b>Panel C: Early marriage (before 18 years old)</b>					
British (vs. French)	-0.06* (0.031)	-0.07* (0.035)	-0.06* (0.032)	-0.06** (0.030)	-0.06** (0.030)
Observations	31,769	19,775	26,168	29,446	30,699
Countries	10	10	10	10	10
Ethno-linguistic families	23	23	23	23	23
<b>Natives</b>					
<b>Panel D: First birth before age 18</b>					
British (vs. French)	-0.06*** (0.023)	-0.07*** (0.026)	-0.07*** (0.025)	-0.07*** (0.023)	-0.07*** (0.023)
Observations	12,323	7,357	9,940	11,286	11,813
<b>Panel E: Age at first sexual intercourse</b>					
British (vs. French)	0.38 (0.277)	0.53** (0.233)	0.43* (0.226)	0.44* (0.238)	0.44* (0.250)
Observations	11,471	6,832	9,281	10,490	11,011
<b>Panel F: Early marriage (before 18 years old)</b>					
British (vs. French)	-0.06* (0.032)	-0.07** (0.034)	-0.06* (0.032)	-0.06** (0.031)	-0.06* (0.032)
Observations	12,785	7,570	10,271	11,690	12,244
Countries	10	10	10	10	10
Ethno-linguistic families	22	22	22	22	22
<b>Natives + Thick border</b>					
<b>Panel G: First birth before age 18</b>					
British (vs. French)	-0.08*** (0.020)	-0.09*** (0.022)	-0.09*** (0.020)	-0.08*** (0.019)	-0.08*** (0.019)
Observations	11,255	6,289	8,872	10,218	10,745
<b>Panel H: Age at first sexual intercourse</b>					
British (vs. French)	0.36 (0.291)	0.56*** (0.212)	0.44* (0.229)	0.45* (0.235)	0.44* (0.253)
Observations	10,410	5,771	8,220	9,429	9,950
<b>Panel I: Early marriage (before 18 years old)</b>					
British (vs. French)	-0.07** (0.031)	-0.09*** (0.032)	-0.07** (0.031)	-0.08** (0.030)	-0.08** (0.030)
Observations	11,666	6,451	9,152	10,571	11,125
Countries	10	10	10	10	10
Ethno-linguistic families	22	22	22	22	22
Ethnic homeland FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓

Note: Specifications in columns (2)-(5) control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2.4: Distance to sea, local economic development, and the opportunity cost of child-bearing

	Light Density (1)	High-skilled workers (2)	Cash earning (3)	Asset (4)
ln(distance to sea)	-1.56*** (0.228)	-0.02*** (0.006)	-0.11*** (0.033)	-0.07*** (0.014)
Observations	760	31,957	13,507	33,467
Mean dep. var.	-2.84	0.02	0.58	0.73
Ethnic homeland FE	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓
Countries	10	10	10	10
Ethno-linguistic families	23	23	23	23

*Note:* Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2.5: Colonial origins and light density

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Panel A: Pixel is lit</b>								
British (vs. French)	0.02 (0.136)	0.16*** (0.030)	0.04 (0.148)	0.15*** (0.051)	0.05 (0.154)	0.15*** (0.052)	0.05 (0.154)	0.16*** (0.052)
Observations	499	499	645	644	724	723	753	752
Countries	8	8	8	9	8	9	8	8
Ethno-linguistic families	14	14	14	16	16	14	17	12
<b>Panel B: Log of light density</b>								
British (vs. French)	0.09 (0.632)	0.65*** (0.143)	0.35 (0.855)	0.64*** (0.215)	0.43 (0.893)	0.65*** (0.215)	0.43 (0.897)	0.68*** (0.217)
Observations	499	499	645	644	724	723	753	752
Countries	8	8	8	9	8	9	8	8
Ethno-linguistic families	14	14	14	16	16	14	17	12
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Panel A: Pixel is lit</b>								
British (vs. French)	-0.01 (0.152)	0.15*** (0.042)	-0.01 (0.139)	0.13** (0.058)	-0.02 (0.139)	0.13** (0.058)	-0.02 (0.138)	0.15** (0.057)
Observations	481	499	625	644	704	723	733	752
Countries	8	8	8	9	8	9	8	8
Ethno-linguistic families	14	14	14	16	16	14	17	12
<b>Panel B: Log of light density</b>								
British (vs. French)	-0.08 (0.648)	0.64*** (0.209)	-0.03 (0.644)	0.56** (0.267)	-0.09 (0.670)	0.57** (0.262)	-0.08 (0.669)	0.64** (0.266)
Observations	481	499	625	644	704	723	733	752
Countries	8	8	8	9	8	9	8	8
Ethno-linguistic families	14	14	14	16	16	14	17	12
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origins. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2.6: Heterogeneity by proximity to the sea coast: colonial origins and fertility

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Dependent variable: Total number of children ever born</b>								
<b>Panel A: Baseline specification</b>								
British (vs. French)	-0.10 (0.117)	-0.46*** (0.076)	-0.15 (0.195)	-0.45*** (0.084)	-0.24 (0.263)	-0.49*** (0.084)	-0.21 (0.237)	-0.51*** (0.086)
Observations	9,598	12,019	12,387	16,183	13,855	18,116	14,416	18,815
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Panel B: Geographic controls</b>								
British (vs. French)	-0.09 (0.106)	-0.47*** (0.096)	-0.13 (0.119)	-0.42*** (0.083)	-0.17 (0.125)	-0.47*** (0.073)	-0.14 (0.112)	-0.52*** (0.076)
Observations	9,136	12,019	11,724	16,183	13,192	18,116	13,753	18,815
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Panel C: Natives</b>								
British (vs. French)	0.05 (0.197)	-0.52*** (0.107)	-0.06 (0.315)	-0.53*** (0.121)	-0.18 (0.376)	-0.57*** (0.111)	-0.15 (0.341)	-0.59*** (0.110)
Observations	3,187	5,211	4,179	7,134	4,823	7,972	5,049	8,304
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Panel D: Natives + Geographic controls</b>								
British (vs. French)	0.07 (0.167)	-0.55*** (0.129)	-0.01 (0.188)	-0.50*** (0.125)	-0.05 (0.192)	-0.53*** (0.091)	-0.03 (0.177)	-0.58*** (0.091)
Observations	3,004	5,211	3,947	7,134	4,591	7,972	4,817	8,304
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Panel E: Natives + Thick border</b>								
British (vs. French)	-0.09 (0.249)	-0.55*** (0.111)	-0.23 (0.343)	-0.57*** (0.104)	-0.35 (0.357)	-0.62*** (0.084)	-0.32 (0.322)	-0.64*** (0.080)
Observations	2,355	4,724	3,339	6,655	3,970	7,506	4,193	7,841
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Panel F: Natives + Thick border + Geographic controls</b>								
British (vs. French)	-0.09 (0.247)	-0.58*** (0.139)	-0.18 (0.226)	-0.51*** (0.133)	-0.23 (0.232)	-0.55*** (0.077)	-0.18 (0.232)	-0.61*** (0.076)
Observations	2,194	4,724	3,129	6,655	3,760	7,506	3,983	7,841
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2.7: Heterogeneity by proximity to the sea coast: colonial origins and other reproductive outcomes

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Baseline specification</b>								
<b>Panel A: First birth before age 18</b>								
British (vs. French)	-0.02 (0.032)	-0.09*** (0.016)	-0.03 (0.029)	-0.07*** (0.015)	-0.03 (0.029)	-0.08*** (0.015)	-0.02 (0.026)	-0.08*** (0.016)
Observations	8,011	11,087	10,206	14,955	11,540	16,744	12,042	17,432
<b>Panel B: Age at first sexual intercourse</b>								
British (vs. French)	0.28* (0.158)	0.80*** (0.268)	0.31* (0.170)	0.65* (0.357)	0.31* (0.190)	0.63* (0.343)	0.29* (0.177)	0.62* (0.350)
Observations	7,264	9,802	9,580	13,422	10,943	14,969	11,478	15,610
<b>Panel C: Early marriage (before 18 years old)</b>								
British (vs. French)	-0.01 (0.040)	-0.09** (0.038)	-0.03 (0.038)	-0.07 (0.046)	-0.04 (0.036)	-0.09** (0.042)	-0.03 (0.030)	-0.09** (0.044)
Observations	8,163	11,612	10,479	15,689	11,851	17,595	12,377	18,322
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Natives</b>								
<b>Panel D: First birth before age 18</b>								
British (vs. French)	-0.05 (0.048)	-0.10*** (0.018)	-0.06 (0.051)	-0.07*** (0.020)	-0.05 (0.043)	-0.08*** (0.019)	-0.04 (0.037)	-0.08*** (0.020)
Observations	2,578	4,779	3,383	6,557	3,969	7,317	4,172	7,641
<b>Panel E: Age at first sexual intercourse</b>								
British (vs. French)	0.39* (0.215)	0.72** (0.300)	0.38* (0.214)	0.47 (0.343)	0.36 (0.224)	0.46 (0.366)	0.30 (0.200)	0.49 (0.366)
Observations	2,541	4,291	3,380	5,901	3,971	6,519	4,180	6,831
<b>Panel F: Early marriage (before 18 years old)</b>								
British (vs. French)	-0.05 (0.050)	-0.09** (0.038)	-0.08 (0.048)	-0.06 (0.040)	-0.07* (0.043)	-0.08* (0.040)	-0.06 (0.036)	-0.08** (0.042)
Observations	2,590	4,980	3,429	6,842	4,017	7,673	4,227	8,017
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Natives + Thick border</b>								
<b>Panel G: First birth before age 18</b>								
British (vs. French)	-0.07 (0.060)	-0.11*** (0.020)	-0.09 (0.054)	-0.08*** (0.023)	-0.07 (0.042)	-0.09*** (0.022)	-0.05 (0.040)	-0.09*** (0.023)
Observations	1,949	4,340	2,747	6,125	3,323	6,895	3,523	7,222
<b>Panel H: Age at first sexual intercourse</b>								
British (vs. French)	0.43 (0.268)	0.60*** (0.232)	0.46* (0.267)	0.35 (0.281)	0.38 (0.243)	0.33 (0.287)	0.28 (0.237)	0.34 (0.275)
Observations	1,902	3,869	2,733	5,487	3,312	6,117	3,518	6,432
<b>Panel I: Early marriage (before 18 years old)</b>								
British (vs. French)	-0.07 (0.056)	-0.10*** (0.039)	-0.10* (0.055)	-0.07* (0.043)	-0.09** (0.039)	-0.08* (0.044)	-0.06** (0.032)	-0.09** (0.046)
Observations	1,941	4,510	2,773	6,379	3,348	7,223	3,555	7,570
Countries	7	9	7	9	8	9	8	9
Ethnic Families	9	17	9	18	12	15	14	14
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2.8: Colonial origins and use of modern methods of birth control

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Dependent variable is current use of any modern method of contraception</b>								
<b>Panel A: Baseline specification</b>								
British (vs. French)	0.20*** (0.039)	0.34*** (0.090)	0.23*** (0.051)	0.32*** (0.096)	0.23*** (0.044)	0.39*** (0.073)	0.21*** (0.038)	0.41*** (0.065)
Observations	2,018	1,216	2,600	1,441	2,881	1,425	2,963	1,388
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Panel B: Natives</b>								
British (vs. French)	0.14** (0.055)	0.40** (0.163)	0.19*** (0.070)	0.34** (0.168)	0.20*** (0.078)	0.42*** (0.100)	0.17*** (0.066)	0.43*** (0.097)
Observations	652	496	812	591	935	554	962	546
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Panel C: Natives + Thick border</b>								
British (vs. French)	0.10* (0.053)	0.36** (0.164)	0.16** (0.072)	0.30 (0.198)	0.20** (0.096)	0.43*** (0.138)	0.17** (0.083)	0.44*** (0.130)
Observations	427	453	583	552	705	516	732	508
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Modern methods of contraception include pill, IUD, injections, diaphragm, condom, female sterilization, male sterilization, lactational amenorrhea, implants/norplant, female condom, and foam/jelly. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of Cameron et al. (2011), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2.9: Colonial origins and female empowerment

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Baseline specification</b>								
<b>Panel A: Female years of education</b>								
British (vs. French)	2.76*** (0.486)	0.75*** (0.283)	2.85*** (0.580)	0.68** (0.322)	2.81*** (0.633)	0.76** (0.297)	2.65*** (0.676)	0.93*** (0.293)
Observations	9,129	12,012	11,716	16,173	13,183	18,106	13,743	18,806
<b>Panel B: Unskilled workers</b>								
British (vs. French)	0.03 (0.040)	-0.16*** (0.049)	0.02 (0.040)	-0.16*** (0.048)	0.04 (0.053)	-0.18*** (0.033)	0.06 (0.053)	-0.19*** (0.032)
Observations	7,723	11,780	10,296	15,854	11,761	17,771	12,320	18,468
<b>Panel C: Cash earning</b>								
British (vs. French)	-0.26 (0.182)	0.10* (0.053)	-0.18 (0.159)	0.09* (0.051)	-0.25* (0.150)	0.13*** (0.036)	-0.25* (0.140)	0.14*** (0.041)
Observations	2,858	4,790	4,567	6,610	5,529	7,154	5,846	7,249
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Natives</b>								
<b>Panel D: Female years of education</b>								
British (vs. French)	2.06*** (0.712)	0.71** (0.276)	2.18*** (0.710)	0.57* (0.312)	2.19*** (0.793)	0.64** (0.303)	2.07** (0.815)	0.81*** (0.284)
Observations	3,001	5,209	3,943	7,132	4,587	7,969	4,813	8,301
<b>Panel E: Unskilled workers</b>								
British (vs. French)	0.08 (0.058)	-0.17*** (0.048)	0.05 (0.046)	-0.16*** (0.052)	0.08 (0.061)	-0.18*** (0.037)	0.09 (0.060)	-0.19*** (0.037)
Observations	2,726	5,106	3,667	6,981	4,310	7,817	4,536	8,148
<b>Panel F: Cash earning</b>								
British (vs. French)	-0.28** (0.122)	0.09 (0.065)	-0.16 (0.122)	0.05 (0.076)	-0.24** (0.114)	0.08*** (0.026)	-0.25*** (0.079)	0.10*** (0.025)
Observations	1,278	2,010	1,979	2,643	2,442	2,681	2,590	2,698
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Natives + Thick border</b>								
<b>Panel G: Female years of education</b>								
British (vs. French)	2.81*** (0.824)	0.84*** (0.310)	3.06*** (0.766)	0.68* (0.363)	2.91*** (0.856)	0.76** (0.367)	2.73*** (0.921)	0.94*** (0.360)
Observations	2,192	4,722	3,126	6,653	3,757	7,503	3,980	7,838
<b>Panel H: Unskilled workers</b>								
British (vs. French)	0.07 (0.051)	-0.19*** (0.050)	0.02 (0.048)	-0.18*** (0.053)	0.08 (0.058)	-0.21*** (0.035)	0.09 (0.062)	-0.22*** (0.037)
Observations	2,051	4,626	2,984	6,509	3,614	7,358	3,837	7,692
<b>Panel I: Cash earning</b>								
British (vs. French)	-0.31*** (0.080)	0.08 (0.068)	-0.14 (0.136)	0.04 (0.083)	-0.30** (0.138)	0.08*** (0.029)	-0.32*** (0.112)	0.09*** (0.033)
Observations	1,026	1,798	1,720	2,438	2,176	2,483	2,323	2,501
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2.10: Colonial origins and child quality

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Baseline specification</b>								
<b>Panel A: Dead children</b>								
British (vs. French)	-0.03 (0.045)	-0.22** (0.091)	-0.06 (0.044)	-0.19** (0.073)	-0.08 (0.047)	-0.24*** (0.081)	-0.06 (0.043)	-0.28*** (0.084)
Observations	9,136	12,019	11,724	16,183	13,192	18,116	13,753	18,815
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Panel B: Mild wasting</b>								
British (vs. French)	0.06** (0.029)	-0.11*** (0.012)	0.07*** (0.023)	-0.09*** (0.025)	0.05** (0.019)	-0.07** (0.030)	0.04** (0.019)	-0.07** (0.027)
Observations	5,104	7,921	6,656	10,627	7,644	12,211	8,189	12,551
Countries	8	8	8	9	8	8	8	8
Ethno-linguistic families	14	14	14	14	17	12	17	11
<b>Panel C: Under-five mortality</b>								
British (vs. French)	0.00 (0.007)	-0.03* (0.017)	-0.01 (0.007)	-0.02* (0.012)	0.00 (0.012)	-0.03* (0.015)	0.00 (0.012)	-0.03* (0.015)
Observations	27,545	35,981	35,384	48,781	40,239	55,182	42,507	57,424
Countries	8	8	8	9	8	8	8	8
Ethno-linguistic families	14	14	14	14	17	12	17	11
<b>Natives</b>								
<b>Panel D: Dead children</b>								
British (vs. French)	-0.06 (0.079)	-0.20** (0.101)	-0.06 (0.082)	-0.15 (0.097)	-0.05 (0.076)	-0.20** (0.087)	-0.03 (0.072)	-0.22** (0.088)
Observations	3,004	5,211	3,947	7,134	4,591	7,972	4,817	8,304
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Panel E: Mild wasting</b>								
British (vs. French)	0.05 (0.043)	-0.07*** (0.021)	0.06 (0.038)	-0.07*** (0.017)	0.03 (0.023)	-0.06*** (0.021)	0.03 (0.022)	-0.05** (0.021)
Observations	1,651	3,503	2,115	4,794	2,540	5,427	2,759	5,620
Countries	8	8	8	9	8	8	8	8
Ethno-linguistic families	13	13	13	14	16	11	16	11
<b>Panel F: Under-five mortality</b>								
British (vs. French)	0.00 (0.014)	-0.03 (0.023)	0.00 (0.016)	-0.01 (0.014)	0.02 (0.021)	-0.02 (0.014)	0.02 (0.021)	-0.02 (0.014)
Observations	9,518	15,880	12,594	22,092	14,725	25,009	15,650	26,208
Countries	8	8	8	9	8	8	8	8
Ethno-linguistic families	13	12	13	14	16	11	16	11
<b>Natives + Thick border</b>								
<b>Panel G: Dead children</b>								
British (vs. French)	-0.11 (0.097)	-0.19* (0.104)	-0.12 (0.117)	-0.14* (0.086)	-0.08 (0.113)	-0.19** (0.077)	-0.06 (0.105)	-0.22*** (0.079)
Observations	2,194	4,724	3,129	6,655	3,760	7,506	3,983	7,841
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Panel H: Mild wasting</b>								
British (vs. French)	0.08* (0.049)	-0.05** (0.027)	0.08* (0.045)	-0.05** (0.024)	0.04 (0.027)	-0.04 (0.027)	0.04 (0.026)	-0.04 (0.026)
Observations	1,336	3,173	1,800	4,464	2,189	5,133	2,407	5,327
Countries	8	8	8	9	8	8	8	8
Ethno-linguistic families	13	13	13	14	16	11	16	11
<b>Panel I: Under-five mortality</b>								
British (vs. French)	-0.02 (0.014)	-0.02 (0.027)	-0.02 (0.019)	-0.01 (0.016)	0.02 (0.032)	-0.01 (0.016)	0.02 (0.032)	-0.01 (0.015)
Observations	7,675	14,361	10,751	20,573	12,800	23,572	13,723	24,773
Countries	8	8	8	9	8	8	8	8
Ethno-linguistic families	13	12	13	14	16	11	16	11
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin, the sex of the child, and fixed effects for child's year of birth. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of Cameron et al. (2011), are in parenthesis. Coefficients that are significantly different from 0 are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

## 2.12 Appendix

Figure A2.1: Colonial Population Laws

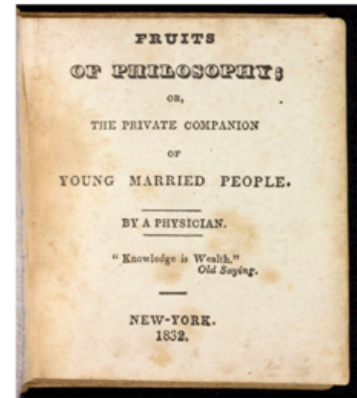


Art. 2. — Sera puni des mêmes peines quiconque aura vendu, mis en vente, ou fait vendre, distribuer, ou fait distribuer, de quelque manière que ce soit, des remèdes, substances, instruments ou objets quelconques, sachant qu'ils étaient destinés à commettre le crime d'avortement, lors même que cet avortement n'aurait été ni consommé, ni tenté, et alors même que ces remèdes, substances, instruments ou objets quelconques proposés comme moyens d'avortement efficaces seraient, en réalité, inaptes à les réaliser.

