**Doctoral Dissertation**

# Determinants of Health among Women and Children in Nepal: Three Pillars of Sustainable Development Perspectives

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# Determinants of Health among Women and Children in Nepal: Three Pillars of Sustainable Development Perspectives

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# DEDICATION

This dissertation is dedicated to the memory of my grandparents, Mr Hem Nath Chalise and Mrs Om Kumari Chalise, as well as Mr Shiva Prasad Gautam and Mrs Sabitri Gautam, who lavished me with love and care throughout my childhood but could not live to this moment to experience my happiness in its fullest. I pray that they may look down on me and feel proud knowing that I did my best to achieve it.

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## ACRONYMS

| | |
|---|---|
| ACLED | Armed Conflict Location and Event Data |
| AOD | Optical Aerosol Depth |
| APGAR | Appearance, Pulse, Grimace Activity and Respiration |
| COVID | Corona Virus Disease |
| CPI | Consumer Price Index |
| CSDH | Commission on Social Determinants of Health |
| DID | Difference-in-difference |
| EAs | Enumeration Areas |
| EHS | Extended Health Services |
| FHD | Family Health Division |
| GBD | Global Burden of Disease |
| GDP | Gross Domestic Product |
| GES-DISC | Goddard Earth Sciences Data and Information Services Centre |
| IOC | Indian Oil Corporation |
| LBW | Low Birth Weight |
| LMICs | Low and Middle-Income Countries |
| MODIS | Moderate Resolution Imaging Spectroradiometer |
| MoH | Ministry of Health |
| MSE | Mean Square Error |
| NASA | National Aeronautics and Space Administration |
| NDHS | Nepal Demographic and Health Survey |
| NPC | National Planning Commission |
| ODK | Open Data Kit |
| OMI | Ozone Monitoring Instrument |
| OZ | Ozone |
| PM | Particulate Matter |
| PMWH | Paropakar Maternity and Women's Hospital |
| PPH | Postpartum Haemorrhage |
| PSU | Primary Sampling Units |
| RD | Regression Discontinuity |
| RDD | Regression Discontinuity Design |
| SDGs | Sustainable Development Goals |
| SDH | Social Determinants of Health |
| UN | United Nations |
| UNICEF | United Nations Children's Fund |
| US | United States |
| USD | United States Dollar |

## SUMMARY OF THE DISSERTATION

Health conditions at the early stage of life are an essential precursor to sustainable development. Children who are healthy during their early years have a higher quality of life, acquire the potential to contribute to their community, and ultimately contribute to long-term economic development. However, children's health is a product of interactions between social, economic and environmental circumstances. Thus, the PhD dissertation aims to explore and understand the social determinants of health from the three pillars of sustainable development perspectives. In this process, the study examines the impacts of economic and environmental factors on maternal and child health, considering trade stagnation due to the 2015 Nepal-India border blockade as an example of economic conditions, air quality changes due to the blockade as an example of environmental conditions, and cultural beliefs about the auspiciousness of full moon births as an example of social conditions.

The PhD dissertation follows three distinct but related quantitative studies, organised into three analytical chapters, using secondary data from a nationally representative population and health survey and the hospital-based longitudinal birth records, supplemented with administrative and geospatial data. The analytical approach is based on a potential outcome framework in which the observed health outcomes are contrasted with the counterfactual outcomes to estimate the health impacts. The analysis mainly uses Regression Discontinuity Design (RDD) to estimate the causal health impacts based on a score and a cutoff.

The first analytical chapter assessed the effects of the 2015 Nepal-India border blockade on child mortality. Using the 2016 Nepal Demographic and Health Survey (NDHS) province panel data, the study shows a significant increase in infant mortality during the blockade months. It suggests increased death probabilities for children within a month of their life after adjusting mother and household characteristics in the individual-level analysis. The result is unconfounded by the pre-existing mortality trend and robust to nonparametric regression discontinuity estimation. Furthermore, the mortality effects are intense among high-risk mothers exposed to blockade during their first trimester. These findings have significant emotional and economic implications for the family and society.

Based on the observation that the border blockade temporarily changed the air quality in Nepal, the PhD dissertation examines the effects of outdoor air pollution on birth weight. The study revealed that children born within the last two months of the blockade had lower birthweight than those born after the blockade. However, the adverse effect disappears with statistically insignificant results when comparing the birthweight to those born in the same months in the adjacent years, adjusting for births in adjacent months. These findings suggest the need for further research with more reliable air quality data to examine the effects of air pollution on children. Furthermore, the study raised intriguing questions regarding the extent to which political instability and the consequent trade stagnation might have contributed to climate change vulnerability in Nepal.

The third analytical utilises the cultural beliefs about the auspiciousness of full moon days in Nepal to investigate the effect of a social factor on birth timing and child health. The study shows significantly higher birth rates on full moon days compared to days after the full moon, with caesarean delivery driving the excess births on full moon days. It also demonstrates that births occurring on or before the full moon day have poor neonatal health outcomes, especially among high-risk mothers. The study findings raised essential concerns about using medical resources and the consequent health outcomes, in response to non-medical reasons, like cultural beliefs, from the sustainable development perspective.

The PhD dissertation illustrates that the three-pillars framework is an integrated and supplementary approach to understanding non-medical causes of health and wellbeing. According to the framework, health and wellbeing depend on the balance of economic, environmental and social aspects of sustainable development, whereas discrepancy in either

component contributes to poor child health outcomes. In other words, the economic, environmental and social components represent independent but intertwining health domains through which maternal and child health is created or constrained. Their interaction must be regulated to achieve Sustainable Development Goals (SDGs). Therefore, the dissertation emphasises continued investment and a favourable policy environment to encourage young children's health and human capital development. It also urges the academic community to elucidate operational ambiguity associated with the three-pillar paradigm and the social determinants of health concepts due to the emerging flexibility in interpreting these theoretical models.

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*"More than ever before, there is a global understanding that long-term social , economic, and environmental development would be impossible without healthy families, communities, and countries."*

~ Gro Harlem Brundtland
The 29th Prime Minister of Norway

# CHAPTER 1 INTRODUCTION

## SUSTAINABLE DEVELOPMENT AND HEALTH: CONCEPTUAL AND METHODOLOGICAL UNDERPINNING

This chapter introduces sustainable development and health concepts, identifies gaps in realising the sustainable development and health nexus, and outlines the dissertation's focus. It begins with setting the background by highlighting maternal and child health status relating to sustainable development.

## BACKGROUND

The past two decades observed substantial improvement in maternal and child health, but the achievements are yet to be sustained. Globally, child mortality was reduced by almost half and neonatal mortality by almost one-third between 2000 and 2019 (Paulson et al. 2021). Similarly, a sharp decline in maternal mortality (by almost 44%) from 385 to 216 deaths per 100,000 live births was witnessed between 1990 and 2015, with simultaneous improvement in the frequency and coverage of essential health interventions[1] for mothers and children (Alkema et al. 2016; Boerma et al. 2018). However, the achievements thus far are sluggish in several Low and Middle-Income Countries (LMICs).[2] For example, most sub-Saharan African and South Asian countries are still lagging in preventing maternal and child mortality up to the level stipulated by the Sustainable Development Goals (SDGs). Around 1.8 million children are expected to die within the first 28 days of their life, 68.8 million children within the first five years and 3.9 million women during their pregnancy in the next ten years (Alkema et al. 2016; Hug et al. 2019; Paulson et al. 2021; You et al. 2015). Furthermore, maternal and child health interventions and services in these countries is of poor quality and unevenly distributed, leading to within-country health inequality based on socioeconomic attributes (Boerma et al. 2018). These results suggest a need for continued focus on mother and child survival for sustainable development.

