Impact of Abortion Reforms on Reproductive and Neonatal Health in Low- and Middle-Income Countries

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Abstract

Unsafe abortion remains a major global public health challenge and contributes to a staggering number of deaths and disabilities annually. Access to safe abortion services is influenced by a wide range of factors, including the legislative environment. A country's abortion law is a key component in determining the enabling environment for safe abortion. Although abortion is a medical procedure, its legal status in many countries has been incorporated in penal codes which specify grounds in which abortion is permitted or prohibited.

Over the past two decades, many low- and middle-income countries (LMICs) have reformed their abortion laws. Most have expanded the grounds on which abortion may be performed legally, while a few have restricted access. Abortion law reforms are hypothesized to influence reproductive, maternal, and neonatal health services and health outcomes, as well as health inequalities. However, evidence on the impact of abortion reforms in LMICs is limited. In addition, abortion reforms may have differential impacts even in countries that experience similar reforms, as the legislative processes that are associated with changing abortion laws occur in highly distinct political, economic, religious, and social contexts. This variation may contribute to heterogeneity in the effects of abortion law reforms across countries.

The **first manuscript** of this thesis presents a systematic review of the literature on the impact of abortion reforms on health services and health outcomes in LMICs. This review highlighted some key limitations and knowledge gaps. First, few available studies can inform causal inference concerning the impact of abortion reforms, due to inherent weaknesses in the study designs implemented and the lack of bias analyses to assess the robustness of empirical findings. Second, most studies have focused on health services, with few examining health

outcomes plausibly affected by abortion reforms, such as neonatal mortality and contraceptive use. Third, few studies have examined whether impacts of abortion reforms are heterogeneous, and whether they may exacerbate socioeconomic inequalities in health outcomes.

The **second and third manuscripts** present estimates of the impact of abortion reforms in the Dominican Republic and Mozambique, including how these associations vary across socioeconomic groups. Using a difference-in-differences design, the second manuscript assesses the effect of a reform that restricted access to abortion in the Dominican Republic on neonatal mortality and contraceptive use. Abortion restriction was associated with an additional 6.3 (95% CI=2.1, 10.5) neonatal deaths per 1,000 live births and a 9.6 (95% CI=4.2, 15.0) percentage-point decrease in modern contraceptive use. The third manuscript assesses the effect of a reform that decriminalized access to abortion in Mozambique on neonatal mortality. Abortion decriminalization was associated with an additional 5.6 (95% CI=1.3, 9.9) neonatal deaths per 1,000 live births. This increase in neonatal mortality following abortion decriminalization may be attributable to the delay in providing clear guidelines and implementing safe abortion services.

The **last manuscript** is a methodological study that quantifies and corrects for potential misclassification bias of neonatal deaths and stillbirths when using national household surveys to estimate the impacts of population level interventions. I applied the MC-SIMEX (Misclassification Simulation Extrapolation) approach to examine the impact of varying degrees of misclassification error when evaluating the association between abortion reforms and neonatal mortality. Our results suggest that the naive estimates of the impact of abortion law

reforms in the Dominican Republic and Mozambique may have been underestimated due to the misclassification of neonatal deaths.

Overall, results from our empirical evaluations suggest that reforms that restrict abortion access may worsen health outcomes and exacerbate socioeconomic inequalities. However, reforms that decriminalize abortion access without attendant investment, implementation and enforcement may not be sufficient for improving abortion access and health outcomes. The collective findings presented in this thesis, by addressing important methodological limitations of existing studies, helps to fill a gap in the literature regarding the impact of abortion reforms in LMICs and strengthen the evidence-base for informing abortion legislation.

Résumé

L'avortement à risque reste un défi majeur de santé publique au niveau mondial et contribue chaque année à un nombre effarant de décès et d'invalidités. L'accès à des services d'avortement sans risque est influencé par un large éventail de facteurs, dont l'environnement législatif. La loi sur l'avortement d'un pays est un élément clé pour déterminer l'environnement favorable à l'avortement sans risque. Bien que l'avortement soit une procédure médicale, son statut juridique dans de nombreux pays a été incorporé dans les codes pénaux qui spécifient les motifs dans lesquels l'avortement est autorisé ou interdit.

Au cours des deux dernières décennies, de nombreux pays à revenu faible ou intermédiaire (PRFM) ont réformé leurs lois sur l'avortement. La plupart ont élargi les motifs pour lesquels l'avortement peut être pratiqué légalement, tandis que quelques-uns en ont restreint l'accès. On suppose que les réformes de la loi sur l'avortement influencent les services de santé génésique, maternelle et néonatale, les résultats sanitaires et les inégalités en matière de santé. Cependant, les preuves de l'impact des réformes de l'avortement dans les PRFM sont limitées. De plus, les réformes de l'avortement peuvent avoir des impacts différents même dans les pays qui connaissent des réformes similaires, car les processus législatifs associés au changement des lois sur l'avortement se déroulent dans des contextes politiques, économiques, religieux et sociaux très distincts. Cette variation peut contribuer à l'hétérogénéité des effets des réformes de la loi sur l'avortement dans les différents pays.

Le **premier manuscrit** de cette thèse présente une revue systématique de la littérature sur l'impact des réformes de l'avortement sur les services de santé et les résultats sanitaires dans les PRFM. Cette revue a mis en évidence certaines limitations et lacunes importantes en

matière de connaissances. Premièrement, peu d'études peuvent informer l'inférence causale concernant l'impact des réformes de l'avortement, en raison du manque de conceptions d'études robustes et d'analyses de biais pour évaluer la robustesse de leurs résultats. Deuxièmement, peu d'études ont évalué les résultats de santé plausiblement affectés par les réformes de l'avortement, tels que la mortalité néonatale et l'utilisation de contraceptifs. Troisièmement, peu d'études ont examiné si les impacts des réformes de l'avortement sont hétérogènes et s'ils peuvent exacerber les inégalités socio-économiques dans les résultats de santé.

Les **deuxième et troisième manuscrits** présentent des estimations de l'impact des réformes de l'avortement en République dominicaine et au Mozambique, y compris la manière dont ces associations varient selon les groupes socio-économiques. En utilisant un modèle de différence dans les différences, le deuxième manuscrit évalue l'effet d'une réforme qui restreint l'accès à l'avortement en République dominicaine sur la mortalité néonatale et l'utilisation de contraceptifs. La restriction de l'avortement a été associée à 6,3 décès néonatals supplémentaires pour 1 000 naissances vivantes et à une diminution de 9,6 points de pourcentage de l'utilisation des contraceptifs modernes. Le troisième manuscrit évalue l'effet d'une réforme qui dépénalise l'accès à l'avortement au Mozambique sur la mortalité néonatale. La dépénalisation de l'avortement a été associée à 5,6 décès néonatals supplémentaires pour 1 000 naissances vivantes.

Le **dernier manuscrit** est une étude méthodologique qui examine la classification erronée de la mortalité néonatale lors de l'utilisation d'enquêtes nationales auprès des ménages dans les PRFM. J'ai appliqué l'approche MC-SIMEX (Misclassification Simulation

Extrapolation) pour comprendre l'impact de différents degrés d'erreur de classification lors de l'évaluation de l'association entre les réformes de l'avortement et la mortalité néonatale.

Dans l'ensemble, les résultats suggèrent que les réformes qui restreignent l'accès à l'avortement peuvent détériorer les résultats sanitaires et exacerber les inégalités socio-économiques. Cependant, les réformes qui dépénalisent l'accès à l'avortement sans une mise en œuvre et une application adéquates peuvent ne pas suffire à améliorer de manière substantielle l'accès à l'avortement et les résultats sanitaires.

En conclusion, les résultats collectifs présentés dans ce travail comblent une lacune dans la littérature concernant l'impact des réformes de l'avortement dans les PRFM, en particulier les limitations méthodologiques importantes des études existantes et renforcent la base de données probantes pour informer la législation sur l'avortement.

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Contribution of authors

The individual manuscripts that constitute this thesis and the respective contributions of the co-authors are listed below. My doctoral study led to the production of five manuscripts, of which four are presented in this thesis. The data used in the analyses are from national household surveys and are outlined in greater detail in the methods overview and each manuscript. I developed the research questions, research plan, and study design for all the included manuscripts. I received substantive support from my academic supervisor, Dr. Arijit Nandi, who provided indispensable guidance for the protocol development, assisted me to refine the research questions, and helped me to build suitable analysis plans for all manuscripts. I was solely responsible for collecting and merging the data, developing the statistical functions or methodological modifications to conduct the analyses, and generating the manuscripts.

My committee members, Dr. Britt McKinnon, Assistant Professor at the Division of Epidemiology, Dalla Lana School of Public Health, University of Toronto and Dr. Seungmi Yang, Assistant Professor at the Department of Epidemiology, Biostatistics and Occupational Health, McGill University, contributed to Manuscript 2.

Dr. Carmeliza Rosario, an Assistant Professor at the Department of Global Public Health, University of Bergen, Norway; and Sally Griffin, Director of the International Centre for Reproductive Health in Mozambique, contributed to Manuscript 3.

Dr. Mabel Carabali, Assistant Professor at Département de médecine sociale et preventive, École de santé publique, Universite de Montreal contributed to Manuscript 4.

Dr. Vivian Ukah, postdoctoral fellow at the Department of Epidemiology, Biostatistics and Occupational Health; and Dr. Babatunde Alli, a dentist and doctoral student at the Department of Dentistry, McGill University, contributed to Manuscript 1.

Manuscript 1: Ishola F, Ukah UV, Alli BY, Nandi A. "Impact of abortion law reforms on health services and health outcomes in low- and middle-income countries: a systematic review". Health Policy Plan. 2021;36(9):1483-98.

I reviewed the literature, conceived and designed the study, performed the data management, risk of bias and quality assessment, interpreted the results and wrote the original manuscript. Dr. Ukah and Dr. Alli provided substantive input on the search strategy, data management, and risk of bias and quality assessment. Dr. Nandi guided the epidemiological design and contributed substantially to the interpretation of the results. All authors provided critical feedback and approved the final version of the manuscript.

Manuscript 2: Ishola F, McKinnon B, Yang S, Nandi A. "Effect of abortion restriction in Dominican Republic on neonatal mortality and contraceptive use". Submission planned for BMJ Sexual and Reproductive Health

I reviewed the literature on abortion restriction in Dominican Republic, conceived and designed the study, conducted data collection, data management, developing the statistical functions for the application of the analytical methods in this context, completed the data analysis, interpreted the results, and wrote the original manuscript. Dr. McKinnon and Dr. Yang provided substantive analytical and methodological inputs for the assessment and

interpretation of the results. Dr. Nandi guided the epidemiological design and contributed substantially to the analysis and interpretation of the results. All authors provided critical feedback and approved the final version of the manuscript.

Manuscript 3: Ishola F, Rosario C, Griffin S, Nandi A. "Effect of abortion decriminalization in Mozambique on neonatal mortality". Submission planned for BMC Health Services Research

I reviewed the literature on abortion decriminalization in Mozambique, conceived and designed the study, conducted data collection, data management, completed the data analysis, interpreted the results, and wrote the original manuscript. Dr. Rosario and Sally Griffin provided substantive knowledge of the abortion policy milieu in Mozambique and contributed to the interpretation of the results. Dr. Nandi guided the epidemiological design and contributed substantially to the analysis and interpretation of the results. All authors provided critical feedback and approved the final version of the manuscript.

Manuscript 4: Ishola F, Carabali M, Nandi A. "Correcting impact evaluation studies for potential misclassification using Misclassification Simulation Extrapolation (MC-SIMEX)". Submission planned for Epidemiology Journal

I conceived and designed the study, developed the statistical functions for the application of the methods in this context, completed the data analysis, interpreted the results, and wrote the original manuscript. Dr. Carabali provided methodological input for the analysis and interpretation of results. Dr. Nandi guided the epidemiological analysis and contributed

substantially to the interpretation of the results. All authors provided critical feedback and approved the final version of the manuscript.

The manuscript not included in this thesis is the protocol for the systematic review presented in manuscript 1. It is published in the journal *Systematic Reviews*.

Ishola F, Ukah UV, Nandi A. Impact of abortion law reforms on women's health services and outcomes: a systematic review protocol. Syst Rev. 2021;10(1):192.

Statement of originality

The work presented in this dissertation thesis is based on original and timely contributions to the impact evaluation of abortion law reforms in LMICs. I received indispensable support and guidance from my supervisor and thesis advisory committee members, but the research questions, objectives, and the analyses performed to answer my overall research goal are of my own. I identified research gaps through the systematic review of impact evaluation of abortion reforms in LMICs, applied quasi-experimental methods to address them and quantified the role of misclassification on these estimates.

In Manuscript 1, I systematically reviewed and synthesized evidence on the impact of abortion reforms on health services and outcomes in LMICS. In Manuscripts 2 and 3, I applied a difference-in-differences design to assess the impact of abortion reforms in the Dominican Republic and Mozambique, including their effect across socioeconomic dimensions. Manuscript 4 was framed as a practice of epidemiology paper, to quantify and correct misclassification error in impact evaluation studies of population level interventions using the MC-SIMEX technique. This approach has not been applied to estimates derived from a difference-in-differences evaluation study to account for potential misclassification bias.

In addition to synthesizing existing studies and generating new evidence, this dissertation offers a novel application of existent methods to address potential misclassification bias and improve the quality of impact evaluation studies of abortion reforms using household survey data.

List of abbreviations

DHS	Demographic Health Survey
DD	Difference-in-Differences
GRADE	Grading of Recommendations Assessment, Development and Evaluation
GDP	Gross Domestic Product
INEGI	Instituto Nacional de Estadística, Geografia e Informática
ITS	Interrupted Time Series
LMICS	Low- and Middle-Income Countries
MC-SIMEX	Misclassification Simulation Extrapolation
MICS	Multiple Indicator Cluster Survey
MMR	Maternal Mortality Ratio
NMR	Neonatal Mortality Rate
P2C	Polychoric Dual-Component
PPP	Purchasing Power Parity
PIS	Perinatal Information System

PRISMA	Preferred Reporting Items for Systematic reviews and Meta-Analyses
PROSPERO	International prospective register of systematic reviews
RD	Regression Discontinuity
ROBINS-I	The Risk of Bias in Non-randomized Studies of Interventions
RRHS	Romanian Reproductive Health Survey
SC	Synthetic Control
SD	Standard Deviation
SES	Socioeconomic Status
SMR	Standardized mortality ratio
SSRN	Social Science Research Network
VTP	Voluntary Termination of Pregnancy
WHO	World Health Organization

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CHAPTER 1. Introduction

An estimated 25.1 million unsafe abortions occur each year, with 97 percent of them taking place in developing countries (1-3). According to the World Health Organization (WHO), about 75% of all abortions are considered unsafe in Latin America and Africa (1, 4). When carried out using WHO recommended methods appropriate to the pregnancy duration, and by someone with the necessary skills, abortion is a safe health care intervention. They become unsafe when the lack of access to safe services leads women to end a pregnancy under conditions that pose a risk to their health (5, 6). Unsafe abortion can lead to major complications such as hemorrhage and septicemia, chronic morbidity such as pelvic pain and infertility, and maternal mortality. Women who are less educated, poor, young, or otherwise marginalized are often disproportionately affected.

Abortion laws are major determinants of safe abortion availability (7, 8). Although abortion is a medical procedure, its legal status in many countries has been incorporated into penal codes that specify when abortion is permitted. About 42% of women of reproductive age live in countries with highly restrictive abortion laws and 93% of these women are in LMICs (2, 9). While restrictive abortion laws still prevail in most LMICs, many countries have reformed their abortion laws, expanding the grounds on which abortion can be performed legally (7, 8, 10-12). Often, there are discrepancies between the written law and what is applicable. On the one hand, restrictive abortion laws may be minimally applied; on the other hand, countries that permit abortion on broad grounds may limit access to safe abortion services due to religious and social stigma, poor implementation, regulation of facilities that are authorized to provide

abortions, mandatory waiting periods, reporting requirements, and lack of health care resources and trained personnel (13-17).

Abortion law reforms are hypothesized to influence women's access to and utilization of safe abortion, as well as reproductive, maternal, and neonatal health outcomes (8, 15, 18-20). Researchers have also emphasized the integral role that abortion reforms may play in shaping health inequities given the potentially heterogeneous relationship between abortion reforms and health by race/ethnicity, education, income level, and other socio-demographic characteristics (21-23). Estimating the causal impact of abortion reforms, as well as their differential effects, is important for understanding how these laws impact health outcomes and health inequities, and for informing evidence-based public health policy.

Evidence on the causal effects of abortion reforms in LMICs is limited (24-27). Most extant literature on the impacts of abortion reforms is derived from high-income countries (HICs). For example, several studies have leveraged state-level restrictions to abortion access as a natural experiment to analyze their effects on fertility, abortion rates, and contraceptive use in the US (28-31). However, due to the variation in reforms and differences in levels of implementation and enforcement cross-nationally, evidence from these evaluation studies may be limited in terms of their generalizability to other contexts (12, 32, 33).

Evaluating the effect of population-level interventions such as abortion law reforms requires a design that permits inference regarding the causal effect of abortion legislation by addressing sources of unmeasured confounding such as time-fixed differences between treatment groups (e.g., treated and control jurisdictions) and secular trends in outcomes (34-36). Randomized controlled trials are designed to address potential confounding bias by

generating exchangeable treatment groups but are infeasible given that abortion policies are unlikely to be randomly assigned. As such, quasi-experimental designs are often utilized since they provide an identification strategy that explicitly address sources of confounding bias, although stronger assumptions are needed for a causal interpretation of treatment effects vis-à-vis a randomized controlled trial framework.

1.1 Conceptual Framework

Abortion law reforms are hypothesized to influence women's access to and utilization of safe abortion, as well as reproductive and health outcomes, including contraceptive use and neonatal outcomes (8, 15, 18).

1.1.1 How do abortion reforms impact neonatal health?

Restrictive abortion laws may contribute to adverse birth outcomes through a variety of channels. First, the inability to access comprehensive abortion care because of restrictive abortion legislation forces pregnant women to continue with unwanted pregnancies and adds to increased psychosocial stress as well as anxiety among pregnant women (37, 38). Studies have reported greater pregnancy complications, including pre-eclampsia, and adverse birth outcomes among women with psychosocial stressors such as anxiety and depression during pregnancy (39-42).

Second, women with unwanted pregnancies may face additional structural barriers in the health care system, such as lack of health coverage, shortages of physicians, and inability to access affordable, comprehensive, and culturally appropriate care, which may contribute to

adverse pregnancy outcomes (23, 43-46). Studies have reported that women with unwanted pregnancies are more likely to avoid prenatal care, decline folic acid or multivitamins, smoke cigarettes, consume alcohol, and use illicit drugs, resulting in adverse maternal, fetal, and neonatal outcomes (40, 47-49).

Third, countries with restrictive abortion laws typically have fewer policies promoting the health and well-being of pregnant women, their children, and their families, such as pregnancy coverage and extended family/medical leave. Inadequate access to comprehensive reproductive health services, including and beyond abortion care, and supportive social policies, limit the reproductive autonomy of women and endanger their health (19, 50-52).

1.1.2 How do abortion reforms impact contraceptive use?

With regards to contraceptive use, it is hypothesized that women who live where abortion is not widely accessible—for instance, because of limited provider access or restrictive abortion policies—will be more inclined to use contraceptives, if available, to avoid unplanned pregnancy, compared to women with greater access (53).

On the other hand, if abortion is decriminalized and widely accessible, it could act like a form of insurance for unplanned pregnancy, making women less inclined to use a more effective contraceptive method to avoid pregnancy (54). In contexts where abortion is legal, others have suggested that post-abortion contraceptive counselling can increase the use of modern methods of contraception (55, 56).

Figure 1.1. Hypothesized relationship between restrictive abortion reform and neonatal health

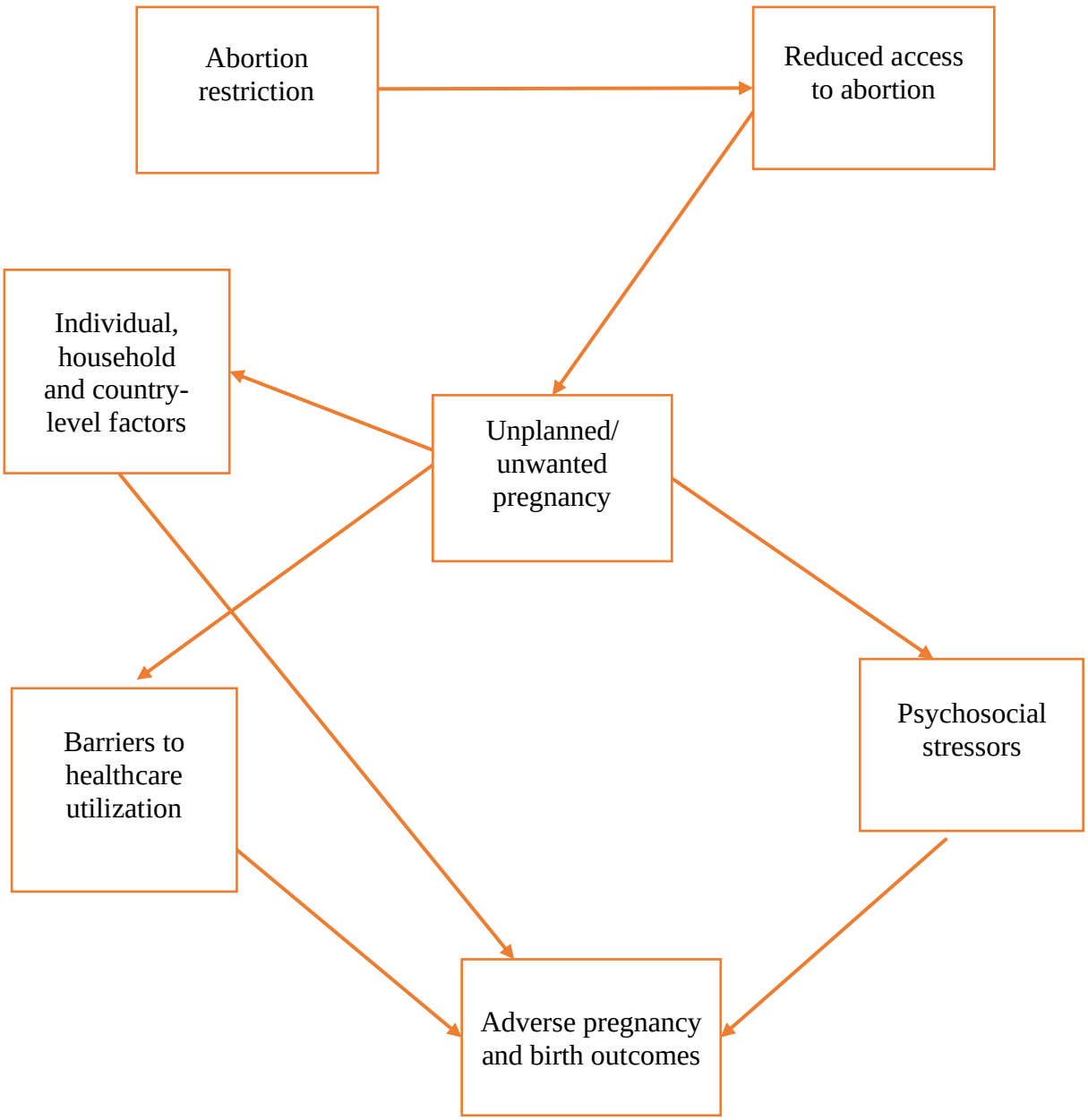
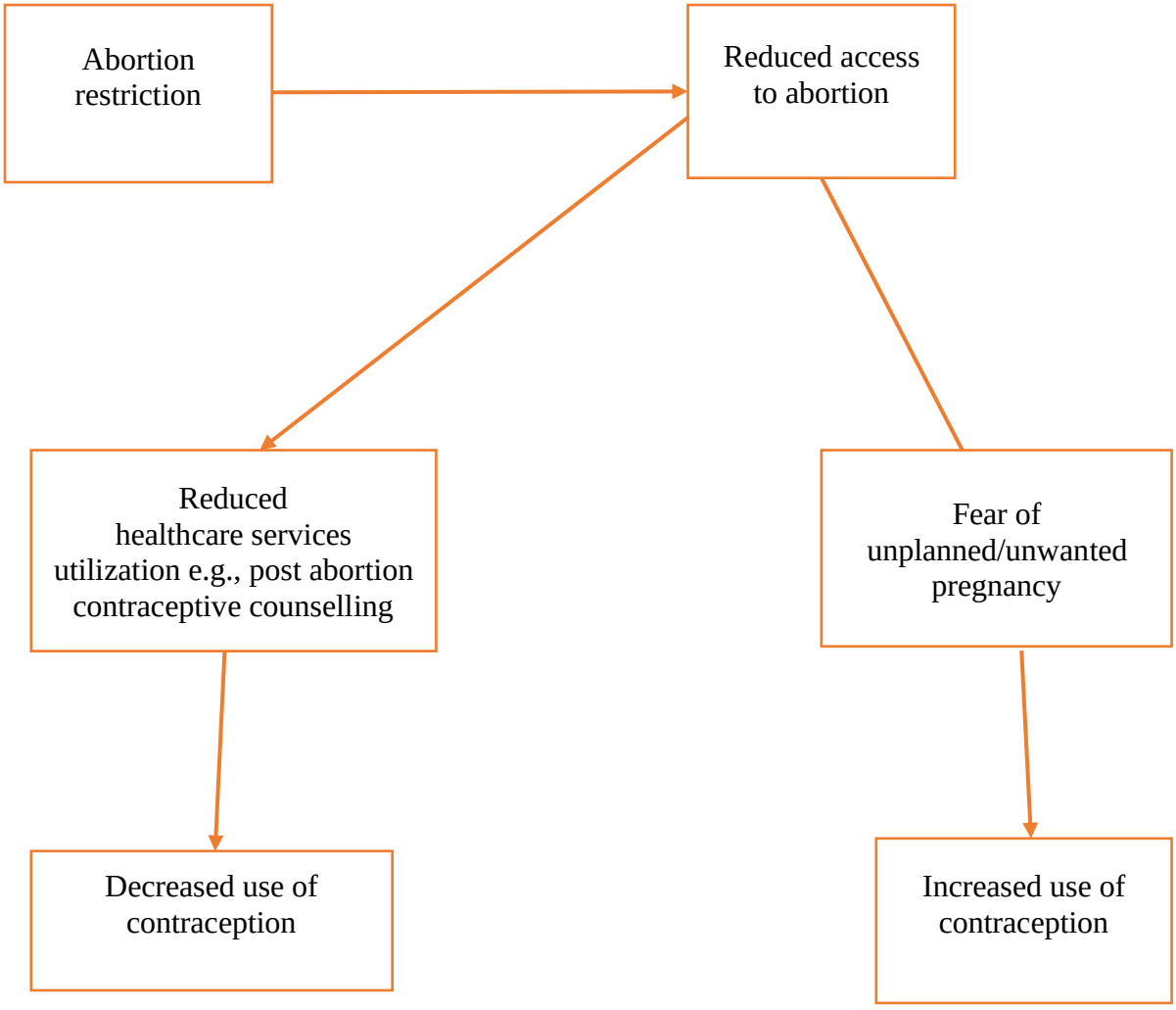


Figure 1.2. Hypothesized relationship between restrictive abortion reform and contraceptive use



1.2 Research Objectives

The overarching goal of this doctoral dissertation was to generate relevant evidence on the impacts of abortion law reforms on reproductive and neonatal health outcomes in LMICs. My specific research objectives were to:

Objective 1: Systematically review and synthesize empirical studies that evaluate the effect of abortion policy reforms on reproductive and neonatal health outcomes in LMICs.

Objective 2: Assess the effect of a policy reform that restricted access to abortion on the probabilities of modern contraceptive use and neonatal death, using the Dominican Republic as a case study.

Objective 3: Assess the effect of a policy reform that expanded access to abortion on the probability of neonatal death using Mozambique as a case study.

Objective 4: Demonstrate how to correct estimates of the impact of abortion reforms for potential misclassification of neonatal mortality using Misclassification Simulation Extrapolation (MC-SIMEX).

1.3 Thesis Structure

This thesis is manuscript-based and contains seven chapters with four corresponding to original research manuscripts. Each manuscript chapter begins with a preface that explains the rationale for the chapter, the research question(s) addressed and the relationship to the thesis objectives. In chapter 1, I present the overarching thesis rationale and state my research objectives. Chapter 2 consists of a systematic literature review (manuscript 1). Chapter 3 briefly

presents the data source, design, and analytical methods I used to complete my thesis objectives. In Chapter 4, I evaluate the impact of a reform that restricted abortion using Dominican Republic as a case study (manuscript 2). In chapter 5, I assess the impact of a reform that decriminalized abortion using Mozambique as a case study (manuscript 3). In chapter 6, I correct estimates of the impact of abortion reforms for misclassification of neonatal mortality using Misclassification Simulation Extrapolation (MC-SIMEX) (manuscript 4). Chapter 7 discusses the overall findings of this thesis, its implications, and future directions, and makes concluding remarks. References to the documents (articles, book chapters, reports, and webpages) cited in this work are provided at the end.

CHAPTER 2. Impact of abortion law reforms on health services and health outcomes in low- and middle-income countries: a systematic review

2.1 Preface: Manuscript 1

Systematic reviews are comprehensive attempts to gather and synthesise data that meets pre-determined eligibility criteria in order to address a specific research issue (53). My methodology was documented a priori in a protocol that was registered in the International Prospective Register of Systematic Reviews (PROSPERO) database CRD42019126927 and published in the journal, *Systematic Reviews*, to reduce bias in the evaluation of available evidence(57).

This review addresses the first objective of my thesis, which is to synthesize evidence on the impact of abortion reforms in LMICs. It was presented as a poster at the *Society for Epidemiological Research (SER)* Conference (December 2020) and published in the Journal of *Health Policy and Planning*(58).

2.2 Manuscript 1

Title: Impact of abortion law reforms on health services and health outcomes in low- and middle-income countries: A systematic review

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Abstract

Background: While restrictive abortion laws still prevail in most low- and middle-income countries (LMICs), many countries have reformed their abortion laws, expanding the grounds on which abortion can be performed legally. However, the implications of these reforms on women's access to and use of health services, as well as their health outcomes, is uncertain. This systematic review aimed to evaluate and synthesize empirical research evidence concerning the effects of abortion law reforms on women's health services and health outcomes in LMICs.

Methods: We searched Medline, Embase, CINAHL, and Web of Science databases, as well as grey literature and reference lists of included studies. We included pre-post and quasi-experimental studies that aimed to estimate the causal effect of a change in abortion law on at least one of four outcomes: (i) use of and access to abortion services, (ii) fertility rates, (iii) maternal and/or neonatal morbidity and mortality, and (iv) contraceptive use. We assessed the quality of studies using the quasi-experimental study design series checklist and synthesized evidence through a narrative description.

Results: Of the 2796 records identified by our search, we included thirteen studies in the review, which covered reforms occurring in Uruguay, Ethiopia, Mexico, Nepal, Chile, Romania, India, and Ghana. Studies employed pre-post, interrupted time series, difference-in-differences, and synthetic control designs. Legislative reforms from highly restrictive to relatively liberal were associated with reductions in fertility, particularly among women 20 to 34 years of age, as well as lower maternal mortality.

Conclusion: Evidence regarding the impact of abortion reforms on other outcomes, as well as whether effects vary by socioeconomic status, is limited. Further research is required to strengthen the evidence-base for informing abortion legislation in LMICs. This review explicitly points to the need for rigorous quasi-experimental studies with sensitivity analyses to assess underlying assumptions.

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Keywords: Abortion, access, health services, policy evaluation, impact

INTRODUCTION

Unsafe abortion remains a major global public health challenge, particularly in developing countries, which account for 97% of the estimated 25·1 million unsafe abortions each year(1-5). According to the World health Organization (WHO), between 4.7% and 13.2% of maternal deaths can be attributed to unsafe abortion, with the majority of these also occurring in developing countries (5, 6). Approximately 7 million women are admitted to hospitals every year due to complications from unsafe abortion such as hemorrhage, infections, septic shock, uterine and intestinal perforation, and peritonitis (7-9).

Lack of access to safe abortion may force women to continue with unplanned pregnancies (10-12). Studies have reported that women with unplanned pregnancies were more likely to avoid prenatal care, decline folic acid or multivitamins, smoke cigarettes, consume alcohol, and use illicit drugs, resulting in adverse maternal, fetal, and neonatal outcomes (13-16). Children from unplanned or unwanted pregnancies are at greater risk of poorer health, lower school performance and psychosomatic problems (17).

Access to safe abortion services is often limited by a wide range of barriers (18-21). A recent systematic review on the barriers to abortion access in low- and middle-income countries (LMICs) implicated the following factors: restrictive abortion laws, lack of knowledge about abortion law or locations that provide abortion, high cost of services, judgmental provider attitudes, scarcity of facilities and medical equipment, poor training and shortage of staff, stigma on social and religious grounds and lack of decision-making power (20).

An important factor regulating access to abortion is abortion law (20, 22, 23). Although abortion is a medical procedure, its legal status in many countries has been incorporated into

penal codes which specify when abortion is permitted. These include: prohibition in all circumstances; to save the woman's life; to preserve the woman's health; in cases of rape, incest, and fetal impairment; for economic or social reasons; and on request with no requirement for justification (22-24).