Art. 3. — Sera puni d'un mois à six mois de prison et d'une amende de cent francs (100 fr.) à cinq mille francs (5,000 fr.), quiconque, dans un but de propagande anticonceptionnelle, aura, par l'un des moyens spécifiés aux articles 1<sup>er</sup> et 2, décrit ou divulgué, ou offert de révéler des procédés propres à prévenir la grossesse, ou encore faciliter l'usage de ces procédés.

Les mêmes peines seront applicables à quiconque, par l'un des moyens énoncés à l'article 23 de la loi du 29 juillet 1881, se sera livré à une propagande anticonceptionnelle ou contre la natalité.

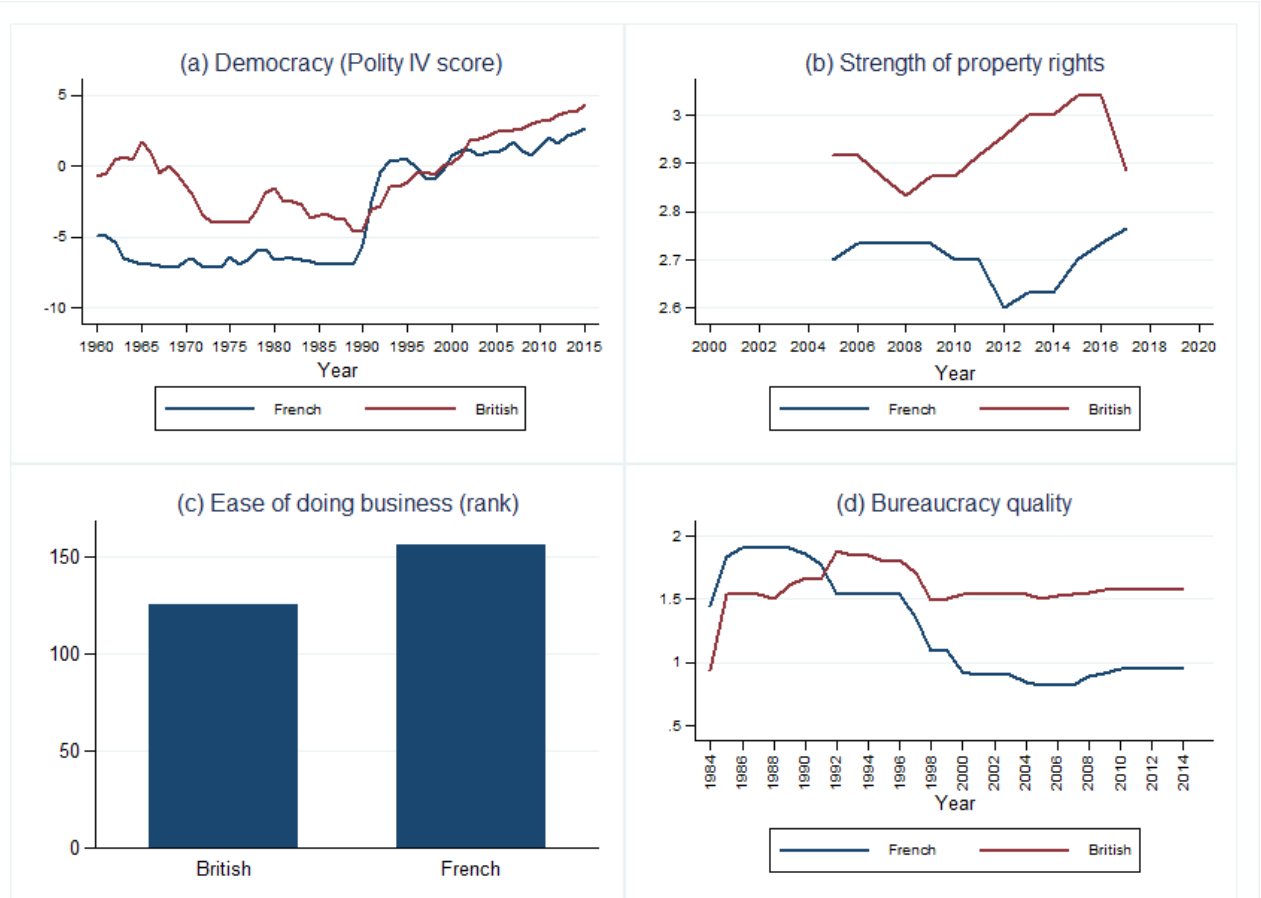
(a) French law of 1920



(b) Charles Knowlton's book and the trial of Annie Besant and Charles Bradlaugh

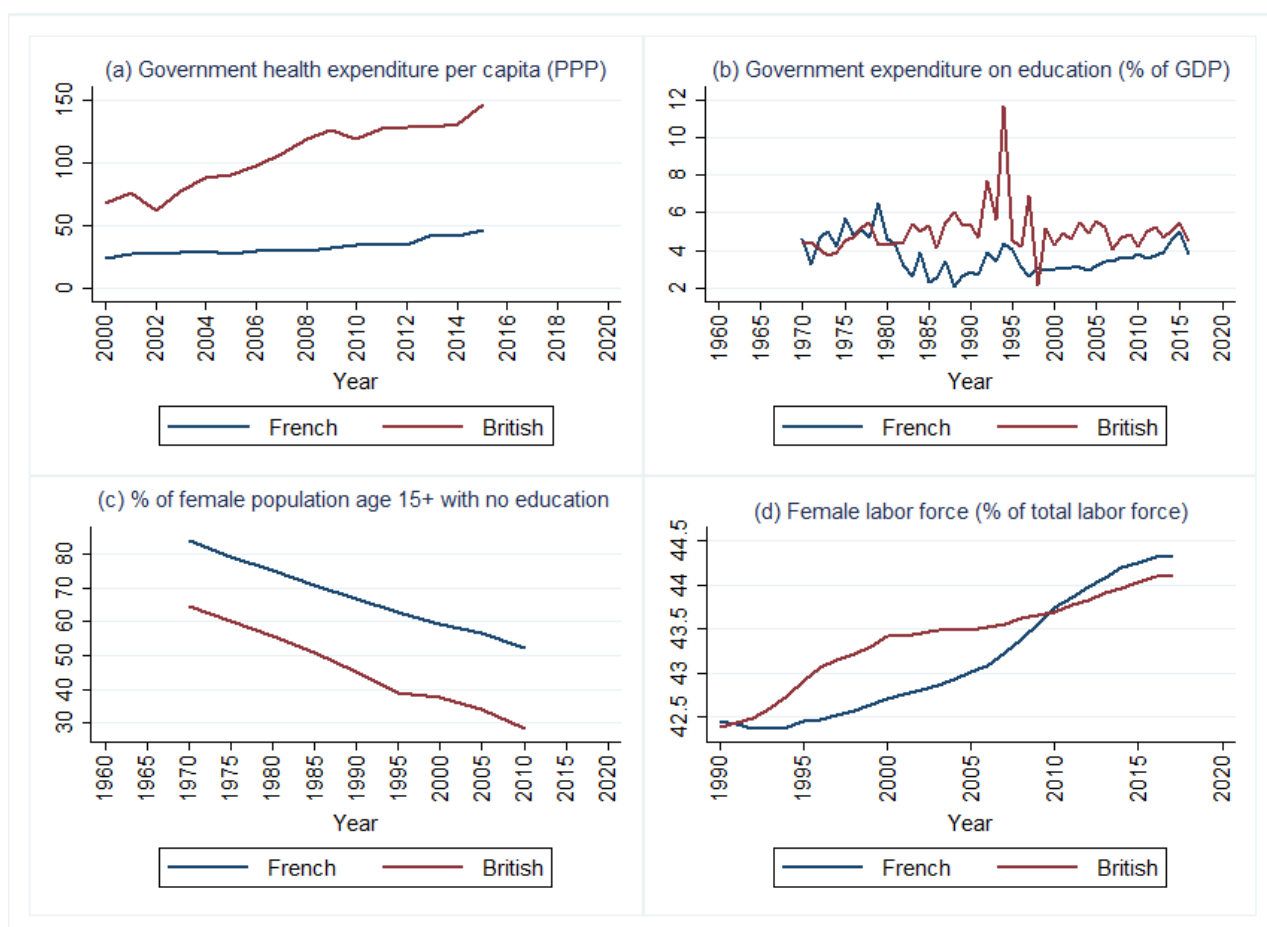
*Note: Figure-a displays selected pages of the "Journal Officiel de la République Française" published in 1920, which advertises the major legal official information for the national Government of France and the French Parliament. On the top of Figure-b is an image of the first page of the physician Charles Knowlton's book which contains information about contraception and was published by Annie Besant and Charles Bradlaugh. At the bottom of Figure-b, we display a portrait of the activists Annie Besant and Charles Bradlaugh drawn from the Internet.*

Figure A2.2: Colonial Origins and Present-day Institutions (1/2)



Source: Political Risk Services and World Bank (Doing Business)

Figure A2.3: Colonial Origins and Present-day Institutions (2/2)



Source: World Development Indicators

Figure A2.4: Colonial origins and fertility with baseline sample

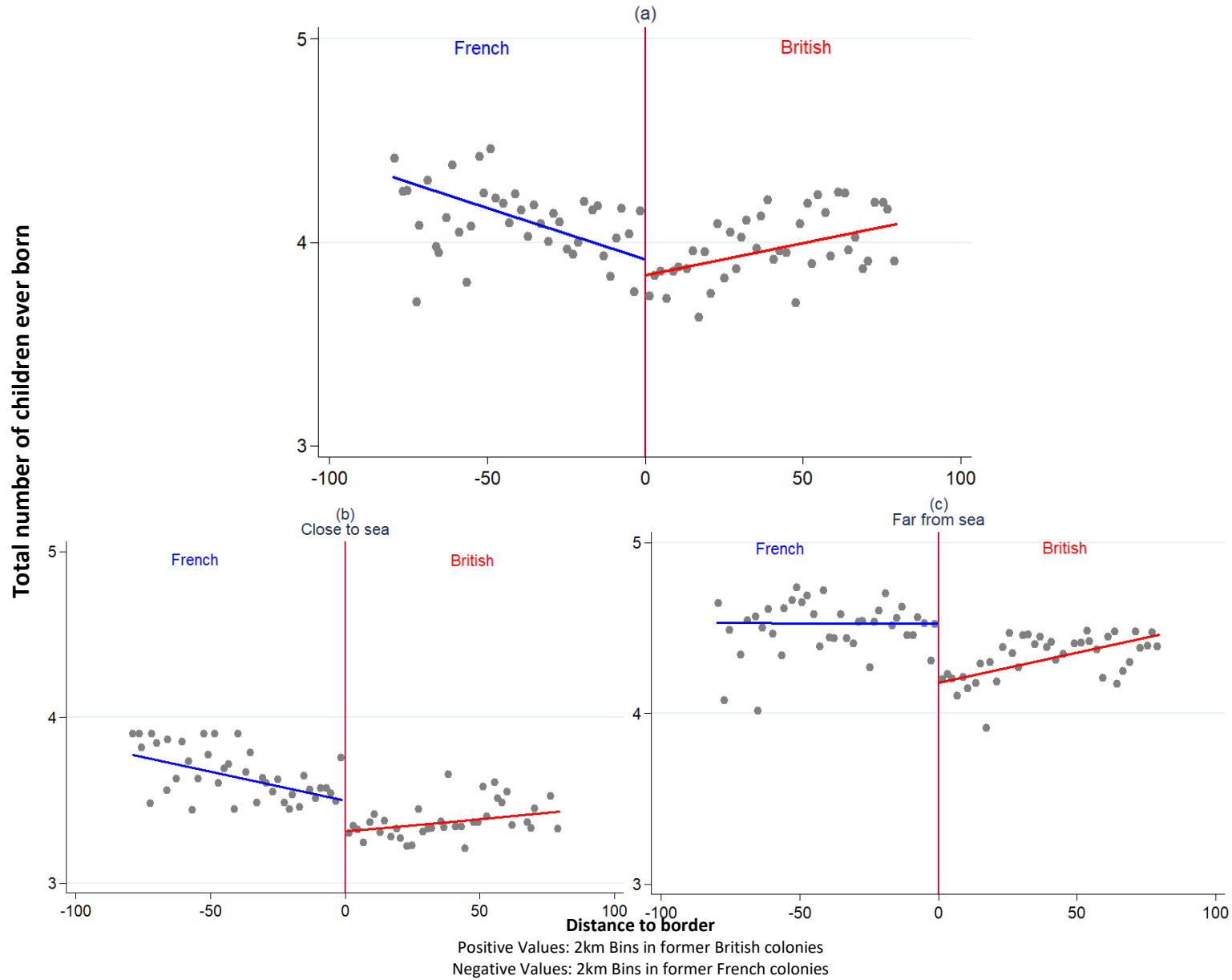


Figure A2.5: Colonial origins and other outcomes with baseline sample

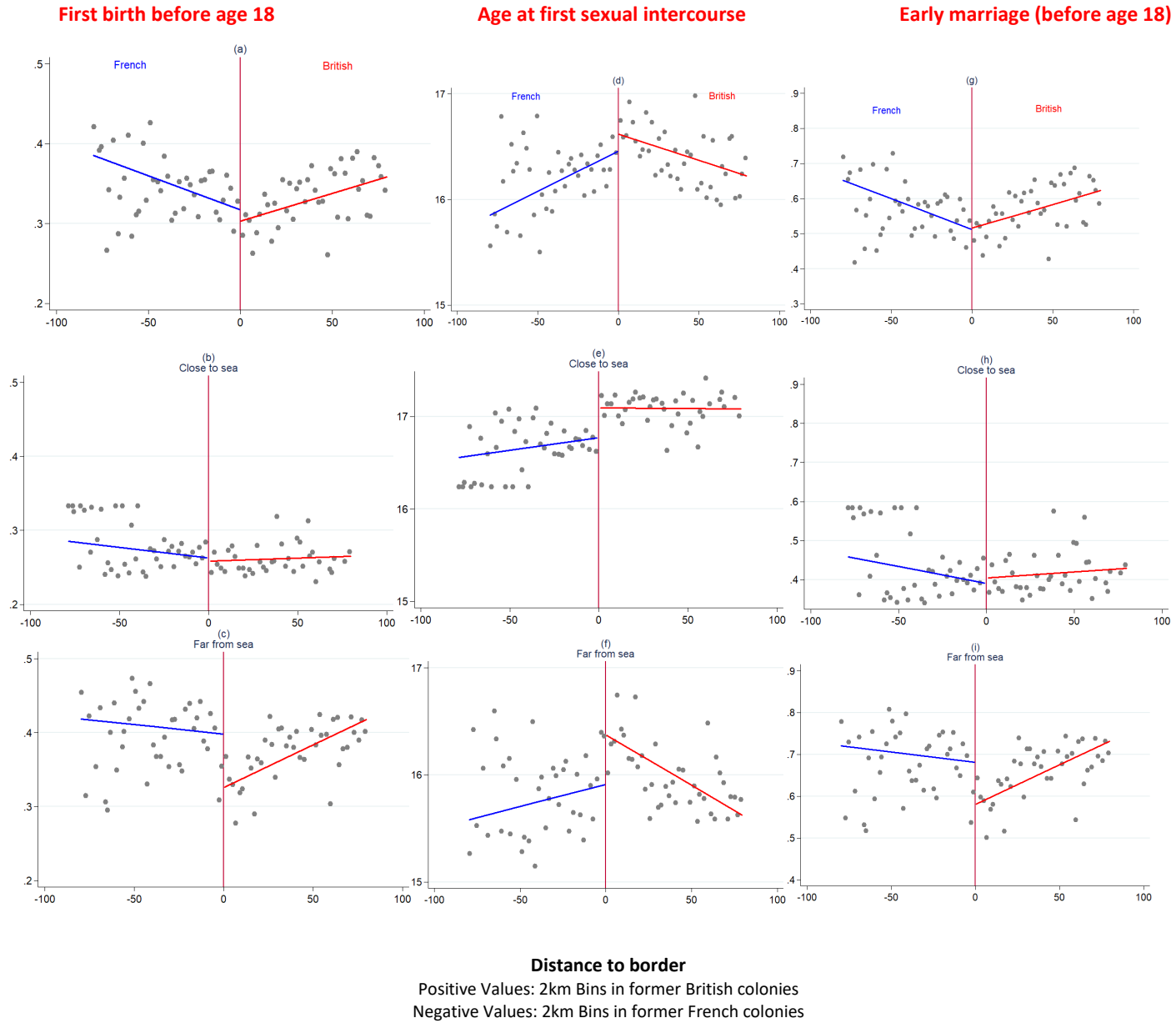


Table A2.1: Descriptive statistics

	Observations	Means	Sd. dev.
<b><i>Panel A: Individual-level means</i></b>			
Children ever born	34,405	4.10	2.90
Age at first sexual intercourse	28,672	16.14	2.96
Early marriage (before age 18)	32,401	0.60	0.49
Age	34,405	31.64	8.21
Year of birth	34,405	1968.35	10.30
Muslim religion	34,328	0.56	0.50
Years of education	34,386	2.09	3.84
Unskilled worker	32,614	0.32	0.47
Cash earning	13,874	0.76	0.43
Respondent had one child dead	34,405	0.44	0.50
Contraceptive use	4,590	0.50	0.50
Mild wasting	22,000	0.70	0.46
Child mortality	104,979	0.23	0.42
<b><i>Panel B: Pixel-level means</i></b>			
Pixel is lit	763	0.38	0.49
Population density	763	121.14	318.76
Elevation	763	307.12	195.24
Soil suitability to agriculture	755	0.38	0.23
Area under water	763	6.73	20.46
Distance to the coast	763	487.00	314.12
Distance to the capital	763	471.74	305.64
Distance to the national border	763	46.91	47.00