In addition to survival, sustainable development is not attainable unless women and children have equal opportunities to fulfil their potential. Children achieve their development potential when they learn the skills necessary for intellectual, behavioural, social and economic success. However, not all surviving children have opportunities to develop to their capacity. Despite the global commitment to advancing early childhood development, around 250 million (43%) children in LMICs are at elevated risk of achieving their development potential (Black et al. 2017). Biological and behavioural imprints formed during pregnancy and early childhood may push these children into poor health and subsequent vulnerability. For example, malnutrition and maternal illness during pregnancy affect health outcomes at birth, such as low birth weight and childhood illness. They also increase the risk of poor health throughout life (e.g. chronic diseases and poor mental health), reduce the likelihood of education attainment and affect economic outcomes (Black et al. 2017; Gluckman et al. 2008; McEniry 2013). Moreover, health adversities in early life linger into the future generation (e.g. [Addo et al.

[1] Essential health interventions consist of health programmes and services targeted to women and children during pregnancy, childbirth and postnatal period consisting of a range of services such as family planning, antenatal care and skilled attendance, immunisation, management of childhood illness and nutrition.

[2] Lawn and KC (2020) provide Nepal-specific context and progress of maternal and child health-related SDGs. See also Sachs et al. (2022) for an indicator wise overview of SDGs in South Asia, including Nepal.

2015]), resulting in a vicious cycle of human capital loss and continued intergenerational poverty.

The determinants of health adversities (or poor health outcomes) in early life are rooted in sociopolitical, economic and environmental factors. These factors may interact with each other and mutually constrain health outcomes. For example, environmental change, migration and urban growth in low-income areas are linked with childhood morbidity, unintentional injuries and malnutrition, reducing children's development potential (Ezeh et al. 2017). Evidence from LMICs further illustrates a significant rise in poor health outcomes (i.e. childhood stunting) due to the interaction between low maternal education, child maltreatment and economic disparities (Richter et al. 2017). Another example is the exposure to armed conflict that may lead to major health setbacks for young children through maternal stress and access to basic needs (Phadera 2021; Tapsoba 2023). These illustrations indicate that health is an essential prerequisite for sustainable development.

## SUSTAINABLE DEVELOPMENT AND HEALTH

Although conceived in the 1970s [3], sustainable development was popularly conceptualised in the Brundtland Commission's report in the late 1980s. The report described sustainable development as a means of addressing human development needs without surpassing planetary resources in the long run (Sachs 2015; von Schirnding 2002). The initial notion of sustainable development centred on the environment and economic interdependence[4], but it has since been expanded to non-economic and non-environmental elements of human existence. Indeed, the 1992 Rio Declaration re-envisioned sustainable development as a harmonious balance between human well-being and nature.[5] It thus recognised the importance of protecting humans from health risks associated with economic activities and environmental degradation (von Schirnding 2002). Ten years later, the Johannesburg Conference extended the focus of sustainable development beyond the economy and environment to social issues. They were articulated as a three-dimensional interwoven framework to vividly convey the contemporary meaning of sustainable development (Haines et al. 2012). The three-dimensional framework is called the triple bottom line or the three pillars of sustainable development.[6]

Kjærgård, Land, and Pedersen (2014) contemplates that health and sustainable development exist in duality.[7] Health is both a predictor and a resource for sustainable development. Studies demonstrated that mortality and morbidity stretch long-term economic development by around 10%, whereas a 10% increase in labour productivity is associated with better health (Bloom et al. 2022; Rocco et al. 2021). However, such views are not always entirely obvious. For example, rarely do studies examining the impact of health on sustainable development go beyond the economic aspect. Macroeconomic studies barely define sustainable

[3] The Stockholm conference on Human Environment in 1972 and a book on "Limits to Growth" by the Club of Rome the same year were the steppingstones to sustainable development (Sachs 2015).

[4] Generally, sustainable development was perceived as balancing the future generation's environmental needs with the present generation's economic needs (von Schirnding 2002).

[5] Agenda 21 of the Rio declaration prioritised primary healthcare, illness prevention, urban health, and environmental sanitation for health and sustainable development (Haines et al. 2012; von Schirnding 2002).

[6] See Purvis, Mao, and Robinson (2019) for conceptual origin of the three pillars of sustainable development framework.

[7] Hancock (1993) offers an initial interpretation of health from the community, economic and environmental aspects. Hancock's framework suggests that interaction between the environment and the economy results in sustainability, whereas the interaction between the environment and community results in health equity.

development in economic terms. They estimate average impacts of health capital on economic growth without any reference to whether the impacts are equally distributed in the population (Subramanian, Belli, and Kawachi 2002). Similarly, microeconomic studies have extensively examined the income effects of early life health conditions but are limited to elucidating the connection between health and other aspects of sustainable development (Prinz et al. 2018).

While human health may constrain or facilitate sustainable development, it is also conditioned by socioeconomic and environmental factors (i.e. the three pillars of sustainable development). For example, one-fifth of global mortality and morbidity are attributed to environmental causes. Approximately one-third of the global burden of childhood diseases is associated with exposure to harmful environmental conditions in low-income countries (Murray et al. 2020). However, socioeconomic and environmental measures such as well-managed sanitation facilities to reduce infectious diseases, an efficient public transport system to prevent obesity, and an affordable housing scheme to increase food security may contribute to better health (Haines et al. 2012; Solar and Irwin 2010). Thus, health is a multisectoral development challenge, not limited to the purview of the health sector or any other sector in silos. The multifaceted nature of health determinants demands health-centred public policies that systematically address the health consequences of economic development and prevent adverse health outcomes.

## THE BIFOLD DEFICIENCY

Having demonstrated that health is a complex development challenge, this section describes two layers of complexities to understanding the health and sustainable development nexus. Firstly, conceptual failure, such as the narrow focus of the health sector on the curative and biomedical approach to health, tends to overlook the roles that socioeconomic and environmental factors play in causing and resolving poor health outcomes (Whitmee et al. 2015). However, the concept of health in the discourse on sustainable development extends beyond the biological causes of diseases or health outcomes. From the sustainable development perspective, health and well-being are states in which human needs and resources are balanced, while disease results from poverty and overconsumption (Ríos-Osorio, Salas-Zapata, and Ortiz-Lobato 2012). Furthermore, gross domestic product (GDP) is overstated in development economics discourse as an indicator of human well-being, despite its limited ability to reliably capture changes in health status (Whitmee et al. 2015).

