Title
The Politics and the Measurement of Health Inequality in the Developing World

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Publication Date
2014

Peer reviewed|Thesis/dissertation

University of California
Los Angeles

The Politics and the Measurement of Health Inequality in the Developing World

A dissertation submitted in partial satisfaction
of the requirements for the degree
Doctor of Philosophy in Political Science

by

Antonio Pedro Ramos

2014

Abstract of the Dissertation

The Politics and the Measurement of Health Inequality in the Developing World

by

Antonio Pedro Ramos

Doctor of Philosophy in Political Science

University of California, Los Angeles, 2014

Professor Jeffrey B. Lewis, Co-chair

Professor Barbara Geddes, Co-chair

Though no longer a major problem for most rich nations, child mortality is still prevalent in the developing world, especially among poor people in the poorest countries. However, data suggest that most of these premature deaths could be easily prevented with relatively cheap medical technology. My dissertation uses new, high resolution data sets and innovative statistical approaches to investigate the politics and the measurement of inequality in child mortality.

Unsing new estimates of national averages of child mortality, I show strong evidence that democratic transitions have heterogeneous effects across countries. In some places, such as sub-Saharan Africa, democratization does have a large impact on national averages of child mortality; however, in most middle income countries, democratization did not change pre-transition time trends. Looking at more than 5 million births from 50 developing countries since 1970, I also show that on average democracy did not reduce the gap in child mortality between rich

and poor, though it did so in a few places, mostly in Sub-Saharan Africa. In both cases, these patterns have not been previously investigated. In the light of the fact that medical technology is already available to prevent these deaths, these findings raise questions about the standard view that democratic governments are more responsive to citizens.

Finally, my dissertation also develops a new methodological approach to investigate total inequality in child mortality over time. Previous work on inequality in child mortality focus on between-group comparisons — e.g. rates for rich versus for the poor. However, disparities within-groups are often larger than between-groups. Using a large data set from India, I show how existing data sources and statistical methods can be used to investigate the distribution of the risk of ill-health across all individuals in a given society. In doing so, I show that inequality in child mortality in India has been increasing over time.

The dissertation of Antonio Pedro Ramos is approved.

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University of California, Los Angeles

2014

To Cristina Brandao,

For her love and wisdom.

Table of Contents

List of Figures

List of Tables

Acknowledgments

Finishing a PhD is not only the result of a long research and learning process but, more fundamentally, it is the end of a lifetime period. While this is true for all PhD students, I think this is particularly true for those who switch countries, lifestyle and languages in order to complete a PhD program.

For someone who has never visited USA for more than a few weeks — and in a context of a family vacation —, applying to American universities was quite an experience on its own. I would like to thank those who supported my PhD application efforts, either teaching me about the whole process or writing me letters of rec. While many people helped in this process, I especially thank Professors Ronaldo Bastos and Octavio Amorim Neto for their support and encouragement. I also thank Macartan Humphreys for answering my questions and helping me to select the best place for my PhD studies.

While at UCLA, many people have contributed to my personal and professional experience. Special thanks to Bon Sang Koo, with whom I have spent hours and hours coding and solving stats problems. I have enjoyed interacting with too many people, including Eric Kramon, Yuki Yanai, Seth Hill, Ryan Enos, Javier Rodriguez, Paasha Mondavi, Fabricio Fialho, Felipe Nunes, Michael Lacour, Dov Levin. Krishna Bhogaonker provided instrumental help in working with the large data sets that I have used in my dissertation. Joseph Brown provided invaluable support. I also thank Jim DeNardo for introducing me to the world of social methodology and Tom Schwartz for teaching me what I know about formal theory.

My dissertation draws upon several different fields of knowledge and without my committee it would be impossible for me to put all pieces together. My committee is unusual not only because it does have more members than a typical one but, more importantly, because *all* of them played an important and active role in mentoring me.

I thank Rob Weiss for his classes on Bayesian Statistics and Longitudinal Models, and for his generous office time. He introduced me to the world of Bayesian data analysis, longitudinal and hierarchical models, statistical graphs, and much more. He helped me to present my statistical results in a much clearer and concise way. Virtually all the data analysis on my dissertation benefited from his advice.

I would like to thank Mark Handcock for his Social Statistics classes, our study group on Bayesian statistics, and his overall advice. Mark was instrumental in helping me to make the connection between statistical models and the underlying substantive questions. He also helped me to think through the several steps of the scientific process from conceptualizing a substantive problem in statistical terms to actually solving it and presenting the results.

Michael Ross introduced me to the political economy literature and taught me how to communicate and frame my research questions in terms of important debates for a broader audience. Also his skepticism of "fancy" methods only helped me to be better at explaining their importance for the questions we care about.

Patrick Heuveline taught me everything I know about demography. He helped

me in making sense of large amounts of demographic data.

Miriam Golden introduced me to the world of distributive politics, which is the core theoretical framework of most of my research.

This dissertation was only possible because of the support of my chairs, Jeffrey Lewis and Barbara Geddes. They are among the most generous people I have ever meet. They are everything I was expecting from an advisor and much more — and I even had two of them! They have supported every aspect of my professional life and more. Both were incredibly accessible, creative, and made my work in graduate school much more enjoyable. They provided the needed guidance for me to work in a field that I was not very familiar with. Both have made me the scholar that I am today. Working with them was also incredibly fun. Jeff fostered my interest in political methodology while always reminding me that the methods should serve a substantive question. Barbara supported my adventures into other scientific fields while always helping to make a connection with political science research. Thank you Jeff and Barbara. I can only hope that one day, I may have the opportunity to contribute to my students as you have to my life and career.

I dedicate all of my efforts that culminated in this dissertation to my wife, Cristina Brandao. She was the one that provided me with unconditional love and support since the times when PhD studies in USA were no more than a vague and juvenile dream. She stood by my side all these years with love, moderation and wisdom. I hope we will be together all years to come.

VITA

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CHAPTER 1

Introduction

Does politics affect human well-being? Do people live better lives under democracies than under dictatorships? Does democracy re-distribute from the poor to the rich? These questions are central to political science not only because of their theoretical importance, but also because they matter for the lives of everyday people throughout the world. My dissertation examines equality, re-distribution and governmental responsiveness to the poor, particularly in the developing world. I investigate the conditions under which governments redistribute to the poor. I use child mortality as a measure of governmental delivery of welfare targeted to the poor as (1) low income families suffer disproportionally from premature deaths, and (2) most of these deaths could be cheaply prevented by governmental interventions.

I have studied these questions at an unprecedented level of detail. For example, I use new panel data records of more than 5.5 million births from 50 developing countries since 1970. These data have allowed me to track changes over time in the levels and the rate of change of child mortality across births from mothers from income levels within countries, while controlling for change in the demographic composition of the population. Doing so, I am able to directly measure the changes in the rich-poor gap in child mortality. Theoretically, I challenge the view that by moving the median voter downward in income, democracies would increase well-being to the poor. I discuss that, unless special conditions are met, this is not likely to happen.

In the first paper of my three-paper dissertation, I revisit the debate on regime type and national averages of child mortality. I discuss how poor data quality and research design issues are behind much of the scholarly disagreement on this topic. In the second paper, I expand the cross-national debate on regime type and well-being provisions beyond national averages of child mortality. I do so by looking at inequalities in child mortality across income groups within each of 50 countries. I discuss why the child mortality gap between rich and poor families *within* countries is a much more powerful way to test theoretical predictions. In my third paper, I go beyond the study of inequality in child mortality between income groups. I focus on within group inequities, which are often larger than the inequality between groups. I develop new statistical approaches to measure children's propensity to die. Based on these findings, I discuss how to create measures such as Gini coefficients for infant mortality so that we can track over time and countries' disparities in the risk of death across children.

I show strong evidence that democratic transitions have heterogeneous effects across countries. In some places like sub-Saharan Africa, transitions have a large impact on child mortality. In most middle income countries, however, democratic transitions did not change previous, pre-transition time trends. I also find that overall democracy did not reduce inequality in the child mortality gap among the rich and poor, though it did so in a few places, such as some countries in Sub-Saharan Africa. Finally, I show how to measure inequality in the risk of death across children from the same group over time. I also discuss how these measurements can be used to increase our understanding in perhaps the most fundamental level of human inequality.

By showing the heterogeneous effects of democratization, my dissertation challenges conventional views that assume that by moving the median voter downward in the income distribution, democratization will produce more mortality-averting public goods. In fact, only under special conditions will that be the case: for example, when the median voter suffers from high levels of child mortality. Finally, by showing how to analyze inequalities in child mortality within groups over time, I open the possibility of developing much more detailed and precise measurements of the distribution of well-being across the developing world. Such measurements can be used for both policy evaluation and testing broader social science theories.

To investigate these questions, my dissertation introduces new data sets and methods. To study national averages of child mortality, I utilize a recently made available data set with much less measurement error than in previously available data and without missing observations. These data cover 187 countries since 1970 and was constructed based on more than 16 thousand measurements of child mortality which were combined using sophisticated and data driven statistical techniques. I also use another unique data set with the individual records of more than 5.5 million births from 50 middle and low income countries, with detailed information about the children, their mothers, and household characteristics. These 50 countries account for roughly 80 % of the under-five death toll in the world in recent decades.

I show that better answers to questions about the effect of regime type on health care provision depend on the careful analysis of time trends and whether these trends were affected by transitions. I take advantage of the third and fourth waves of democratization since late 1970s to investigate whether the introduction of democracy changed pre-transition trends. I use a variety of techniques that are relatively unknown and unused in political science, such as random coefficients models for longitudinal data, non-parametric methods, meta-analysis, bent lines to track discontinuities over time, and new visualization techniques of raw data.

Besides its implications for theories of democratic politics, my study explains a real outcome that affects the everyday welfare of citizens. Child mortality unquestionably matters for the lives of many, particularly for the lives and well-being of the very poor, in the least developed regions of the world.

Paper 1: Do democratic transitions reduce child mortality? An analysis of 185 countries, 1970-2008.

The first paper of my dissertation investigates whether the introduction of democracy makes governments more responsive to the needs of the poor. It revisits the debate on the effects of democracy on national averages of child mortality. Since most child deaths are concentrated among the poor and are preventable with current medical technology, scholars have assumed that child mortality would be lower if governments were more responsive. Much of the disagreement among scholars about this issue has arisen from poor data quality and research designs that pooled observations over many years instead of looking at time trends. My analysis corrects these issues.

I focus on democratization episodes and whether they were followed by systematic reductions in the levels and rates of change in child mortality. I use random effects coefficient models to investigate time-trends in child mortality across the globe. A simple tool called bent lines is used to detect changes in the levels and rates of change after democratic transitions. Graphical displays of data expectations further confirm the models' findings.

I show that democratization does change previous, pre-democratization trends and levels of child mortality in statistically and substantively significant ways. Though the changes are often very small in the short run, they can make a substantial difference over a few decades. Further, I show that the effects of democratization vary across countries. While in low income countries, especially those in Sub-Saharan Africa, transitions reduce national averages of child mortality below the trend line, the same cannot be said about middle income countries. I suggest that the reason for this difference is due to the position of the median voter. When the median voter is poor enough to care about child mortality, such as in Sub-Saharan Africa, democratization may produce health improvements. Yet, when the median voter is already rich enough so that premature death is no longer an issue for their children, democratization might not produce further reductions.

Paper 2: Have democratic transitions reduced the gap in child mortality between the rich and the poor? An analysis of 5.5 million births from 52 middle and low income countries since 1970.

Theoretical results suggest that democratization should redistribute from the rich to the poor and therefore reduce the income gap in child mortality. To date, this hypothesis has only been investigated indirectly, by looking at the effect of democracy on average national child mortality rates. Such studies assume that reductions are due to improvements among the poor. Yet, national averages of child mortality (1) are not necessarily a good measure of what is happening among the poor, especially in high mortality places, and (2) do not inform us about the gap between rich and poor.

This study is the first investigation on whether democracy affects the child mortality gap between rich and poor. I track over-time changes in the child mortality gap within each of 50 countries and then use a meta-analysis to combine the results. I examine whether baseline levels of inequality and overall time trends for all 50 countries are related to political factors. For a subset of 22 countries that experienced democratization during the time period, I investigate whether democratic transitions reduce either pre-democratic mortality gaps between the rich and the poor or rates of change in the child mortality gap.

My analysis shows that, on average, regime type has little effect on inequality in child mortality overtime. Moreover, the introduction of democracy does not seem to affect the pre-democratization trends in the overtime reduction in the mortality gap. I find very strong differences in the effects of democratization across countries. Even though the average impact of democratization was barely significant either substantively or statistically in the full sample of countries, it has an important effect in some parts of the world, notably in sub-Saharan countries.

Paper 3: Measuring Total Inequality in Child Mortality Over Time A Bayesian Analysis of India

Inequality in child survival is perhaps the most important dimension of human inequality. While widely studied, inequality across groups of people (countries, income levels, educational attainment, etc) only accounts for less than 50 % of the variance in premature deaths. Thus differences in survival rates across individuals from the same group is a crucial dimension of inequality. This is a methodologically challenging topic. While we only observe children either dead or alive, in order to investigate within-group inequalities, we need to actually calculate the unobserved probability of death for each child. Within-group inequalities can be seen as a measure of risk or insecurity in survival from a given group.

I show how existing data sources and statistical models can be used to measure total inequality in child mortality in the developing world. To do so, I use data from the Demographic and Health Surveys from India and random effects logistic regression models. The data set is used to construct a retrospective panel on children survival over time. The random effects models take advantage of several levels of clusterings available in the data set (mothers, DHS sampling clusters, districts and states and years) to account for unmeasured factors and correlations at these levels. Taking advantage of the flexibility of the Bayesian framework for model estimation, I used children predicted death probabilities to calculate posterior predictive distribution of several traditional inequality indexes, such as Gini. By doing so, the uncertainty associated with children's estimated probability of death (first step of the analysis) is incorporated into the Gini coefficients or regression models (second step). I show that the total inequality in child mortality in India is increasing over time. This pattern has not been previously documented.

The potential application of these techniques is quite broad. It allows us to construct measures of inequality in child survival over time for several different countries. It also helps to develop models that explain these over time changes through the effects of covariates. Doing so, it makes it possible for us to measure and understand a fundamental but understudied feature of inequality in human welfare.

Conclusion

This dissertation revisits classical debates on redistribution, equality and governmental responsiveness. It focuses on child mortality as a measure of governmental delivery of welfare-enhancing goods to the poor, who suffer disproportionately from premature death. It challenges theoretical views that assume that democratization, by moving the median voter downward in the income distribution, will result in re-distribution and welfare improvements to the poor. It introduces new, high resolution data sets, allowing us to investigate these questions at an unprecedented level of detail. Finally, it also extends the debate on health inequality by extending existing cross-sectional approaches to investigate inequality within groups overtime, using existing data sources.

Theoretically my results challenge the common interpretation of the median voter theorem. I discuss that, unless special conditions are met, democratization will not produce substantial gains in health care for the poor. As a consequence, it opens new lines of research: why does democratization work in some places but not in others? Is it because of the relative demands of the median voter? Or because of other political factors that remain to be accounted for, such as governmental ideology? My future research will address these questions.

Finally, I study total inequality in survival across children from India. I develop a new approach that allow us to use exiting data sets and statistical techniques to track over-time changes in inequality in child mortality. By doing so, I show that total inequality in child mortality in India, as measured by indexed such as Gini, is increasing over time. This pattern has not been previously documented. These techniques can be largely applied to other countries, allowing for the description and explanation of one of the most important dimensions of human inequality.

CHAPTER 2

Does Democracy Reduce Infant Mortality? Evidence from new data, for 181 countries between 1970 and 2009

2.1 Introduction

Which form of government is most responsive to its citizens' needs? For the world's poor in developing countries, this issue is particularly important. Government provides critical services that impact health, welfare, and life expectancy for these citizens. Scholars and policy practitioners alike have argued that democracies are more responsive to the needs of the poor than non-democracies. The median voter theorem (Meltzer and Richards, 1981) and its extensions such as Acemoglu and Robinson (2000) point out that democratization moves the median voter down the income spectrum. Consequently, governments become more responsive to the needs of the poor. Supporting this logic, studies have found that democracies are beneficial to many aspects of human well-being. For example, democracy increases calorie intake (Blaydes and Kayser, 2011); prevents famines (Sen 1981, 1999); improves access to electricity (Min, 2008; Brown and Mobarak, 2009); increases spending on primary education (Stasavage, 2005); reduces child mortality (Przeworski et al., 2000; Ross, 2006); and, in general, funds public services better (Avelino et al., 2000) than non-democracy does. Many studies implicitly model democracy as having a homogenous effect on citizen well-being across time and countries; none explicitly allow for the possibility that democracy can have heterogenous effects.

Among the most salient issues that the poor face in the developing world is that of infant health. Child mortality is often concentrated among the poor and, for the most part, it can be easily preventable with current medical technology (Black et al., 2003; Jones et al., 2003; Bryce et al., 2003; Victora et al., 2003). It is correlated with other measures of health and well-being (e.g. sanitation, literacy) that are not easily measurable and comparable across countries (Ross, 2006). While some health policies might provoke genuine debate — for example, HIV prevention or family planning — the goal of reducing child mortality is relatively uncontroversial. Following the logic of the median voter theorem, (1) since child mortality is concentrated in the low quantiles of income, (2) it can be easily prevented with current medical technology and (3) democracies redistribute from the rich to the poor, we should expected democracies to reduce child mortality (Lake and Baum, 2001).

Yet, previous literature on regime type and democracy are inconclusive. Przeworski et al. (2000) report that democracy does provide better health outcomes, including lower infant mortality. Lake and Baum (2001) found that a move from complete autocracy to complete democracy substantially reduces infant mortality. Focusing on transitions in sub-Saharan Africa, Kudamatsu (2012) found that democracy reduces infant mortality. Yet, these results have been challenged. Ross (2006) demonstrates that once high income dictatorships are included and missing data is accounted for, there is no evidence that democracy is beneficial to poor infants. Gerring et al. (2012) did not identify any contemporaneous effects of democracy on health, though he argues that the accumulated stock of democracy is important for current levels of child mortality. These negative findings can be supported by a demographic evidence: under dictatorship China, from the early 1960s to the mid-1970s, reduced child mortality by a factor of three. This is one of the most significant improvements in history (Caldwell, 1986).

At the center of this controversy lie several data and methodological challenges. For instance, due to missing observations, Przeworski et al. (2000) employed just 1,417 observations out of 4,126 possible country-years, thereby drawing conclusions from only 34 % of the data. Because observations are not missing at random—high income dictatorships are more likely to be excluded—statistical estimates may be biased. Even in completely observed data sets, measurement error can still be substantial. To illustrate the magnitude of measurement error in reported data, consider the following: the list of the ten countries with the most rapid declines in child mortality between 1990 and 2007 from UNICEF in 2008, UNICEF in 2009, and the UN Population Division (UNPD) in 2009, have only three countries in common - Portugal, Vietnam, and the Maldives. In 2008, UNICEF reported that Thailand had the fastest rate of decline in the world, leading researchers to undertake a case study of this success. Yet, in 2009, UNICEF reported that Thailand had only the 47th fastest rate of decline while the UNPD reported it as the 4th fastest rate of decline for the 1990 and 2007 period (Rajaratnam et al., 2010).

Previous studies suffer from a variety of methodological problems. These problems include counterfactual scenarios that lie outside the range of the data, models that are sensitive to the inclusion of countries with very little information on key quantities, and models that assume unrealistic time trends. Lake and Baum (2001) found that moving from one extreme to other on the Polity IV score greatly reduces predicted child mortality. No country ever transformed from one polity score extreme to the other; this is equivalent to study of how child mortality would change if Saudi Arabia became a Scandinavian democracy. Although counterfactual scenarios can be illuminating tools, this is one lies outside the range of the current data (King, 2006). In the same vein, Ross (2006) demonstrates that by including high income dictatorships in the analysis, results change drastically. Yet these countries can tell us very little about the effects of democracy on health, simply because we do not have well-defined counterfactual scenarios for them. Finally, many models implicitly assume that child mortality in countries as disparate as Nigeria and Denmark change over time at the same rate and that the effect of democracy on child mortality is the same across all type of democratic transitions.

This paper uses new data and methods to investigate the linkages between democracy and child mortality for a sample of 181 countries since 1970. I use a new data set *without any* missing information (Rajaratnam et al., 2010) and with much less measurement error than previously available. Although we are far from experimental conditions, the research design presented here allows me to draw causal inferences about the effects of democracy on health. This research design takes advantage of the recent democratization waves to understand whether pre-democratic transition levels and rates of change of child mortality were affected by these political transformations. By focusing on time trends and possible deviations from it following democratic episodes, I argue, one can have much better leverage on the causal effects of democracy on health. This approach can help us to answer the question: what would the child mortality rates have been in the absence of democracy? By investigating whether democratization episodes further accelerate the trends in child mortality (that were already existent before the transition), this approach not only produces counterfactual scenarios within the range of the data, but is also insensitive to the inclusion and exclusion of countries that add little information on democracy's effects on health. To do so, I use *bent lines*—a simple technique, widely used in other fields but relatively unknown in political science—to compare rates of change in child mortality before and after the transition. To account for the large disparities across countries in the world, I stratify the sample into several clusters based on income levels and regime type in order to investigate the effects of democracy within each strata. Finally, I evaluate long-term time trends and post-democratization deviations from these trends through a variety of statistical models.

I demonstrate significant heterogeneity in democracy's effects across countries, over time, within income levels and with political history. I show that democratization, on average, accelerated reductions in child mortality by a small but statistically significant amount. Although the yearly reduction is small, these reductions accrue over time, yielding a substantial impact after a decade or so. The small average effect obscures substantial heterogeneity among countries. In all countries, democratization increased the rate of decline in infant mortality, but the acceleration was greater in Sub-Saharan Africa than elsewhere. Political history also affects the consequences of democratization. Democratization increased the rate of decline in relatively stable countries that experienced one democratic transition, but not in countries that experienced multiple transitions to and from democracy. The effect of democratization on child mortality is more complex: democratization did not change the number of infant deaths in poor countries, but it actually *increased* the level of deaths middle-income countries. In fact, for many but not all middle income countries, the introduction of democratic government was followed by a short-term increase in the levels of child mortality, particularly in the first two years. This finding is novel and I discuss the reasons for it in the last section of this paper. Overall, the findings presented in this paper highlight insufficiencies in the traditional median-voter model. Whereas the median-voter model predicts democratization would be followed by an immediate reduction of child mortality across all types of countries, I advance a more nuanced model explaining the effect of political regimes on public health.

In the next sections, I review the theoretical and empirical literature on the topic. I then present a new data set. Turning next to the methodology, I explain how focusing on time trends and post-democratization deviations lends insight into democracy's causal effect on child mortality. I also advocate a random effects model as a flexible estimation framework. Following several robustness checks, the last section discusses the theoretical significance of these empirical results.

2.2 Democracy, Redistribution and Health: The Theoretical Link

Democracy has been linked to child mortality in a number of ways. The theoretical approaches are varied but all tend to emphasize *elections* or electoral accountability and responsiveness. All of these approaches emphasize the accountability of democratic governments. This accountability and responsiveness is driven by elections: through contested elections politicians are punished as they fail to attend citizens' demands. Thus while a fully working democracy entails many dimensions, *the introduction of electoral competition should be enough to trigger reduction in child mortality.* Thus one should look for measures of democracy linked to electoral outcomes.

2.2.1 Distribution for the Poor via Contested Elections

Many studies link democracy to infant mortality through mechanisms of redistribution. Redistribution can take the form of public goods/services and income redistribution through taxes or transfers. According to this line of reasoning, democracies help the poor by producing more public goods and more income redistribution than non-democracies. Forced by the electoral process, democracies produce more public goods because politicians needs to spend their revenues on government services, while autocratic governments face no such constraint. Democracies also have more income redistribution than non-democracies because in unequal societies, the median voter has less than the mean income and therefore voters collectively pressure the government to redistribute wealth down the income distribution. Both public goods and income redistribution disproportionately help the poor, the sector of society where premature infant death is concentrated.

More specifically, democracy leads to income redistribution according to the median voter theorem (Meltzer and Richards, 1981). This theory states that as suffrage expands, the position of the median voter—whose preferences determine government policies—shifts down in the income distribution[1]. Under universal suffrage the median voter will earn the median income; when income is unequally distributed, however, the median voter's income is less than the mean income. When voters' income decreases, their demand for redistribution increases. Since the decisive voters now earns a below-average income, they favor a higher income tax rate (since it will fall most heavily on the wealthy) and more economic transfers. In short, democracy brings more people with below-average incomes to the polls, and they collectively force the government to redistribute income downwards. With new wealth gains, the poorest member of society can dedicate additional resources to their children's health, an uncontroversial priority. Thus improvements in child mortality are expected to follow the introduction of

[1]See Ross (2006) for an alternative interpretation as discussed below.

democratic elections [2].

2.2.2 Theoretical Challenges

Some scholars have challenged the theoretical link between democracy and redistribution to the poor. These theoretical critiques either provide a new interpretation of the median voter theory or stress that the median voter model itself may not be capturing important features of democratic politics in developing countries.

Challenging the most common interpretation of the median voter theorem, Ross (2006) argues that democracy will not necessarily re-distribute to the poor. Democratization moves the median voter down the income spectrum, from the rich to the middle class, but not necessarily to the poor. (Ross, 2006) claims it is much more reasonable to assume that the median voter is around the median income level, not the poorest in the society. Thus gains from democratization accrue mainly among the middle class—not among the poor. If this is the case, the median voters may not suffer from child mortality and may not care about it more than the rich. Accordingly, voters in the lowest income level who are most concerned about child mortality will not have their preferences represented. Only in some very poor countries might the median voter actually suffer from high levels of child mortality. The conclusion to draw from this challenge is

[2] There are still other approaches that also emphasize elections as the key channel for improving living conditions of the poor, thereby reducing child mortality. For example, in his study on famine and deprivation, Sen (1981,1999) describe electoral competition in democracies as a political device that precludes famine in many parts of the world. In this case, elections are suppose to work as an information channel, one in which rules are able to more quickly respond to mass starvation.

not that the median voter theorem does not find empirical support but, instead, that the common interpretations could be mistaken. The theory does not imply improvements for those at the bottom of income, as is often assumed. Instead, it most commonly implies improvements for those in the middle income strata.

Nelson (2007) also challenges the view that democracy and competitive elections alone are sufficient to redistribute wealth toward the poor. Reviewing a series of empirical and theoretical studies, he concludes that often democracy is not associated with better health outcomes and, in some cases, electoral pressures can actually impede public health. Under new democracies, it is common to have divergence between governmental efforts and societal demands, even in the context of competitive elections. Electoral rules, social cleavages, party ideology, and the natural difficulties for ordinary citizens to understand large scale complex institutional and policy reforms may all conspire against the provision of better health services. Moreover, other non-electoral factors such as specials interest group influence and decentralization might hinder improvements as well. Still others such as Iversen and Soskice (2006) call attention to other variables—such as race, ethnicity and religion—that might force citizens to vote along non-economic lines, further hindering policies that improve health outcomes.

2.3 Empirical Studies on Democracy and Well-Being

Many cross-national empirical studies focus on the provision of public goods, whose main beneficiary tends to be the poor. While these studies do not directly address health outcomes, they are relevant insofar as they address democracy's effect on other important dimensions of well-being. Stasavage (2005) finds the democratic transitions in Africa have increased public spending on the primary education which is particularly beneficial for those at the bottom of the income level. Min (2008) finds that democracy is associate with reduction in the share of the population that lacks access to electricity; Brown and Mobarak (2009) demonstrates that in poorer countries, democratization increases the residential share of electricity relatively to industry, which is beneficial to the poor. In addition, Blaydes and Kayser (2011) shows that full democracies and hybrid regimes are better than autocracies at translating economic growth into higher calorie intake among the population. These other markers of development provide a broader picture of democracy's effects.

More specifically, scholars have empirically examined links between political regime and health. Przeworski et al. (2000) reports that controlling for selection bias, democracy does provide the poor with a better standard. Navia and Zweifel (2003) shows that lower infant mortality rates are correlated with political rights. Lake and Baum (2001) noted that a move from complete autocracy to complete democracy substantially reduces infant mortality.Ross (2006) discusses how the exclusion of high income dictatorships from Przeworski et al. (2000) leads to biased inferences. Including the high-income dictatorships actually reverses the perceived effect of democracy on health. Kudamatsu (2012) uses a different data set—with individual-level data from the Demographic and Health Services (DHS), not national averages—and focuses on sub-Saharan African countries only. Using regime transitions as a part of their research design, this study found that democratization reduces child mortality in Africa. Gerring et al. (2012) found no short-term effect of democracy and therefore agrees with my findings. He argues instead that what is important for human development is the accumulated stock of democracy in a given country, measured as the number of democratic years.

While this literature represents a massive effort, it has reached contradictory findings. Common problems include missing data and measurement error on the outcome variable. Deeper problems include flawed research designs that fail to model time trends properly, are sensitive to inclusion of countries with very little information on the causal effects of regime type on child mortality, and counterfactuals scenarios unsupported by the data. Below I discuss a robust research design that overcomes these problems while relying on minimal modeling assumptions.

2.4 Data

2.4.1 New Data on Child Mortality

The Institute for Health Metrics and Evaluation (IHME) from University of Washington at Seattle has created new data sets on infant deaths (Rajaratnam et al., 2010). This advance has been made possible by four important developments. They have collected information on 16,174 measurements of mortality in children younger than 5 years for 187 countries from 1970 to 2009. They have collected data from all available sources, including vital registration systems, summary birth histories in censuses and surveys, and complete birth histories. Thus for each country-year they compile information from up to ten data sources. Sophisticated statistical techniques average and impute over this data set, so that in the final data analysis, each country-year is summarize by just one data point. The details of their data are presented in the supplemental materials. Importantly, most of these data come from independent sources, such as the DSH. Thus these data set is much less likely to suffer manipulation of governmental statistics than previous sources, which were a major concern in the past (Ross, 2006)[3].

2.4.2 Measures of Regime Type

Recent scholarship has generated many measures of democracy. I use one well-established metric developed by Przeworski et al. (2000) and extended by Cheibub and Gandhi (2010). This binary measure is highly comparable across countries and based on objectively observable characteristics. It focuses on elections, the hallmark of a functional democracy. Importantly, it has a clear meaning: a change from 0 to 1 signifies a specific set of rules were introduced. This is not the case with other measures of democracy such as the popular "Polity" metric. Changes in this ordinal metric do not translate into clear and specific

[3] I am also using a new data set on Maternal Education. Gakidou et al. (2010b) compiled publicly available censuses and nationally representative surveys of respondents' educational attainment. They used 915 sources of data from 219 countries, gathered between 1953 and 2008 (see their web appendix pp. 2535). Classical predictors of infant mortality that will be used in this analysis as covariates are presented in the appendix: Per Capita Income, prevalence and Maternal Education.

changes in the rules of the political game, especially the electoral game [4]. Due to its relative simplicity and emphasis on elections, the Przeworski et al. (2000) democracy variable facilitates clear comparisons when democratic electoral rules are introduced in different parts of the developing world.