Although abortion laws in different countries are usually compared based on the grounds under which legal abortions are allowed, these comparisons rarely take into account components of the legal framework that may have strongly restrictive implications, such as regulation of facilities that are authorized to provide abortions, mandatory waiting periods, reporting requirements in cases of rape, limited choice in terms of the method of abortion, and requirements for third-party authorizations(21, 23, 25, 26). For example, the Zambian Termination of Pregnancy Act permits abortion on socio-economic grounds. It is considered liberal, as it permits legal abortions for more indications than most countries in Sub-Saharan Africa; however, abortions must only be provided in registered hospitals, and three medical doctors - one of whom must be a specialist - must provide signatures to allow the procedure to take place (26). Given the critical shortage of doctors in Zambia (27), this is in fact a major restriction that is only captured by a thorough analysis of the conditions under which abortion services are provided.

Additionally, abortion laws may exist outside the penal codes in some countries, where they are supplemented by health legislation and regulations such as public health statutes, reproductive health acts, court decisions, medical ethic codes, practice guidelines, and general health acts (22, 23, 28). This diversity of regulatory documents may lead to conflicting directives about the grounds under which abortion is lawful (23). For example, in Kenya and Uganda,

standards and guidelines supported by the constitution contradicted the penal code, leaving room for an ambiguous interpretation of the legal environment (29). Such discordance in legislation affects health-care access to and provision of comprehensive abortion care.

Regulations restricting the range of abortion methods from which women can choose, including medication abortion in particular, may also affect abortion access (30, 31). A literature review contextualizing medication abortion in seven African countries reported that incidence of medication abortion is low despite being a safe, effective, and low-cost abortion method, likely due to legal restrictions on access to the medications (31).

Over the past two decades, many LMICs have reformed their abortion laws (32, 33). Most have expanded the grounds on which abortion may be performed legally, while a few have restricted access. Countries like Uruguay and South Africa have amended their laws to allow abortion on request in the first trimester of pregnancy (34, 35). Conversely, in Nicaragua, a law to ban all abortion without any exception was introduced in 2006 (36).

Abortion law reforms are hypothesized to influence women's access to and utilization of safe abortion, as well as reproductive and health outcomes, including fertility, contraceptive use, abortion related maternal morbidity and mortality, and pregnancy and birth outcomes (20, 34, 37). The implementation of more restrictive abortion laws is hypothesized to reduce access to safe abortion and decrease abortion rates (38, 39). However, some have theorized that abortion restriction could reduce access without decreasing abortion rates since most women faced with an unplanned pregnancy will resort to unsafe abortion, irrespective of the law (40).

With regards to contraceptive use, it is hypothesized that a woman who lives where abortion is not widely accessible—for instance, because of limited provider access or restrictive

abortion policies—will be more inclined to use contraceptives, if available, to avoid unplanned pregnancy, compared to a woman with greater access (41). On the other hand, if abortion is decriminalized and widely accessible, it could act like a form of insurance for unplanned pregnancy, making women less inclined to use a more effective contraceptive method to avoid pregnancy (42). In contexts where abortion is legal, others have suggested that post-abortion contraceptive counselling can increase the use of modern methods of contraception (43, 44).

It has also been theorized that transition to a more restrictive abortion context may force women to continue with unplanned pregnancies (10, 11). Studies have reported that women with unplanned pregnancies were more likely to avoid prenatal care, decline folic acid or multivitamins, smoke cigarettes, consume alcohol, and use illicit drugs, resulting in adverse maternal, fetal and neonatal outcomes (13-16).

Abortion law reforms may have heterogeneous effects. They may yield different outcomes even in countries that experience similar reforms, as the legislative processes that are associated with changing abortion laws take place in highly distinct political, economic and social contexts (23, 25, 26). For example, differences in the regulation of facilities that are authorized to provide abortions, mandatory waiting periods, availability of qualified doctors and well-equipped facilities, and choice in terms of the method of abortion (i.e., medication abortion) may influence the effect of abortion reforms on related outcomes.

Extant empirical literature has examined changes in abortion-related morbidity and mortality, contraceptive usage, fertility and other health-related outcomes following reforms to abortion laws (45-47). To the best of our knowledge, however, the growing literature on the impact of abortion law reforms on women's health services and health outcomes in LMICs has

not been systematically reviewed. A study by Benson et al. evaluated evidence on the impact of abortion policy reforms on maternal death in three countries, Romania, South Africa and Bangladesh, where reforms were immediately followed by strategies to implement abortion services, scale up accessibility and establish complementary reproductive and maternal health services (48). However, the review focused on a selection of countries that have enacted similar reforms and it is unclear if its conclusions are more widely generalizable. We therefore conducted a systematic review of evidence on the causal effect of abortion law reforms on women's health, including their access to and use of health services, as well as maternal and neonatal health outcomes, in LMICs. Through this review, we hope to synthesize evidence while identifying research gaps, in order to inform decision-making and future research concerning the impact of abortion reforms.

METHODS

Search strategy

We searched Medline, Embase, CINAHL and Web of Science databases from inception until February 27, 2019. There were no language, date or year restrictions. We also searched the grey literature using Google and Social Science Research Network (SSRN) and reference lists of included studies for further relevant literature. Search terms combining subject headings (i.e., MeSH) and keywords were developed for each electronic database. Full search terms are provided in the Supplementary Table 2.1. We followed the PRISMA guidelines throughout our

review (49). The review protocol was registered in the International Prospective Register of Systematic Reviews (PROSPERO) database CRD42019126927.

Inclusion criteria

We included pre-post studies and more robust quasi-experimental designs, such as interrupted time series (ITS), differences-in-differences (DD), synthetic control (SC) and regression discontinuity (RD) designs. We focused on quasi-experimental designs in order to inform inference regarding the causal effect of abortion legislation. We also included randomized controlled trials or cluster randomized trials. We excluded other observational studies (i.e., cross-sectional studies, cohort studies, and case-control studies), reviews/meta-analyses, qualitative studies, editorials, letters, book reviews, correspondence, and case reports/studies. Supplementary Table 2.2 provides the definition of the included quasi experimental designs and their level of evidence.

Population

Studies that included women of reproductive age (15–49 years) or neonates (≤1 month) in LMICs were eligible for inclusion. We used the country classification specified in the World Bank Data Catalogue to identify LMICs.

Intervention

The exposure of interest was any change in abortion law or policy, either from a restrictive policy to a non-restrictive or less restrictive one, or vice versa. It also included policies occurring outside the penal code with legal standing, such as: national constitutions; supreme court decisions; customary or religious law; and regulatory standards and guidelines governing the

provision of abortion. We did not include studies investigating the consequences of individual-level access to or use of abortion services.

Comparator

Studies with a comparison or control period or group (for example, outcome trends in countries that did not experience a change in abortion law) were eligible for inclusion.

Outcome measures

The prespecified primary outcomes were use of abortion services and access to abortion services. Use of abortion services was defined as induced pregnancy termination, including medication abortion. Access to abortion was defined based on the review by Dennis et al. (50) which includes affordability (gaining entry into the healthcare system without financial barriers); geographic availability (distribution of safe and affordable abortion services); competent care (sufficient capacity of provider cadres to provide high-quality services); lack of stigma, religious or sociocultural barriers and knowledge and information (women's awareness and perception of legal status) (50). Secondary outcomes were fertility rates, maternal morbidity and mortality, neonatal morbidity and mortality, and contraceptive use (including current use, future use, demand, and unmet need for family planning).

Data management and analysis

Two authors screened titles and abstracts and assessed eligibility of each full-text article. Any discordance was reviewed by a third author before a final decision was made to include or exclude the paper. Two reviewers used a piloted standardized extraction form to extract information on authors, date of publication, country of study, type and year of policy reform,

data source, sample, comparison group, type of outcomes, evaluation design, qualitative conclusion, and information to assess risk of bias (sensitivity analyses). We synthesized the main findings through a narrative description. Heterogeneity in policy changes, outcomes, and study designs precluded a quantitative meta-analysis of the study results.

Risk of bias and quality assessment

To assess the risk of bias in each study, two authors performed a quality assessment independently using the quasi-experimental study designs series risk of bias checklist (51). This checklist provides criteria for grading the quality of quasi-experimental studies that relate directly to the intrinsic strength of the studies in inferring causality (e.g., control for confounding). An overview of the risk of bias in each study and their corresponding scores is presented in Table 2.1. Each of the six criteria was assessed separately and categorized with a 'yes', 'no' or 'possibly'. A study was judged to be at "low risk of bias" if at least 4 of the six criteria were met and "high risk of bias" otherwise. Disagreements were discussed until a consensus was reached, with consultation with a third author if needed. In addition, we assessed the methodological strengths, limitations, and potential for biases that could result from unmeasured confounding, under reporting and measurement error. We also evaluated the quality of the collective evidence using Grading of Recommendations Assessment, Development and Evaluation (GRADE) working group methodology (Supplementary Table 2.3) (52).

Table 2.1. Risk of bias assessment

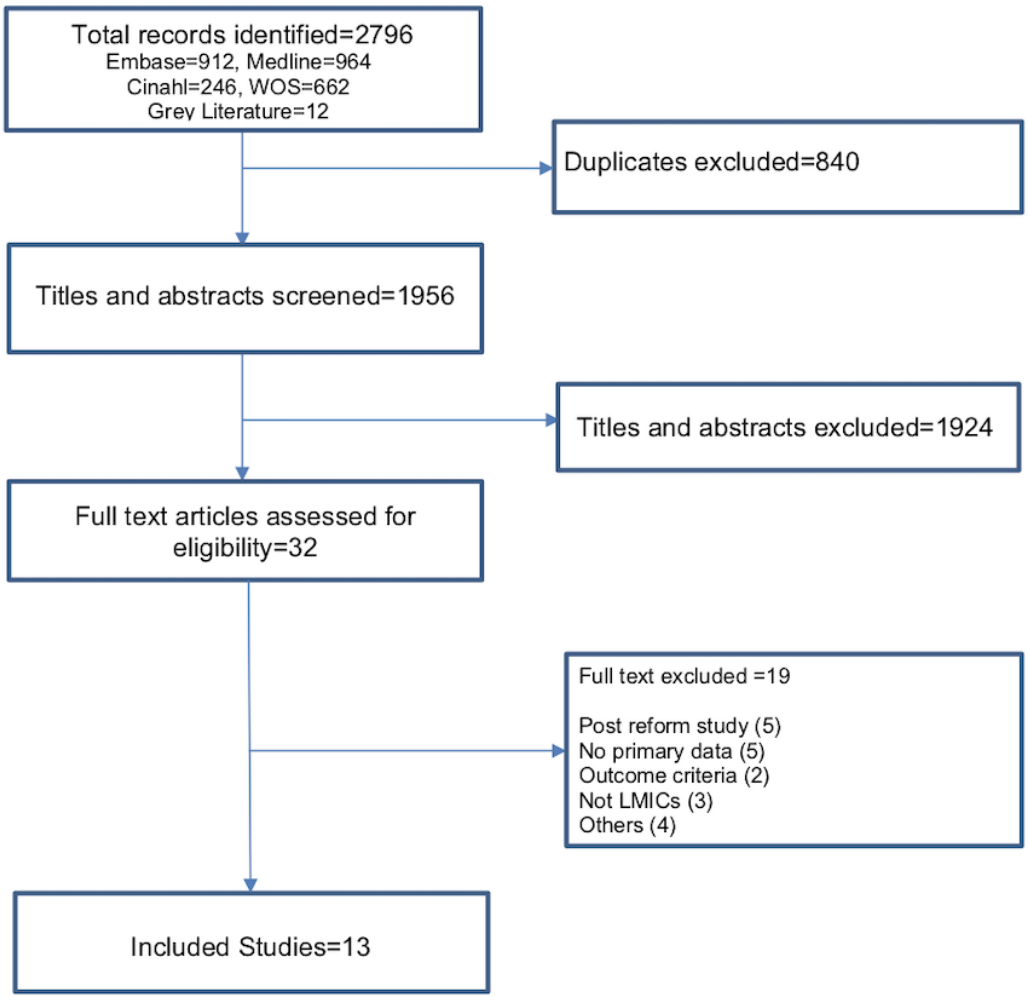
Citation	Relevant Comparison	Outcome assessment	Intervention effect estimation	Control for confounding			Groups of individuals or clusters formed with time or location differences	Baseline assessment of outcome variables	Risk of Bias
				Accounts for unmeasured time fixed confounders	Accounts for shared secular trends	Control for time varying confounders			
(Anton et al 2016)	Possibly	Before and after only	Change over time	Yes	No	No	Yes	N/A	High
(Anton et al 2018)	Yes	Multiple time points before and after	Change over time and difference between groups	Yes	Yes	Yes	Yes	N/A	Low
(Nozar et al. 2016)	Possibly	Before and after only	Change over time	Yes	No	No	Yes	N/A	High
(Mitrut and Wolff 2011)	Yes	Multiple time points before and after	Change over time and difference between groups	Yes	Yes	No	Yes	N/A	Low
(Serbanescu et al. 1995)	Possibly	Before and after only	Change over time	Yes	No	No	Yes	N/A	High
(Legge 1983)	Yes	Multiple time points before and after	Change over time and difference between groups	Yes	Yes	No	Yes	N/A	Low
(Gutierrez Vazquez and Parrado 2016)	Yes	Multiple time points before and after	Change over time and difference between groups	Yes	Yes	Yes	Yes	N/A	Low
(Clarke and Muhlrad 2018)	Yes	Multiple time points before and after	Change over time and difference between groups	Yes	Yes	Yes	Yes	N/A	Low
(Henderson et al. 2013)	Possibly	Before and after only	Change over time	Yes	No	No	Yes	N/A	High
(Koch et al. 2012)	Yes	Multiple time points before and after	Change over time	Yes	Yes	No	Yes	N/A	Low
(Malhotra and Davi 1979)	Possibly	Before and after only	Change over time	Yes	No	No	Yes	N/A	High

(Finlay et al 2013)	Yes	Multiple time points before and after	Change over time	Yes	Yes	No	Yes	N/A	Low
(Gebrehiwot and Liabsuetrakul 2009)	Possibly	Before and after only	Change over time	Yes	No	No	Yes	N/A	High

RESULTS

The search yielded 2796 citations, of which 840 duplicates were removed (Figure 2.1). After title and abstract screening, we identified 32 articles for full text review. Based on the full text review, 13 studies met the inclusion criteria and were included in this systematic review.

Figure 2.1. Flowchart for selection of articles



Characteristics of included studies

Full descriptive information on the included studies and policy changes is presented in Table 2.2. The studies evaluated abortion reforms occurring in Uruguay, Ethiopia, Mexico, Nepal, Chile, Romania, India, and Ghana. Data were collected from medical records, administrative databases, and household surveys. The year of the abortion policy reforms ranged from 1972 to 2012. Abortion reforms were restrictive (prohibition without exception) in only two studies. Liberal reforms included allowing abortion on request until the 12th week of gestation, 20th week of gestation, to save the woman's life, to preserve the woman's health, and in cases of rape, incest, and fetal impairment. Participants included women with secondary education, women of reproductive age group, women undergoing abortion, women seeking post abortion care, and children 0-60 months.

Four of the thirteen studies reported data on maternal mortality, six on fertility rates, four on abortion complications, three on use of contraceptives, two on low birth weight and two on use of abortion services. In terms of study designs, six were pre-post designs, three used interrupted time series (ITS), three used differences-in-differences (DD), and one used both DD and synthetic control (SC) designs. Seven studies reported receiving financial support, four studies declared no conflict of interest, and nine studies did not comment on this. One study reported a conflict of interest for one of the involved authors.

Table 2.2. Characteristics of included studies

Citation	Country	Policy change	Data source	Population	Comparators	Outcome	Main findings	Sensitivity analysis
(Anton et al 2016) [a]	Uruguay	Policy change in 2012 from abortion availability only to save the woman's life, to preserve the woman's health, and in cases of rape to abortion allowed up until the 12th week of gestation	Perinatal Information System (PIS)	Births to mothers under 20 years old who reached the 13th week of pregnancy between January 1, 2010, and June 11, 2014.	Comparison of outcomes before and after liberalization of abortion	Number of births per week, low weight at birth and prematurity	Limited evidence of change in the number of births per week (73.0±11.2 vs 74.6±8.9); higher birthweights (3147g ± 609g vs 3179g ± 585.8g); lower prevalence of low birth weight (0.11±0.31 vs 0.10±0.30)	None
(Anton et al 2018) [c]	Uruguay	Policy change in 2012 from abortion availability only to save the woman's life, to preserve the woman's health, and in cases of rape to abortion allowed up until the 12th week of gestation.	Perinatal Information System (PIS)	Births to women in 15 largest maternity hospitals in Montevideo, the capital of Uruguay between 20th June 2011 and 18th May 2014	The trend in outcomes for planned births serve as the counterfactual in this DD analysis.	Number of births	11% reduction in the number of births from unplanned pregnancies (-0.109; SE 0.05) compared to other subpopulations of women.	Placebo interventions
(Nozar et al. 2016) [a]	Uruguay	Policy change in 2012 from abortion	Data from the sexual and reproductive	648 women receiving post abortion services	Comparison of outcomes before and	Use of contraceptive methods and	Use of the intrauterine device (IUD) was	None

		availability only to save the woman's life, to preserve the woman's health, and in cases of rape to abortion allowed up until the 12th week of gestation	care services provided at the Pereira Rossell Hospital's Sexual and Reproductive Health Service	at the Pereira Rossell Hospital before (May 2007–July 2009 and 375 women after (August 2014–August 2015)	after the liberalisation of abortion	use of abortion services	significantly higher after compared to before the reform (12.3% vs 2.4%). The percentage of women who were seen after abortion was significantly higher after liberalization of the law than before liberalization. (33.0% vs 25.8%)	
(Mitrut and Wolff 2011) [c]	Romania	Policy change in 1989 that reverted the status of abortion from the following circumstances; to save the woman's life, to preserve the woman's health, and in cases of rape and fetal impairment to abortion available on request up to 12wks.	Romanian Integrated Household Surveys (RIHS)	Children 0–60 months of age between (January–June 1990) and (July–December 1990)	The trend in outcomes for children born between (January–June 1990) serve as the counterfactual in this DD analysis.	Birthweight	Probability of having a low birth weight reduced by 3.7% point for children born after the abortion ban was lifted. (-0.037; SE 0.020)	In-time placebos by replication of empirical strategy using children born in 1991 and 1992
(Serbanescu et al. 1995) [a]	Romania	Policy change in 1989 that reverted the	Romanian reproductive health survey	Women 15-44 years old from (June 1987 to	Comparison of outcomes before and	Total fertility rate, use of contraceptives	28% decline in fertility rates following	None

		status of abortion from the following circumstances; to save the woman's life, to preserve the woman's health, and in cases of rape and fetal impairment to abortion available on request up to 12wks.		May 1990) and from (June 1990 to May 1993)	after the liberalisation of abortion		abortion liberalization in Romania, with the greatest decreases occurring among women aged 30-34 and 35-39 years. 20% increase in use of modern contraceptives following legalization	
(Legge 1983)[b]	Romania	Policy change in 1966 that reverted the liberal status of abortion availability on request to only available to save the woman's life, to preserve the woman's health, and in cases of rape and fetal impairment.	World Health Statistics Annual	Women living in Romania between (1961-1966) and (1967-1975)	Before and after comparison of outcomes in a treatment group (i.e., Romania) and control group (Yugoslavia and Poland)	Number of maternal deaths from abortion	Increase in the total number of maternal deaths from abortion in Romania when compared to other two countries Yugoslavia and Poland	None

(Gutierrez-Vazquez and Parrado 2016) [c]	Mexico	Policy change in 2007 that expanded the previous law, which had allowed legal abortions only to save the woman's life, to preserve the woman's health, and in cases of rape and fetal impairment to now allow abortion on request in the first 12 weeks of pregnancy.	Public use samples of census records from the Mexican National Institute of Statistics and Geography (INEGI)	Women of reproductive ages (14 to 49 years old) residing in the 60 Mexican metropolitan areas, including Mexico City and the Greater Mexico City Metropolitan area between 1990-2000 and 2000-2010	Before and after comparison of outcomes in a treatment group (i.e., Mexico City) and control group (other metropolitan areas)	Overall fertility rate, parity-specific birth rate	Reduced number of births in Mexico City by 4 percentage points relative to the changes that would have occurred without the law (-0.04 points; SE 0.01). Effect more evident among women in their prime reproductive ages 20 to 34. Little evidence for change in teenage fertility (0.08% points; SE 0.02)	Control for prior fertility levels and trends in models predicting fertility differentials in the post-legalization period
(Clarke and Muhlrad 2018) [c,d]	Mexico	Legislation decriminalizing elective abortion in the first 12 weeks of pregnancy in Mexico City in 2007	Instituto Nacional de Estadística, Geografia e Informática (INEGI)	Women 15-49 years-old between 2001 to 2016	Before and after comparison of outcomes in a treatment group (i.e., Mexico City) and control group (other non-reform states)	Maternal morbidity and mortality, Fertility rates	Decline in fertility rate 5-6%, reduction in overall maternal deaths (-0.6 deaths per 100,000 fertile aged women) and abortion related maternal mortality (between -0.07 to -0.10 deaths per 100,000 fertile aged women), 10%	Robustness to synthetic control methods

							reduction in abortion related morbidity, and a 40% reduction in rates of haemorrhage in Mexico when compared to non-reform states after abortion liberalization	
(Henderson et al. 2013) [a]	Nepal	Policy change from abortion allowed only to save a woman's life to abortion allowed on request up to 12 weeks, and 18 weeks for rape or incest in 2002	Medical records review from four largest tertiary referral centers	All abortion-related admissions occurring from January 2001 through December 2010	Outcomes compared across three time periods before implementation (2001–2003), early implementation (2004–2006), and later implementation (2007–2010).	Proportion of serious abortion complications	Overall reduction in the proportion of serious abortion complications from spontaneous and induced abortion (OR=0.7, 95% CI 0.64, 0.85)	None
(Koch et al. 2012) [b]	Chile	Policy change in 1989 from abortion allowed to save the life of a woman to prohibiting	Chilean National Institutes of Statistics (INI)	Women living in Chile between 1957 and 2007	Comparison of outcomes before and after the liberalisation of abortion	Maternal Mortality Ratio	Decrease in the maternal mortality ratio (MMR) in Chile from 41.3 to 12.7 deaths per	None

		abortion in all circumstances					100,000 live births	
(Malhotra and Davi 1979) [a]	India	Effect of 1971 policy change from abortion available only to safe a woman's life to legalization of abortion up to 20 weeks of pregnancy	Medical records at P.G.I Chandigarh hospital	Women admitted to P.G.I Chandigarh hospital for induced septic abortion between (July 1969 - Dec 1971) and (July 1973 - Dec 1975)	Comparison of outcomes before and after the liberalisation of abortion	Seriously ill patients with fulminant peritonitis, septicemia and renal failure.	Increase in complications arising from induced septic abortion after abortion laws were liberalized in India in 1972 (34% vs 43%)	None
(Finlay et al 2013) [b]	Ghana	Policy change in 1985 from abortion restricted in all circumstances to availability in the following circumstances: to save the life of the mother; to save the physical health of the mother; in cases of fetal impairment; in cases of rape; and to save the mental health of the mother	Ghana Demographic Health Survey	Women aged 15-34 years between 1975- 2005	Comparison of outcomes before and after the liberalisation of abortion	Fertility rate	Lower odds of having a child (OR=0.985, 95% CI 0.977, 0.993) among women who were at least 25 years of age following the liberalization of abortion law.	None
(Gebrehiwot and Liabsuetrakul 2009) [a]	Ethiopia	Revision of abortion law in May 2005 to include four legal grounds in which abortion	Hospital medical records	A total of 773 women seeking abortion and post-abortion care from 1 January 2003 to	Period of 2 years before and 2 years after the revision of the laws on	Abortion ratios, Abortion-related maternal mortality ratios, hospital-based abortion complications	Decreased trends of abortion ratio (8000 vs 2000 per 100,000 live births) in years 2003-2005	None

	can be made available: rape and incest, lethal congenital malformation, physical health and mental health	31 December 2007.	abortion in May 2005	compared to 2005-2007.

Evaluation design

[a] Pre-post designs

[b] Interrupted time series designs

[c] Difference-in-differences,

[d] Synthetic controls

Policy Characteristics

Liberalizations

All but two studies included in this review evaluated the impact of liberalizing abortion laws (mostly from very restrictive to relatively liberal). In Uruguay, the Voluntary Termination of Pregnancy (VTP) Law was approved in 2012, allowing abortion on demand up to 12 weeks of pregnancy or up to 14 weeks in the case of rape (43, 53, 54). Similarly, in Nepal (46) and Mexico (55), legislation allowing women to request abortion until the 12th weeks of gestation was passed in 2002 and 2007, respectively. In other countries, extensions to gestational age limits varied from 14 weeks (Romania) (56) to 20 weeks (India) (57).

However, most of these reforms have been implemented with certain caveats. For example, women in Uruguay must appear before a board of three health care professionals who must counsel them on the risks, options and support available before they can have an abortion (43, 53). In Mexico, girls under 18 must obtain written consent from and be accompanied by a parent or guardian in order to receive abortion services (55). In Ghana and India abortion can be conducted only by a registered medical practitioner at a government hospital, registered private hospital, or a place approved by the Minister of Health (57, 58).

Restrictions

In comparison to studies evaluating liberal reforms, only two studies evaluated the impact of restrictive reforms. In Romania, a law restricting abortion without exception was passed in October 1966 in an effort to increase fertility rates and the country's population (59). Women who obtained illegal abortions and those who aided them, including physicians, were subject to

fines and imprisonment. This policy was reversed in 1989, and since that time, abortion has been legal on request in Romania. In Chile, abortion was criminalized without exception in 1989(60). A medical doctor practicing an illegal abortion was subject to 15 years in jail and pregnant women were subject to five years in jail for receiving an illegal abortion, whether it was performed by someone else or done personally. In September 2017, the Chilean National Congress passed a new law that ended the 28-year ban on abortion.

Impacts of Abortion Policies

Use of abortion services

The impact of abortion reforms on use of abortion services was assessed in two studies using the pre-post design. Using data from hospital medical records of women seeking post-abortion care, Gebrehiwot and Liabsuetrakul (61) reported a decrease in the number of induced abortions in the study period before liberalization of abortion in Ethiopia (2003-2005) versus after (2005-2007) (8000 vs 2000 per 100,000 live births respectively). It was suggested that the significant reduction of abortion cases in this tertiary hospital was explained by improved access to other facilities which might offer abortion care after legal revision. However, using a similar design, Nozar, Greif (43) found that the percentage of women seen after abortion was significantly higher in the period after liberalization (2014–2015) than in the period before liberalization (2007–2009) (33.0% vs 25.8%).

Fertility rates

Out of the six studies that reported on fertility rates, liberalization of abortion was associated with a decline in fertility in five studies while one study reported no difference in the average number of births per week. Using a DD design applied to data from the Perinatal Information System (PIS) in Uruguay, Antón, Ferre (54) concluded that the liberalization of abortion resulted in a 11% reduction in unplanned births (births from only pregnancies which are mistimed or unwanted) among women between 20 to 34 years old who completed secondary education [-0.11; standard error (SE)=0.05] compared to other subpopulations of women (those between 20 to 34 years old and above 34 who completed tertiary education). Using a similar DD design with data from the Mexican National Institute of Statistics and Geography (INEGI), a study by Vazquez and Parrado (55) suggested abortion legalization led to a 4 percentage-point reduction (SE=0.01) in fertility rates in Mexico City relative to other metropolitan areas in Mexico without abortion legalization. The impact of the law was more evident among women in their prime reproductive ages (20 to 34); however, there was little evidence for any effect on teenage fertility (0.08 percentage-points; SE=0.02). Similarly, a pre-post study using data from the Perinatal Information System (PIS) in Uruguay found no evidence supporting a change in the number of births per week to teenage mothers when comparing the period before and after abortion legalization (73.0$\pm$11.2 vs 74.6$\pm$8.9 births per week) (53).

Using a pre-post design with data from Romanian Reproductive Health Survey (RRHS), Serbanescu, Morris (45) reported a 28% decline in fertility rates following abortion liberalization in Romania, with the greatest decreases occurring among women aged 30-34 and 35-39 years. A study by Clarke and Mühlrad (62) used both DD and synthetic control designs to assess the impact of abortion legalization in Mexico City on fertility rates with data from Instituto Nacional de

Estadística, Geografia e Informática (INEGI). Results showed that the introduction of the reform resulted in a 5-6% decline in the fertility rate when compared to non-reform states, with both methods generating comparable results. Applying an ITS design to data from the Ghana Demographic Health Surveys, Finlay and Fox (58) reported slightly lower odds of having a child (OR=0.99, 95% CI 0.98, 0.99) among women who were at least 25 years of age following the liberalization of the abortion law in Ghana.

Maternal morbidity

After liberalization of abortion laws, two studies found evidence of a reduction in maternal morbidity. Clarke and Mühlrad (62) reported a 10% reduction in abortion related morbidity (all forms of morbidity classified in ICD-10 codes O02- O08), and a 40% reduction in rates of haemorrhage after the liberalization of abortion in Mexico City compared to non-reform states. Using a pre-post design with data from medical charts of all abortion-related admissions occurring in the four largest public maternity hospitals in Nepal, Henderson, Puri (46) also found an overall reduction in the proportion of serious abortion complications from spontaneous and induced abortion, such as sepsis, peritonitis, evidence of foreign body, organ failure, or death (OR=0.70, 95% CI 0.64, 0.85). Conversely, two studies reported an increase in abortion morbidities. Malhotra S and Devi PK (57) found an increase in complications arising from induced septic abortion after abortion laws were liberalized in India in 1972 (34% vs 43%). Gebrehiwot and Liabsuetrakul (61) reported a two-fold increase in cases of septic shock and organ failure and increased intensive care unit admission (from 1.8% to 5.1%) among women seeking post abortion care in Ethiopia.

Maternal mortality

One study reported a moderate decline in the abortion related maternal mortality ratio (110 vs 90 deaths per 100,000 live births) when comparing the period before and after liberalization of abortion in Ethiopia (61). Clarke and Mühlrad (62) also reported a significant reduction in overall maternal deaths (-0.6 deaths per 100,000 fertile aged women) and abortion related maternal mortality (between -0.07 to -0.10 deaths per 100,000 fertile aged women) in Mexico when compared to non-reform states. Comparing before and after a reform restricting abortion, Legge (59) reported a significant increase in abortion related maternal deaths in Romania compared to two countries without restrictive reforms, Yugoslavia and Poland. Conversely, a study by Koch, Thorp (60) reported that abortion restriction was associated with a decrease in the maternal mortality ratio (MMR) in Chile from 41.3 to 12.7 deaths per 100,000 live births.

Neonatal morbidity

Using a pre-post design, Anton, Ferre (53) reported comparable birthweights (3147g $\pm$ 609g vs 3179g $\pm$ 585.8g) and prevalence of low birth weight (0.11$\pm$0.31 vs 0.10$\pm$0.30) when comparing the periods before and after abortion liberalization in Uruguay. The DD analysis of Mitrut and Wolff (56) using data from the Romanian Integrated Household Surveys suggested that the removal of the abortion ban lowered the probability of having a low-birth-weight baby by 3.7 percentage-points (SE=0.020).

Contraceptive use

The pre-post analyses of Serbanescu, Morris (45) reported a 20% increase in use of modern contraceptives following legalization, using data from the RRHS, which was highest

among women aged 25-34 with greater educational attainment. Using a similar design, Nozar, Greif (43) reported a higher use of intra-uterine devices (IUD) in the period after versus before legalization (12.3% vs 2.4%), while Clarke and Mühlrad (62) found little evidence of an effect on using any contraceptive (-1.2 percentage-points; SE=0.914) or using modern contraceptives (-1.3 percentage-points; SE=0.901) when comparing Mexico City to other non-reform states after abortion liberalization.

Methodological strengths and weaknesses of the included studies

Studies applied a variety of techniques to estimate the effect of abortion reforms, including pre-post analyses and more robust quasi-experimental designs. Quasi-experimental designs are characterized by an identification strategy that accounts for sources of unmeasured confounding, such as time-fixed differences between exposure groups (e.g., treated and control jurisdictions) and/or secular trends in outcomes that likely bias other observational designs (63).

The before-and-after or pre-post design was the most commonly used method (43, 45, 46, 53, 57, 61). However, this design does not adequately account for secular changes that occurred before vs. after the reform and affected abortion-related outcomes, such as the density of providers or socio-demographic characteristics of the population (e.g., age or educational attainment). For example, Anton, Ferre (53) reported that the abortion reform in Uruguay coincided with a broader reform of the Uruguayan health system; this may have had an independent effect on fertility rates, potentially confounding effect estimates. As such, pre-post designs are unlikely to yield unbiased evidence, and therefore causal inference cannot be drawn from the majority of research on the impacts of abortion law reforms in LMICs.

Some studies used the ITS design to assess the impact of abortion reforms by comparing the observed post-reform trend to the extrapolated trend from the pre-reform time series (58-60). In this case, identification of a causal effect relies on the assumption that the extrapolated post-reform trend in the outcome represents what would have occurred in the absence of the reform (i.e., the counterfactual) (64, 65). The ITS design also assumes that there are no unmeasured time-varying confounders such as other events that co-occur with the reform, which potentially influence the outcome. For example, Finlay et al. (58) reported that abortion liberalization was implemented in response to a severe famine, which itself could have had an independent effect on fertility rates. There is also the challenge of reverse causality; for example, restricting abortion in response to low population growth might confound effects on fertility rates.