The second level of complexity is the information or evidence gaps, such as limited biomedical research investigating health and human system (i.e. socio-political, economic and environmental) linkages. Although epidemiological methods have been a proven strategy for determining the impact of risk factors on health outcomes, they focus on examining biological determinants and their mediations (McMichael 1999; Whitmee et al. 2015). The social determinants of health are increasingly prioritised in public health research, but there remain as many, if not more, uncertainties about the mechanism behind their influence on health. They are notoriously challenging to study due to the complexity of the underlying causal pathways and the frequently lengthy time frames over which socioeconomic factors unfold health impacts (Braveman and Gottlieb 2014; McQueen 2009; Palmer et al. 2019). Consequently, most studies on social determinants of health are confined to correlation analysis of a few social factors such as education, gender and income (Palmer et al. 2019). These limitations lead us to believe that a multidisciplinary approach is necessary to investigate the causes and solutions for the complex problem.

## FOCUS OF THE DISSERTATION

Given the conceptual and evidence gap discussed in the previous section, the PhD dissertation aims to explore and understand the social determinants of health from the three pillars of sustainable development perspectives. More specifically, this study aims to examine Nepal's economic, environmental and social impacts on maternal and child health through an empirical approach. In so doing, this study considers actions or events that represent the economic, environmental and social dimension of the three-pillar framework. The study focuses on following specific objectives to achieve the overarching goal,

- Estimate the impact of trade stagnation (i.e. economic factor) on child mortality using the 2015 Nepal-India border blockade as a natural political experiment.
- Assess the effect of outdoor air pollution (i.e. environmental factor) on birthweight among Nepali children based on the temporal changes in air quality during the 2015 border blockade.
- Examine the birth timing and child health impacts due to the cultural belief (i.e. social factors) about the auspiciousness of full moon days in Nepal.

The conceptual framework presented by the Commission on Social Determinants of Health (CSDH) has inspired the development of this study (CSDH 2008; Solar and Irwin 2010). The CSDH framework suggests that many complex societal problems (such as chronic diseases, delinquency and poverty) originate in early childhood and are manifested in poor maternal and child health. The framework further states that the causes of poor health outcomes are embedded in social, economic, political, cultural and environmental factors, referred to as the upstream or distal social factors (CSDH 2008; Solar and Irwin 2010). These upstream determinants are the critical resources and opportunities that mediate individuals' life circumstances (e.g. living conditions and food availability), health behaviour and biology (Braveman, Egerter, and Williams 2011).

This study, therefore, started with the position that children who are healthy in their early childhood have a better quality of life, develop the capacity to contribute to their society, attain economic prosperity and eventually contribute to sustainable development (Richter et al. 2017). However, health in early life is the product of interaction between economic, ecological and social constructs. These three intertwining components are to refer by the three pillars framework as the sustainable development pathways through which health is determined beyond the traditional and exclusive focus of the biomedical approach alone (Kjærgård, Land, and Pedersen 2014; Ríos-Osorio, Salas-Zapata, and Ortiz-Lobato 2012).

According to the framework, health is created when economic development is ecologically sustainable and equitable. Equally important are the environmental circumstances (e.g. built environment) that create healthy communities and the sociocultural values that govern the use of natural resources (Hancock 1993; Kjærgård, Land, and Pedersen 2014). The three essential components, as the pathways outlined for sustainable development (Purvis, Mao, and Robinson 2019), laid the foundation of this study. The conceptual framework emphasises the three upstream determinants as a prerequisite for sustainable development. This information about the theoretical underpinning allows for applying multidisciplinary knowledge to the problems of poor health and their solutions, conceptualising health as a multifaceted phenomenon. The theoretical framework should aid new insights into contemplating health from the sustainable development perspective.

The methodological assumptions of this study follow the causal inference analysis in line with the study objectives. Thus, the researcher accepts an ontological assumption that counterfactual health outcomes exist without intervention or the treatment under investigation. In other words, the researcher admits the existence of alternative outcomes that would have occurred had there been no intervention or treatment (Sarvet and Stensrud 2022). The researcher also concurrently considers the epistemological assumption that the observed and counterfactual health outcomes are independent of all other observed and unobserved characteristics, such as human behaviour, social values and external environment except the treatment. The researcher then estimates the counterfactual outcomes to quantify the treatment effects. In so doing, the researcher applies various empirical methods from econometrics.

This study is mainly built on the regression discontinuity design (RDD) using the cross-sectional survey data and the longitudinal hospital-based data. The RDD allows the researcher to estimate counterfactuals considering the randomness in treatment assignment due to the score received by the study units relative to the specific cutoff (Cattaneo, Titiunik, and Vazquez-Bare 2020). A detailed elaboration of empirical methods is presented in the subsequent analytical chapters. The empirical approaches used in this study should contribute to understanding methodological issues and challenges in analysing the impacts of upstream determinants of health. Furthermore, the findings of this study should help health sector stakeholders in Nepal monitor opportunities and challenges in achieving health-related SDGs by 2030. It should also aid policymakers in better appreciating the upstream determinants they should emphasise in the existing and future child health policies.

## ORGANISATION OF THE DISSERTATION

The overall construction of this dissertation takes the form of five chapters consisting of this introductory chapter, three analytical chapters and the final chapter on general discussion. Chapter 1 provided an overview of progress towards achieving SDGs regarding maternal and child health, highlighted the nexus between sustainable development and health, reviewed challenges in examining the nexus, and outlined the focus of this dissertation. Chapters 2 to 4 are the analytical chapters derived from the statistical analysis in line with the specific research objectives. Chapter 3 estimates the effect of economic factors on child mortality, considering the trade stagnation due to the 2015 Nepal-India border blockade as a natural political experiment. Based on the expectation that the border blockade temporarily changed the air quality in Nepal, Chapter 3 examines the effects of outdoor air pollution on birth weight. Chapter 4 utilises the cultural beliefs about the auspiciousness of full moon days in Nepal to investigate the effect of a social factor on birth timing and child health. The final chapter is a general discussion of the findings from the three analytical chapters. It reflects on conceptual and methodological aspects of understanding sustainable development and health concerning the existing literature and findings from the analytical chapters. It also addresses the implication of the PhD dissertation in terms of health and sustainable development policies and practices. References are listed at the end of each chapter.

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*"For the only way in which a durable peace can be created is by world-wide restoration of economic activity and international trade."*

~ James Vincent Forrestal
US Defense Secretary

# CHAPTER 2 ECONOMY AND HEALTH

## EFFECT OF THE 2015 NEPAL-INDIA BORDER BLOCKADE ON CHILD MORTALITY

A volatile political climate, growing public unrest, or consequent outbreaks of violence can stagnate domestic economic activities and hamper the inflows of people, goods, and money from foreign countries (Ades and Chua 1997; Martin, Mayer, and Thoenig 2008). Trade stagnation due to an unstable socio-political environment can be even worse in landlocked developing countries that depend heavily on neighbouring countries for their imports. Finding alternative trading channels may challenge these countries during the socio-political unrest.[8] Moreover, high trade costs caused by poor infrastructure may further impede efficient logistic services (Faye et al. 2004). Landlocked low-income countries, thus, have limited means to insure against vagaries of the supply of goods caused by social turmoil and consequent trade stagnation.