2.4.3 What Do the New Data Show?

Figure 2.1 displays a series of box-plots showing the distribution of mortality rates over time for dictatorships and democracies. Overall mortality rates are higher for dictatorships than for democracies, regardless of the year. However, the discrepancy is decreasing over time. This study asks whether the substantial declines in child morality among democracies are *caused* by the political regime or are merely *correlative.*

When the data are disaggregated by regime type and income level, it becomes apparent that the association between democracy and health is not, in fact, causal. Figure 2.2 displays exactly the same data but now clusters the data by country. Different colors indicates regime change. To make the analysis easier - and the countries more comparable - countries are divided into 12 cate-

[4] According to Cheibub and Gandhi (2010), a country is democratic if and only if the following conditions are simultaneously satisfied:

1. The chief executive is chosen by popular election or by a body that was itself popularly elected.
2. The legislature is popularly elected.
3. More than one party is competing in the elections.
4. An alternation in power under electoral rules - identical to the ones that brought the incumbent to office - must have taken place.

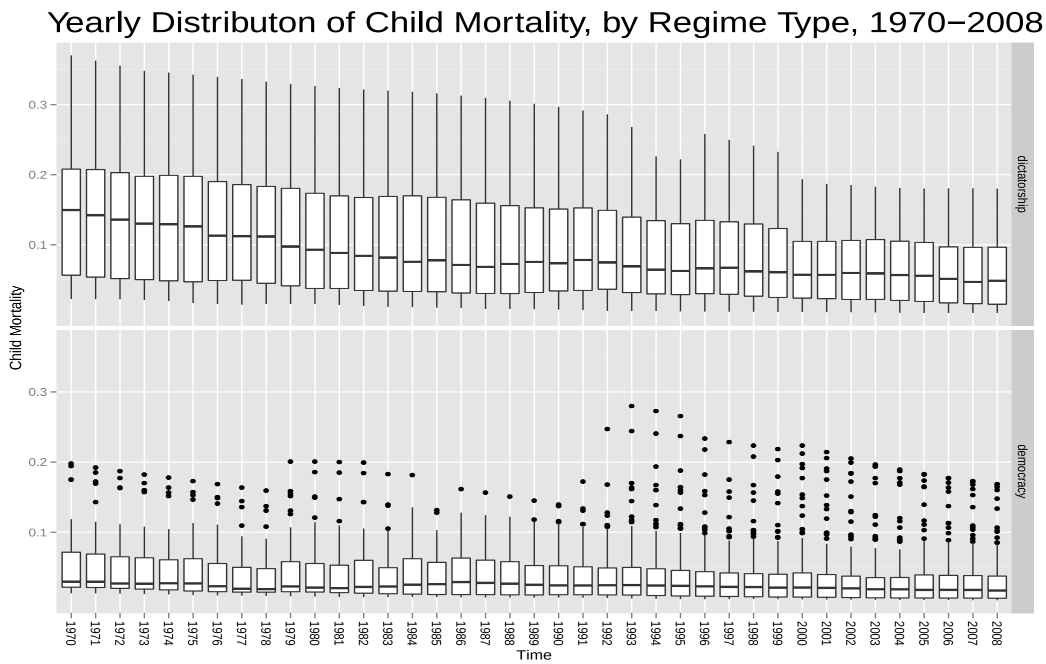

Figure 2.1: *Child mortality has declined over time for both dictatorships and democracies. Democracies have lower child mortality. Dictatorships have greater variability in child mortality, with some extreme outliers. Data includes a sample of 180 countries from 1970 to 2008. Countries are classified by regime type each year so a country that is democratic one year could be classified as a dictatorship in another year. Since the early 1990's, almost all democratic countries with high child mortality are found in Sub-Saharan Africa.*

gories. There are three income levels (low, middle and high, grouped according to their income level in the first year of the study, 1970) and four political regime types (countries that are always democratic, those which endure one transition to democracy, those with many and, finally, the stable dictatorships). We only observe 10 of the categories because no high income countries undergo regime change during the observed period. [5]. Regardless of regime type and income level, all countries in the world are reducing child mortality over time. There is more variance across countries in the previous years than at the present time. Except for a few jumps, such as genocides in Rwanda, Armenia and Cambodia, mortality rates over time are very smooth. So we see child mortality decreasing as the number of democracies are increasing.

[5] This clustering procedure is very simple and robust; the details will be presented in the methods section.

Figure 2.2: *Trajectories of child mortality rates over time for the sample of countries and years. The countries are clustered by income and political regime type. Different colors represent different political regimes. Black lines are fitted robust least squares estimates and the gray area around these lines are 95% confident intervals.*

Given that child mortality is declining over time, the question is whether democratic transition further reduce child mortality beyond what one would expected based on previous, pre-transition time trends. Different countries experience transitions at different points in time, thereby making it difficult to graphically evaluate whether these transitions were followed by significant average reductions in child mortality. Figure 2.4 makes this analysis clear. I focus on those countries which experienced a single transition to democracy, did not revert to authoritarian rule, and have more than five years of data. These countries represent the most successful transitions for the period under analysis. Therefore, they represent the best possible scenario for observing any effect of democracy on child mortality. The graph provide very little evidence of substantial changes following democratic transitions. Hence, as we model the impact of democratization on child mortality, we should not expect to detect large effects.

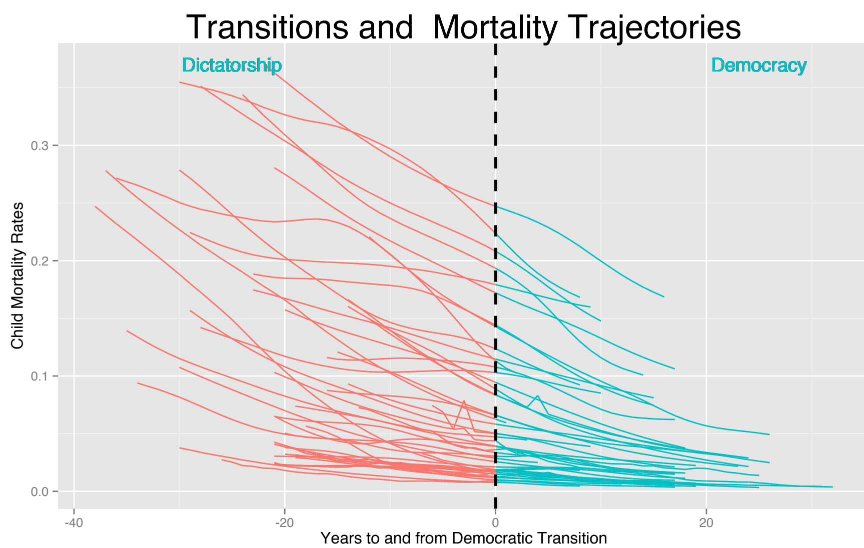

Figure 2.3: *This graph displays time to transition*

Figure 2.4: *Mortality rates as a function of time (in years) to and from the democratic transition. Country trajectories are plotted one on the top of the other as if the transition year were the same for them all. Each line is a county trajectory and the dotted vertical lines over zero represent the time of the transition.*

2.5 Methods

2.5.1 Research Design

Suppose that democratization episodes can be considered a treatment on the outcome variable, child mortality. Then the causal effect of regime type on child mortality in a given country can be simply defined as the difference in the outcome between two treatment conditions — democracy and dictatorship. The fundamental problem of causal inference, however, is that at any given time point a country cannot be simultaneously observed under democracy and dictatorship (Holland, 1986). Therefore we cannot calculate the difference in these rates between political regimes, which would be the causal effect of democracy on child mortality. For transitions countries, however, the same country can be observed at different political regimes (treatment states) but *at different point in time*. If time had no effect, one could use this information to calculate causal effects of interest as the difference in the outcome between the democracy and dictatorships.

Yet, we know that child mortality does change over time. Not only are child mortality rates decreasing over time but the number of democratic countries is increasing. Thus time is actually a major confounding factor when studying political regimes and child mortality. If child mortality changes in predictable ways over time, however, we can model its trajectory and investigate whether the previous, pre-democratic trajectories were affected by the democratic transitions. Democratic transitions can be seen as intervention in the previous, pre-transition trends and levels of child mortality.

This approach assumes that the timing of democratization is not itself endogenous to changes child mortality. This is a difficult assumption. As stressed by modernization theory, it could be the case that a third factor, such as societal development, has caused both lower level of child mortality and more democratic governments (Lipset, 1959). As a partial remedy, one can control for factors (e.g. per capita income) that might be related to both the timing of democratization and decreases in child mortality.

To gain leverage on this puzzle, I investigate whether authoritarian countries that underwent democratic transitions experienced different levels and rates of change in child mortality than they would have without a regime change. By focusing on time trends within countries I account for many unobserved characteristics that make countries different from each other. All these unobserved characteristics are absorbed by the time trends across countries. Although this is far from being a randomized experiment, the research design provides statistical control for the many other variables.

This approach focuses mostly on the transitional countries. It does so because these are the countries that can help us to learn more about the causal effect of the democracy on child mortality. These are also the countries in which we do have the clearest counterfactual scenario. Countries for which we do not have clear counterfactual scenarios, either because they were never observed under different political regimes or because there is no country similar to them in all background characteristics, can tell us little about the effects of democracy on child mortality

(King, 2006). Yet, by focusing on time trends we can still ask questions about non-transitional countries that might help us to understand whether political regimes does matter. For example, are transition countries reducing their child mortality at a faster rate than countries that never transitioned?

2.5.1.1 Alternative approaches used by the previous literature

A popular framework to investigate the longitudinal data in comparative politics is the so-called "fixed effects" model. Using dummy indicators for democracy, it is given by the following equation

$$Y_{i,j} = \pi_0 + \pi_1 \text{years}_j + \pi_2 \text{countries}_{i-1} + \pi_3 \text{democracy}_{i-1,j} + \beta \mathbf{X_{i-1,j}} + \epsilon_{i,j} \quad (2.1)$$

$$\epsilon_{i,j} \sim N(0, \sigma^2) \quad (2.2)$$

In equation 1, π_0 is the intercept; π_1 are dummy indicators for the j years, which are intercepts deviations for each year from π_0 from the baseline year and country; π_2 are country fixed effects, which are also intercepts deviations for each country from the baseline country at the baseline years; $\pi 3$ is a dummy for democracy, which differentiates democratic years from those which are not; and β is a vector of covariates, such as income per capita and HIV prevalence. The model also assumes a very simple random effects structure for the error term, which is given by ϵ_{ij}. Since this error terms term ignores clustering and auto-correlation, many previous studies have attempted to "correct" for this error structure using robust standard errors (i.e. a sandwich estimator) that explicitly models these features of the data.

While this model has proven useful in a variety of contexts, it is problematic in the present project. First, the model assumes that countries change over time in parallel—they have the same rate of change over time. By way of illustration, the model assumes that Denmark and Saudi Arabia have the same rate of change in child mortality. This assumption is not supported by the data. Secondly, the model assumes that democracy affects child mortality is only via changes in levels (intercept shifts), which is a restrictive assumptions and likely wrong. The graphical analysis above has already shown we cannot expect large changes in the level of child mortality following democratization episodes. Third, the model assumes that the effect of democracy is exactly the same for all countries in all years. Again, by theoretical expectations and common sense, one cannot expect that democratization in, say, Sub-Saharan African and Eastern Europe to be the same. In contrast, one of the major findings of this paper is that the effect of democracy on health is highly heterogeneous, with substantial variation across countries. These issues are illustrated in the appendix.

A superior model should have three main characteristics. First, it should allow different countries to have different rates of change in child mortality over time. Second, the model should capture democracy's effects on not only levels of child mortality but also its rate of change. Third, the model should allow for heterogeneous effects of democracy across the world. In short, the "fixed effects" specification makes strong assumptions about how mortality rates change over time. Rather than allow those rates of change to emerge from the data, the fixed effects specification imposes a strict structure.

The fixed effects model assumes year dummies, which are typically interpreted as unstructured time trends in the statistical literature. While this is a flexible approach, the flexibility is unnecessary: we know by graphical analysis and demographic theory that infant mortality changes very little from one year to the next, except in the case of shocks such as war or genocides. Thus we can easily used more structured time trends. In the log scale, child mortality actually follows a linear time trend - testing for quadratic time trends is unnecessary. Polynomial or more complex trends such as smoother could be added, if needed. A quick check for whether one needs a unstructured time trend or not is just to compare models with the same random effects structure and covariates, but different time trends. Thus I run three different models: (1) dummies for time and countries, the above equation; (2) linear time trends for the whole data plus dummies for each countries, so that each country has its own intercept but the same linear slope; (3) linear time trends for each country, so now each country has its own intercepts and slopes. We can compare these models using several approaches, using AIC, BIC and and also using Residual Sum of Squares (RSS) from each model via chi-square tests. All tests indicate that the fixed effects model performs as poorly as a model that assumes all countries follow the same time trends! On the other hand, all test statistics indicate that a model that assume a linear time trend for each country is much preferred to the fixed effects specification. This provides further indication that the "fixed effects" specification is not really capturing global trends in any meaningful way. Results are available upon request.

2.5.2 Modeling Time Trends

To implement the research design, I develop a statistical model of countries' child mortality trajectories over time. For the countries that undergo democratization, the model should capture possible deviations from the pre-transitions trajectories. Moreover, the model should account for the correlated nature of the data and therefore have good statistical properties in terms of estimation and prediction. To model time trends I experiment with linear, quadratic and higher polynomial time trends. As demonstrated above in the graphical analysis, these trends are mostly well-behaved and monotonically declining overtime.

To detect deviations from previous, pre-transitions trajectories, I will employ a simple tool called *bent line*. It is just a variable that tracks the passage of time after democratization. For example, if Brazil democratized in 1985 but it has available data since 1970, the column in the data frame for the bent line will be coded zero from 1970 up to 1985 and from 1986 on it will just count the passage of time, e.g. $bentline = (1970 = 0, ..., 1985 = 0, 1986 = 1, 1987 = 2, 1988 = 3, ...)$. This variable will decompose time trends for transitional countries into pre- and post-democratization trends. Thus we can test the hypothesis whether they are the same or not [6].

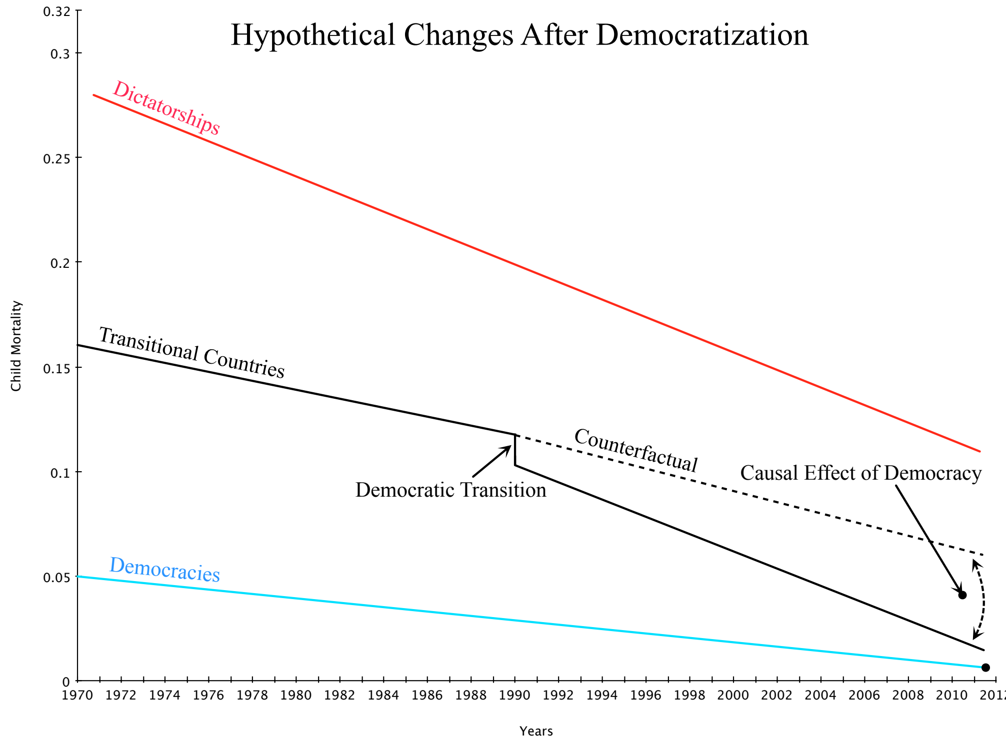

Figure 2.5: *This graph depicts theoretical time trends in child mortality for three categories of countries: democracies, dictatorships, and those which transition. For simplicity, the transitional group is modeled as having a single transition from dictatorship to democracy. Each group has a different slope and a different intercept. Democracies always perform better than the other groups. Dictatorships always have the worst (highest) child mortality rates. In this illustration, the democratic transition occurs in 1990. The horizontal dotted lines following the transition represents the counterfactual scenario (i.e. without the democratic transition). The plot shows one possibility—a shift in both the intercept and the slope.*

2.5.2.1 Regression Models, Time Trends and Bent Lines

The basic framework for the bent line and time trend model is illustrated in figure 2.5. As noted above, in almost all countries, child mortality has monoton-

[6]Bent Lines are widely used in the field of public health, biostatistics, statistics, education, etc. For a non-technical introduction to the basic methods see J. D. Singer (2003), Weiss (2005)

ically decreased over time. Countries that never democratize display on average higher mortality rates than democracies. Transition countries typically started out at an intermediary position between rich democracies (low mortality) and poor dictatorships (high mortality), but all have followed a downward trend in mortality over time. *The question is whether democratic transitions further accelerate the reduction that was already in place before the regime change.* One possibility is a change in levels around the transition. Another is that there is a change in the rate of change after the transition. The model below accounts for both scenarios. Reflecting the model graphically depicted in 2.5, I use the following specification

$$Y_{ij} = \pi_0 + \pi_1 \text{time}_j + \pi_2 \text{bentlines}_j + \pi_3 \text{democracy}_{j-p} + \beta \mathbf{X}_{j,i-1} + error \quad (2.3)$$

There are j years and i countries. Let p denote the lag of the transition, which is typically, but not necessarily, only one year. π_0 is the intercept and π_1 is the slope for the linear time trends—it measures the rate of change of the mean in child morality (conditional on the time-varying covariates) for all countries in the world over time. π_2 are the bent lines (linear in this case) which detect differences in trends after the transition; of course, bent lines can be only defined for transition countries. π_4 is a dummy variable indicating whether there was any transition in the previous p year (e.g. one year ago, two years, etc). Finally $\beta\mathbf{X}$ is a vector of time-varying covariates for the country j, maternal education, HIV prevalence and income per capita, which are typically lagged one year (thus the subscript $j-1$). For transition countries, before the transition the time trends are given by

or Fitzmaurice et al. (2011).

π_2 only, but after are given by $\pi_1 + \pi_3$. For non-transition countries, time trends are always given by π_2 only. Finally, the π_4 should detect changes in levels after the time of transitions.

One methodological problem that arises is that the model is under-defined. It has too many parameter to be estimated. To see this, consider that there are 181 time trends plus around 75 bent lines for the transitional countries, plus another 181 intercepts (one for every country). If quadratic and cubic terms are needed to capture the time trends, this numbers can further increase. As a consequence, most of these coefficients are unlikely to be estimated precisely. An alternative would be to define time trends and intercepts for batches of countries, for example, following the stratification already mentioned. Thus an alternative basic specification is

$$
\begin{aligned}
Y_{ij} &= \pi_0 + \pi_1 \text{time}_j + \pi_4 \text{time}_j * \text{cluster}_c + \pi_5 \text{cluster}_c && (2.4)\\
&+ \pi_2 \text{bentlines}_j + \pi_3 \text{democracy}_{i-p} + \beta \mathbf{X}_{j,i-1} + error && (2.5)
\end{aligned}
$$

Here, each of the $c = 10$ clusters have their own slopes and intercepts, which makes sense from the graphical analysis - and it is preferred for this data according to statistical test like log-likelihood ratio tests. With only four countries in this country, there are too few to estimate a reliable bentline for them. Yet, one problem remain here. The data is still clustered at the county level. Though the regression coefficients don't need to necessarily account for that, the error structure of the model does. Moreover, we still have to deal temporal correlation. Thus we need a more complex error structure. Fortunately, there are models to solve all these problems and they are easy to implement with current software.

2.5.3 Stratifying Countries by Income Level and Political Regime Type

Countries are clustered by income and political transition. To demonstrate the robustness of my results, I also look at episode clusters orthogonal to those, such as sub-Saharan African countries. Still other clusters are possible, such as oil states—which never democratized—or former communist countries[7].

2.5.3.1 Per Capita Income

One of the main sources of heterogeneity in infant mortality across the world is per capita income (Caldwell, 1986). Thus I stratify the sample of countries based on per capita income levels at the baseline of the study (in 1970) and regime type. Accordingly, countries are divided into three income categories based on citizens' average wealth in 1970: low (below \$2000), middle (between \$2000 and \$9000) and high (above\$9000), with roughly the same number of countries in each category. There is no indication the results are sensitive to the choice of these values.

2.5.3.2 Political Regime

Countries are separated into at minimum three groups: always democracies, always dictatorships and transitional countries. Yet, some transitional countries went through multiple transitions. Thus it is also important to distinguish

[7]More complex procedures, such as mixture models, are possible but they will increase the complexity of the analysis

successful from unsuccessful democratic transitions. Mixing both kinds of transitions may reduce the effect of democracy on health, because the unsuccessful cases will obscure the effect of the successful cases. By pooling these two types of transitional countries, we might underestimate the benefits of democracy. *The expectation is that countries that implemented successful transitions should be able to change health outcomes* (Gerring et al., 2012).

How can one define a successful transition for the purpose of stratifying the sample? One option is to distinguish between countries that underwent a single transition and those that had multiple transitions. If democracy matters, ones needs a long enough time so that the new political institutions could actually take an effect on health outcomes in society (Gerring et al., 2012). Naturally, the potential mechanisms by which democracy could effect health—e.g. representative elections, redistribution, infrastructure—all take time to implement. Brazil, Chile, and most of Eastern Europe are examples of one-time transitions. On the other hand, places like Thailand have transitioned back and forth many times and therefore will be clustered in another group. More complicated scenarios arise in places like Argentina, which has had one transition in the middle of a authoritarian very short period in 1973 and then a longer democratic period, after 1983. Thankfully, there are only a few such countries (see appendix). Though my results were robust to classifying Argentina as having either one transition or not I argue this country should be classified as having a single transition. This is because (1) the transition in the middle of an authoritarian period was too short to change the health system and (2) after 1982 it transformed to democracy that has persisted ever since, so any potential changes to the health system should have taken effect. The appendix contains a list of all such countries.

2.5.4 Modeling the Covariance Structure: Dynamic Models with Random Effects Models

So far, I have focused on functional forms of the regression coefficients — the fixed effects component of the model. Yet, we need to properly account for the correlation structure of the data, such as the fact that observations are clustered by country and are time dependent. Dynamic regression models with random effects provide a simple solution to the estimation problem described above. Random effects models are commonly used in many fields such as Economics, Statistics, Health Science and Education. These models are known by various names including Mixed Effects Models, Random Effects Models, Random Coefficients Models, among others denominations[8]. They are all similar to the basic regression model (and its extensions, such as generalized linear models) but have additional structure on the error term to handle more complex data sets (Gelman and Hill, 2006), (Weiss, 2005), (Bates, 2010), (J. D. Singer, 2003). There are becoming increasingly popular in Political Science, especially in American politics (Shor et al., 2007), (Gelman et al., 2007), (Park et al., 2004). Several recent papers have introduced or applied these models to comparative politics (Pang, 2010a,b; Park, 2012; Western and Jackman, 1994; Western, 1998; Beck and Katz, 2007) [9]. Random Effects Models display superior statistical proper-

[8]see Gelman and Hill (2006) on the conflicting denominations for the same family of models

[9]The Autumn 2005 edition of *Political Analysis* is devoted to the analysis of multilevel data sets. According to Beck and Katz (2007) "In this article we show that the RCM, estimated via

ties, such as smaller mean square error (Robinson, 1991), (Bates, 2010), (Shor et al., 2007). Moreover, the assumptions from the random effects models, such as normality of the groups, can be actually tested.

2.5.5 Basic Specification

Consider the following basic random effects model. The regression coefficients are

$$\begin{aligned} Y_{ij} &= \pi_0 + \pi_1 \text{time}_j + \pi_4 \text{time}_j * \text{cluster}_c + \pi_5 \text{cluster}_c && (2.6) \\ &+ \pi_2 \text{bentlines}_j + \pi_3 \text{democracy}_{i-p} + \beta \mathbf{X}_{j,i-1} + error && (2.7) \end{aligned}$$

where $group_i =$ are countries' clustering, for example stratifying by regime type and income level. And their random effects components are

$$\begin{aligned} \pi_{0i} &= \gamma_{00} + \xi_{0i} \\ \pi_{1i} &= \gamma_{10} + \xi_{1i} \\ \begin{bmatrix} \xi_{0i} \\ \xi_{1i} \end{bmatrix} &\sim MVN\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_0^2 & \sigma_{01} \\ \sigma_{10} & \sigma_1^2 \end{bmatrix} \right) \end{aligned}$$

The random effects are for countries,π_{0i}, and years, π_{1i}, as strongly suggested by the data. Moreover, I am also modeling the correlation between these two random effects using the multivariate normal statistics. To begin with, we can hypothesize that the residuals follow some type of AR(p) autocorrelation, but I

classical maximum likelihood, performs very well and should be more utilized by students of comparative political economy" will also experiment with more complex residual auto-correlation structures.

$$\begin{aligned} \epsilon_i &= \rho \epsilon_{i-p} + \nu_i \\ \nu_i &\sim N(0, \sigma_\nu^2) \end{aligned}$$

This model is a fully dynamic random effects model, suggested by graphical analysis, preliminary modeling and theoretical considerations. Using the R package LME, this models will be estimated using both Maximum Likelihood and Restricted Maximum Likelihood, as they are roughly equivalent but each one of them allows for different model comparison.

2.6 Results

I summarize the main results and then I discuss them in detail. The main finding is that democratization episodes are followed by further acceleration in the reductions in child mortality that were already in place before the democratic transition. However there is significant heterogeneity in those effects. In the following section, I present robustness checks and I discuss the heterogeneous effects of democratization.

2.6.1 Overview of the Findings

Using fixed effects models, I find that democracy is associated with lower levels of child mortality. On average, democracies have 15% lower child mortality than non-democracies. The inclusion of covariates and year and country effects (dummies) , however, reduces this difference to only 3%, though it is still statistically significant. Using bent line models, I find that democratization further accelerates the child mortality reduction that was already in place before the democratic transition. The yearly *additional* reduction after the democratization is small but it is statistically significant and can represent a substantial change over the course of a decade of so.

However, these effects depend on countries' income levels and political history. For the middle income countries, which transition to democracy only once, the rate of reduction in child mortality before transition is 2.62% (CI:3.35, 1.89) and the additional post-transition yearly reduction is 0.27% (CI: 0.446, 0.094; p-value:0.002). Low income countries are already reducing their child mortality rate at the yearly rate of 2.7% (CI: 3.48, 1.95) but this rate is further accelerated by 0.34% (CI: 0.59, 0.08; p-value:0.009) a year after the transition. These additional reductions in child mortality after the transition can be attributed to Sub-Saharan African countries, where the additional yearly reduction is 0.4%, and statistically significant. In terms of political history, the inclusion of countries that experienced multiple transitions minimize these effects, regardless of their income levels. Using bent line models, I find little change in the level of child mortality after the democratization episodes for most countries. For middle income countries that experience only one transition, however, democratic episodes did *increase* child mortality in the short run, particularly during the first 2 years following the democratization episodes. The intercept change after the transition is given by 1.86% (CI:1.17, 2.54; p-value:0.000). Even though the deleterious effects are concentrated in the first two years, it takes, on average,

	Model 1	Model 2	Model 3	Model 4
Democracy	-0.15^*	-0.08^*	-0.08^*	-0.03^*
	(0.01)	(0.01)	(0.01)	(0.01)
Per capita income (log)	-0.30^*	-0.15^*	-0.34^*	-0.15^*
	(0.01)	(0.01)	(0.01)	(0.01)
Maternal education	-0.16^*	-0.24^*	-0.13^*	-0.12^*
	(0.00)	(0.00)	(0.00)	(0.01)
HIV prevalence	2.71^*	3.11^*	4.01^*	3.43^*
	(0.17)	(0.10)	(0.21)	(0.10)
Country Effects	No	Yes	No	Yes
Year Effects	No	No	Yes	Yes
N	6860	6860	6860	6860
adj. R^2	0.85	0.97	0.87	0.98
Resid. sd	0.43	0.18	0.40	0.17

Robust standard errors in parentheses

* indicates significance at $p < 0.05$

Table 2.1: *Results from the fixed effects models.*

around 7 years for the country return to the previous levels. These results are robust of a myriad of robustness checks and are consistent with raw data. In the concluding section of this paper, I interpret these findings.

2.6.2 Results for the Fixed Effects Models

Table 2.1 presents the results of several model specifications. The coefficient for democracy is significant for all specifications. The first model estimates that controlling for the relevant covariates, the difference between democracies and dictatorships are constant overtime at around 15%. The inclusion of year fixed effects or country fixed effect reduces these differences to 8%. The inclusion of both types of dummies simultaneously will further reduce the difference to 3% or 2%, depending on whether maternal education is included or not. Thus the apparent 15 % difference across regime types shrinks a great deal once country and time "effects" are accounted for. While small, this difference is not negligible and is statistically significant. *Thus the new data and fixed effects models support the notion that democracy is associated with better health outcomes.* Yet, the real questions is whether this difference in means is capturing any causal effect of democracy on child mortality.

2.6.3 Results for the Random Effects Models

I present the results from models' covariance structure first. Estimated regression coefficients are not sensitive to that but prediction and hypothesis tests are, as in any regression model (Weiss, 2005), (Fitzmaurice et al., 2011), (J. D. Singer, 2003), (Gelman and Hill, 2006). Uninterested readers may skip this first section.