A few studies (54-56, 62) used the DD design by adding a comparison group that did not experience a similar reform to infer what would have happened in the treatment group had it not enacted the policy (66). However, time-varying characteristics, including a distinct policy reform implemented concurrently with the abortion reform in the treated (or control) group, would bias effect estimates if they were associated with the outcome of interest (67). For example, Anton et al (54) employed the DD design to estimate the causal effect of abortion liberalization on fertility rates, under the assumption that only unplanned pregnancies would be impacted by the policy change whereas planned pregnancies would not. While this assumption is debatable, the authors assessed whether outcomes trends were parallel in the pre-reform period. They also performed a falsification test with a placebo intervention. Other approaches used to test the robustness of DD designs in this review include multiple control

groups (55) and other placebo outcomes (56). A related design employed by one study (62) is the SC method, which uses a weighted combination of comparison units (the "synthetic control") to represent the counterfactual (68, 69). Due to concerns of geographic spillovers in access to abortion from nearby Mexico State, Morelos and Hidalgo, Clarke et al (62) excluded these three states from the "donor" pool from which the synthetic controls were derived. Results were similar to the main model that did not exclude these states suggesting limited spillover effect of abortion reforms on rates of birth and maternal morbidity.

Studies using pre-post were judged to be at a high risk of bias, while studies using ITS, DD and/or synthetic controls design were judged to be at low risk of bias. Studies using an ITS design are not necessarily at greater risk of bias than studies using a DD design, since both theoretically account for time fixed characteristics and secular trends and are susceptible to unmeasured time varying confounding. While studies using ITS in this review did not assess the robustness of their findings through sensitivity analysis, they included enough pre-intervention time points to characterize temporal trends and one of the studies included a control group.

It is important for quasi-experimental studies to argue convincingly that the intervention is likely exogenous and not subject to confounding biases, including reverse causation and unmeasured time-varying confounding (70). Additionally, reporting of some outcomes, such as use of abortion services and abortion related complications, are likely affected by the abortion reforms themselves. Measurement error may be particularly problematic when outcomes are self-reported (71) since women are more likely to selectively suppress information about their abortion history or be reluctant to seek medical services in the event of complications before than after liberalization, due to social and cultural stigma, and fear of legal consequences.

Hospital records may also be prone to such information bias (72) due to legal consequences for patients and providers alike. While both data sources may be prone to error, induced abortion underreporting is more prevalent in self-reports compared to medical records (73). Some studies have assessed underreporting by comparing self-reported with expected abortion incidence determined from provider data (71, 73, 74). Another suggested technique to evaluate and report levels of systematic error in abortion related underreporting is multiple bias analysis (75). Measurement errors in these studies could plausibly contribute to non-differential or differential misclassification; the latter could bias estimates either towards or away from the null. None of the studies reviewed included sensitivity analyses to quantify the role of measurement error. Our GRADE assessment also suggested that the quality of evidence was low.

DISCUSSION

Our review included 13 studies that evaluated the impact of abortion reforms in LMICs, with most focusing on the impact of liberalization on fertility, maternal mortality, and maternal morbidity, such as abortion complications. Few studies assessed impacts on contraception and neonatal health outcomes. We found mixed results for the association between abortion law reforms and use of abortion services, fertility, contraceptive use, maternal morbidity and mortality and neonatal health. Nonetheless, abortion decriminalization was associated with decreased fertility and maternal mortality. There was limited evidence for an association between abortion reforms and other outcomes.

We did not identify any relevant randomized studies given that abortion policy is unlikely to be randomly assigned. Also, we did not identify any study on the impact of abortion reforms on access to abortion services. One potential explanation is the lack of consensus on how to measure access to abortion services, due to its multifaceted nature. Some studies have suggested access to encompass abortion services that are affordable, geographically proximate, timely and integrated (76, 77). Others have included trained and qualified staff, essential equipment, respectful care and supportive counselling (50, 78). These proxies for safe abortion access may be limited in their ability to capture the barriers women who seek abortion face, such as stigma.

Similarly, there were only two studies on use of abortion services, neither of which could be used to draw causal inference. Utilization of abortion services, particularly the rates of induced abortion, are difficult to measure because of frequent underreporting or misclassification in surveys, hospital records and health statistics (79-81). The widespread informal use of misoprostol to induce abortions also affects accurate estimates of abortion incidence and related morbidity (79, 82, 83). Strategies such as indirect questioning, the abortion incidence complications method (AICM), and self-administered surveys, should be considered to address underreporting (79, 81, 84, 85).

Similar to literature from USA and Europe reporting lower birth rates following increased abortion availability (38, 86-89), we found evidence of reduced fertility due to liberalizing abortion policies (45, 54, 55, 58, 62), particularly among women between 20 and 34 years old. This could be explained by a decline in births from unplanned pregnancies (10, 54). Abortion law reforms did not lead to a substantial change in the reproductive behavior of adolescents in Mexico or Uruguay (53, 55). This might be connected to the requirement of parental or guardian

consent for abortion and approval by a board of three health care professionals for women under 18 years of age in these countries. By comparison, evidence on impacts on teen fertility from higher income contexts are mixed, with several studies (86, 88, 90, 91) suggesting that abortion legalization led to lower rates of teen fertility. Additionally, we found some evidence that women with lower socioeconomic status, such as those reporting secondary education or less, were more likely to experience a substantial decline in fertility after liberalization compared to women with higher levels of education, (54) which coheres with prior research from the US (89). Our review suggested that abortion legalization expanded women's use of contraception (43, 45), which might be attributable to contraceptive counselling that is widely available post legalization. On the contrary, studies from the US have reported a decline in contraceptive use post reform (91, 92).

We found evidence for a decrease in maternal mortality after abortion liberalization (61, 62) similar with growing evidence that liberalized abortion laws are associated with a decrease in maternal mortality (47, 93, 94), including large mortality reductions observed in Romania and South Africa (59, 95, 96). While these declines seem impressive, studies have reported significant overestimation of maternal mortality due to counting of deaths that are unrelated to induced abortion and utilization of incomplete hospital records (97, 98). Our findings on abortion complications were heterogenous, which parallels prior work from higher income contexts (99, 100). Although abortion liberalization might reduce unsafe abortion and attendant complications (40, 93), it might also increase health seeking behavior and reporting of complications. Increase in abortion complications in India after decriminalization could also be explained by the health system constraints and caveats to abortion reforms in India where

abortion can only be conducted at a government hospital, or a place approved by the Minister of Health (57).

It has been hypothesized that liberalization of abortion laws might result in improvements in neonatal outcomes such as birthweight, by preventing births following unplanned pregnancies (17). However, pathways explaining such effects have not been adequately explored. We found little evidence of positive effects of abortion law liberalization on neonatal health. By comparison, prior research did not detect an effect of new restrictions on Medicaid funding for abortion across US states on birth weight (101, 102). A study in Romania reported large, short term-increases in stillbirths and infant death following the introduction of a restrictive policy (17); the authors hypothesized that these increases could be explained by reduced access to pre and post-natal care due to possible crowding of hospitals and health clinics. Nevertheless, additional research is needed to elucidate the effect of abortion law reforms on perinatal and neonatal outcomes.

For policy making, it is critical to examine the impact of abortion legislation since reforms in abortion policy alone, which are often subject to local contextual influences, may not be sufficient for substantial change in women's health services and health outcomes. Policy makers should endeavor to support multiple approaches, with choices focusing more on details of the design, implementation and enforcement of abortion reforms. With an ever-growing number of countries reforming their abortion laws, it is more important than ever that researchers and policy makers have a comprehensive understanding of how these laws impact health services and health outcomes. To better inform decision making, policy makers should consider how health system constraints and caveats to abortion reforms, such as mandatory waiting periods,

requirements for third-party authorizations and parental involvement laws, affect access to abortion services.

Our review of the literature highlights some research gaps. First, the majority of the literature from LMICs has focused on the impact of legalizations and there has been far less attention to the impact of restrictions, including mandatory waiting periods, requirements for third-party authorizations and parental involvement laws, which are increasingly being implemented in LMICs. Second, abortion laws lie along a gradient and may have distinct effects. For example, countries that changed from very restrictive to relatively liberal abortion laws may experience different outcomes from those with changes from modest restrictions to abortion available on request. Third, as our review has shown, few studies have evaluated outcomes plausibly affected by abortion policies, such as access to and use of abortion services, contraceptives, and neonatal and child health. Fourth, few studies have examined whether impacts of abortion reforms are heterogeneous, and whether they affect social inequalities in access in particular. Fifth, there is a dearth of rigorous quasi-experimental studies in LMICs.

Limitations

Although abortion law reforms are occurring worldwide, our review was restricted to LMICs. The inclusion of only quasi experimental studies in order to inform inference regarding the causal effect of abortion legislation limited our assessment to a subset of the relevant literature. The small number of studies, inconsistency in the reporting of outcomes across studies (diverse types of measures of association used, missing standard errors and confidence intervals) made it difficult to properly summarize estimates, conduct a meta-analysis and include funnel

plots. The high risk of bias in most of the studies in this review, particularly pre-post studies, limits our ability to infer causality. Our findings should be interpreted with caution given the methodologic concerns about the quality of the empirical evidence.

CONCLUSION

Our review of the literature allows us to draw some provisional conclusions. Legislative reforms from highly restrictive to relatively liberal were associated with reductions in fertility and maternal mortality. There was little evidence that abortion reforms in LMICs had any consistent impact on access to and use of abortion services, contraceptive use, maternal morbidity and neonatal health, primarily due to the lack of rigorous empirical research in these areas. Women between 20 and 34 years of age were more likely to be influenced by abortion reforms, but there was limited evidence for heterogeneity by socioeconomic status. Further research is required to strengthen the evidence-base for informing abortion legislation in LMICs. This review explicitly points to the need for more quasi-experimental studies with robust approaches and sensitivity analysis to assess underlying assumptions. Strengthening the evidence-base for informing abortion legislation would help policy makers make informed decision, improve implementation of abortion policies, and scale-up of services.

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2.3 Supplemental material: Manuscript 1

Supplementary Table 2.1. Search Strategy

	Embase Classic+Embase	**Result**
1	low income country/ or middle income country/ or developing country/	97798
2	Developing Countries .sh,kf.	64313
3	(Africa or Asia or Caribbean or West Indies or South America or Latin America or Central America).hw,kf,ti,ab,cp.	320469
4	((developing or less* developed or under developed or underdeveloped or middle income or low* income or underserved or under served or deprived or poor*) adj (countr* or nation? or population? or world)).ti,ab.	110534
5	((developing or less* developed or under developed or underdeveloped or middle income or low* income) adj (economy or economies)).ti,ab.	598
6	(low* adj (gdp or gnp or gross domestic or gross national)).ti,ab.	329
7	(low adj3 middle adj3 countr*).ti,ab.	14024
8	(lmic or lmics or third world or lami countr*).ti,ab.	7319
9	transitional countr*.ti,ab.	215
10	(Afghanistan or Albania or Algeria or Angola or Antigua or Barbuda or Argentina or Armenia or Armenian or Aruba or Azerbaijan or Bahrain or Bangladesh or Barbados or Benin or Byelarus or Byelorussian or Belarus or Belorussian or Belorussia or Belize or Bhutan or Bolivia or Bosnia or Herzegovina or Hercegovina or Botswana or Brasil or Brazil or Bulgaria or Burkina Faso or Burkina Fasso or Upper Volta or Burundi or Urundi or Cambodia or Khmer Republic or Kampuchea or Cameroon or Cameroons or Cameron or Camerons or Cape Verde or Central African Republic or Chad or Chile or China or Colombia or Comoros or Comoro Islands or Comores or Mayotte or Congo or Zaire or Costa Rica or Cote d'Ivoire or Ivory Coast or Croatia or Cuba or Cyprus or Czechoslovakia or Czech Republic or Slovakia or Slovak Republic or	1972700

	Djibouti or French Somaliland or Dominica or Dominican Republic or East Timor or East Timur or Timor Leste or Ecuador or Egypt or United Arab Republic or El Salvador or Eritrea or Estonia or Ethiopia or Fiji or Gabon or Gabonese Republic or Gambia or Gaza or Georgia Republic or Georgian Republic or Ghana or Gold Coast or Greece or Grenada or Guatemala or Guinea or Guam or Guiana or Guyana or Haiti or Honduras or Hungary or India or Maldives or Indonesia or Iran or Iraq or Isle of Man or Jamaica or Jordan or Kazakhstan or Kazakh or Kenya or Kiribati or Korea or Kosovo or Kyrgyzstan or Kirghizia or Kyrgyz Republic or Kirghiz or Kirgizstan or Lao PDR or Laos or Latvia or Lebanon or Lesotho or Basutoland or Liberia or Libya or Lithuania or Macedonia or Madagascar or Malagasy Republic or Malaysia or Malaya or Malay or Sabah or Sarawak or Malawi or Nyasaland or Mali or Malta or Marshall Islands or Mauritania or Mauritius or Agalega Islands or Mexico or Micronesia or Middle East or Moldova or Moldovia or Moldovian or Mongolia or Montenegro or Morocco or Ifni or Mozambique or Myanmar or Myanma or Burma or Namibia or Nepal or Netherlands Antilles or New Caledonia or Nicaragua or Niger or Nigeria or Northern Mariana Islands or Oman or Muscat or Pakistan or Palau or Palestine or Panama or Paraguay or Peru or Philippines or Philipines or Phillipines or Phillippines or Poland or Portugal or Puerto Rico or Romania or Rumania or Roumania or Russia or Russian or Rwanda or Ruanda or Saint Kitts or St Kitts or Nevis or Saint Lucia or St Lucia or Saint Vincent or St Vincent or Grenadines or Samoa or Samoan Islands or Navigator Island or Navigator Islands or Sao Tome or Saudi Arabia or Senegal or Serbia or Montenegro or Seychelles or Sierra Leone or Slovenia or Sri Lanka or Ceylon or Solomon Islands or Somalia or South Africa or Sudan or Suriname or Surinam or Swaziland or Syria or Tajikistan or Tadzhikistan or Tadjikistan or Tadzhik or Tanzania or Thailand or Togo or Togolese Republic or Tonga or Trinidad or Tobago or Tunisia or Turkey or Turkmenistan or Turkmen or Uganda or Ukraine or Uruguay or USSR or Soviet Union or Union of Soviet Socialist Republics or Uzbekistan or Uzbek or Vanuatu or New Hebrides or Venezuela or Vietnam or Viet Nam or West Bank or Yemen or Yugoslavia or Zambia or Zimbabwe or Rhodesia).mp.	
11	or/1-10	2170731

12	((abortion*) adj4 (polic* or law* or reform* or change* or legal* or decriminali#ation* or jurisprudence or legislat* or liberali#ation* or liberali#e or restrict*)).mp.	6786
13	exp induced abortion/	36842
14	exp jurisprudence/ or exp legal aspect/ or exp law/	1046451
15	13 and 14	11029
16	12 or 15	14986
17	11 and 16	2949
18	(association or impact or effect or correlat* or increase* or reduce* or outcom*).ti,ab.	13392718
19	17 and 18	900

Supplementary Appendix 2.2. Definition of Quasi experimental designs and level of evidence

Quasi experimental designs	Definitions	Level of Evidence
Pre-Post	A pre-post design is a method for assessing the impact of an intervention by comparing outcomes before and after an intervention occurs(103).	Low
Interrupted Time Series (ITS)	The ITS design is used to assess the causal effect by comparing the observed post-intervention trend to the extrapolated trend from the pre-intervention time series. Identification of a causal effect relies on the assumption that the extrapolated post-intervention trend in the outcome represents what would have occurred in the absence of the treatment(64, 65).	High
Differences-in-Differences (DD)	Difference-in-differences (DD) methods identify causal effects by contrasting the change in outcomes pre- and post-	High

	intervention, for the treatment and control groups. DD assumes that, in the absence of treatment, the average outcomes for the treated and control groups would have followed parallel trends over time(67).	
Synthetic Controls (SC)	The SC method uses a weighted combination of comparison units (the “synthetic control”) to represent the counterfactual (what would have happened in the absence of the treatment.(68, 69)	High
Regression Discontinuity (RD)	Regression Discontinuity (RD) is a design that measures the impact of an intervention, or treatment, by applying a treatment assignment rule to a continuous assignment or forcing variable. Treatment assignment is based on whether a numeric rating falls above or below a certain threshold or cut-point(104).	High

Supplementary Appendix 2.3. GRADE evidence profile

Quality assessment						
No of Studies	**Limitations**	**Inconsistency**	**Indirectness**	**Imprecision**	**Publication bias**	**Quality**
Outcome (number of studies)						
Use of abortion services (2)	Serious	N.A	Serious	N.A	N.A	N.A
Fertility rate (6)	No Serious	No Serious	No Serious	No Serious	Unlikely	Moderate
Maternal morbidity (4)	Serious	N.A	No Serious	N.A	N.A	N.A
Maternal mortality (4)	No Serious	Serious	No Serious	Serious	Unlikely	Low
Neonatal morbidity (2)	Serious	N.A	No Serious	N.A	N.A	N.A
Contraceptive use (3)	Serious	Serious	No Serious	Serious	Unlikely	Very Low

N.A- Not applicable

CHAPTER 3. Overview of Data and Methods

The literature review described in the previous chapter identified several methodological gaps that limit the inference that can be drawn concerning the causal impact of abortion reforms in LMICs. Specifically, the review uncovered a dearth of rigorous quasi-experimental studies with appropriate designs and sensitivity analyses to assess underlying assumptions. In addition, few studies have evaluated outcomes plausibly affected by abortion policies, such as neonatal mortality and contraceptive use.

In this chapter, I provide justification for the selection of countries used as case studies (i.e., the Dominican Republic and Mozambique) and provide an overview of the data sources and methods used in Manuscripts 2-4. This section is intended to provide an overview of our analytical approach and describe some key measurement issues and challenges of this work.

3.1 Data Sources

3.2.1 Demographic and Health Surveys (DHS)

The DHS are comparable household surveys that provide information on socio-demographics and health indicators, including maternal and child health, nutrition, HIV/AIDS, malaria, and family planning (59). The Measure DHS program is funded by the United States Agency for International Development. Surveys typically cover 5000-30 000 households and have been conducted in more than 90 countries over the past 30 years. DHS surveys are generally repeated in a country approximately every 5 years in order to monitor trends in population health and uptake of public health interventions (60). For many LMICs, DHS surveys

provide the only available nationally representative source of information on neonatal deaths (59).

DHS uses a multi-stage stratified cluster design to ensure representativeness at the county, regional, and urban-rural levels. A household questionnaire provides data on demographic, socioeconomic, and environmental conditions of each household. A gender-specific survey is administered to all women aged 15-49 who spent the night before the survey in the sampled household. Women are asked to provide background characteristics (e.g., age, education, employment, etc.), complete birth histories, and information related to the use of reproductive, maternal and child health services. DHS uses a set of standardized questionnaires, which facilitates comparisons of survey data across different time periods and countries. As a result of careful design and piloting of questionnaires, extensive training of country supervisors and interviewers, and data quality control measures, DHS data on maternal and child health are of high quality (59-62).

3.2.2 Multiple Indicator Cluster Surveys (MICS)

The Multiple Indicator Cluster Survey (MICS) is a household survey program developed and supported by UNICEF (63). MICS is designed to collect estimates of key indicators that are used to assess the circumstances of children and women. These surveys cover birth histories, fertility, water and sanitation, breastfeeding, immunization, salt iodization, diarrhea. Additional topics include fertility and mortality, contraceptive use and unmet need, maternal and newborn health, female genital mutilation, menstrual hygiene management, child illness and treatment, and child development and nutrition. MICS also acts as a major source of data on child health

and nutrition. Socio-demographic information, such as household characteristics, wealth, and religion/ethnicity, are also included in MICS. Individual women and men aged 15–49 provide self-reported data, whereas data on children under 5 years of age and aged 5–17 years comes from their mothers (or caregivers if mothers do not live in the household or are deceased) (63, 64).

The MICS surveys use a multi-stage sampling design based on an existing sample frame, such as the latest population and housing census or a suitable master sample frame. In the first stage of the sampling design, census enumeration areas (EAs) are selected with their probability of selection proportional to population size (though other allocations can be considered). In the second stage, households in the selected EAs are listed to ensure that the measures of size are fully updated (except in cases where an updated list is available from a master sampling frame, another survey, or a recent census, within the last 12 months). From the updated listing, households are selected using random systematic sampling, forming the survey clusters. Clusters have a moderate size of 20–25 households (65).

Consistent with prior studies using harmonized DHS and MICS data for assessing trends in reproductive, maternal, and neonatal health outcomes (65-67), my evaluations of abortion law reforms used harmonized data from the DHS and MICS. The datasets used in the analyses for each chapter are presented in table 3.1. The second manuscript (Chapter 4) focused on evaluating a policy that restricted abortion in the Dominican Republic and used data from the treated country and sixteen control countries with surveys conducted between 1999-2019. In Chapter 5, I evaluated a reform that decriminalized abortion in Mozambique using data from the treated country and sixteen control countries with surveys conducted between 2004-2018.

Finally, the fourth manuscript in Chapter 6 uses national household surveys to quantify and correct estimates of the impact of abortion reforms for the potential misclassification of neonatal mortality.

Table 3.1. DHS and MICS datasets used in each thesis chapter

Chapter 4	Dominican Republic (1999, 2002, 2007, 2013/14, 2019); Turkey (2003, 2008, 2013), Namibia (2000, 2006, 2013); Jordan (2002,2007,2012,2017/18); Armenia (2000,2005,2010,2015/16); Benin (2001, 2006, 2011, 2017, 2018); Cambodia (2000, 2005, 2010, 2014); Cameroon (2004,2011,2018); Egypt (2000,2005,2008,2014); Ghana(2003,2008,2014); Guatemala (1999,2015); Haiti (2000,2005, 2012, 2016/17); Nigeria (2003,2008, 2013, 2018); Pakistan (2006,2012, 2017); Philippines (2003,2008, 2013, 2017/18); Zambia (2001,2007,2013,2018); Zimbabwe (1999, 2005, 2010, 2015)
Chapter 5	Mozambique (2004, 2008, 2011, 2015, 2018); Benin (2006, 2011/12, 2017/18), Burundi(2010/11, 2016/17); Cameroon (2004,2011,2018/19); Gambia(2010,2013,2019/20); Guinea (2005,2012,2018); Ghana (2004, 2014, 2017); Liberia (2006/7, 2013, 2019/20); Madagascar (2004,2008/9,2016,2018); Malawi(2004/5, 2010, 2015/16); Mali (2006, 2012/13, 2018); Nigeria (2008, 2013, 2018); Rwanda (2005,2010/11,2014/15, 2019/20); Tanzania (2004/5 ,2009/10, 2015/16); Uganda (2006, 2011, 2016); Zambia (2007, 2013/14, 2018/19); Zimbabwe (2005, 2010, 2015/ 2019)

3.2 Measures

3.2.1 Exposure or intervention

The exposure or intervention is the adoption of an abortion policy reform. We used the Global Abortion Policy Database and the Harvard Center for Population and Development Studies Dataverse (68, 69) to identify LMICs that have reformed their abortion laws (either decriminalization or restriction) within the past 15 years. To assess the feasibility of evaluating these reforms, we then cross-referenced the reforms with the availability of data from the Demographic Health Surveys (DHS) and/or UNICEF Multiple Indicator Cluster Surveys (MICS), both before and after the reforms of interest. This narrowed our selection to the Dominican Republic and Mozambique.

We used the Dominican Republic as a case study of abortion restriction, since the Dominican Republic introduced a law to ban all abortions without any exception in September 2009, when a constitutional amendment declaring the right to life as "inviolable from conception until death" was approved in Congress (70-72).

We used Mozambique as a case study of abortion decriminalization. In July 2014, the Parliament introduced legislation for the Voluntary Interruption of Pregnancy (IVG), which decriminalized abortion as part of a new penal code (Law No. 35/2014) promulgated by President Armando Guebuza (73). This law allowed IVG to be performed by a physician or other qualified health professional in an official health facility in the first 12 weeks of pregnancy, with the consent of the pregnant woman. In cases of rape or incest, abortion was legalized during the first 16 weeks, and in cases of fetal anomaly it was made legal in the first 24 weeks (74, 75). (76).

3.2.2 Outcomes

In Manuscript 2, we examined the effect of abortion restriction on contraceptive use and neonatal mortality in the Dominican Republic, whereas in Manuscript 2, we examined impacts of abortion decriminalization on only neonatal mortality in Mozambique. This was due to the lack of data on contraceptive use in Mozambique. In addition, we could not assess other outcomes such as abortion utilization due to lack of data.

Contraceptive use was measured by an indicator of whether a woman reported themselves or their partners as currently using at least one modern method of contraception (77, 78). Modern methods referred to any of the following: female sterilisation, male sterilisation, oral contraceptive pills, the intrauterine contraceptive device, injectables, implants, male and/or female condoms, diaphragms, contraceptive foam and contraceptive jelly, and lactational amenorrhoea method, as well as other modern contraceptive methods (including cervical cap, contraceptive sponge and others).

Neonatal death was measured by an indicator of whether a child who was born alive died within the first month of life (60). The birth history asks mothers about all live births they have ever had, including if the baby was still alive and, if not, their age at death. Mothers who reported a deceased child were asked to report the age at death in days if the death occurred in the first 30 days of life.

3.2.3 Measuring neonatal mortality

Neonatal mortality data is derived from retrospective birth histories collected from all women aged 15-49 who participated in DHS/MICS surveys. While the true neonatal period is limited to the first 28 days of life, the DHS/MICS considers deaths occurring during the first

month as neonatal deaths (79). This is primarily because the distribution of death dates based on retrospective recall frequently reveals a tendency for reports to cluster around key time points (such as one week, one month, etc). The difference between 28 days and 1 month is believed to be insignificant and unlikely to affect the estimated effects on neonatal mortality (80).

Retrospective birth histories collected through household surveys are currently the only nationally representative source of information on neonatal mortality for nearly three-quarters of births occurring in countries without a well-developed system of vital registration (81). However, as is common in retrospective analyses of routinely collected data, there are limitations inherent to the DHS and MICS that may bias estimated effects. Underreporting of neonatal deaths remains a challenge because of difficulties associated with accurate recall and willingness to report both the occurrence and timing of events (81, 82). Misclassification between stillbirths and early neonatal deaths based on women's self-reports is also common (83, 84)

Women may not possess accurate knowledge to distinguish between stillbirths and neonatal deaths and may report neonatal deaths as stillbirths or vice versa. Babies born with limited signs of life, such as crying, breathing, and movement, and who died shortly after birth, may be mistakenly or intentionally reported as stillbirths. Moreover, women may recount to the interviewer what they believe to be a true description of the event but may have misunderstood information from a medical provider (83, 85). Thus, efforts to quantify, describe, and correct for misclassification may have important implications when evaluating the effects of population-level interventions on neonatal mortality. In Chapter 6, we used the

Misclassification Simulation Extrapolation (MC-SIMEX) method to quantify the impact of varying degrees of misclassification of neonatal deaths on the estimated impacts of abortion reforms.

3.2.4 Measuring socioeconomic status

Chapters 4 and 5 of my thesis involve examining the impact of abortion reforms on socioeconomic inequalities in health outcomes. Many LMICs lack reliable and timely data on income and consumption (86). Apart from missing data, using income or consumption measures can be challenging, as income can be highly variable month to month or difficult to accurately measure (86, 87). Due to the difficulties associated with measuring SES through income and consumption, proxy measures such as the wealth index are used (88, 89). Wealth indices are constructed using data on household durable assets, such as housing materials, access to toilets or latrines, phone ownership, or agricultural land and livestock, that are routinely collected in most household surveys. Their usage has grown in popularity in part due to the pre-existing availability of data on household durable assets in key standardised household surveys such as the DHS and MICS (90, 91).

We used the polychoric dual-component wealth index (P2C) as an alternative to the DHS wealth index to address the bias towards patterns of urban wealth (92). The DHS wealth index is known to be skewed in favour of urban wealth patterns, referred to as the "urban bias" (90) . As a result of wealth concentration in cities, the direction of the axis of the first principal component is strongly influenced by urban-centric wealth components. Rural wealth patterns, such as agricultural land and domestic animal ownership, are frequently underrepresented along that axis. It is not uncommon to see negative coefficients for such commodities, which

effectively reduce the wealth scores of households that own such assets when included in the computations (92).

The P2C wealth index uses a similar set of assets as the standard DHS wealth index, including those related to household conditions (e.g., water source, sanitation facilities, electricity) and ownership of consumer goods (e.g., a bicycle, a telephone, a refrigerator, agricultural land and domestic animals). However, in contrast to the DHS wealth index, categorical variables are converted into ordered categorical (ordinal) variables to harness additional discriminatory power and avoid the questionable use of multiple binary (dummy) variables to represent a single asset; squared multiple correlations (SMC) are used to eliminate variables that are weakly correlated with asset-based wealth; two principal components, rather than one, are used to account for key aspects of rural wealth and balance the urban bias (92).

3.3 Study Designs

3.3.1 Difference-in-differences (DD) design

An important limitation of prior research identified in the literature review pertains to the application of observational study designs, including before-and-after or pre-post designs. Comparisons of outcomes in the same population before and after a policy change may be biased due to underlying secular trends affecting outcomes. As such, pre-post designs are unlikely to yield unbiased evidence and causal inference cannot be drawn from most research on the impacts of abortion law reforms in LMICs.

To address this limitation, I opted for a difference-in-differences (DD) design, which is a quasi-experimental design frequently used to assess the impact of policy and other population-

level interventions in health research (93, 94). Difference-in-differences (DD) can inform causal inference by accounting for underlying secular trends in outcomes of interest using a control group to estimate the counterfactual outcome trajectories of the treated group (95-97). Although DD accounts for unobservable or unmeasured variables that are fixed over time, and for underlying trends in outcomes, it requires strong assumptions and a careful planning of each analytic step. In the following paragraphs, I will briefly describe the study design and statistical analyses, as well as additional analytical considerations, strengths, assumptions, and limitations.

The difference-in-differences compares changes in outcomes between a treated and control group over time, with the post-intervention trend in the control group assumed to substitute for the trend in the treated group had it not been treated (i.e., the counterfactual). Control groups should be selected to be as close to the treated group as possible, except for the intervention. Specifically, to prevent violations of the parallel trends assumption, a control group is selected that: (i) does not experience the policy change (treatment) during the study period and (ii) exhibits similar pre-treatment trends in the outcome (94, 97, 98).

To perform a difference-in-differences analysis, repeated outcome data is required to measure trends, which may take the form of longitudinal/panel data or repeated cross-sectional data from national surveys or other sources (95, 99). Outcome trends before and after the policy reform are compared between the treated group and the control group, accounting for other time-varying factors, such as compositional changes that may have occurred in one group but not the other. The double difference between pre-and post-intervention trends for the treated and control groups is referred to as the DD estimate, which represents the average

treatment effect on the treated (ATT). By including group and time fixed effects, the DD analysis allows disentangling the impact of the intervention from (i) fixed differences between control and treated groups (potential confounding factors) and (ii) temporal trends in the outcome unrelated to the intervention (95, 100). However, the DD approach requires the assumption of no unmeasured time-varying confounding. In other words, we assume that the only time-varying difference that affects the outcome between the treated and control group is the exposure/intervention.

Regression Framework

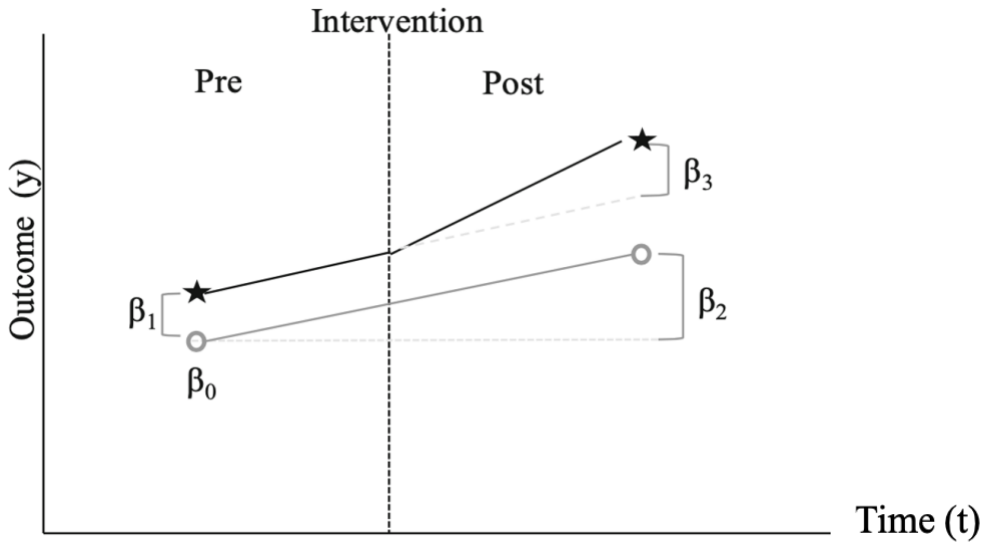
The basic DD regression with two groups, treated (j = 1) and control (j = 0), and two time periods representing pre- (t = 0) and post-policy (t = 1) periods, is as follows:

$$Y_{ijt} = \beta_0 + \beta_1 \mathrm{E_j} + \beta_2 \mathrm{Post_t} + \beta_3 \mathrm{E_j} * \mathrm{Post_t} + \beta_4 \mathrm{X_{ijt}} + \varepsilon_{\mathrm{ijt}}$$

where Y_{ijt} is the outcome for individual i in group j at time t, $\mathrm{E_j}$ is an indicator variable for exposure group j, $\mathrm{Post_t}$ is an indicator variable for time t being after the policy change, $\mathrm{X_{ijt}}$ is a vector of time-varying individual-level covariates, and $\varepsilon_{\mathrm{ijt}}$ is the error term. $\mathrm{E_j}$ is equal to one if the observation is in a group that changes its policy, regardless of the value of t, and equal to zero in a country that does not change its policy. $\mathrm{Post_t}$ is equal to one if the observation occurs after the policy change, regardless of the value of j. The interaction term, $\mathrm{E_j} * \mathrm{Post_t}$, therefore equals one only for observations that are in the exposed group after the policy change. The estimated coefficient, β_3 , represents the additional change in outcome Y from the pre-policy to the post-policy period in the treated group compared to the control group. The simple two-group, two-period example used for explication can be easily expanded to accommodate multiple groups and multiple time periods (94, 96, 101).