An ethnic movement in Nepal restricted trade flows across Nepal-India borders leading to an unofficial embargo in Nepal from 23 September 2015 to the first week of February 2016 (Tripathi 2019). The blockade profoundly impacted all spheres of life, with the health sector bearing the brunt of the damage. The healthcare system, already stretched by the aftermath of the 2015 earthquake,[9] observed a dearth of life-saving drugs (Sharma, Mishra, and Kaplan 2017). The severe fuel shortage further halted the supply of food, medicine and vaccines, putting women and children at risk of mortality and morbidity. Based on these observations, therefore, we exploit the 2015 Nepal-India border blockade as a natural political experiment to investigate the health consequences of trade stagnation in Nepal.

There is no dearth of literature suggesting adverse health consequences of trade stagnation. The list of compelling cases includes, but is not limited to, Cuba subject to the United States (US) embargo from 1960 to 2017 (Garfield and Santana 1997), Iraq under the United Nations (UN) sanctions in the 1990s during the Gulf war (Ali and Shah 2000; Daponte and Garfield 2000), and Haiti embargoed by the UN between 1993 and 1994 (Gibbons and Garfield 1999). More recent examples are Iran under international sanctions reinforced between 2012 and 2015 against its nuclear programme (Aloosh, Salavati, and Aloosh 2019), and the Democratic Republic of the Congo affected by US regulations in conflict minerals in the 2010s (Parker, Foltz, and Elsea 2016).

This study contributes to the above literature in two ways. First, while previously published studies either qualitatively described the adverse health effects or assessed the association between trade and health,[10] we attempt to maximise the empirical rigour in

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[8] One typical example is an acute shortage of domestic supplies experienced by landlocked African countries such as Malawi and Rwanda in the 1980s due to civil unrest in neighbouring Mozambique, Uganda and Tanzania (Ades and Chua 1997).

[9] The 7.8 magnitude earthquake of April 2015 and several aftershocks affected 14 districts of central Nepal with the loss of 9000 lives and more than 22000 injuries. The earthquake destroyed 446 health facilities with interrupted services and supply of health commodities in the affected areas (NPC 2015).

[10] Many studies examined economic impacts; for example, Juhász (2018) explored the effect of the Napoleonic Blockade on long-term economic development. Frankel (1982) finds that the US self-imposed embargo of 1807-1809 effectively put Britain in a state of autarky, leading to a significant rise in relative prices in Britain. Irwin (2005) also explored this historical event from the US perspective, finding that the embargo cost the US approximately 5% of its Gross Domestic Product (GDP) in 1808. A few studies empirically analysed health effects (Allen and Lektzian 2013; Gutmann, Neuenkirch, and Neumeier 2021; Peksen 2011), but they provide cross-country perspectives with little explanation of the health effects in resource-poor landlocked countries.

assessing the health impact of trade stagnation. Our empirical strategy involves province panel analysis, within-mother comparison (i.e. between pregnancies of the same mother) and the Regression Discontinuity Design (RDD) type estimation from the 2016 Nepal Demographic and Health Survey (NDHS) data.

Second, existing studies focus on legally binding, relatively long-term (lasting at least a year or more) trade sanctions in maritime countries, whereas our focus is on unofficial short-term embargo caused by ethnic divisiveness in a landlocked setting. Our findings show that an unofficial embargo can critically impact pregnant mothers and infants even though the blockade was for 134 days. Thus, further actions must be taken for vulnerable populations internationally and domestically to progress towards the SDGs and achieve the principle of "Leaving No One Behind".

Furthermore, this study relates to the literature on the consequences of landlockedness. The relationship between landlockedness and economic growth has traditionally been discussed in cross-sectional growth regressions (e.g. [Sachs and Warner 1997; Collier and Gunning 1999]). Most studies thus focus on macroeconomic consequences such as GDP and trade (e.g. [Levine and Rothman 2006; Frankel and Romer 1999]), with few focusing on other aspects of the vulnerability of landlocked countries. We document the linkage between international trade and health in a landlocked and resource-constrained setting. Our empirical findings benefit attention from policymakers of landlocked developing countries to ensure their health care system while facing cross-border trade disputes.

The remainder of this paper is organised as follows. The following section will briefly overview the Nepal-India border blockade, focusing on health consequences. We outline our empirical strategies in Section 3 and present the findings in Section 4, elucidating possible pathways through which the blockade may have contributed to neonatal mortality. The final section draws a conclusion based on the significant findings.

## THE 2015 NEPAL BLOCKADE AND TRADE STAGNATION

Nepal and India's age-old relationship is founded on cultural, religious, linguistic, and geographic grounds. However, it was only formally recognised in 1950 by the Treaty of Peace and Friendship.[11] India-Nepal relation after the 1950s is often marked by India's involvement in Nepal's political affairs coinciding with some aspects of geopolitics in South Asia (Bhatnagar and Ahmed 2021). India's foreign policy establishment, much to the discontent of Nepal, considers such intervention a valid justification for protecting its interest in the region (Bhatnagar and Ahmed 2021; Tripathi 2019).[12]

India's political interaction with Nepal created discord between the two countries, a few of which resulted in trade and transit restrictions. The 2015 border blockade, the third in the history of the Nepal-India relationship[13], was an unofficial restriction at the Nepal-India border between 23 September 2015 and 3 February 2016. Unlike the previous apparent tension

[11] The treaty authorised special privilege for nationals of both states in terms of residence, property ownership and trade participation. Similarly, the two nations signed the Treaty of Trade and Transit to facilitate their trans-border trade.

[12] See Jayaraman and Shrestha (1976), Subedi (1994), Dabhade and Pant (2004), Sahu (2015), and Nayak (2016) for more on India's geopolitical influence on Nepal.

[13] The first embargo prohibited cross-border transactions in 1969 when Nepal eliminated Indian security checkpoints within its territory. The effect of the blockade is still unclear, given its short duration (24 hours) and the lack of statistics. India's abrupt withdrawal from the Trade and Transit Treaty on 23 March 1989 culminated in a second embargo stemming from bilateral political tensions that had festered since 1975 (Bhatnagar and Ahmed 2021). The blockade lasted for 15 months, dramatically reducing GDP growth from 9.7% to 1.5% between 1988 and 1989.

between the two countries, the 2015 blockade was primarily rooted in Nepal's political instability (Tripathi 2019). Since the first Constitutional Assembly in 2008,[14] Nepal's political environment has been replete with frequent government changes because of rampant competition between various political parties. Due to the partisan strife, Nepal embraced the new constitution just in 2015. Although promulgated after intense political consultation, the new constitution received some objections from a section of ethnic groups in Nepal. The Madhesi, an ethnic group with a solid social and geographical proximity to India, were at the forefront of the dissatisfaction, so much so that it turned into a protest at the Nepal-India trade checkpoints (Tripathi 2019).