2.6.3.1 Model Selection of the Covariance Structure

Graphical analysis suggests a random intercept and slope model is most appropriate, but I formally test whether this intuition is correct. The formal model selection is presented in Table 2.2. I experiment with different covariance structures and residual autocorrelation functions. Whenever the models are nested, I formally test goodness of fit between pairs of them using log-likelihood ratio tests. When that is not possible, I compare them using BIC and AIC statistics. All tests indicate the random intercept and slope model with AR(1) residual structure as the best fitting covariance structure.

The results from the random intercept and slope model are the following. There is more variation among initial conditions $\sigma_0^2 \approx 0.5$ than among time trends $\sigma_1^2 \approx 0.01$. Both results agree with the graphical analysis. The correlation between intercepts and slopes is negative but very small, such that $\sigma_{01}^2 = \sigma_{01}^2 \approx -.02$. Values close to zero indicate no correlation. *This means time trends are not sensitive to initial conditions.* Specific results for each country are available and are presented in the supplementary material. Finally, and unsurprisingly, the residual auto-correlation is very high. The AR(1) residual autocorrelation, $\rho \approx .99$ indicates that there is strong time dependency.

2.6.3.2 Results for the Time Trends and Bent Lines

Figure 2.6 displays point estimates and 95% confidence intervals for the main models. Coefficients with confidence intervals that cross the vertical dotted line are not statistically significant at this level. Here I am estimating the bent lines for the one-time transitions only. This group contains only are middle and low income countries (no high income country experienced democratic transition). More detailed information, including numerical summaries and p-values, is presented in the appendix. Since I am presenting several models, graphs are better than tables for facilitating comparison across them (Kastellec and Leoni, 2007; Gelman and

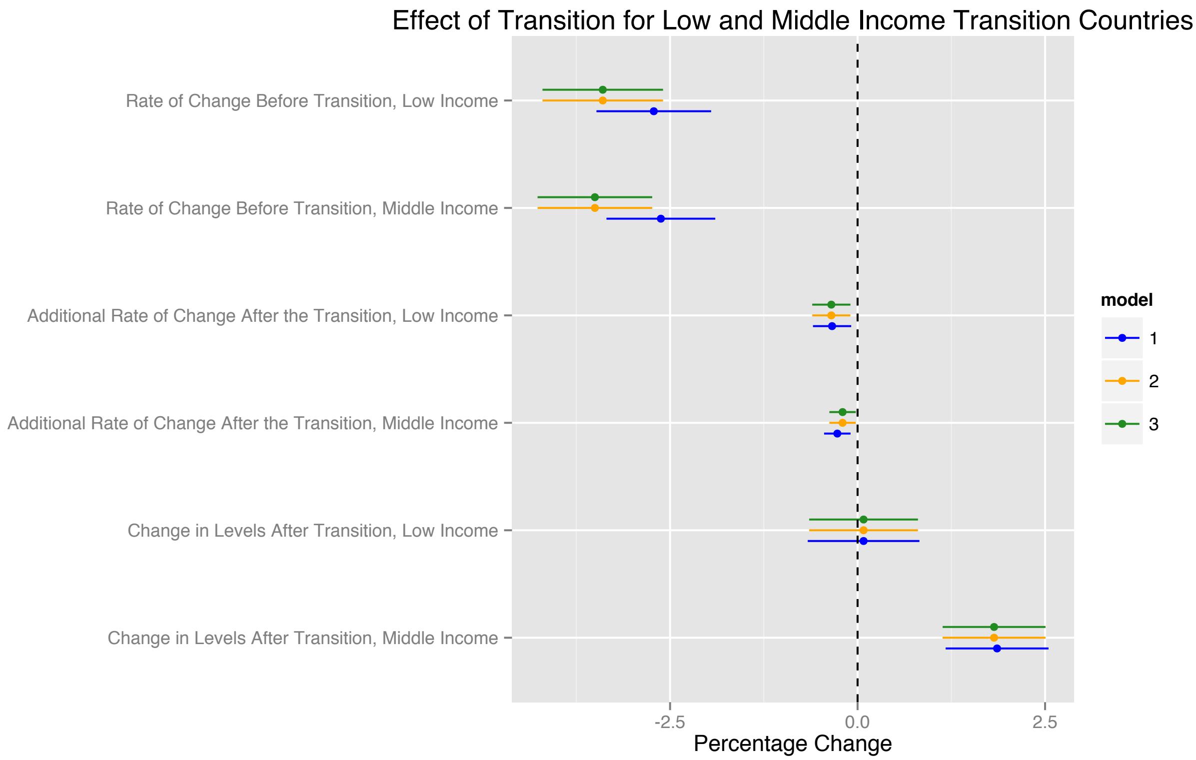

Figure 2.6: *Results for the key quantities of interest from the main bent lines random effects models. Model 1 includes income and maternal education; Model 2 includes income but not maternal education; model 3 includes maternal education but not income. The point estimates are the dots and the error bars denote 95% confident intervals. When the error bar crosses the vertical dotted line, the coefficient is not statistically significant at the 95 % confidence interval.*

Hill, 2006). The graphs also help to illustrate the heterogeneity and robustness of the results. These models are essentially similar: model 1 includes maternal education and income per capita; model 2 excludes maternal education; model 3 excludes income per capita. The exclusion of maternal education is due to the fact that, since it is a new variable not included in the previous studies, it is important to have a sense of how much it is affecting the results. The exclusion of income per capita is due to the fact that the clustering procedure is already controlling for income (recall that I use income at the baseline to create the clusters).

Low income countries are already reducing child mortality at the yearly rate of -2.7% $(-3.48, -1.95)$. After the transition, this rate is further accelerated by -0.34% $(-0.59, -0.08)$ a year and is statistically significant (p-value:0.009). The intercept change after the democratic transition for low income countries is very wide 0.08% $(-0.66, 0.82)$ and not significantly different from zero (p-value: 0.837)—it is not statistically significant. *Thus for low income countries, democratic transition further accelerates the rate of reduction in child mortality, but it has no effects on its levels.* As we can see in the graph, the results are quite robust across models. The one exception is the rate of change before the transition. The estimated effect is higher for models 2 and 3, though their confidence intervals overlap and therefore they are not statistically different from model 1.

For the middle income countries, the rate of reduction in child mortality before the democratic transitions is -2.62% $(-3.35, -1.89)$. After the transition, the additional yearly reduction is of -0.27% $(-0.446, -0.094)$, which is statically significant (p-value: 0.002 different from zero). The intercept change after the transition is given by 1.86% $(1.17, 2.54)$, which is statistically significant and different from zero (p-value: 0.000). Thus, for middle income countries, democratic transitions not only increase the level of child mortality, but also accelerate the reduction already in place before the transition. One needs to wait around 7 years after the transition before the deleterious effects subside. Also, the additional yearly reduction in child mortality after the transition is smaller than for the low income countries but these countries already have lower incidence of child death.

Taken together, these findings confirm that democracy does reduce child mortality, however the gains are small in the short run, and concentrated to low income countries. Additionally, they provide a sense of the heterogeneity of the effects of democracy on child mortality that has not been previously documented. I will further discuss it in the robustness section.

2.7 Robustness and Heterogeneous Effects

This section focuses on the results for the random effects models. I show that the main results are robust to alternative definitions of democratic transitions, the sample of countries, different leads and lags of democratic transitions, and several other checks. These robustness checks provide further evidence of the heterogeneous effects of the democratic transitions on child mortality.

2.7.1 Robustness Sample Selection and Alternative Definitions of Transitions

A natural concern is whether the bent line estimates are effected by the inclusion of non-transitional countries. Although the coefficient for the bent lines cannot be estimated by countries other than transitional countries, non-transition countries still influence other aspects of the model. This concern is related to the issue of ensuring counterfactual estimates lie within the range of the data (King, 2006). Another concern is related to the heterogeneity of the transitions and to the definition of democratic transition employed here. Are my results sensitive to the particular clustering procedure? Are there clusters that fail to overlap with my own procedures, such as Sub-Saharan Africa (Kudamatsu, 2012)? How does the inclusion of countries that experience multiple transitions affect my estimates?

To address the first concern, I estimate the same models using a subset of my sample of low and middle income transitional countries. To address the second concern, I estimate different bent lines for a separate set of transitions: (1) all transitions, (2) one time transitions for low income countries or middle income countries and also (3) a bent line for sub-saharan Africa. The estimates are presented graphically, so that they are readily comparable.

Figure 2.7 presents the results of robustness checks for sample selection and alternative definitions of democratic transitions. The restricted sample includes transition countries only. As expected, the inclusion of the non-transition countries doesn't affect the estimates of the bent lines, though it does help to model the general patterns in the world. Bent lines are always significantly different from zero, though very small substantively. The impact is higher for sub-Saharan African countries - full sample $-.4\%$ and reduced sample $-.42\%$, both statistically significant - to lower middle income transitions - full sample $-.27\% p-value =$

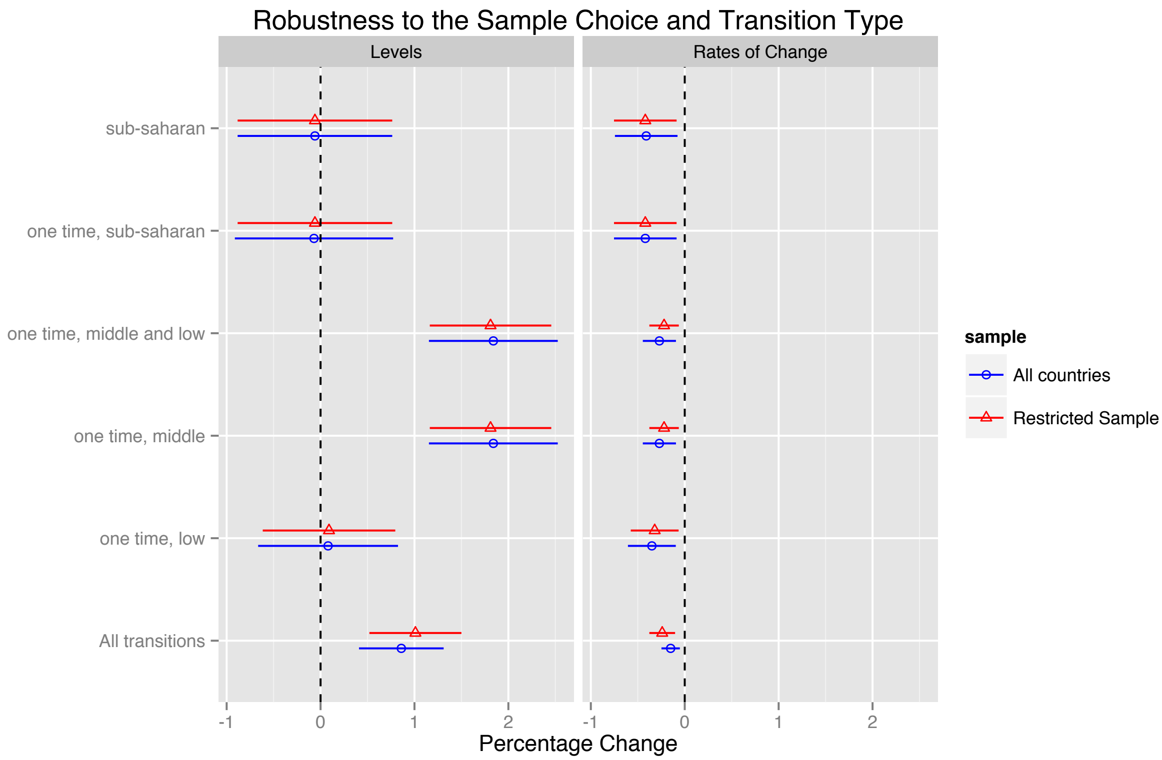

Figure 2.7: *Robustness of the bent lines estimate for different sample choices and transition types. The restricted sample includes only transitional countries. Dotted lines indicate no effect.*

.0025 and reduced sample $-.22\% p - value = .009$. The change in level (intercepts) after the transition is only significantly different from zero with the inclusion of the middle income transitions, either alone or along with the low income transitions.

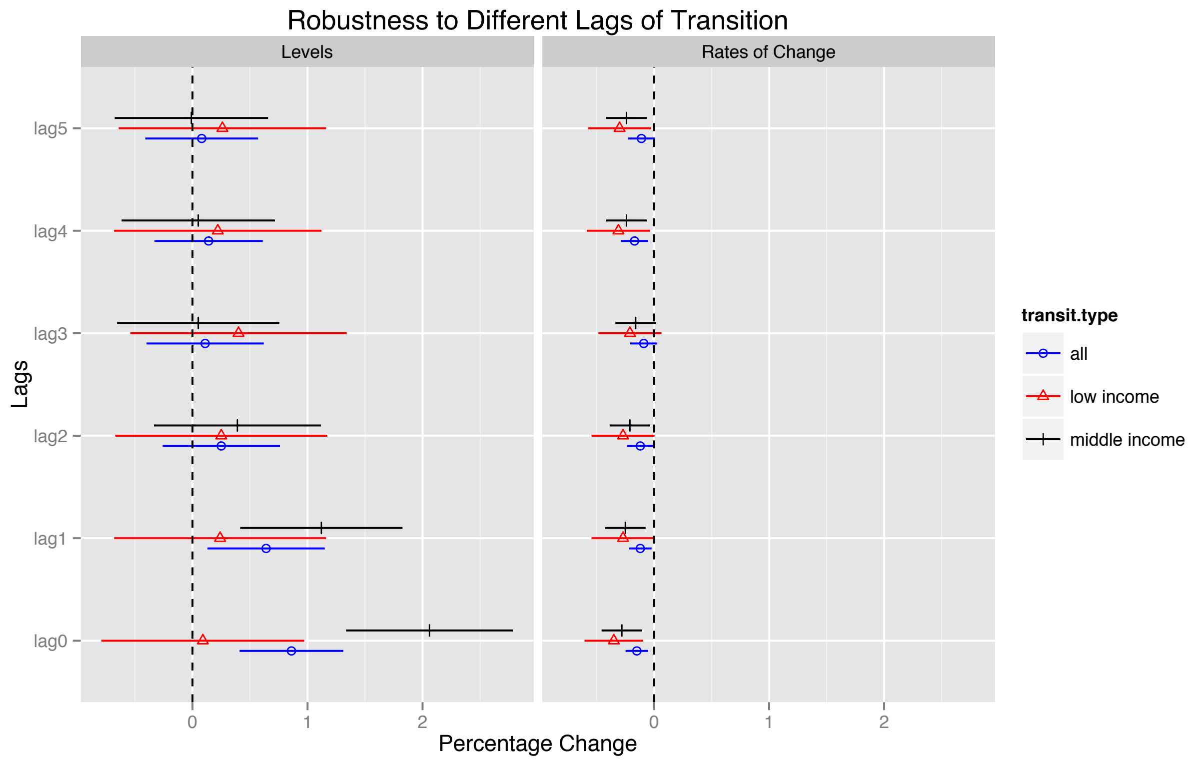

Figure 2.8: *Robustness of the estimates to different lags of years and by batches of transition.*

2.7.2 Robustness to Different Lags of Democracy

Another concern is whether there are lagged effects. Even if successful democratic transitions can improve people's lives, it takes sometime before changes in the government can be translated into services directed towards the children. Thus, one might expect some lagged effects of transitions on health, although neither theory nor empirics are very clear on the size of the lag. Moreover, these lagged effects may vary across countries. To investigate this issue, I run several models in which the bent lines (which capture rates of change) and intercepts (which capture levels) are lagged up to five years. For each of these five years, I run two models: one in which all transitions are clustered together and another in which I estimate in the same model separated coefficients for middle and low income transitions.

Figure 2.8 displays the results. The bent lines are quite robust: different lags don't change the previous results. Thus the rates of change are not sensitive to the lagged effects of democratization. The intercepts for low income countries are always statistically insignificant. Intercepts for middle income countries exhibit a different pattern: they are highly significant, both substantively and statistically for the first two lags but not after that. It indicates that democratization does increase the levels of child mortality among new democracies and that the deleterious effects are concentrated in the two years after the transition. Below, I discuss the possible explanations for this pattern.

2.7.3 Additional Model Checks

Additionally, I have investigated models' residual structure. I have run the basic specification with the other highly selected covariance structures from table; and I have also run the same fixed effect specification using ordinary least squares. None of these checks indicate problems in the models. I have also checked alternative ways to cluster countries by different income levels. Results from all these checks are available upon request.

2.8 Discussion

This paper revisits the question of whether government matters for the well-being of the country's population. It does so by investigating the relation between regime type and child mortality rates across the globe in recent decades. Estimating the *causal effect* of democracy on child mortality in a cross-national context is challenging, however. This is because one cannot randomly assign regime type to sets of countries. Yet, I argued that by focusing on countries' over time trends in child mortality, and the deviations from their long term trends followed by democratization episodes, one can get leverage on the understanding it. In particular, I have focused on whether democratic transitions affected rates of change and levels of child mortality across countries all over the world. I have introduced simple techniques, such as bent lines and sample stratification, to estimate and compare the differences between rates of changes and levels in child mortality before and after the transition for a batch of more comparable countries. I have introduced the random effects model as a tool to properly measure the dynamic process associated with trends in child mortality around the globe. My sample is formed by 181 countries since 1970, and it consists of a new data set with no missing observations and much less measurement error than previous available.

Regarding *rates of change*, my findings indicate that democratization further accelerated the reduction of child mortality that was already in place before the transition. Even though the yearly additional reduction upon the previous rates is minimal, it can be more substantial over the course of a decade of so. I found that countries' income levels and political history matters. The effect of democratization on the rates of change can be captured more clearly when one focuses on countries that transition to democracy only once. These can be thought of as the "successful" transitions. Conversely, including countries that went through multiple transitions obscures the apparent beneficial effects of democratic transition. Finally, there is heterogeneity in the effect of transitions on the rates of change across income levels and global regions. Democratic transitions have a larger effect for low income countries, especially in Sub-Saharan Africa, and a smaller effect for middle income countries. Regarding *levels*, my findings indicate very little change after democratization. For most countries, there is no larger reduction or increase in child mortality levels following democratic transitions. Yet, while democratization hardly affected levels in most countries, it did *increase* mortality level for middle income countries. Moreover, the deleterious effect of transitions for middle income countries are mostly concentrated in the first two years after the regime change. These findings are robust to different model specifications, sample choice, lags and leads of the transition, and are consistent with graphical analysis of the raw data.

Why did democratic transitions have a larger effect in low income than in middle income countries? Why did democracy increase child mortality in the aftermath of democratic transition for middle income countries? The latter finding is of particular theoretical interest, as it is not predicted by the median voter theorem. In fact, from this theoretical framework, it makes very little sense to claim that one would expect that democracies would lead to more infant deaths than dictatorships. To understand these results and their theoretical significance, we need to further explore the details of this findings.

Not all middle income countries experience an increase in child mortality after the democratization episodes. My sample includes 40 middle income countries, of which 17 were former communist countries from the Soviet Block — 8 former Soviet Union and 9 from the Iron Curtain. Within former communist countries, the deleterious effects were concentrated in Bulgaria, Latvia, Lithuania, and Romania. All of these countries experienced significant increases in child mortality immediately after the transition. Poland and Ukraine seems also to be affected, though not immediately after the transition. Poland experienced an increase in child mortality a few year after the democratic transition and Ukraine experienced almost a decade without any substantial reduction in child mortality.

Lest we think these trends are isolated to the former communist countries alone, child mortality increases appear in other countries. In Argentina, the democratic transition in 1983 not only increased child mortality in the short run but also make the progress toward lower rates slower. Greece seems to be slightly different since child mortality was already increasing before the democratic transition took place, though it peaks only after that. The Greek case is consistent with King and Zeng (2001), that found that state failure is often preceded by increase in child mortality.

What mechanism explains this post-democratization increase in child mortality? Since most of these cases are from former communists countries that simultaneously democratized and adopted a market economy, one possible explanation is that market reforms were the main underlying cause. Supporting this explanation is the fact that countries which did not fully democratize but *did* liberalize their economies—i.e. Russia—also experience increasing mortality rates. **?** reports the results of extensive research on the effects of the transitions on child health in former communists countries. In many countries in central and eastern Europe, democratic transitions reduced economic growth and increased poverty and adult mortality. The changes to the health care system resulting from a transition to a market economy also affected the child-monitoring systems. However, at that point families had become fragmented. There were increases in divorce rates relative to marriages. Parents also experienced unemployment, which was previously non-existent. Disease incidence also increased, such as anemia in pregnant women, tuberculosis among children, and other maladies. All these experiences have affect children, more so than other vulnerable populations, such as the elderly. Thus it seems that to some extent, the market transition adversely affected health. Or at the least, the conversion to the market economy introduced some short term changes in the health system that negatively affected child health.

However not all former communists countries experienced an increase in child mortality. The impact of transitions in the health outcomes has been shown to be highly country-specific (**?**). Thus it remains an open question whether these heterogeneities can be explained by political factors. The main political drivers cannot be distinguished by the data in this analysis.

Can the child mortality difference between successful and unsuccessful transitioning countries be explained by countries' politics? While the answer to this question is beyond the scope of this paper, I can offer some speculations. The heterogeneous effects of democratic transition on health suggests that the median voter model does not provide a satisfactory explanation. In its basic interpretation, this model suggests that the introduction of free elections should be enough to improve welfare in recently democratized countries. However, I have documented that when the same electoral rules were introduced in different countries, they not only produce different health outcomes but, more surprisingly, child mortality can increase in some countries. We need a model of the democratic politics during transition times that explains why politicians fail to prioritize citizen health. They may make economic or political calculations that adversely affect child mortality or they may not accurately predict the negative repercussions of their choices. Since the median voter theorem is a highly stylized representation of the democratic process, it is likely that the model misses important features of the democratic politics, especially in times of transition.

Nelson (2007) offers an explanation of why democracy may have short term cost but long term benefits. He indicates particular conditions under which democracy alone might not be enough to produce better social outcomes. In discussing the challenges that new democratic governments face in producing functional states, provides a clue for what has occurred in some middle income countries. Even from a purely electoral perspective, factors like the choice of the electoral system, existing social cleavages, and poorly informed voters might be enough to preclude complex public health reforms from being implemented.

My findings support previous studies that highlight the short term cost of democratic transitions Nelson (2007). My research contributes the insight that these costs are more salient in the richer areas of the globe than in the very poor areas. Nevertheless, within rich countries we still observe some heterogeneity. Future research should explore heterogeneity in transitions, especially across middle income countries. There are two important sets of questions. Why do we observe such heterogeneous effects in an otherwise homogeneous group of countries, such as former communist countries? Can these country-specific effects be explained by political factors? Another set of questions is related to transitions in Sub-Saharan Africa: why were democratic transitions followed by child mortality reductions in that region? It this tendency the result of governmental efforts or, instead, of an unobserved factor, such as foreign aid? For example, it has been found that more development assistance for health decreases the level of domestic spending on health (Lu et al., 2010). Since Sub-Saharan countries has been receiving large amounts of foreign aid dedicate to health, one might wonder whether international efforts, not democracy, are behind this region recent success (but see Kudamatsu (2012) for an alternative interpretation.) Finally, more detailed measurements of regime type might also improve our understanding of these heterogeneous effects. For example, this paper ignores the difference between types of democracy and dictatorship, which could be explored further. Though these differences are likely to be smaller than those between regime type, still they can be very informative. And my framework can be extended to trichotomous measures of democracies.

Appendix 1:

Model Selection for the Covariance Structure of the Random Effects Models

Covariance	#	df	AIC	BIC	logLik	Test	L.Ratio	p-value
RI	1	4	-1697.21	-1670.03	852.61			
RIAS	2	6	-12270.13	-12229.36	6141.07	1 vs 2	10576.92	0.00
RI+ AR	3	5	-26432.73	-26398.74	13221.36	2 vs 3	14160.59	0.00
RI+ AR1+ HE	4	6	-26908.41	-26867.63	13460.21	3 vs 4	477.69	0.00
AR1	5	4	-26434.73	-26407.54	13221.36	4 vs 5	477.69	0.00
AR1+ HE	6	5	-26800.85	-26766.87	13405.42	5 vs 6	368.12	0.00
ARMA11	7	5	-17926.25	-17892.27	8968.12			
ARMA11 + HE	8	6	-28194.74	-28153.97	14103.37	7 vs 8	10270.50	0.00
RIAS + AR1 + HE	9	8	-28101.33	-28046.96	14058.67	8 vs 9	89.41	0.00
RIAQS + AR1 + HE	10	11	-28373.17	-28298.41	14197.58	9 vs 10	277.83	0.00
RIASB + AR1 + HE	11	11	-28147.90	-28073.15	14084.95			

Table 2.2: *Model comparison for different covariance models (random effects) where the log of the child mortality rate is predicted as a function of time (the only fixed effects, measured in years). All models are fitted with Restricted Maximum Likelihood. Whenever possible, formal log-likelihood ratio tests are provided. The covariance column describes the variance components of the model. The abbreviations are the following: HO, homocedasticity; HE, over time heterocedasticity; AR(1) process and ARMA(1,1) RI, random slopes; RIAS, random slopes and intercepts; RIAQS, random slopes, intercepts, slopes and quadratic slopes; RIASB random intercepts, slopes and bent lines after democratization. Recall that RIAS, RIAQS and RIABS allows for heterocedasticity by design.*

Appendix 2:

Recoded Countries, Unmatched Data and Other Details

There are countries in which health and political data don't match. I highlight what I did and any suggestions are welcomed. Countries from the former Soviet Union, such as Ukraine, counted as a separate country in the health data sets - thus having its own specific data - while in the data on political indicators they all count as a single entity, Soviet Union. For this cases, the solution was easy, I've just kept them as separate countries with their own health indicators but use the same definition of political regime for them all. Czech Republic and Slovakia also has similar problems and thus I adopt the same solutions.

Yugoslavia was a more complex situation. There is indeed health information for Serbia, Bosnia, Herzegovina, Montenegro, Croatia and Slovenia but not Kosovo. Except for Bosnia, Herzegovina and Montenegro, all other countries with available data (i.e. excluding Kosovo) I kept the health data separately, but use the political indicators of Yugoslavia for them all. After the end of the communist rule I just use regular indicators from (Cheibub and Gandhi, 2010).). Montenegro I was just able for keep in the data after 2006 and Bosnia and Herzegovina since 1991.

As for Germany there is no separate health data for West and East countries before the re-unification. Yet obviously one could not recoded both country as if they were under the same political regime before that time. The solution was use the political information from West Germany before the re-unification, since mostly of the health information come from there. While not 100 % satisfactory the alternative was to eliminate Germany before re-unification from the sample, which was not optimal either. Thus in these data, Germany means basically West Germany

Some countries, specially African ones, were colonies until very recently and thus they are not present in these data base on political indicators since 1970. Thus they were included just after their independence from the colonial rule. A full list with their year of independence can be found in table 2.4. Vietnam I have data since the end of the war in 1976.

Finally, I have recoded some country-years from the original data from (Cheibub and Gandhi, 2010). The list of countries can be found at table 2.3. In this report I will present both recoded and unrecorded data but for modeling purposes I will mostly use the recoded version. I am interested in the long run effect of democracy on health, and thus the recoded version will be more useful for my purposes. Moreover, the use of the un-recoded data sets mostly reduce the effect of democracy on infant health.

Country	Democratic Interregnum	Final Transition
Argentina	1973-5	1983-present
Bolivia	1979	1982-present
Ghana	1969-71; 1979-80	1992-present
Honduras	1971	1982-present
Niger	1993-5	2002-present
Nigeria	1979-82	1999-present
Sierra Leone	1996	1998-present

Table 2.3: *List of country-years from Przeworki et al. data set recoded as one time transitions for some models, despite an additional short democratic interregnum (see dates).*

Country	Year of Independence
Angola	1974
Bahrain	1970
Bangladesh	1970
Belize	1980
Cape Verde	1976
Comoros	1974
Djibouti	1976
Dominica	1977
Eritrea	1992
Guinea-Bissau	1974
Kiribati	1978
Marshal Islands	1989
Mauritius	1968
Micronesia	1990
Micronesia	1990
Mozambique	1974
Namibia	1989
Papua New Guinea	1974
Qatar	1970
Seychelles	1975
Solomon Islands	1977
Suriname	1974
Swaziland	1968
Timor Leste	2001
Tonga	1969
United Arab Emirates	1971
Yemen	1989

Table 2.4: *List of recently independent countries, follow Cheibub at all (2010).These are the countries which are not present in the data set since 1970.*

Appendix 3:

Fixed Effects Models and Over Time Trajectories

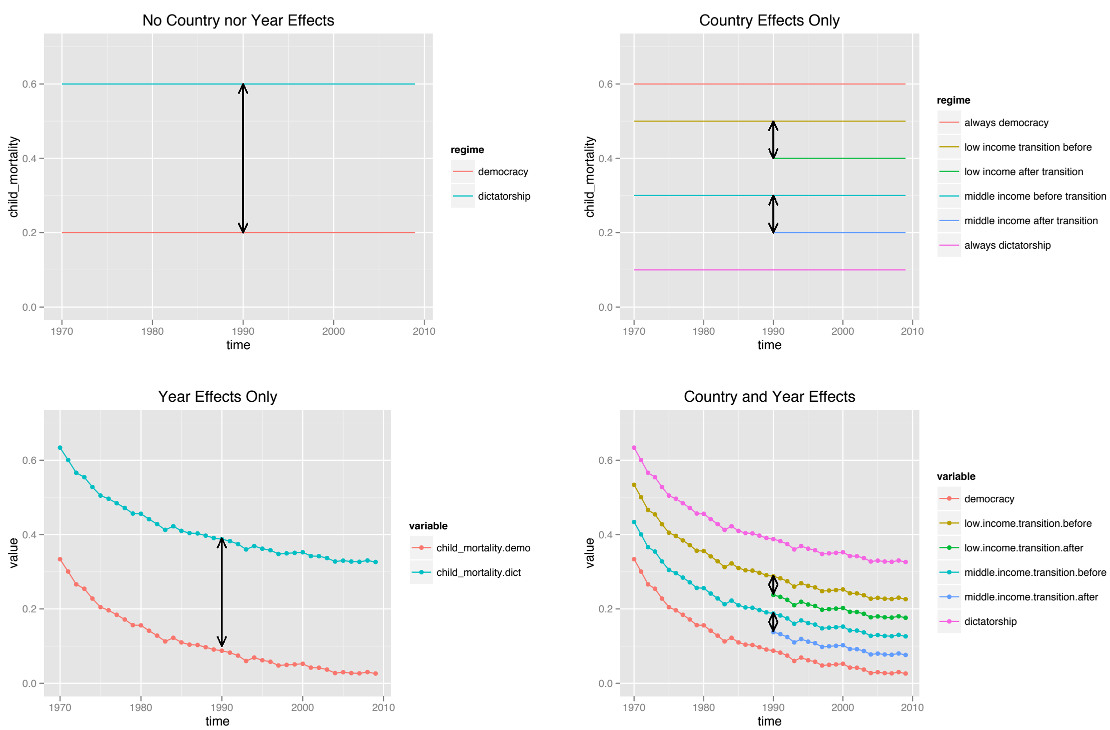

Figure 2.9: *Fixed Effects and Trajectories Overtime*

This figure illustrates how the assumptions implied by the standard fixed effect models diverge from the current data set. For simplicity a draw these pictures using simulated data, 40 years and only four countries: one always democratic country, another that is always a dictatorships and two transitional countries: a low income and a middle income. In all graphs, the coefficient for democracy - the size of the effect - is represented by vertical black double arrows. On the top left panel, it is represent a model without any dummies for time or country and thus: (1) countries trajectories are flat, as if mortality rates never decline overtime and (2) the different between democracies and dictatorships are constant overtime. Thus this model assumes that countries only follow either path, and thus countries jumps from, say, Denmark to, say, Saudi Arabia. On the top left panel country fixed effects are add to this basic specification. Now countries have their own overtime trajectories but they are parallel and constant. Thus even though Saudi Arabia and Denmark have their own initial conditions at the beginning of the study (intercepts) they all follow parallel and constant overtime changes. For this case, the coefficient for democracy means a jump on the trajectories following the democratization year. Yet, this jump is suppose to be the same for all countries, and therefore there is no heterogeneity in the effect of democracy is allowed for. The bottom left panel illustrates the case time dummies only. Here, time trends are very flexible: the can go up in a given year and go down in the next one and then go down again. Yet, this is not so useful in my case as we know that infant mortality rates follow a downward trends overtime.In this specification, and even though overtime change is very flexible, the distance between democratic and non-democratic countries are constant: they differ by the same amount every single year - which we know it is not true. Moreover, as in the model without any dummies for time nor countries, countries overtime paths moves from poor dictatorships to rich democracies, without any room for in between paths. Finally, in the bottom right, I illustrate the paths assumed by the fixed effects model for year and countries. Now all countries overtime trajectories follow the unstructured time trends but they differ by initial conditions (intercepts). Thus all countries follow parallel path, with the same overtime change. Here the coefficient for democracy is a is a jump in countries trajectories after the democratization. Yet, this jump is the same for all countries.