Table A2.2: Robustness check : Third order RD polynomial - Colonial origins and reproductive behavior

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Baseline specification</b>								
<b>Panel A: Total number of children ever born</b>								
British (vs. French)	-0.09 (0.105)	-0.47*** (0.094)	-0.14 (0.123)	-0.43*** (0.082)	-0.18 (0.119)	-0.46*** (0.069)	-0.16 (0.109)	-0.52*** (0.075)
Observations	9,136	12,019	11,724	16,183	13,192	18,116	13,753	18,815
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Panel B: First birth before age 18</b>								
British (vs. French)	-0.02 (0.031)	-0.09*** (0.015)	-0.03 (0.029)	-0.07*** (0.015)	-0.03 (0.029)	-0.08*** (0.015)	-0.03 (0.027)	-0.08*** (0.016)
Observations	8,011	11,087	10,206	14,955	11,540	16,744	12,042	17,432
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Panel C: Age at first sexual intercourse</b>								
British (vs. French)	0.27* (0.148)	0.80*** (0.266)	0.31* (0.167)	0.66* (0.353)	0.32* (0.185)	0.63* (0.346)	0.31* (0.175)	0.63* (0.353)
Observations	7,264	9,802	9,580	13,422	10,943	14,969	11,478	15,610
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
<b>Panel D: Early marriage (before 18 years old)</b>								
British (vs. French)	-0.01 (0.040)	-0.09** (0.038)	-0.03 (0.039)	-0.07* (0.045)	-0.04 (0.038)	-0.08* (0.043)	-0.03 (0.031)	-0.09** (0.043)
Observations	8,163	11,612	10,479	15,689	11,851	17,595	12,377	18,322
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a third-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of *Cameron et al. (2011)*, are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A2.3: Robustness check : Third order RD polynomial - Colonial origins and reproductive behavior (Control for spillover at the border)

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Natives</b>								
<b>Panel A: Total number of children ever born</b>								
British (vs. French)	0.07 (0.167)	-0.54*** (0.125)	-0.01 (0.185)	-0.50*** (0.120)	-0.06 (0.184)	-0.53*** (0.092)	-0.04 (0.177)	-0.58*** (0.091)
Observations	3,004	5,211	3,947	7,134	4,591	7,972	4,817	8,304
<b>Panel B: First birth before age 18</b>								
British (vs. French)	-0.05 (0.049)	-0.10*** (0.018)	-0.06 (0.050)	-0.07*** (0.021)	-0.05 (0.044)	-0.08*** (0.019)	-0.04 (0.039)	-0.08*** (0.020)
Observations	2,578	4,779	3,383	6,557	3,969	7,317	4,172	7,641
<b>Panel C: Age at first sexual intercourse</b>								
British (vs. French)	0.39* (0.218)	0.72** (0.302)	0.38* (0.207)	0.48 (0.340)	0.37* (0.219)	0.48 (0.359)	0.30 (0.196)	0.48 (0.365)
Observations	2,541	4,291	3,380	5,901	3,971	6,519	4,180	6,831
<b>Panel D: Early marriage (before 18 years old)</b>								
British (vs. French)	-0.05 (0.050)	-0.09** (0.038)	-0.08 (0.049)	-0.07* (0.039)	-0.07* (0.043)	-0.08* (0.040)	-0.06* (0.036)	-0.08** (0.040)
Observations	2,590	4,980	3,429	6,842	4,017	7,673	4,227	8,017
<b>Natives + Thick border</b>								
<b>Panel E: Total number of children ever born</b>								
British (vs. French)	-0.08 (0.219)	-0.57*** (0.139)	-0.19 (0.224)	-0.51*** (0.130)	-0.23 (0.226)	-0.56*** (0.077)	-0.18 (0.235)	-0.62*** (0.075)
Observations	2,194	4,724	3,129	6,655	3,760	7,506	3,983	7,841
<b>Panel F: First birth before age 18</b>								
British (vs. French)	-0.07 (0.059)	-0.11*** (0.019)	-0.09 (0.054)	-0.08*** (0.024)	-0.07 (0.043)	-0.09*** (0.022)	-0.05 (0.041)	-0.09*** (0.023)
Observations	1,949	4,340	2,747	6,125	3,323	6,895	3,523	7,222
<b>Panel G: Age at first sexual intercourse</b>								
British (vs. French)	0.43 (0.269)	0.59*** (0.228)	0.46* (0.260)	0.34 (0.279)	0.38 (0.243)	0.33 (0.281)	0.28 (0.234)	0.31 (0.266)
Observations	1,902	3,869	2,733	5,487	3,312	6,117	3,518	6,432
<b>Panel H: Early marriage (before 18 years old)</b>								
British (vs. French)	-0.07 (0.056)	-0.10*** (0.038)	-0.10* (0.055)	-0.07* (0.042)	-0.09** (0.042)	-0.08* (0.042)	-0.07** (0.034)	-0.09** (0.042)
Observations	1,941	4,510	2,773	6,379	3,348	7,223	3,555	7,570
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a third-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A2.4: Robustness check : RD polynomial in latitude and longitude - Colonial origins and reproductive behavior

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Baseline specification</b>								
<b>Panel A: Total number of children ever born</b>								
British (vs. French)	0.06 (0.145)	-0.48*** (0.117)	0.14 (0.110)	-0.42*** (0.102)	0.09 (0.098)	-0.45*** (0.097)	0.01 (0.097)	-0.46*** (0.101)
Observations	9,136	12,019	11,724	16,183	13,192	18,116	13,753	18,815
<b>Panel B: First birth before age 18</b>								
British (vs. French)	0.00 (0.022)	-0.09*** (0.017)	-0.02 (0.027)	-0.07*** (0.015)	-0.02 (0.027)	-0.08*** (0.016)	-0.03 (0.025)	-0.07*** (0.015)
Observations	8,011	11,087	10,206	14,955	11,540	16,744	12,042	17,432
<b>Panel C: Age at first sexual intercourse</b>								
British (vs. French)	0.22 (0.157)	0.85*** (0.267)	0.34* (0.181)	0.64* (0.385)	0.23 (0.150)	0.59 (0.402)	0.35** (0.172)	0.54 (0.440)
Observations	7,264	9,802	9,580	13,422	10,943	14,969	11,478	15,610
<b>Panel D: Early marriage (before 18 years old)</b>								
British (vs. French)	0.03 (0.046)	-0.10*** (0.032)	0.00 (0.053)	-0.07 (0.048)	0.00 (0.044)	-0.07 (0.047)	-0.01 (0.040)	-0.07 (0.049)
Observations	8,163	11,612	10,479	15,689	11,851	17,595	12,377	18,322
Countries	8	9	8	9	8	9	8	9
Ethno-linguistic families	10	18	10	18	15	16	15	16
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a polynomial in the latitude and longitude of the pixel. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A2.5: Robustness check : RD polynomial in latitude and longitude - Colonial origins and reproductive behavior (Control for spillover at the border)

	<60 km of bound.		<100 km of bound.		<150 km of bound.		<200 km of bound.	
	Close (1)	Far (2)	Close (3)	Far (4)	Close (5)	Far (6)	Close (7)	Far (8)
<b>Natives</b>								
<b>Panel A: Total number of children ever born</b>								
British (vs. French)	0.20 (0.199)	-0.54*** (0.127)	0.28 (0.277)	-0.51*** (0.129)	0.24* (0.142)	-0.56*** (0.109)	0.12 (0.165)	-0.58*** (0.110)
Observations	3,004	5,211	3,947	7,134	4,591	7,972	4,817	8,304
<b>Panel B: First birth before age 18</b>								
British (vs. French)	-0.05 (0.051)	-0.10*** (0.021)	-0.06* (0.033)	-0.09*** (0.022)	-0.04 (0.027)	-0.10*** (0.019)	-0.03 (0.032)	-0.09*** (0.017)
Observations	2,578	4,779	3,383	6,557	3,969	7,317	4,172	7,641
<b>Panel C: Age at first sexual intercourse</b>								
British (vs. French)	0.41*** (0.149)	0.71** (0.284)	0.45*** (0.128)	0.47 (0.427)	0.22 (0.145)	0.46 (0.403)	0.25 (0.175)	0.44 (0.432)
Observations	2,541	4,291	3,380	5,901	3,971	6,519	4,180	6,831
<b>Panel D: Early marriage (before 18 years old)</b>								
British (vs. French)	-0.02 (0.079)	-0.09*** (0.028)	-0.06 (0.064)	-0.07 (0.041)	-0.03 (0.040)	-0.07 (0.044)	-0.04 (0.036)	-0.06 (0.045)
Observations	2,590	4,980	3,429	6,842	4,017	7,673	4,227	8,017
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
<b>Natives + Thick border</b>								
<b>Panel E: Total number of children ever born</b>								
British (vs. French)	0.08 (0.337)	-0.48*** (0.172)	0.25 (0.422)	-0.48*** (0.133)	0.13 (0.201)	-0.55*** (0.097)	0.02 (0.250)	-0.57*** (0.131)
Observations	2,194	4,724	3,129	6,655	3,760	7,506	3,983	7,841
<b>Panel F: First birth before age 18</b>								
British (vs. French)	-0.10* (0.050)	-0.11*** (0.020)	-0.10* (0.055)	-0.09*** (0.028)	-0.06** (0.029)	-0.10*** (0.027)	-0.04 (0.034)	-0.10*** (0.026)
Observations	1,949	4,340	2,747	6,125	3,323	6,895	3,523	7,222
<b>Panel G: Age at first sexual intercourse</b>								
British (vs. French)	0.53** (0.210)	0.68** (0.288)	0.53** (0.235)	0.36 (0.332)	0.18 (0.185)	0.31 (0.324)	0.09 (0.292)	0.24 (0.299)
Observations	1,902	3,869	2,733	5,487	3,312	6,117	3,518	6,432
<b>Panel H: Early marriage (before 18 years old)</b>								
British (vs. French)	-0.05 (0.085)	-0.10*** (0.029)	-0.08 (0.077)	-0.07 (0.045)	-0.05 (0.041)	-0.07 (0.047)	-0.05 (0.034)	-0.06 (0.044)
Observations	1,941	4,510	2,773	6,379	3,348	7,223	3,555	7,570
Countries	7	9	7	9	8	9	8	9
Ethno-linguistic families	9	17	9	18	12	15	14	14
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: In each specification we control for a polynomial in the latitude and longitude of the pixel. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A2.6: Effect of colonial origins on fertility in areas far from sea (Control for education and income)

	<u>&lt;60 km of bound.</u>		<u>&lt;100 km of bound.</u>		<u>&lt;150 km of bound.</u>		<u>&lt;200 km of bound.</u>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Dependent variable: Total number of children ever born</b>								
<b>Panel A: Baseline specification</b>								
British (vs. French)	-0.37*** (0.084)	-0.37*** (0.089)	-0.38*** (0.107)	-0.37*** (0.111)	-0.42*** (0.108)	-0.40*** (0.111)	-0.44*** (0.110)	-0.42*** (0.112)
Observations	11,930	12,012	16,060	16,173	17,998	18,106	18,685	18,806
<b>Panel B: Geographic controls</b>								
British (vs. French)	-0.38*** (0.105)	-0.39*** (0.109)	-0.35*** (0.100)	-0.35*** (0.102)	-0.39*** (0.092)	-0.38*** (0.097)	-0.43*** (0.091)	-0.41*** (0.093)
Observations	11,930	12,012	16,060	16,173	17,998	18,106	18,685	18,806
<b>Panel C: Natives</b>								
British (vs. French)	-0.46*** (0.114)	-0.47*** (0.124)	-0.49*** (0.134)	-0.49*** (0.143)	-0.53*** (0.129)	-0.53*** (0.137)	-0.55*** (0.132)	-0.55*** (0.137)
Observations	5,187	5,209	7,102	7,132	7,938	7,969	8,264	8,301
<b>Panel D: Natives + Geographic controls</b>								
British (vs. French)	-0.48*** (0.136)	-0.49*** (0.147)	-0.46*** (0.132)	-0.46*** (0.139)	-0.48*** (0.106)	-0.48*** (0.114)	-0.52*** (0.105)	-0.52*** (0.109)
Observations	5,187	5,209	7,102	7,132	7,938	7,969	8,264	8,301
<b>Panel E: Natives + Thick border</b>								
British (vs. French)	-0.47*** (0.098)	-0.48*** (0.103)	-0.52*** (0.103)	-0.51*** (0.111)	-0.56*** (0.085)	-0.56*** (0.092)	-0.59*** (0.085)	-0.59*** (0.090)
Observations	4,702	4,722	6,625	6,653	7,474	7,503	7,803	7,838
<b>Panel F: Natives + Thick border + Geographic controls</b>								
British (vs. French)	-0.49*** (0.132)	-0.51*** (0.136)	-0.45*** (0.123)	-0.45*** (0.126)	-0.49*** (0.070)	-0.49*** (0.076)	-0.54*** (0.067)	-0.54*** (0.072)
Observations	4,702	4,722	6,625	6,653	7,474	7,503	7,803	7,838
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓	✓	✓
Education control	✓	✓	✓	✓	✓	✓	✓	✓
Asset control	✓		✓		✓		✓	
Light density		✓		✓		✓		✓

Note: In each specification we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A2.7: Average effect - Colonial origins and fertility (Excluding Cameroon and Togo)

	RDD - Bandwidth				
	OLS (1)	<60 km of bound. (2)	<100 km of bound. (3)	<150 km of bound. (4)	<200 km of bound. (5)
<b>Dependent variable: Total number of children ever born</b>					
<b>Panel A: Baseline specification</b>					
British (vs. French)	-0.51*** (0.164)	-0.40*** (0.084)	-0.41** (0.177)	-0.48** (0.203)	-0.50** (0.199)
Observations	28,906	16,336	23,148	26,549	27,809
<b>Panel B: Geographic controls</b>					
British (vs. French)	-0.47*** (0.132)	-0.47*** (0.097)	-0.42*** (0.100)	-0.45*** (0.105)	-0.45*** (0.130)
Observations	28,243	15,874	22,485	25,886	27,146
<b>Panel C: Natives</b>					
British (vs. French)	-0.58*** (0.143)	-0.46*** (0.131)	-0.51*** (0.159)	-0.57*** (0.148)	-0.59*** (0.150)
Observations	12,398	6,942	9,792	11,274	11,832
<b>Panel D: Natives + Geographic controls</b>					
British (vs. French)	-0.52*** (0.154)	-0.55*** (0.143)	-0.50*** (0.151)	-0.49*** (0.133)	-0.50*** (0.143)
Observations	12,166	6,759	9,560	11,042	11,600
<b>Panel E: Natives + Thick border</b>					
British (vs. French)	-0.62*** (0.133)	-0.47*** (0.145)	-0.54*** (0.162)	-0.61*** (0.148)	-0.63*** (0.149)
Observations	11,578	6,122	8,972	10,454	11,012
<b>Panel F: Natives + Thick border + Geographic controls</b>					
British (vs. French)	-0.56*** (0.133)	-0.57*** (0.157)	-0.51*** (0.148)	-0.53*** (0.125)	-0.53*** (0.145)
Observations	11,368	5,961	8,762	10,244	10,802
Ethnic homeland FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓

Note: Specifications in columns (2)-(5) control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethnolinguistic family level using the approach of [Cameron et al. \(2011\)](#), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A2.8: Bootstrap: Average effect - Colonial origins and fertility

	RDD - Bandwidth				
	OLS (1)	<60 km of bound. (2)	<100 km of bound. (3)	<150 km of bound. (4)	<200 km of bound. (5)
<b>Dependent variable: Total number of children ever born</b>					
<b>Panel A: Baseline specification</b>					
British (vs. French)	-0.44*** (0.112)	-0.33*** (0.107)	-0.35*** (0.097)	-0.43*** (0.118)	-0.45*** (0.123)
Observations	34,328	21,617	28,570	31,971	33,231
Bootstrap: WCR p-value by country	0.023	0.113	0.066	0.023	0.023
Bootstrap: WCR p-value by ethno-linguistic families	0.003	0.013	0.009	0.005	0.006
Bootstrap: WCR p-value by country-ethno-linguistic families	0.001	0.025	0.012	0.002	0.002
<b>Panel B: Geographic controls</b>					
British (vs. French)	-0.41*** (0.077)	-0.34*** (0.109)	-0.35*** (0.081)	-0.40*** (0.078)	-0.41*** (0.081)
Observations	33,665	21,155	27,907	31,308	32,568
Bootstrap: WCR p-value by country	0.021	0.165	0.098	0.033	0.037
Bootstrap: WCR p-value by ethno-linguistic families	0.008	0.020	0.010	0.007	0.008
Bootstrap: WCR p-value by country-ethno-linguistic families	0.006	0.039	0.018	0.009	0.009
<b>Panel C: Natives</b>					
British (vs. French)	-0.51*** (0.155)	-0.39** (0.156)	-0.44*** (0.168)	-0.51*** (0.174)	-0.52*** (0.177)
Observations	13,919	8,398	11,313	12,795	13,353
Bootstrap: WCR p-value by country	0.033	0.057	0.070	0.045	0.045
Bootstrap: WCR p-value by ethno-linguistic families	0.007	0.056	0.032	0.014	0.011
Bootstrap: WCR p-value by country-ethno-linguistic families	0.008	0.017	0.011	0.010	0.008
<b>Panel D: Natives + Geographic controls</b>					
British (vs. French)	-0.45*** (0.141)	-0.38** (0.166)	-0.40*** (0.154)	-0.42*** (0.135)	-0.44*** (0.140)
Observations	13,687	8,215	11,081	12,563	13,121
Bootstrap: WCR p-value by country	0.051	0.096	0.082	0.047	0.061
Bootstrap: WCR p-value by ethno-linguistic families	0.018	0.077	0.039	0.022	0.025
Bootstrap: WCR p-value by country-ethno-linguistic families	0.010	0.049	0.029	0.010	0.010
<b>Panel E: Natives + Thick border</b>					
British (vs. French)	-0.59*** (0.102)	-0.46*** (0.116)	-0.52*** (0.128)	-0.59*** (0.119)	-0.61*** (0.120)
Observations	12,600	7,079	9,994	11,476	12,034
Bootstrap: WCR p-value by country	0.018	0.037	0.039	0.035	0.031
Bootstrap: WCR p-value by ethno-linguistic families	0.008	0.036	0.028	0.015	0.014
Bootstrap: WCR p-value by country-ethno-linguistic families	0.005	0.014	0.009	0.006	0.006
<b>Panel F: Natives + Thick border + Geographic controls</b>					
British (vs. French)	-0.53*** (0.104)	-0.49*** (0.130)	-0.48*** (0.123)	-0.51*** (0.104)	-0.52*** (0.117)
Observations	12,390	6,918	9,784	11,266	11,824
Bootstrap: WCR p-value by country	0.031	0.045	0.039	0.031	0.047
Bootstrap: WCR p-value by ethno-linguistic families	0.013	0.031	0.026	0.017	0.024
Bootstrap: WCR p-value by country-ethno-linguistic families	0.004	0.010	0.010	0.004	0.007
Ethnic homeland FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓

Note: Specifications in columns (2)-(5) control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different colonial origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of Cameron et al. (2011), are in parenthesis. Coefficients that are significantly different from zero are denoted by the following system: \*\*\*p<0.01, \*\*p<0.05, \*p<0.1. Following Roodman et al. (2019) and Nielsen et al. (2020), the first, second, and third Bootstrap use Wild Cluster bootstrap symmetric P values with clustering at the country level, the ethno-linguistic family level, and the country - ethno-linguistic family level, respectively. We set the number of replications to 9999.