The blockade interrupted Nepal's import of its petroleum from India. While Nepal's daily import was around 300 petroleum trucks, it dramatically declined to the occasional passage of a few trucks per day.[15] Figure (1) depicts a significant decrease in petroleum imports from India during the blockade. Total petroleum imports were less than $25 million during the blockade months, while imports were more than $75 million during the same months the previous year and about $50 million the year after the embargo. Petroleum imports from countries other than India show no discernible variations during the blockade and regular months (see Annex 1, Figure [7]).

The restricted flow of imports at the border points also created a shortage of essential medicines and surgical equipment in Nepal. Notably, around 60% of Nepal's pharmaceutical imports are from India. Figure (2) shows that the import of all retail medicine from India declined sharply the during the blockade months compared to the same months of the previous year. In contrast, pharmaceutical imports from countries other than India slightly increased after the blockade (see Annex 1, Figure [8]).

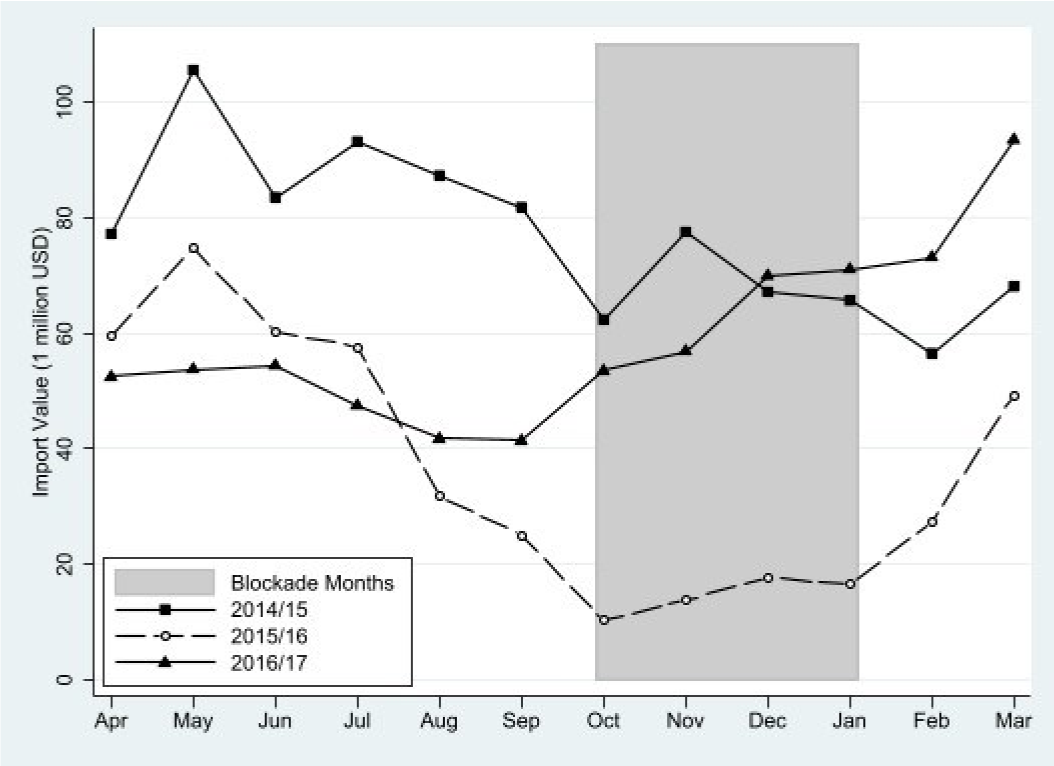
The major disruption across the Nepal-India border increased the prices of food commodities. For example, the Consumer Price Index (CPI) for food and beverages was almost twice as high during the blockade months as it had been the year before and nearly three times as high as it had been during the same months the year after the embargo (Figure [3]). The reduced medical supplies and higher food prices indicate the severe blockade impacts, putting more than three million children at risk of harmful conditions such as hypothermia, respiratory infections, and malnutrition, thereby increasing the chance of death.[16]

[14] The previous two decades in Nepal have seen dramatic shifts in the country's political landscape. The 2006 peace agreement with Maoists concluded a decade of political upheaval, pursuing the need for a new constitution in the post-conflict situation. The Constitutional Assembly was re-elected in 2013 after the first Assembly failed to deliver a new constitution on time. Political parties in Nepal were strongly drawn into ongoing political dispute and power struggle amid the drafting of the constitution (Dixit 2012; Edrisinha 2017).

[15] Indian Oil Corporation (IOC) refuses to provide fuel despite assurances." (Kathmandu Post, October 5, 2015).

[16] "Nepal: Serious shortage of essential supplies threatens millions of children this winter." (UNICEF, November 30, 2015).

*Figure 1: Petroleum imports from India, 2014-2016.*




*Source: Authors' drawing from UN Comtrade database. Note: Imported products depicted in this figure are those with the HS (Harmonized System) code 2709 and 2710 (crude and non-crude petroleum oils, respectively).*

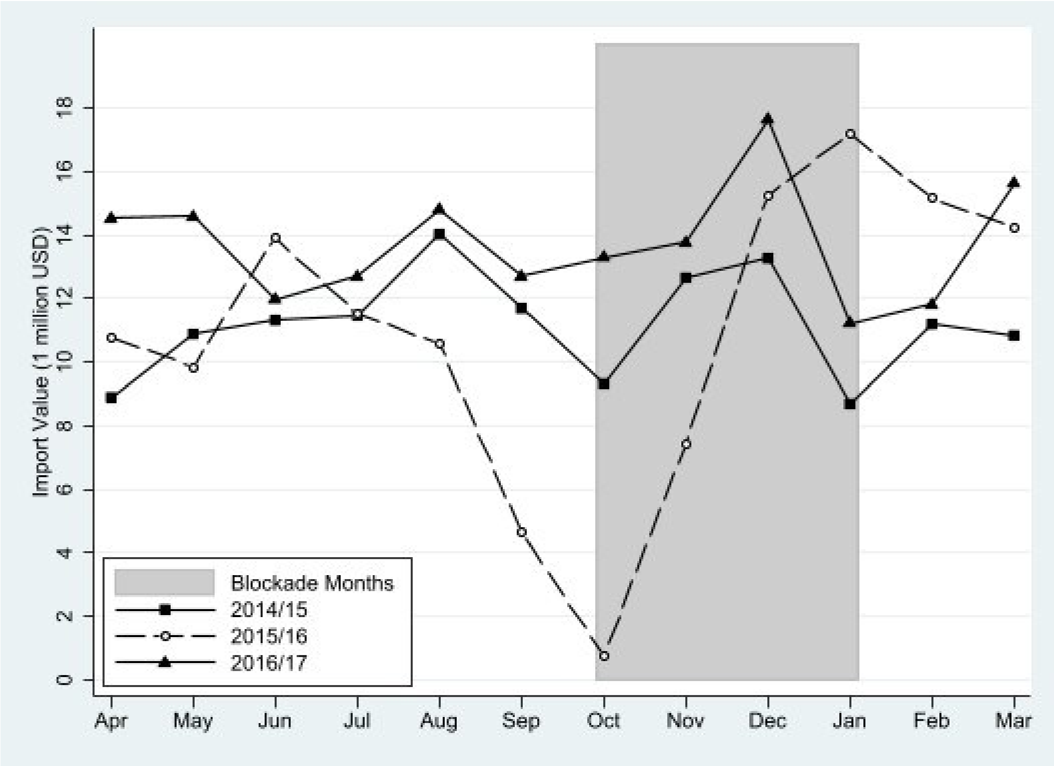
## DATA AND ANALYSIS

The primary data source for this study is the 2016 Nepal Demographic and Health Survey (NDHS). A detailed description of the 2016 NDHS methods is reported elsewhere (MoH, New ERA, and ICF 2017). Briefly, the 2016 NDHS was carried out using a two-stage cluster sampling design. The survey selected 383 census Enumeration Areas (EAs) as Primary Sampling Units (PSUs) in the first stage. In the second stage, a sample of 11,040 households was included from the selected PSUs. From the selected households, 12,862 women aged 15 to 49 participated in an interview to complete a questionnaire designed for women of reproductive age (MoH, New ERA, and ICF 2017).