In the case of a binary outcome, the DD model could be estimated using a linear probability model (OLS), a generalized linear model with a binomial distribution and an identity or log link, or a logistic regression model(96). The first two models make the implicit assumption that the joint effect pattern between exposure and time is additive, while the latter two assume that it is multiplicative. The DD literature mostly relies on the additive scale to assess parallel trends and to facilitate interpretation of estimated effects on the absolute probability scale. If there is preference to fit non-linear models because of their statistical advantages, effects can still be estimated on the additive scale using post-estimation commands (e.g., "margins") to compute marginal effects of the treatment.

Figure 3.1. Differences-in-Differences design



Differences-in differences design showing pre and post intervention period. Adapted from Saeed S, Moodie EEM, Strumpf EC, Klein MB. Evaluating the impact of health policies: using a difference-in-differences approach. Int J Public Health. 2019;64(4):637-42

Assumptions

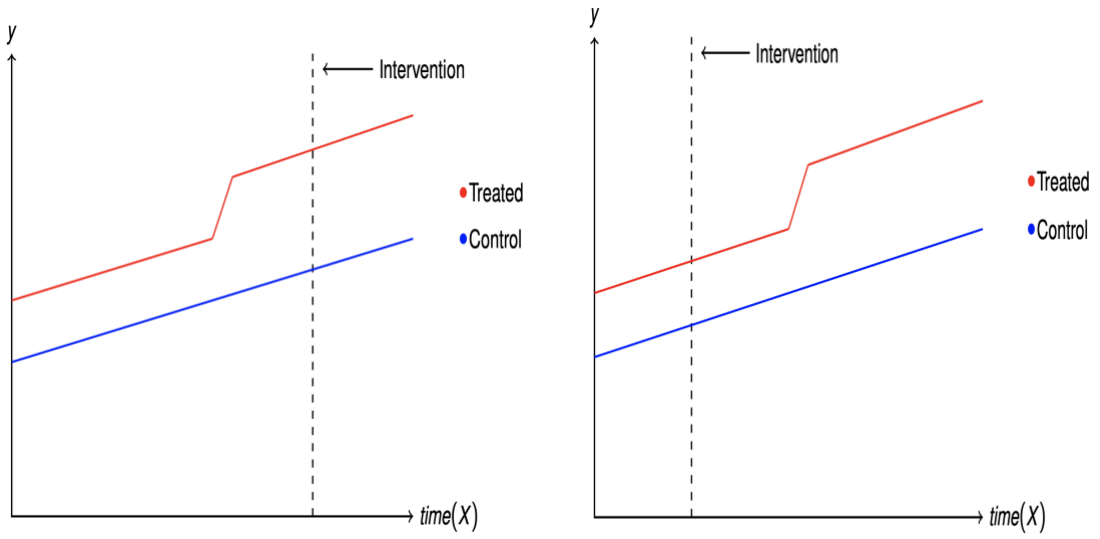
The validity of the DD model depends on the assumption that the intervention is as good as random, i.e., independent of unobserved time-varying confounders, no other parameters change differentially between the two groups during the study (94, 98). The assumption of parallel trends cannot be empirically verified, but we can assess its credibility by comparing trends in the outcome in the pre-intervention period, with evidence of parallel pre-intervention outcome trends suggesting that the control group serves as an adequate proxy for the counterfactual. The choice of control group is therefore of fundamental importance for the validity of the DD estimate. This assumption is often verified in two ways: 1) graphically by examining plots of outcome trends for each group during the pre-intervention period, and/or 2) modelling and testing for heterogeneity of outcome trends across groups during the pre-intervention period. When appropriate control groups are not available to achieve parallel trends, alternative methods include: (i) selecting a subset of control units that have an average pre-intervention outcome trend that is parallel to corresponding trends in the treated group and (ii) using ‘‘synthetic controls’’ or propensity score weighting to identify a combination of control units that better substitute for the counterfactual (102, 103)

Another assumption necessary for a causal interpretation of the DD estimate is that the policy change is exogenous (95, 100). This indicates that the policy change is not a result of pre-policy outcomes (no reverse causation) or influenced by unmeasured time-varying confounding from other events that co-occur with the policy and influence the outcome. For example, if broad health reforms occurred around the time of an abortion policy change and influenced reproductive and neonatal health outcomes, the DD model might erroneously attribute these

effects to the abortion policy change. Although the assumption of no unmeasured confounding cannot be verified, substantive knowledge concerning the policy change and the conditions under which it occurred can help improve the validity of the DD approach by omitting control groups that may not substitute for the counterfactual and informing which measured covariates to control for in the regression framework. If the above assumptions underlying the DD approach are valid, the method provides a valid estimate of the ATT (100).

Other techniques, such as examining lead and lag effects and testing the effect of the policy on a negative control outcome (i.e., an outcome for which an effect of the treatment would not be expected, but that would be affected by any confounding policy changes), can also be used as sensitivity analyses for the DD evaluation. Lead-time effects examine whether effects were observed before the adoption of the policy, which may suggest that pre-treatment levels or changes in the outcome may have prompted policy developments (95). Conversely, lagged effects examine whether there was a delay before the treatment had a measurable impact on the outcome and whether it was temporary or sustained over time.

Figure 3.2. Lead and Lag Effect



The left figure represents the lead effect, and the right figure represents the lag effect.

In generating standard errors for our effect estimates, we must consider the possibility of correlation between model errors due to clustering. The use of fixed-effects terms for group, the estimation of cluster-robust standard errors, and bootstrapping are some of the ways that have been proposed to address this issue (100, 104).

3.3.2 Misclassification Simulation Extrapolation (MC-SIMEX)

To address the potential misclassification of neonatal mortality, we employed the MC-SIMEX method. In Cook and Stefanski's 1994 introduction of SIMEX (Simulation-Extrapolation) (105), they made no distributional assumptions about misclassified data. SIMEX leverages the link between measurement error and naive estimator bias. The method adds measurement error σ_u^2 to the data and measures induced bias in relation to the error variance. After

establishing a trend between error and biased estimates, it extrapolates back to the case of no error.

In 2006, Kuchenhoff et al.(106) extended the SIMEX approach to correct coefficient estimates for potential misclassification of a binary outcome or binary covariates. The naïve estimator β^* refers to the potentially biased estimate that ignores measurement error. For a binary variable, the misclassification error can be described by the misclassification matrix Π instead of σ_u^2. If we denote the possibly biased outcome variable as Y^* and the true value as Y, the misclassification matrix Π is defined as $\pi ij = P(Y^* = i|Y = j)$ (a k × k matrix, where k is the number of possible outcomes for Y). The misclassification matrix for a binary outcome is

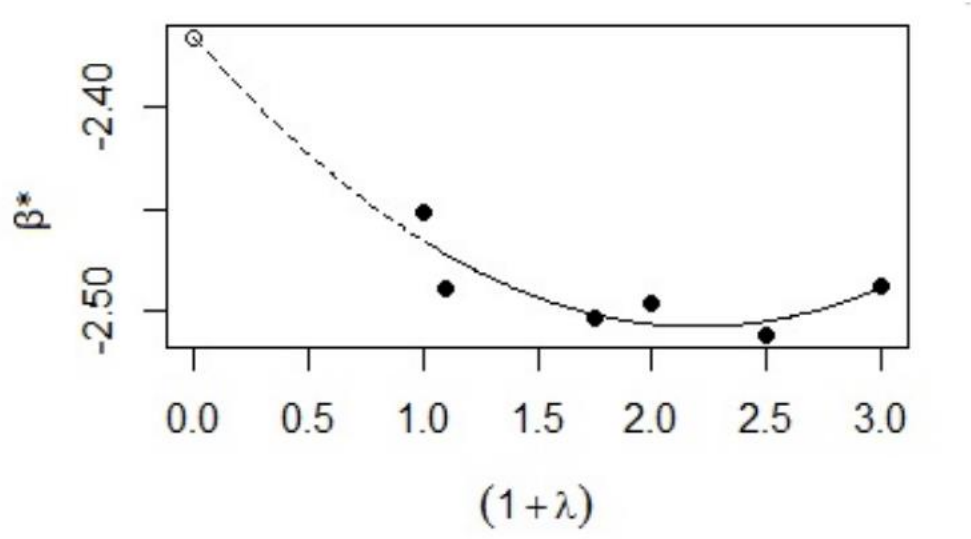
$$\Pi = \begin{pmatrix} \pi 00 & 1 - \pi 11 \\ 1 - \pi 00 & \pi\, 11 \end{pmatrix}$$

, where $\pi 11 = P(Y^* = 1|Y = 1)$ denotes the sensitivity and $\pi 00 = P(Y^* = 0|Y = 0)$ denotes the specificity. The MC-SIMEX estimator λ can be defined by a parametric approximation of

$$\lambda \longrightarrow \beta^*\left(\Pi^\lambda\right) = f(1 + \lambda)$$

, where Π^λ can be expressed as $\Pi^\lambda = E\Lambda^\lambda E^{-1}$ via spectral decomposition, with Λ being the diagonal matrix of eigenvalues and E the corresponding matrix of eigenvectors. Then by performing a similar simulation step (i.e., generate pseudo data and compute the naive estimators for each λ) and extrapolation step (i.e., fit a curve for the relationship of $X = \lambda$ vs. $Y = f(1 + \lambda)$ and find the Y value that corresponds to $X = \lambda = -1$), the MC-SIMEX estimator is computed as $\hat{\beta}\, MCSIMEX = \hat{f}(0)$.

Figure 3.3. Proportion of Measurement Error $(1 + \lambda)$ vs Naive Estimate $(\beta *)$



The y-axis represents the proportion of Measurement Error $(1 + \lambda)$, while the x-axis represents the naive estimate $(\beta *)$ with the Superimposed Estimated Extrapolating Function. Adapted from Stanley B. Outcome Misclassification in Logistic Regression: Examining Hospitalization Risk and its Association with Health Literacy. Vanderbilt University Institutional Repository. Nashville, Tennessee 2019

The determinant must be greater than 0 for these functions to be well-defined i.e., both the specificity and sensitivity must be more than 0.5. This is intuitive, and it aligns with earlier studies that show that when specificity or sensitivity are less than 50%, the data collection method performs worse than chance, leaving the data unusable. Misclassification probabilities can be calculated using a validation sample or from hypothesized values. Linear, quadratic (the most frequent), and log-linear distributions are commonly utilised (107).

CHAPTER 4. Effect of abortion restriction in the Dominican Republic on neonatal mortality and contraceptive use: a difference-in-differences analysis

4.1 Preface: Manuscript 2

This manuscript aims to address gaps identified in the systematic review, such as the dearth of studies evaluating the impact of abortion restriction from LMICs and underuse of designs with a strategy to identifying the impact of abortion reforms.

This study is the first to assess the impact of abortion restriction in the Dominican Republic. I applied the DD design described in Chapter 3 to estimate the impact of abortion restriction on neonatal mortality and contraceptive use and examine heterogeneity across socioeconomic groups.

4.2 Manuscript 2

Title: Effect of abortion restriction in the Dominican Republic on neonatal mortality and contraceptive use: a difference-in-differences analysis

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Abstract

Background: In 2009, the Dominican Republic's Constitutional Assembly banned abortion under all circumstances, including cases of rape and/or situations in which the mother's health is at risk. Abortion policies have the potential to influence access to reproductive and neonatal health services, health outcomes, and health equity. Limited research has examined their implications for neonatal mortality and contraceptive use. In this study, we evaluated the impact of the 2009 constitutional abortion reform in the Dominican Republic on neonatal mortality and modern contraceptive use using a difference-in-differences design.

Methods: We used data from Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS). We estimated the effect of abortion restriction on the probability of neonatal mortality and modern contraceptive use by comparing outcome trajectories in the Dominican Republic to a group of 16 control countries that did not change their abortion policies during the study period (1999-2019) and are assumed to represent the counterfactual. Fixed effects for country and year were included to control for unobserved time-invariant confounders that varied across countries and temporal trends that were shared across countries, respectively.

Results: Abortion restriction was associated with an additional 6.3 (95% CI=2.1, 10.5) neonatal deaths per 1,000 live births and a 9.6 (95% CI=4.2, 15.0) percentage-point decrease in modern contraceptive use. The adverse effect of abortion restriction on neonatal mortality was stronger among households in the lowest quintile of household SES, women with no education and those residing in rural areas.

Conclusion: Our analyses suggest that restrictive abortion policy was associated with an increase in neonatal mortality and decrease in modern contraceptive use in the Dominican Republic. This relation varied by household wealth, educational and residency status, suggesting that restrictive abortion policies may exacerbate socioeconomic inequities. Capturing the causal impact of abortion reforms is important to understand how these laws impact health outcomes and health inequities.

Keywords: Abortion restriction, neonatal mortality, contraceptive use, health inequalities, impact evaluation, Dominican Republic

INTRODUCTION

Abortion is a common but controversial procedure globally and the subject of highly politicised debate. The legality of abortion is often restricted, leaving a substantial number of women without access to safe abortion (1, 2). About 42% of women of reproductive age live in countries with highly restrictive abortion laws and 93% of these women are in low and middle income countries (LMICs)(3, 4).

The legal and ethical debate over abortion is embedded in every country's socioeconomic and political context, which varies across the world. Current literature points to various socioeconomic, religious, cultural, and political correlates of abortion laws (5-7). Political affiliation and religious sentiments of elected leaders have been reported as important predictors of abortion legislation (7, 8).

Restrictive abortion law is hypothesized to force women to continue with unintended pregnancies (9, 10). Increased psychosocial stress has been reported among women with unintended pregnancies (11, 12). Studies have reported more frequent pregnancy complications (e.g., pre -eclampsia) and subsequent adverse neonatal outcomes among women with psychosocial stressors such as anxiety and depression during pregnancy (13-15).

In addition, women with unintended pregnancies may face additional structural barriers, including unwillingness or inability to access comprehensive and culturally appropriate prenatal care, which may contribute to pregnancy complications and adverse neonatal outcomes (16-20). For instance, studies have reported that women with unintended pregnancies were more likely to delay or avoid prenatal care, decline folic acid or multivitamins, smoke cigarettes, consume alcohol, use illicit drugs, and develop pregnancy complications

resulting in brain damage, neural tube defect, preterm delivery, low birth weight or fetal death (21-23).

There is evidence of associations between abortion reforms and a range of reproductive and neonatal outcomes, including contraceptive use and neonatal mortality (24-26). Most studies from LMICs have evaluated laws that have liberalized abortion access. For example, studies in Uruguay and Romania reported a decline in the probability of low birth weight following reforms that made abortion more accessible (24, 27). Other studies reported increases in the use of modern contraceptives following abortion liberalization (25, 28).

Evaluations of abortion restrictions on neonatal outcomes and contraceptive use have been conducted almost exclusively in high-income countries and results have been inconsistent (29-33). Abortion reforms may also have heterogeneous effects even in countries that experience similar reforms, as the legislative processes that are associated with changing abortion laws take place in highly distinct political, economic, and social contexts (34, 35).

Importantly, most studies in LMICs have applied pre-post designs comparing outcomes before and after abortion reforms (36). Comparisons of outcomes in the same population before and after a policy change may be biased due to underlying secular trends affecting outcomes, limiting causal inference. Capturing the causal impact of abortion reforms is important to understand how these laws impact health outcomes and inequities and to strengthen the evidence-base for informing abortion legislation. Quasi-experimental designs, such as difference-in-differences (DD), can inform causal inference by accounting for underlying secular trends in outcomes of interest using a series of control countries to proxy the

counterfactual outcome trajectories of the treated country, had it not adopted the abortion reform (37).

In this study, we evaluated the impact of a 2009 constitutional abortion reform in the Dominican Republic using a DD design. Using a series of control countries to account for secular trends, we estimated the causal effect of abortion restriction on neonatal mortality and modern contraceptive use among women of reproductive age.

BACKGROUND

Abortion rights have long been highly controversial in Dominican Republic. The country has among the most legally restrictive abortion laws and policies globally, limiting the provision of abortion and post-abortion care (38, 39). The first Constitution of the Dominican Republic was promulgated in 1844, immediately after the nation achieved independence from Haiti. By 1966, the Dominican Republic had adopted thirty-five constitutional amendments (40). To better understand the context for abortion reforms in the Dominican Republic, it is important to highlight that Roman Catholicism is the official religion of the Dominican Republic, established by a Concordat with the Vatican (41). Since most of the population is Catholic and the nation does not have a separation of church and state ideology in its legal system, the Catholic Church has always played a major role in legislation and has been consistently in favor of a total ban on abortion (42).

In October 2009, the Dominican Republic's Constitutional Assembly passed the thirty-eighth version of its Constitution, including more than forty amended articles that took effect

on January 26, 2010. One of the most significant measures introduced by the constitutional reform was the inclusion of article 37 which establishes that the right to life is inviolable from conception until death (43, 44). Effectively, this provision banned abortion under all circumstances, including cases of rape and situations in which the mother's health is at risk. Supported by the nation's strong Catholic beliefs, the amendment was approved by a majority vote of 128 to 34 in the Dominican Parliament. Consequently, the Dominican Republic became the fifth nation in the world with a complete (no exceptions) ban on abortion after Malta, El Salvador, Nicaragua, and Vatican City.

The Dominican Congress tried to enact a new criminal code, Law No. 550-14, in 2014 to decriminalize abortion in three circumstances: if the pregnancy poses a risk to the life of a pregnant woman or girl, if the fetus could not survive outside the womb, and if the pregnancy is the result of rape or incest (45, 46). However, the Constitutional Court declared this law unconstitutional (TC/0599/17) leaving the previous Criminal Code enacted by Decree-Law No. 2274, which imposed prison sentences from six months to two years on women and girls who received an abortion and individuals who aided them and five to twenty years for medical professionals who performed them (44). Although criminal actions against women and girls who seek abortions, and those who help them, are relatively rare, the law has created a pervasive fear that drives women and girls to desperate measures to end unwanted pregnancies, and leaves healthcare providers unable to protect the health and lives of their patients (42, 47).

METHODS

Data Sources

We used data from Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) conducted between 1999-2019. The DHS are nationally representative household surveys that are repeated approximately every 5 years to monitor trends in population health in LMICs. Standard DHS use a two-stage cluster sampling design, with the first stage selecting clusters and the second stage selecting households. Structured questionnaires are administered to all women ages 15-49 who spent the night before the survey in the household. Information is collected on a wide range of health-related and socio-demographic characteristics, with an emphasis on maternal and child health. In addition to collecting demographic information, the surveys also collect information on household assets and features of the dwelling units. Trained interviewers and standardized tools and measurement techniques are used to ensure comparability of surveys across countries and survey waves. Further details regarding sampling strategies and study procedures are available elsewhere (48, 49).

The MICS are nationally representative household surveys that employ similar sampling strategies, methods, and measures to the DHS, and similarly collect information on birth histories, fertility, mortality, contraceptive use, maternal and newborn health, and other key indicators from interviews with household members (50). Many studies have used harmonized data from the DHS and MICS for assessing trends in reproductive, maternal, and neonatal health outcomes (51-53). In this study, we considered birth histories of 5 years (up to 59 months) before the interview, for consistency across surveys and to minimise self-reporting errors (54).

Sample

In addition to the Dominican Republic, we included 16 potential control countries that did not implement changes to their abortion laws during the study period (1999-2019). Controls were selected based on the following criteria: (i) similar middle-income classification as the Dominican Republic as specified in the World Bank Data Catalogue at the start of the study period, (ii) at least two available DHS/MICS surveys, including at least one conducted prior to the intervention, and (iii) no evidence of major reproductive health reforms during the study period that might have affected our primary outcome of interest.

We created two separate samples, one for our analyses of neonatal mortality and the other for analyses of modern contraceptive use. For neonatal mortality, our sample comprised all live births occurring to DHS/MICS respondents between 1999 and 2019. Women were asked to provide birth histories up to 5 years before the interview and those reporting a deceased child were asked to give the age at death. These data were used to construct a panel of live births by year of birth. We restricted to live births that occurred at least 29 days prior to the interview date to ascertain whether each child survived the neonatal period following birth. Thus, these analyses included 719 152 children born between 1999 and 2019 from 63 DHS/MICS surveys across 17 countries including the Dominican Republic. As pre-reform trends in neonatal mortality between 1999 and 2009 for control countries appeared relatively parallel to corresponding trends for the Dominican Republic, we did not eliminate any control countries for analyses of this outcome.

For current use of modern contraceptives, our sample included women of reproductive age (15-49 years) surveyed in the DHS/MICS between 1999 and 2019 who provided information concerning modern contraceptive use at the time of interview. We lacked annual data on modern contraceptive use because the contraceptive calendar providing a history of contraceptive use up to 5 years prior to the interview date was not available for most of our sampled countries. As such, we were limited to data on modern contraceptive use at the time of interview. We restricted our sample of control countries to those with similar pre-intervention trends compared to the Dominican Republic, in order to identify a credible counterfactual for the Dominican Republic had it not reformed its abortion law. To avoid violations of the parallel trends assumption, we eliminated potential control countries if their change in the annual rate of modern contraceptive use between 1999 and 2009 deviated from the change in the annual rate for the Dominican Republic by more than 1 percentage point on the absolute scale (0.88 ± 1 percentage point per year). Our resulting sample for analyses of modern contraceptive use included 417,110 women ages 15-49 between 1999 and 2019 from 31 DHS/MICS surveys across 8 countries (i.e., Dominican Republic, Turkey, Namibia, Cambodia, Egypt, Haiti, Zambia, Zimbabwe). Supplementary Tables 1 and 2 reports survey years, birth years and sample sizes for the sampled countries

Measures

The exposure of interest was the adoption of a policy restricting abortion in the Dominican Republic, which was evaluated in relation to two binary outcome measures: neonatal mortality and modern contraceptive use. Neonatal mortality was measured by a

binary indicator of whether a child who was born alive died within the first 4 weeks of life using the 5-year birth histories provided by women interviewed in the DHS/MICS. Modern contraceptive use was measured by an indicator of whether the respondent reported that they or their partner used at least one modern contraceptive method at the time of interview. Modern methods referred to any of the following: female sterilisation, male sterilisation, oral contraceptive pills, the intrauterine contraceptive device, injectables, implants, male and/or female condoms, diaphragms, contraceptive foam, and contraceptive jelly, and lactational amenorrhoea method, and other modern contraceptive methods (including cervical cap, contraceptive sponge, and others).

We identified covariates associated with neonatal mortality and modern contraceptive use in LMICs based on relevant literature (55-58). Controlling for strong predictors of our outcomes has the potential to improve the precision of our effect estimates, and account for compositional changes in our sample over time. For neonatal death, covariates at the individual and household levels included birth interval (≥ 24 months, <24 months), plurality of birth, women's age at birth, mother's education (none, primary, secondary, or higher), number of living children, place of residence (urban or rural) and household wealth. For contraceptive use, covariates at the individual and household levels included women's education (none, primary, secondary, or higher), women's age, number of living children, marital status (single, married, separated, divorced, or widowed), place of residence (urban or rural), and household wealth.

For household wealth, we used the polychoric dual-component wealth index (P2C) as an alternative to the DHS wealth index to address the "urban bias" towards patterns of urban wealth (59). The P2C wealth index uses a similar set of assets as the standard DHS wealth index,

based on a set of variables related to household conditions (e.g., water source, sanitation facilities, electricity) and ownership of consumer goods (e.g., a bicycle, a telephone, a refrigerator, agricultural land and domestic animals). However, unlike the DHS wealth index, categorical variables are converted into ordered categorical (ordinal) variables to harness additional discriminatory power and avoid the questionable use of multiple binary (dummy) variables to represent a single asset. This approach takes better account of key characteristics of rural wealth and is designed to redress the urban bias of the standard DHS approach.

In addition, to account for potential confounding by country-level characteristics that may be associated with changes to abortion policies and with our outcomes, we included measures from the World Bank's World Development Indicators and Global Development Finance databases (60). These variables included gross domestic product (GDP) per capita (constant 2017 international dollars) based on purchasing power parity (PPP), the female labour force participation rate (percentage of female population ages 15–64 years), government health expenditures per capita based on PPP (constant 2017 international dollar) and total health expenditure.

Statistical Analysis

We examined the effects of abortion restriction in the Dominican Republic on use of a modern contraceptive and neonatal mortality using a linear probability model of the general form:

$$P(Y_{ict}) = \mathrm{b}_0 + \mathrm{b}_1 * Policy_{ct} + \mathrm{g}_t + \mathrm{d}_c + \sum \mathrm{b}_n * X_{ict} + \sum \mathrm{b}_k * Z_{ct} + \varepsilon_{ict},$$

where b_1 measures the additive effect on the risk difference scale of the abortion law reform on the probabilities of neonatal death and current modern contraceptive use, Y_{ict} , for each observation i in country c in birth/survey year t. $Policy_{ct}$ is a time-varying indicator for whether the observation was before or after the implementation of the abortion reform in the Dominican Republic. We included fixed effects for country, d_c , and year, g_t , to account for unobserved time-invariant confounders that vary across countries and temporal trends in our outcomes that are shared across countries, respectively. X_{ict} is a vector of measured individual/household level covariates and Z_{ct} is a vector of time-varying country-level potential confounders.

The effects of the abortion law reform were identified by changes in outcomes occurring within the Dominican Republic (treated country) during the study period relative to corresponding changes in outcomes in countries that did not modify their policies during the study period (control countries). In the first model, we estimated the effect of the abortion law reform on both outcomes after including country and year fixed effects (Model 1). In the second model, we additionally controlled for individual and household characteristics (Model 2). In the third model, we included country-level characteristics, specifically per capita GDP, female labor force participation, per capita total, and government health expenditures (Model 3). In addition, we conducted stratified analysis to examine heterogeneity in effect estimates by household wealth, educational status and rural/urban residency. We used tests of homogeneity to provide statistical evidence of whether effects were similar across strata.

All models incorporated sampling weights to account for individual survey sampling designs. Per DHS guidelines, we used information on the number of women aged 15–49 years in each survey-year, available from the Population Division of the United Nations (61), and applied the de-normalization of standard weights approach described in the DHS Sampling and Household Listing Manual (62), to calculate an appropriate sampling weight for each observation in the analyses. We also estimated robust standard errors to account for clustering at the country level. We performed all analyses using Stata version 15(63). The DHS data are publicly available, but users must first register with the DHS program. The statistical code for producing the analytic dataset and replicating our results (given DHS data that must be downloaded from the DHS program), are also available.

Sensitivity analyses

We conducted sensitivity analyses to assess the robustness of our main findings. First, we examined whether our results were sensitive to the inclusion of sampling weights by reporting unweighted estimates. Second, we estimated the effect of the abortion policy change on both outcomes using a logistic regression model, with using post-estimation to obtain comparable marginal effects on the risk difference scale. Third, we examined whether the association between abortion restriction and neonatal mortality varied by timing of exposure. Based on the mechanisms through which abortion reforms are hypothesized to influence neonatal mortality, such as increased psychosocial stressors during pregnancy and poor prenatal care, it seems plausible that some births occurring immediately after the reform may have been unaffected by the reform while in utero. As such, we modified the exposure

definition to include estimated time of conception (266 days from birth). Fourth, for the analyses of neonatal mortality, we added to our primary exposure indexed by birth year (t), parameters representing the potential lead effect of the reform in preceding years (t-1, t-2, t-3) and lagged effect of the reform in subsequent years (t+1, t+2, t+3). This was used to examine whether there were delayed or persistent effects of the policy reform, as well as whether associations were observed prior to the implementation of the abortion reform, which would be inconsistent with a causal impact of the reform. Fifth, for contraceptive samples, we assessed whether our findings were sensitive to the selection of control countries by including control countries with an annual rate of change within 0.5 percentage points on the absolute scale (0.88 ± 0.5 percentage point per year); there were five remaining control countries (i.e., Namibia, Cambodia, Egypt, Haiti & Zimbabwe).

RESULTS

Table 4.1 shows the pre-intervention distributions of individual, household, and country level covariates for treated and control countries.

Table 4.1. Distribution of sample characteristics for treated and control countries in the pre-intervention period (1999-2008)

	Contraceptive Sample N=201,166 women		**Neonatal Sample N=451,966 live births**	
	Treated	**Control**	**Treated**	**Control**
Mean (SD) contraptive use prevalence	0.51 (0.49)	0.35 (0.48)		
Mean (SD) neonatal mortality			0.020 (0.14)	0.029 (0.17)
Individual-level and household-level and covariates				

Mean (SD) number of children alive	2.1 (1.9)	2.4 (2.2)	2.9 (1.7)	3.6(2.1)
Mean (SD) mother's education (years)	8.3 (5.1)	5.8 (4.9)	7.9 (4.5)	5.3 (5.0)
Mean (SD) mother's age (years)	29.5(9.9)	30.6(9.6)		
Mean (SD) mother's age at birth (years)			24.4(6.0)	27.0 (6.5)
Educational attainment				
No schooling	2,561 (4.9%)	37.668 (25.3%)	2,019 (5.4 %)	144,777 (35.0%)
Primary	23,336 (45.0%)	51,806 (34.7%)	17,048 (45.2%)	115,560 (27.9%)
Secondary+	25,968 (50.1%)	59,827 (40.1%)	18,689 (49.5%)	153,487 (37.1%)
Residence				
Rural	20,103 (38.8%)	87,574 (58.7%)	15,457 (40.5%)	257,973 (62.3%)
Urban	31,762 (61.2%)	61,727 (41.3%)	22,676 (59.5%)	155,860 (37.7%)
Wealth Index				
Poorest	796 (3.6%)	27,426 (19.6%)	1197 (6.7%)	107,613 (26.7%)
Second	2,554 (11.5%)	32,609 (23.3%)	2721 (15.2%)	82,061 (20.4%)
Middle	4,909 (22.1%)	29,287 (20.9%)	4132 (23.0%)	76,236 (18.9%)
Fourth	7,305 (32.9%)	34,708 (24.8%)	4958(27.6%)	78,814 (19.6%)
Wealthiest	6,646 (29.9%)	15,875 (11.4%)	4928 (27.5%)	57,964 (14.4%)
Marital Status				
Single	11,676 (22.5%)	29,539 (19.8%)	641 (1.7%)	10,470 (2.5%)
Married/common law	31,129 (60.0%)	107,248(71.8%)	31,444 (82.5%)	383,523(92.7%)
Divorced/Separated/Widowed	9,060 (17.5%)	12,511 (8.4%)	6048 (16.0%)	19,834 (4.8%)
Type of Birth				
Singleton			37,288 (97.8%)	399,918(96.6%)
Twin/Triplet			845 (2.2%)	13,915 (3.4%)
Birth Interval				
>=24months			24,563 (64.4%)	336,030(81.2%)
< 24months			13,570 (35.6%)	77,803 (18.8%)

Birth Order				
First			9,668 (32.0%)	101,047(24.4%)
Second			8,080 (26.7%)	85,933 (20.8%)
Third			6,073 (20.1%)	67,136 (16.21%)
Fourth+			6,435 (21.3%)	159,717(38.6%)
Country-level covariates				
Mean (SD) GDP per capita, PPP (constant 2017 international $)	10757.8(1053.2)	6517.7(4770.8)	11997.5(1656.1)	5663.8(3873.6)
Mean (SD) government health expenditure per capita, PPP (current international $)	143.7(23.5)	138.3(150.5)	204.2(86.4)	104.4(149.1)
Mean (SD) labour force participation rate, female (% of female population ages 15+ years)	40.5(1.6)	48.1(23.8)	41.7(2.3)	51.1(22.2)
Mean (SD) total health expenditure per capita, PPP (constant 2017 international $)	384.9(32.2)	327.3(235.7)	500.5(169.6)	269.5(230.8)

Mean values are weighted by Demographic and Health Survey weight. Values are numbers (percentages) unless stated otherwise. GDP, gross domestic product; PPP, purchasing power parity. Treated country is Dominican Republic. Control countries are the 16 countries that did not experience a change in abortion policy during the study period.

Over the study period, the rate of neonatal mortality was 27.0 per 1,000 live births and 39.0% of women reported using modern contraceptives. Trends in neonatal mortality and modern contraceptive use in treated and control countries were stable in the pre-treatment period (Figures 4.1 and 4.2). Supplementary Figure 4.1 shows similar pre-intervention trends in neonatal mortality for treated and control countries. Supplementary Figure 4.2 shows trends for countries with similar pre-intervention trends in modern contraceptive use as the Dominican Republic.