## Appendix Data Sources

**Light density at night:** Light Density is calculated averaging light density observations across pixels that fall within the unit of analysis. We use the 2013 Nighttime Light (NTL) data (stable lights dataset) from the U.S. Air Force's Defense Meteorological Satellite Program/Operational Linescan System (DMSP/OLS). This dataset is made available by the U.S. National Oceanographic and Atmospheric Administration (NOAA). The pixel light (gain) values range from 0 to 63 with 0 being the absence of light. *Available at* <https://www.ngdc.noaa.gov/eog/dmsp/downloadV4composites>

**Population density:** Population per sq. km. in 2000. Source: Nelson, Andy, 2004. African Population Database Documentation, UNEP GRID Sioux Falls. *Available at* <https://www.arcgis.com/home/item.html?id=9ec46c83ca5c47ebb1a25bd43131b483>

**Elevation:** Average elevation above sea level of each pixel. Source: National Oceanic and Atmospheric Administration (NOAA) and U.S. National Geophysical Data Center, TerrainBase, release 1.0 (CD-ROM), Boulder, Colorado. *Available at* <http://nelson.wisc.edu/sage/data-and-models/atlas/data.php?incdataset=Topography>

**Soil suitability for agriculture:** Average land quality for cultivation within each pixel. This index is based on the temperature and soil conditions of each grid cell. Source: Ramankutty, N., J.A. Foley, J. Norman, and K. McSweeney. The global distribution of cultivable lands: current patterns and sensitivity to possible climate change. *Available at* <http://nelson.wisc.edu/sage/data-and-models/atlas/data.php?incdataset=Suitability%20for%20Agriculture>

**Water area:** Total area covered by rivers or lakes in sq. km within each pixel. Constructed using the Level 3 of the Global Lakes and Wetlands Database (GLWD) which comprises lakes, reservoirs, rivers, and different wetland types in the form of a global raster map at 30-sec resolution. Source: Lehner, B. and Doell, P. (2004): Development and validation of a global database of lakes, reservoirs and wetlands. *Journal of Hydrology* 296/1-4: 1-22. *Available at* <http://www.arcgis.com/home/item.html?id=1ac6777abcc24ab4a9fe39f27c4cb01f>

**Distance to the sea coast:** The geodesic distance (in kilometers) from the centroid of each pixel to the nearest coastline. Constructed using Africa coastline data. *Available at* <http://omap.africanmarineatlas.org/BASE/pages/coastline.htm>

**Distance to the capital city:** The geodesic distance (in kilometers) from the centroid of each pixel to the capital city in the same country. Geographical coordinates for the capital cities were derived from the cShapes dataset. Source: Weidmann, Nils B., Doreen Kuse, and Kristian Skrede Gleditsch. 2010. The Geography of the International System: The CShapes Dataset. *International Interactions* 36 (1). *Available at* <http://nils.weidmann.ws/projects/cshapes.html>

**Distance to the national border:** The geodesic distance to the nearest national border from the centroid of each pixel. Constructed using the border from the digital chart of the world projection *Available at* <https://worldmap.harvard.edu/data>

**Petroleum:** Indicator variable that equals one if there is an oil field in the pixel. We use the petroleum Dataset v.1.2 which contains information on all known on-shore oil and gas deposits throughout the world. Source: Lujala, PÄivi; Jan Ketil RÄ\_d, Nadia Thieme, 2007. "Fighting over Oil: Introducing A New Dataset", *Conflict Management and Peace Science* 24(3), 239-256. *Available at* <https://www.prio.org/Data/Geographical-and-Resource-Datasets/Petroleum-Dataset/Petroleum-Dataset-v-12/>

**Diamonds:** Indicator variable that equals one if there is a diamond mine in the pixel. We use the Diamonds dataset offers a comprehensive list of all known diamond deposits throughout the world. Source: Gilmore, Elisabeth; Nils Petter Gleditsch, PÄivi Lujala Jan Ketil RÄ\_d, 2005. "Conflict Diamonds: A New Dataset", *Conflict Management and Peace Science* 22(3): 257â292. *Available at* <https://www.prio.org/Data/Geographical-and-Resource-Datasets/Diamond-Resources/>

# Chapter 3

## Traditional Norms, Legal Origins, and Female Empowerment

### 3.1 Introduction

A large body of work shows that legal institutions have important effects on economic and social outcomes (La Porta et al. (2008)). At the same time, it is argued that the effect of legal institutions might depend on the cultural and traditional contexts in which these institutions are implemented (Alesina and Giuliano (2015); Bisin and Verdier (2017); Nunn (2012)). However, we continue to have a very limited understanding of particular cultural contexts that affect the efficacy of legal institutions. Such an understanding is important for the optimal design of institutions.

In this chapter, we analyze the interaction of legal origins and ancestral cultural norms on gender roles in determining female economic empowerment in sub-Saharan Africa. Sub-Saharan Africa is an ideal setting for studying this question because women's rights and economic opportunities are severely limited in this region. Indeed, the plight of African women is of utmost concern. As of 2012, only 75 percent of primary school-aged girls attended school and secondary gross enrolment ratios were lower than 40 percent. Only 64 percent of young women were literate in comparison to 76 percent for young men. About two-fifths of sub-Saharan African women were married before age 18, and experience of sexual violence for these women is the highest in the world (United Nations (2015)).

Legal traditions, and their interaction with ancestral or traditional norms, may have profound effects on women. A large literature argues that the common law system generates more economic opportunities than the civil law, and promotes institutions that foster economic development (e.g., [La Porta et al. \(1999\)](#); [North \(1989\)](#)). The common law system is characterized by greater independence of the judicial system, lower government ownership, and stronger legal protection of investors (e.g., [Beck, Demirgüç-Kunt, and Levine \(2003\)](#)). These features are known to reduce the risk of expropriation and to promote contract enforcement and secured property rights, resulting in more investment and economic growth in common law countries compared to civil law countries. It follows that the common law system is likely to reduce the (relative) cost of human capital investment through affecting both the supply of, and the returns to, education.

To formally understand how legal origins may interact with traditional norms, we develop a very simple model in which parents decide on the quantity and quality of children. The relative price of human capital depends on legal origins and the utility-weight assigned to child quality is a function of traditional norms. The key prediction of the model is that a lower cost of human capital is associated with greater investment in human capital, but this relationship is more pronounced in societies where traditional norms assign greater utility to child quality. In other words, compared to the civil law system, the common law system better promotes female education (and economic empowerment), and this positive effect is greater in societies where ancestral norms of gender roles promote women more. In addition, to the extent that the promotion of women's rights is antithetical to that of men's rights, our model directly predicts that, although the common law system may also positively affect male education, this positive effect should be less pronounced in societies where traditions promote women's rights.

To provide direct empirical evidence for the effect of legal origins on female economic empowerment in sub-Saharan Africa and how this effect depends on traditional norms of gender roles, we follow the approach of [Michalopoulos and Papaioannou \(2014\)](#) and exploit the fact that colonists partitioned several ethnic groups into adjacent countries with different formal institutions ([Michalopoulos and Papaioannou \(2016\)](#)). More precisely, we exploit the exogenous variation in legal origins across countries and directly compare women among the same ethnic group living in civil law countries and common law countries. Using this spatial Regression Discontinuity Design (RDD), we show that

women in common law countries have about two years of education more than women in civil law countries. This “average” effect is large as the mean years of education in our sample is 3.12. This result is exactly the relationships our theory predicts. One potential issue is that women in civil law countries are more likely to have higher grade repetition rates, which could lead to higher dropout rates (Dupraz (2019)). We tackle this issue by analyzing the impact of legal origins on other educational outcomes. Our RDD estimates suggest that women in common law countries are approximately 20 percentage points more likely to have at least one year of education, suggesting that our findings are not entirely driven by higher grade repetition rates in civil law countries. Similarly, women in common law countries are significantly more likely to have completed primary and secondary school by 7 and 2 percentage points, respectively.

Consistent with this positive average effect of common law on female education, our RDD estimates also show that women in common law countries are significantly more likely to work in the professional sector (e.g. managerial position, clerical) and they are significantly less likely to marry before age 18.

Turning to the central part of this chapter, we investigate how the effect of legal origins on female outcomes depends on ancestral cultural norms of gender roles. We consider three classical norms arising from ancestral mode of subsistence and marriage traditions: (1) female participation in agriculture, (2) bride price payment, and (3) post-marital residence rules (patrilocal vs. matrilocality).<sup>1</sup> We validate the prediction that the positive effect of the common law system on female education (and economic empowerment) is lower in settings where cultural norms do not traditionally promote women’s rights and empowerment. More precisely, we find that the positive effect of the common law system on female education is lower in societies where women were not allowed to participate in agricultural activities, in societies that practise bride price, and in patrilocal societies. We find similar heterogeneous effects of the common law system on female employment, early marriage, and the husband-wife age gap. Interestingly, while the common law system has a negative effect on the husband-wife age gap in societies that traditionally promote women, its effect is positive and significant in societies where women’s rights are limited. In addition to these main results, we examine the heterogeneous effect of common law on

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<sup>1</sup>Patrilocal refers to a post-marital residence norm where the newly-wed couple lives with or near the husband’s family. In contrast, in matrilocality, the couple lives with the wife’s family.

male education, finding that it has a positive effect that is significantly lower in societies where ancestral traditions promote women’s rights and economic empowerment. All of these findings imply that the long-term impact of legal origins varies within the same country as a function of ancestral traditions. They strongly suggest that, to be optimal, the design of formal institutions should take into account the cultural and traditional context in which they will be implemented.

Our study contributes to a literature on the historical determinants of gender roles and women’s outcomes. Previous studies have analyzed the importance of traditional agricultural practices (e.g., [Alesina et al. \(2013\)](#); [Hansen et al. \(2015\)](#)), intergenerational transmission of values (e.g., [Fernández et al. \(2004\)](#)), disequilibria in sex ratios (e.g., [Teso \(2019\)](#)), medical improvements (e.g., [Albanesi and Olivetti \(2016\)](#)), and colonial population policies ([Canning et al. \(2020\)](#)). In a recent study, [Anderson \(2018\)](#) documents that female HIV rates are significantly higher in common law sub-Saharan African countries in comparison to civil law countries. This paper explains this relationship by focusing on differences in female property rights under the two codes of law.<sup>2</sup> The literature has so far not examined the interplay between historical formal institutions and cultural traditions. Our main contribution is to show that legal origins significantly interact with ancestral norms of gender roles to determine present-day women’s economic empowerment.

We also view our analysis as complementing a recent study examining interactions between a contemporary policy reform and traditional norms of gender roles. [Ashraf et al. \(2020\)](#) show that a particular development policy intervention consisting of a school construction program in India and Zambia increased female education only in bride price societies. We find instead that the positive effect of the common law system on female education (and other outcomes measuring women’s economic empowerment) is greater in societies that traditionally promote women’s rights, including non-bride-price societies. The difference between our findings might be explained by the fact that [Ashraf et al. \(2020\)](#) focus on a one-time policy intervention that was implemented within a relatively short time period, whereas we focus on historical legal institutions, which are more lasting and which, in addition to affecting the supply of education like a school construction

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<sup>2</sup>Interestingly, our findings on the “average” effect of common law on early marriage can be viewed as providing another mechanism through which this legal system affects HIV rates. Indeed, marriage delay may increase exposure to risky sexual interactions with non-marital partners, increasing the likelihood of HIV infection.

program, have other functions like affecting the returns to education. Another novelty of our study is that we also consider male education and show that it is positively affected by the common law system, but that this positive effect is less visible in societies where ancestral traditions promote women's rights. Our findings seem to suggest that traditions of gender norms directly oppose women's rights to men's rights, and this incompatibility interacts with formal institutions in a non-trivial way. They imply that institutional interventions designed to promote women's economic empowerment in societies where their rights are traditionally constrained should engage men and obtain their support and cooperation.

The remainder of this chapter is structured as follows. In section 3.2, we present our conceptual framework. Section 3.3 details the data sets and provides descriptive statistics. Section 3.4 presents the methodology and the model specifications. Section 3.5 presents estimates of the relationship between legal origins and female outcomes in sub-Saharan Africa. We also show how this relationship depends on ancestral cultural norms of gender roles, which is the central question we address. The last section briefly concludes.

## 3.2 The Theoretical Determinants of Female Human Capital

### 3.2.1 Legal Institutions: Civil vs Common Law

A country's legal system encompasses an array of economic policies and institutions that are likely to determine development outcomes. In Africa, the legal system was primarily introduced through colonization and conquest (Watson (1993)) and it varies between common law and civil law colonizers. Typically, former British colonies inherited the British common law system and former European continental colonies (countries colonized by France, Spain, Portugal, Italy and Belgium) retained the roman civil code of laws. In the past two decades, economists have produced a considerable amount of research suggesting that differences in the origin and the ideology of the common and civil legal traditions have generated different institutional arrangements as well as different economic outcomes today (La Porta et al. (1997), La Porta et al. (1998), La Porta et al. (1999),

Joireman (2001), Mahoney (2001), Glaeser and Shleifer (2002), La Porta et al. (2008)).

Consistent with Hayek (1973)'s claim of the superiority of English versus French legal traditions in term of several legal outcomes, the economic literature provides strong empirical evidence that common law countries exhibit greater independence of the judicial system, lower government ownership, and stronger legal protection of investors. These features imply that the design of governmental legal institutions in common law countries limits the extent of expropriation and fosters strong legal enforcement of property and contract rights which are associated with greater investment and growth. This is in sharp contrast with the lower level of scrutiny of executive action that characterizes the civil law system and which ensures that governments in civil law countries have more scope for alteration of property and contract rights.

The aforementioned structural differences between the two major legal systems in Africa have contributed to explaining the comparative economic development of common and civil law African countries, with the former outperforming the latter in different economic dimensions including education, health, and economic growth (La Porta et al. (1998), Djankov et al. (2002), Glaeser and Shleifer (2002)). By promoting institutions that foster economic development, the common law system generates more economic opportunities with increasing availability of jobs that require education. The common law system could then lead to more female education.

Economists have argued that the rise in female education following economic development can be linked to several economic factors. In particular, it has been shown that as economic development proceeds, the demand for female employment increases. This in turn raises the market return of female education, providing more incentives for investment in girls' education (Pitt et al. (2012), Jayachandran (2015), Heath and Jayachandran (2016)). The rise in female education following economic development could also be driven by the positive impact of economic development on household income. The rationale is, to the extent that parents view girls' education more as a luxury good than boys' education, a positive change in family income will translate into higher spending on girls' education (Heath and Jayachandran (2016)).

As the legal system of a country was introduced by a specific colonizer, it is likely that a country's legal origins correlate with his colonial history (Klerman et al. (2011)).

It follows that civil and common law countries could also differ in terms of the colonial strategies implemented by the colonizer. In particular, differences in colonial education policies could directly influence educational outcomes in civil and common law countries. It is well established in the literature that former British colonies outperformed former French colonies in terms of educational outcomes, and this difference has been linked to differences in colonial educational policies (Cogneau and Moradi (2014), Dupraz (2019)). Unlike the French educational strategy, the British educational strategy relied heavily on mission societies to provide and diffuse education. To achieve this goal, the British colonial government highly invested in school infrastructures and provided incentives to missionaries, especially by according grants-to-aid based on the efficiency of the missions' schools (Cogneau and Moradi (2014)). In contrast, the French colonial government barely subsidized mission's schools and made limited investments in school infrastructures. The British-French differences in education policies generate an advantage in educational outcomes in former British colonies. This advantage was especially stronger for females given the greater importance Protestant missions placed on the education of women than Catholic missions (Nunn et al. (2014)).

### 3.2.2 Ancestral Cultural Norms

Societies often have rules of social behavior that are based on cultural values and beliefs about women's roles in the society. These values and beliefs mainly originated from pre-industrial socio-economic arrangements. Despite changes in these initial economic conditions, ancestral cultural norms of gender roles have persisted over time, with important socio-economic consequences in the long run. In particular, anthropologists and economists have shown that differences across societies in traditional norms that determine women's socio-economic status can account for contemporary differences in gender related development outcomes (see, for instance, Boserup (1970), Dyson and Moore (1983), Basu (1992, 1998), Rammohan and Robertson (2012), Alesina et al. (2013), Alesina et al. (2016), Lowes and Nunn (2017)). In this chapter, we focus on the cultural determinants of gender roles arising from ancestral mode of subsistence and marriage traditions.

### 3.2.2.1 Traditional Mode of Subsistence

Social scientists are increasingly emphasizing the role of traditional mode of subsistence in shaping the evolution and persistence of gender roles in the long run. The central idea is that when in past societies, women were highly involved in subsistence activities, their perceived economic value was high, and this in turn fostered the development of social norms in ways that viewed women as productive and more equal to men (Alesina et al. (2016)).

As early as 1970, economists examined this claim by primarily focusing on differences in traditional agricultural practices. In a qualitative study, Boserup (1970) distinguished between plough and non-plough (shifting cultivation) agriculture and argued that descendants of ethnic groups that practised plough agriculture have currently less equal beliefs about gender roles. While plough agriculture is capital-intensive and requires more physical strength, shifting cultivation is labor-intensive and relies heavily on the use of hand-held tools such as the hoe or the digging stick for soil preparation. As a result, with male having a physiological comparative advantage in brawn-based activities, plough agriculture led to a division of labor in which men specialized in productive activities outside the home while women specialized in activities in and around the household. It follows that this gender-based division of labor may have led to deeply-held and persistent cultural beliefs that the appropriate role of women is in the domestic sphere, resulting in lower female participation in productive activities outside the household.