We utilise the birth and death information to construct the mortality data. NDHS uses a standardised set of questions to ask reproductive-age women about their birth history, including the date of all births, death of any newborn, age at death of each deceased baby and age of women. The survey also asks respondents about their sibling histories, such as the sibling's death and the date of death. While the month and year of the siblings' deaths are available for all ages, the information on the month and year of the children's deaths are only available for those who died under two years old.[17] Therefore, we exclusively focus on child mortality under the age of two.

[17] NDHS imputes mortality age for deaths at two years or older. For example, the age at death is reported as 36 months for children dying between 36 to 47 months.

*Figure 2: Pharmaceuticals imports from India.*

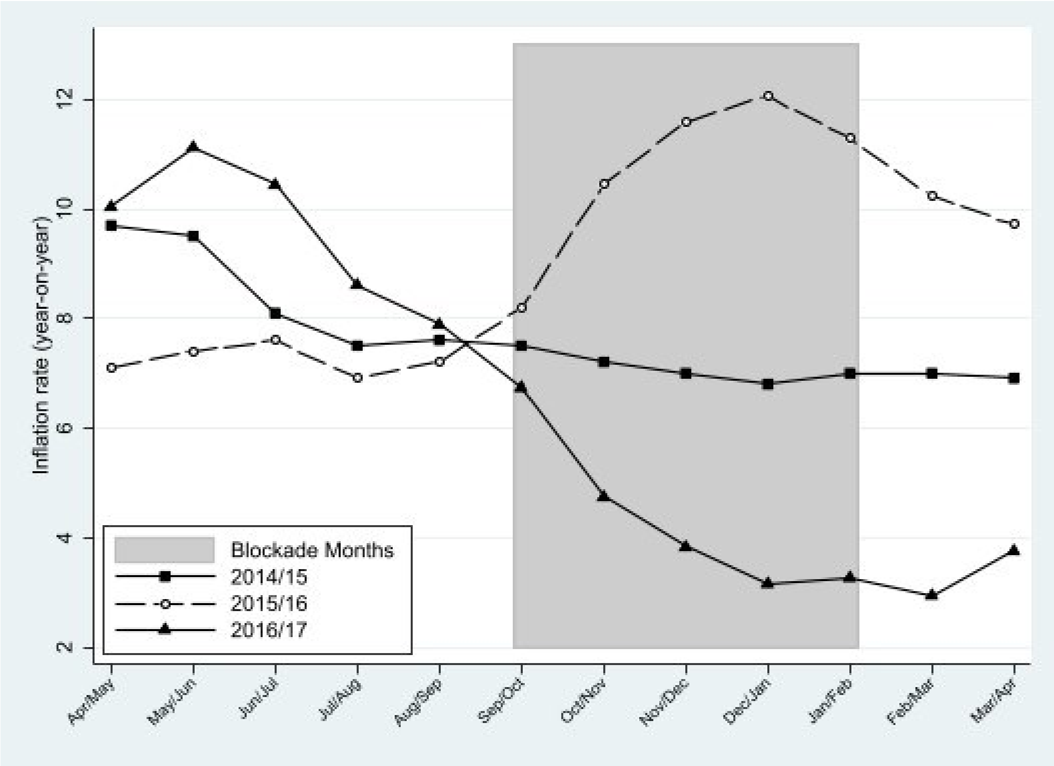



*Source: Authors' drawing from UN Comtrade database. Note: Imported products depicted in this figure are those with the HS (Harmonized System) code 3003 and 3004 (medicaments).*

Although a relatively large-scale survey, the retrospective nature of the NDHS data may be subject to misclassification in the dependent variable. For example, Neal (2012) reports data heaping due to misreporting the timing of death. As a result, the early neonatal deaths may be displaced to the late neonatal, vis-a-vis the post-neonatal death displaced to neonatal deaths. However, it is worth noting that the NDHS questionnaire contains several probes administered by trained enumerators to minimise the measurement error in mortality due to misreporting (Boerma and Sommerfelt 1993; Wang 2003; Neal 2012)[18]. Since we focused on mortality incidence for the last five years preceding the survey, reporting errors in birthday and the date of death should be minimal (Wang 2003). We also assume that the bias is narrow and random to adequately render the estimates, given that the NDHS is only the viable source of child mortality data in Nepal.

[18] For example, the 2016 NDHS questionnaire contains a probe that asks women to ascertain if the baby died later, although born alive. It also repeatedly probes for information such as date of birth and the total number of pregnancies to validate the response (MoH, New ERA, and ICF 2017).

*Figure 3: Inflation rate, Nepal 2014 – 2016.*




*Source: Authors' drawing from Database on the Nepalese Economy, Nepal Rastra Bank (2014-2016).*

We perform a province-level analysis by constructing province panel data on mortality rates by age and area (rural/urban), where province-months are the cross-sectional unit and months are longitudinal. The longitudinal unit spans mid-April 2011 to mid-April 2016, or the first to final months of 2068 and 2072 in the Bikram Sambat calendar.[19] We computed death rates for children aged under 1 and 1 to 2 years from birth records. The mortality ($M^{a}_{imy}$) rate is derived by dividing the number of deaths by the number of live births in the age group ($a$), province ($i$), and month of the year ($my$).[20] We estimate the impact of the blockade on monthly mortality rates based on the following Equation:

$$M_{imy}^{a} = \alpha + \beta^{a} Blockade_{my} + \gamma_{im}^{a} + \gamma_{iy}^{a} + \delta r_{im}^{a} + u_{imy}^{a} \quad (2.1)$$

where $Blockade_{my}$ is an indicator variable that takes unity when the month and year are in the blockade period (from mid-October 2015 to mid-January 2016) and zeroes otherwise, $\gamma^{a}_{im}$ and $\gamma^{a}_{iy}$ are province-month and province-year fixed effects, $\delta r^{a}_{im}$ captures province-month linear

[19] Note that the NDHS only records Bikram Sambat dates. Therefore, the Nepali months do not correspond precisely with the Gregorian calendar. For example, the first Bikram Sambat month is approximately mid-April to mid-May. See chapter four for further details on the Bikram Sambat calendar.