Figure 4.1. Trends in neonatal mortality over the study period for the Dominican Republic and control countries

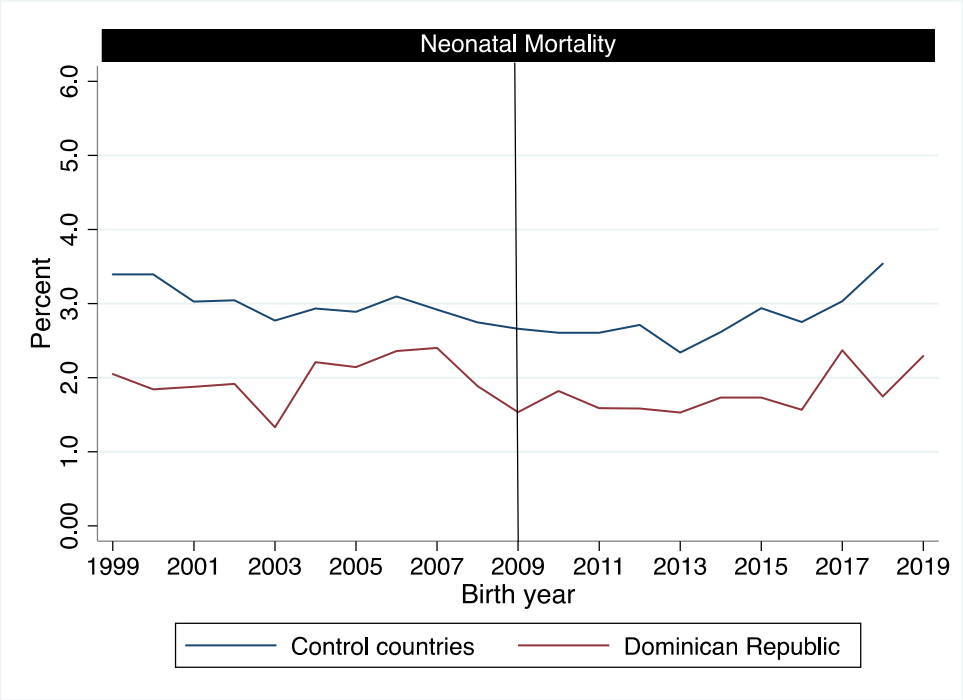
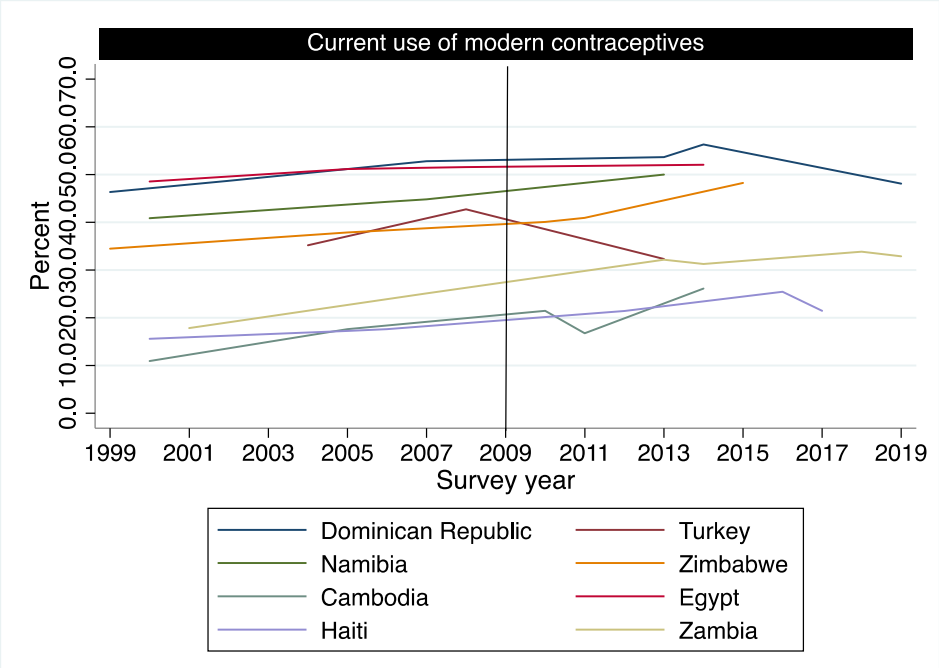


Figure 4.2. Trends in current contraceptive use over the study period for the Dominican Republic and control countries



Socioeconomic gradients in neonatal mortality and contraceptive use

There was evidence of socioeconomic gradients in neonatal mortality in the overall study sample. There were, on average, 8.9 (95% Confidence Interval (CI)=1.0, 16.9) fewer neonatal deaths per 1,000 live births in households in the highest wealth index quintile compared to the lowest wealth index quintile. Rural residence was associated with 1.6 (95% CI=0.5, 2.7) additional neonatal deaths per 1000 live births compared to urban residence. Birth characteristics, including short interval from the previous birth (<24 months) and maternal age at birth (<20 and >35 compared to 20-34 years) were associated with neonatal mortality. For example, a short birth interval was associated with an additional 35.9 (95% CI=24.5, 47.4) neonatal deaths per 1,000 live births. Giving birth before the age of 20 years or after age 35 years was associated with an additional 11.3 (95% CI=3.7, 18.9) and 33.5 (95% CI=25.4, 41.5) neonatal deaths per 1,000 live births, respectively.

The use of modern contraceptives was 4.9 (95% CI=0.9, 9.0) percentage-points higher among women of reproductive age in the highest quintile of household SES compared to the lowest quintile. Use of modern contraception was 3.4 (95% CI=0.9, 6.0) percentage-points lower among rural compared to urban residents and 7.3 (95% CI=5.3, 9.4) percentage-points higher among women with at least a secondary education compared to no education. Use of contraceptives was 10.1 (95% CI=4.6, 15.7) and 5.3 (95% CI=0.8, 9.9) percentage-points lower, respectively, among women less than 20 years of age or at least 35 years of age at the time of survey compared with maternal ages 20-34. Country-level characteristics were not associated with neonatal mortality or modern contraceptive use.

Impact of restricting abortion

Table 4.2 shows the effect of abortion restriction on the probability of neonatal death. In the fully adjusted model (Model 3), abortion restriction was associated with an additional 6.3 (95% CI=2.1, 10.5) neonatal deaths per 1,000 live births. Table 4.3 shows the effect of abortion restriction on the probability of modern contraceptive use. In the fully adjusted model (Model 3), abortion restriction was associated with a 9.6 (95% CI=4.2, 15.0) percentage-point decrease in modern contraceptive use.

Table 4.2. Effect of abortion restriction in the Dominican Republic on the number of neonatal deaths per 1,000 live births

	Model 1			**Model 2**			**Model 3**		
	Est	*LCL*[a]	*UCL*	*Est*	*LCL*	*UCL*	*Est*	*LCL*	*UCL*
Abortion restriction	**4.5**	**2.2**	**6.9**	**7.7**	**4.9**	**10.4**	**6.3**	**2.1**	**10.5**
	Individual and household-level covariates[b]								
Number of children alive				-22.1	-28.5	-15.8	-22.5	-28.7	-16.3
Primary education				-2.5	-5.8	0.8	-2.7	-5.4	-0.1
Secondary+				-10.0	-13.3	-6.8	-9.9	-13.2	-6.7
Rural residence				1.9	0.3	3.5	1.6	0.5	2.7
2nd wealth quintile				-4.4	-8.9	0.1	-4.7	-9.1	-0.3
3rd wealth quintile				-5.1	-9.1	-1.2	-5.9	-9.2	-2.6
4th wealth quintile				-9.4	-17.4	-1.4	-10.1	-17.5	-2.7
5th quintile (highest)				-7.6	-16.6	1.2	-8.9	-16.9	-1.0
Married/common law				-17.2	-8.5	-25.9	-16.9	-9.7	-24.1
Divorced/Separated				-9.9	-2.1	-17.8	-9.6	-4.6	-14.6
Maternal age <20				11.2	3.6	18.7	11.3	3.7	18.9
Maternal age >=35				33.1	25.1	41.1	33.5	25.4	41.5
Twin/triplet				132	107	158	131	105	158
birth interval <24 months				35.7	24.2	47.3	35.9	24.5	47.4
2nd birth order				-6.3	-12.3	-0.3	-6.5	-12.4	-0.6
3rd birth order				13.6	5.4	21.8	13.8	5.7	21.8
4th birth order				53.1	39.7	66.5	53.8	40.6	67.0
	Country-level covariates								
GDP per capita							0.008	-0.00	0.01
female labor force participation							0.1	-1.4	1.7
government health expenditure							0.03	-0.04	0.1
total health expenditure							-0.1	-0.2	0.0

[a] LCL and UCL indicate lower and upper limits of the 95% confidence interval, respectively
[b] Reference categories for categorical variables are no schooling, urban (vs. rural) residence, the 1st (lowest) wealth quintile, single marital status, maternal age at birth (20-34 years), and singleton (vs. twin/triplet), >=24 month (vs. <24 month) birth interval and first birth order.

Table 4.3. Effect of abortion restriction in the Dominican Republic on the prevalence of modern contraceptive use

	Model 1			Model 2			Model 3		
	Est	*LCL*[a]	*UCL*	*Est*	*LCL*	*UCL*	*Est*	*LCL*	*UCL*
Abortion restriction	**--8.5**	**-11.8**	**-5.1**	**-7.4**	**-12.7**	**-2.1**	**-9.6**	**-15.0**	**-4.2**
	Individual and household-level covariates[b]								
Number of children alive				3.9	1.6	6.2	3.9	1.5	6.3
Primary education				6.0	1.5	10.4	5.8	1.5	10.2
Secondary+				7.2	5.2	9.4	7.3	5.3	9.4
Rural residence				-3.8	-6.3	-1.4	-3.4	-6.0	-0.9
2nd wealth quintile				2.5	1.4	3.5	2.2	1.0	3.3
3rd wealth quintile				4.0	1.1	7.1	3.7	0.3	6.9
4th wealth quintile				4.0	0.0	8.1	3.2	-0.8	7.2
5th quintile (highest)				5.3	1.3	9.3	4.9	0.9	9.0
Married/common law				16.1	4.1	28.2	15.8	4.1	27.5
Divorced/Separated				-5.9	24.3	12.5	-6.1	-25.5	13.2
Maternal age <20				-10.2	-15.7	-4.7	-10.1	-15.7	-4.6
Maternal age >=35				-5.0	-9.6	-0.5	-5.3	-9.9	-0.8
	Country-level covariates								
GDP per capita							-0.01	-0.0	-0.0
female labor force participation							-1.0	-1.6	-0.5
government health expenditure							0.13	0.1	-0.17
total health expenditure							0.0	-0.02	0.03

[a] LCL and UCL indicate lower and upper limits of the 95% confidence interval, respectively
[b] Reference categories for categorical variables are no schooling, urban (vs. rural) residence, the 1st (lowest) wealth quintile, single marital status, maternal age (20-34 years).

Heterogeneity in the effect of abortion restriction on neonatal mortality and contraceptive use

Overall, the effect of abortion restriction on neonatal mortality was stronger among households in the lowest quintile of household SES, women with no education and those residing in rural areas (Figure 4.3). Abortion restriction was associated with 8.3 (95% CI= 3.3, 13.3) additional neonatal deaths per 1000 live births among households in the lowest quintile of household wealth, compared with about 1.0 (95% CI = -1.1, 3.1) fewer neonatal deaths per 1000 live births among respondents in the highest quintile of household wealth (Supplementary

Figure 4.3. Forest plot showing heterogeneity in the effect of abortion restriction in the Dominican Republic on probabilities of neonatal mortality across subgroups

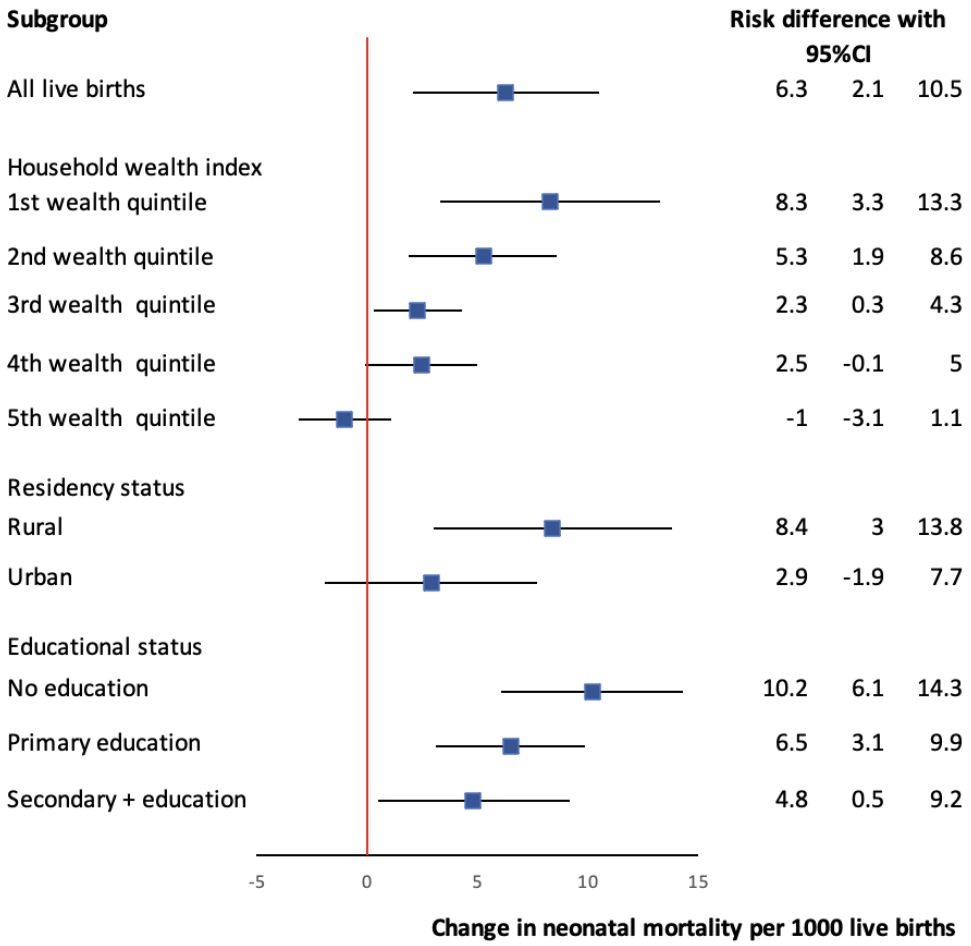
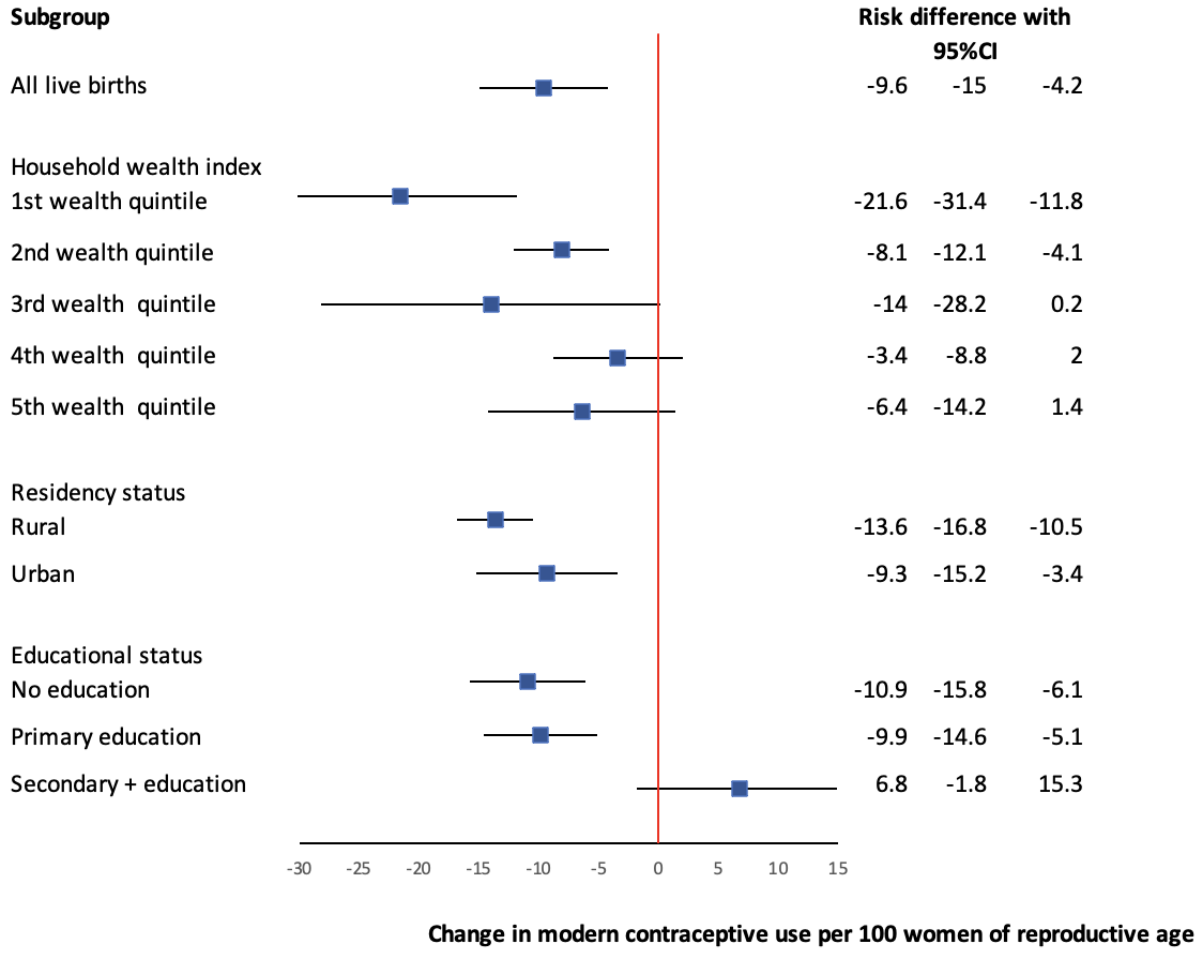


Table 4.3). The estimates were statistically different across strata (p=0.003). There were 8.4 (95% CI =3.0, 13.8) additional neonatal deaths per 1000 live births among women in rural areas compared to 2.9 (95% CI= −1.9, 7.7) additional deaths per 1000 live births in urban areas (Supplementary Table 4.4). The effects were statistically different across strata (p=0.04). There were 10.2 (95% CI= 6.1, 14.3) additional neonatal deaths per 1000 live births among women with no education compared with 4.8 (95% CI= 0.5, 9.2) additional deaths among women with at least a secondary education. The effects were statistically different across strata (p=0.02).

Figure 4.4.Forest plot showing heterogeneity in the effect of abortion restriction in the Dominican Republic on probabilities of modern contraceptive use across subgroups



For contraceptive use, abortion restriction was associated with a decrease of 6.4 (95% CI=-1.4, 14.2) percentage-points among women of reproductive age in the highest quintile of household SES compared to a decrease of 21.6 (95% CI=11.8, 31.4) percentage-points among women in the lowest quintile (Figure 4.4) (Supplementary Table 4.4). The estimates were statistically different across strata (p=0.01). Use of modern contraception was 13.6 (95% CI=10.5, 16.8) percentage-points lower among rural residents compared to a decrease of 9.3 (95% CI=3.4, 15.2) percentage-points among urban residents (Supplementary Table 4.7). The

estimates were statistically different across strata (p=0.04). There was a 6.8 (95% CI=-15.3, 1.8) percentage-points increase in contraceptive use among women with at least a secondary education compared with a decrease of 10.9 (95% CI=6.1, 15.8) percentage-points among women with no education. The effects were statistically different across strata (p<0.001).

Results from sensitivity analyses for neonatal mortality and modern contraceptive use are shown in Supplementary Table 4.5 and Table 4.6, respectively. The effects of abortion restriction on neonatal mortality were moderately robust. When logistic models were used, abortion restriction was associated with a comparable increase in neonatal mortality of an additional 9.3 (95% CI=6.4, 12.3) deaths per 1,000 live births in the fully adjusted model (Model A). The effect of abortion restriction on neonatal mortality was similar in unweighted and weighted models, with unweighted estimates showing that abortion restriction was associated with an additional 5.2 (95% CI=2.8, 7.7) neonatal deaths per 1,000 live births (Model B). When we varied the timing of exposure to include gestational period, abortion restriction was associated with an increase of 7.8 (95% CI=3.8, 9.0) neonatal deaths per 1,000 live births (Model C). For the lagged effects, abortion restriction was associated with neonatal mortality while the lead effects were closer to the null, although these parameters were estimated with substantial imprecision (Model D).

Abortion restriction was associated with a 11.7 (95% CI=8.1, 15.3) percentage-point reduction in use of modern contraceptives in logistic models (Model A). Unweighted estimates suggested that abortion restriction was associated with a 10.5 (95% CI=5.7, 15.4) percentage-point reduction in modern contraceptive use (Model B). Additionally, abortion restriction was associated with a larger absolute reduction in modern contraceptive use of 13.0 (95% CI=7.5,

18.5) percentage-points when we restricted our sample to the four control countries that more closely approximated the pre-intervention trends for the Dominican Republic between 1999 and 2009 (Model C).

DISCUSSION

We used samples of 719,152 live births in 17 LMICs and 417,110 women aged 15-49 in 8 LMICs recorded in DHS/MICS surveys between 1999 and 2009 to evaluate the impact of abortion restriction in the Dominican Republic on neonatal mortality and modern contraceptive use. Our quasi-experimental DD analyses suggest that abortion restriction was associated with an additional 6.3 (95% CI=2.1, 10.5) neonatal deaths per 1,000 live births and a 9.6 (95% CI=4.2, 15.0) percentage-point decrease in modern contraceptive use. These findings were relatively robust to alternative model specifications.

While the abortion environment in the Dominican Republic prior to the abortion reform was already restrictive with no standard guidelines for abortion access (64), our results indicate the constitutional reform banning abortion in all circumstances increased neonatal mortality and reduced modern contraceptive use. Policies restricting abortion may increase unwanted pregnancies, which may induce psychological distress among women and have detrimental effects on birth outcomes (11-13, 15). In addition, women with unwanted pregnancies may face additional barriers that put them at increased risk of adverse pregnancy outcomes, including an inability or unwillingness to access adequate prenatal care or institutional delivery and a lack of health insurance and access to health providers (16-20). A growing literature exploiting variation in state-level restrictive abortion laws in the US suggests that restrictive abortion laws

are associated with poorer birth outcomes (29-32), although it is unclear if these findings can be generalized to LMICs. Our results indicate that abortion restriction may have increased neonatal mortality in the Dominican Republic.

Evidence on the relationship between abortion restriction and contraceptive use has been mixed. Similar to some observations from higher income countries reporting a reduction in the use of more effective contraceptive methods following shifts to a more restrictive abortion environment (65, 66), our study found that restrictive abortion policy resulted in a reduction in modern contraceptive use among women of reproductive age. However, a few studies reported an increase in the use of contraceptives by women following restrictive abortion reforms (67, 68) while other studies have reported no effect on contraceptive use (69, 70). There are varied explanations for why changes in abortion access may influence patterns of contraceptive use. Some have posited that women use more effective methods of birth control in the face of anti-abortion legislation (33, 67) while others have suggested abortion restrictions undermine access to comprehensive reproductive health care, including contraceptive education, and reduce utilization (65). Some studies of individual level access to abortion have reported that increasing abortion access reduced use of more effective contraceptive methods (71-73), however research examining if restricting abortion affects contraceptive use is limited.

Our findings showed that the relationship between restrictive abortion policies and neonatal mortality differed by household wealth, rural residency, and education status, suggesting heterogeneity in the impact of restrictive abortion policies on neonatal mortality across socioeconomic dimensions and potential exacerbation of existing social inequities in neonatal mortality. Some studies have examined how restrictive abortion policies affect

socioeconomic groups differentially (31, 74). Redd et al. used linear probability models with state and year fixed effects to assess if the relation between state-level restrictive abortion policies and adverse birth outcomes varied for people of different racial/ethnic identities and education levels in the United States. They reported that increased exposure to restrictive abortion policies was associated with a 2 to 5% increase in the probability of low birthweight among those with less than a college degree compared to college graduates and a 3% higher probability of preterm birth among Black compared to non-Black individuals (74).

Our analyses also found evidence of heterogeneity in the impact of restrictive abortion policies on contraceptive use. Women with primary education or less, those in the lowest wealth quintile and those residing in rural areas had the greatest reductions in contraceptive utilization following abortion restriction. While the contraceptive prevalence in the Dominican Republic is among the highest of Latin American countries (75, 76), contraceptive uptake among teenagers is one of the lowest(77, 78). Adolescents make up 19% of the country's total population, of which 43.8% are not using a method of contraception and 22% have been pregnant, a rate that is 34% higher than average for Latin America (79, 80). The high teenage pregnancy rate is further concentrated in poorer communities and among those with no education or only primary education, which may be explained by a lack of access to contraception and sexual health education (76, 81, 82). Felker et al (65) reported differential effects among women based on race/ethnicity, income, and age, when examining the association between restrictions on abortion and effective contraceptive use in the US. Further research into how restrictive abortion policies compound racial, ethnic, and socioeconomic inequities in contraceptive use is needed.

There were limitations to our study. First, there is the potential for unmeasured confounding. We controlled for potential confounding by including individual, household, and country-level covariates and included country and year fixed effects to account for unobserved time-invariant confounders that may have varied across countries and any secular trends in outcomes that were common across countries, respectively. Nonetheless, there may have been other changes that coincided with the timing of the abortion reform in the Dominican Republic and influenced our outcomes of interest. Second, measurement of our outcomes, neonatal mortality and modern contraceptive use, are subject to underreporting or misclassification since they depend on recall by women interviewed. Accurate recall may become more difficult as the memory of the event fades with time (83) and women may not possess accurate knowledge to distinguish between neonatal death and stillbirths (84). Such underreporting or misclassification may bias our estimates. Third, we lacked longitudinal measurement on modern contraceptive use as most surveys for our sampled countries asked about current contraceptive use at the time of interview and did not administer the contraceptive calendar common in DHS surveys, making it difficult to assess evidence for pre-intervention parallel trends. However, we limited our control countries to those with a similar pre-intervention annual rate of change to form a more credible counterfactual for the Dominican Republic had it not reformed its abortion policy. Fourth, our measure of abortion restriction focused on policy enactment due to the lack of information on policy compliance and implementation. Existing evidence suggests that enacted laws may impact service provision even before they are enforced (85, 86); we assessed potential lead and lag effects to examine the sensitivity of our results to decisions regarding treatment assignment. Fifth, we lacked measures on abortion

uptake and as such cannot ascertain the degree to which abortion restriction in Dominican Republic influenced utilization of abortion. Finally, generalisation of our results should be made cautiously, as results may vary for countries with different political, cultural, religious, and sociodemographic contexts vis-à-vis the Dominican Republic.

CONCLUSION

Our analyses suggest that a reform restricting abortion was associated with an increase in neonatal mortality and decrease in modern contraceptive use in the Dominican Republic. This relationship varied by household wealth, educational attainment and rural vs. urban residence, suggesting that restrictive abortion policies may exacerbate socioeconomic inequities. Further work is needed to elucidate the mechanisms that explain this relationship. Capturing the causal impact of abortion reforms is important to understand how these laws impact health outcomes and health inequities and strengthen the evidence-base for informing abortion legislation.

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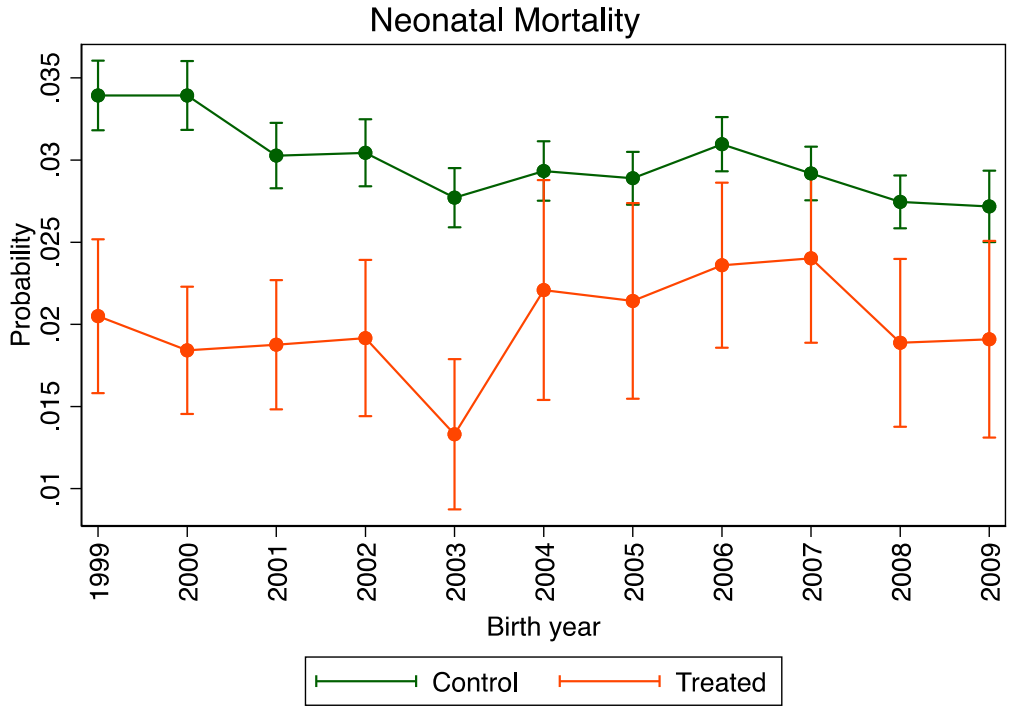
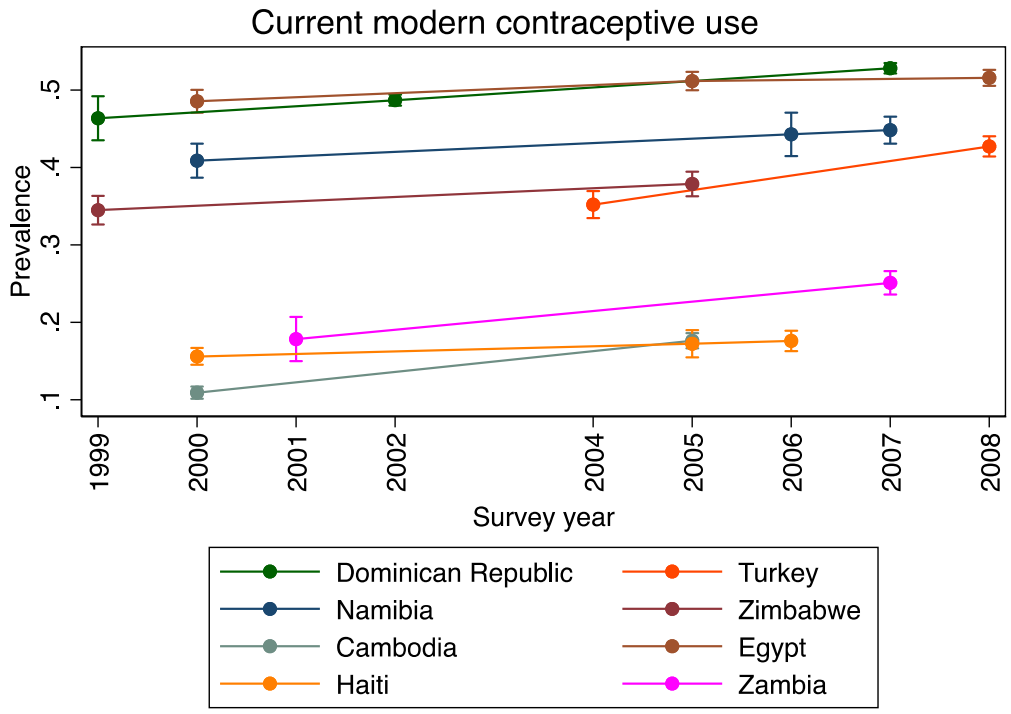
4.3 Supplemental material: Manuscript 2

Supplementary Figure 4.1: Examination of pre-intervention trends in neonatal mortality



Supplementary Figure 4.2. Examination of pre-intervention trends in contraceptive use



Supplementary Table 4.1. Surveys years, birth years and sample sizes for analyses of neonatal mortality; DHS and MICS surveys.

Country	**DHS/MICS survey years before policy**	**DHS/MICS survey years after policy**	**Birth years available**	**Sample size**
Treated country				
Dominican Republic	1999, 2002, 2007,	2013, 2014, 2019	1999-2019	73,013
Control Countries				
Turkey	2003, 2008	2013	1999-2013	17,008
Namibia	2000, 2006	2013	1999-2013	17,613
Jordan	2002, 2007	2012, 2017/18	1999-2018	55,517
Armenia	2000, 2005	2010, 2015/16	1999-2016	7690
Benin	2001, 2006	2011, 2017/18	1999-2018	70,596
Cambodia	2000, 2005	2010, 2014	1999-2014	39,482
Cameroon	2004	2011, 2018	1999-2018	42,697
Egypt	2000, 2005, 2008	2014	1999-2014	63,544
Ghana	2003, 2008	2014	1999-2014	17,816

Guatemala	1999	2015	1999, 2006-2015	19,906
Haiti	2000, 2005	2012, 2016/17	1999-2017	32,486
Nigeria	2003, 2008	2013, 2018	1999-2018	156,136
Pakistan	2006	2012, 2017	1999-2018	55,061
Philippines	2003, 2008,	2013, 2017/18	1999-2017	46,689
Zambia	2001, 2007,	2013, 2018	1999-2018	51,797
Zimbabwe	1999,2005,	2010, 2015		25,114
All control countries				719,152

Supplementary Table 4.2. Survey years, birth years and sample sizes for analyses of contraceptive use; DHS and MICS surveys.