Formally, Alesina et al. (2013) empirically test Boserup (1970)'s hypothesis that plough agriculture influences gender division of labor in a way that promotes more unequal gender norms. They compared plough and non-plough agricultural societies from around the world and conclude that ethnic groups that practise plough agriculture are characterized by greater gender inequality today. In particular, they found that female descendants of societies that practised plough are less likely to participate in the workplace, politics, and entrepreneurial activities.

Analyzing a variety of ancestral modes of production and income generation in the context of Africa, Alesina et al. (2016) also find evidence consistent with the idea that societies where women were actively involved in subsistence activities are associated with better female socio-economic outcomes today. They show that in societies that tradition-

ally practise plough agriculture, where as shown in [Alesina et al. \(2013\)](#), women participate less in agriculture, violence against women is higher. They also find similar results for hunting and fishing societies where subsistence activities were mainly dominated by men. In contrast, [Alesina et al. \(2016\)](#) find that dependence on agriculture for production and income generation in ancient times, as opposed to hunting, fishing, gathering and pastoralism, is qualitatively associated with a lower level of domestic violence. They argue that, since most African societies were based upon non-plough agriculture<sup>3</sup>, high dependence on agriculture for production and income generation implies that women could participate more in economic activities. Women could therefore develop a more equal status in the society and the family, and this may generate norms of greater gender equality. Taking advantage of Murdock’s indicator of female participation in agriculture in past societies (see Section 3.3 for a detailed description of this variable), we find, consistent with the theory, that in our setting, there is a positive association between female participation in agriculture and female education (see Table A3.5).

### 3.2.2.2 Marriage Traditions

Another influential line of research has concentrated on the role of traditional marriage practices in shaping gender roles. In this chapter, we focus on two main marriage traditions: marriage payment and post-marital residence rules.

Bride price and dowry are the two most well-known types of marriage payment. The custom of bride price is defined as a cash or in kind transfer from the groom or his family to the bride’s family at the time of marriage. In contrast, dowry refers to payment that the bride’s parents make to the couple at the time of marriage. Historically, the bride price tradition has been practised throughout African societies, with 83% of African societies listed in Murdock ethnographic Atlas following a norm of bride price ([Murdock \(1967\)](#)). In recent years, number of anecdotal evidence and descriptive studies have nurtured the debate over the adverse consequences of bride price for girls and women ([Hague et al. \(2011\)](#), [Kizito \(2013\)](#), [Mtui \(2013\)](#)). First, bride price payment is viewed as an abusive practise that play an important role in incentivizing parents to “sell” their daughters early in exchange of a cash or in kind payment. Owing to this perception, some scholars claim that this practise is likely to increase a woman’s chance of early marriage, with negative

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<sup>3</sup>In [Alesina et al. \(2016\)](#), 6% of women’s ancestors in the sample traditionally used the plough.

impact on their education and lifetime reproductive health. Second, because bride price payment is associated with a purchase by the groom of the rights to the bride's labor and reproductive ability (Anderson (2007)), it is argued that the custom of bride price leads to the commodification of women with important adverse consequences (Lowe and Nunn (2017)). For instance, to the extent that husbands feel the right to mistreat their wives because they have paid for them (Lowe and Nunn (2017)), bride price tradition is more likely to encourage spousal violence. In addition, the fact that bride price payment induces an explicit economic value for women may also limit their relative independence. A number of scholars have argued in fact that the payment of bride price upon marriage lowers the probability for married women to end or escape an abusive marriage because their parents have to return the bride price back (Ansell (2001)).

Empirically, bride price has been linked to female outcomes in a complex way. Few empirical studies find evidence of the adverse consequences of bride price for girls and women. For instance, focusing on early marriage, Corno and Voena (2016) and Corno et al. (2016) find evidence that a negative shock to family income increases a girl's risk of early marriage in societies that traditionally practise bride price. They conclude that bride price payment obtained upon marriage is used to smooth consumption in the absence of well functioning credit markets. In contrast, many recent empirical studies found either no effect or surprising benefits of bride price traditions for females. On one hand, examining the relationship between bride price amount and variety of outcomes across marriages in Congo, Lowe and Nunn (2017) find no robust evidence that higher bride price is associated with early marriage, higher fertility, or acceptability of domestic violence. On the other hand, Alesina et al. (2016) show that female descendants of ethnic group that have bride price traditions are significantly less likely to experience intimate partner violence. In the same vein, Ashraf et al. (2020) focus on female education to demonstrate that ethnic groups that traditionally practise bride price payment upon marriage increased female school enrolment in response to a school construction program in Zambia and Indonesia. They show that within these groups, higher female education at marriage is associated with higher bride price payment received, resulting in greater incentives for parents to invest in girls' education. In our setting however, consistent with Corno and Voena (2016) and Corno et al. (2016), there is a negative correlation between the practise of bride price and female education (see Table A3.5).

Another marriage tradition that shapes gender roles in the society is related to post-marital residence norms which refer to social rules that define the place where a newly-wed couple lives after marriage. Anthropologists commonly distinguish between two types of post-marital residence norms. The patrilocal residence, where the newly-wed couple lives with or near the husband's family or kin; and the matrilocal residence, where the couple instead lives with the wife's family. While in our empirical analysis we will oppose these two types of post marital residence it is worth mentioning that anthropologists also recognize the existence of ambilocal residence where the couple can choose either patrilocal or matrilocal residence and the neolocal residence where the couple choose a new residence after marriage.

The anthropological literature on the historical origins of gender roles has typically opposed patrilocal to matrilocal social structures, showing in general that gender differences in parental investment in child quality were more pronounced in patrilocal societies compared to matrilocal societies (Dyson and Moore (1983), Basu (1992, 1998), Malhotra et al. (1995)). The theoretical explanation is that matrilocal (vs patrilocal) post-marital residence increases parental financial connections with daughters relative to sons, and, therefore, increases parental incentives to invest in their daughters human capital relative to their sons. A growing economic literature has tested this proposition formally. Focusing mainly on India and Indonesia, most empirical studies examining the link between kinship norms and gender inequality in economic outcomes find a negative association between patrilocal exogamy and women's empowerment outcomes including female education, marriage outcomes, and female autonomy (Buttenheim and Nobles (2009), Levine and Kevane (2003), Rammohan and Robertson (2012), Bau (2020)). In the African context, evidence of the relationship between post-marital migration and female empowerment are scarce. Focusing on Indonesia, Ghana, and Mexico, Bau (2020) examines how cultural norms affect parents' human capital investment in their children through strategic channels. She finds that patrilocal (vs neolocal) is associated with higher enrolment rates for sons relative to daughters. Similarly, Table A3.5 shows a negative correlation between patrilocal and female education in our setting.

### 3.2.3 A Simple Model of Interaction Between Legal Origins and Culture

Existing works have widely studied colonial institutions and culture as independent triggers of female human capital formation, in turn adjudicating between the relative importance of each theoretical determinant. In this section, we propose a simple theoretical framework to highlight how institutions that promote women's economic empowerment interact with traditional norms on gender roles to affect parental investment in girls' education. The model is very stylized and serves as a motivation for our empirical analysis.

Consider an economy in which an altruistic parent decides on his own consumption, the number of offspring (quantity of children), and the quality of children. As in [Galor and Moav \(2002\)](#), assume that the parent's preferences are represented by the following log-linear utility function:

$$u = (1 - \pi) \ln c + \pi(\ln n + \alpha_N \ln e) \quad (1)$$

where  $c$  refers to non-child consumption goods,  $n$  is the total number of living children, and  $e$  is the child's level of schooling (quality of children). The exogenous constant parameter  $\pi \in (0, 1)$  represents parental preferences for children over other forms of consumption goods. The degree of altruism of the representative parent or his/her preferences for child quality is given by  $\alpha_N \in (0, 1)$ . Unlike existing models of quantity-quality trade-off, we allow parental preferences for child quality to vary based on the cultural norm of the ethnic group to which the household belongs. We therefore assume that  $\alpha_N$  is a function that increases with the extent to which the cultural norm promotes women's human capital and participation in economic activities.

The representative household is endowed with one unit of time. The parent can allocate his/her time between labor market and childrearing activities. If the parent dedicates all his/her time to labor market activities, he/she will generate an income  $y$ . The time used for childrearing is time away from the labor market and thus represents the opportunity cost of childbearing. There are two costs (in time) associated with childbearing.  $K^n$  is the fraction of the parent's time dedicated to caring for children.  $K^e$  is the time cost for investing in one unit of education. It follows that the parent in this

economy faces the following budget constraint:

$$yn(K^n + K^e e) + c = y \quad (2)$$

Substituting (2) in (1), and solving for the optimization problem leads to the following optimal demand for quality and quantity of children:

$$e^* = \frac{\alpha_N}{1 - \alpha_N} \frac{K_n}{K_e} \quad (3)$$

$$n^* = \frac{\Pi}{K_n + K_e e} \quad (4)$$

It is straightforward to see that:

$$\frac{\partial e}{\partial K_e} = -\frac{\alpha_N}{1 - \alpha_N} \frac{K_n}{K_e^2} < 0 \quad (5)$$

$$\frac{\partial^2 e}{\partial K_e \partial \alpha_N} = -\frac{1}{(1 - \alpha_N)^2} \frac{K_n}{K_e^2} < 0 \quad (6)$$

The first comparative static captures the intuition that the parent will invest more in his/her daughter's education when the (relative) price of education decreases. However, the second comparative static implies that when the (relative) price of education decreases, education increases less in settings where cultural norms do not promote female empowerment. Based on the literature describes above, these comparative statics imply that parents will invest more in female education in the common law system as compared to the civil law system; however, this positive effect of the common law system on female education should be lower in settings where traditions constrain women's rights and empowerment. In summary, these comparative statics lead to the following proposition that we will formally test in Section 3.5.

**Proposition I:** *If women are more empowered in societies characterized by higher female participation in agricultural activities, no bride price payment, and matrilineal post-marital norm:*

1. *The impact of common law on female education will be stronger in societies where women were highly involved in agriculture.*
2. *The impact of common law on female education will be stronger in societies that do not practise bride price payment.*
3. *The impact of common law on female education will be stronger in matrilineal societies.*

Remark that although we emphasize female education when interpreting the model, it can also be interpreted with a reference to male education. In fact, to the extent that the promotion of women's rights is antithetical to that of men's rights (as in a constant-sum game of rights and resource allocation), our model predicts that the common law system will positively affect male education, but that this positive effect will be less pronounced in societies where traditional norms promote women's rights. Alternatively, the positive effect of the common law system on male education should be greater in societies where ancestral norms do not promote women's rights. Despite the fact that male education is not our main outcome in this chapter, we test and validate this hypothesis.

### **3.3 Data and Descriptive Statistics**

To investigate how legal origins interact with ancestral norms of gender roles to shape long-term women's economic empowerment, this chapter associates contemporary individual-level data with ancestral lifeways characteristics of the ethnic group from which they are descended. Our analysis relies on several data sources including the historical data on pre-colonial or ancestral ethnic homelands in Africa with detailed socio-economic characteristics of these groups, the Demographic and Health Surveys from which we obtain measures of women's empowerment outcomes, and other datasets with geographic and ecological variables relevant for our empirical analysis. First we will describe each of these datasets and we will highlight the methodology used to link individuals to their ancestral ethnic group. Second we will describe in detail the main variables used in our empirical analysis.

### 3.3.1 Individual-Level Data

We obtain individual-level information from the Demographic and Health Surveys (DHS). DHS have been conducted in sub-Saharan African countries since 1986. These nationally representative surveys provide reliable information on socio-economic and demographic characteristics of women aged 15 to 49 years. We exploit this set of information to construct our main outcomes of interest.

The central goal in this chapter is to compare women’s empowerment outcomes across civil and common law countries, and analyze how the effect of legal origins depends on ancestral cultural norms of gender roles. We therefore focus on the DHS conducted in the subset of adjacent countries with different legal origins.<sup>4</sup> In addition, we only consider DHS waves for which location coordinates of enumeration areas (or villages) are available and for which information on the ethnic identity of the respondent is collected. To ensure full exposure to primary education, we further restrict our analysis to women aged at least 20 years old. This leaves us with 142,301 women living in 19 countries among which 9 are common law countries and 10 are civil law countries (see appendix table A3.7 for a list of the countries by legal traditions).

### 3.3.2 Ethnicity-Level Data

Our empirical strategy includes ethnic homeland fixed effects. It is thus necessary to identify historical ethnic homelands that span across countries with different legal origins. We draw information about the spatial location of pre-colonial ethnic homelands in Africa from the George Peter Murdock’s Ethnographic Map of Africa (1959). This map portrays the spatial boundaries of 826 ethnic groups across Africa in the eve of the colonial era. Following a similar approach as in Michalopoulos and Papaioannou (2014, 2016) and Anderson (2018), we intersect Murdock’s ethnographic map of Africa with the digital chart of the world which portrays contemporary national boundaries in Africa. This allows us to identify 86 ancestral ethnic homelands divided across the set of 19 countries with different legal origins (Figure 3.1). Using geospatial information of enumeration areas in the DHS, we were able to associate each individual to his/her ancestral ethnic

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<sup>4</sup>Data on legal origins come from La Porta et al. (1999).

homeland.

Due to migration, relying on the current place of residence may not accurately identify the descendants of each historical ethnic homeland. To address this issue, we exploit the ethnic identity declared in the DHS to link individuals with the ethnic homeland of their ancestors. Given the discordance of ethnicity names available in DHS and Murdock’s ethnographic map, we rely on a new system of concordance of ethnic group in Africa to match ethnicities in the DHS with their corresponding ancestral ethnic groups in Murdock’s ethnographic map. This system of concordance has been constructed by [Michalopoulos et al. \(2019\)](#). There is a positive correspondence for 41,681 women. Among these women, we restrict our analysis to respondents who are currently living in the homelands of their declared ethnicity. Finally, our empirical analysis is based on a sample of 16,966 women living in 39 ethnic groups partitioned across 7 common law countries and 6 civil law countries.

We augment the final dataset with information on the pre-industrial socio-economic characteristics of historical ethnic groups in Africa. We draw this information from Murdock’s Ethnographic Atlas (1967) which is a database of around 60 variables describing the social, economic and political traits of 1270 historical ethnic groups around the world. In this study, we focus on three socio-economic characteristics: (1) female participation in agriculture, (2) bride price tradition, and (3) post-marital residence rules. We provide more details on these variables in the next section.

### 3.3.3 Pixel-Level Data

To validate our empirical strategy, we provide evidence that legal origins is orthogonal to several geographical characteristics (see Section 3.4). We measure these geographical characteristics at a very fine level. Following [Michalopoulos and Papaioannou \(2013\)](#), we divide Africa into pixel units of 12km  $\times$  12km. For each pixel, we rely on different sources (see Section 3.8) to collect information on the following measures of geographic and natural endowments: elevation, soil suitability for agriculture, area under water (rivers, lakes, streams), natural resources, distance from the centroid of a pixel to the sea coast, and distance from the centroid of a pixel to the national border. In Section 3.4, we show that civil and common law countries do not systematically differ along

these characteristics, which is reassuring as it implies that within each ethnic homeland, areas across the national border with different legal origins are good counterfactuals. Nevertheless, in the empirical analysis in Section 3.5, we also show regression results controlling for these variables.

### 3.3.4 Descriptive Statistics

#### 3.3.4.1 Dependent Variables

The first part of Table 3.1 reports summary statistics for our dependent variables, which include different measures of women’s empowerment. Our main outcome of interest is “female education”. In our empirical analysis we consider four different indicators measuring female education. The first indicator is a continuous variable equal to the total number of years of education declared by the respondent at the time of the survey. The three other measures are three binary indicators for whether the respondent has completed at least one year of schooling, primary education, and secondary education, respectively. Focusing on the first four rows in Table 3.1 we see that women in our sample have on average 3 years of education. On average, 52%, 24%, and 5% of women have completed at least one year of schooling, primary education, and secondary education, respectively.

Other measures of female economic empowerment that we consider include: female employment and marriage outcomes. First, we measure female employment with a dummy variable indicating whether the respondent is working in the professional sector (e.g. managerial positions or clerical). Table 3.1 shows that only 3% of women in our sample are working in the professional sector. Second, we focus on two marriage outcomes. We consider early marriage defined by two dummies indicating if the respondent marry before age 16 or 18. The second marriage outcome we consider is the partner age gap calculated as the husband-wife age difference. Table 3.1 shows that 32% and 56% of women in our sample marry before age 16 and 18, respectively. In addition, the mean husband-wife age gap is 8 years.

### 3.3.4.2 Ancestral Norms of Gender Roles

Throughout the analysis in this chapter, we will focus on three ancestral cultural characteristics that are measured using Murdock’s ethnographic Atlas. The first is “female participation in agriculture”. Murdock’s Atlas contains detailed information on productive activities prior to industrialization including agricultural activities. In addition, it reports for each productive activity, information about gender differences in participation in this activity. The variable providing information on gender differences in participation in agriculture contains the following categories: differentiated but equal participation; equal participation; female appreciably more; females only; males only; and males appreciably more. Using this variable, we construct our indicator for female participation in agriculture which is a dummy variable equal 1 if there was differentiated but equal participation, there was equal participation, women participated more, or if women only participated, and zero otherwise. Descriptive statistics reported in Table 3.1 suggest that 64% of women in the sample belongs to ethnic groups where there was equal gender participation or women participated more in agricultural activities.

Murdock’s Atlas also reports detailed information on a number of marriage traditions. One of these marriage traditions that we focus on in this chapter is the bride price custom. Bride price is a transfer in cash or in kind made by the groom or his family to the bride’s family. Based on the payment categories listed in Murdock’s Atlas, we define bride price as a dummy equal to 1 if the prevalent mode of marriage payment prior to industrialization was equivalent to a transfer of bride price or wealth to the bride’s family, an exchange of a bride service to the bride’s family, or a payment of a token bride price, and zero otherwise.<sup>5</sup> In our sample, 66% of women belongs to ethnic groups that traditionally practise bride price payment upon marriage.

The last traditional norm that we consider in this chapter is the post-marital settlement rule. Murdock distinguishes between the following post-marital residence categories: Patrilocal, Virilocal, Ambilocal, Avunculocal, Uxorilocal, Matrilocal, Neolocal, No common residence, Optionally patrilocal, Optionally uxoriocal or avunculocal. We define the variable patrilocality as a dummy equal to 1 if the ethnic group follows the patrilocal or the virilocal post-marital residence rule; and zero otherwise. Table 3.1 shows that 67%

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<sup>5</sup>The other categories in Murdock’s Atlas include: absence of consideration, gift exchange reciprocal, sister or female relative exchanged for bride.

of women in our sample originate from patrilocal societies.