CHAPTER 3

Has Democracy reduced Inequalities in Child Mortality? An analysis of 5 million births from 50 developing countries since 1970

3.1 Introduction

This paper is about the relationship, if any, between democracy and equality. Theories of democracy lead to the expectation that democratic governments will provide more welfare enhancing goods for the poor than autocracies (Meltzer and Richards, 1981),(Acemoglu and Robinson, 2000),(Lake and Baum, 2001),(Sen, 1999),(Kudamatsu, 2012),(Przeworski et al., 2000). I test this argument using the gap in child mortality between the rich and the poor as a measure of the government delivery of welfare enhancing goods (Ross, 2006),(Victora et al., 2003). Although governmental policies are not the only influence on infant mortality rates, they do make a substantial contribution, for example, for the delivery of clean water, vaccination campaigns and by creating health clinics for the poor (Black et al., 2003),(Jones et al., 2003),(Bryce et al., 2003). More specifically, the introduction of democracy should make a difference in the previous, pre-transition trends and levels of child mortality reduction across different income levels within previously authoritarian countries.

The median voter theorem (Meltzer and Richards, 1981) and its extensions (Acemoglu and Robinson, 2000) predict that democratization moves the median voter downward towards the poor. Thus forces governments to provide better services for those outside the rich elites as otherwise they will lose electoral support. Other political economy models predict that under competitive elections with universal suffrage, politicians will be forced to provide more public goods for the population (De Mesquita et al., 2002),(Lizzeri and Persico, 2001). These theories have implications for the provision of health, including the reduction of infant death: since those at the bottom of the income distribution suffer disproportionally from child mortality rates (Black et al., 2003) and relatively inexpensive policy interventions could prevent most infant deaths (Jones et al., 2003). Therefore it follows from standard political economy models that democracy, by producing more services to the poor, should reduce overall child mortality.

To date, there is an extensive cross-national literature on regime type and infant death (Gerring et al., 2012),(Kudamatsu, 2012),(Baum and Lake, 2003),(Ross, 2006),(Navia and Zweifel, 2003),(Przeworski et al., 2000)[1]. These studies use na-

[1]Child mortality is a measure that is sensitive to many other conditions, including access to clean water and sanitation, indoor air quality, female education and literacy, prenatal and neonatal health services, caloric intake, disease, income, that are hard to measure among the very poor (Sen, 1999),(Victora et al., 2003). Other commonly used measures of well-being, such as poverty rates, school enrollment rates, and access to primary health care, tend to be less reliable (and less comparable) since their definitions vary from country to country and over time (Ross, 2006). In addition, focus on child health offers us insight into future dimensions of well-being in the developing world. For example, Hatton (2013), using height as a measure of well-being improvement across Europe, found that the main factor improved heigh in these

tional averages of child mortality and investigate whether lower child mortality rates are associated with democracy [2]. Reductions in national averages of child mortality, however, do not necessarily imply in health improvements for the poor. While this is particularly true for high mortality countries, where births from all income levels suffer high death risk, it is also true for middle and low income countries in general, where reductions in the national averages of child mortality may be caused by health improvements across individuals from all income levels, not only the poor. Thus political economy theories can be more precisely tested if one investigates the effect of democracy for child mortality rates at each income level.

By focusing on *national averages*, inequalities in child mortalities across subpopulations from different income levels cannot not be captured. Indeed, it is well-known that countries with the same national averages of child mortality may have totally different distributions of the populations at risk (Gakidou and King, 2002). Thus, by looking at national averages of child mortality, one cannot know whether overall rates are declining due to improvements among the poor or improvements among those with middle or upper income levels (Victora et al., 2003). National averages of child mortality are not of sufficient high resolution to test many political economy models. Yet, when working with national averages of child mortality, scholars in political science implicitly attribute improvements across the lower income strata. More nuanced measures reveal that this assumption is rarely accurate, particularly in high-mortality places.

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continent was the decline of the disease environment as reflected by the fall of infant mortality.

[2]Kudamatsu (2012) is an important exception as it uses individual level data. Yet, it still focuses on the *mean* effects of democracy on child health

Secondly, national averages of child mortality might mistake changes in the demographic composition of the population for well-being improvements. For example, the age of the mother, her level of education, and whether she lives in a rural or urban area, all impact her children's probability of survival. National averages of child mortality fluctuate as a function of all these and other demographic features. Thus to test the impact of democracy on well-being we want to control for demographic changes at each income level within each country. We ideally want to exploit variation over time within fixed demographic groups — i.e. young, low-income mothers from rural areas — within each country to infer the effect of democracy. And these are not minor points. As suggested by Modernization Theory (Lipset, 1959), demographic changes are often confounded with both democratization and child mortality reduction[3].

I investigate the effect of democracy on child mortality rates at an unprecedented level of detail. I analyze records of 5.5 million births from over 50 middle and low income countries that account for over 75% of the infant death toll in the world. With these data, I investigate changes in mortality rates over time for births from each income level in each country while controlling for changes in the demographic composition of the population. In doing so I test whether democracy actually improves health outcomes for the poor as compared to the rich, while controlling for demographic composition as well as prior child mortality level and trends. These fine grained data and research design allow me to test political economy theories more directly than previous research.

[3]This, of course, raises the question of whether democracy is acting indirectly, by reducing the number of births from more vulnerable subgroups. By disagregating across income levels, this research framework that separates out direct and indirect effects.

This study demonstrate a rich and poor gap in child mortality continuous to exist even after controlling for demographic composition effects. I also show that these inequalities are declining over time. Yet I find complex linkages between political factors and health care provision. On average, political regimes do not affect either countries' initial levels of inequality nor their over time rate of change. Also on average democratic transitions do not systematically change the previous rates of reduction in the rich-poor gap. However, there is remarkable heterogeneity in the effects of the democratic transitions across countries. For example, the introduction of democracy in Pakistan is always associated with an increased rich-poor gap in child mortality. On the other hand, in most Sub-Saharan countries, democratization is associated with a reduction in child mortality gap.

The paper is organized as follows: first, I review previous literature on democracy, redistribution and child mortality. I show that the gap between rich and poor has not been adequately analyzed and that it is a quantity of major theoretical interest. Second, I discuss how the focus on national averages of child mortality, though important, may not be a good proxy for well-being among the poorest in the developing world. Next, I present new data set, describing how it will advance our understanding about inequalities between rich and poor[4]. I discuss the methodological challenges and propose a research design to get reliable answers. I then present my results. Finally, I conclude by discussing the theoretical implications of these results.

[4] A detailed discussion is presented in the data appendix.

3.2 Democracy, Redistribution and Infant Death

How does democracy affect public health, especially children's health? Many political economy models implicitly assume that governments can indeed change levels and/or trends in child mortality, especially among the poor. Building on this assumption, scholars focus on the conditions under which governments will have incentive to provide better health care across income levels. If child mortality is largely a function of factors beyond governmental control, however, democracy and political incentives will likely not change health outcomes. For example, suppose tropical climate, by fostering dangerous epidemics, is a major vector illness and thus a major factor behind child mortality; or, similarly, suppose governments from low income countries lack the resources to prevent premature deaths. The public health literature, on the other hand, has long investigated how low-resources governments can affect health outcomes. To understand how political institutions can affect health in the developing world, we need to review and integrate both scientific fields.

3.2.1 Can Premature Infant Deaths be Prevented by Poor Goverments?

In a series of studies published by The Lancet in 2003, a set of fundamental questions of to political economy were investigated: where are children dying and why? Could these deaths be prevented with current medical technology and existing resources? If so, why aren't these deaths averted? What can be done to improve health systems?

Black et al. (2003) review myriad of studies and a wealth data on the causes of premature death in the developing world in recent decades. They find that 90% of all premature infant deaths were concentrated in 42 countries and half of them in only six (in order of the death toll: India, Nigeria, China, Pakistan, Congo and Ethiopia). Common challenges across different countries include undernutrition, infectious diseases, and particularly the effect of multiple concurrent illness. For example, measles or malaria are often complicated by pneumonia and diarrhea. Undernutrition is the underlying cause of a substantial proportion of all child deaths. For infants aged 0-5 months, lack of breastfeeding is associated with five-fold to seven-fold increase in death risk while non-exclusive breastfeeding is associated with a two-fold increase. Vitamin A deficiency increases death risk from diarrhea, pneumonia, measles and malaria by 20-25 %. Likewise, zinc deficiency increases the risk of death from malaria, diarrhea, pneumonia by 13-21 % [5]. AIDS is a more localized cause of infant death: it is responsible for only 3 % of deaths and it only accounts for more than 10% of the infant deaths in 3 of the 42 countries with the highest level of mortality. Yet, in Zimbabwe and Botswana, it accounts for over 50 % of the under 5 deaths.

Jones et al. (2003) investigate whether public health interventions can reach the majority of citizens in low income countries, where governments have limited resources. The analysis focused on the 42 countries in which 90% of premature infant deaths occur. Instead of focusing on poverty or physical environment,

[5]Estimates and uncertainty bounds for the main causes are the following: 22% of deaths attributed to diarrhea (14-30%), 21% to pneumonia (14-24%), 9% to malaria (6-13%), 1% to measles (1-9%), 33% to neonatal causes (29-36%), 9% to other causes, and fewer than 1% to unknown causes.

it looks at the more proximal determinants that can be affected by healthcare. Jones et al. (2003) do not consider factors outside of the health sector that are know to impact child mortality, such as maternal education. Within the health sector, however, their study investigates interventions that reduced both exposure to diseases and disease mortality. In their calculations, they only include interventions with known effects and thus the estimates from their studies are somewhat conservative.

The study concludes that roughly two-thirds of the under 5 deaths in these 42 countries could be prevented with appropriated interventions. For example, in most cases diarrhea can be treated with simple oral rehydration therapy. Malaria may be avoided with simple measures such as insecticide-treated bed nets or treated with inexpensive anti-malarials. Measles, another common disease, can be prevented through cheap and effective vaccine. A group of effective nutrition interventions including breastfeeding, complementary feeding, vitamin A, and zinc supplementation could save about 24 million children each year (25% of total deaths at the year of the study). Effective and integrated case management of childhood infections (diarrhea and dysentery, pneumonia, malaria, and neonatal sepsis) could save 32 million children each year (33% of total deaths). Hence, there is no need for expensive new drugs, technologies or vaccines to achieve large further reductions in child mortality in poor.

Bryce et al. (2003) discuss reasons for such low health care coverage and possibles remedies. For instance, in Brazil, Egypt, Philippines and Mexico, diarrhea-control programs and oral rehydration therapy led to mortality reductions. In

Latin America, governmental programs have eradicated polio and made measles quite rare. The main point of the study is that strengthening national health systems is of paramount importance.

Thus there exists plenty of evidence that governments from poor regions of the world do have the resources to greatly reduce child mortality. The political question is under which conditions are they willing to do so?

3.2.2 Regime Type, Redistribution and Health Provision for the Poor

There are many ways in which politics, health and redistribution are linked. In a series of studies on famine, poverty and deprivation, Sen (1999) and Sen and Dreze (2002) describe electoral competition and free press as political devices that force governments to provide for the poor, specially in periods of crises. Perhaps the most influential approaches linking politics and well-being have focused on the provision of health services as a redistributive issue. The central idea in these studies is that democracies help the poor by providing them with more redistribution than non-democracies. Because child mortality is mostly concentrated among the poor (Ross, 2006), (You et al., 2010), targeting them with basic health services should have the effect of reducing child mortality.

One influential argument regarding redistribution comes from Meltzer and Richards (1981)[6]. Here, the key players are a wealthy elite, the remaining citizens, and the government. Under dictatorship, government seeks political support from only the wealth elite. Democracy expands suffrage such that the poor are

[6] See alsoMuller (2003) for a comprehensive, if somewhat dated, review of the literature.

included among the electorate. As a consequence, democratization moves the median voter down in the income distribution since the richest are no longer the only ones voting. To see this, consider the following: suppose income is unequally distributed in the society before the democratization. Then the median voter, immediately after the democratization (i.e, the suffrage expansion), will earn less than the median income. Assuming voters choose politicians that maximize their own economic welfare, the median voter will support policies that tax the wealthy and redistribute to middle and low income classes. According to this logic, democracy should favor redistribution from the rich to the poor [7] .

Boix (2003) builds on this model by incorporating capital mobility and an strategic elite that controls the state under authoritarian rule. and the mass public, who controls power under democracy. In their model, the mass public controls power under democracy, which indicates redistribution toward the poor. Acemoglu and Robinson (2000) explore the conditions in which states democratize; it suggests that authoritarian government favors the interest of the elite, while democracy supports redistribution for a large fraction of the electorate. Lizzeri and Persico (2004) and De Mesquita et al. (2002) argue that under competitive elections with universal suffrage, providing public goods for the mass electorate is a lower cost strategy for politicians to win than direct transfer to specific voters groups. This is because under democracy politicians need to ap-

[7] Though this is the standard presentation on the literature, it is not entirely descriptively accurate. In fact, most modern dictatorships held universal suffrage. The problem though, is not so much that the poor don't vote, but instead no one's votes choose who rules. Possibly the rich choose who rules in some other way, or maybe rulers and their allies become rich and aren't forced to share power in order to maintain their rule. Yet the basic final outcomes are similar for my purposes: under non-democratic elections, government don't have incentives to design policies that reach those outside the elite groups.

peal to a large number of votes. Though there is nothing inherently pro-poor in providing public goods, most of child mortality reducing measures such as vaccination campaigns, public health clinics, and clean water would be provided as public goods.

None of these studies focus on health issues, let alone child mortality. Yet all these models suggest that the introduction of democracy should provide redistribution to the poor, where child mortality is highly concentrated. Also, all these works focus on elections as the main incentive for redistribution.

3.2.3 Previous Empirical Studies on Regime Type and Health

Previous empirical studies have provided contradictory findings on the effect of regime type on health. Przeworski et al. (2000) reported that democracies do provide better health outcomes, including lower infant mortality. Lake and Baum (2001) found that a move from complete autocracy to complete democracy substantially reduces infant mortality. Besley and Kudamatsu (2006) found a link between democracy, life expectancy and infant mortality. Focusing on transitions in sub-Saharan Africa, Kudamatsu (2012) found that democracy did reduce infant mortality. Yet, recently, some of these results have been challenged. Ross (2006) found that once high income dictatorships are included and missing data is accounted for, there is no evidence that democracy is beneficial to the poor infants. Gerring et al. (2012) did not find contemporaneous effects of democracy on health, though they argue that the accumulate stock of democracy is important for current level of child mortality. Focusing on caloric intake,Blaydes and

Kayser (2011) find that democracies and hybrid regimes are better at translating economic growth into higher calorie intake, which was used as a proxy for redistribution.

The view that democracy produces superior health outcomes was challenged by an influential empirical study by Ross (2006). Based on its empirical findings — no effect of democracy on child mortality — it challenges this theoretical literature by providing an alternative theory. According to Ross (2006), infant mortality averting goods are relatively inelastic: as long as households don't suffer from severe budgets constraints, they will buy those goods anyway on the private market. The middle and upper income strata can privately purchase these goods. However, the poor rely on public provision in order to have access to them. Thus the demand of mortality averting goods as a public goods is specific from the lowest income strata and governments supply these goods only insofar as they can or want to respond to the needs of the low income household.

Ross (2006) is not the only one to challenge the view that democracy will produce more redistribution. As Nelson (2007) argues, often the introduction of democracy is not associated with better health outcomes and, in some cases, electoral pressures actually impedes services for the poor. Typical pathologies of new democracies may diverge governmental efforts and societal demands, even in a context of competitive elections. Electoral rules, social cleavages, party ideology and the natural difficulties for ordinary citizens to understand large scale complex institutional and policy reforms may all undermine efforts to improve health services. Moreover, interest groups and political decentralization might hinder improvements as well. Still others such as Iversen and Soskice (2006) also call attention to the social composition of the citizens, including race, ethnicity and religion, that might along these lines, further hindering pro-poor policies.

Thus whether democracy and elections actually redistribute to the poor is and open an active debate. I hope this paper can further advance this debate by focusing on an important but overlooked issue, the child mortality gap between rich and poor.

3.2.3.1 Measures of Regime Type

Recent scholarships provide us with several measures of democracy. These measures are often highly correlated. While one could compare results across different measures, here I focus on a well-established measure of democracy that are based on country observable characteristics and focused on elections. In fact, one of the core assumptions from the theoretical literature is that the free elections are enough to trigger redistribution [8]. I employ the measure of democracy developed by Przeworski et al. (2000) and extended by Cheibub and Gandhi (2010). The advantage of this measure is that it is highly comparable across countries. Thus we can investigate changes across the developing world when democratic electoral rules are introduced.

[8] Popular measures of democracy include Polity IV and Freedom House. There are at least two important problems associated with these in the context of my study: (1) they do not focus on elections (2) they are not based on countries' observable characteristics.

3.3 Limitations of Studies Using National Averages of Child Mortality

National averages of child mortality are only one of the many ways to measure premature death. They measure the total premature death toll in a given society in a given year. They also address a specific and important question: how many children born in a given year made it to the age of, say, 5 years old? Our ability to measure this important quantity has improved remarkably (Rajaratnam et al., 2010). It is often used as a proxy for well-being of the poor or as an indication of the rich and poor gap. Yet, these applications are often misguided. For example, changes in the national averages of child mortality need not reflect changes in these rates among the poor, especially in high mortality places. Moreover, national averages of child mortality, by construction, cannot tell us the difference in rates across income levels, which is a major quantity of theoretical interest. Finally, by using national averages of child mortality one cannot control for changes over time in demographic factors associated with both democratization and reduction in child mortality, as the ones highlighted by modernization theory. Thus, by using individual level data, one can have much more leverage in estimating the *causal* effect of democracy on infant health (Kudamatsu, 2012).

3.3.1 Inequality in Child Mortality Within Countries

3.3.1.1 Overall Inequities

Within developing nations, there are enormous variations in child mortality across subpopulations. And countries with the same national averages can and often do have different distribution populations at risk. For example, Gakidou and King (2002) compare Benin and Central African Republic, showing that while both countries have quite similar average probability of death, they also present markedly different distributions of the actual survival times and hence divergent health inequality. In the Central African Republic, about 25% of children have a probability of death lower than three percent. In contrast, children in Benin have risks of death more closely distributed around the mean, with only 4% of its children having a probability of death lower than three percent. Clearly, at the lower end of the distributions, Benin has a worse performance, but it does much better at the higher extreme. For example, in Benin, less than 1% of children have a probability of death greater than forty percent, whereas the Central African Republic more than 4% of children have that probability of death.

3.3.1.2 Inequities Across Income Levels

Victora et al. (2003) document wide disparities between rich and poor not only across countries but also *within the same country.* They also find that the poor are more likely to be exposed to health risks. Inadequate water and sanitation, indoor air pollution, crowding and exposure to diseases are common problems for the poor. Also, the poor have less resistance to diseases because of undernutrition and other hazards typical in poor communities. These inequalities are most likely the results of unequal access to preventive care and health services. The poorest children are least likely to be vaccinated, to receive vitamin A or to sleep under a treated mosquito net. They also note that public subsides often go to the middle class or even to the richest communities. In countries such as Guinea (1994), Ecuador(1998) and India(1995-6) most government subsides to the health sector goes to the richest 20 %, while places like Costa Rica (1992) and Sri Lanka(1995-6) do better in reaching the poor.

As a consequence, the mortality gap between rich and poor children is not only wide but also growing in some places (Victora et al., 2003). In Indonesia, for example, under-5 mortality is nearly four times higher in the poorest fifth of the population than in the richest fifth. These gaps exist within all regions. In Bolivia, under-5 mortality decreased during the 1990s by 34% in the richest quintile but only by 8% among the poorest quintile. In Vietnam, poor children saw no appreciable improvement in their survival prospects during the late 1980s and early 1990s. A policy intervention that eliminated these inequities - e.g., by bringing rates in the poorest 80% of the population down to those prevailing in the richest 20% - would have a major effect on the under-5 mortality rate for the country as a whole, even in low-inequality regions. Worldwide, about 40% of all under-5 deaths could be prevented this way. In several African countries, mortality rates among poor children actually rose during the 1990s, even though they fell among better-off children.

3.3.2 Demographic Compositional Effects

Individual level data on infant death also has clear advantages in helping us to have a more causal interpretation of the effects of democracy on infant health[9]. Even though there is no random assignment of political regimes to countries (and hence causal inference is problematic), by using individual level data on child mortality one can control for changes in demographic factors that might influence both democracy and mortality. In fact, modernization theory Lipset (1959) holds that democratization is a consequence of an overall societal process where more traditional social structures are replaced by more westernized, urban life styles with widespread use of modern technology and medicine. These processes also imply a change in cultural and moral values. For example, modernization is often associated with an increase in maternal education and a reduction in the number of families living in rural areas. It is also implied a more equalitarian position for women in society, and a widening in political participation. Since some of these factors are strong predictors of child mortality, modernization also changes demographic factors that are relevant for child survival.

The data I employ allow me to exploit changes over time within specific demographic groups in each country, instead of only relying on cross-country or within country comparisons. For example, one can look at the changes in levels and rates of change of child mortality for poor, low aged mothers from rural areas. Further, one can analyze trends in subgroups of theoretical interest, such as rich versus poor, while controlling for other demographic variables. As

[9]This point will be discussed in more details in the methods section.

a consequence, results are robust to changes in the demographic composition of the population over time that drive both democratization and changes in level of child mortality, but with no direct relationship between the two. These data also allow me to evaluate whether the effect of democracy is *indirect*, via changes on the demographic composition of the population. In that case, instead of reducing, say, mortality rates from low aged mothers, democracy would be acting indirectly, by reducing the fraction of mothers that belong to this high risk group. While this is an important question, it can only be answered with individual level data. National averages of child mortality cannot separate out net (marginal) and conditional effects of democracy[10].

3.4 Data

The data set used in this study come from the Demographic and Health Surveys (DHS)
(http://www.measuredhs.com/). These are nationally representative surveys that have been conducted in more than 85 countries since 1984 (Corsi et al., 2012),(Fabic et al., 2012). These surveys collect a great deal of information from these countries, particularly on the fertility and reproductive health of their population. Low income countries and international agencies have long relied on it to monitor the health of their population. For example, the national child mortality averages are often estimated from DHS (Rajaratnam et al., 2010). DHS has standard procedures which makes their data highly comparable across countries

[10]I am using "marginal" in the probability of summing over all demographic levels.

and thus easier to use in cross national studies (Gakidou and King, 2002).

DHS also collects information on indicators of permanent income for each household, such as ownership of car, radios and TVs; whether the household has electricity and running water; type of the materials used the walls, floor and the roof of the house; and the type of toilet in the household. This information is used to construct an indicator of permanent household income. Details of the model used to construct this indicator are discussed by Rutstein (2008), but they are also discussed in the data appendix.

DHS data are based on *retrospective surveys* that can be used to form *retrospective panels*, which are a common source of information in demography and health sciences, particularly from developing countries. Some countries were surveyed only once, such as Brazil, while others have multiple waves, such as India[11]. Taken together the data contain information for approximately 5.5 million births. But the sample size varies considerably from country to country. While Kazakistan has the records of less than 15 thousands births, India has over a million recoded births. Retrospective panels are constructed from these surveys as follows: at the year in which the survey is conducted, mothers of reproductive age (usually 15-45) from a sample of representative households in the country are interviewed. These mothers answer several questions, including ones about their complete birth histories — how many children they had and when. These answers are use to form retrospective panels where each observation represent a child born to a given mother in a given year. Additionally, interviews collect objective

[11]Detailed information is available on the online appendix.

information from the household, such as household assets. These surveys are representative at the national level, but sometimes they are also representative at subnational levels, such as in India.

One main advantage of using these data over conventional sources, such as official government reports, is that these data are largely immune to political manipulation. It is an USAID-funded project currently implemented by a private company ICF International (Corsi et al., 2012),(Fabic et al., 2012). The data itself has been used and validated by thousands of researches all over the globe. Thus most of the previous concern about miss-reporting due to political reasons (Ross, 2006) are greatly minimized here [12].

These data are subject to several problems, such as recall bias, lack of representatives of some subpopulations, and a few types of censoring and measurement error in the variables that were not collected by the time of the interview. I discuss all of these issue in detail in the appendix. Overall, there are very few disadvantages in using these data as opposed to using national averages of child mortality, *even if one only cares about national averages.* In fact, at least for the sample of countries I have included here, the best national averages of child mortality closely match smoothed versions of the proportion of children from the DHS sample[13]. Even for catastrophic events, such as the genocide episode in Rwanda, the DHS data follows quite well the best national averages of child mortality.

In using these surveys, I have tried to maximize the number of countries

[12] Though this is also true for more recent estimates of National Averages of Child Mortality

[13] This is shown graphically on the appendix.

included in the analysis. Yet, I needed to include countries for which the data coverage was long enough that I could construct a representative panel of low and middle income countries over time. I include any countries for which the wealth information was available, excluding the first wave of the survey, from the mid 1980's. Thus, I have included all countries with data available since the second wave of the surveys: 50 low and middle income countries (see data appendix). Within these countries, I have excluded all births before 1970. Before 1970, most countries had very few birth documents, and they did not represent their population, as we can see when this information is compared with the national averages of child mortality.

The sample of countries included in my sample are quite representative of the premature, infant death toll in the world. Even excluding China, the countries in my sample account for more than the 75% of infant deaths in the world, from 1970 to 2010. Details are in the data appendix.

3.4.1 Time Trends in Mortality Rates by Income Level Within Countries

Figure 3.1 describes changes in child mortality for rich and poor children in my sample. Each line represents a country. The left panel represents the richest while the right panel reflects poor within each country [14]. Child mortality is declining for both the rich and the poor strata of the population. The gap between them are mostly closing over time. Yet, the poor suffer from disproportionally higher

[14] In the appendix, country-by-county plots are available for a very detail look the data.

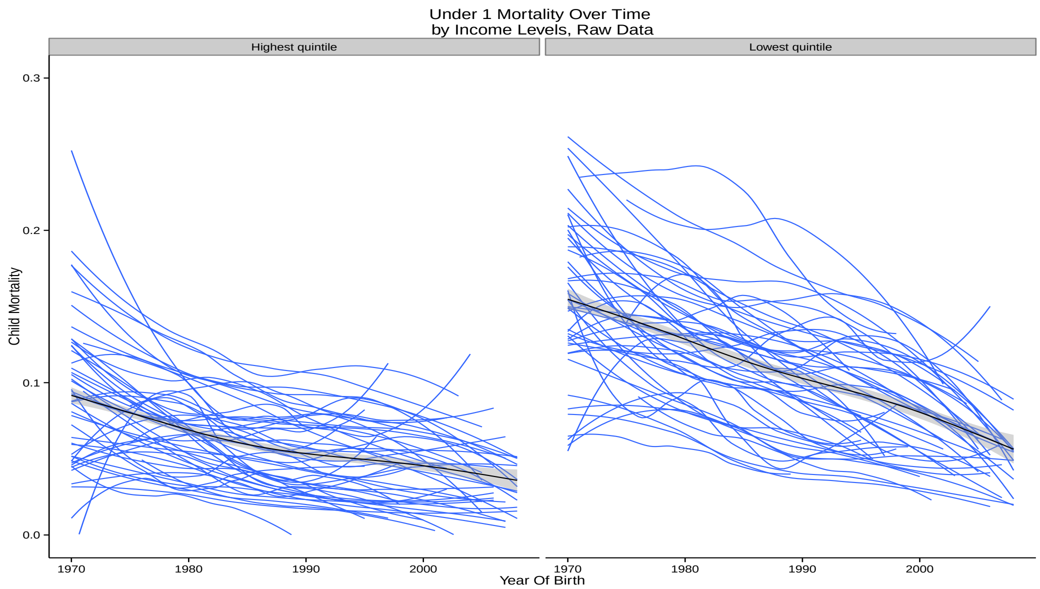

Figure 3.1: *Empirical distribution of Child Mortality Rates for rich and poor across all countries and years. Each line is a simple GAM model in which the only predictor is the time trend. The black lines in the centers of each distribution are the overall averages time trends and the shaded areas are the confidences interval around them.*

death rates than the rich. There is also more variance among the poor across countries, even though infant deaths among the poor is also falling over time. Careful investigation of this overtime trends via statistical modeling offer us the opportunity to disentangle long term over time trends from changes induced by political factors.