Country	**DHS/MICS survey years before policy**	**DHS/MICS survey years after policy**	**Sample size**
Treated country			
Dominican Republic	1999, 2002, 2007,	2013/14, 2019	112,732
Control Countries			
Turkey	2004, 2008	2013	22,185
Namibia	2000, 2006/7	2013	25,829
Cambodia	2000, 2005	2010/11, 2014	62,624
Egypt	2000, 2005, 2008	2014	73,336
Haiti	2000, 2005/6	2012, 2016/17	49,663
Zambia	2001, 2007	2013/14, 2018/19	38,866
Zimbabwe	1999, 2005	2010/11, 2015	31,875
All control countries			417,110

Supplementary Table 4.3. Table showing heterogeneity in the effect of abortion restriction in the Dominican Republic on probabilities of neonatal mortality across subgroups

	Est	*LCL*	*UCL*
Abortion restriction			
Household wealth index			
1st wealth quintile	8.3	3.3	13.3
2nd wealth quintile	5.3	1.9	8.6
3rd wealth quintile	2.3	0.3	4.3
4th wealth quintile	2.5	-0.1	5.0
5th quintile (highest)	-1.0	-3.1	1.1

Residency status			
Rural	8.4	3.0	13.8
Urban	2.9	-1.9	7.7
Educational status			
No education	10.2	6.1	14.3
Primary	6.5	3.1	9.9
Secondary	4.8	0.5	9.2

Supplementary Table 4.4. Table showing heterogeneity in the effect of abortion restriction in the Dominican Republic on probabilities of modern contraceptive use across subgroups

	Est	*LCL*	*UCL*
Abortion restriction			
Household wealth index			
1st wealth quintile	-21.6	-31.4	-11.8
2nd wealth quintile	-8.1	-12.1	-4.1
3rd wealth quintile	-14.0	-28.2	0.2
4th wealth quintile	-3.4	-8.8	2.0
5th quintile (highest)	-6.4	-14.2	1.4
Residency status			
Rural	-13.6	-16.8	-10.5
Urban	-9.3	-15.2	-3.4
Educational status			
No education	-10.9	-15.8	-6.1
Primary	-9.9	-14.6	-5.1
Secondary	6.8	-1.8	15.3

Supplementary Table 4.5: Sensitivity analyses comparing the effects of abortion restriction in the Dominican Republic on the number of neonatal deaths per 1,000 live births across different model specifications

	a) Logistic estimates			(b) Unweighted estimates			c) Variation in exposure definition			d) Analysis of lags and leads		
	Est	*LCL*[a]	*UCL*	*Est*	*LCL*	*UCL*	*Est*	*LCL*	*UCL*	*Est*	*LCL*	*UCL*
Abortion restriction												
Lagged three years, t-3										**6.0**	**1.3**	**10.7**
Lagged two years, t-2										**8.9**	**3.6**	**14.2**
Lagged one year, t-1										**9.6**	**3.9**	**15.3**
Concurrent, t	**9.3**	**6.4**	**12.3**	**5.2**	**2.8**	**7.7**	**7.6**	**4.9**	**10.4**	**6.3**	**3.0**	**9.6**
Lead, t+1										**5.2**	**-0.6**	**11.0**
Lead two years, t+2										**4.5**	**-0.5**	**9.5**
Lead three years, t+3										**5.8**	**-1.0**	**12.6**
	Individual and household-level covariates[b]											
Mother's education (years)	-1.6	-2.1	-1.1	-1.4	-1.6	-1.1	-1.3	-1.6	-1.1	-1.3	-1.6	-1.0
Primary	7.9	2.9	13.1	-2.4	-5.7	0.9	-2.5	-5.8	0.7	-2.9	-5.5	-0.2
Secondary+	8.5	0.9	16.1	-10.3	-12.6	-7.9	-10.0	-13.2	-6.8	-10.3	-12.7	-7.9
Rural residence	2.5	1.1	3.9	2.5	1.6	3.5	3.3	2.5	4.4	1.6	0.5	2.7
2nd wealth quintile	-1.2	-3.2	0.8	0.2	-1.2	1.8	0.4	-1.5	2.3	-4.7	-9.1	-0.3
3rd wealth quintile	-1.5	-3.7	0.6	-0.7	-4.5	3.1	-0.1	-3.7	3.5	-5.9	-9.2	-2.6
4th wealth quintile	-4.3	-8.8	0.2	-1.9	-5.6	1.7	-1.8	-5.7	2.1	-8.9	-16.9	-0.1
5th quintile (highest)	-2.2	-8.5	4.2	-1.2	-5.3	2.9	-0.4	-4.8	3.9	-8.9	-16.9	-1.0
Married/common law	11.7	9.7	13.7	9.1	4.2	14.1	9.9	4.8	15.0	16.9	9.7	24.1
Divorced/Separated	6.6	4.9	8.4	7.1	2.8	11.4	8.0	3.2	12.7	9.6	4.6	14.6
Maternal age at birth <20	8.5	6.3	10.7	6.4	-2.8	10.1	6.8	3.1	10.4	11.3	3.7	18.9
Maternal age >=35	31.5	6.4	40.7	28.6	21.4	35.7	28.1	20.9	35.4	33.5	25.4	41.5
Number of children alive	-20.0	-22.9	-17.0	-20.0	-25.0	-15.0	-18.3	-21.3	-15.4	-22.5	-28.7	-16.3
Twin/triplet	147	140	155	116	100	132	116	98	134	131	105	158
birth interval <24 months	33.2	29.9	36.6	26.0	19.3	32.6	26.8	19.7	34.1	35.9	24.5	47.4
2nd birth order	-5.3	-7.4	-3.2	-1.5	-4.8	1.8	-1.4	-5.4	2.6	-6.5	-12.4	-0.6
3rd birth order	4.8	-1.5	11.3	12.2	7.7	16.6	12.9	7.1	18.8	13.8	5.7	21.8
4th birth order	51.2	35.3	67.1	46.4	39.5	53.3	47.7	40.0	55.4	53.8	40.6	67.0
	Country-level covariates											
GDP per capita	0.003	0.0	0.01	0.005	-0.0	0.01	0.00	0.00	0.00	0.004	-0.0	0.01
female labor force participation	-0.4	-1.6	0.7	-0.2	-1.0	0.8	-0.1	-1.0	-0.9	-0.6	-2.1	0.6
government health expenditure	-0.1	-0.3	0.0	-0.1	-0.2	0.0	-0.1	-0.2	0.0	-0.2	-0.3	-0.0
total health expenditure	0.1	-0.0	0.2	0.01	-0.0	0.1	0.0	-0.0	0.1	0.1	-0.0	0.3

Model (a) provides logistic estimates
Model (b) provides unweighted estimates
Model (c) examines variation in exposure definition
Model (d) examines if there were lead (t+1, t+2, t+3) or lagged effects (t-3, t-2, t-1) of the policy change

Supplementary Table 4.6. Sensitivity analyses comparing the effects of abortion restriction in the Dominican Republic on the prevalence of modern contraceptive use among women of reproductive age across different model specifications

	a) Logistic estimates[a]			**(b) Unweighted estimates**[b]			**c) Restricted control countries**[c]		
	Est	*LCL*[a]	*UCL*	*Est*	*LCL*	*UCL*	*Est*	*LCL*	*UCL*
Abortion restriction	**-11.7**	**-15.3**	**-8.1**	**-10.5**	**-15.4**	**-5.7**	**-13.0**	**-18.5**	**-7.5**
Individual and household-level covariates									
Number of children alive	4.8	3.2	6.5	3.9	1.6	6.1	4.4	1.5	7.4
Mother's education (years)	0.3	0.04	0.6	0.4	0.1	0.7	0.2	0.03	0.4
Primary	3.8	1.6	6.1	6.1	1.5	10.8	5.0	1.1	10.2
Secondary+	7.1	4.5	9.7	7.8	2.4	13.3	6.4	1.4	12.4
Rural residence	-3.5	-5.1	-1.9	-3.4	-5.8	-0.9	-3.3	-6.7	-0.0
2nd wealth quintile	3.2	1.3	5.0	2.3	1.2	3.5	2.5	0.7	4.3
3rd wealth quintile	6.4	2.2	10.6	3.9	1.2	6.7	4.0	1.0	7.0
4th wealth quintile	7.4	2.4	12.4	5.0	1.0	8.9	5.7	0.6	10.8
5th quintile (highest)	7.2	2.3	12.2	3.0	-0.6	6.7	3.8	-1.2	8.9
Married/common law	22.7	8.1	37.4	15.5	4.0	27.0	17.0	5.6	28.5
Divorced/Separated	-8.8	-27.8	10.2	-5.7	-24.4	12.8	-7.3	-33	18.8
Maternal age <20	-14.8	-16.5	-13.1	-9.3	-13.1	-5.5	-9.2	-15.1	-3.5
Maternal age >=35	-5.3	-8.7	-2.1	-6.2	-10.5	-1.9	-6.9	-12.5	-1.3
Country-level covariates									
GDP per capita	-0.10	-0.12	-0.00	-0.0	-0.0	-0.0	-0.0	-0.0	-0.0
female labor force participation	-1.6	-2.2	-0.9	-0.9	-1.6	-0.5	-1.4	-1.6	-1.1
government health expenditure	0.2	0.1	0.3	0.1	0.1	-0.17	0.17	0.14	0.19
total health expenditure	-0.0	-0.04	0.0	0.0	-0.03	0.04	0.00	-0.04	0.06

Model (a) provides logistic estimates
Model (b) provides unweighted estimates
Model (c) restricted the set of control countries to those with similar trends in contraceptive use with Dominican Republic in the decade prior to the policy (Namibia, Cambodia, Egypt, Haiti & Zimbabwe)

CHAPTER 5. Effect of abortion decriminalization in Mozambique on neonatal mortality

5.1 Preface: Manuscript 3

This manuscript assesses the impact of a reform that decriminalizes abortion in specific circumstances in Mozambique. It hypothesizes that reforms that are not associated with adequate implementation may fail to achieve intended outcomes. It also examines heterogeneity across socioeconomic groups.

5.2 Manuscript 3

Title: Effect of abortion decriminalization in Mozambique on neonatal mortality

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Abstract

Background: An important factor regulating access to safe abortion in LMICs is abortion law. Abortion law reforms have been hypothesized to influence reproductive, maternal, and neonatal health services and health outcomes, as well as social inequalities in health. Creating a policy environment that is supportive of women's health and reproductive rights, including their access to abortion, requires understanding the impacts of policy alternatives. We evaluated the impact of a 2014 abortion reform in Mozambique on neonatal mortality using a difference-in-differences design.

Methods: We used birth history data collected via the Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) to assemble a panel of 476 939 live births across 17 countries including Mozambique from 2004 and 2018. We estimated the effect of abortion reform on the probability of neonatal mortality by comparing Mozambique to a series of control countries that did not change their abortion policies. We included country and year fixed effects and controlled for individual, household and country-level characteristics. We also conducted stratified analyses to examine heterogeneity in effect estimates by household wealth, educational status, and rural/urban residency.

Results: Abortion reform in Mozambique was associated with an additional 5.6 (95% CI=1.3, 9.9) neonatal deaths per 1,000 live births. There was evidence of a differential effect by household SES, educational attainment and rural urban status. The abortion reform was associated with poorer neonatal outcomes among socially disadvantaged women, including those in households with fewer assets, residing in rural areas and who didn't complete secondary school.

Conclusion: Our analyses suggest that abortion reform in Mozambique was associated with an increase in neonatal mortality, particularly for socially disadvantaged women. This may be attributable to the delay in providing clear guidelines and implementing safe abortion services following the reform. Abortion decriminalization alone may not be sufficient for improving abortion access and related health outcomes without adequate implementation and enforcement.

Keywords: Abortion decriminalization, neonatal mortality, health inequalities, impact evaluation, Mozambique

INTRODUCTION

An estimated 25·1 million unsafe abortions occur each year, with 97% of these in low- and middle-income countries (LMICs) (1-3). An important factor regulating access to safe abortion in LMICs is abortion law (4, 5). Over the past two decades, many LMICs have reformed their abortion laws, with most expanding the legal grounds on which a woman may terminate a pregnancy (2, 6, 7). These reforms have been hypothesized to influence reproductive, maternal, and neonatal health services and health outcomes, as well as social inequalities in health (8-11). Most women who live where abortion is widely accessible are not forced to seek unsafe abortions or carry unwanted pregnancies to term and are more likely to seek prenatal care and follow clinical advice for reducing the risk of adverse pregnancy and birth outcomes (8, 12-15).

Similarly, availability of legal and safe abortion services prevents heightened psychosocial stress and anxiety that often accompanies unwanted pregnancy (16, 17). Research suggests that women experiencing psychological stressors such as anxiety and depression during pregnancy have a higher risk of pregnancy complications such as pre-eclampsia and a higher risk of adverse pregnancy and birth outcomes (18, 19).

Few empirical studies have examined the impact of abortion decriminalization on neonatal outcomes (11, 20-24). A systematic review examining the impact of abortion reforms on health services and outcomes in LMICs found limited evidence of an impact of abortion decriminalization on neonatal outcomes (25).

Existing studies examining the impact of abortion decriminalization on neonatal outcomes have mostly utilized pre-post (before and after) designs, which lack a strategy for identifying the impact of abortion reforms. For example, Anton et al. utilized a pre-post design

when examining the effect of abortion decriminalization in Uruguay on birth outcomes. These designs, however, do not account for secular trends in outcomes and cannot inform inference regarding the causal impact of abortion law reforms (25). In addition, it may be difficult to generalize from prior evaluations of abortion reforms because legislative processes that are associated with changing abortion laws take place in highly distinct political, economic, and social contexts (26-28), which may modify the impact of these interventions.

Creating a policy environment that is supportive of women's health and reproductive rights, including their access to abortion laws, requires understanding the impacts of policy alternatives. Accordingly, there is a need for rigorous evaluation studies, including quasi-experimental studies, and the application of sensitivity analyses to strengthen the evidence-base concerning the potential impacts of abortion reforms. In this study, we evaluated the impact of a 2014 abortion reform in Mozambique using a difference-in-differences (DD) design. Using a group of control countries to account for secular trends, our goal was to estimate the causal effect of abortion decriminalization on neonatal mortality rates (NMR).

BACKGROUND

Since colonial times, abortion has been illegal in Mozambique. The nation's criminal code, inherited from the Portuguese colonial code, established a punishment of 2-8 years of imprisonment for the provision or receipt of abortion, unless the woman's health or life was at risk (29, 30). However, a 1981 Ministry of Health (MOH) decree supported a broad interpretation of this risk and abortion services were provided in designated public hospitals throughout the country (31). Hospital directors had the discretion to authorize abortion on an

as-needed basis with varied conditions such as a request letter, photo identification, a photo of the male partner, and cash payment ranging between US$ 24 to US$ 100(32). About 3,000 requests for abortion were approved annually and medical personnel and hospital administrators had come to regard abortion services as a significant source of revenue (30, 32). During this period, abortion in Mozambique was regarded as quasi-legal due to the discrepancy between the written law and everyday practice (30, 31, 33)

In July 2014, the Parliament introduced legislation for the Voluntary Interruption of Pregnancy (IVG), which decriminalized abortion in some specific circumstances as part of a new penal code (Law No. 35/2014) promulgated by President Armando Guebuza (34). This law allowed abortion to be performed by a physician or other qualified health professional in approved health facilities in the first 12 weeks of pregnancy, with the consent of the pregnant woman. In cases of rape or incest, abortion was legalized during the first 16 weeks, and in cases of fetal anomaly it was made legal in the first 24 weeks.

Women under the age of 16 or those unable to make their own decisions required the approval of a parent, guardian or another trusted adult. A request letter and an examination to determine the gestational age and check for probable contraindications to abortion are required before accessing abortion services. The law allowed for conscientious objection on moral or religious grounds and provided partial funding in public facilities (35, 36).

When the law came into effect in 2014, it cancelled the ministerial decree, but implementation was delayed and there were no clear guidelines for health facilities and providers (33). During the period between the passage of the law and the provision of implementation guidelines, providers refrained from performing abortion services due to

confusion and lack of clarity around the policy guidelines, as well as fear of legal repercussions. Abortion services that were previously available for specified fees were no longer provided (33, 37).

Provision of abortion services started to improve in early 2018, following the publication and dissemination of Diploma Ministerial no. 60/2017 along with the clinical guidelines that defined standards for safe abortion for health facilities and providers, in parallel with training of health providers and community outreach work that publicized the registered health facilities offering abortion services (37).

However, institutional opposition, lack of government resources, conscientious objection by health workers, and knowledge gaps on the part of decision-makers have hindered the wider implementation of safe abortion care in Mozambique (38, 39).

METHODS

Data Sources

We used Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) to measure neonatal mortality and other individual and household-level characteristics. The DHS and MICS are nationally representative household surveys that collect standardized health and demographic data, including information on birth histories, sexual and reproductive health, maternal and neonatal health, as well as other key socio-demographic characteristics such as household assets. The DHS employs a two-stage cluster sampling methodology, with the first stage selecting clusters and the second stage selecting households (40, 41). All women aged 15 to 49 who slept in the household the night before the survey were invited to complete

a structured questionnaire. To ensure comparability, the surveys employ trained interviewers and standardized tools and methodologies. The MICS makes use of similar sampling procedures, methods, and measures (42). Numerous studies have analyzed trends in reproductive, maternal, and neonatal health outcomes using harmonized DHS and MICS data (43-45).

Sample

For the analysis, we harmonized DHS and MICS between 2004 and 2020. We included 17 potential control countries that did not implement changes to their abortion laws during the study period. These control countries were identified based on the following criteria: (i) a similar sub-Saharan classification to that of Mozambique as indicated in the World Bank Data Catalogue, (ii) at least two available DHS/MICS surveys, including at least one conducted prior to the abortion reform, and (iii) no evidence of significant reproductive health reforms during the study period that might have affected our primary outcome of interest.

Because DHS/MICS surveys were not available for Mozambique beyond 2018, our study period extended from 2004 to 2018. Our analytical sample comprised all live births to DHS/MICS respondents in this period, including 476 939 children born between 2004 and 2018 from 51 DHS/MICS surveys across 17 countries, including Mozambique. Trends in neonatal mortality in the pre-reform period from 2004 and 2014 were approximately parallel for Mozambique compared to the potential control countries, a necessary but not sufficient criterion for the DD approach to provide a valid estimate of the average treatment effect on the treated (ATT)(46, 47). Thus, we did not exclude any control countries from our analyses. Supplementary Table 1 shows the survey years, birth years, and sample sizes for each country.

Measures

Our outcome variable was neonatal mortality, which was measured by a binary indicator for whether a child who was born alive died within the first month of life. Women interviewed in the DHS/MICS were asked to provide birth histories up to 5 years before the interview and those reporting a deceased child were asked to give the age at death. These data were used to construct a panel of live births by year of birth. We restricted to live births that occurred at least 29 days prior to the interview date to ascertain whether each child survived the neonatal period following birth. We included data on neonatal deaths occurring up to 5 years prior to the date of each survey to ensure uniformity across surveys and to minimize self-reporting errors (48).

The exposure of interest was the adoption of a policy decriminalizing abortion in Mozambique in July 2014. Sampled births from Mozambique prior to July 2014 were considered unexposed and those occurring after 2014 were defined as exposed.

Based on relevant literature, we included covariates associated with neonatal mortality in LMICs at the household, maternal and child levels (49, 50). These included maternal age at birth, multiple birth, number of living children, mother's education (none, primary, secondary, or higher), place of residence (e.g., urban or rural), and birth interval (≥ 24 months, <24 months). Additionally, we incorporated the polychoric dual-component household wealth index (P2C) as a substitute for the DHS wealth index to account for the bias toward urban wealth patterns (urban bias) (51). The P2C wealth index is calculated similarly to the standard DHS wealth index using a set of variables relating to household conditions (e.g., water source, sanitation facilities, and electricity), as well as ownership of consumer goods (e.g., a bicycle, a

telephone, a refrigerator, agricultural land, and domestic animals). Unlike the DHS wealth index, however, categorical variables are transformed into ordered categorical (ordinal) variables to increase discriminatory power and avoid using multiple binary (dummy) variables to represent a single asset. This technique is more sensitive to key characteristics of rural wealth and is intended to account for the urban bias of the regular DHS approach.

In addition, to minimise confounding by country level differences, we controlled for country-level characteristics that may be associated with changes to abortion policies and with trends in neonatal mortality from the World Bank's World Development Indicators and Global Development Finance databases (52). These variables included gross domestic product (GDP) per capita (constant 2017 international dollars) based on purchasing power parity (PPP), the female labour force participation rate (percentage of female population ages 15–64 years), government health expenditures per capita based on PPP (constant 2017 international dollars) and total health expenditure.

Statistical Analysis

Effect of abortion decriminalization

We estimated the effect of abortion decriminalization in Mozambique on neonatal mortality using a linear probability model of the general form:

$$P(Y_{ict}) = \beta_0 + \beta_1 Policy_{ct} + g_t + d_c + \sum b_n X_{ict} + \sum b_k Z_{ct} + \varepsilon_{ict},$$

where Y_{ict} represents the outcome for each observation i in country c in birth year t. $Policy_{ct}$ is a time-varying indicator for whether the observation was measured before or after the implementation of the abortion reform in Mozambique. β_1 represents the effect on the risk difference scale of the reform on the probability of neonatal death. In the first model, we included fixed effects for country, d_c , and year, g_t , to account for, respectively, unobserved time-invariant confounders that vary across countries and shared temporal trends in our outcome (Model 1). In the second model, we additionally adjusted for individual and household characteristics, represented by the vector X_{ict} (Model 2). In the third model, we further controlled for time-varying country-level potential confounders, specifically per capita GDP, female labor force participation, per capita total and government health expenditures, represented by the vector Z_{ct} (Model 3). The effects of the abortion law reform were therefore identified by changes in the probability of neonatal mortality in Mozambique (treated country) before and after its abortion law reform compared to corresponding changes in control countries that did not modify their policies during the study period. In addition, we conducted stratified analyses to examine heterogeneity in effect estimates by household wealth, educational attainment and rural/urban residency. We used tests of homogeneity to examine whether effects varied across strata.

All three models incorporated respondent-level sampling weights to account for individual survey sampling designs. Per DHS guidelines, we applied the de-normalization of standard weights approach described in the DHS Sampling and Household Listing Manual (53) to calculate an appropriate sampling weight for each observation in the analyses, using information on the number of women aged 15–49 years in each survey-year, available from the

Population Division of the United Nations (54). We also estimated robust standard errors to account for clustering at the country level. We performed all analyses using Stata version 15 (55). The DHS data are publicly available, but users must first register with the DHS program. The statistical code for producing the analytic dataset and replicating our results (given DHS data that must be downloaded from the DHS program), are also available.

Sensitivity analyses

We conducted sensitivity analyses to assess the robustness of our main findings. First, we reported unweighted estimates to examine whether our results were sensitive to the inclusion of sampling weights. Second, we estimated the effect of the policy reform using a logistic regression model, with post-estimation to derive marginal effects on the risk difference scale. Third, we examined whether the association between the abortion reform and neonatal mortality depended on how we defined the treatment. We modified the exposure definition to include the estimated time of conception since births occurring immediately after the reform may have been unaffected while in utero. Fourth, we added to our primary exposure indexed by birth year (t), parameters representing the potential lead effect of the reform in preceding years (t-1, t-2, t-3) and lagged effect of the reform in subsequent years (t+1, t+2, t+3). These analyses examined whether there were delayed or persistent effects of the policy reform, as well as whether associations were observed prior to the implementation of the abortion reform, which would be inconsistent with evidence of a causal impact of the reform.

RESULTS

Table 5.1 shows the distribution of individual, household, and country level characteristics for treated and control countries prior to the implementation of the abortion law reform in Mozambique in 2014. In the pre-intervention period, the rate of neonatal mortality was 28.7 per 1,000 live births.

Table 5.1. Sample characteristics for treated and control countries in the pre-intervention period (2004-2013)

	N=347,802 live births	
	Treated	**Control**
Mean (SD) neonatal mortality	0.021 (0.14)	0.029 (0.17)
Individual-level and household-level covariates		
Mean (SD) number of children alive in the household	3.4 (1.9)	3.5 (2.1)
Mean (SD) mother's education (years)	3.8 (4.0)	4.1 (4.3)
Mean (SD) mother's age at birth	26.2 (7.0)	27.0 (6.8)
Educational attainment		
No schooling	5,304 (22.0%)	124,710 (38.9%)
Primary	13,422 (56.0%)	124,732 (39.0%)
Secondary+	5,265 (22.0%)	70,794 (22.1%)
Residence		
Rural	17,581 (63.8%)	235,895 (73.7%)
Urban	9,970 (36.2%)	84,356 (26.3%)
Wealth Index		
Poorest	10,184 (37.0%)	87,301 (27.3%)
Second	5,963 (21.6%)	64,066 (20.0%)
Middle	4 ,446 (16.1%)	62,949 (19.7%)

Fourth	4,407 (16.0%)	60,081 (18.8%)
Wealthiest	2,551 (9.3%)	45,765 (14.3%)
Marital Status		
Single	870 (5.2%)	11,398 (3.8%)
Married/common law	12,761 (76.5%)	266,080 (89.7%)
Divorced/Separated/Widowed	3,059 (18.3%)	19,035 (6.4%)
Type of Birth		
Singleton	26,549 (96.4%)	308,754 (96.4%)
Twin/Triplet	1002 (3.6%)	11,497 (3.6%)
Birth Interval		
>=24months	26,041 (94.5%)	272,711 (85.2%)
< 24months	1510 (5.5%)	47,540 (14.8%)
Country-level covariates		
Mean (SD) GDP per capita, PPP (constant 2017 international $)	1061 (110)	2734 (1357)
Mean (SD) government health expenditure per capita, PPP (current international $)	14.8 (4.7)	26.8 (13.9)
Mean (SD) labour force participation rate, female (% of female population ages 15+ years)	82.2 (2.4)	68.0 (11.1)
Mean (SD) total health expenditure per capita, PPP (constant 2017 international $)	55.4 (20.5)	123.1 (46.5)

Treated country is Mozambique. Control countries are Benin, Burundi, Cameroon, Gambia, Guinea, Ghana, Liberia, Madagascar, Malawi, Mali, Nigeria, Rwanda, Tanzania, Uganda, Zambia, Zimbabwe.

Trends in neonatal mortality in treated and control countries were stable in the pre-treatment period, with an increase in neonatal mortality in Mozambique after the abortion law reform (Figure 5.1). Supplementary Figure 5.1 shows similar pre-intervention trends in neonatal mortality for treated and control countries.

Figure 5.1. Trends in neonatal mortality for Mozambique and control countries over the study period. 2004-2018

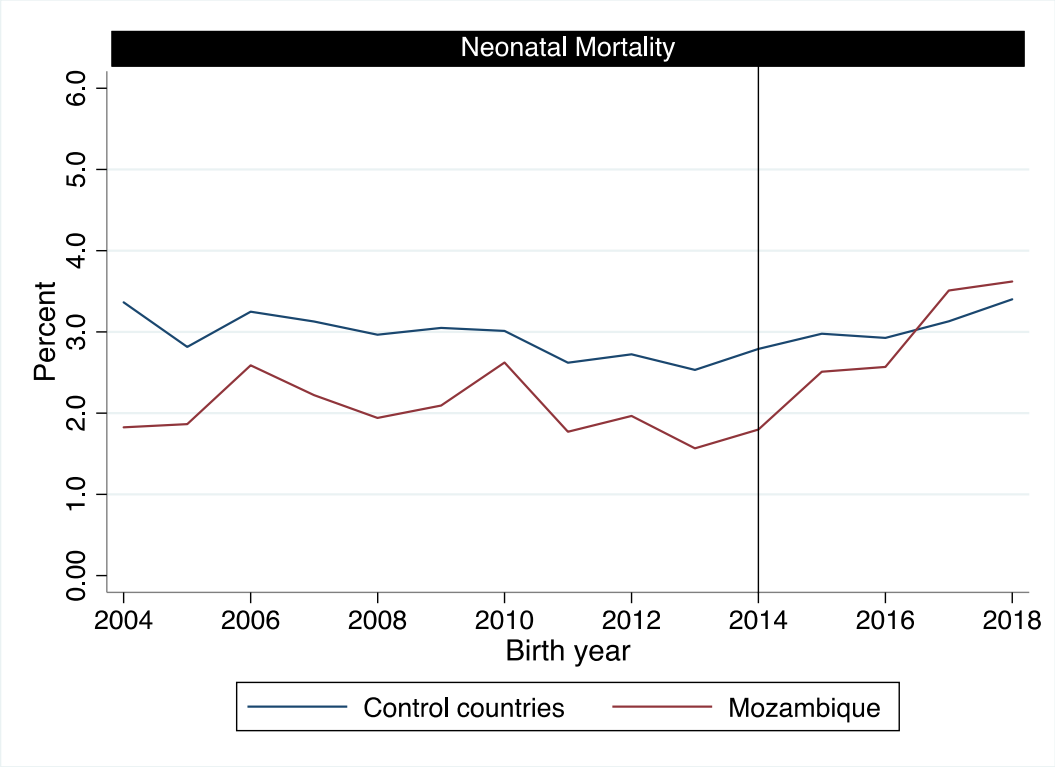


Table 5.2 provides estimates of the effect of abortion decriminalization on the probability of neonatal death. In the fully adjusted model (Model 3), abortion decriminalization was associated with an additional 5.6 (95% Confidence Interval (CI)=1.3, 9.9) neonatal deaths per 1,000 live births. There was evidence of socioeconomic gradients in neonatal mortality. There were, on average, 13.4 (95% CI=10.3, 16.5) fewer neonatal deaths per 1,000 live births among women with secondary education and above compared to those with no education. Rural residence was associated with 3.4 (95% CI=1.4, 5.3) additional neonatal deaths per 1000 live births compared to urban residence. A short birth interval (<24 months) was associated with an additional 26.9 (95% CI=22.6, 30.0) neonatal deaths per 1,000 live births.