[20] We undertook a similar analysis on adults but found no significant mortality effects (see Table [18] in Annex 1). We do not examine the impact on adult mortality at individual-level data for two reasons. First, adult mortalities captured by the survey are those of the sample mothers' siblings. Due to independent living, they often differ from sample mothers regarding household and residential characteristics.

trends, and $\upsilon^{a}{}_{imy}$ is an unobserved component.[21] We set the blockade period ($Blockade_{my}$) from the first day of Kartik 2072 to the end of Paush 2072, corresponding to 18 October 2015 to 14 January 2016 in the Gregorian calendar.

The second analysis is implemented at the individual level using the birth record of the NDHS dataset. We examine the probability that children born in the last five years die within a month of birth.[22] We observe fewer children for some months while constructing province panel data on infant mortality. For example, the average number of children is 83 in the urban sample and 63 in the rural. Therefore, the monthly mortality rates in the province panel may contain small sample biases. We intend to minimise the biases in the individual-level analysis. It also allows us to control the child's age and sex, the mother's age and education, dummies for a hospital birth and caesarean delivery, and the household wealth index. The baseline specification for analysis takes the following form:

$$D_{ihjt} = \alpha + \ \beta blockade_{t(s)} + \theta X_{ih} + \lambda_{jt} + v_{ihjt} \qquad (2.2)$$

where $D_{ihjt}$ is a binary variable that takes unity if a child $i$ born in household $h$ in district $j$ in period $t$ died within a month of birth and zero otherwise; $blockade_{t(s)}$ is an indicator variable that takes one if children born in period $t$ were exposed to the blockade at developmental stage $s$ (such as neonatal and infant periods); $X_{ih}$ is a vector of individual, birth-related, mother, and household characteristics; $\lambda_{jt}$ captures district-time (month and year) fixed effects; and $\upsilon_{ihjt}$ is an unobserved component.

## EFFECTS ON MORTALITY

### *INFANT MORTALITY*

We begin by examining the impact of the border blockade on infant mortality using province panel data.[23] Table (1) presents the estimated impacts of the blockade on infant mortality (Columns 1 to 3) and children aged 1 to 2 years (Columns 4 to 6). Panel A shows that infant mortality increased by 4.171% in urban areas (Column 1). Since the average monthly mortality rate was 2.151 in the last five years, excluding the blockade period, the mortality rate approximately tripled during the blockade months. Eliminating province-month linear trends changes the coefficient estimates in Columns 2 and 3, but their magnitude increases, implying that unobserved pre-existing health trends do not drive the adverse impact of the blockade on infant mortality in urban areas. The effects are positive but statistically insignificant in rural areas (Panel B). Similarly, we observe no significant effects on mortalities for children aged 1 to 2 years in urban and rural areas.

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[21] We estimate Equation (2.1) without the province-month linear trends ($\delta r^{a}{}_{it}$) as the baseline model, and with the linear trend as the full specification. When conducting the baseline estimation, we implement the panel fixed-effect estimation (though the year-demeaning transformation) since the province-month is the cross-section unit and $\gamma^{a}{}_{im}$ is the incidental parameter. In the estimation of the full specification, we estimate the following first differenced equation by the panel fixed-effects estimation

$$\Delta_{im} M^{a}_{imy} = \Delta_{im} \beta^{a} \text{Blockade}_{my} + \Delta_{im} \lambda^{a}_{iy} + \delta^{a}_{im} + \Delta_{im} \varepsilon_{imy},$$

where $\Delta_{im} z_{imy} = z_{imy} - z_{im,(y-1)}$, and now $\delta^{a}_{im}$ is the incidental parameter.

[22] The individual-level analysis focuses only on neonates due to their vulnerability to blockade exposure. Compared to older children, blockade consequences may be intense for younger children at their earlier stage.

[23] The summary statistics of the main empirical variables are presented in Table (19) and (20) in Annex 1.

*Table 1: Impact of the blockade on child mortality, province panel data.*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **A) Urban** | | | | | | |
| Outcome: child mortality | Under one year | | | 1 to 2 years | | |
| Blockade | 4.171** | 4.939** | 5.318* | 0.323 | 0.000 | 0.331 |
| | (1.525) | (1.799) | (2.214) | (0.170) | (0.000) | (0.250) |
| Province-year fixed effects | Yes | No | Yes | Yes | No | Yes |
| Province-month fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Province-month linear trends | No | Yes | Yes | No | Yes | Yes |
| Observations | 420 | 420 | 420 | 420 | 420 | 420 |
| R-squared | 0.111 | 0.016 | 0.081 | 0.084 | 0.000 | 0.062 |
| **B) Rural** | | | | | | |
| Outcome: child mortality | Under one year | | | 1 to 2 years | | |
| Treatment | 1.126 | 0.171 | 1.377 | 0.938 | 0.673 | 0.823 |
| | (1.443) | (3.051) | (3.375) | (0.780) | (0.674) | (0.987) |
| Province-year fixed effects | Yes | No | Yes | Yes | No | Yes |
| Province-month fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Province-month linear trends | No | Yes | Yes | No | Yes | Yes |
| Observations | 420 | 420 | 420 | 420 | 420 | 420 |
| R-squared | 0.096 | 0.000 | 0.082 | 0.078 | 0.003 | 0.076 |

*Source: Authors' calculation from the 2016 NHDS data using Equation (2.1). Note: Robust standard errors in the parenthesis. Province-month is an observation (i.e. seven provinces x 60 months). Mortality rates are interpreted as the percentage of deaths. The mortality rate is calculated as the number of deaths of children in the age category c in month m divided by the number of children alive in that age category at the beginning of the corresponding month. Regarding neonatal mortality, the denominator is the number of live births at the beginning of the corresponding month. Blockade months correspond to the first day of Kartik 2072 to the end of Paush 2072 in the Bikram Sambat calendar. The baseline results from Equation (2.1), without province-month linear trends, are in Columns 1 and 4. The rest of the Columns show full specification results with linear trends. Significance levels: *** p<0.01, ** p<0.05, * p<0.1*

### *DEATH PROBABILITY WITHIN A MONTH OF BIRTH*

We extend our analysis to neonatal mortality by controlling household, maternal and children's characteristics in Equation (2.2). Table (2) depicts the impact of the blockade on neonatal deaths. We disaggregate neonatal deaths into those that occurred within 15 days of birth and the day of delivery to examine if the length of exposure varies the death probabilities. Results show that neonatal death significantly increased for children born during the blockade. Columns 3 to 6 indicate that neonatal deaths occurred mainly within the first 15 days of life, and about 60% of the increase in neonatal mortality is explainable by the increase in deaths on the day of birth. After adjusting for maternal and household characteristics, point estimates are almost unchanged. The consistency of the findings across extended controls supports the assumption that blockade exposure (treatment status) is unrelated to unobserved individual and household factors.