3.4.2 Covariates

The covariates are grouped in from 3 levels: (1) child, (2) mother/household and (3) country. At the child level, I have included the basic demographic variables: gender, birth order, year of birth and the age of the mother at birth. At the mother level, I have included their highest level of education and household income. At the country level, I have included time and income. These are well-known predicators of child mortality. All models include covariates that are standard in the health literature.

3.5 Methods

Before the formal presentation of the statistical machinery I will discuss the goal, objectives and limitations of the statistical analysis on this study. Given available data, the challenge is to find out a research design that will reveal the causal effect of democracy on child mortality gap between rich and poor. Following that, I will discuss the statistical tools available.

3.5.1 Goals and Limitations of the Statistical Analysis

The causal effect of a treatment on a unit can be simply defined as the difference in an outcome between two conditions — with and without the treatment. The fundamental problem of causal inference, however, is that a unit cannot be observed both with and without the treatment (Holland, 1986). Suppose that a democratization episode can be considered a treatment. Thus at any given point in time, a country, say Brazil, is either democratic or not, but never both. Thus, we cannot observe the child mortality rates for Brazil under both conditions, democracy and dictatorships, simultaneously. This would be the causal effect of democracy on child mortality. In some situations, however, the same country can be observed at different treatment states but *at different point in time*. If time had no effect, one could use this information to calculate causal effects of interest as the difference in the outcome between the treatment time and the control time.

Yet, in this study, time clearly has an effect. Not only have mortality rates declined over time, but the number of democracies has increased. Brazil in the 1970s was authoritarian and plagued by high levels of child mortality. By the late 1990s, it was a working democracy with much better health outcomes.Yet, it would be naive simply attribute that change to democracy alone. In fact, something else altogether may have caused both phenomena in Brazil. For example, suppose that modernization theory (Lipset, 1959) is correct in that lower child mortality and democracy are functions of modernization of the society. Or suppose that some unobservable factor, not democracy, causes reduction in child mortality. In fact, many countries reduce child mortality under dictatorships, most notably perhaps China, which reduced it by a factor of three in a few decades (Caldwell, 1986). If we are able to assume that infant mortality evolves in a predictable way, then it is possible to use the longitudinal structure of the data to estimate what would have been in Brazil in the late 1990s without democracy. To do so, we need to have enough information from the pre-democratization time trends so that we can extrapolate them into the future and then ask the question: what would Brazil be like in the absence of democracy? Comparing counterfactual scenarios with actual scenarios should give an estimate of the causal effect of interest.

While this approach does help with the non-random selection nature of the "treatment", the democratization episodes, it does not help with whether the timing of the treatment is endogenous. For example, suppose something else such as income or maternal education is causing both child mortality reduction and democratization. As modernization theory suggests, democracy might very well be endogenous to countries' mechanism of child mortality reduction (**?**). And we know that maternal education is one of the strongest predicators of child mortality (Gakidou et al., 2010a). One way to tackle this problem is to control for the demographic covariates that were suggested to be causing both (Kudamatsu, 2012). This strategy will help to account for societal demographic changes that are associated with both child mortality reduction and democratization. By focusing on time trends within demographic groups within countries, I account for many unobserved characteristics that not only make countries different from each other but, even more importantly, make people across income levels different from each other. All these unobserved characteristics are absorbed by the time trends across demographics within countries.

Figure 3.2 illustrates the issue. The goal is to estimate the degree in which the democratization episodes shifts previous trends in child mortality inequality. This strategy is related to interrupted time series models, which have extensive use in social sciences (Mogan and Winshop, 2007), (Gelman and Hill, 2006). It is also related to the more recent approaches of synthetic case control studies (Abadie et al., 2010),(Abadie and Hainmueller, 2012).

The primary weakness of this approach is that previous time trends might not be good predictors of future time trends. There are a few ways to address

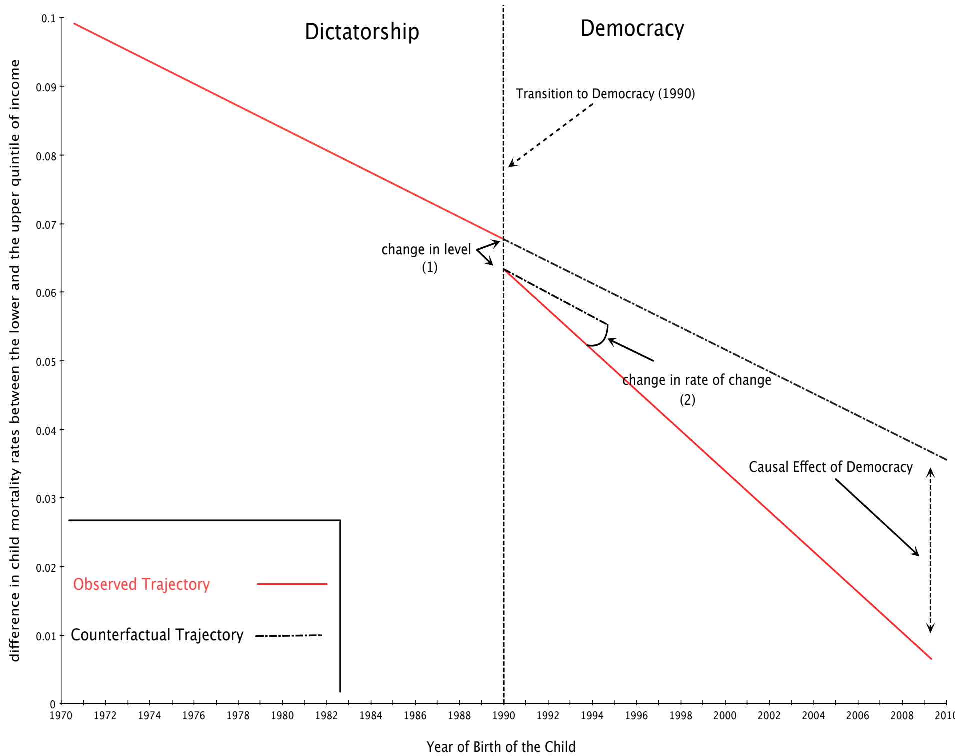

Figure 3.2: *Hypothetical scenario describing the effect of democratization on inequality in child mortality. Solid red lines represent the observed inequality trajectory before and after the democratization. The dotted line represents an unobserved, counterfactual trajectory of what would have been inequality trajectory in the absense of democratization. The vertical dotted line represents the year of transition (1990). Two types of changes introduced by democratization on inequality trajectory are illustrated: (1) changes in inequality levels and (2) changes in the rates of change over time. My statistical model is designed to capture both types of changes.*

that. First, I am using several covariates that may impact time trends. Secondly, I experiment with different time trend extrapolations and allow different time trends for each demographic group within countries. This is a quite a flexible approach. Finally, I use several countries in the analysis simultaneously.

This approach does not use information from countries that never experienced democratic transitions. However, I am keeping these non-transition countries so that I can compare countries which made the transition with countries that never did it. Similarly, I can compare countries that have always been democracies to those which always have been dictatorships to analyze whether this affects trends and levels of inequality over time.

Thus, my goal is to measures over time trends, investigating whether democratizations have affected them. To do so, I focus on two major approaches: The first is to check whether countries' trends in the rich and poor gap are related to regime type. For example, are transition countries reducing the gap faster than dictatorships? Does the number of transitions in a country affects time trends? The second is related to the introduction of democracy in previously authoritarian places. Does democratization changes previous, pre-democratization levels and rates of changes?

I propose to answer the following questions:

- Are baseline levels in child mortality driven by regime type?
- Are rates of change over time in child mortality driven by regime type?
- Does democratization change levels in child mortality?
- Does democratization change the previous rate of change over time in child mortality?

That said, I am far from an experimental situation and therefore causal inference is always problematic. Stated simply, this project aims at prediction and inference but with an eye on the underlying causal scientific question of interest.

3.5.2 Measuring the Rich and Poor Gap in Child Mortality

As discussed, health disparities varies widely across subpopulations within the same country. Race, ethnicity and income levels are only some of the possible grouping variables. Here I focus on the inequalities that reflects theoretical expectations from the political economy theories. These are inequalities between income levels, specially the rich and poor gap. One approach is to define inequality as the *ratio* between death probabilities from rich and poor children: how more likely to premature death are poor children compared to rich ones? Yet, ratios can become unstable when the rich children approaches zero probability of death. A simple alternative is to calculate the predicted difference between rich and poor. This is a simple contrast from regression equations. Thus I am defining inequality here as the rich-poor gap in predicted mortality rates, controlling for standard demographic variables:

$$\text{INEQUALITY} = \widehat{\text{CH}}_{poor} - \widehat{\text{CH}}_{rich}$$

3.5.3 Random Effects, Fixed Effects and Clustered Data

The response variable is a binary outcome: whether a child born in a particular country and year, with certain characteristics (mothers's age, sex, place of residence, etc) lives to the age of one or not [15]. The source of political variation, democratization episodes, takes place at the country level. The data exhibit complex clustered structure and a longitudinal profile. For example, children born from the same mother, in the same countries and in the same years may have correlated risk of death. Years are also correlated in the sense that the probability of death in any given year is in general more similar to that of proximal years. It is important to account for this clustering for both statistical and substantive reasons. Not accounting for the clustering will produce incorrect standard errors and can lead to incorrect statistical inferences and scientific conclusions.

3.5.3.1 Country Level Clustering

The data are clustered at the country level and by year, with at least several thousand of observations in each cluster. Because of clustered nature of the data, a simple approach would be to fit a full random coefficients' model using data from all countries (Shor et al., 2007),(Gelman et al., 2007),(Park et al., 2004),(Pang, 2010a),(Pang, 2010b),(Park, 2012),(Western and Jackman, 1994),(Western, 1998),(Beck and Katz, 2007) [16]. Random Effects Models display superior statistical properties, such as smaller mean square error than alternative approaches (Robinson, 1991),(Bates, 2010),(Shor et al., 2007). These models can be easily extended for the case of generalized linear models, such as logistic and probit regression for binary outcomes. This allows us to model the heterogeneity

[15] I focus on mortality under 1 (Neonatal and Posneonatal) because it reduces the censoring regarding the children that did note have the chance to die, and thus increases sample size.

[16] See also Autumn 2005 edition of *Political Analysis* devoted to the analysis of multilevel data set.

across countries. Yet, given the size of the data set, it is not computationally feasible to fit a full random effects model. An alternative approach is to run separate regressions for each country and then to combine the results using meta-analysis.

3.5.3.2 Within-Country Clustering

In addition to the between country clustering, there is within-country clustering. For example, there are clusters for children born to the same mothers or from the same village or state. In previous research, some attention has been paid to the within-mother clustering. Some of the literature in social and health sciences that has worked with this data suggests controlling for "mothers unobserved effects". The flavor of the control strategy varies: "fixed effects" in development economics (Kudamatsu, 2012) or random effects in health sciences (Gakidou and King, 2002) (Baird et al. (2011) also uses DHS data but without "mothers unobserved effects"). I formally test for whether "mother effects" improves model' fit. For a subset of countries in which the number of children per mother was higher than total sample averages, I fit models with and without mother effects, comparing models' fit using several statistics (AIC, BIC, deviance, etc). The results do not show any significant improvements by modeling mother effects (they are available upon request). Given the computation complexity of adding mothers effects in the context of a logistic regression, I do not include these effects here [17].

[17] The lack of improvement after accounting for mothers effects actually makes sense. First, most mothers in the data set have only one child. The median number of children per mother in my sample is 3, but it varies from only 2 to up to 6 for very few countries. This is already very low figure to estimate mother effects but when one investigates how many infant deaths each mother experienced the figures are even lower: 76 % of the mother experienced no death

3.5.3.3 Modelling Time Trends

Modeling time trends in the decline of child mortality for children born from mothers at different income levels is the key component of my analysis. Though there are many observations, the outcome is binary and therefore each observation does not contain a great deal of information about the underlying individual probabilities of death. I calculate $5 * 50 = 250$ time trends, one for each quintile of income for each country. This is especially challenging for countries with large variability over time. Moreover, for the transitional countries, I decompose the trends after and before the transition in order to investigate whether a democratic transition changes previous trends.

Increasingly complex time trends such as higher order polynomials and B-splines would be able to capture more details in the time dependent changes. Yet, these models are harder to estimate, and they suffer from higher risk of capturing sampling variability as opposed to actual changes in the true underlying population. These models are also more difficult to summarize across countries and to feed their results into the meta-analysis. On the other hand, simple time trends such as a low order polynomials are easier to summarize and interpret.

---

of their children, 15 % one death and only the remaining more than one death. Furthermore, mother effects would be unlikely to be useful in a longitudinal context, even if enough data was available. The age of the mother at birth is one of the most important predictors of the child probability of survival. In fact, mothers' abilities to give birth to a health child varies widely over their age. Thus even if enough children were available per mother, we will only be able to estimate some type of time invariance unobserved characteristic of the mothers, which likely would not inform us much about latent factors related to their fertility. Finally, and perhaps most importantly in the context of this study, the inclusion of mothers' effects will reduce my ability to use covariates at the mother level, such as income and education, which are key for the scientific question here addressed. These is so because these variables are strongly correlated with mothers' effects.

They also allow for easy decomposition of time trends before and after the democratic transitions and can also calculate overall time trends over the entire period more efficiently.

I estimate the basic specification using linear time trends at each income level from each country (see details below). This is quite a flexible approach already. However, I will also use Generalized Additive Models (GAM) to check the robustness of my findings to deviations from linearity.

3.5.3.4 Country Level Logistic Regressions

For each country, I fit a logistic regression with linear time trends

$$
\begin{aligned}
Pr(y_i = 1) = \text{logit}^{-1}(\mathbf{X_i}\beta)\\
&= \beta_1 \text{wealth} * (\beta_2 \text{time} + \beta_3 \text{new.time} + \beta_4 \text{baseline} + \beta_5 \text{new.baseline})\\
&+ \beta_6 \text{maternal.education} + \beta_7 \text{household.income} +\\
&+ \beta_8 \text{country.income} + \beta_9 \text{new.time.genocide} + \beta_{10} \text{new.intercept.genocide}\\
&+ \beta_{11} \text{residence} + \beta_{12} \text{gender} + \beta_{13} \text{birth.order}\\
&+ \beta_{14} \text{age.mother.at.birth} + \beta_{15} \text{age.mother.at.birth}^2
\end{aligned}
$$

For transitional countries, time trends in child mortality before the democratic transition is given by β_2 and, after the transition, by $\beta_2 + \beta_3$; for non-transition countries time trends is given by β_2. Similarly, for transition countries, the baseline level of child mortality before the transition is given by β_4 and after is given by $\beta_4 + \beta_5$; for non-transition countries, the baseline level of child mortality is given by β_4. Thus they key coefficients are β_3 and β_5 because they capture possible changes introduced by democratization episodes: β_3 captures change in levels of child mortality (change (1) in figure 3.2) while β_5 captures changes in the over time rates of change introduced by democratization change (2) in figure 3.2). For countries with many democratic episodes, β_3 and β_5 captures the averages changes introduced by democratization. All key coefficients interact with the wealth so that I can estimate possible changes in levels and rates of changes of child mortality at each income level. I am especially interested in how the differences between the rich (upper quintile of income) and the poor (lower quintile of income) were affected by the democratization episodes. In addition to the classical demographic(household income, maternal education, gender, birth order, place of residence — urban or rural —, and age of the mother at birth and its squared term) and country level (income) predictors, I have also include two variables to captures abrupt changes in levels and rates of change over time in child mortality introduced by genocide episodes (Rwanda, Cambodia, Armenia). In this model, time trends are assumed to be linear, as previously discussed. However, while it is linear in the logit scale, these variables are not linear on the probability scale, which adds additional flexibility to the model but it also makes the results more difficult to interpret. The variables are centered so that they have an easier interpretation. This model has the advantage of being easily incorporated into a meta-analysis.

3.5.3.5 Generalized Adddtive Models

As noted above, more complex alternatives to the linear time trends models include B-Splines and higher order polynomials. These models have their own challengers, such as model selection for the optimal polynomial degree or choosing where to place the knots for the splines. A more systematic approach is fitting a Generalize Additive Model (GAM) to over time trends by income levels. GAMs are a generalization of Generalized Linear Models, such as Logistic regressions, where the functional form of some or all covariates are estimated from the data, non-parametrically (Beck and Jackman, 1998). These models use robust statistical procedures to estimate the exact functional form of the time trends at each income level from the data. Thus, instead of considering several different possibilities for, say, the basis function for the B-spline or the polynomial order, comparing the fits each time, we can fit a GAM with the smoother over time trends by income. Though not widely known in Political Science research, GAMs are routinely used in many scientific fields exactly to investigate the miss-specification in parametric forms, such as the linear time trends models [18]. GAMs include GLMs as special cases when linearity at the level of the predictors is assumed. If we want to test whether a GLM is well-specified, we can do so by comparing it to a GAM. This is especially useful in my case where we want to check the robustness of the linear-time trends to different functional forms. Define $\mathbf{X}\beta$ as the matrix with all other covariates from the previous equation, including the intercept but excluding time trends. Instead of assuming that the time trends follow a particular polynomial, I use a smoother over these trends, which allows their functional form to be estimated from the data. I have also interacted these smother over time with the household wealth indicator, which

[18] Recall that in this study GAMs are also use to investigate the exact functional form of the effect of the age of the mother on mortality rates over time, due to the censoring of that variable.

allows different time trends by different income levels to follow different non-linear trajectories [19] [20]. Figure 3.10 illustrate the bent line approach using GAM models.

The biggest drawback of using GAM is that different countries have will have different sets of parameters summarizing their over time changes at each income level. Thus, one can no longer easily feed an exact set of coefficients into a meta-analysis and get an overall result. Still we can: 1) conduct statistical tests to compare overall fits across GAM and GLM; 2) get prediction from these GAM models, comparing them against those from the GLS; 3) include linear time trends for the bent line while keeping the GAM smoother for the overall time trends[21].

3.5.4 Using Contrasts To Estimate the Poor-Rich Gap

Once we fit a Logistic Regression or a GAM model to the data, we need to extract the quantities of interest to feed into the meta-analysis. These quantities are *contrasts*, which are differences in factor level means from the estimated logistic regression models. The contrasts I am particularly interested in are the

[19]Smooth terms are represented using penalized regression splines (or similar smoothers) with smoothing parameters selected, in my case by GCV/UBRE/AIC/REML.

[20]gam in R package mgcv solves the smoothing parameter estimation problem by using the Generalized Cross Validation (GCV) or an Un-Biased Risk Estimator (UBRE)criterion. Please see the manual the R package for details.

[21]A still more flexible approach would be to use fixed effects for each years in every country - i.e. unstructured dummies' indicators for each year in every country logistic regression. I have experimented with this approach as well. While in expectation it would provide unbiased estimates of the changes in child mortality at every single year in every country for each income level it does not work in practice. Instead it produces estimates with huge standard errors and mean values that are inconsistent with the raw data, the other regression estimates, and even with the common sense, such as that the death rates being higher for rich than for *poor most of the time*. I would almost certainly erase any effect that democracy might have in child mortality, if any. Therefore I abandoned it, though a few country examples are available upon request.

differences between the rich and the poor across countries, as well as their associated measures of uncertainty. A simple example helps to illustrate the issue. Suppose, children are either from rich or poor mothers, who either have primary or higher education. Further, suppose that $\mathbf{X}$ is a vector of covariates that we want to hold constant, such as the sex of the children, birth order and place of residence of the mother. Let $\widehat{\text{Rich}}$ be the estimate baseline (at the beginning of the study) probability of death for the children from a rich mother with higher education while $\widehat{\text{Poor}}$ is the probability of death from a birth from a low income mother with only primary education. Using these facts we can estimate Δ as the difference between the probability of deaths as a linear contrast (in the logit scale)

$$\begin{aligned}
\widehat{\text{Poor}} =& \hat{\alpha} + (\hat{\beta}_1 * \text{poor}) * 1 + (\hat{\beta}_2 * \text{primary}) * 1 + \mathbf{X}\hat{\beta} \\
\widehat{\text{Rich}} =& \hat{\alpha} + (\hat{\beta}_1 * \text{poor}) * 0 + (\hat{\beta}_2 * \text{primary}) * 0 + \mathbf{X}\hat{\beta} \\
\Delta =& \widehat{\text{Poor}} - \widehat{\text{Rich}} = \hat{\beta}_1 * \text{poor} + \hat{\beta}_2 * \text{primary}
\end{aligned}$$

The standard deviation of these contrasts can be easily calculated using the formula of the variance of two correlated random variables

$$Var(\Delta) = Var(\hat{\beta}_1) + Var(\hat{\beta}_2) - 2\text{Cov}(\hat{\beta}_1, \hat{\beta}_2)$$

These quantities are available in the variance-covariance matrices of the fitted logit or gam models.

3.5.5 Combining Information from Contrasts from the Country-by-Country Regressions using Meta Analysis

Suppose we have fitted the country-by-country regressions and calculated the desired contrasts. How do we go about estimating the effect jointly for all countries? Meta-analyses are commonly used in health and statistical sciences when the goal is to combine information from several studies with similar targets. The simpler version of such a procedure is the fixed effects meta-analysis. Let $i = 1, \ldots, k$ independent effects size estimates, each corresponding to a true effect size, from example a contrast between rich and poor at the baseline for each i country, Δ_i. We shall assume that

$$y_i = \Delta_i + \varepsilon_i$$

where y_i is the observed level effect from i-th study independent effects size estimates, corresponding the the true effect and $\epsilon_i \sim N(0, \nu_i)$. The y_i's are the unbiased and normally distributed estimates of the true effects, Δ_i. The sampling variance is also assumed to be known and in my case is simply the estimated standard error of the contrasts, Δ_i.

The random effects models for meta-analysis builds upon these simpler fixed effect formulation by allowing for the possibility of variability among the true effects. This is especially useful here, where there are remarkable difference in the sample characteristics across countries Thus we have

$$\Delta_i = \mu + v_i$$

where $\upsilon_i \sim N(0, \tau^2)$. Hence the true effects are assumed to be normally distributed with mean μ and variance τ^2. Here the goal is to estimate μ, the average true effect and τ^2, the total heterogeneity of the true effects. If $\tau^2 = 0$, implies homogeneity. Mixed effects meta-analytic models adds further modeling flexibility, by letting us investigate the sources of heterogeneity across the true effects with one or more moderators. They are very similar to mixed effects regression models

$$\theta_i = \beta_0 + \beta_1 x_{i1} + \cdots + \beta_p x_{ip} + \upsilon_i$$

where β_{ip} is the value of the j-th moderator variable for the i-th study. Again we assume that $\upsilon_i \sim N(0, \tau^2)$ but now τ^2 is the amount of residual heterogeneity in the true effects not accounted by the moderators. In this study, moderators are simple country levels variables such as the income level of the baseline, political regime type (transition, democracy or dictatorships) or the number of democratic transitions the country has experienced.

In the case of homogeneity among the true effects, the distinction among all these methods disappears as $\mu = \bar{\theta}_w = \bar{\theta}_\upsilon \equiv \theta$. I will present results from the random effects models, which have advantages. Results are also robust to that choice. Various measures have been proposed to interpret τ^2. The I^2 statistics is in percentage scale — how much of the total variability in the effects size estimates is due to heterogeneity among the true effects as oppose to sample variability($\tau^2 = 0$ implies $I^2 = 0\%$).

The fixed effects meta-analysis provides information about *conditional inference*: What is the size of the true effects among the set of k studies included in the sample. On the other hand, the random/mixed effects models provide *unconditional inferences* about a set of larger studies in which the k included studies is considered to be a random sample. The later can answer questions such as how large is the true effect is among the larger population, middle and low income countries.

3.6 Results

I present the results of the analysis in several steps. At the core of the analysis is the logit regression model described above, which poses interpretation challenges. Meta-analysis and associated statistical inference will be conducted in the log-odds metrics but, whenever possible, I will illustrate the effects size in the probability scale.v First, I provide a sense of how well the model fits the data. Second, I will discuss the baseline difference and overall time trends for all 50 countries. I will presents results from a mixed effects meta-analysis to investigate whether these results can be explained by political factors. Then I will turn to the analysis of the 22 transition countries. I will discuses the results from the bent line approach to investigate whether the introduction of democracy changed previous levels and trends in inequality. Finally, I will illustrate the counterfactual scenarios in the probability scale.

3.6.1 Basic Models Fit: Comparing GAM and GLM

Both the GLS and the GAM models fit the data well. The provide predictions that resemble important features of the raw data (more on that below). Confi-

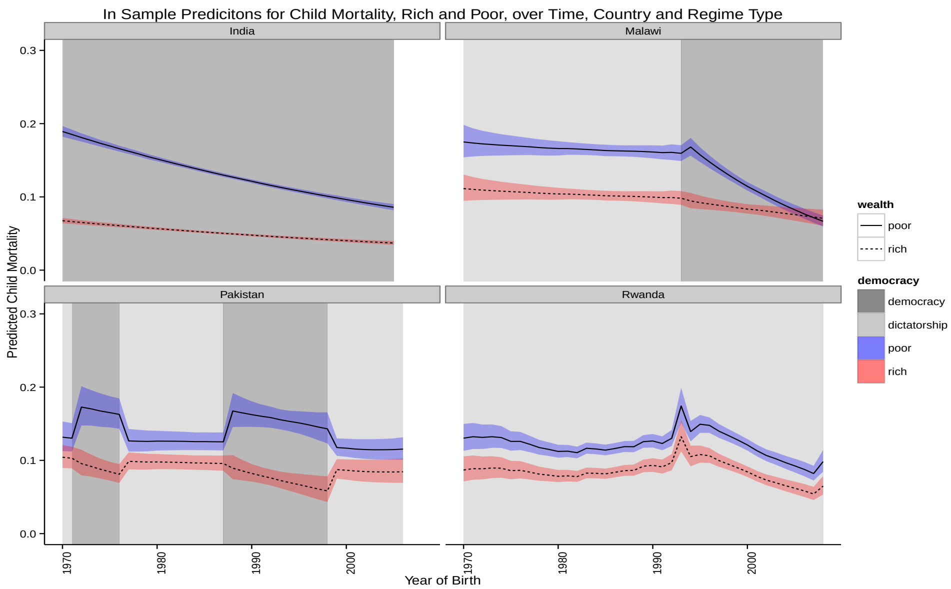

Figure 3.3: *Predictions from the linear time trends models in detail for 4 types of countries. Always democracies, India; Always Dictatorships, Rwanda; One time transitions, Malawi; and, finally, multiple transitions countries, Pakistan. The Dark grey represent dictatorial periods, while light gray democracies. Dotted lines with read shades, are conditional mortality rates for the poor, while solid lines for blue shade are for the poor. The shades are 95 confidence intervals.*

dence intervals are small enough so that in most cases the difference between the poor and the rich are statistically significant throught the analysis.

Figure 3.3 presents predictions from the linear time trend models for the four basic (political) type of countries: always democratic, such as India; always dictatorships, such as Rwanda — which was also affected by a genocide episode; countries which endure one democratic transition, such as Malawi; and finally countries that experience many democratic transitions, such as Pakistan. Linear time trends models are able to capture several important features of the data, such as the genocide episode in Rwanda. Some patterns are visually interesting, such as in Pakistan. For this country each time that democracy was introduced, child mortality increased for the poor, thus widening the rich-poor gap. Figure 3.9, in the appendix, shows the results of the predictions for all countries using the linear time trends models.

One may wonder whether these estimates are not artifacts of the models. To check for that I fit GAM's where information about political episodes were not included. The predictions are remarkably similar to the logistic regressions with linear time trends, however. This provides confidence that these patters actually exist in the data. For example, the gap introduced by democratization in Pakistan or the genocide episode in Rwanda (without change in regime type) are both captured by the GAM models. For some countries like Indonesia and Guatemala, it seems that linear time trends actually represent a better fit. Detailed results are available upon request [22].

[22] To formally compare the likelihood of all models from the GAM fit against all those from the GLM fit I have used the following χ test

$$\begin{aligned} D =& -2 * \sum_{i=1}^{50} \ell_{\text{gam}} + 2 * \sum_{i=1}^{50} \ell_{\text{gam}} \\ D \sim& \chi_{df} \\ df =& \sum df_{gam} - \sum df_{glm} \end{aligned}$$

The statistical test indicates a better fit for the GAM, as one would expected. Yet, linear time trends allow us to decompose the trends in a more amenable manner to capture our scientific question of interest while producing overall similar results. Thus the point is that these models can reproduce important feature of the data and therefore should be able to capture discontinuities introduced by the political process.

3.6.2 Baseline Differences

Figure 3.4 displays the contrast between the rich and the poor across countries at the baseline year for each one of the 50 country studies. Detailed numerical summaries in Table 3.1 in the appendix[23]. As we can see in Figure 3.4, and except for a few cases, most countries exhibit a gap in child mortality for the rich and poor. The estimated difference (in log-odds scale) is 5.1 with $se = .04$, which is highly statistically significant($p_{\text{value}} < .0001$). The exceptions are Haiti, Chad, Nicaragua, Cambodia. Morocco, Viet Nam, and Armenia. Some countries, such as Kazakhstan, Comoros, Togo. Uzbekistan, and South Africa exhibit large disparities. Accordingly, a test for heterogeneity finds that it exists and it is highly statistically significant. The I^2 statistic indicates that 81% of the heterogeneity is due to the actual differences across countries' baseline conditions, not sampling variability. This makes sense based on the contrasts presented in the Figure 3.4.

In order to explore possible sources of heterogeneity across countries I fitted a mixed effects meta-analysis where I investigate the association between the baseline rich-poor gap and political factors — whether it is a transition country, a democratic country (for the entire period) or a dictatorship country (for the entire period). I have also controlled for per capita income at the baseline of the study. An alternative way to see what I am doing is to test whether controlling for income, these groups of countries display baseline differences in the inequality levels. Since none of variables explain countries' differences at the baseline, we know that baseline differences are not grouped by regime type.

[23] The baseline year is 1970 for all countries but Bangladesh (1971), Comoros (1975), and Vietnam (1976).

3.6.3 Overall Rate of Change

Now I turn to changes over time. The main points to be investigated are: (1) whether countries changed inequality levels over time; (2) the heterogeneity across these changes and (3); if (1) and (2) are linked to political factors.

Figure 3.5 (again, numerical details in the appendix, Table 3.2) displays the rate of change in the log odds scale for each one of these countries. The actual numeric summaries for all countries are also presented in the figure. For many, the gap is decreasing while there is no statistically significant change for some and, the gap is actually increasing for a few countries. Overall, the gap is decreasing. The meta analysis demonstrates that this decrease is statistically significant, $-.01$ log-odds for each additional yearly reduction in the gap between the rich and the poor, with $p_{value} = .0005$. Yet, the heterogeneity is very high: $I^2 = 80\%$ and statistically significant. It means that the variability in early reductions shown in figure 3.5 are real and not a product of sampling variability.