Table 5.2. Effect of abortion decriminalization in Mozambique on the number of neonatal deaths per 1,000 live births

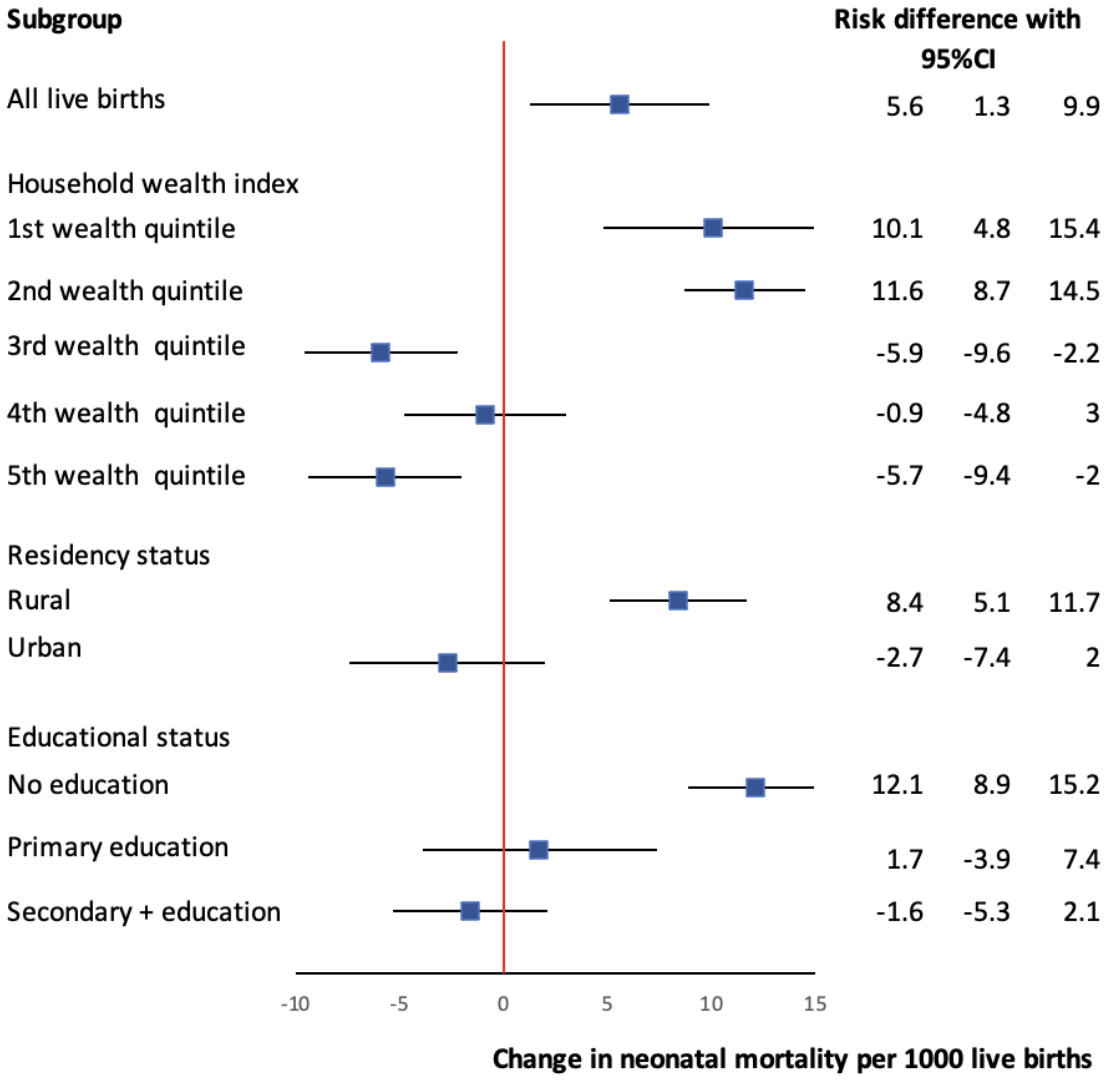
	Model 1			Model 2			Model 3		
	Est	*LCL*[a]	*UCL*	*Est*	*LCL*	*UCL*	*Est*	*LCL*	*UCL*
Abortion decriminalization	**9.6**	**7.0**	**12.3**	**5.5**	**2.9**	**8.3**	**5.6**	**1.3**	**9.9**
Individual and household-level covariates[b]									
Primary education				-1.9	-3.6	-0.3	-1.9	-3.5	-0.2
Secondary+				-13.5	-16.5	-10.4	-13.4	-16.5	-10.3
Rural residence				3.9	2.0	5.9	3.4	1.4	5.3
2nd wealth quintile				1.3	-0.4	3.1	1.3	-0.4	3.1
3rd wealth quintile				2.5	0.7	4.2	2.3	-0.6	5.1
4th wealth quintile				1.5	-1.1	4.2	1.4	-1.1	4.0
5th quintile (highest)				1.6	-1.3	4.6	1.6	-0.2	3.3
Maternal age <20				-5.9	-7.6	-4.2	-5.1	-6.7	-3.6
Maternal age >=35				38.7	33.2	44.1	39.1	33.6	44.7
Number of children alive				-11.9	-13.7	-10.2	-12.0	-13.6	-10.2
Twin/triplet				124	113	135	126	115	137
birth interval <24 months				27.0	22.7	31.2	26.9	22.6	30.0
Country-level covariates									
GDP per capita							0.005	-0.00	0.01
female labor force participation							0.5	-0.7	1.7
government health expenditure							-0.1	-0.4	0.0
total health expenditure							0.1	-0.0	0.1

[a] LCL and UCL indicate lower and upper limits of the 95% confidence interval, respectively
[b]Reference categories for categorical variables are no schooling, urban (vs. rural) residence, the 1st (lowest) wealth quintile, maternal age (20-34 years), and singleton (vs. twin/triplet), >=24 month (vs. <24 month) birth interval.

There was evidence of a differential effect of abortion decriminalization on neonatal mortality by household SES, educational attainment and rural vs. urban residence (Figure 4.7) (Supplementary table 5.2). We estimated 10.1 (95% CI=4.8, 15.4) additional neonatal deaths per 1000 live births among respondents in the lowest quintile of household wealth, compared with about 5.7 (95% CI=1.9, 9.4,) fewer neonatal deaths per 1000 live births among respondents in highest quintile of household wealth; estimates were statistically different across strata (p<0.001). Among women in rural areas, there were 8.4 (95% CI=5.1, 11.7) additional neonatal deaths per 1000 live births compared to 2.7 (95% CI= -2.0, 7.4) fewer deaths per 1000 live births in urban areas, which represented a statistically significant difference across strata (p=0.02). There were 12.1 (95% CI= 8.9, 15.2) additional neonatal deaths per 1000 live births among women with no education compared with 1.6 (95% CI= -2.1,

5.3) fewer deaths among women with at least a secondary education. The estimates were statistically different across strata (p=0.04).

Figure 5.2. Forest plot showing heterogeneity in the effect of abortion decriminalization in Mozambique on probabilities of neonatal mortality across subgroups



Results from sensitivity analyses are shown in Table 5.3. The effects of abortion decriminalization on neonatal mortality were robust across most specifications. When logistic models were used, abortion decriminalization was associated with a comparable increase in neonatal mortality of an additional 6.8 (95% CI=2.2, 11.4) deaths per 1,000 live births (Model

A). The effects of abortion decriminalization on neonatal mortality were similar in unweighted and weighted models, with unweighted estimates showing that abortion decriminalization was associated with an additional 5.8 (95% CI=2.2, 9.3) neonatal deaths per 1,000 live births (Model B). When we varied the timing of exposure to include gestational period, abortion decriminalization was associated with 6.4 (95% CI=3.8, 9.0) additional neonatal deaths per 1,000 live births (Model C). For the examination of lead and lag effects, abortion decriminalization was associated with a persistent effect on neonatal mortality in the years following the reform, with no evidence of lead effects (Model D).

Table 5.3. Sensitivity analyses comparing the effects of abortion decriminalization in Mozambique on the number of neonatal deaths per 1,000 live births across different model specifications

	a) Logistic estimates			**(b) Unweighted estimates**			**c) Variation of exposure definition**			**d) Analysis of lags and leads**		
	Est	*LCL*[a]	*UCL*	*Est*	*LCL*	*UCL*	*Est*	*LCL*	*UCL*	*Est*	*LCL*	*UCL*
Abortion decriminalization												
Lagged three years, t-3										**5.5**	**2.9**	**8.2**
Lagged two years, t-2										**9.6**	**6.4**	**12.9**
Lagged one year, t-1										**11.8**	**7.9**	**15.6**
Concurrent, t	**6.8**	**2.2**	**11.4**	**5.8**	**2.2**	**9.3**	**6.4**	**3.8**	**9.0**	**5.6**	**1.8**	**9.4**
Lead, t+1										**0.2**	**-4.1**	**4.6**
Lead two years, t+2										**1.4**	**-3.1**	**6.0**
Lead three years, t+3										**2.3**	**-2.0**	**6.7**
Individual and household-level covariates[b]												
Primary education	-1.3	-2.9	0.4	-2.2	-3.8	-0.6	-2.2	-3.8	-0.6	-2.0	-3.5	-0.5
Secondary+	-11.2	-12.7	-9.8	-13.1	-15.8	-10.4	-13.3	-16.0	-10.8	-13.4	-16.2	-10.6
Rural residence	3.6	1.9	5.4	3.4	1.6	5.1	3.3	1.5	5.0	4.0	2.0	5.9
2nd wealth quintile	1.1	-0.5	2.9	1.8	0.5	3.2	2.1	0.8	3.3	1.3	-0.7	3.3
3rd wealth quintile	2.3	0.6	4.0	2.6	1.1	4.2	2.9	1.6	4.3	2.4	0.6	4.2
4th wealth quintile	1.2	-1.3	3.8	1.3	-1.2	3.8	1.8	-0.7	4.3	1.4	-1.3	4.1
5th quintile (highest)	1.4	-1.1	4.0	0.7	-1.4	2.8	1.2	-0.8	3.3	1.4	-1.5	4.3
Maternal age at birth <20	-3.8	-4.7	-2.8	-4.5	-6.0	-2.9	-4.4	-6.0	-2.8	-5.1	-7.0	-3.9
Maternal age >=35	38.8	33.3	44.3	38.5	33.7	43.2	38.7	34.0	43.2	38.7	33.4	44.0
Number of children alive	-13.5	-14.7	-12.3	-11.7	-13.3	-10.2	-11.8	-13.3	-10.2	-12.0	-13.5	-10.1
Twin/triplet	129	118	140	122	114	130	122	114	130	126	116	139
birth interval <24 months	30.2	27.2	33.3	27.1	23.0	31.2	27.4	23.3	31.4	26.9	22.9	30.4
Country-level covariates												

GDP per capita	0.00	-0.00	0.00	0.01	0.0	0.01	0.00	-0.00	0.01	0.005	-0.00	0.01
female labor force participation	0.6	-0.7	1.7	0.7	-0.4	1.8	0.8	-0.4	2.0	0.5	-0.7	1.7
government health expenditure	-0.0	-0.0	0.0	-0.2	-0.3	0.0	-0.1	-0.4	0.0	-0.1	-0.4	0.0
total health expenditure	0.1	-0.0	0.1	0.04	-0.0	0.1	0.1	-0.02	0.1	0.1	-0.0	0.1

Model (a) provides logistic estimates
Model (b) provides unweighted estimates
Model (c) examines variation in timing of exposure
Model (d) examines if there were lead (t+1, t+2, t+3) or lagged effects (t-3, t-2, t-1) of the policy change

DISCUSSION

We used samples of 467 303 live births in 17 LMICs recorded in DHS/MICS surveys between 2004 and 2018 to evaluate the impact of abortion decriminalization in Mozambique on neonatal mortality. Our analyses suggest that abortion decriminalization was associated with an additional 5.6 (95% CI=1.3, 9.9) neonatal deaths per 1,000 live births. These findings were robust to alternative model specifications.

Policies decriminalizing abortion are expected to increase access to safe abortion, decrease unwanted pregnancies and ultimately improve neonatal outcomes (16, 22, 56). Women with planned pregnancies are more likely to attend prenatal care, take folic acid or multivitamins and avoid behaviour that could result in adverse fetal and neonatal outcomes (14, 18, 57). However, evidence on the relation between abortion decriminalization and neonatal outcomes is mixed. Studies reported a reduction in neonatal mortality and improvement in birth outcomes such as birthweight following abortion liberalization in Romania, Oregon, New York and Hawaii (11, 23, 24, 58, 59). In contrast, other studies did not detect any effect of abortion decriminalization on neonatal mortality or other neonatal

outcomes following reforms in Nepal and Uruguay (21, 22, 60). Our results indicate that abortion decriminalization in Mozambique was associated with an increase in neonatal mortality.

The quasi-legal status of abortion in Mozambique prior to the abortion reform made abortion partially accessible through designated public hospitals under the discretion of hospital directors (30, 31). Following abortion decriminalization, the delay in providing clear guidelines and limited access to safe abortion services may have reduced access to abortion and led to an increase in neonatal death during this period. Clinical standards for safe abortion and clearer standards and guidelines for health facilities and providers were disseminated in 2018, after which services were slowly rolled out across the country (33, 37). For example, in Tete province, the third most populous province, services were initially provided in only 6 districts in 2019, out of a total of 15, and are currently provided in approximately half of districts. Services are also generally available only in district hospitals and not in rural health facilities.

However, our analyses do not capture the period after clinical guidelines and standards for professionals and health facilities were accessible and we do not have information on current availability of abortion services in Mozambique. Impacts of the abortion reform on sexual and reproductive health outcomes in the post-implementation period warrants further investigation.

Although reforms to liberalize abortion are intended to improve access, ineffective implementation and enforcement may result in unintended consequences (61, 62). Abortion legislation that fails to reflect and anticipate how liberalized grounds for abortion will be

established may deter medical practitioners from promptly delivering care to eligible women (63-65). Several recent decisions of international human rights courts address government duties to implement abortion laws, including prompt access to safe and quality abortion care (63).

In a study describing abortion procedures and exploring factors influencing the abortion decision-making process among young women who received an abortion in Mozambique following decriminalization, none of the participants had followed a legal procedure to obtain abortion and half had received abortion outside of health facilities (39). Women cited lack of knowledge about the new abortion law and lack of legal abortion services. This research highlights the importance of clarifying and informing women and providers of their legal entitlements and ensuring that abortion services are available in all circumstances described in the law.

Furthermore, our analyses showed heterogeneity in the effect of abortion decriminalization on neonatal mortality in Mozambique. Socially disadvantaged women, including those in households with fewer assets, residing in rural areas and who didn't complete secondary school, experienced poorer neonatal outcomes following abortion decriminalization. Some studies have examined how abortion legalization affected socioeconomic groups differentially (15, 22, 56). Miller et al. (22) reported differential effects based on race when assessing the impact of the legalization of abortion on neonatal mortality in the United States using monthly time series data. Although abortion legalization did not result in an overall reduction in neonatal mortality for both whites and non-whites, states with more accessibility and public funding for abortion showed a decline of 9% in neonatal mortality

among non-whites compared to 2.4% in whites. Following abortion decriminalization in Mozambique, Frederico et al. (39) reported that only people with a certain level of education, income and a sufficiently large social network could access legal and safe abortion procedures. It is possible that abortion reform in Mozambique exacerbated extant disparities in access to abortion services.

There were limitations to our study. First, data was only available up to 2018, and therefore did not cover the period when abortion services were introduced following the publication and dissemination of Diploma Ministerial no. 60/2017. Second, there is the potential for unmeasured confounding. We accounted for potential confounding by controlling for individual, household, and country-level covariates and including country and year fixed effects to account for unobserved time-invariant confounders that vary across countries and any secular trends in outcomes that are common across countries, respectively. Nonetheless, there may have been other changes that coincided with the timing of the abortion reform in Mozambique and influenced our outcomes of interest. Third, measurement of neonatal mortality is subject to underreporting or misclassification since it depends on recall by women interviewed. Accurate recall may become more difficult as the memory of the event fades with time (67) and women may not possess accurate knowledge to distinguish between neonatal death and stillbirths (68). Such underreporting or misclassification may bias our estimates. Fourth, we lacked measures on abortion uptake and as such cannot ascertain the degree to which abortion decriminalization in Mozambique influenced utilization of abortion. Finally, generalisation of our results should be done cautiously, as results may vary for countries with different political, cultural, religious, and sociodemographic contexts vis-à-vis Mozambique.

CONCLUSION

While acknowledging the limitations in this study, our analyses suggest that abortion reform in Mozambique was associated with an increase in neonatal mortality, particularly for socially disadvantaged women. This may be attributable to the delay in provision of clear guidelines and implementation of safe abortion services. Abortion decriminalization alone may not be sufficient improving abortion access and attendant health outcomes without adequate implementation and enforcement. A longer follow-up is needed to assess if the immediate adverse effect of the abortion reform was reversed after 2018. Further research is also needed to understand how constraints to implementation and enforcement of abortion reforms affect access to abortion services.

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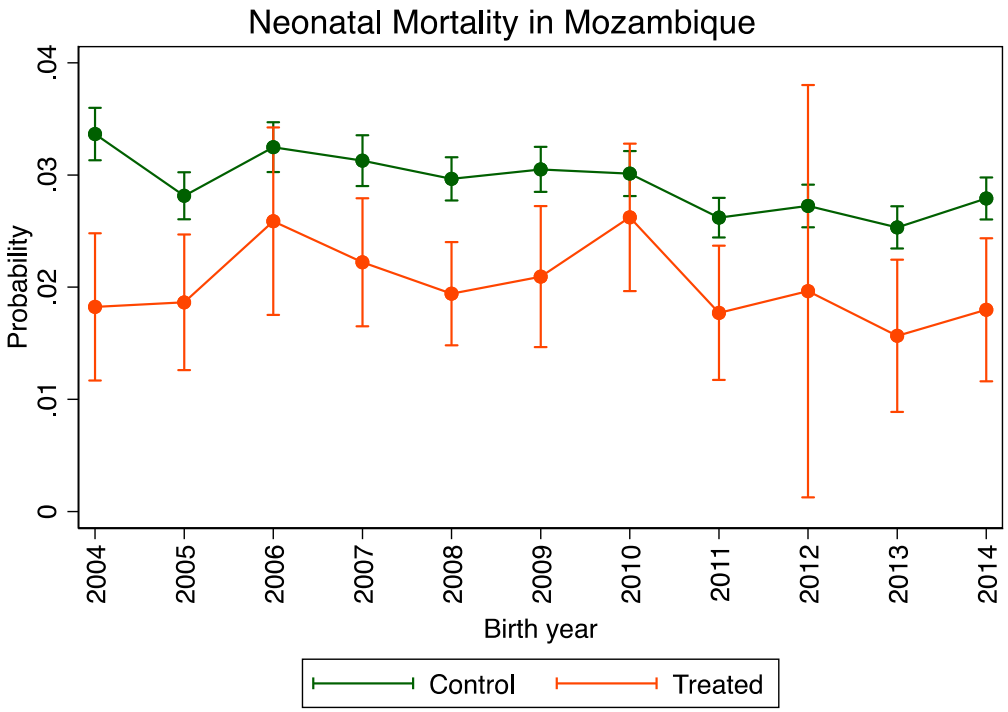
5.3 Supplemental material: Manuscript 3

Supplementary Table 5.1: DHS/MICS survey years, birth years and sample sizes for analyses of neonatal mortality.

Country	DHS/MICS survey years before policy	DHS/MICS survey years after policy	Birth years used	Sample size
Treated country				
Mozambique	2004, 2008, 2011	2015, 2018	2004-2018	32,396
Control Countries				
Benin	2006, 2011/12	2017/18	2004-2018	36,457
Burundi	2010/11	2016/17	2005-2017	21,145
Cameroon	2004, 2011	2018/19	2004,2006-2018	22,469
Gambia	2010, 2013	2019/20	2005-2018	14,566
Guinea	2005,2012	2018	2004/5,2007-2018	17,250
Ghana	2004, 2014	2017	2004-2017	10,500
Liberia	2006/7, 2013	2019/20	2004-2018	15,936
Madagascar	2004, 2008/9	2016, 2018	2004-2018	32,514
Malawi	2004/5, 2010	2015/16	2004-2016	39,901
Mali	2006, 2012/13	2018	2004-2018	28,207
Nigeria	2008, 2013	2018	2004-2018	93,846
Rwanda	2005, 2010/11	2014/15, 2019/20	2004-2018	25,944
Tanzania	2004/5, 2009/10	2015/16	2004-2016	20,185
Uganda	2006, 2011	2016	2004-2016	28,129
Zambia	2007, 2013/14	2018/19	2004-2018	28,386
Zimbabwe	2005/6, 2010/11	2015, 2019	2004-2018	19,329

Supplementary Figure 5.1: Examination of pre-intervention trends in neonatal mortality



Supplementary Table 5.2: Table showing heterogeneity in the effect of abortion decriminalization in Mozambique on probabilities of neonatal mortality across subgroups

	Est	*LCL*	*UCL*
Abortion decriminalization			
Household wealth index			
1st wealth quintile	10.1	4.8	15.4
2nd wealth quintile	11.6	8.7	14.5
3rd wealth quintile	-5.9	-9.6	-2.2
4th wealth quintile	-0.9	-4.8	3.0
5th quintile (highest)	-5.7	-9.4	-2.0
Residency status			
Rural	8.4	5.1	11.7
Urban	-2.7	-7.4	2.0
Educational status			
No education	12.1	8.9	15.2
Primary	1.7	-3.9	7.4
Secondary	-1.6	-5.3	2.1

CHAPTER 6: Correcting impact evaluation studies for potential misclassification using Misclassification Simulation Extrapolation (MC-SIMEX)

6.1 Preface: Manuscript 4

This manuscript aims to illustrate how to quantify and correct for potential misclassification bias when estimating the impacts of population level interventions using national household survey, with the objective of informing evidence-based decision making.

It examines the effects of misclassification error on the estimated impacts of abortion reforms on neonatal mortality in the Dominican Republic and Mozambique using the MC-SIMEX technique.

The manuscript is framed as a practice of epidemiology paper.

6.2 Manuscript 4

Title: Correcting impact evaluation studies for potential misclassification using Misclassification Simulation Extrapolation (MC-SIMEX)

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Abstract

Background: Large household survey programs like the Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) are the primary source of data for estimating the impact of population level interventions on reproductive and pregnancy outcomes, as well as child health, in LMICs. Despite their great value, these household surveys are subject to measurement error, including underreporting and misclassification of outcomes such as neonatal mortality and stillbirth. This paper examines the effects of misclassification error on the estimated impacts of abortion reforms on neonatal mortality in the Dominican Republic and Mozambique using the MC-SIMEX technique.

Methods: We corrected for possible misclassification of neonatal mortality using the MC-SIMEX approach, assuming non-differential misclassification of neonatal deaths across countries. We explored different levels of misclassification by varying the values of sensitivity and specificity using hypothesized misclassification probabilities. The MC-SIMEX procedure was performed with 100 simulations with the variance calculated using the asymptotic estimation method. We investigated both quadratic and log-linear forms for the extrapolant function.

Results: The MC-SIMEX estimates (both quadratic and log-linear), corrected for the potential misclassification of neonatal deaths, yielded higher risk differences and standard errors compared to the naïve estimates for all misclassification scenarios examined. In general, quadratic and log-linear extrapolant functions yielded similar corrected estimates.

Conclusion: Our analyses suggest that the naive estimates of the impact of abortion law reforms in the Dominican Republic and Mozambique were likely underestimated due to the misclassification of neonatal deaths in self-reported survey data. The MC-SIMEX method can provide corrected estimates and elucidate the consequences of misclassification when misclassification probabilities are known or hypothesized, including the potential magnitude and direction of bias in the naïve estimator.

Keywords: MC-SIMEX, impact evaluation, misclassification, neonatal mortality, survey data

INTRODUCTION

Large household survey programs like the Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) are the primary source of data for estimating the impact of population level interventions on reproductive and pregnancy outcomes, as well as child health, in LMICs (1-3). These household surveys rely on retrospective reporting from the respondent and involve asking a nationally representative sample of women about all previous births and their outcomes, with more detailed information collected for births occurring in the five years preceding the interview (4, 5). The DHS and MICS are often the most reliable sources of health and demographic data where health systems capacity is limited, primarily because they provide information on births occurring outside of health facilities (6, 7). However, despite their great value, routine household surveys are subject to measurement error, including underreporting and misclassification of outcomes such as neonatal mortality and stillbirth (8, 9).

According to the World Health Organization, stillbirths are deaths after 28 weeks of pregnancy, but before or during delivery, and neonatal deaths are defined as deaths among live births during the first 28 completed days of life. Obtaining accurate data for neonatal deaths/stillbirths using household surveys such as DHS/MICS remains challenging due to misclassification error, occurring when outcomes are assigned to a different category than the one to which they should be assigned. For example, although most neonatal deaths may be correctly identified in survey data via self-report, some are misclassified as stillbirths. Similarly, most stillbirths may be correctly classified as stillbirths, however some are misclassified as neonatal deaths (9, 10).

The accuracy of self-reported birth outcomes is affected by several factors. Social desirability bias may curb women's willingness to report neonatal deaths and women may choose to conceal them due to social stigma or cultural practices (11, 12). Even when women are willing to disclose these events, accurate recall may diminish over time. Also, women may not possess the knowledge to distinguish between stillbirths and neonatal deaths and may report neonatal deaths as stillbirths or vice versa (11, 13). Children born with limited signs of life, such as crying, breathing, and movement, who died in the neonatal period, may be mistakenly or intentionally reported as stillbirths (14-16). Moreover, women may recount to the interviewer what they consider to be an accurate description of the event but may have received incorrect information or misunderstood information from a medical provider (12, 17). In some contexts, women bear more blame if their newborns die than if they have had stillbirths and thus may intentionally misreport neonatal deaths as stillbirths (18).

To assess and quantify the level of misclassification of neonatal deaths and stillbirths, validation studies often compare self-reported outcomes to those from verbal autopsy interviews, treating the latter as the reference standard (19-21). The rationale for using a verbal autopsy as a reference standard is that additional and thorough questioning of the event may elicit detailed information specifically on whether children ever cried, moved or breathed, thus improving classification validity. Although treating verbal autopsy as the gold standard requires strong assumptions, it is considered one of the best available methods for diagnosing cause of death in settings where many deaths occur outside of medical care and lack proper medical certification (11, 19, 22).

Some studies have attempted to quantify misclassification of neonatal deaths and stillbirths. In a cross-sectional population-based survey of women of reproductive age in five countries, 3.4% of neonatal deaths were reclassified as stillbirths and 24.6% of stillbirths were reclassified as neonatal deaths, after additional questions assessing vital status were included (23). According to a review of Afghanistan's 2010 mortality data (15), 2.7% of neonatal deaths from a household survey were reclassified as stillbirths following verbal autopsy and 6.4% of stillbirths reclassified as early neonatal deaths. In Guinea-Bissau, a study reported that 8.4% of neonatal deaths from a survey were reclassified as stillbirths and 16.9% of stillbirths were reclassified as neonatal deaths after verbal autopsy (16). In Malawi, 20.5% of neonatal deaths identified in a survey were reclassified as stillbirths after verbal autopsy, but this study did not include events classified as stillbirths (11).

The DHS/MICS program remains an important source for estimating global neonatal mortality trends and is increasingly used to evaluate the impacts of population-level interventions on neonatal health (3, 24-26). However, few studies have attempted to quantify the role of misclassification, including the magnitude of the information bias, on these estimates. Ignoring this potential misclassification may bias inference, by overestimating the precision of estimated effects or biasing the magnitude of the association.

Kuchenhoff et al. (27) created a modification of the Simulation Extrapolation (SIMEX) procedure, called the MC-SIMEX (Misclassification SIMEX) technique, to correct for potential bias related to measurement error with binary/categorical variables, which has been applied in recent research (28). While MC-SIMEX does not require validation data, it does require that the misclassification probabilities are known or can be well-estimated (29). These probabilities may

be estimated from available validation data, derived from relevant literature, or simply hypothesized (and varied), as in a sensitivity analysis. In the few existing applications in the epidemiological literature, the error distribution has been determined or estimated using a combination of expert judgment and data from the literature (28, 30-34).

Using misclassification probabilities drawn from the literature, this paper examines the effects of misclassification error on the estimated impacts of abortion reforms on neonatal mortality in the Dominican Republic and Mozambique using the MC-SIMEX technique.

METHODS

Motivating data set

We illustrate the use of MC-SIMEX to account for misclassification of neonatal mortality and stillbirth using harmonized data from the DHS and MICS. Both surveys collect comparable information on socio-demographic characteristics, birth histories, fertility, mortality, contraceptive use, maternal and newborn health, and other key indicators from a nationally representative sample of households in selected LMICs using a two-stage cluster sampling design. Detailed information is collected from women of reproductive age (15–49 years) about all children born alive in the 5 years before the interview and those reporting a deceased child were asked to give the age at death (4, 5). These data were used to construct a panel of live births over a consistent set of years and countries. Two separate samples were used to evaluate abortion law reforms in the Dominican Republic and Mozambique. The Dominican Republic sample comprised 719 152 live births between 1999 and 2019 from 63 DHS/MICS surveys across 17 countries including the Dominican Republic. The Mozambique sample comprised 476

939 live births between 2004 and 2018 from 51 DHS/MICS surveys across 17 countries including Mozambique.

The exposure of interest was the adoption of a policy restricting abortion in the Dominican Republic in 2009 and decriminalizing abortion in Mozambique in 2014(35). In October 2009, the Dominican Republic's Constitutional Assembly included article 37 in its constitution, which established that the right to life is inviolable from conception until death, thus banning abortion under all circumstances, including cases of rape and situations in which the woman's health is at risk (36, 37). In July 2014, the Mozambique parliament introduced legislation which decriminalized abortion in the first 12 weeks of pregnancy; this extended to the first 16 weeks in cases of rape or incest and the first 24 weeks in cases of fetal anomaly. However, implementation was delayed and there were no clear guidelines for health facilities and providers (38). During the period between passing the law and the provision of implementation guidelines, providers refrained from providing abortion services due to confusion and lack of clarity around the policy guidelines, as well as fear of legal repercussions (36, 37).

Our outcome, neonatal mortality, was measured by a binary indicator of whether a child who was born alive died within the first month of life using the 5-year birth histories provided by women interviewed in the DHS/MICS.

MC-SIMEX

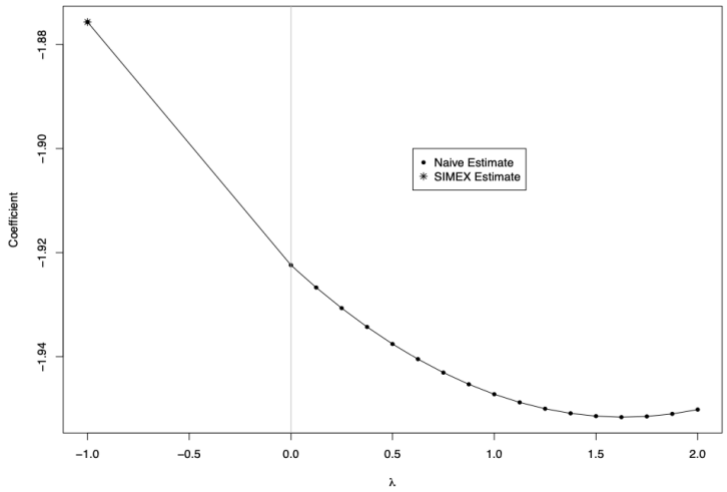
The Simulation and extrapolation (SIMEX) technique deals with additive measurement error in continuous variables (39). SIMEX was extended to handle misclassification of

categorical variables (27). This method, called MC-SIMEX, makes efficient use of misclassification rates (i.e., sensitivity and specificity) to produce bias-corrected estimates (27, 28). The probabilities of misclassification can be denoted in the form of a kxk misclassification matrix (Π) with k being the number of categories:

$$\Pi = \begin{pmatrix} \pi 00 & 1 - \pi 11 \\ 1 - \pi 00 & \pi\, 11 \end{pmatrix}.$$

If we denote the possibly mis-specified binary outcome by $Y^*$and the corresponding correctly measured (gold standard) variable by Y, the misclassification matrix (Π) is based on measured or hypothesized sensitivity and specificity parameters where $\pi\ 11\ =\ \mathrm{P}\ (Y^*\ =\ 1|\mathrm{Y}\ =\ 1)$ is the sensitivity and $\pi\ 00\ =\ \mathrm{P}\ (Y^*\ =\ 0|\mathrm{Y}\ =\ 0)$ is the specificity. The parameter of interest is β. If misclassification is ignored, then the estimator is classically called the naive estimator. We denote the limit of the naïve estimator (when the sample size goes to infinity) by $\beta^*(\Pi)$, since it depends on the misclassification matrix(Π).

Figure 6.1. A generic plot of the effect of misclassification on parameter estimates.



The degree of misclassification error (λ) is presented on the X axis. The MCSIMEX estimate is an extrapolation to λ = −1 whereas the naive estimate occurs at λ= 0. Adapted from Parveen N, Moodie EM and Brenner B. The non-zero mean SIMEX: Improving estimation in the face of measurement error. Observational studies. 2015; 2(1): 90-123.

The MC-SIMEX technique employs the function

$$\lambda \rightarrow \beta^*(\Pi^\lambda)$$

indicating that $\beta^*(\Pi^\lambda)$, the value of β^* at a particular level of misclassification, Π^λ, is a function of the degree of misclassification error λ. The procedure consists of a simulation step followed by an extrapolation step. In the simulation step, a degree or range of measurement error, in terms of the sensitivity and specificity of the misclassified covariate, is applied to the observed data to generate naïve estimates. In the extrapolation step, the results are extrapolated back to an expected point of absence of measurement error where $\lambda = -1$ (40, 41). As shown in

Figure 1, the MC-SIMEX estimate is an extrapolation to error level λ = −1 whereas the naive estimate occurs at error level λ= 0.

Statistical Analysis

We estimated the effect of abortion restriction in the Dominican Republic and abortion decriminalization in Mozambique on the probability of neonatal mortality using a difference-in-differences design, including fixed effects for country and year, and adjusted for individual, household, and country level covariates. The effect of abortion law reforms is therefore identified by changes in neonatal mortality occurring within each treated country during the study period relative to corresponding changes in countries that did not modify their policies during the study period (control countries). We used logistic regression with post-estimation to generate estimated effects on the risk difference scale:

$$logit[P(Y^*_{ict})] = \beta_0 + \beta^*_1 Policy_{ct} + g_t + d_c + \sum b_n X_{ict} + \sum b_k Z_{ct} + \varepsilon_{ict},$$

, where Y^*_{ict} represents the observed (error-prone) outcome for each observation i in country c and birth year t. We further assume that the misclassification is non-differential, so that the distribution of Y^*_{ict} does not depend on $Policy_{ct}$. Specifically, given the true unobserved values Y_{ict}, the observed values Y^*_{ict} contain no additional information about $Policy_{ct}$. Naive estimation substitutes Y^*_{ict} for Y_{ict} and yields the naive estimator β^*.

We corrected for possible misclassification of neonatal mortality using the MC-SIMEX approach, assuming non-differential misclassification of neonatal deaths across countries. We

explored different levels of misclassification by varying the values of sensitivity and specificity. We varied the degree of misclassification from 0 to 50%, since it is the largest upper bound of the reported literature and data with a suspected misclassification rate above 50% is considered systematically flawed (42). We examined two simulated scenarios: 1) maintaining the specificity at 100% and varying the sensitivity from 50% to 100% and 2) maintaining the sensitivity at 100% and varying the specificity from 50% to 100%. We selected these scenarios that held either sensitivity or specificity constant to examine the unique effect of each level of sensitivity or specificity, as done in previous studies using hypothesized probabilities in the absence of reliable validation data (43, 44).

The MC-SIMEX procedure was performed with 100 simulations with the variance calculated using the asymptotic estimation method. We investigated both quadratic and log-linear forms for the extrapolant function. While the default is quadratic, we compared estimates with the loglinear function as it yields less biased estimates than the quadratic fit (40). We used the R package 'simex' version 1.8 (45) which provides functions to use the MC–SIMEX method on our regression-based analysis, to produce graphics and summary statistics of the corrected estimates. All analyses were conducted using R Studio (46).