## 3.4 Identification Strategy

### 3.4.1 Baseline Specification

Our goal is to estimate the causal impact of legal origins on women’s empowerment, and to analyze how this effect varies with ancestral norms of gender roles. To identify this effect, we implement a spatial Regression Discontinuity Design with ethnic homeland fixed effects. Relying on the quasi-natural experiment that occurred during the “Scramble for Africa”, our methodology exploits the arbitrary drawing of African borders and the fact that it splits ancestral ethnic homelands across countries with different legal traditions. Our main empirical specification is represented by the following equation:

$$Y_{ipcet} = \alpha + \beta \text{Common}_c + f(\text{BD}_{pce}) + \delta_e + \gamma_t + \theta_r + X'_{ipcet} \sigma + Z'_{pce} \mu + \varepsilon_{ipcet} \quad (1)$$

The dependent variable  $Y_{ipcet}$  reflects the outcome of interest for a women  $i$ , born at time  $t$ , and whose current residence is on the historical homeland of ethnic group  $e$ , located in pixel  $p$  within country  $c$ . The variable  $\text{Common}_c$  is equal to one for common law countries and zero for civil law countries.  $\beta$  is the coefficient of interest. It measures the local average effect of common law (vs. civil law) traditions on the outcome of interest.  $\delta_e$ ,  $\gamma_t$ , and  $\theta_r$  correspond to ethnic homeland fixed effects, cohort of birth fixed effects, and religion fixed effects, respectively.  $X'_{ipcet}$  is a vector of individual controls including age and age squared. In most specifications we control for a rich set of location and geographical variables including the shortest distances from the centroid of the pixel to the coastline and to the national border, area under water, elevation, soil suitability for agriculture, pixel area, and natural resources. These variables are measured at the pixel level and are denoted by the vector  $Z'_{pce}$ .

The running variable in our RD specification is given by the distance from the centroid of each pixel to the nearest national border with different legal origins. To allow the

intercept of the relationship between the outcome of interest and the running variable to be different on each side of the border, we add to our baseline specification a local quadratic polynomial of the distance from the centroid of each pixel to the nearest national border with different legal origins. This non-parametric functional form is represented by  $f(\text{BD}_{pce})$ . Yet, we will show in Section 3.5 that our results are robust to a specification that considers a cubic RD polynomial of the distance from the centroid of each pixel to the nearest national border with different legal origins. To account for the multidimensional nature of the cut-off in a spatial RDD, we follow Dell (2010) and show that our results are also robust to controlling for a cubic RD polynomial in latitude and longitude of the pixel.

In our setting, non-adjusted standard errors are likely to be biased due to autocorrelation. As several ethnic homelands in the same country will have similar legal origins, clustering standard errors at the country-level is required. Moreover, given that Murdock (1959) identifies more than 800 pre-colonial ethnic groups across Africa and assigns them into 96 ethnolinguistic families, clustering at the ethnic-family level is also relevant. Following Cameron et al. (2011) we, therefore, cluster standard errors along both the country and the ethnic-family dimensions. In addition to account for the aforementioned concerns, the double-clustering parameterization also accounts for arbitrary residual correlation within each dimension.

### 3.4.2 Validity of the Identification Strategy

Our identification strategy consists in comparing the outcome of interest for women within the historical homeland of the same ethnic group in adjacent countries with different legal origins. There are two key assumptions underlying this empirical strategy. The first assumption requires that assignment across common and civil law traditions is as good as random. Put differently, the border between civil and common law countries should not be influenced by factors that themselves determine the outcome of interest. This interpretation is supported by the extensive historical and economic literature providing evidence that African borders were drawn in an arbitrary manner (see, for instance, Michalopoulos and Papaioannou (2015, 2013) for an extensive review of this literature). The central argument in this literature is that at the time of the “Scramble for Africa”, in the late 19th

century, Europeans divided Africa with limited knowledge of local conditions.

The second identification assumption requires that relevant determinants of the outcome of interest, besides the treatment, should not systematically vary at the border. In our setting it means that ethnic areas across the border should be similar across all relevant determinants of women’s empowerment, with the exception of legal origins. We empirically investigate whether differences in legal origins translate into differences in various geographical characteristics of the ethnic homeland that are likely to influence women’s empowerment outcomes. Table 3.2 reports the RD estimates that associate geographic, ecological, and natural resources measures with legal origins. We focus in this analysis on pixels falling within 60km on each side of the national border. Results are supportive of our empirical strategy. In general, we find no significant effect of common law on any of the numerous geographic, ecological, and natural resources measures. Even though this result ensures that the two areas of the same ethnic homeland partitioned between civil and common law countries are good counterfactual, we control for these geographical characteristics in our empirical analysis.

## **3.5 Regression Discontinuity Results**

In this section, we formally implement the RD design described in Section 3.4 to estimate the long-term impact of legal origins on women empowerment in a variety of cultural contexts. The first subsection describes the average causal impact of common law (vs. civil law) on women empowerment outcomes. The second subsection describes how this effect depends on ancestral ethnic norms which may determine gender roles.

### **3.5.1 Legal Origins and Female Empowerment**

#### **3.5.1.1 Female Education**

The theoretical literature on the historical determinants of female education suggests that there should be a positive relationship between the common law system and female education. We test this proposition by estimating equation 1 using four key outcome variables: an indicator for the total number of years of education and three dummies

indicating whether the respondent has completed at least one year of schooling, primary education, and secondary education, respectively. Before presenting these estimation results, we first provide a graphical illustration of the RD estimates.

The RD plots are displayed in Figure 3.2 where the running variable is given by the geodesic distance (in kilometers) from the centroid of each pixel to the nearest national border with different legal origins. The vertical red line represents the national border in adjacent countries with different legal origins. Each graph plots, for individuals within bins of 5 kilometer width, the average of the outcome of interest predicted using equation 1.<sup>6</sup> We report result using a 100 kilometer bandwidth. Each RD graph also plots the estimated values from a local polynomial regression of the outcome of interest on the running variable fitted separately using data from common law countries (where the running variable takes on positive values) and data from civil law countries (where the running variable takes on negative values). We see from Figures 3.2-a to 3.2-d that regardless of how we measure female education, women living in common law countries are significantly more educated than women living in civil law countries.

The positive association between the common law legal system and female education is corroborated in the regressions results. Table 3.3 presents results from estimating equation 1. This table shows the RD estimates of the effect of the common law system (in comparison to the civil law) on our four key measures of female education. In *Panel A*, the dependent variable is the total number of years of education. In *Panels B, C, and D*, the dependent variables are dummies indicating whether a woman has completed at least one year of schooling, primary education, and secondary education, respectively. Each specification in this table controls for ethnic homelands fixed effects, age and age squared, cohort of birth fixed effects, and religion fixed effects. For inference purpose, and to account for spatial autocorrelation, robust standard errors adjusted for double-clustering at the country and ethno-linguistic family are reported in parentheses. We start by displaying results using a naive within-ethnic homeland Ordinary Least Squares (OLS) regressions without the RD polynomial in column (1). In columns (2)-(5), we present the RD estimates using different choices of bandwidths and controlling for a quadratic RD-polynomial in the distance from the centroid of each pixel to the national border with

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<sup>6</sup>GPS latitude/longitude positions are randomly displaced in the DHS by a maximum of 5 kilometers. This is to ensure confidentiality. This random displacement should not affect our findings since the displacement is done within the region of residence of the respondents.

different legal origins.

Across all specifications, we find results consistent with the RD graphs. We see a statistically significant positive impact of common law on female education. This result holds regardless of how we measure female education. For example, *Panel A* shows that the total number of years of education is between 1.2 and 1.7 greater for women in common law countries compared to their counterparts in civil law countries. This effect is large in magnitude. It implies that the female educational level is about 38% higher in common law countries, as the average educational attainment throughout the region examined in this chapter is 3.12 years of education.

Although we have shown that areas on the two sides of the border display balanced geographical characteristics and are thus good counterfactuals (see Table 3.2), we nevertheless control for these characteristics in alternative specifications. Results are shown in *Panels E-H* of Table 3.3. Controlling for geographic features including altitude, soil suitability for agriculture, area under water, natural resources, and distance to the sea coast does not change our results. Additionally, Appendix tables A3.2 and A3.3 also probe the robustness of our results to specifications that control for different functional forms of the RD-polynomial including a cubic polynomial in distance to the nearest national border with different legal origins and a polynomial in latitude-longitude.

We also check whether the effect of the common law on female education is fading away over time. In Appendix Table A3.4, we split our sample into two subgroups: women born before or in 1976 and women born after 1976, respectively. This year is chosen to obtain subsamples of equal size. We find that the estimated effect is larger for younger cohorts, suggesting that the disparities in education between women of similar ethnic groups in common and civil law countries is not fading away.

### 3.5.1.2 Female Employment and Marriage

In Table 3.4, we report estimates of the effect of common law on women's employment and marriage outcomes. As in Table 3.3, we present the OLS estimates in column 1 and the RD estimates where we control for a second-order polynomial in the distance to the national border with different legal origins in columns (2)-(5).

DHS surveys report information about the type of occupation in which the respon-

dent is currently working. The categories include non-agricultural high skilled workers, agricultural workers and low or unskilled workers. We focus on professional or high-skill activities as our objective is to measure the extent of female empowerment through female participation in activities that require a reasonable education level. In *Panel A*, we explore the impact of common law on the likelihood of working in the professional sector. The estimates show that women in common law countries are more likely to work in the professional sector. The estimates are statistically significant at conventional levels in most specifications. The estimates suggest that the common law system increases the likelihood to work in the professional sector by about 1 percentage point, which compared to the sample mean represents a rise of around 33%.

Analogous estimates of the effect of common law on marriage outcomes are displayed in *Panels B-D* of Table 3.4. We focus on measures of the risk of early marriage (that is marriage before age 16 or 18) and the age gap between spouses. We see that women in common law countries are less likely to marry at a young age. The effect is statistically significant only for the risk of marriage before age 18 (*Panel C*). Moreover, common law is positively associated with the age gap between a woman and her partner (*Panel D*), but this effect is not significant at conventional levels.

Note that our results are also robust to specifications where we augment the baseline regression in equation 1 with a rich set of geographic controls. In results not shown, we find that these estimates are also robust to other functional forms of the RD polynomial. Overall, we find that women in common law countries are more likely to work in the professional sector and less likely to marry before age 18.

### **3.5.2 Heterogeneity: Traditional Norms, Legal Origins, and Female Empowerment**

We now turn to the analysis of how the presence of ancestral or traditional norms which determine gender roles may generate heterogeneity in the effect of legal origins. Following our conceptual framework in Section 3.2, we argue that the positive relationship between common law and women’s empowerment outcomes should be lower in societies with ancestral norms that foster gender inequality including: (1) norms of low female participation in agriculture, (2) norms of bride price payment, and (3) patrilocal norms.

Before turning to this analysis, we first check whether these traditional norms are significantly related to female education. Appendix Table [A3.5](#) reports estimates in which the dependent variable is the number of years of education for women in our sample and the variables of interest are our three traditional norms. Columns 1, 3 and 5 do not include control variables, whereas columns 2, 4 and 6 control for age and age-squared. Overall, we find strong evidence that women living in societies that traditionally fostered gender inequality are less educated. In columns 1 and 2, we find that women currently living in societies with a traditional norm of high female participation in agriculture are significantly more educated. The estimates presented in columns 3 and 4 suggest that respondents living in societies with the bride price are less educated. Last, columns 5 and 6 present evidence that women in societies with a patrilocal norm are also less educated.

We now test our main hypotheses by comparing the causal impact of legal origins in different cultural contexts resulting from the three aforementioned traditional norms of gender roles. Formally, we split our sample in two subsamples depending on whether or not the respondent belongs to an ethnic group that traditionally practised the corresponding norm (e.g., bride price). We then estimate equation [1](#) separately for each subsample. Results are reported in Tables [3.5](#) and [3.6](#). In each of these tables, we present results for observations falling within 60 kilometers on each side of the national border with different legal origins. Across all specifications in these tables, we control for ethnic homeland fixed effects, age and age-squared, cohort of birth fixed effects, religion fixed effects, a second-order RD polynomial in distance to the national border with different legal origins, and a set of geographical features. We corroborate findings in Tables [3.5](#) and [3.6](#) by estimating a variant of equation [1](#) where we interact the common law dummy with the ancestral norm indicator variable. Results using the latter specification are reported in Appendix Table [A3.1](#).

### **3.5.2.1 Female Participation in Agriculture**

Our first hypothesis is that the impact of common law on female education is stronger in societies where, historically, women were highly involved in agriculture. As discussed in Section [3.3](#), we measure female participation in agriculture using ethnographic sources.

Results from estimating the effect of legal origins on female education separately for

ethnic groups with high female participation in agriculture and those with low female participation in agriculture are reported in columns (1) and (2) of Table 3.5, respectively. In *Panel A*, the dependent variable is the total number of years of education. We see that the effect of common law on female education is positive and significant in the two subsamples. However, consistent with the first hypothesis, the positive effect of common law on the number of years of education is greater for women originating from societies where women were highly involved in agricultural activities. Estimates suggest that women in common law countries have 2.5 (0.9) years of education more in common law countries for the subsample of societies where, historically, women were (not) highly involved in agriculture. Similarly, women in common law countries are significantly more likely to have graduated from primary and/or secondary school than women in civil law countries in societies where women were highly involved in agricultural activities (*Panels B-D*).

These results are consistent with the idea that in societies that historically encouraged women's participation in productive activities, institutions or policies that decrease the price or the relative price of education incentivize parents to invest more in girls' education.

In Table 3.6, we focus on the heterogeneous impact of legal origins on women's employment and marital outcomes. The results in the first two columns show that women are significantly more likely to work in the professional sector only in societies where in the past women were highly involved in agricultural activities. In contrast, the effect is small and not statistically different from zero in ancestral societies with low female participation in agriculture. This result is also consistent with the stronger positive impact of common law on female education in ancestral societies characterized by high female participation in agricultural activities.

Examining heterogeneity in the effect of common law on marriage outcomes also reveals that the negative impact of common law on the risk of early marriage is only observable for women originating from societies with high participation of women in agricultural activities. Similarly, we find that the age gap between the spouses is significantly lower for women in common law countries only when in the past, women were highly involved in agricultural activities. On average, the age gap is 0.7 years lower in common law (vs. civil law) countries among ethnic groups that traditionally promote the partici-

pation of women in subsistence activities. In contrast, the age gap is significantly larger in common law (vs. civil law) countries among ethnic groups where historically women were less likely to participate in productive activities. These results imply that the average positive effect of common law on the partner age gap (see Table 3.4) in fact mask important heterogeneity depending on the cultural context.

### 3.5.2.2 Bride Price

The second hypothesis is that the impact of common law on female education will be stronger in societies that do not practise bride price payment. Using the indicator of bride price custom defined in Section 3.3, we split the sample into two subgroups depending on whether the respondent belongs to an ethnic group that traditionally practise bride price payment upon marriage.

Results from estimating the effect of common law on female education separately in each subsample are displayed in columns (3) and (4) of Table 3.5). Common law has a positive and statistically significant impact on female education (in all panels) in both subgroups with and without bride price payment. But, consistent with the theory, the positive effect of common law on education is greater for non-bride price ethnic groups. The estimated effect suggests that common law increases the number of years of education by about 2.5 in non-bride price ethnic groups, while this effect is only 0.70 in bride price ethnic groups.

Columns (3) and (4) of Table 3.6 report heterogeneous effects of common law on female employment and marital outcomes by bride price custom. Consistent with its heterogeneous effects on female education, we also find that the estimated of common law on a woman's chance of working in the professional sector is significantly greater only in the non-bride price ethnic groups (*Panel A*). In addition, we also find that for the non-bride price ethnic groups, the risk of early marriage and the age gap between the spouses are significantly lower in common law (vs. civil law) countries, while in societies that practise bride price payment, we observe no effect of common law on early marriage and a positive effect on the husband-wife age gap (*Panels B-D*).

### 3.5.2.3 Patrilocality versus Matrilocality

Our third hypothesis is that the positive impact of common law on female education is stronger in matrilocal (vs. patrilocal) societies. As previously discussed, matrilocal post-marital residence increases parental financial connections with daughters relative to sons, and it generates a greater incentive for parents to invest in their daughters' human capital. As a result, the response to a decline in the price of education is likely to be stronger for female education in matrilocal societies. This interpretation is consistent with the empirical results shown in columns (5) and (6) of Table 3.5. In this table, we estimate the effect of common law separately in matrilocal and patrilocal societies. We see that the positive effect of common law on female education is much greater for women in matrilocal societies compared to patrilocal societies. For instance, in matrilocal societies, we find that women in common law (vs. civil law) countries have, on average, 2.5 more years of education. In comparison, in patrilocal societies, women in common law countries have only 0.72 more years of education than their counterparts in civil law countries.

Finally, estimates showing how the effects of legal origins on female employment and marriage outcomes depend on the post-marital residence rule are displayed in columns (5) and (6) of Table 3.6. Consistent with the findings uncovered so far, we find that common law has a positive and significant impact on the likelihood of working in the professional sector only in matrilocal ethnic groups (*Panel A*). Moreover, common law has a significant negative impact on the risk of early marriage and on the age gap between the spouses only in matrilocal societies (*Panels B-D*).

All of these findings are consistent with the predictions of our model.

### 3.5.2.4 Ancestral Norms, Legal Origins, and Male Education

The heterogeneous results describe above imply that, formal institutions that sufficiently reduce the cost (or the relative cost) of education, such as the common law system, provide incentives for parents to invest more in their daughters' education only in settings with traditional norms that historically foster women's empowerment. This conclusion is strengthened by the regression results showing the effect of legal origins on male education. We see in Appendix Table A3.6 that common law has a stronger benefit for male education

mainly in societies where ancestral norms of gender roles do not promote women’s rights. Appendix Table A3.6 shows that the positive impact of common law on male education is much larger in societies where women were not involved in agricultural activities, and that this positive impact is only present in bride price societies and in patrilocal societies. These findings are consistent across all the measures of education we examine.

Overall, the heterogeneous effect of common law on women’s outcomes strongly suggests that, to be optimal, the design of formal institutions should take into account the cultural and traditional context in which they will be implemented. An alternative interpretation of our findings is that better formal institutions can attenuate the negative impact of ancestral cultural norms that constrain women’s rights on their economic empowerment. Indeed, as we have shown, ancestral norms that constrain women’s rights negatively affect their outcomes (Appendix Table A3.5), but these negative effects are attenuated by the common law system (Appendix Table A3.1). This interpretation has important policy relevance, especially given that formal institutions are easier to change than ancestral traditions. Finally, the fact that the common law system positively affects male education mainly in societies that do not traditionally promote women’s rights suggests traditional norms of gender roles directly oppose women’s rights to men’s rights, which in turn implies that the design of formal institutions aimed to address the plight of women in societies that constrain them should win the support of men.