I also fit a mixed effects meta-analysis to understand the forces driving the differential rates of change in the rich and poor gap for these 50 countries. I explain the over time changes in inequality by countries' regime type, income level at the baseline, inequality in child mortality at the baseline and the number of transitions endure by the country. Again, the political factors don't seem to matter. Higher income at the baseline is associated with lower reduction in child mortality, but greater inequality at the baseline is associated with faster reductions.

3.6.4 Does Democratization Changed Previous Levels of Child Mortality?

Now we focus on the 22 transition countries and ask the question of whether democratic transitions changed previous level of inequality between rich and poor. The contrasts for each one of the 22 countries are displayed in Figure 3.6 as well as the overall effect. For almost all countries, the effects of democratization are not significant and nor is the main effect over all countries— 95% CI for the log-odds $(-.04, .08)$ includes zero. Corroborating the visual inspection in the plot, the heterogeneity is low, $I^2 < 1\%$. This means that democratic transitions did not impact previous inequality levels. The only exceptions are Brazil, where the transition did reduce inequality child mortality, and Pakistan, where the opposite happened.

3.6.5 Does Democratization Changed Previous Rate of Change in Inequality in Child Mortality?

Finally we ask: do democratic transitions accelerate the yearly rate of reduction in the child mortality gap? Figure 3.7 display the results of the meta-analysis. The answer is no, democratization do not have an accelerating effect. However, there is large heterogeneity across transitions — much more so than democracy's effects on level. On average, each additional year after the democratic transition further closes gap in child mortality following the trend that was already in place before democratic transitions by $-.01$ log-odds with 95 % confidence intervals of $(-.03, .01)$. This is not statistically different from pre-existing trends. Still, the statistic $I^2 = 72\%$ indicates that the effects are heterogeneous.

Since the main effects are not statistically significant, I do not fitting a mixed effects meta-analysis. Yet, we can still look at the graphs to investigate whether democratic transitions further accelerate the reduction in the child mortality gap. Most of the countries in which democratization increases the rate at which the rich-poor gap in infant mortality is being reduced are in Africa: Kenya, Ghana, Madagascar, Malawi but also Indonesia from South-East Asia. On the other hand, for some countries it seems that the democratization slowed down the previous rate of reduction or even increased the gap, even though the effects are not quite statistically significant.

3.6.6 Robustness Check: Relaxing the Linearity Assumption for Time Trends Using GAM for the Time Trends

Recall that in the main statistical model I have assumed that time trends are linear in the logit scale — β_2 is a polynomial of order 1. To test the robustness of my main findings to this assumption I use the aforementioned GAM. Here, the main time trends by income levels (which coefficients and its interaction is given by $\beta_1 \times \beta_2$ from the logistic regression model) are estimated using the GAM. Thus time trends by income levels are no longer linear and, instead, they can be described by quite complex non-linear patterns, if allowed by the data. In this context, the coefficient for the new time variable (β_3), which is still linear, represents linear deviation from the GAM (non-linear) trends, after the democratization episodes. The results are quite similar either in terms of the lack of significance for the effects of transitions and for the heterogeneity of these effects. Details are available upon request.

3.6.7 Illustration of the Heterogeneity of the Effects in The Probability Scale

While statistical inference on the logit scale are relatively is relatively straightforward, it is much harder to have a sense of the actual size effects and their heterogeneity. Thus I make counterfactual predictions for all transitions countries, country-by-country. These are the same models used in the meta-analysis but now I am using them to make *conditional* predictions over time. Specifically, I compare births from rich and poor mothers, holding constant gender of the child (female), place of residence (urban for rich and rural for poor), birth order (first birth) and the age of the mother at the birth of the child (18 years old). The education of the mother is a more complicated covariate to be kept constant. For example, for some Sub-Saharan countries, even rich mothers rarely have secondary education, let alone higher; for some former communist countries, even the poor have higher education. Also, while in some countries there are huge educational disparities across income levels. Thus "holding education constant" both across income levels within and across countries produces unrealistic estimates, outside the ranges of the data. A simple solution is use the typical (modal) value of the maternal education at each income level for each country. Thus I am letting education follow income, as the latter is the major focus of this study [24].

[24] An interesting complementary analysis, will be to let education be the main driver and let income follow it.

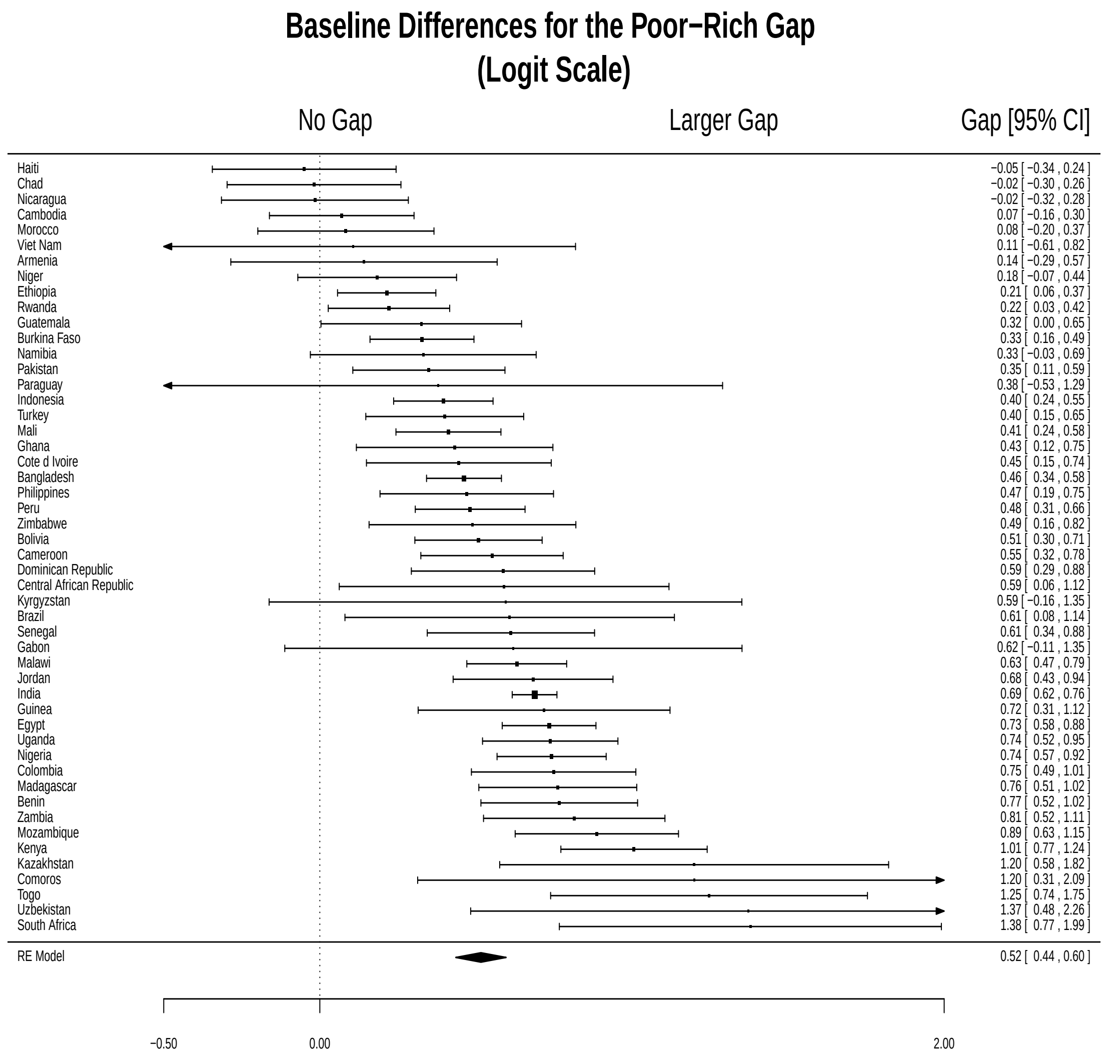

Figure 3.4: *Baseline levels of inequality in the rich and poor gap in child mortality as calculated from the linear time trends models. Each square represents a country study (total of 50 countries, country names on the left of the graph). The horizontal lines crossing each square represent 95 % confidence intervals for each study. The arrows indicate whether confidence intervals are larger than displayed in the graph. Confidence interval lines crossing the dotted vertical line indicates lack of statistically significance. Numerical results are available on the righ of the graph. The diamond at the bottom of the figure indicates the overal result of the meta-analysis.*

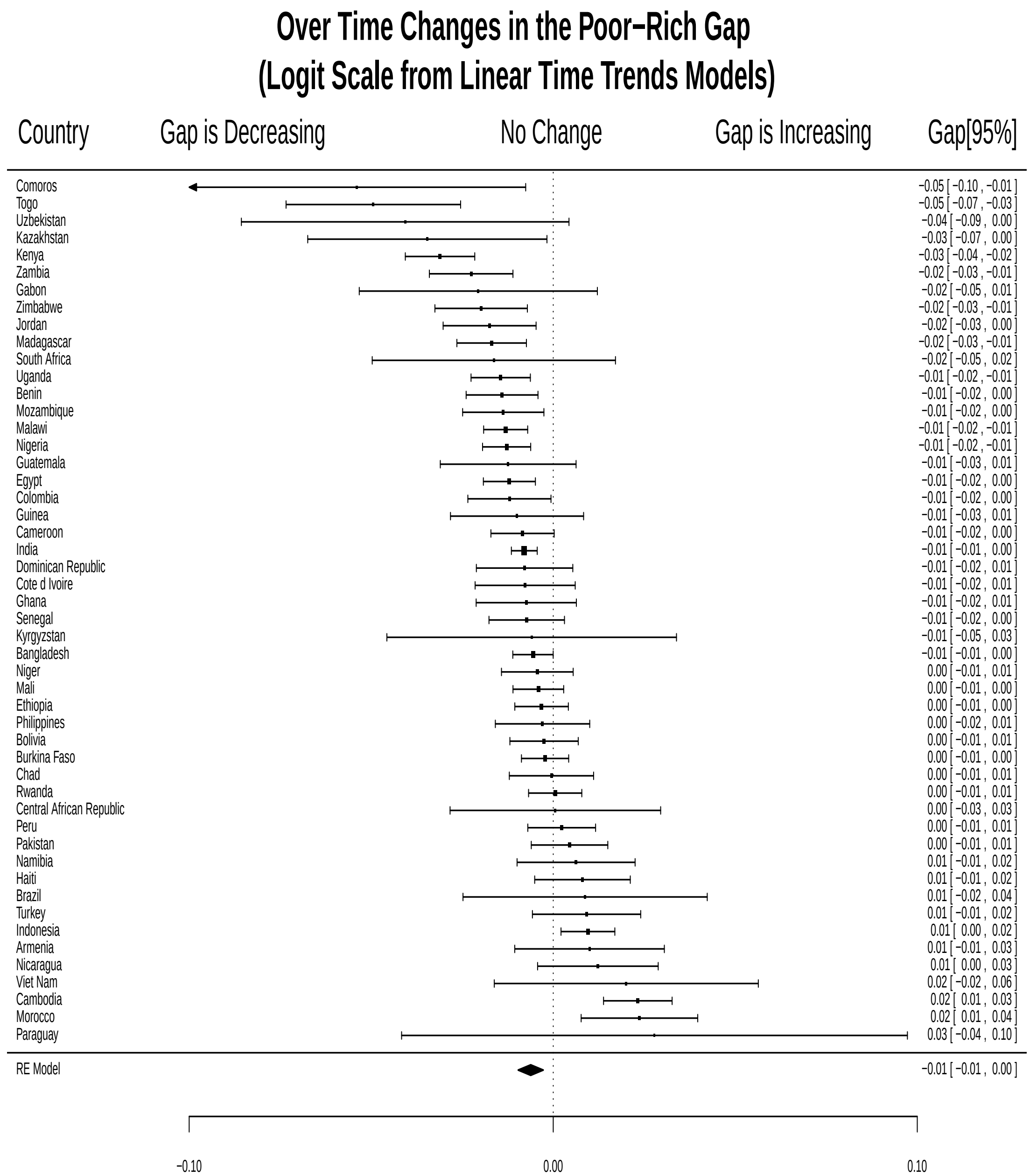

Figure 3.5: *Overall time trends for the rich-poor gap in child mortality. These contrasts were estimated using the linear time trend models.The diamond at the bottom of the figure indicates that the overal result of the meta-analysis is statistically significant.*

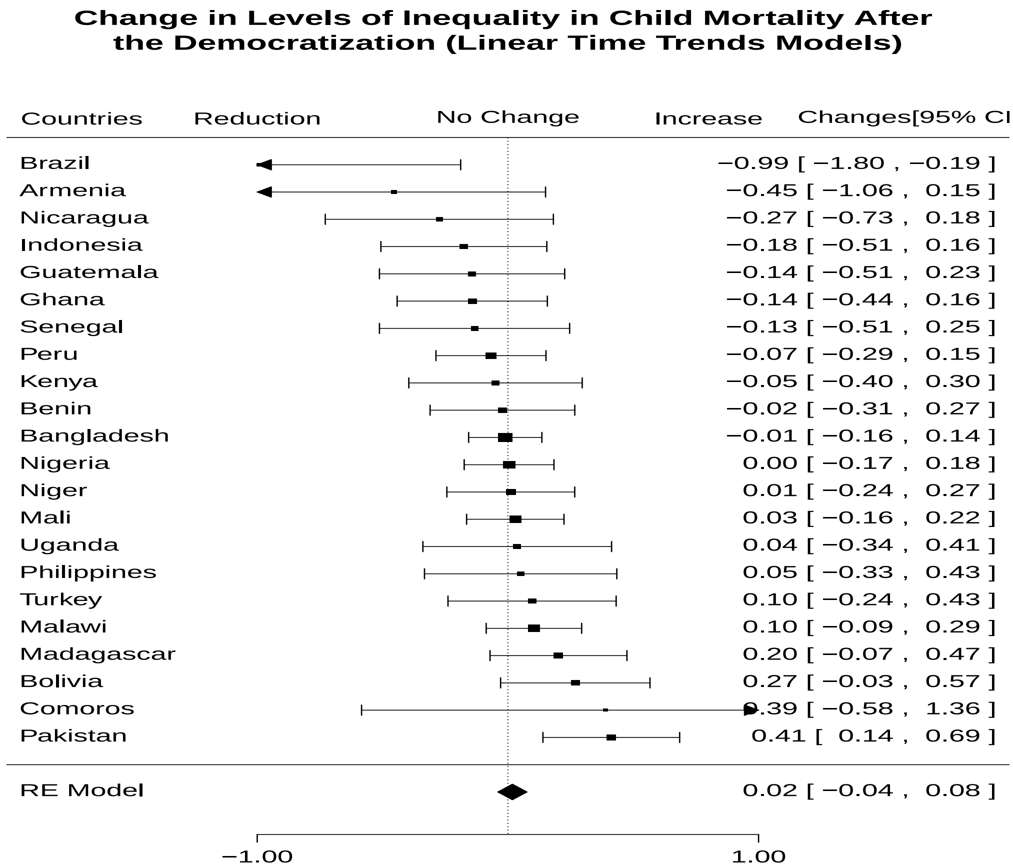

Figure 3.6: *Meta-analysis for the changes in the level of inequality in child mortality between births from rich and poor mothers after democratization episodes. These contrasts were estimated using the linear time trend models. The diamond at the bottom of the figure indicates that the overall result of the meta-analysis is not significant statistically.*

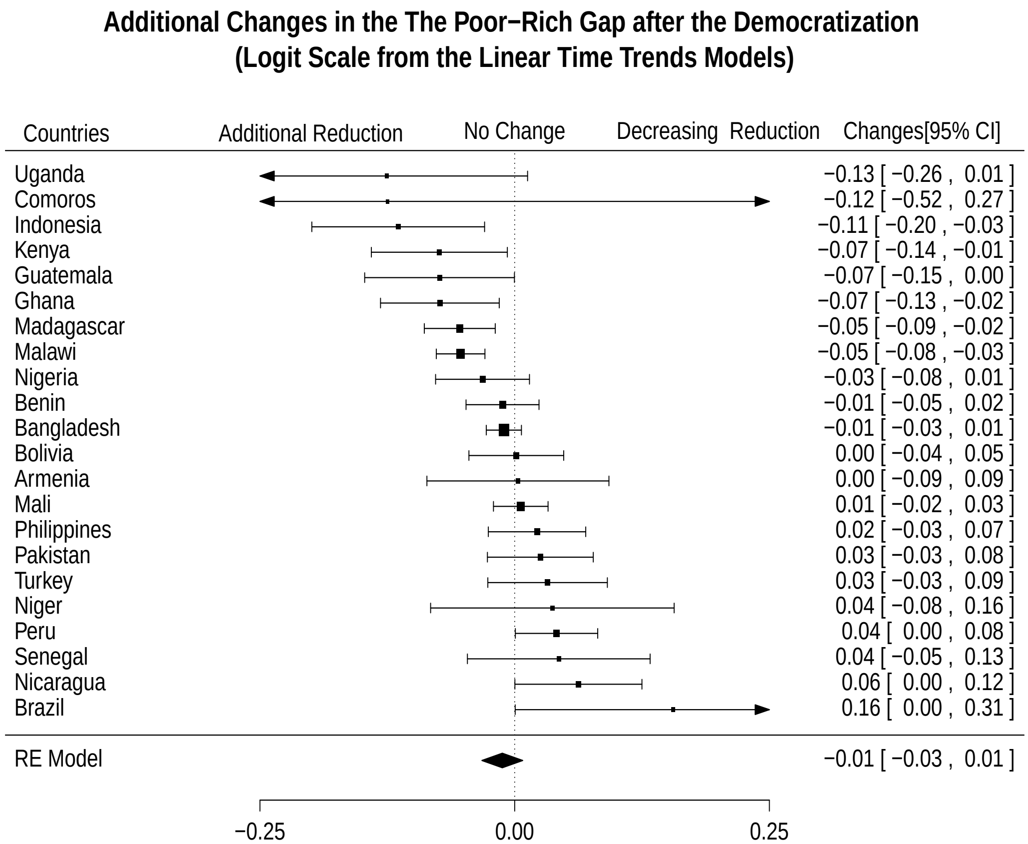

Figure 3.7: *Meta-analysis on the effects of the democratization on time trends for the rich-poor gap. These contrasts were estimate using the linear time trends models.*

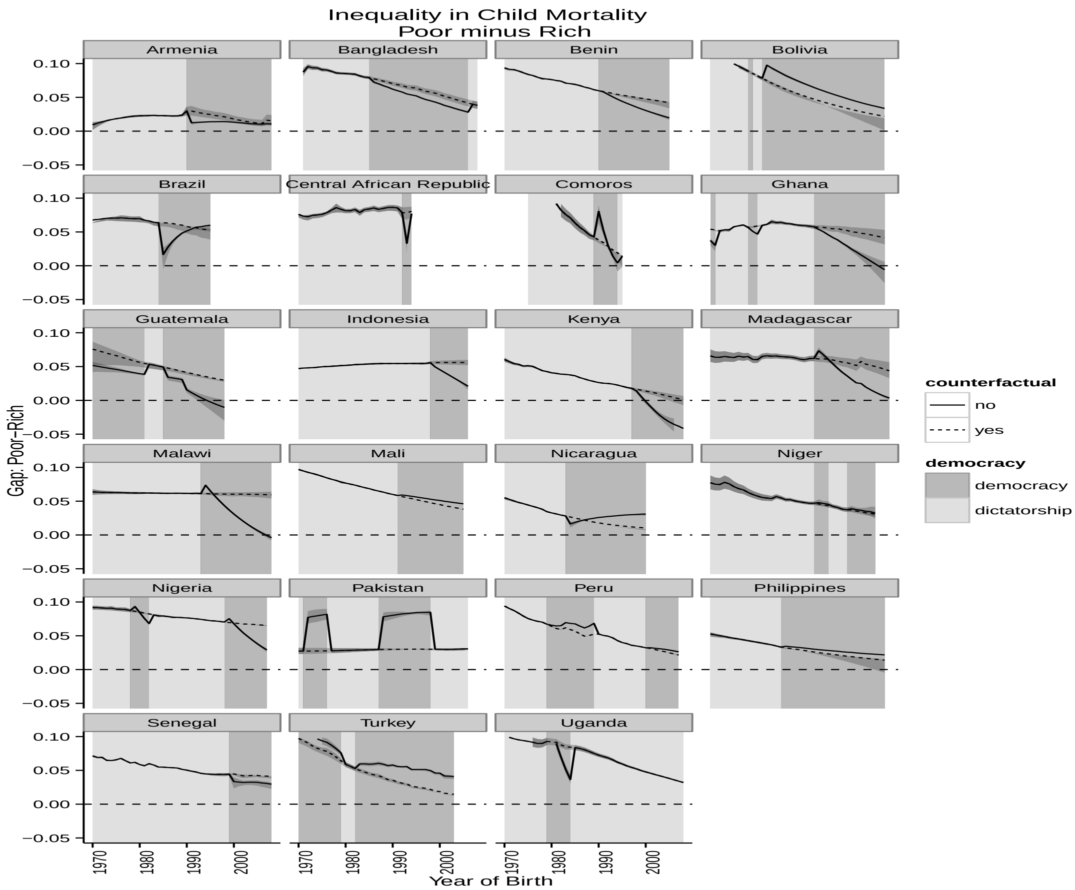

Figure 3.8: *An ilustration of the effects of democratic transitions on the rich and poor gap in child mortality and their heterogeneity. Only transition countries are shown. The light gray areas are dictatorial periods while the dark grays are democratic ones. The solid lines are the actual, in sample predictions in the gap for child mortality. The dotted lines are counterfactual scenarios where the bent lines were set to zero.*

Figure 3.8 illustrates the size and the heterogeneity of the effects of the transitions on the scale of the data - the probability scale. For each country, both counterfactual and actual predictions come from the same model. The difference between the actual and the counterfactual scenarios is that for the later I set the bent lines (the slopes shifts after democratization) and the intercepts shifts after democratization both to zero, as if democracy never happened. In the probability scale, both the size of the effects and the heterogeneity are clear. For some countries, such as Uganda, there is a big reduction on the level of child mortality after a short democratic period. Pakistan also has a huge increase in the inequality level every time a democratization happens, even though it does not affect its over time change. Many countries in Sub-Saharan Africa undergo fast declines in child mortality after the introduction of democracy, such as Ghana, Madagascar and Kenya.

3.6.7.1 Summary of the Findings

In brief, the main findings are as follows:

- Almost all countries exhibit a wide gap in child mortality rates between the rich and the poor quintiles of income. These are not only substantively but also statistically significant. At the baseline, the overall average difference is around 5% of excess of deaths for the poor in relation to the richest, though it can vary from almost zero to over 10% for some countries. These baseline differences are not explained by either per capita income or regime type.

- Most countries in the world are reducing their rich-poor gap in child mortality and the overall decrease is statistically significant. On average, the difference in mortality rates for the rich and the poor decreased from 5% to 2% between 1970 and 2005, though there is heterogeneity across countries. Higher income per capita at the baseline is associated with lower rate of reduction, but a higher initial gap is associated with faster reduction. Again regime type and other political factors don't seem to affect these trends.

- Democratization episodes did not change previous levels of inequality. This is uniformly true, with Pakistan and Brazil as the only exceptions.

- Overall democratization episodes don't seem to impact the previous trends in the reduction of child mortality. Yet, there is heterogeneity in these effects. Thus for some subsets of countries, such as few Sub-Saharan countries, it seems that democratic transitions did reduce the gap, however the opposite is true for countries such as Brazil and Pakistan (although not quite statistically significant at the conventional levels).

- All these results are robust to using either linear time trends or GAM's.

3.7 Discussion and Conclusions

The rich and poor gap in child mortality does exist around the developing world, even controlling for individual level demographic factors. These inequalities are decreasing over time. However, there is no evidence that either baseline differences or over time trends are systematically linked to political factors. I investigate the effects of the introduction of democracy on previous levels and rates of change in child mortality in transitional countries and find that neither the levels nor the previous rates of reduction in the rich and poor gap in child mortality are significantly affected by democratization episodes. While all of this points to an essentially null effect of democracy on health and equality, I do find substantial heterogeneity in these effects, beyond what one would expect based on sampling variability only. This is especially true for the democratization of previously authoritarian countries. For example, in countries such as Pakistan, democratic transitions were always associated with an increasing gap between rich and poor while the opposite is true for a most Sub-Saharan countries. This is an important unexplained finding that deserves further investigation.

In understanding these results, it is worth revisiting theoretical ideas from Ross (2006). As previously discussed, Ross (2006) provides a more subtle interpretation of the median voter theorem. He points to the fact that the median voter (likely around the median income) may have no more interest than the rich (top 20 %) do in providing policies that disproportionately benefit the poor (bottom 20%). Thus in seeking political support from a broader electorate, governments do not need to appeal so much to the poor but instead mostly to the middle class. Thus median voter theories imply some redistribution, but from the rich to the middle class, and not necessarily to the poor.

Yet, sometimes democracy does reduce the mortality gap between the rich and the poor, particularly in poor countries [25]. Child mortality is not entirely

[25] This finding is also corroborated by another study in which I have used more recent estimates of national averages of child mortality, with no missing data and less measurement error

concentrated among the poorest quintile within countries. For example, in some poor countries, child mortality maybe endemic across all income levels. In particular, it may very well affect the "middle class" in poor countries - and thus the median voter. This analysis suggest that (1) when the median voter is actually affected by child mortality and (2) there exist enough disparities in child mortality between the middle class and the rich, democratization might reduce child mortality gap between these groups. Further, if health care is provided as a public good, democracy may also reduce child mortality for the poor. On the other hand, if all income levels are severely affected by child mortality, democratization might reduce it across all levels without necessarily reducing gaps. As a next step, I will directly test these extensions.

It is worth emphasizing the median voter theorem is a very simple model of democratic politics and as such it might be lacking elements to explain politics in some places. As Nelson (2007) points out, there is both theoretical and empirical evidence that elections alone are not necessary to produce social desirable outcomes. Party ideology, electoral systems and the natural difficulties of translating to the mass public the need of large scale complex reforms may all conspire against successful transitions. For example, there exist evidence that the ideology of the government might help increase redistribution from the rich to the poor. Thus future research should also consider these possibilities, though they are often hard to test cross-nationally.

Another limitation of this study is that I am looking at conditional effects

than it was previously available.

of democracy upon child mortality — not its net (marginal) effects. To see the difference between the two consider the following: suppose democracy did not reduce child mortality for some high risk group, say, 18 year old low-income mothers with a low educational background. Still, democratization might have reduced the fraction of the population that belongs to this group, by increasing levels of education, increasing urbanization or the age of the mother at her first birth. Thus by holding constant a certain demographic profile I might be underestimating the effect of democracy on the child mortality gap. In fact, one might argue that democracy acts indirectly, thus changing the demographic profile of the country but not necessarily improving well-being within demographic groups. Though my exploratory analysis did not indicate any big net (marginal) effects of this nature, I am currently investigating a way to test for that possibility more systematically. Even if these effects are salient, it would be difficult to advance a causal interpretation for those, as reduction on these high risk groups themselves might help bring about democratization, as noted above.

Future studies could explore the huge heterogeneity across countries found here. It would be especially interesting to investigate in more detail the effects of political factors on sub-Saharan Africa child mortality and its inequities [26]. Another approach would be to focus on case studies where beneficial or deleterious effects of democratization were more pronounced. Some countries such as Brazil do have very detailed data on both child mortality and political variables (Md et al., 2011). From the health science point of view, it would be a welcomed

[26] Another study found beneficial effects of democracy on mean child mortality across countries in these region Kudamatsu (2012).

effort to include more countries in the analysis, using sources other than the DHS. Finally, it would be interesting to investigate other sources of inequalities beyond the rich and poor gap Gakidou and King (2002) and study whether these are linked to political factors.