RESULTS

Table 6.1 displays the MC-SIMEX corrected estimates of the effect of abortion restriction on neonatal mortality in the Dominican Republic. Our naïve estimate indicates that abortion restriction was associated with an additional 9.3 (Standard Error (SE)=1.7) neonatal deaths per 1,000 live births. The MC-SIMEX estimates, correcting for the potential

misclassification of neonatal deaths, yielded higher risk differences and standard errors compared to the naïve estimates for all misclassification scenarios examined. At 90% sensitivity and 100% specificity, the quadratic and log-linear MC-SIMEX models showed that abortion restriction was associated with an additional 9.4 (SE=1.9) and 9.8 (SE=2.1) neonatal deaths per 1,000 live births, respectively. In comparison, when the sensitivity was reduced to 50% and specificity remained 100%, the estimated risk differences increased to 11.9 (SE=2.2) and 12.6 (SE=2.0) additional neonatal deaths per 1,000 live births, respectively. In general, quadratic and log-linear extrapolant functions yielded similar corrected estimates.

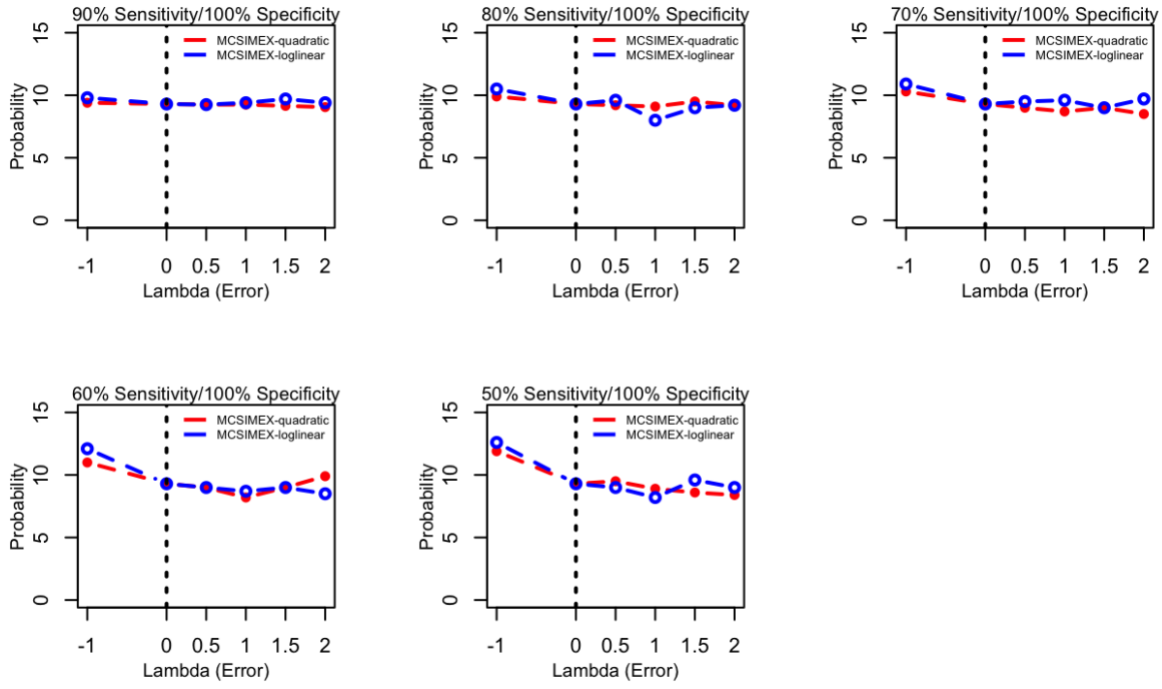
Table 6.1. Naïve and corrected risk difference estimates of the impact of abortion restriction in the Dominican Republic on the probability of neonatal death per 1000 live births, with different levels of misclassification.

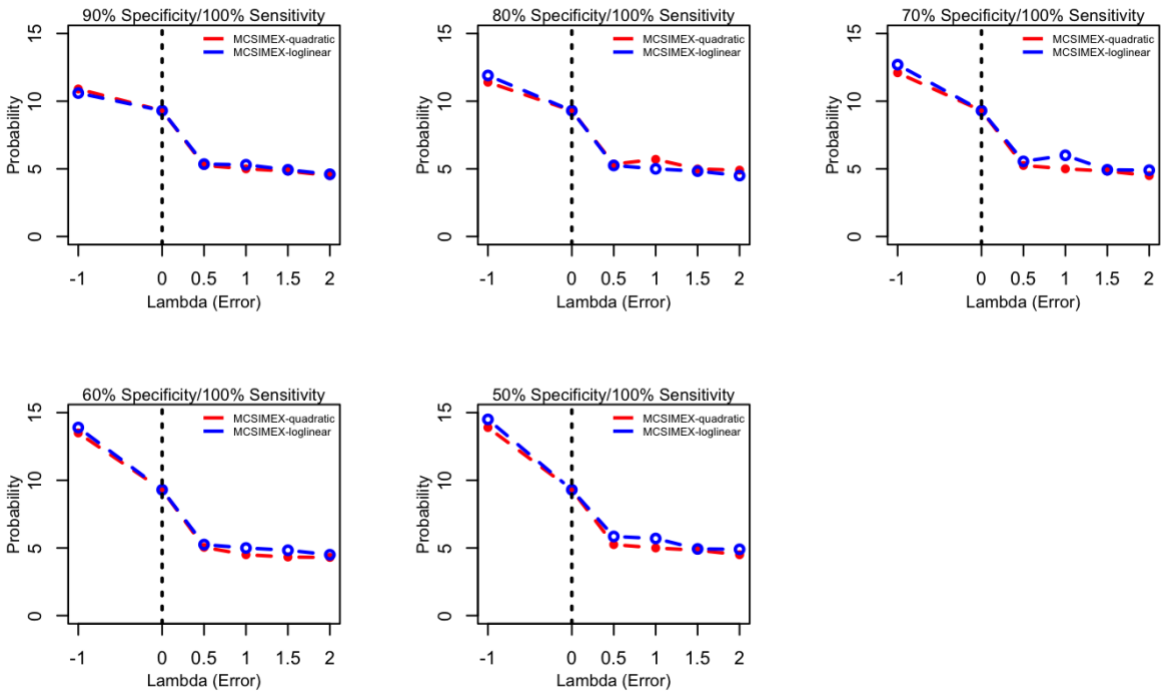
	Risk difference	**SE**	**Risk difference**	**SE**
Naïve Model	9.3	1.7	9.3	1.7
	(π11, π00) = (0.9, 1.0)		(π11, π00) = (1.0, 0.9)	
MC-SIMEX (Q)	9.4	1.9	10.6	2.2
MC-SIMEX (LOG)	9.8	2.1	10.9	2.3
	(π11, π00) = (0.8, 1.0)		(π11, π00) = (1.0, 0.8)	
MC-SIMEX (Q)	9.9	2.0	11.4	2.3
MC-SIMEX (LOG)	10.5	2.1	11.9	2.0
	(π11, π00) = (0.7, 1.0)		(π11, π00) = (1.0, 0.7)	
MC-SIMEX (Q)	10.3	2.0	12.1	2.1
MC-SIMEX (LOG)	10.9	2.2	12.7	2.2
	(π11, π00) = (0.6, 1.0)		(π11, π00) = (1.0, 0.6)	
MC-SIMEX (Q)	11.0	1.9	13.5	2.3
MC-SIMEX (LOG)	12.1	2.1	13.9	2.3
	(π11, π00) = (0.5, 1.0)		(π11, π00) = (1.0, 0.5)	
MC-SIMEX (Q)	11.9	2.2	14.0	2.2
MC-SIMEX (LOG)	12.6	2.0	14.5	2.3

π11 is the sensitivity, π00 is the specificity, MC-SIMEX(Q) is the quadratic fit in the extrapolation step; MC-SIMEX(LOG) is the log-linear fit in the extrapolation step

When we varied the specificity and kept sensitivity constant at 100%, MC-SIMEX corrected estimates were greater in magnitude and variance compared to the naïve estimator. At 90% specificity and 100% sensitivity, abortion restriction was associated with an additional 10.6 (SE=2.2) and 10.9 (SE=2.3) neonatal deaths per 1,000 live births based on quadratic and loglinear extrapolations, respectively. The log-linear models consistently yielded higher estimates than the quadratic function across models with different specificities. Figure 6.2 shows simulated and extrapolated (corrected) estimates of the impact of abortion restriction in the Dominican Republic on the probability of neonatal mortality, with MC-SIMEX corrected estimates at $\lambda = -1$ and the naïve estimate at $\lambda = 0$.

Figure 6.2. Simulation Extrapolation results. Estimated risk differences measuring the impact of abortion restriction on the probability of neonatal mortality per 1000 live births in the Dominican Republic.





The estimated risk differences are presented on the Y axis and the degree of misclassification error (λ) on the X axis. The dotted line at $\lambda = 0$ indicates the naive (uncorrected) values. In the simulation step, risk differences are estimated with increasing levels of measurement error ($\lambda = 0.5$ to $\lambda = 2$). Estimates at λ=-1 represent the extrapolated (corrected) values accounting for misclassification. Top: results showing changes in sensitivity. Bottom: results showing changes in specificity

Table 6.2 shows the MC-SIMEX corrected estimates of the effect of abortion decriminalization on neonatal mortality in Mozambique. With the naïve estimator, abortion decriminalization was associated with an additional 6.8 (SE=2.4) neonatal deaths per 1,000 live births. The MC-SIMEX corrected estimates (both quadratic and log-linear) were larger in magnitude and variance compared to the naïve estimates for all scenarios. At 90% sensitivity and 100% specificity, MC-SIMEX models showed that abortion decriminalization was associated with an additional 6.9 (SE=2.3) and 7.1 (SE=2.4) neonatal deaths per 1,000 live births for quadratic and log-linear models, respectively. When the sensitivity was reduced to 50%,

estimated risk differences increased to 8.9 (SE=2.3) and 9.7 (SE=2.5) neonatal deaths per 1,000 live births, respectively.

Table 6.2. Naïve and corrected risk difference estimates of the impact of abortion decriminalization in Mozambique on the probability of neonatal mortality per 1000 live births, with different levels of misclassification.

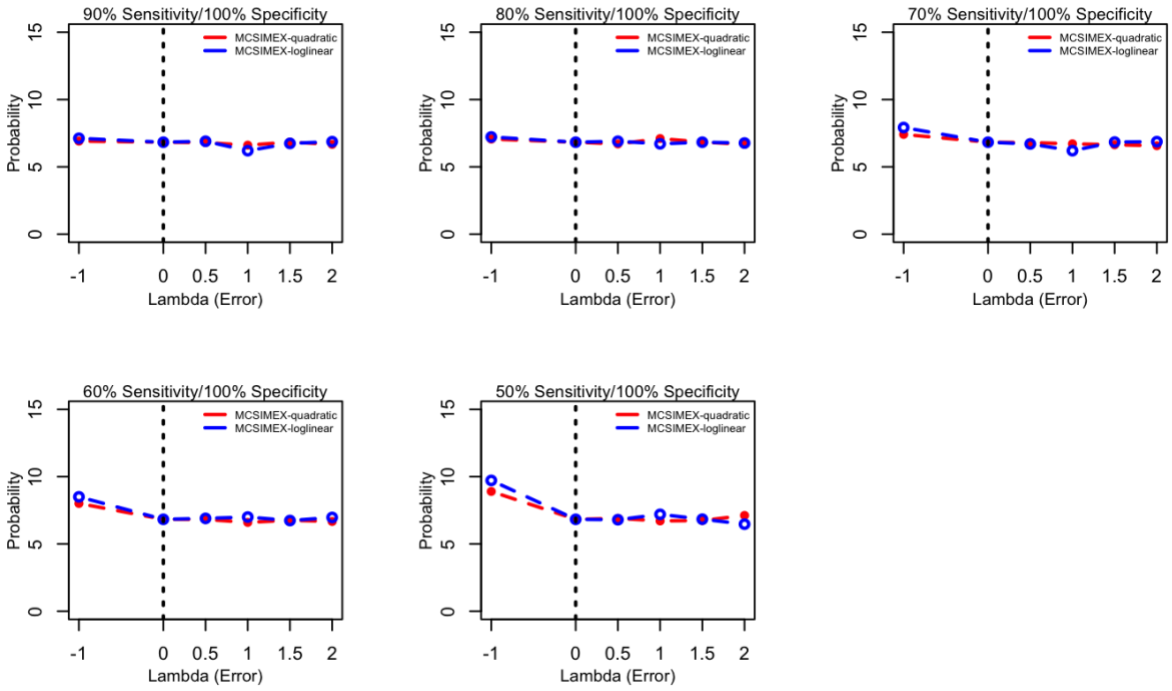
	Risk difference	**SE**	**Risk difference**	**SE**
Naïve Model	6.8	2.4	6.8	2.4
	(π11, π00) = (0.9, 1.0)		(π11, π00) = (1.0, 0.9)	
MC-SIMEX (Q)	6.9	2.3	7.5	2.4
MC-SIMEX (LOG)	7.1	2.4	7.9	2.4
	(π11, π00) = (0.8, 1.0)		(π11, π00) = (1.0, 0.8)	
MC-SIMEX (Q)	7.0	2.5	8.4	2.3
MC-SIMEX (LOG)	7.2	2.5	9.1	2.4
	(π11, π00) = (0.7, 1.0)		(π11, π00) = (1.0, 0.7)	
MC-SIMEX (Q)	7.4	2.3	9.6	2.2
MC-SIMEX (LOG)	7.9	2.4	9.9	2.3
	(π11, π00) = (0.6, 1.0)		(π11, π00) = (1.0, 0.6)	
MC-SIMEX (Q)	8.0	2.5	10.7	2.4
MC-SIMEX (LOG)	8.5	2.4	11.0	2.4
	(π11, π00) = (0.5, 1.0)		(π11, π00) = (1.0, 0.5)	
MC-SIMEX (Q)	8.9	2.3	11.1	2.3
MC-SIMEX (LOG)	9.7	2.5	12.0	2.4

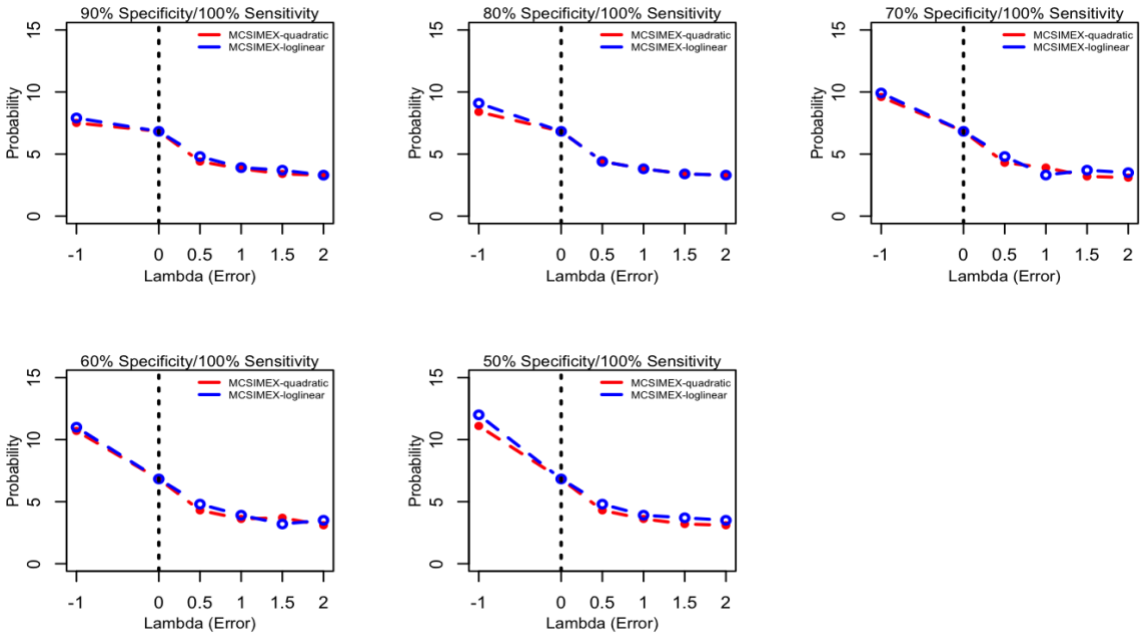
π11 is the sensitivity, π00 is the specificity, MC-SIMEX(Q) is the quadratic fit in the extrapolation step; MC-SIMEX(LOG) is the log-linear fit in the extrapolation step

When we varied the specificity and kept sensitivity at 100%, MC-SIMEX models suggested that the naïve estimate was underestimated to a greater extent than scenarios with imperfect sensitivity. At 90% specificity and 100% sensitivity, MC-SIMEX models indicated that abortion decriminalization was associated with an additional 7.5 (SE=2.4) and 7.9 (SE=2.4) neonatal deaths per 1,000 live births for quadratic and log-linear specifications, respectively. At 50% specificity and 100% sensitivity, we estimated an additional 11.1 (SE=2.3) and 12.0 (SE=2.4)

neonatal deaths per 1,000 live births from quadratic and log-linear models, respectively. As in the case of the Dominican Republic, estimates from log-linear extrapolant functions were greater than from quadratic forms. Figure 6.3 shows MC-SIMEX estimates from quadratic and log-linear models at varying levels of misclassification, as well as the naïve and corrected (extrapolated) estimates.

Figure 6.3. Simulation Extrapolation results. Estimated risk differences measuring the impact of abortion decriminalization on the probability of neonatal mortality per 1000 live births in Mozambique.





The estimated risk differences are presented on the Y axis and the degree of misclassification error (λ) on the X axis. The dotted line at $\lambda = 0$ indicates the naive (uncorrected) values. In the simulation step, risk differences are estimated with increasing levels of measurement error ($\lambda = 0.5$ to $\lambda = 2$). Estimates at λ=-1 represent the extrapolated (corrected) values accounting for misclassification. Top: results showing changes in sensitivity. Bottom: results showing changes in specificity

DISCUSSION

For all scenarios examined, corrected risk differences were consistently higher compared to naïve estimates, indicating that misclassification of neonatal deaths may have contributed to the underestimation of the impact of abortion reforms in the Dominican Republic and Mozambique. Our simulation extrapolation assessment of misclassification provides insight into the direction and magnitude of the bias. Naïve estimates were biased towards the null in both evaluations and the magnitude of the bias varied depending on the degree of misclassification and the extrapolant function.

Misclassification of neonatal mortality and stillbirth can occur in both directions. Some neonatal deaths may be recorded as stillbirths and stillbirths may also be erroneously classified as neonatal deaths (11, 14-16). Consequently, the magnitude of the bias may be unpredictable and usually depends on the misclassification probabilities (47, 48). Peven et al (14), in a multi-country, multi-site study assessing the accuracy of register-recorded stillbirth rates in LMICs, reported that neonatal deaths were more likely to be misclassified as stillbirths: 58.3% of neonatal deaths were misclassified as stillbirths vs 4.0% of stillbirths misclassified as neonatal deaths. This may suggest that misclassification of neonatal mortality as stillbirth is more common than the reverse, and that situations of imperfect sensitivity and higher specificity may be more reflective of real-world scenarios.

Sensitivity and specificity parameters can be estimated with information from external sources (external validation data) or additional data collection of a "gold standard" outcome in a subsample (internal validation data) to derive corrected estimates (44, 49). Verbal autopsy or additional questions in survey instruments are often used as to establish if a newborn showed any signs of life at birth and assist in the differentiation between stillbirths and neonatal deaths (10, 11, 14, 50). However, these reference standards may be imperfect. A caveat to all validation studies is that they assume that the gold-standard outcome measure represents the true outcome (49, 51). Caution should be exercised when using externally derived estimates of misclassification probabilities, as misclassification can be context specific and vary across populations.

Given the increasing use of routinely collected household surveys such as DHS/MICS for evaluating the impact of population level interventions, it is important to understand the

implications of outcome misclassification that may be common in such surveys. Our findings are germane to research on neonatal mortality and/or stillbirth as the outcome of interest, in particular. Misclassification of neonatal deaths and/or stillbirths could lead to biased estimates, as observed in our study, and threaten the validity of conclusions. Although our study assumed non-differential misclassification, differential misclassification of the outcome could lead to meaningfully different results.

An important limitation of this study was the use of hypothesized sensitivity and specificity parameters, which may not proxy the true misclassification probabilities in our study contexts. However, we assessed a range of plausible scenarios. In addition, we assumed that misclassification probabilities did not change over time for our DD design. However, if the sensitivity and specificity vary over time, then this could bias effects in unpredictable ways. One potential extension of our work could be the comparison of MC-SIMEX to other correction methods such as regression calibration and pooled estimation, with proper consideration of underlying assumptions.

CONCLUSION

Our analyses suggest that the naive estimates of the impact of abortion law reforms in the Dominican Republic and Mozambique were likely underestimated due to the misclassification of neonatal mortality in self-reported survey data. The MC-SIMEX method can provide corrected estimates and elucidate the consequences of misclassification when misclassification probabilities are known or hypothesized, including the potential magnitude and direction of bias in the naïve estimator. Rather than speculate about the effects of outcome

misclassification, we encourage the application of rigorous sensitivity analyses designed to quantify and correct for potential misclassification bias when estimating the impacts of population level interventions and the uncertainty around these estimates, with the objective of informing evidence-based decision making.

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6.3 Supplemental material: Manuscript 4

Supplementary Table 6.1: A generic table showing misclassification of neonatal mortality/stillbirth, column(Y)= verbal autopsy (gold standard), row(Y*)= self-reports from household survey

Household Survey(Y*)	Verbal Autopsy (Y)			
	Neonatal deaths	Stillbirths	Total	Misclassification rate (%)
Neonatal deaths	900	100	1000	10
Stillbirths	200	800	1000	20
Total	1100	900	2000	

CHAPTER 7. General discussion

7.1 Summary of Findings

The findings of this thesis provide a detailed picture of the impact of abortion reforms on reproductive and neonatal health and health inequalities in the Dominican Republic and Mozambique.

Chapter 2 (manuscript 1) provides a systematic review of the impact of abortion reforms in LMICs and highlights that the current state of knowledge provides little clarity or guidance, given that previous studies were limited in their internal and external validity. Most studies did not have an appropriate design to assess the causal effect of abortion reforms, did not include sensitivity analyses to examine the robustness of findings, and did not examine if reforms may have affected socioeconomic inequalities. Additionally, there was limited evidence for the effect of abortion reforms on outcomes such as neonatal mortality and contraceptive use.

Using difference in differences analyses, chapter 4 (manuscript 2) examines the impact of a reform that restricted abortion access in the Dominican Republic, which represents an improvement over previous evaluations that primarily used simple pre-post designs, which cannot disentangle the effect of policy from underlying secular trends in outcomes. Our DD analyses suggest that abortion restriction was associated with an additional 6.3 (95% CI= 2.1, 10.5) neonatal deaths per 1,000 live births and a 9.6 (95% CI= 4.2, 15.0) percentage-point decrease in modern contraceptive use, and this relationship varied by household wealth, rural residency, and educational attainment, suggesting heterogeneity in the impact of restrictive abortion policies on neonatal mortality and contraceptive use across socioeconomic dimensions.

Chapter 5 (manuscript 3) examines the impact of a reform that decriminalized abortion in some specific circumstances in Mozambique. Of note is the fact that implementation was delayed and there were no clear guidelines for health facilities and providers for a few years after the passing of the law. Our DD analyses suggest that abortion reform was associated with an additional 5.6 (95% CI= 1.3, 9.9) neonatal deaths per 1,000 live births and showed heterogeneity in the effect of abortion reform on neonatal mortality.

For socially disadvantaged women, including those in households with fewer assets, residing in rural areas, and those who didn't complete secondary school, the reform was associated with poorer neonatal outcomes. This result suggests that reforms decriminalizing abortion, if not accompanied by prompt implementation and guidelines, may also worsen neonatal mortality, and affect socioeconomic groups differentially. While the evidence suggests that abortion decriminalization was initially associated with increased neonatal mortality, without further data on neonatal mortality after the policy was fully implemented, it is difficult to assess whether the immediate adverse effect of the reform was reversed.

Chapter 6 (manuscript 4) applied the MC-SIMEX (Misclassification Simulation Extrapolation) to examine the effects of misclassification error on the estimated impacts of abortion reforms on neonatal mortality in the Dominican Republic and Mozambique when using national household surveys in LMICs. Our results suggest a potential underestimation of the impact of abortion reforms on neonatal mortality due to the misclassification of neonatal deaths. Given the increasing emphasis on utilizing routinely collected household surveys such as DHS/MICS for conducting impact evaluation of population level interventions, it is important to understand the implications of outcome misclassification that is common in such surveys.

The analyses conducted in this manuscript can be applied to other impact evaluation studies examining misclassification.

It is interesting that governments can act quickly to restrict abortion services and establish sanctions for illegal abortions but fail to establish how more liberalised grounds for lawful abortion are implemented, thus discouraging medical practitioners from providing the care to which women are legally entitled as soon as possible. Even with liberal law reforms, access to abortion can be restricted through reduced provider availability and increased financial and logistic barriers to care (15, 108, 109).

The availability of safe abortion depends not only on liberal legislation but also on political support, a permissive environment, and the ability and willingness of health services and providers to make abortion available (20, 33, 110, 111). National and international courts and tribunals are increasingly declaring that, while governments may use criminal sanctions to deter illegal abortion, they also have a duty regarding prompt implementation of liberal abortion reforms (112, 113).

This thesis provides an essential takeaway for policymakers and courts considering legislation affecting abortion access. Policies that gradually increase the constraints and expenses encountered by women in need of abortion services can have the same influence on access as large-scale policy changes.

The influence of abortion reforms on health inequities across socioeconomic dimensions was shown through stratified analysis conducted in the second and third manuscripts. Abortion reforms are likely to disproportionately affect women of lower socioeconomic status, including those with lower educational attainment, fewer assets, and those living in rural areas,

potentially exacerbating health outcomes for them and their infants. Inequities in abortion access are driven by structural factors including decreased access to health care, provider availability, lack of insurance coverage, and economic disadvantage (18, 21, 114-117).

Understanding the systemic nature of these disparities and their relationship to health outcomes provides an essential context for the consideration of disparities in the impact of abortion reforms. Our findings encourage further research on socioeconomic inequities in the effects of abortion policies and other health outcomes given the persistent sociopolitical context surrounding reproductive policies.

7.2 Methodological Discussion: Strengths and Limitations

This dissertation comprises methodological approaches aimed to avoid or mitigate the potential threats to validity posed by the study designs and/or the nature of the data. Here, I provide an overview of the methodological issues addressed throughout the development of this dissertation.

7.2.1. Use of Household Survey Data

The systematic sampling methodology of the DHS/MICS is designed to make the sample as representative of the general population as possible (59). However, the overall probability of selection of each household is not a constant. In order for our statistical inferences drawn from the survey data to be generalizable to the target population, we incorporated sampling weights to account for individual survey sampling design. We assessed the sensitivity of our main findings by comparing effect estimates with and without the incorporation of respondent-level sampling weights. Also, we benefited from the availability of multiple DHS surveys allowing the

construction of 10 years of pre-intervention data. This enabled us to model pre-intervention trends in the outcomes and check for violations of the parallel trends assumption.

7.2.2. Measurement error

There is potential for measurement error due to underreporting and misclassification of outcomes. This is not uncommon in retrospective histories collected through household surveys such as DHS/MICS because of difficulties associated with accurate recall or willingness to report both the occurrence and timing of events and may bias estimated effects (81, 82). For contraceptive use, women are typically asked to provide information on all contraceptive use during the five calendar years preceding the survey. However, we utilized only data on modern contraceptive use at the time of interview because the contraceptive calendar providing a history of contraceptive use up to 5 years prior to the interview was not available for most of our sampled countries. As such, we expect the reporting of contraceptive use to be less subjected to measurement error.

For neonatal mortality, we considered birth histories of 5 years (up to 59 months) before the interview. Underreporting and misclassification of birth events such as neonatal mortality and stillbirth are not uncommon in DHS/MICS survey. While there is potential for differential misclassification of the outcome, such that error rates in attribution of neonatal deaths as stillbirths or vice versa depend on the treatment assignment, this is unlikely. Thus, we accounted for non-differential misclassification using hypothesized misclassification probabilities. As reflected in our analysis, the potential effect is an underestimation of the observed association while the magnitude varied depending on the sensitivity/specificity parameters. Nonetheless, underreporting and misclassification should be assessed using

reliable measures of misclassification probabilities or validation studies. Another limitation of using birth histories to collect data on neonatal mortality is that only surviving mothers can be interviewed. This also likely leads to an underestimation of effect estimates.

7.2.3. Statistical Inference

As with any difference-in-differences analysis, attribution of contraception and neonatal mortality estimates to the abortion reform in the Dominican Republic and Mozambique assumes that there are no unobserved time-varying confounders correlated with the timing of the abortion reform which could influence the outcomes. Although this assumption is not directly testable and we could not account for all potential sources of such confounding, we controlled for time-invariant country differences, secular time trends, and relevant individual, household, and country level characteristics.

We also performed several sensitivity analyses to assess the robustness of our results, including logistic models, unweighted estimates, and leads and lags. They generally indicated that observed associations were robust to alternate specifications. In addition, observations in DD analyses are generally not independent. Concerns arise about both the correlation between individuals in a cluster at a point in time and the serial correlation of the same cluster over time (98, 104). We used cluster robust standard error in DD models to account for this correlation. However, it may be downwardly biased, particularly in analyses with a small number of units and variable cluster sizes like ours.

7.2.4. Power and sample size

Our study might be underpowered, even with multiple time points and control units. Recent simulations demonstrated that in addition to the number of time points, other factors such as the sample size per time point, similar pre-and post-intervention timepoints, correlation structure and expected effect size need to be considered to denote a DD analysis as sufficiently powered (118). Outcomes such as neonatal mortality are infrequent, and with a limited sample size, there may be substantial uncertainty in some of our estimates.

7.2.5. Other issues

We lacked measures on abortion uptake and, as such, cannot ascertain the degree to which abortion reforms in the Dominican Republic or Mozambique influenced the utilization of abortion. Utilization of abortion is difficult to measure due to the stigmatisation and restrictive nature of the procedure in many contexts, which will affect the data sources, reliability, and validity (119, 120). Women may purchase misoprostol to induce abortion from drug sellers (licensed or unlicensed), community networks and increasingly online (5, 121, 122). This informal use of misoprostol to induce abortions also affects accurate estimates of abortion uptake. Nevertheless, these limitations highlight areas for future research, including examination of the varying degrees to which different abortion policy reforms influence access to and uptake of abortion and subsequent health outcomes.

Based on the mechanisms through which abortion reforms are hypothesized to influence neonatal mortality such as increased psychosocial stressors during pregnancy and increased pregnancy complication, it seems plausible that births occurring after reforms may have been unaffected by the reform while in utero. As such, the estimated date of conception

might be as important as the timing of the birth in defining our exposure period. Thus, we examined whether the association between abortion reform and neonatal mortality varied by timing of exposure by modifying exposure definition to include gestational period.

In addition, we did not assess the associations between abortion decriminalization and modern contraceptive use in Mozambique because of the lack of contraceptive calendar data in DHS/MICS in Mozambique. Finally, generalizing our findings to other settings is challenging due to the varied political, cultural, religious, and socioeconomic contexts of abortion legislation. However, we view our analysis as providing a useful benchmark of the potential impacts of proposed reforms.

7.3 Opportunities for future research

There could not be a more relevant time to assess the impact of abortion reforms given the recent overturning of Roe v. Wade in the United States. While a lot remains unknown about the broader impact of this reform, it could have drastic impact on funding and policies for sexual and reproductive health and rights internationally, including cuts to US multilateral and bilateral funding for safe abortion services, family planning, sexual education, and gender-related public health programs in LMICS.

As more data becomes available and as the landscape for reproductive health care continues to change, it will be important for researchers to continue to evaluate abortion policy reforms in LMICs. Future research could examine the effects of legislation that impact abortion access including gestational age limits, mandatory waiting periods, requirements for third-party authorizations and parental involvement laws that require unmarried teen minors to obtain parental consent or require providers to notify the minor's parent before an abortion is

performed. Similarly, abortion reform can have significant impacts beyond reproductive and neonatal health outcomes, extending to broader spheres including educational attainment and labour market outcomes. Future research could potentially focus on the much broader impacts of abortion reforms in LMICs.

In addition, although prior research has set a scientific foundation on which to understand the influence of abortion reforms on reproductive, maternal, and neonatal health outcomes, notable conceptual and methodological gaps in the literature remain, particularly for neonatal health. Further research is needed to elucidate the mechanisms that explain the relationship between abortion reforms and neonatal health.

7.4 Conclusion

This thesis provides a robust evaluation of the impact of abortion reforms by examining the causal impacts of abortion decriminalization, as well as restrictive law changes, on reproductive and neonatal health using national household survey data. In addition, we examined heterogeneous effects of the reforms as well as quantifying and correcting for potential misclassification bias. While acknowledging the limitations of this thesis, the approaches presented here have the capacity to expand and improve upon the current body of evidence and contribute to the scientific knowledge on the impact of abortion reforms in LMICs.

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