### 3.6 Conclusion

In this chapter, we studied the interplay of legal origins and ancestral cultural norms of gender roles in shaping female economic empowerment in sub-Saharan Africa. We analyzed outcomes such as female education, female employment, early marriage, and the husband-wife age gap. Exploiting the arbitrary division of ancestral ethnic homelands across countries with different legal origins, we directly compared women among the same ethnic group living in civil law countries and common law countries. We found that, on average, women in common law countries are significantly more educated than their counterparts in civil law countries. Similarly, they are more likely to work in the professional sector, and are less likely to marry at young age.

These effects are either absent or significantly lower in settings where ancestral cul-

tural norms do not promote women's rights and empowerment. In particular, we found little effect in bride price societies, patrilocal societies, and societies where women were not involved in agriculture in the past. An alternative interpretation of our findings is that better formal institutions can significantly attenuate the negative impacts of traditional cultural norms that constrain women's rights. In addition, the results suggest that the ancestral promotion of women's rights directly opposes that of men's rights, as we found a positive effect of the common law system on male education mainly in societies that do not traditionally promote women's rights. Our findings imply that, to be optimal, the design of legal institutions should account for ancestral traditions. Indeed, legal origins and institutions alone might not be enough to promote women's economic outcomes. The interplay between deeply-rooted cultural norms and economic and legal institutions is key to ensure significant progress in important dimensions of women's economic empowerment.

### 3.7 Figures

Figure 3.1: Ethnic homelands partitioned between civil and common law countries

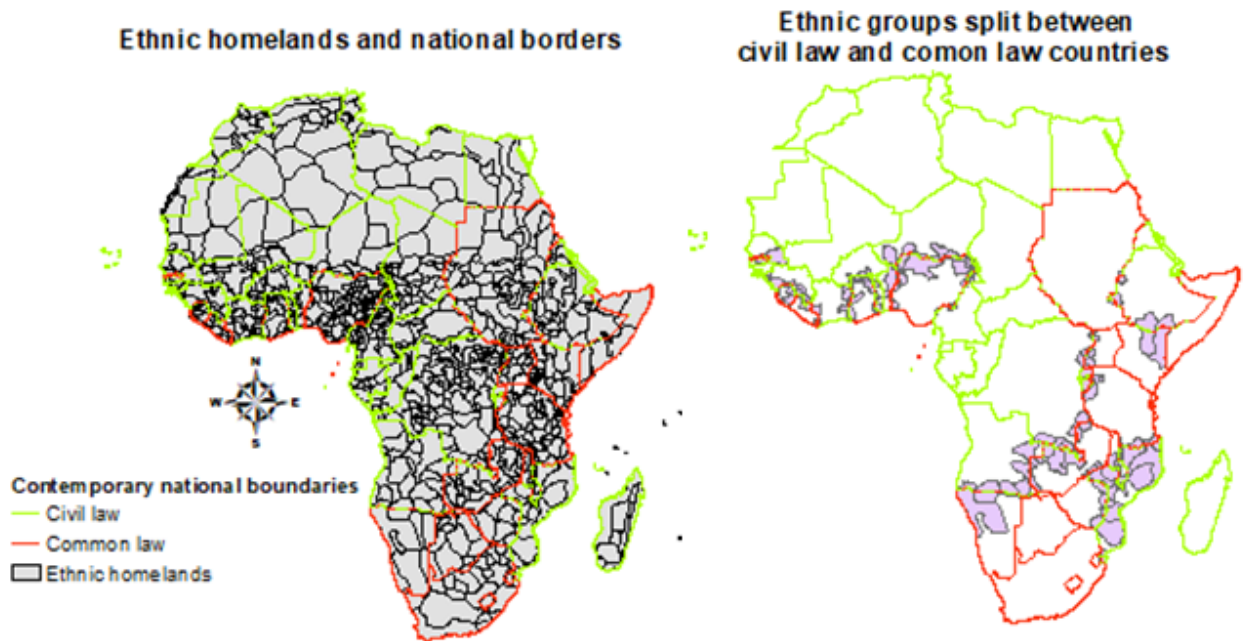
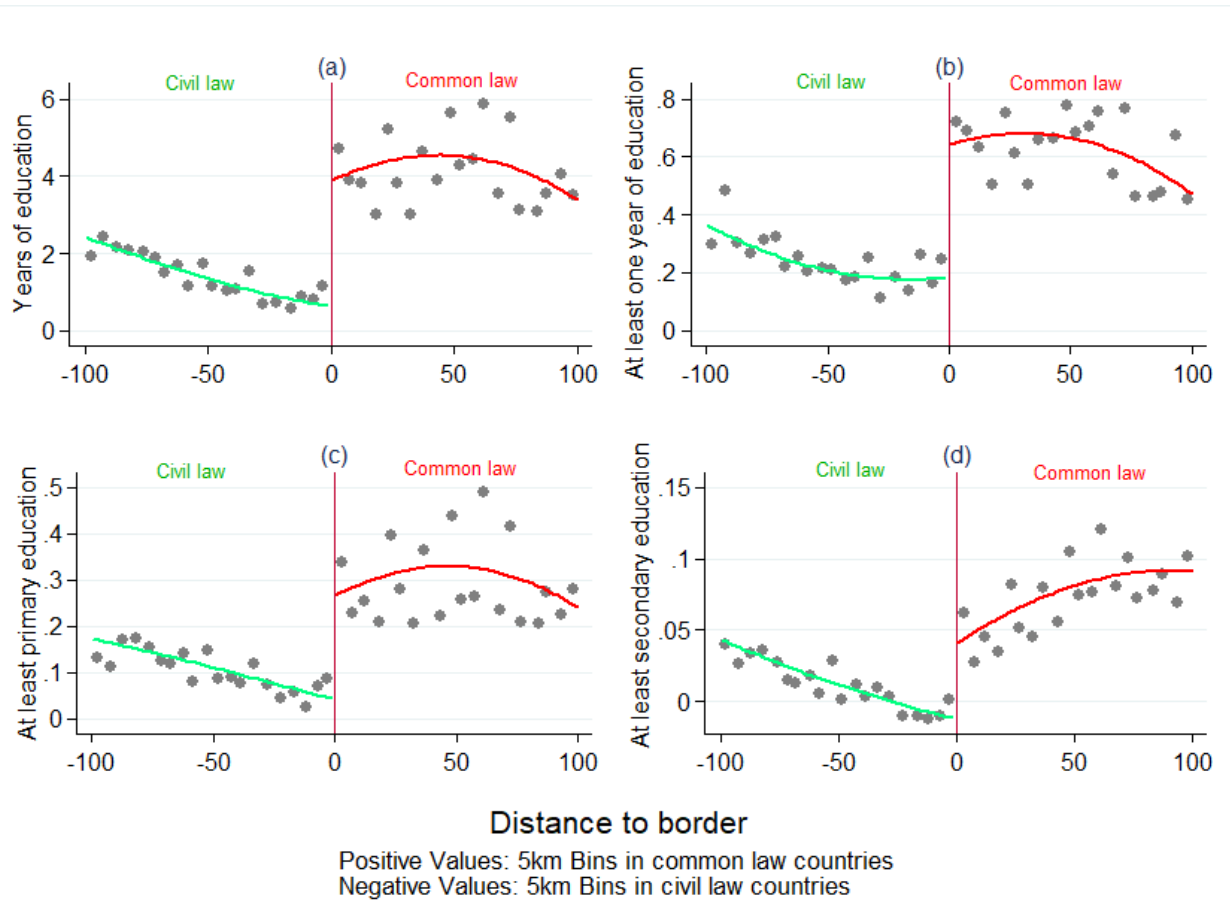


Figure 3.2: Average effects: Legal origins and female education



## 3.8 Tables

Table 3.1: Descriptive Statistics - Individual level data

	N	Mean	Std. de.	Min.	Max.
Years of education	8,406	3.29	3.98	0	17
Has no education	8,406	0.52	0.50	0	1
Has at least primary education	8,417	0.24	0.43	0	1
Has at least secondary education	8,417	0.05	0.21	0	1
Year of birth	8,419	1974	9.48	1948	1994
Age at survey	8,419	31.59	8.24	20	49
Mother is married	7,930	0.70	0.46	0	1
Early marriage (before age 16)	7,016	0.32	0.47	0	1
Early marriage (before age 18)	7,016	0.56	0.50	0	1
Percentage of muslim	8,400	0.33	0.47	0	1
Percentage of christian	8,400	0.46	0.50	0	1
Husband-wife age gap	6,188	8.42	7.49	-16	57
Respondent's working	7,909	0.69	0.46	0	1
Professional sector	7,909	0.03	0.17	0	1
Female participation in agriculture	6,987	0.64	0.48	0	1
Bride price	7,321	0.66	0.47	0	1
Patrilocality	7,211	0.67	0.47	0	1

Table 3.2: Validity of identification design at the pixel level

	Dependent variable is:						
	Elevation (1)	Soil suitability (2)	Area under water (3)	Pixel area (4)	Diamonds (5)	Dist. to the border (6)	Dist. to sea (7)
Common law	45.49 (47.734)	0.07*** (0.006)	6.71 (4.942)	2.55 (3.865)	0.00 (0.002)	-0.11 (0.881)	9.16 (41.456)
N	437	434	437	437	437	437	437
Countries	13	13	13	13	13	13	13
Ethno-linguistic families	23	23	23	23	23	23	23
Ethnic homeland FE	✓	✓	✓	✓	✓	✓	✓

*Note: In each specification, the unit of observation is the pixel. The table reports RDD estimates associating various location and geographical characteristics at the pixel level with legal origins. In each specification we restrict the analysis to observations within 60km of the border and we control for a second-order polynomial in the distance from the centroid of each pixel to the national border with different legal origins. Below the estimates, we report in parentheses standard errors clustered both at the country and the ethno-linguistic family levels. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .*

Table 3.3: Average effects: Legal origins and female education

	RDD - Bandwidth				
	OLS (1)	<250 km of bound. (2)	<150 km of bound. (3)	<100 km of bound. (4)	<60 km of bound. (5)
<b>Baseline specification</b>					
<b>Panel A: Years of education</b>					
Common law	1.74*** (0.493)	1.26** (0.523)	1.27** (0.550)	1.31** (0.644)	1.18** (0.486)
Observations	16,307	15,069	13,251	11,062	7,880
<b>Panel B: Completed one year of schooling</b>					
Common law	0.22*** (0.071)	0.17** (0.073)	0.17** (0.076)	0.18** (0.085)	0.18*** (0.069)
Observations	16,307	15,069	13,251	11,062	7,880
<b>Panel C: Completed primary schooling</b>					
Common law	0.12*** (0.019)	0.09*** (0.025)	0.09*** (0.027)	0.09*** (0.034)	0.07*** (0.020)
Observations	16,319	15,081	13,262	11,073	7,890
<b>Panel D: Completed secondary schooling</b>					
Common law	0.05*** (0.004)	0.04*** (0.007)	0.04*** (0.007)	0.04*** (0.011)	0.02*** (0.006)
Observations	16,319	15,081	13,262	11,073	7,890
Countries	13	13	13	13	13
Ethno-linguistic families	23	23	23	23	23
<b>Geographic controls</b>					
<b>Panel E: Years of education</b>					
Common law	1.53*** (0.486)	1.24*** (0.443)	1.27** (0.505)	1.25** (0.612)	1.17*** (0.445)
Observations	16,232	15,019	13,221	11,032	7,850
<b>Panel F: Completed one year of schooling</b>					
Common law	0.20*** (0.067)	0.18*** (0.065)	0.18*** (0.070)	0.18** (0.080)	0.18*** (0.069)
Observations	16,232	15,019	13,221	11,032	7,850
<b>Panel G: Completed primary schooling</b>					
Common law	0.10*** (0.028)	0.08*** (0.021)	0.09*** (0.025)	0.08** (0.033)	0.07*** (0.018)
Observations	16,244	15,031	13,232	11,043	7,860
<b>Panel H: Completed secondary schooling</b>					
Common law	0.04*** (0.005)	0.03*** (0.006)	0.03*** (0.008)	0.03** (0.013)	0.02*** (0.005)
Observations	16,244	15,031	13,232	11,043	7,860
Countries	13	13	13	13	13
Ethno-linguistic families	23	23	23	23	23
Ethnic homeland FE	✓	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓
Age controls	✓	✓	✓	✓	✓

*Note:* Specifications in columns (2)-(5) control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron *et al.*, 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, light density, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 3.4: Average effects: Legal origins, female employment, and marriage

	RDD - Bandwidth				
	OLS (1)	<250 km of bound. (2)	<150 km of bound. (3)	<100 km of bound. (4)	<60 km of bound. (5)
<b>Baseline specification</b>					
<b>Panel A: Work in the professional sector</b>					
Common law	0.0130*** (0.003)	0.0086*** (0.003)	0.0093*** (0.003)	0.0111** (0.004)	0.0055 (0.008)
Observations	16,319	15,081	13,262	11,073	7,890
<b>Panel B: Early marriage (before age 16)</b>					
Common law	-0.03 (0.034)	-0.03 (0.036)	-0.03 (0.034)	-0.03 (0.036)	-0.03 (0.026)
Observations	14,787	13,714	11,959	9,857	6,983
<b>Panel C: Early marriage (before age 18)</b>					
Common law	-0.08*** (0.019)	-0.08*** (0.023)	-0.08*** (0.021)	-0.06*** (0.023)	-0.04*** (0.012)
Observations	14,787	13,714	11,959	9,857	6,983
<b>Panel D: Partner age gap (Male-female)</b>					
Common law	1.57 (1.081)	1.48 (1.145)	1.46 (1.158)	1.36 (1.165)	1.73** (0.879)
Observations	13,107	12,198	10,613	8,724	6,160
<b>Geographic controls</b>					
<b>Panel E: Work in the professional sector</b>					
Common law	0.0110*** (0.002)	0.0092** (0.004)	0.0101** (0.005)	0.0117* (0.006)	0.0054 (0.008)
Observations	16,244	15,031	13,232	11,043	7,860
<b>Panel F: Early marriage (before age 16)</b>					
Common law	-0.02 (0.048)	-0.02 (0.034)	-0.04 (0.029)	-0.03 (0.034)	-0.04* (0.025)
Observations	14,719	13,670	11,935	9,833	6,959
<b>Panel G: Early marriage (before age 18)</b>					
Common law	-0.06* (0.033)	-0.06** (0.023)	-0.07*** (0.022)	-0.06** (0.025)	-0.04*** (0.011)
Observations	14,719	13,670	11,935	9,833	6,959
<b>Panel H: Partner age gap (Male-female)</b>					
Common law	1.51 (1.180)	1.47 (1.084)	1.51 (1.126)	1.44 (1.087)	1.71** (0.798)
Observations	13,050	12,161	10,594	8,705	6,141
Ethnic homeland FE	✓	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓
Age controls	✓	✓	✓	✓	✓
Countries	13	13	13	13	13
Ethno-linguistic families	23	23	23	23	23

*Note:* Specifications in columns (2)-(5) control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron et al., 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, light density, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 3.5: Heterogeneity: Traditional norms, legal origins and female education

	Female participation in agri.		Bride Price		Patrilocality	
	(1) (No)	(2) (Yes)	(3) (No)	(4) (Yes)	(5) (No)	(6) (Yes)
<b>Panel A: Years of education</b>						
Common law	0.89*** (0.165)	2.51*** (0.132)	2.57*** (0.064)	0.70*** (0.176)	2.56*** (0.059)	0.72*** (0.177)
Observations	2,333	4,239	2,498	4,405	2,388	4,405
<b>Panel B: Completed one year of schooling</b>						
Common law	0.16*** (0.016)	0.39*** (0.015)	0.40*** (0.004)	0.10*** (0.025)	0.40*** (0.003)	0.10*** (0.025)
Observations	2,333	4,239	2,498	4,405	2,388	4,405
<b>Panel C: Completed primary schooling</b>						
Common law	0.10*** (0.012)	0.15*** (0.011)	0.14*** (0.005)	0.07*** (0.021)	0.14*** (0.007)	0.08*** (0.021)
Observations	2,334	4,248	2,498	4,415	2,388	4,415
<b>Panel D: Completed secondary schooling</b>						
Common law	0.01* (0.008)	0.04*** (0.010)	0.04*** (0.001)	0.02** (0.009)	0.04*** (0.002)	0.02*** (0.008)
Observations	2,334	4,248	2,498	4,415	2,388	4,415
Ethnic homeland FE	✓	✓	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
Age controls	✓	✓	✓	✓	✓	✓
Countries	7	9	4	8	3	8
Ethno-linguistic families	7	12	5	14	4	15

*Note:* In each specification we restrict the analysis to observations within 60km of the border and we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origins. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron et al., 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, light density, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 3.6: Heterogeneity: Traditional norms, legal origins, female employment and marriage

	Female participation in agri.		Bride Price		Patrilocality	
	(1) (No)	(2) (Yes)	(3) (No)	(4) (Yes)	(5) (No)	(6) (Yes)
<b>Panel A: Work in the professional sector</b>						
Common law	-0.0046 (0.008)	0.0147** (0.007)	0.0145*** (0.003)	0.0000 (0.008)	0.0130*** (0.004)	0.0005 (0.008)
Observations	2,334	4,248	2,498	4,415	2,388	4,415
<b>Panel B: Early marriage (before age 16)</b>						
Common law	-0.00 (0.018)	-0.11*** (0.013)	-0.11*** (0.021)	0.02 (0.018)	-0.10*** (0.008)	0.02 (0.017)
Observations	2,287	3,523	2,413	3,690	2,303	3,690
<b>Panel C: Early marriage (before age 18)</b>						
Common law	-0.04 (0.032)	-0.03*** (0.009)	-0.05* (0.028)	-0.03 (0.029)	-0.04** (0.017)	-0.03 (0.029)
Observations	2,287	3,523	2,413	3,690	2,303	3,690
<b>Panel D: Partner age gap (Male-female)</b>						
Common law	2.67*** (0.445)	-0.69** (0.282)	-0.43*** (0.099)	2.80*** (0.328)	-0.43*** (0.067)	2.86*** (0.346)
Observations	2,174	2,993	2,039	3,358	1,949	3,360
Ethnic homeland FE	✓	✓	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
Age controls	✓	✓	✓	✓	✓	✓
Countries	7	9	4	8	3	8
Ethno-linguistic families	7	12	5	14	4	15

*Note:* In each specification we restrict the analysis to observations within 60km of the border and we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origins. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron et al., 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, light density, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

### 3.9 Appendix

Table A3.1: Traditional norms, legal origins, and female education (Specification with interaction term)