Appendix

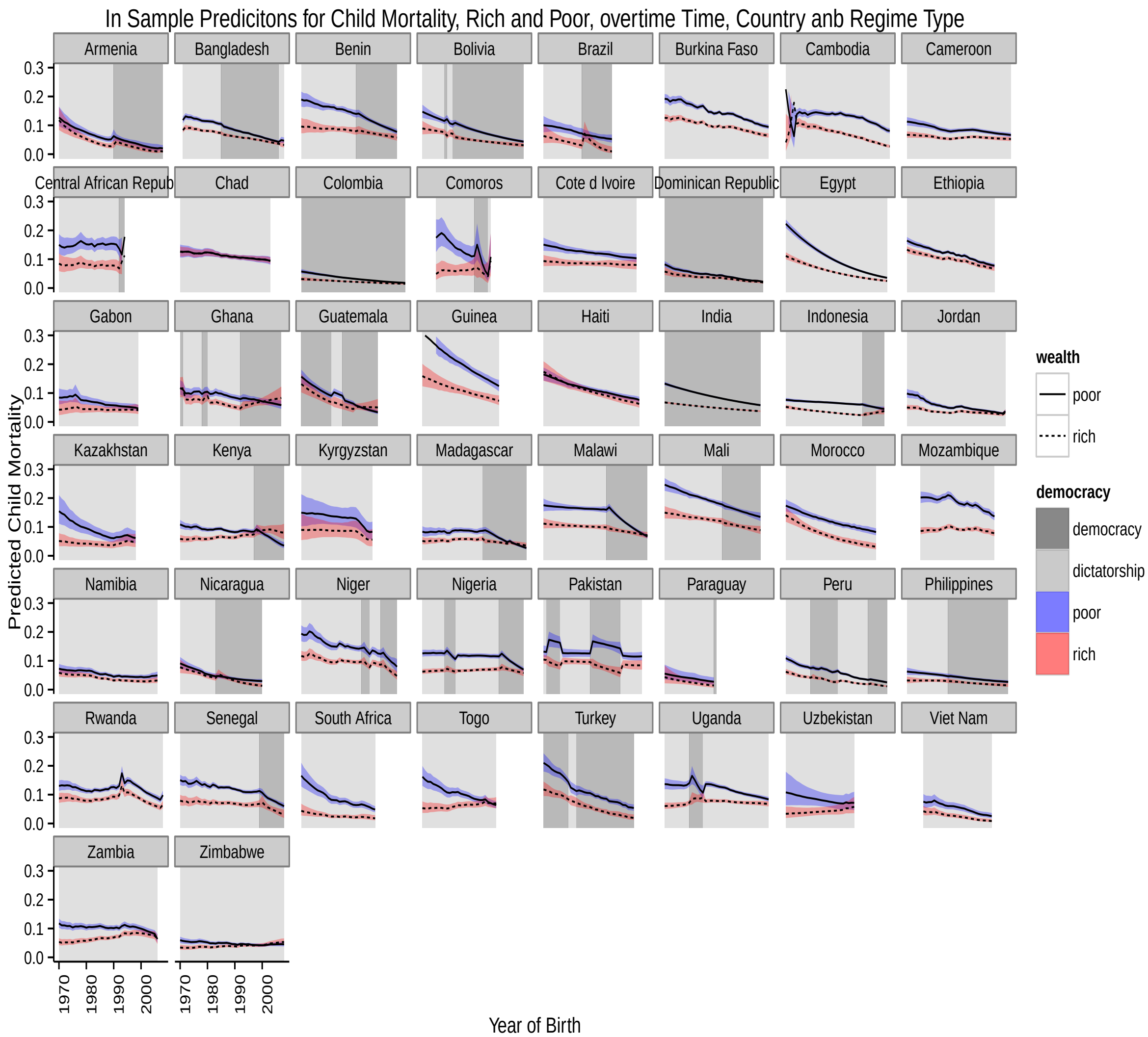

Figure 3.9: *Predictions from the linear time trends models. I compare mortality rates from births from rich and poor mothers, holding constant gender of the child (female), place of residence (urban for rich and rural for poor), birth order (first birth) and the age of the mother at the birth of the child (18 years old). Maternal education is fixed at its mode in each country, for each income level. Lines are points estimates and shades are 95 % confidence intervals. The solid, blue shaded lines are predictions for the poor and the dotted, red shaded lines are predictions for the rich. Darker shades of gray represent democratic periods and lighter shades dictatorships. Spikes in Armenia, Cambodia and Rwanda are genocide episodes.*

Baseline Differences in the Rich-Poor Gap in Child Mortality						
	Reduced			Full		
	Est.	SE	pval	Est.	SE	pval
Intercept	0.51	0.04	0.0001	0.29	0.43	49
Dictatorship				0.01	0.08	0.9
Democracy				0.15	0.15	0.3
Genocide				0.15	0.15	0.3
Income per capita				0.03	0.06	.61
N	50			50		
DF	2			6		
AIC	30.5046			32.8614		
BIC	34.3287			44.3336		
log-likelihood	-13.2523			-10.4307		
I^2(heterogeneity/sample variability)	81.19%			74.72 %		
Test for Heterogeneity	p-value=0.001			p-value=0.001		
Test for Moderators				0.1934		

Table 3.1: *Results from the Mixed Effects Meta-Analysis for the baseline differences in the rich-poor gap in child mortality. The outcome variable is in the log-odds scale and is a contrasts from the country-by-country logist regression models with linear time trends. Income per capita is in the log-scale. All 50 countries were included. The reduced model include no moderators (covariates) to account for the baseline differences. The log-likelihood ratio test is 5.64 (p-value:0.2274), indicates no statistically significant models improvements after the inclusion of the moderators, which is also corroborated by minimal change in the residual heterogeneity across models (see I^2).*

Over Time Trends in the Rich-Poor Gap in Child Mortality						
	Reduced			Full		
	Est.	SE	pval	Est.	SE	pval
Intercept	-0.0062	0.0018	0.0001	0.0070	0.0094	0.4586
Dictatorship				0.0040	0.0028	0.1447
Democracy				0.0029	0.0029	0.3282
Genocide				0.0024	0.0041	0.5575
Baseline income per capita				0.030	0.0014	0.0308
Baseline rich-poor gap				-0.03357	0.0035	0.0001
number of transitions				0.0004	0.0011	.07212
N	50			50		
DF	2			8		
AIC	-333.7077			-280.0219		
BIC	-318.4115			-276.1978		
log-likelihood	174.8539			142.0109		
I^2	79.77%			1.85 %		
Test for Heterogeneity	p-value=0.0001			p-value=0.0982		
Test for Moderators				p-value=0.0001		

Table 3.2: *Results from the Mixed Effects Meta-Analysis investigating over time trends in the rich-poor gap in child mortality. The outcome variable is in the log-odds scale and is a contrast from the country-by-country logist regressions models with linear time trends. Income per capita is in the log-scale. All 50 countries were included. Reduce Model include no moderators (covariates) to account for the baseline differences. Number of transitions refer to number of democratic transitions. The log-likelihood ratio test is 65.6859 (p-value:0.0001), indicating strong and statistically significant model improvement after the inclusion of the moderators, which is also corroborated by the large decline in heterogeneity across models (see I^2).*

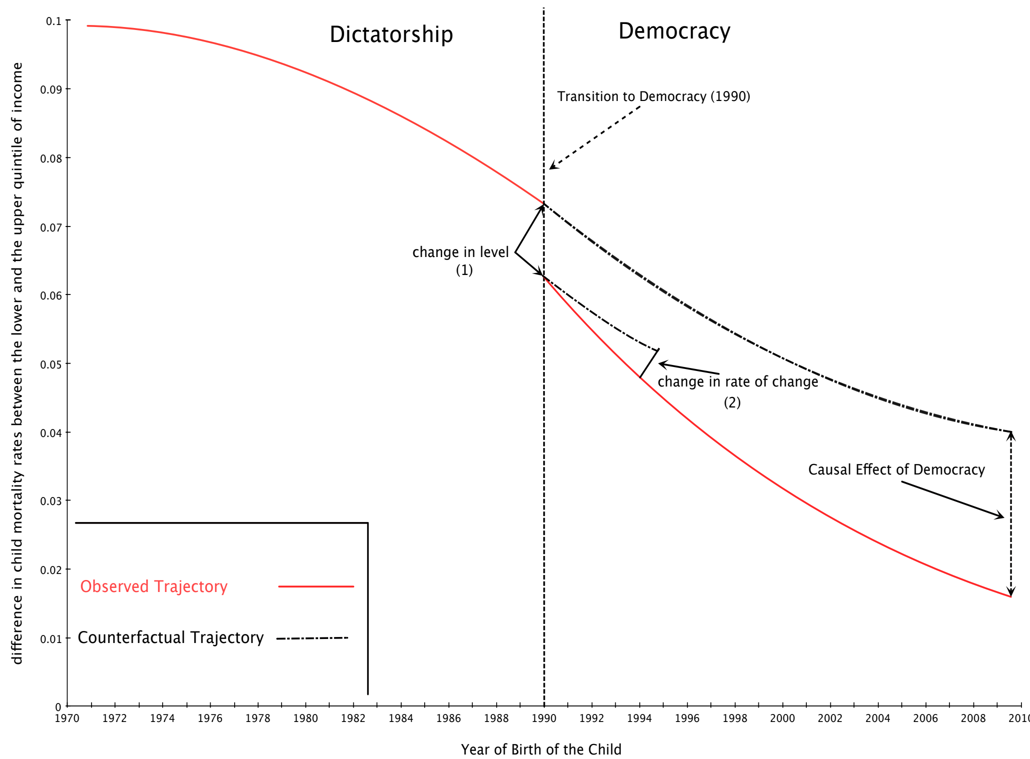

Figure 3.10: *This figure illustrates the bent line approach using GAM models. Country observed trajectory in child mortality follows a non-linear trend over time but the non-linearity is itself estimated from the data instead of assuming a particular parametric form — for example, quadratic, cubic, or B-Splines. In this approach, the bent lines that estimate the counterfactual scenarios become linear deviations from the main non-linear observed trajectories.*

CHAPTER 4

Measuring within-group inequality in child mortality in the developing world: A Bayesian Analysis of India

4.1 Introduction

Reducing child mortality and its inequities is a major societal goal. For example, the Millennium Development Goals (MDG) include child mortality reduction among their top priorities Kath A Moser (2005); Stuckler et al. (2010). While other health goals, such as family planning and immunization campaigns are controversial, child mortality reduction is a universal objective. Child mortality is not only an important indicator of the current health of a population, but it also offers insights about future health Hatton (2013).

Disparities in child health are a feature of the modern world. In the richest countries, national averages of child mortality fall below 10 deaths per thousand births. However, in many poor countries, these rates can be higher than 200 deaths per thousand births. Within-country disparities in child mortality can be even larger than disparities across countries. Large inequalities exist across income groups, where the poor in a country fare worse than the country's rich, sometimes by a great deal Victora et al. (2003); Sastry (2004); Wagstaff (2000). Within-country studies have also documented significant child mortality inequities along other dimensions including race and ethnicity M. Brockerhoff (2000); Prabir C. Bhattacharya (2011); Antai (2011).

Although between-group comparisons do offer important insights, these groups themselves may contain a high degree of heterogeneity. The heterogeneity is significant. Within-country disparities in child mortality are often larger than those across countries Gakidou and King (2003). In traditionally defined groups within countries—for example, births from urban areas—also show remarkable difference in death rates Marta M. Jankowska (2013). It is important to document these within-group inequalities because effective policy interventions depend on targeting the highest risk children. In particular, *it is important to identify high risk populations within as well as across traditionally defined groups (e.g. income, race, caste, urban/rural).* Thus within-group inequality is a major component of the inequities public health interventions target.

While there is a great deal of work on the measurement and comparison of child mortality across traditionally defined groups of people, there is much less work characterizing the entire distribution of mortality risks across individuals *within* these groups Gakidou and King (2002, 2003). Average mortality risk for any given group is an important and useful measure. Yet it does not capture the variance or inequality that exists within that group. A complementary approach to the study of between-group inequality is to evaluate within-group inequality, to characterize the *entire* distribution of risk within any given group. This is a methodologically challenging task: to estimate within-group heterogeneity, one needs to calculate the unobserved probability of death for each child that belongs to the group, or some approximation of it. In binary outcome models, the mean and the variance are directly related, which makes it harder to estimate variability in the data. By contrast, to study group-level inequalities in child mortality, group level death rates can be easily calculated as a proportion of children from that group who die[1].

This paper analyzes patterns and trends in child mortality using individual level retrospective data from India. It demonstrates how we can extend existing approaches to study total inequality in health outcomes, capturing the within- and between- group components. Child mortality inequities within India have been documented across states De and Dhar (2013), districts Prabir C. Bhattacharya (2011), socioeconomic groups Nidhi Jain (2013), and gender Kishor (1993), among other categories. Past scholarship has focused on these important between-group comparisons. However, substantial heterogeneity can be found within the groups themselves Abhishek Kumar (2014). Individuals facing higher mortality risk than the group average need tailored policy interventions. Within-group comparisons reveal these inequities. Between-group comparisons, where sub-groups are pre-defined, obscure these high risk groups that cut across traditionally defined categories.

[1] The maximum likelihood estimate for the death rate across an entire group is given by $\widehat{\text{MLE}} = \frac{\text{Deaths}}{\text{Births}}$. Note that this simple formulation relies on the assumption of constant variance within group.

In this paper, I make both a methodological and a substantive contribution to the previous literature. Substantively, I show that in India total inequality in child mortality is increasing over time. Taking advantage of the Bayesian paradigm, I calculate the posterior distribution of several classical inequality measures and show that, for all of them, disparities in child mortality has been increasing over time. I show that the posterior probability that the inequality has increased over the period under analysis is 99 %. This is the first comprehensive investigation of India's total inequality in child mortality over time.

Methodologically, I expand upon previous work on inequality in child mortality by applying statistical techniques to characterize the entire distribution of the death risk within a population of interest over time and across several levels of clustering. I fit individual choice random effects models to estimate the latent risk of death for a child, exploring several sources of clustering in the data simultaneously — mothers, sampling clusters, districts, states and time. While in binary data it is difficult to estimate variability in the data, we can take advantage of clustered data and random effects models to study inequality. I show the benefits of Bayesian inference for the study of inequality.

These contributions are significant. From a policy perspective, I am providing both policy-makers and academic researchers with necessary tools to measure the total health inequality across groups of people and to identify new high-risk populations that may not belong to any previously identified group. Despite it being a major societal goal, policy interventions aimed at reducing child mortality have been limited to traditionally defined social groups, such as income or racial groups, where within-group homogeneity is often assumed. We already know substantial heterogeneity exists within these groups and thus certain individuals suffer a higher risk of premature death than the group average suggests. By providing new tools for policy makers, public health interventions can be precisely targeted toward groups of high-risk individuals that may or may not otherwise be targeted in traditional categories or may even be categorized in multiple groups. Moreover, this research will provide academics and policy makers with measurements and statistical tools to draw a new map of inequality in child mortality across the developing world in a manner akin to maps of income inequality. In doing so, this research will help document inequities at an unprecedented level of detail and inform broad policy agendas like the Millennium Development Goals.

4.2 Within-Group Inequality in Child Mortality

Previous published work on the measurement of within-group inequality in child mortality is scant: it includes one paper and a few book chapters Gakidou and King (2003, 2002). It focused on the estimation of cross-national measures of inequality in child mortality using Demography and Health Surveys (DHS) as well as in the study of the major determinants of these inequalities. Two main statistical studies were conducted. The first estimated and compared the total health inequality within countries for a sample of 50 countries Gakidou and King (2002). In this instance, an extended beta-binomial model was used to approximate the underlying probability of death for each child. These models used as a response variable the number of surviving children per the total number of children ever born to each mother in the survey. Thus all predictors are aggregated at the level of the mother. Once the probability of death is estimated from these models, the second step is to create summary measures. Four measures were used: coefficient of variance, standard deviation, Gini index and their own measures. For a sample of 39 countries,Gakidou and King (2003) investigated the determinants of inequality in child mortality. In this book chapter, Gakidou and King (2003) uses a logit model with an additional latent parameter for each child to estimate inequality in child mortality.

Gakidou and King (2002) show that countries with similar national averages of child mortality may have quite different levels of health inequality. Gakidou and King (2003) have two main findings. First, they show that most of the variance in child mortality occurs within income groups in countries. Based on this finding, they examined which factors are associated with these inequalities. They find that income inequality is associated with mortality related inequality in only a handful of countries. In most cases, unequal access to health services seems to be associated with inequality in child mortality. Overall these finding are encouraging for our project: 1) they suggest that within-group inequality does exist in a non-trivial size; 2) within-group inequality can be studied from existing data sources; and 3) it does have policy implications.

The previous work has noteworthy limitations arising from the use of beta-binomial models. First, beta-binomial models entail loss of information because the data needs to be aggregated at the mother level. Thus classical predictors of child mortality (sex, the age of the mother at birth, birth order, etc.) cannot be used in the model unless averaged by mother. This is also a problem because one is often interested in the impact of individual level predictors *per se*, especially when the aim is to provide policy recommendations. Second, beta-binomial models cannot be easily extended to account for several sources of clustering simultaneously, such as geographic location and mother effects (this is not a minor point and we shall discuss it in section 3.1.2). Moreover, previous approaches are *cross-sectional*. They do not allow for the estimation of temporal variation in mortality inequalities. Yet, the DHS data were initially used to estimate changes over time in national averages of child mortality Rajaratnam et al. (2010). These data have temporal variation that can be used to estimate time trends in child mortality inequality while controlling for demographic decomposition effects.

Previous cross-country comparisons are further limited in that they do not allow for statistical inference (e.g. are countries' differences statistically significant?) and they focus on a few summary indices such as Gini coefficients Gakidou and King (2002). These indices may lose valuable information about within-group variation. More importantly, these limitations make it difficult to identify new subpopulations under high risk of death. A more comprehensive approach would be to compare the full distribution of the death risk across countries and across time (see section 3.3).

4.3 Methods

Previous work highlights the importance of within-group inequality in health disparities and establishes the basic facts about it. However, it has also some limitations. First, beta-binomial models forces the data to be aggregated at some level thus implying in information loss. For example, in the early application the unit of analysis was the mother and thus individual level predictors (birth order, birth interval, gender, etc) need to be averaged by mother. Second, previous research was cross-sectional thus it does not allow us the study over time changes in inequality. Finally, it does not explore sources of clustering other than that of the mothers. As in the previous research, the major methodological challenge arises because the outcome is binary and therefore mean and variance are not independent: they cannot be estimated separately. For example, if the rich display lower mortality rates than the poor, they will also have lower variance than the poor. More generally, regardless of how many data points we have, we will have as many predictions as covariate combinations in the data. By exploring several sources of clustering, one can gain leverage in the study of inequality.

One way to address that is to fit hierarchical models, where the same covariate combination will be allowed to vary within a given cluster. Allowing for different types of clustering is thus a major point. Without any random effects, every child that belongs to the same covariate combination group (age of the mother at birth of the child, same gender, etc.) will be estimated to have the exact same probability of death. Thus, even though our data sets can be very large, in reality we would have as many different predictions as there are covariate combinations in the data. The introduction of random effects changes this, allowing for variability among births with the same covariate combination group. For example, births with the same covariate combination and from different locations, different mothers, or both, can get different predictions. As a result, my approach greatly minimizes the risk of underestimating the true variability in the population.

4.3.1 Random Effects Logit and Probit Regressions

I use random effects logit models to predict one year survival. These models allow me to use covariates from whatever level they exist in the data as predictors in the model — such as the individual level, which include gender, age of the mother at birth, birth order, birth interval, and at the household level which predictors include wealth, and education. Thus the effect of classical predictors of child mortality can be estimated from the data. Time trends can be easily accommodated in these models, using flexible, non-parametric models, such as generalized additive models, if needed. An even more compelling reason to choose hierarchical logit and probit regressions is that these models allow for several sources of clustering in the data *simultaneously*, such as births clustered by mothers and geographical locations. Multiple geographic locations are available (sampling cluster, village, district, state).

I fit variations of the basic following generalized linear mixed effects models to estimate death risk. Suppose a regression is fitted with two levels of clustering, mother and geographic location. Let i index children from a given mother, j index mothers, and k index geographical locations within India, such that we have $i = 1, ..., n_{jk}$ children, $j = 1, ..., n_k$ mothers, and $k = 1, ..., K$ locations.

Then the model

$$Y_{ijk}|\pi_{ijk} \sim \text{Bern}(\pi_{ijk}) \tag{4.1}$$

$$\text{logit}(\pi_{ijk}) = \beta_0 + x'_{ijk}\alpha + \delta_{jk} + \gamma_k \tag{4.2}$$

$$\delta_{jk} \sim N(0, \sigma_1^2) \tag{4.3}$$

$$\gamma_k \sim N(0, \sigma_2^2), \text{ where} \tag{4.4}$$

- Y_{ijk} is the response variable, whether child i born from mother j in the year t_{ijk} and in the location k was alive at the age of 1 years old, $y_{ijk} = 0$ if alive, $y_{ijk} = 1$ if dead.

- π_{ijk} is the unobserved death probability of the ijk child.

- x'_{ijk} are covariates. These include demographic predictors, for example, child covariates (sex, birth order, birth interval, maternal age at birth of child, gender), mother and household covariates (wealth, education) and village covariates (access to health services).

- α are the fixed effects corresponding to the covariates in x'_{ijk}.

- δ_{jk} is mother random effect. It accounts for correlation between children with the same mother and unmeasured variables at the level of the mother and household. Its variance is σ_1^2.

- γ_k is the location random effect. It accounts for correlation between observations in the same location and unmeasured variables at the geographic level. Its variance is σ_2^2.

4.3.2 Bayesian Estimation

I fit the model described above using a Bayesian framework, taking advantage of the flexibility of this approach. It is easy to get estimates, predictions and measures of uncertainty Gelman and Hill (2006). Bayesian methods tend to be superior to classical methods for fitting binary data because they do not rely on asymptotic results and because they can be used for both complex models and small data sets Weiss (2005). Additionally, random effects and correlations among them can be easily accommodated within this framework. Information is available about sampling clusters, districts and states.

Sampling uncertainty across all levels of the analysis is automatically incorporated by the Bayesian framework. Moreover, it is easy to generate predictions and non-standard inferential quantities of scientific interest using a Bayesian framework. Thus Bayesian inference provides a more straightforward approach to obtain desired predictions with associated uncertainty. Formal model comparisons can be done using DIC and deviance statistics.

4.3.3 Priors

I use flat priors for fixed effects parameters and uniform priors for the random effects variances. For scientific purposes, my results are numerically comparable, though not identical, to maximum likelihood estimates.

4.3.4 Inferential Targets

The goal is to estimate the posterior distribution of inequality indexes over time, such as Gini coefficients (see next section bellow for a discussion of these indexes). These coefficients are functions of the probabilities π_{ijk} of death. To account for the population-level variability, one needs to include in the calculation of these indexes models' sources of uncertainty. To do so, I take advantage of the Bayesian approach. I take the predicted probability π_{ijk} of death for each kid, given his or her covariate and random effect values in the data. Since the models are estimated from MCMC methods, I have many posterior samples of the entire set of probabilities π_{ijk}. For each sample, I calculate an index over time. Therefore, the MCMC results will produce a distribution of the index. I do that for each year, producing a series of posteriors of the Gini or other index over time. This is a very natural way to propagate the uncertainty from the first step of the analysis — the estimation of the latent death risk for each birth — to the second step, where I use the indexes to measured inequality over time in India.

4.3.5 Measurement of Inequality

I use standard measures of inequality. I follow the literature on income inequality and define inequality as the absence of equality. Equality exists when all births have identical underlying death risks. In the income inequality literature, deviations from equality are measured in terms of ratios, for example, when a unit has a disproportionally high share of some item. I defined inequality in child mortality in terms of ratios as well. This is an important point because we want measures of inequality that are independent of mean levels. We want to investigate the spread of a distribution given its mean levels.

It can be shown that the measurements of inequality used here all follow a simple formula. Different measurements, however, will capture different aspects of the distributional change. This is important because it helps us to have more robust and comprehensive conclusions from the data. As shown byFirebaugah (1999) andFirebaugah (2002), these indexes differ only because they employ different functions for the income ratios. For each MCMC sample, the general formulae is

$$\text{Inequality} = \sum\nolimits_{ijk} f(r_{ijk}), \text{ and} \tag{4.5}$$

$$r_{ijk} = \frac{\widehat{\pi}_{ijk}}{\frac{1}{n_{ijk}} \sum_{ijk} \widehat{\pi}_{ijk}} \tag{4.6}$$

where r_{ijk} is the ratio of the posterior predicted death risk for the ijk child to the average birth posterior predictive death risk in the population in that sample, $\frac{1}{n_{ijk}} \sum_{ijk} \widehat{\pi}_{ijk}$ Based on this general approach, I use four popular measures of inequality for the $f(.)$: squared coefficient of variance (V^2), variance of the logs, the Theil index, and the Gini index,

$$v_{ijk} = (r_{ijk} - 1)^2 \tag{4.7}$$

$$t_{ijk} = r_j \log(r_{ijk}) \tag{4.8}$$

$$l_{ijk} = \log\{r_{ijk} - E[\log(r_{ijk})]\}^2 \tag{4.9}$$

$$g_{ijk} = r_{ijk}(q_{ijk} - Q_{ijk}) \tag{4.10}$$

where E is the expected value, log is the natural logarithm, q_{ijk} is the proportion of total population with higher death risk than unit ijk and Q_{ijk} is the proportion of total population with lower risk than unit ijk so that $q_{ijk} + Q_{ijk} = 1$.

All these measures are *scale invariant*: suppose that the probability of death declines over time for all births from a fixed population of mothers. Then the average child mortality will decline but the ratio remains the same. This property is also called mean independence, and is important because we are focused on ratios not gaps.

The Bayesian approach discussed above is very useful for making inferences about these measures. Using the predictions described in the previous section, I calculate the posterior predictive distributions for each of these measurements. To do so, I calculate each inequality measure for each MCMC sample. Using this approach, I am fully propagating the uncertainty in estimating parameters into the analysis of inequality.

4.4 Data

I use data from the Demographic and Health Surveys (DHS).[2] These are nationally representative surveys that have been conducted in more than 85 countries since 1984 Corsi et al. (2012); Fabic et al. (2012). These surveys collect a great deal of information from these countries, particularly on the fertility and reproductive health of their population. Low income countries and international agencies have long relied on it to monitor the health of their population. For example, the national child mortality averages are often estimated from DHS

[2] Available: http://www.measuredhs.com/

Rajaratnam et al. (2010). It is an USAID-funded project currently implemented by a private company ICF International Corsi et al. (2012); Fabic et al. (2012). The data itself has been used and validated by thousands of researches all over the globe.

DHS also collects information on indicators of permanent income for each household, such as ownership of car, radios and TVs; whether the household has electricity and running water; type of the materials used the walls, floor and the roof of the house; and the type of toilet in the household. This information is used to construct an indicator of permanent household income. Details of the model used to construct this indicator are discussed byRutstein (2008).

DHS data are based on *retrospective surveys* that can be used to form *retrospective panels*, which are a common source of information in demography and health sciences, particularly from developing countries. Retrospective panels are constructed from these surveys as follows: at the year in which the survey is conducted, mothers of reproductive age (usually 15-45) from a sample of representative households in the country are interviewed. These mothers answer several questions, including ones about their complete birth histories — how many children they had and when. These answers are use to form retrospective panels where each observation represent a child born to a given mother in a given year. Additionally, interviewers collect objective information from the household, such as household assets. In India, these surveys are representative at both national and state levels.

cluster level	sample size
births	131,743
mothers	56,914
sampling clusters	3,471
districts	388
states	25

Table 4.1: *Sample size for each clustering level.*

4.4.1 Data Challeges

The data are subject to several problems, such as recall bias, lack of representativeness of some subpopulations, and a few types of censoring and measurement error in the variables that were not collected at the time of the interview. Maternal age at the child's birth is censored. In fact, while women from all reproductive ages are represented at the year(s) in which a survey is taken, their ages decline monotonically, as we move back in history. This censoring raises concern because it means that, as we move back in time, we no longer have information regarding births from mothers from all age groups. Because our sample of births come from increasingly young women, it is difficult to calculate overall inequality in child mortality for women from all age-groups as we look back in time. I restrict my sample to births from women aged 15 to 35 years. While this mostly eliminates the problem, it limits my inferences to to births from women at that age range. It is not a major problem, however, only a minority of births are from women older than 35 years.

A second issue is that retrospective information is subject to recall bias, especially as we move far back in time. Because giving birth is such a significant event in a woman's life, recall bias is much less of a concern in our study than it would be for income data. The DHS interviews perform a series of checks to ensure the quality of the complete birth history data. Moreover, recent studies found no systematic recall bias while comparing mortality rates from earlier and later surveys for the same time period Hill and Choi (2006); Kudamatsu (2012). Excluding births far away from the data in which the survey was taken also minimizes this problem.

A final data challenge arises because household level variables in the DHS were collected at the time of the interview, not at the time the child was born. Thus, these variables might be poor proxies for children's socioeconomic conditions at the time of their birth, especially wealth and maternal education. However, indicators of wealth from the DSH have been used before to make inferences for births across income levels and over time. For example, DHS data have been used by the World Bank country reports to provide mortality estimates across quintiles of income for a host of countries Houweling et al. (2005); DR et al. (2007). The interviewers asked each woman for educational information. In contrast to the wealth indicators, this is an absolute measure. Indicators of education have also been previously investigated. Using earlier waves of DHS,Gakidou (2013) compared it with estimates fromBarro and Lee (2001) and found that the data from DHS does indeed match national level estimates. More recently, DHS has been used to provide high quality estimates of maternal education over time

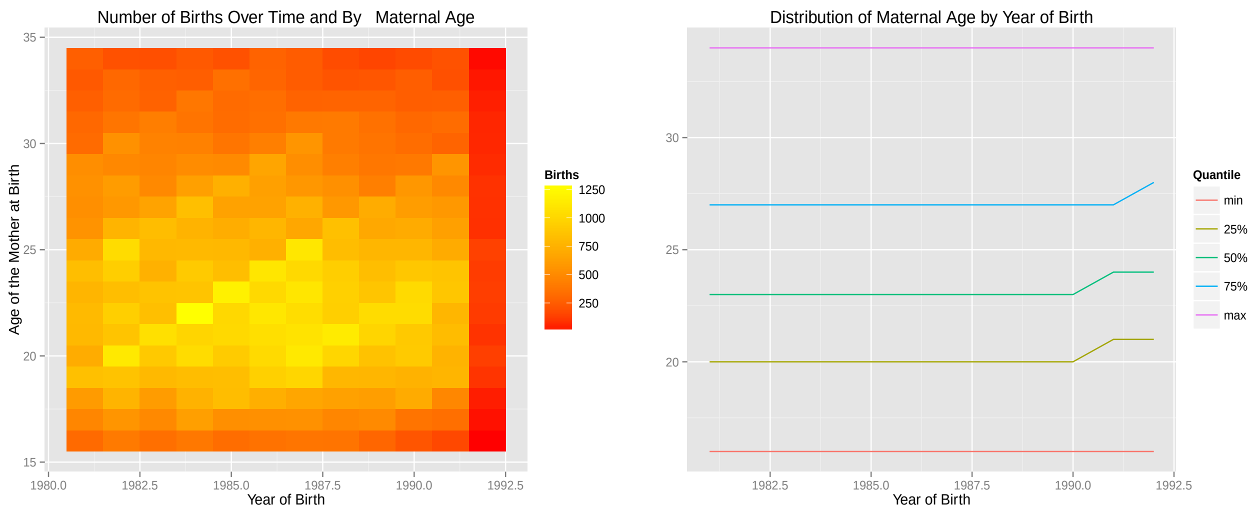

Figure 4.1: *Distribution of the of the Age of the Mother at the birth of the Child Over Time. The left panel plots the sample size by maternal age over time. The right panel plots minimum, 25%, median, 75% and maximum age of the mother over time in my sample.*

Gakidou et al. (2010b). A final sample is displayed in figure 4.1 and summary statistics are available on table 4.1.

4.4.2 Graphical Analysis

Figure displays mortality rates over time for the main subgroups in the data set: Maternal education (no education, primary, secondary, and higher); relative wealth in quintiles; case (schedule tribe, schedule caste and other); religion (christian, hindu, muslim, sikh and other); place of residence (urban, rural); gender. Within each plot panel, each line represents a different subgroup. For the most part, these covariates seems to matter. Maternal education seems to be an important predictor of child mortality as lower levels are associated with higher risk of death. Wealth also seems also important, but the two poorest groups display similar levels of child mortality. Scheduled caste have higher mortality rates than the other castes. Christians have lower rates while hindus have the highest, though these differences are decreasing over time. Children born in rural areas are more likely to die than those from urban areas, even though these differences are also decreasing. Gender does not seem to make much difference, which is interesting given the debate on "missing women" in India Kishor (1993).

As previously mentioned, my ability to get additional sources of variance in the data beyond the ones available in the covariates is very important. Thus, a key question is whether we do have additional sources of variance in these clusters. The actual description of the variation across clusters are of scientific interest. Figure 4.3 explores these questions. Each panel is one of the five main clusters available in the data set: mothers, sampling clusters, districts and states. The range of these histograms are not comparable, as the range of the distribution of the child mortality varies greatly from cluster to cluster. However, their mean death rate in each clusters is quite similar much (state=0.07; district=0.08; sampling clusters=0.07; mothers=0.07). Most mothers experience no death of their infants. The largest range, can be found across sampling clusters and districts. These geographic variation is in line with previous research De and Dhar (2013).Given the level of variance present in these clusters, we should expected that statistical models that explore them actually improve the estimate of the variability in the data beyond existing covariate combinations.