	Female participation in agri.		Bride Price		Patrilocality	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: Years of education</b>						
Common law	0.55*** (0.090)	0.50* (0.259)	2.48*** (0.058)	2.45*** (0.235)	2.48*** (0.064)	2.43*** (0.268)
Common law x Culture	1.96*** (0.127)	1.93*** (0.500)	-1.96*** (0.150)	-2.02*** (0.472)	-1.97*** (0.155)	-1.97*** (0.550)
Observations	12,489	12,439	13,541	13,491	12,902	12,852
<b>Panel B: Has at least one year of education</b>						
Common law	0.08*** (0.010)	0.08** (0.030)	0.34*** (0.008)	0.35*** (0.027)	0.34*** (0.009)	0.34*** (0.032)
Common law x Culture	0.27*** (0.012)	0.27*** (0.060)	-0.27*** (0.013)	-0.28*** (0.055)	-0.27*** (0.013)	-0.27*** (0.064)
Observations	12,489	12,439	13,541	13,491	12,902	12,852
<b>Panel C: Has at least primary education</b>						
Common law	0.05*** (0.010)	0.04 (0.035)	0.15*** (0.004)	0.15*** (0.028)	0.15*** (0.004)	0.15*** (0.031)
Common law x Culture	0.10*** (0.012)	0.11* (0.062)	-0.09*** (0.014)	-0.11* (0.056)	-0.09*** (0.014)	-0.11* (0.064)
Observations	12,500	12,450	13,553	13,503	12,914	12,864
<b>Panel D: Has at least secondary education</b>						
Common law	0.04*** (0.011)	0.02** (0.009)	0.03*** (0.005)	0.04*** (0.008)	0.03*** (0.005)	0.04*** (0.009)
Common law x Culture	-0.01 (0.008)	0.02 (0.017)	0.01 (0.007)	-0.02 (0.018)	0.01 (0.008)	-0.02 (0.020)
Observations	12,500	12,450	13,553	13,503	12,914	12,864
Ethnic homeland FE	✓	✓	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓	✓
Geographic controls		✓		✓		✓
Age controls	✓	✓	✓	✓	✓	✓

*Note:* In each specification we restrict the analysis to observations within 60Km of the border and we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron et al., 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, light density, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A3.2: Robustness check - Third order polynomial: legal origins and female education

	RDD - Bandwidth				
	OLS (1)	<250 km of bound. (2)	<150 km of bound. (3)	<100 km of bound. (4)	<60 km of bound. (5)
<b>Baseline specification</b>					
<i>Panel A: Years of education</i>					
Common law	1.74*** (0.493)	1.25** (0.521)	1.26** (0.579)	1.32** (0.619)	1.18** (0.482)
Observations	16,307	15,069	13,251	11,062	7,880
<i>Panel B: Has at least one year of education</i>					
Common law	0.22*** (0.071)	0.17** (0.073)	0.17** (0.078)	0.18** (0.083)	0.18*** (0.069)
Observations	16,307	15,069	13,251	11,062	7,880
<i>Panel C: Has at least primary education</i>					
Common law	0.12*** (0.019)	0.09*** (0.026)	0.09*** (0.030)	0.09*** (0.032)	0.07*** (0.019)
Observations	16,319	15,081	13,262	11,073	7,890
<i>Panel D: Has at least secondary education</i>					
Common law	0.05*** (0.004)	0.04*** (0.006)	0.04*** (0.008)	0.04*** (0.009)	0.02*** (0.006)
Observations	16,319	15,081	13,262	11,073	7,890
<b>Geographic controls</b>					
<i>Panel E: Years of education</i>					
Common law	1.53*** (0.486)	1.26*** (0.463)	1.26** (0.530)	1.27** (0.586)	1.17*** (0.441)
Observations	16,232	15,019	13,221	11,032	7,850
<i>Panel F: Has at least one year of education</i>					
Common law	0.20*** (0.067)	0.18*** (0.067)	0.18** (0.073)	0.18** (0.078)	0.18*** (0.069)
Observations	16,232	15,019	13,221	11,032	7,850
<i>Panel G: Has at least primary education</i>					
Common law	0.10*** (0.028)	0.09*** (0.022)	0.08*** (0.027)	0.08*** (0.031)	0.07*** (0.017)
Observations	16,244	15,031	13,232	11,043	7,860
<i>Panel H: Has at least secondary education</i>					
Common law	0.04*** (0.005)	0.03*** (0.007)	0.03*** (0.009)	0.03** (0.012)	0.02*** (0.004)
Observations	16,244	15,031	13,232	11,043	7,860
Ethnic homeland FE	✓	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓
Age controls	✓	✓	✓	✓	✓

*Note:* Specifications in columns (2)-(5) control for a third-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origins. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron et al., 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A3.3: Robustness check - Polynomial in latitude-longitude: legal origins and female education

	RDD - Bandwidth				
	OLS (1)	<250 km of bound. (2)	<150 km of bound. (3)	<100 km of bound. (4)	<60 km of bound. (5)
<b>Baseline specification</b>					
<i>Panel A: Years of education</i>					
Common law	1.74*** (0.493)	1.25** (0.598)	1.10 (0.718)	0.83 (0.902)	1.30* (0.716)
Observations	16,307	15,069	13,251	11,062	7,880
<i>Panel B: Has at least one year of education</i>					
Common law	0.22*** (0.071)	0.19** (0.083)	0.18* (0.092)	0.16 (0.107)	0.22** (0.101)
Observations	16,307	15,069	13,251	11,062	7,880
<i>Panel C: Has at least primary education</i>					
Common law	0.12*** (0.019)	0.09*** (0.034)	0.07* (0.045)	0.06 (0.060)	0.08** (0.037)
Observations	16,319	15,081	13,262	11,073	7,890
<i>Panel D: Has at least secondary education</i>					
Common law	0.05*** (0.004)	0.04** (0.014)	0.03 (0.024)	0.00 (0.036)	0.01 (0.024)
Observations	16,319	15,081	13,262	11,073	7,890
<b>Geographic controls</b>					
<i>Panel E: Years of education</i>					
Common law	1.06* (0.589)	0.94** (0.464)	1.02* (0.545)	1.14* (0.612)	1.46*** (0.540)
Observations	16,232	15,019	13,221	11,032	7,850
<i>Panel F: Has at least one year of education</i>					
Common law	0.17** (0.079)	0.17** (0.074)	0.18** (0.080)	0.20** (0.084)	0.25*** (0.084)
Observations	16,232	15,019	13,221	11,032	7,850
<i>Panel G: Has at least primary education</i>					
Common law	0.06* (0.032)	0.06** (0.024)	0.06** (0.026)	0.08*** (0.031)	0.09*** (0.015)
Observations	16,244	15,031	13,232	11,043	7,860
<i>Panel H: Has at least secondary education</i>					
Common law	0.01 (0.009)	0.02 (0.012)	0.02 (0.014)	0.01 (0.018)	0.01 (0.009)
Observations	16,244	15,031	13,232	11,043	7,860
Ethnic homeland FE	✓	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓
Age controls	✓	✓	✓	✓	✓

Note: Specifications in columns (2)-(5) control for a cubic polynomial in longitude and latitude. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron et al., 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, light density, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A3.4: Heterogeneity by birth cohort

	<250 km of bound.		<60 km of bound.	
	(1)	(2)	(3)	(4)
	[1948 1976]	[1977 1994]	[1948 1976]	[1977 1994]
<b>Panel A: Years of education</b>				
Common law	0.75** (0.321)	2.20*** (0.429)	0.84*** (0.311)	2.03*** (0.470)
Observations	7,688	7,331	4,337	3,513
<b>Panel B: Has at least one year of education</b>				
Common law	0.110** (0.051)	0.286*** (0.079)	0.111** (0.045)	0.291*** (0.087)
Observations	7,688	7,331	4,337	3,513
<b>Panel C: Has at least primary education</b>				
Common law	0.055*** (0.018)	0.150*** (0.011)	0.062** (0.028)	0.121*** (0.012)
Observations	7,697	7,334	4,344	3,516
<b>Panel D: Has at least secondary education</b>				
Common law	0.007 (0.009)	0.057*** (0.004)	0.005 (0.004)	0.043*** (0.010)
Observations	7,697	7,334	4,344	3,516
Ethnic homeland FE	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓
Age controls	✓	✓	✓	✓

*Note:* In each specification we restrict the analysis to observations within 60Km of the border and we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron *et al.*, 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A3.5: Education and culture

	Years of education					
	(1)	(2)	(3)	(4)	(5)	(6)
Female Partiticipation in Agri.	2.99*** (0.065)	2.96*** (0.064)				
Bride Price			-0.74*** (0.064)	-0.72*** (0.062)		
Patrilocality					-1.14*** (0.069)	-1.11*** (0.067)
Observations	13,008	13,008	15,112	15,112	13,733	13,733
Age controls		✓		✓		✓

*Note:* In this table we report results from a simple OLS specification where we regress the respondent's number of years of education on a variable that indicates the ancestral cultural norm of his ethnic group. Robust standard errors are in parenthesis. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

### 3.10 Appendix

Table A3.6: Men - Ancestral norms, legal origins, and male education (specification with interaction term)

	Female participation in agri. (1)	Bride Price (2)	Patrilocality (3)
<b><i>Panel A: Years of education</i></b>			
Common law	3.24*** (0.136)	-0.73 (0.858)	-0.76 (0.852)
Common law x Culture	-1.22*** (0.059)	3.61*** (0.503)	3.62*** (0.506)
Observations	2,653	2,823	2,752
<b><i>Panel B: Has at least one year of education</i></b>			
Common law	0.25*** (0.014)	0.00 (0.119)	0.00 (0.122)
Common law x Culture	-0.21*** (0.001)	0.19*** (0.045)	0.19*** (0.046)
Observations	2,653	2,823	2,752
<b><i>Panel C: Has at least primary education</i></b>			
Common law	0.31*** (0.014)	-0.00 (0.081)	-0.00 (0.081)
Common law x Culture	-0.11*** (0.009)	0.27*** (0.050)	0.27*** (0.050)
Observations	2,655	2,827	2,756
<b><i>Panel D: Has at least secondary education</i></b>			
Common law	0.15*** (0.012)	-0.02 (0.066)	-0.02 (0.066)
Common law x Culture	-0.09*** (0.007)	0.14*** (0.036)	0.14*** (0.037)
Observations	2,655	2,827	2,756
Ethnic homeland FE	✓	✓	✓
Cohort of birth FE	✓	✓	✓
Religion FE	✓	✓	✓
Age controls	✓	✓	✓

*Note:* In each specification we restrict the analysis to observations within 60Km of the border and we control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron et al., 2011), are in parenthesis. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table A3.7: Countries, DHS surveys, and legal origins

Countries	DHS year of surveys	Legal Origins
Benin	1996, 2001, 2011-12, 2017-18	Civil law
Burkina Faso	1993, 1998-99, 2003, 2010	Civil law
Cameroon	1991, 2004, 2011	Civil law
Cote d'Ivoire	1994, 1998-99, 2011-12	Civil law
Ethiopia	2000, 2005, 2011, 2016	Civil law
Ghana	1993, 1998, 2003, 2008, 2014	Common law
Guinea	1999, 2005, 2012	Civil law
Kenya	2003, 2008-09, 2014	Common law
Liberia	1986, 2013	Common law
Malawi	2000, 2004, 2010, 2015	Common law
Mozambique	2011	Civil law
Namibia	2000, 2006-07, 2013	Common law
Niger	1992, 1998	Civil law
Nigeria	2008, 2013	Common law
Sierra Leone	2008, 2013	Common law
Togo	1988, 1998, 2013-14	Civil law
Uganda	2011, 2016	Common law
Zaire	2007, 2013-14	Civil law
Zambia	2007, 2013-14	Common law

Table A3.8: Bootstrap - Average effects: Legal origins and female education

	RDD - Bandwidth				
	OLS (1)	<250 km of bound. (2)	<150 km of bound. (3)	<100 km of bound. (4)	<60 km of bound. (5)
<b>Baseline specification</b>					
<b>Panel A: Years of education</b>					
Common law	1.74*** (0.493)	1.26** (0.523)	1.27** (0.550)	1.31** (0.644)	1.18** (0.486)
Observations	16,307	15,069	13,251	11,062	7,880
Bootstrap: WCR p-value by country	0.014	0.038	0.003	0.002	0.018
Bootstrap: WCR p-value by ethno-linguistic families	0.000	0.023	0.000	0.001	0.001
Bootstrap: WCR p-value by country-ethno-linguistic families	0.008	0.016	0.001	0.001	0.006
<b>Panel B: Completed one year of schooling</b>					
Common law	0.22*** (0.071)	0.17** (0.073)	0.17** (0.076)	0.18** (0.085)	0.18*** (0.069)
Observations	16,307	15,069	13,251	11,062	7,880
Bootstrap: WCR p-value by country	0.020	0.081	0.036	0.062	0.036
Bootstrap: WCR p-value by ethno-linguistic families	0.000	0.028	0.016	0.043	0.002
Bootstrap: WCR p-value by country-ethno-linguistic families	0.010	0.047	0.015	0.028	0.016
<b>Panel C: Completed primary schooling</b>					
Common law	0.12*** (0.019)	0.09*** (0.025)	0.09*** (0.027)	0.09*** (0.034)	0.07*** (0.020)
Observations	16,319	15,081	13,262	11,073	7,890
Bootstrap: WCR p-value by country	0.011	0.024	0.005	0.005	0.019
Bootstrap: WCR p-value by ethno-linguistic families	0.000	0.000	0.000	0.002	0.003
Bootstrap: WCR p-value by country-ethno-linguistic families	0.006	0.009	0.003	0.005	0.008
<b>Panel D: Completed secondary schooling</b>					
Common law	0.05*** (0.004)	0.04*** (0.007)	0.04*** (0.007)	0.04*** (0.011)	0.02*** (0.006)
Observations	16,319	15,081	13,262	11,073	7,890
Bootstrap: WCR p-value by country	0.056	0.034	0.048	0.151	0.091
Bootstrap: WCR p-value by ethno-linguistic families	0.202	0.130	0.005	0.002	0.082
Bootstrap: WCR p-value by country-ethno-linguistic families	0.051	0.031	0.049	0.201	0.075
Ethnic homeland FE	✓	✓	✓	✓	✓
Cohort of birth FE	✓	✓	✓	✓	✓
Religion FE	✓	✓	✓	✓	✓
Age controls	✓	✓	✓	✓	✓

*Note:* Specifications in columns (2)-(5) control for a second-order polynomial in the distance from the centroid of each pixel to the nearest national border with different legal origin. Robust standard errors, adjusted for double-clustering at the country and the ethno-linguistic family level using the approach of (Cameron et al., 2011), are in parenthesis. Geographic controls include area under water, elevation, soil suitability for agriculture, light density, natural resources, distance to the sea coast, distance to the national border, and area of the country-ethnic homeland. Age controls include indicators for respondent's age and age squared. Coefficients that are significantly different from zero are denoted by the following system: \*\*\*p<0.01, \*\*p<0.05, \*p<0.1. Following Roodman et al. (2019) and Nielsen et al. (2020), the first, second, and third Bootstrap use Wild Cluster bootstrap symmetric P-values with clustering at the country level, the ethno-linguistic family level, and the country - ethno-linguistic family level, respectively. We set the number of replications to 9999.

## Appendix Data Sources

**Light density at night:** Light Density is calculated averaging light density observations across pixels that fall within the unit of analysis. We use the 2013 Nighttime Light (NTL) data (stable lights dataset) from the U.S. Air Force's Defense Meteorological Satellite Program/Operational Linescan System (DMSP/OLS). This dataset is made available by the U.S. National Oceanographic and Atmospheric Administration (NOAA). The pixel light (gain) values range from 0 to 63 with 0 being the absence of light. *Available at* <https://www.ngdc.noaa.gov/eog/dmsp/downloadV4composites>

**Population density:** Population per sq. km. in 2000. Source: Nelson, Andy, 2004. African Population Database Documentation, UNEP GRID Sioux Falls. *Available at* <https://www.arcgis.com/home/item.html?id=9ec46c83ca5c47ebb1a25bd43131b483>

**Elevation:** Average elevation above sea level of each pixel. Source: National Oceanic and Atmospheric Administration (NOAA) and U.S. National Geophysical Data Center, TerrainBase, release 1.0 (CD-ROM), Boulder, Colorado. *Available at* <http://nelson.wisc.edu/sage/data-and-models/atlas/data.php?incdataset=Topography>

**Soil suitability for agriculture:** Average land quality for cultivation within each pixel. This index is based on the temperature and soil conditions of each grid cell. Source: Ramankutty, N., J.A. Foley, J. Norman, and K. McSweeney. The global distribution of cultivable lands: current patterns and sensitivity to possible climate change. *Available at* <http://nelson.wisc.edu/sage/data-and-models/atlas/data.php?incdataset=Suitability%20for%20Agriculture>

**Water area:** Total area covered by rivers or lakes in sq. km within each pixel. Constructed using the Level 3 of the Global Lakes and Wetlands Database (GLWD) which comprises lakes, reservoirs, rivers, and different wetland types in the form of a global raster map at 30-sec resolution. Source: Lehner, B. and Doell, P. (2004): Development and validation of a global database of lakes, reservoirs and wetlands. *Journal of Hydrology* 296/1-4: 1-22. *Available at* <http://www.arcgis.com/home/item.html?id=1ac6777abcc24ab4a9fe39f27c4cb01f>

**Distance to the sea coast:** The geodesic distance (in kilometers) from the centroid of each pixel to the nearest coastline. Constructed using Africa coastline data. *Available at* <http://omap.africanmarineatlas.org/BASE/pages/coastline.htm>

**Distance to the capital city:** The geodesic distance (in kilometers) from the centroid of each pixel to the capital city in the same country. Geographical coordinates for the capital cities were derived from the cShapes dataset. Source: Weidmann, Nils B., Doreen Kuse, and Kristian Skrede Gleditsch. 2010. The Geography of the International System: The CShapes Dataset. *International Interactions* 36 (1). *Available at* <http://nils.weidmann.ws/projects/cshapes.html>

**Distance to the national border:** The geodesic distance to the nearest national border from the centroid of each pixel. Constructed using the border from the digital chart of the world projection *Available at* <https://worldmap.harvard.edu/data>

**Petroleum:** Indicator variable that equals one if there is an oil field in the pixel. We use the petroleum Dataset v.1.2 which contains information on all known on-shore oil and gas deposits throughout the world. Source: Lujala, Päivi; Jan Ketil Rød, and Nadia Thieme, 2007. "Fighting over Oil: Introducing A New Dataset", *Conflict Management and Peace Science* 24(3), 239-256. *Available at* <https://www.prio.org/Data/Geographical-and-Resource-Datasets/Petroleum-Dataset/Petroleum-Dataset-v-12/>

**Diamonds:** Indicator variable that equals one if there is a diamond mine in the pixel. We use the Diamonds dataset offers a comprehensive list of all known diamond deposits throughout the world. Source: Gilmore, Elisabeth; Nils Petter Gleditsch, Paivi Lujala Jan Ketil Rod, 2005. "Conflict Diamonds: A New Dataset", *Conflict Management and Peace Science* 22(3): 257-292. *Available at* <https://www.prio.org/Data/Geographical-and-Resource-Datasets/Diamond-Resources/>

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