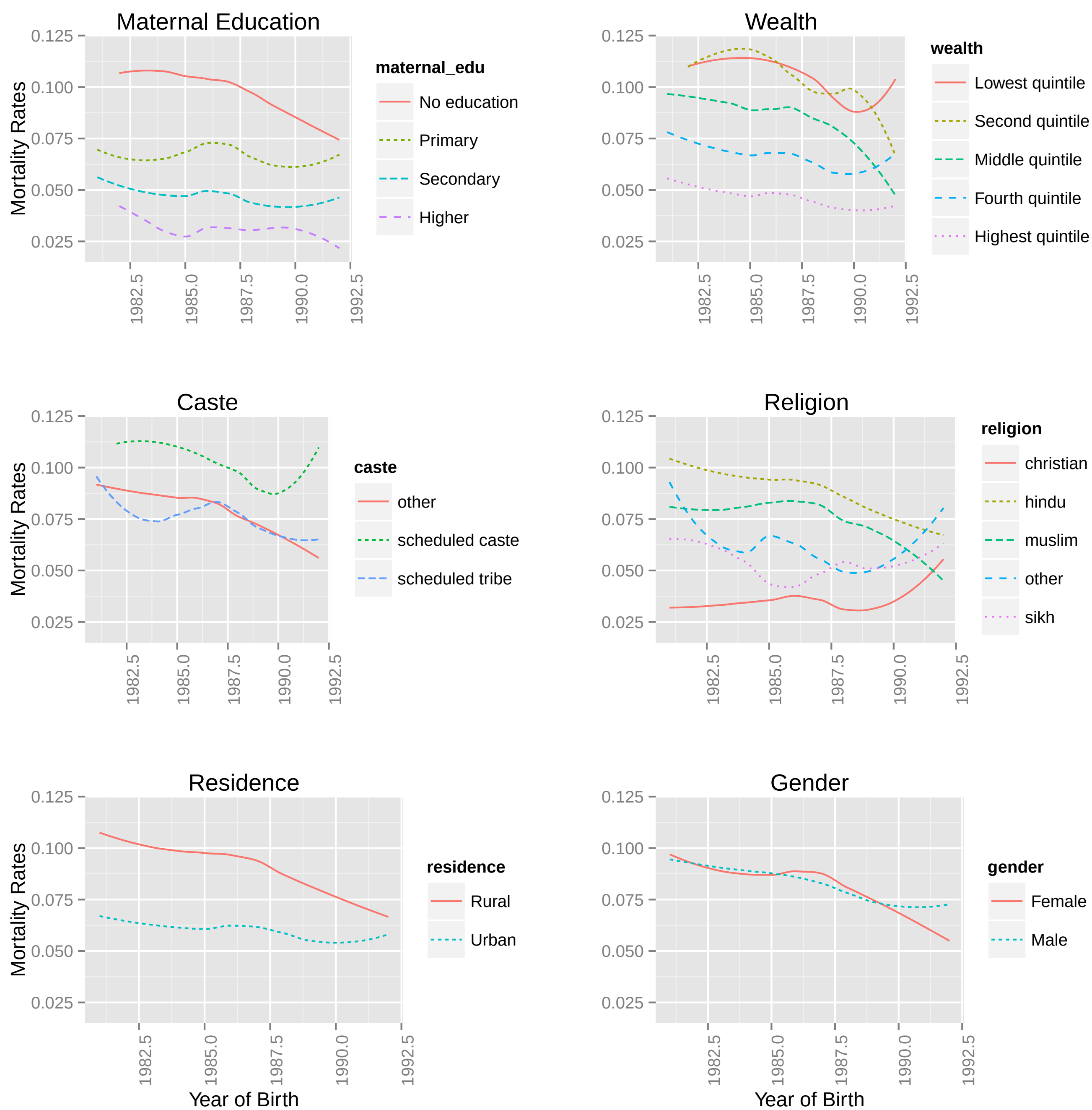

Figure 4.2: *Mortality rates by socioeconomic groups.*

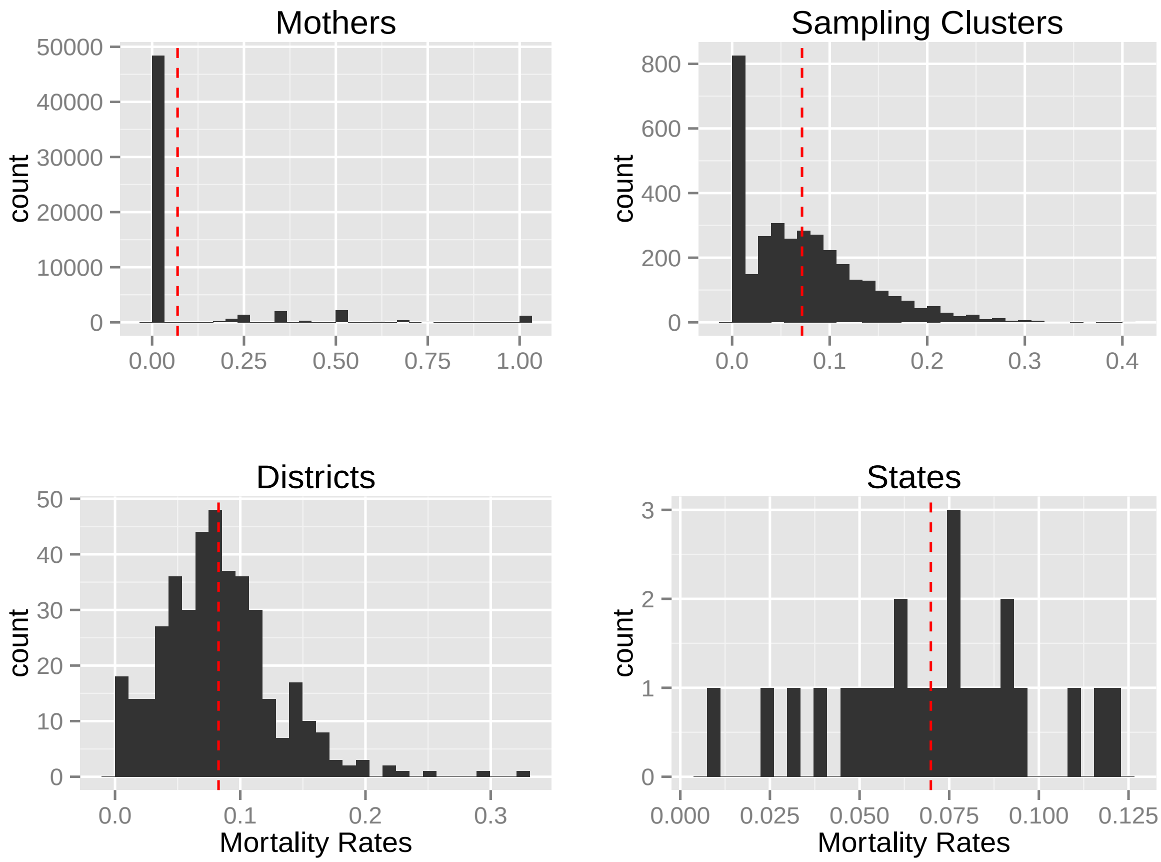

Figure 4.3: *Mortality rates by clusters. Histograms are in different ranges. The vertical dotted red lines are the mean mortality rates in each cluster (25 states, Mortality Rates (MR)=0.07; 388 districts, MR=0.08; 3471 sampling clusters, MR=0.07; 56914 mothers, MR=0.07)*

4.5 Results

4.5.1 Random Effects Models

I fit several nested random effects models to estimate the underlying death probability for each child in my sample. The models were estimated using Markov

Chain Monte Carlo (MCMC) methods from the "MCMCglmm" package. I run one long MCMC chain for each model, with 100,000 iterations and a burn-in of 20,000 iterations. I thinned the remainder of the chain by 20 thus saving 4,000 posterior samples for each model. Conventional checks suggest models' convergency. For each model, the fixed effects predictors are the same, but I made the hierarchical structure progressively more complex, by including additional nested clustering levels (mothers, sampling clusters, districts, and states). I experimented with non-linear time trends, but lack of statistical significance for the higher order polynomials suggests a linear time trend. I have also interacted key predictors in the model, such as wealth and time, but again, lack of statistical significance suggested a model without interaction. I compare models' fit using DIC and Deviance statistics, which are usual in Bayesian models. Results are presented on table 4.2 and the figure 4.4.

The largest increase in models' fit is accomplished by adding random effects for mothers to the null model — the one without any clustering. The standard deviation of the mothers' random effects does not change much as I add other levels of clustering in the model. This suggests that mothers have a strong impact on the children's odds of survival, even after controlling for mothers' observable characteristics, such as her wealth, education, and age at the birth of her child . Adding the additional levels of clustering does increase models' fit, however. This increase is statistically and substantively significant. We can see the effects of additional levels of clustering models' fit by looking at DIC and deviance statistics. The results match the finding from the previous literature and the graphical analysis. There exist important but unmeasured outcomes at the level of the sampling clusters, districts and states (India is a federal state). These goodness of fit statistics also show that adding levels of clustering beyond the ones used in the previous literature lend insight into inequality. They help us to model additional variability in the data beyond the covariate values. These findings confirm the graphical analysis which shows large variance in child mortality in each level of clustering.

The results for the covariates are quite stable across models. Except for the difference between the first and the second poorest income quintiles, wealth is always significant. Maternal education is also highly significant across all levels. The place of residence is never significant and neither is gender. These last results are an interesting contrast to the graphical analysis. In figure 4.2, children have similar death rates across genders but different places of residence are associated with different rates. However, controlling for other factors, the rural and urban divided no longer exist. Religion is substantively more important than caste but in both instances, statistical significance dependends on the models' random effects structure. In my models, Christians were coded as the reference religion. All other religions are significantly different from Christian, but when district effects are included, they are no longer different from Others (i.e. someone who is neither a Hindu nor a Muslim not a Sikh) and when state effects are included, only Hindus have statistically different (higher) mortality rates than Christians. Births from schedule caste have statistically and substantively higher mortality rates than the other two code castes.

It is difficult to evaluate the magnitude of these effects because logistic regression coefficients are not in the scale of the data. To provide a sense of the magnitude of the effects I will focus on model 5, the best fitting one, although, as we have seen, results are quite stable across all models.

In this model, the intercept is the death risk before the age of one for a child who belongs to the lowest wealth quintile, lives in rural area, is a female, was born in 1982, and whose mother has no education, is low income, gave birth at the age of 18 years old, is Christian and does not belong to either schedule caste or tribe. In the probability scale, that person has a death risk of $\approx$ .045 CI[.032,.061]. The difference between the poorest and the second quintile is not significant but if we pick another birth with the exact the same characteristics but now in the other quantiles of income, the death risk is, respectively, 3Q=0.038,4Q=0.032, 5Q=0.025. This is a substantively significant drop. The effect of the maternal education is also substantial. Risk of death drops as education increases from the baseline case, .045, where the mother has no education, to Primary =.035, Secondary=.027, Higher=0.017. These associations are even larger than income. The combined effects of both, which are quite likely to exist in practice, are even larger. For example, keeping the other variables constant but choosing a birth from a mother that is both rich and highly educated has an average death risk of only .01, a significant drop from .045. This is a likely scenario, since over 92 % of the highly educated mothers are also on the highest income quintile.

I use in-sample predictions from model 5 — the best fitting one — where both random effects and covariate values will be set to their actual data values. These

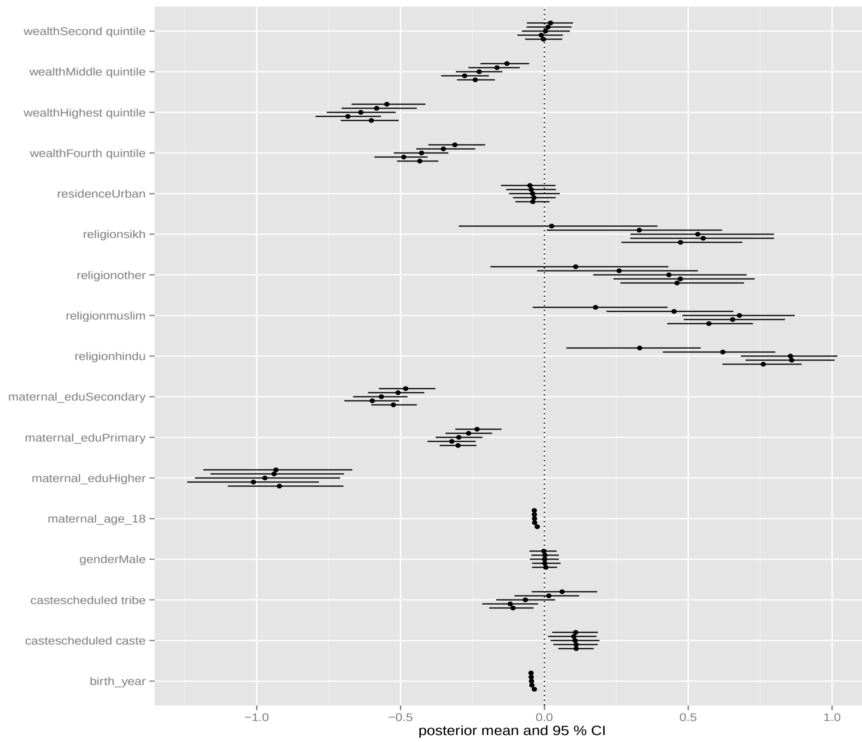

Figure 4.4: *Models's results. Graphical comparison of the fixed effects coeficients; Intercept omitted. Vertical dotted line indicates statistical significance at 95%.*

predictions are the underlying risk of death of each birth in my data set. First, I graphically display these predictions, them I show posterior predictive summary measures of inequalities and, finally, I discuss the high risk births.

Figure 4.5 are box plots displaying temporal changes in the total inequality in child mortality. The left panel displays the full distribution while the right panel excludes the births that have a death risk above 5 % so that we can focus on the analysis of main trends. The box plots indicate an over time reduction in median level of child mortality but only a moderate change in the range of interquartile range (IQR) — the difference between 25% and 75% values. In fact, in 1982 the death risk was 25 percentile = .04, 50 percentile =.06 and the 75 percentile was .09 while in 1992 is was, respectively, .2, .4, .7 yield an interquartile drop of .05 to .04, which is modest. Interestingly, the box plots shows a number of high risk births. In fact, there are 5,548 births with death probability are higher than 10 %.

	Model 1	Model 2	Model 3	Model 4	Model 5
(Intercept)	-2.75	-3.28	-3.34	-3.19	-3.12
	(-2.93, -2.61)	(-3.47, -3.10)	(-3.57, -3.16)	(-3.41, -2.96)	(-3.44, -2.79)
Mother's Age	-0.02	-0.03	-0.03	-0.03	-0.04
	(-0.03, -0.02)	(-0.04, -0.03)	(-0.04, -0.03)	(-0.04, -0.03)	(-0.04, -0.03)
Year of birth	-0.03	-0.04	-0.04	-0.05	-0.05
	(-0.04, -0.03)	(-0.05, -0.04)	(-0.05, -0.04)	(-0.05, -0.04)	(-0.05, -0.04)
Wealth - 2nd Q	-0.00	-0.01	0.00	0.01	0.02
	(-0.07, 0.06)	(-0.09, 0.06)	(-0.08, 0.09)	(-0.06, 0.09)	(-0.06, 0.10)
Wealth - 3rd Q	-0.24	-0.28	-0.23	-0.16	-0.13
	(-0.30, -0.17)	(-0.36, -0.19)	(-0.31, -0.15)	(-0.26, -0.09)	(-0.22, -0.05)
Wealth - 4th Q	-0.43	-0.49	-0.43	-0.35	-0.31
	(-0.51, -0.37)	(-0.59, -0.41)	(-0.52, -0.33)	(-0.45, -0.24)	(-0.40, -0.21)
Wealth - 5th Q	-0.60	-0.68	-0.64	-0.58	-0.55
	(-0.71, -0.51)	(-0.80, -0.57)	(-0.76, -0.52)	(-0.70, -0.44)	(-0.67, -0.41)
Mother's Edu - Primary	-0.30	-0.32	-0.30	-0.26	-0.23
	(-0.36, -0.24)	(-0.41, -0.24)	(-0.38, -0.22)	(-0.34, -0.18)	(-0.31, -0.15)
Mother's Edu - Secondary	-0.52	-0.60	-0.57	-0.51	-0.48
	(-0.60, -0.44)	(-0.70, -0.51)	(-0.67, -0.48)	(-0.61, -0.42)	(-0.58, -0.38)
Mother's Edu - Higher	-0.92	-1.01	-0.97	-0.94	-0.93
	(-1.10, -0.70)	(-1.24, -0.78)	(-1.21, -0.71)	(-1.16, -0.70)	(-1.19, -0.67)
Caste	0.11	0.11	0.11	0.10	0.11
	(0.05, 0.17)	(0.03, 0.19)	(0.02, 0.19)	(0.01, 0.18)	(0.03, 0.19)
Tribe	-0.11	-0.12	-0.07	0.02	0.06
	(-0.19, -0.04)	(-0.22, -0.02)	(-0.17, 0.04)	(-0.10, 0.12)	(-0.04, 0.18)
Hindu	0.76	0.86	0.86	0.62	0.33
	(0.62, 0.89)	(0.70, 1.01)	(0.68, 1.02)	(0.41, 0.80)	(0.08, 0.54)
Muslim	0.57	0.65	0.68	0.45	0.18
	(0.43, 0.72)	(0.48, 0.84)	(0.48, 0.87)	(0.22, 0.66)	(-0.04, 0.43)
Other religion	0.46	0.47	0.43	0.26	0.11
	(0.26, 0.69)	(0.24, 0.73)	(0.17, 0.70)	(-0.03, 0.53)	(-0.19, 0.43)
Sikh	0.47	0.55	0.53	0.33	0.03
	(0.27, 0.69)	(0.30, 0.80)	(0.30, 0.80)	(0.01, 0.62)	(-0.30, 0.39)
Urban	-0.04	-0.04	-0.04	-0.05	-0.05
	(-0.10, 0.02)	(-0.11, 0.04)	(-0.12, 0.05)	(-0.13, 0.04)	(-0.15, 0.04)
Male	0.01	0.00	0.00	0.00	-0.00
	(-0.04, 0.04)	(-0.04, 0.06)	(-0.05, 0.05)	(-0.05, 0.05)	(-0.05, 0.04)
Mother		1.663	1.464	1.445	1.452
Cluster			0.245	0.123	0.121
district				0.206	0.124
State					0.337
DIC	72508.98	67712.91	67446.62	67273.21	67219.79
Deviance	64933.45	54951.07	54797.51	54761.36	54726.59

Table 4.2: *Results from the random effects models*

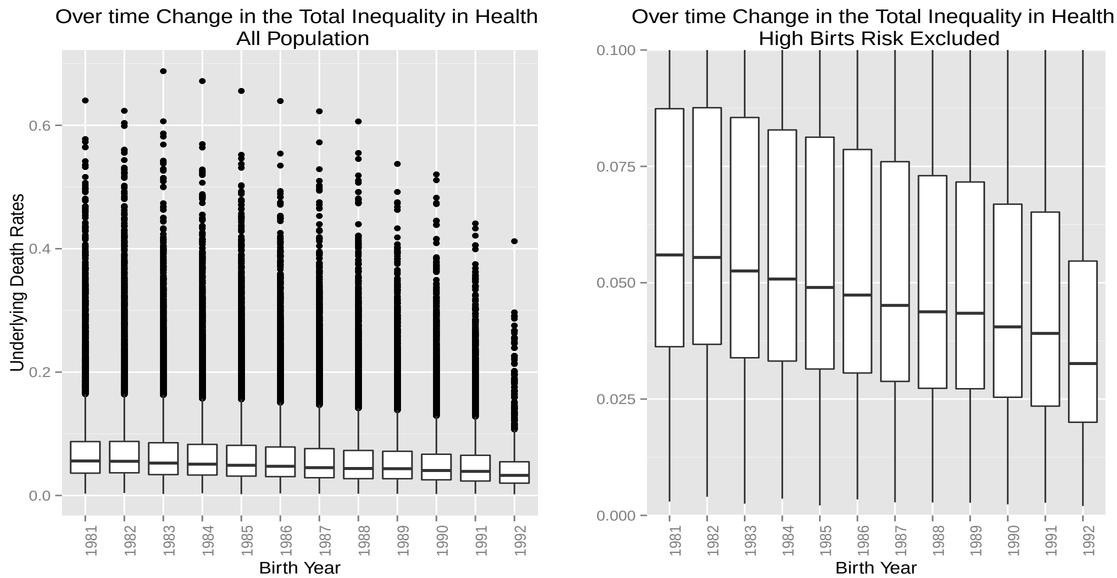

Figure 4.5: *Box plots of the estimated death probability for each child in the sample by year. The left panel including all births (131,743). The right panel excludes high risk births (5,548 births with death probability higher than 10%) to allow for clear visualization of the main trends. Median levels of child mortality are declining over time but the interquartile range does not indicate significant reduction on inequality levels.*

Inter-quantile ranges are not a good measure of inequality, however, as they are not scale invariant and thus sensitive to mean changes. Thus we now focus our attention to the classical measures of inequalities that I have discussed in the methods section: Gini index, theil index, squared coefficient of variance, and variance of the logs. Figure 4.6 plots the over time increase in total child mortality for all Indian population. The lines are the mean predictive value in each year and the shaded area are the 95 % confidence interval from the Bayesian

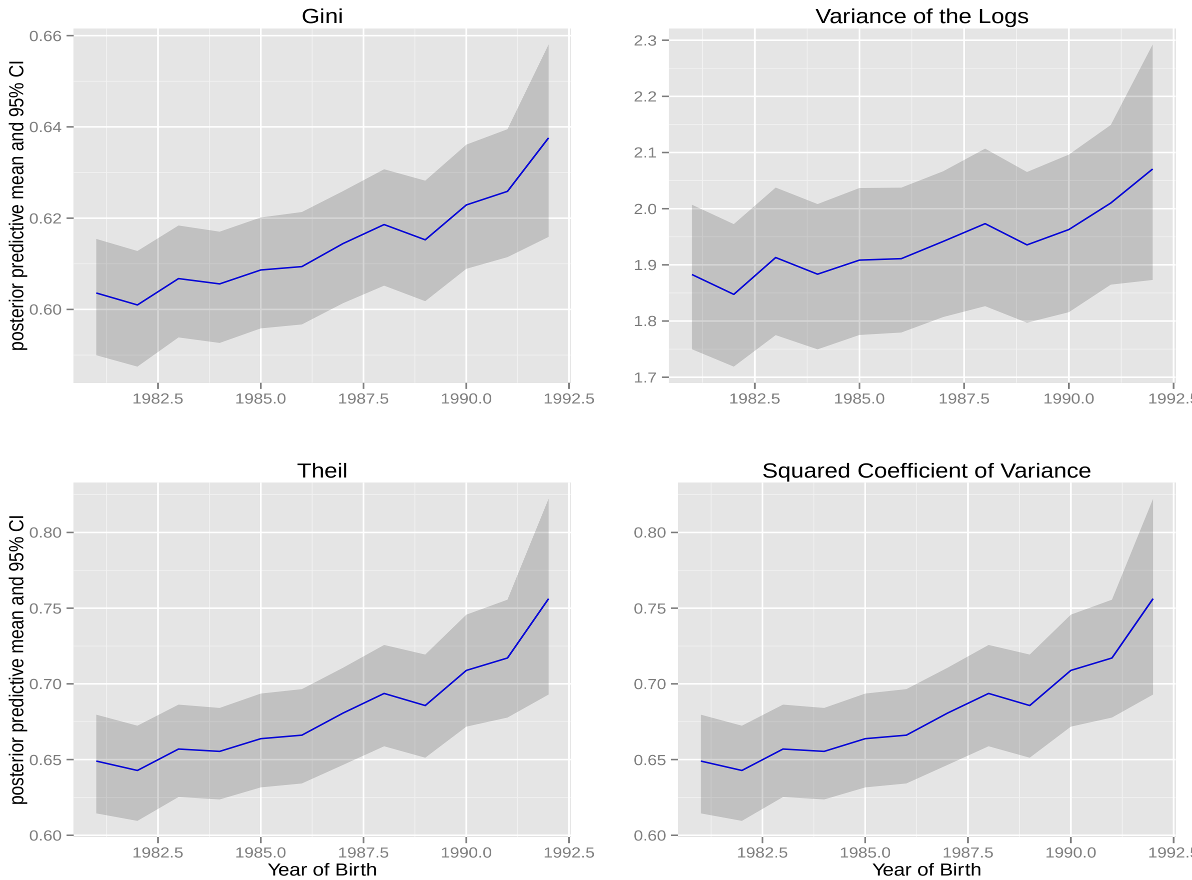

Figure 4.6: *Posterior predictive measures of total inequality over time and their 95 % confidence interval. The lines are the mean values and the shaded area are the confidence intervals. All measures indicate increase in child mortality over time.*

simulation. These measurements were calculated from the MCMC samples in the way described in the methods section so that they incorporate uncertainty from the predictions naturally, which is one of the innovations of these study. The Gini changed from estimated 0.60 [95% CI: 59,0.61] to 0.64 [95%: 0.62 0.66]; Theil index from 0.65 [95% CI: 0.61;0.68] to 0.78 [95% CI: 0.67; 0.82]; the squared coefficient of variation from 0.65 [95% CI: 0.61; 0.68] to 0.76 [95% CI: 0.7; 0.8];

and the variance of the logs from 1.88 [95% CI: 1.74; 2] to 2.07 [95% CI: 1.88; 2.3].

All measurements indicate that child mortality is increasing over time. For each measurement, I calculate the probability that the inequality was higher in the baseline year than in the last year. To do so, I again take advantage of the MCMC samples. Since all samples are equally likely, for each pair of samples I calculate whether one year was higher than the other than I average the results across all samples. My results indicates that the probability that inequality in the first year is higher than for the last year is at least 99% for all measurements. This pattern is important and has not been previously documented. This also demonstrates the utility of the Bayesian MCMC approach for calculating non-standard quantities of theoretical interest.

4.5.2 Detecting High Risk Groups

Figure 4.5 suggests that high risk births were common in India but their prevalence declined over time. They are trending down while inequality is trending up, which suggest that the surge in inequality is unrelated to them. In any case, it is of policy interest to investigate the demographics of those births. Table 4.3 present the summary statistics for the high risk births. They are mostly from low income, low educated mothers, from rural areas. The mothers are neither from schedule or castes or tribes, and are Hindus. Gender does not seem to have an impact. However, my analysis is optimistic in the sense that it seems that these groups are a shrinking share of the total Indian population.

4.6 Discussion

Reducing premature infant death is one of the Millennium Development Goals top priorities. Worldwide, there are huge disparities in child health. While premature child death is as low as 10 deaths per thousand births in the richest regions of the world it can be higher than 200 per thousand in the poorest regions. The first step to combating these inequities is understanding their patterns and sources. Many studies have investigated inequality in child mortality across traditionally defined groups of people (countries, race, income levels, etc.). Very few studies, however, have characterized the entire distribution of death risk across the populations. Studies that do not consider the entire distribution of risk across all children in a given group are apt to miss high risk subpopulations that exist within or across these groups. These high risk subpopulations have special needs that should be targeted accordingly. More generally, when one calculates rates of premature death for given groups, one often assumes that the groups are homogeneous populations, where children have the same death risk. However, this homogeneity assumption is often inaccurate. Within-group inequality is an important component that is missing from prior analyses and previous literature on this topic is minimal.

This paper uses survey data from India and Bayesian hierarchical models to fill this gap. Methodologically, I show that we can use existing statistical tools and data sources to extend the analysis of total inequality over time. To do so, I employ individual level retrospective panel data on child mortality. I use random effects models and take advantage of the hierarchical structure in the data to estimate latent death risk for each child in my data set. I summarize the latent death risk for each child using several classical inequality measures. I use Bayesian inference, which allows me to propagate the the uncertainty from the estimated death risk from each child into my summary measures of inequality.

This is the first paper that studies total inequality in child mortality over time in India, a country in which extensive research have documented between-group inequality. My findings indicate that inequality in child mortality is increasing in India. This finding is robust to several alternative classical measures of inequality. I also hypothesize that this fact is mostly due to the concentration of the high risk births across poor and low educated mothers. These patterns have not been previously documented on the literature on inequality in India. It also suggests that the missing within-group component is a important one and that the study of inequality in child mortality will be enriched by considering it.

A main limitation of this study lies in the measures of inequality used. Even though they are standard measures in the study of inequality, none of them are designed to distinguish between growth in the lower and upper tails of the distribution. While they can detect temporal increases in inequality, they cannot distinguish between polarization (increase in both tails) from upgrading (increase in the lower tail) from downgrading (increase in the lower tail). Since we care about high risk groups, this is a noteworthy limitation.

This research can be extended into several directions. Methodologically, there are two main lines of improvements. First, since the models' goal is prediction, one can employ more general approaches, such as Bayesian Model Averaging.

This technique often increases models' performance and accounts for the inheriting uncertainty in the model selection procedure. Secondly, we can employ other measures of inequality that capture other quantities of theoretical interest. They are often, the only one used in the study of inequality, none of the measures used here are designed to distinguish where the distributional changes are happening. For example, are those changes due to polarization in the distribution or, instead, due to increase in the high risk births. More detailed measures of the change in inequality, such as those discussed by Handcock and Morris (1999).

Substantively, one can extend this analysis to several other countries. In fact, DHS data are available for several countries. The MICS surveys, which are comparable to DHS, also exist for several other countries. Taken together, these two surveys have data for approximately one hundred countries that are potentially amenable to the type of the analysis implemented here.

Covariate	Level	Proportion
Income	Lowest quintile	0.34
	Second quintile	0.32
	Middle quintile	0.19
	Fourth quintile	0.11
	Highest quintile	0.04
Maternal education	No education	0.89
	Primary	0.07
	Secondary	0.03
	Higher	0.00
Gender	Female	0.48
	Male	0.52
Caste	other	0.69
	scheduled caste	0.20
	scheduled tribe	0.11
	christian	0.01
Religion	hindu	0.88
	muslim	0.09
	other	0.01
	sikh	0.01
Place of Residence	Rural	0.89
	Urban	0.11

Table 4.3: *Descriptive statistics for high risk births (death risk higher than .15).*

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