*Table 2: Impact of the blockade on neonatal mortality, individual-level data.*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Outcome: Death | < 1 month | | within 15 days | | on birthday | |
| Mean (std. dev.) | 0.0156 (0.1239) | | 0.0134 (0.1150) | | 0.0099 (0.0991) | |
| Blockade | 0.052*** | 0.050*** | 0.049** | 0.048** | 0.033** | 0.032** |
| | (0.020) | (0.019) | (0.020) | (0.019) | (0.014) | (0.013) |
| District-month fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| District-year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Mother & HH characteristics | No | Yes | No | Yes | No | Yes |
| Observations | 2,840 | 2,840 | 2,840 | 2,840 | 2,840 | 2,840 |
| R-squared | 0.336 | 0.372 | 0.323 | 0.349 | 0.325 | 0.351 |

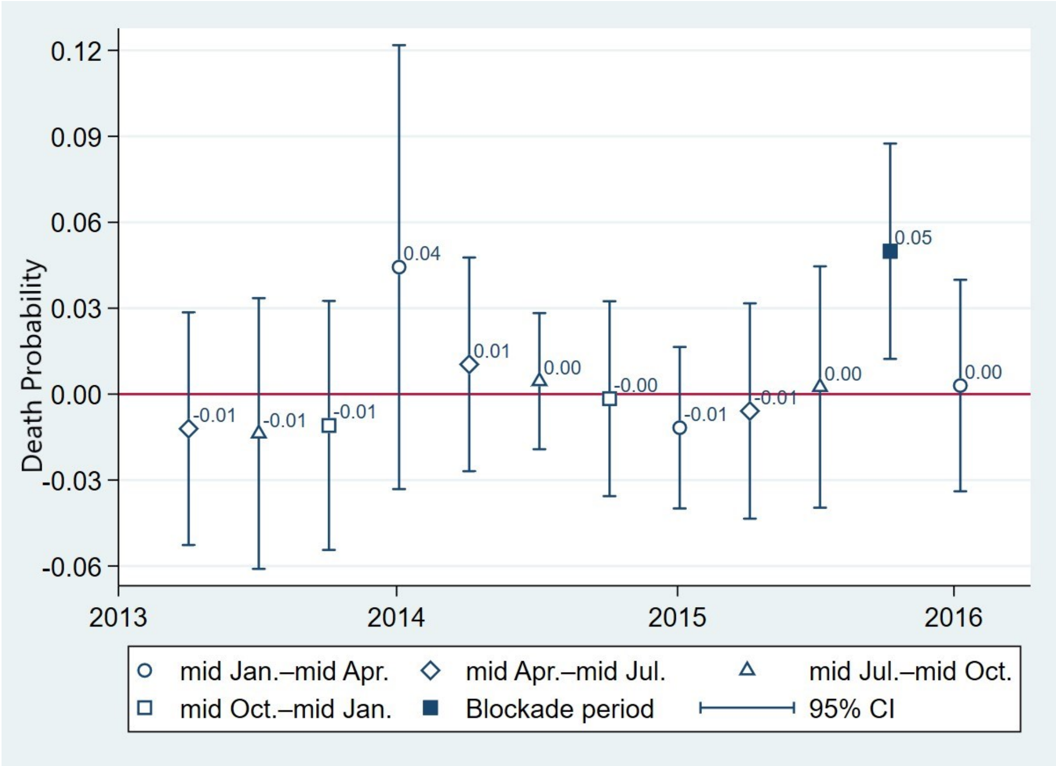
*Source: Authors' calculation from the birth record of 2016 NHDS data using Equation (2.2). Note: Standard errors in the parenthesis are clustered at NDHS primary sampling units. An observation is a child born within five years preceding the survey. Estimates are interpreted as the proportion of deaths. Blockade months correspond to the first day of Kartik 2072 to the end of Paush 2072 in the Bikram Sambat calendar. Coefficients are adjusted for district-month fixed effects, district-year fixed effects, maternal (e.g. age, education, delivery method and place of delivery) and household characteristics (e.g. wealth index). Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$*

## POTENTIAL CONFOUNDING

Our identification strategy is restricted to time series fluctuation in mortality rate, prohibiting us from adjusting year-month fixed effects. We thus estimated Equation (2.2) using placebo dummies for non-blockade months to check for unobserved confounding shocks.[24] In other words, we altered the values of the blockade dummy ($blockade_{t(s)}$) to non-blockade months. The results of this falsification test are shown in Figure (4), which suggests no significant mortality risk for children born during the non-blockade months. Moreover, the death probability for children born during blockade months is statistically different from that of the non-blockade period. These results indicate that non-blockade periods (including the Nepal 2015 earthquake) did not observe any health shocks. If there were unobserved confounders, then the average mortality rate during the blockade months and the non-blockade months would not have been different.

[24] We conducted a similar test on infant mortality rate using province panel data and observed no evidence of previous health trends (see Table [21] in Annex 1). The blockade timing and its influence are thus exogenous to any pre-existing health trends. This assumption is essential for the internal validity of our empirical strategy ($\beta^a$ and $\beta$ in Equations [2.1] and [2.2]).

*Figure 4: Treatment and placebo effects.*




*Source: Authors' drawing from the birth record of 2016 NHDS data using Equation (2.2). Note: The coefficients for placebo dummies are highlighted with circles, diamonds, triangles, and open squares. All estimates are based on the specification (i.e. adjusted for district-month fixed effects, district-year fixed effects, and maternal and household characteristics) outlined in the second column of Table (2). Estimates are interpreted as the proportion of deaths.*

## LOCAL POLYNOMIAL REGRESSION

We used birth record data to illustrate the local polynomial regression lines estimated from the child mortality rates for each month from July 2014 to June 2016 (see Annex 1). Figure (9) in Annex 1 shows a sudden rise in post-blockade neonatal mortality in urban areas, whereas no changes for older children. Similarly, Figure (10) in Annex 1 reveals no discontinuities in death rates for any age group of children. Furthermore, Figures (9) and (10) rule out potential confounding by pre-existing events. For example, stable child mortality rates between the earthquake and blockade (April to September 2015) imply that the earthquake had little or no effect on child mortality.

We conducted a nonparametric regression discontinuity (RD) local polynomial estimation with Mean Square Error (MSE)-optimal bandwidth to verify statistical changes in mortality at the onset of the blockade (Calonico, Cattaneo, and Farrell 2020).[25] A detailed description of RD estimation is outlined in Equation (2.3) in Annex 1. The RD estimation results (Table [3]) show statistically significant changes in neonatal mortality in urban areas (Column 1) but not for older children nor in rural areas (Columns 2 to 6). The differences in neonatal mortality between the rural and urban areas may be because the rural households

[25] We do not employ the RD estimation methods in our primary specification for two reasons. First, seasonal variations in birth and death may confound treatment effects, given that the blockade was short-lived. Second, and more importantly, our purpose is not to estimate the local average treatment effect at the cutoff (at the blockade onset) but the average treatment effect during the blockade.

lacked access to medical care at their baseline. For example, the 2016 NDHS shows that 69% of urban children are born in health facilities compared to 44% of rural children (MoH, New ERA, and ICF 2017). Thus, child mortality rates in rural areas may have been relatively high before the blockade. Another explanation might be that rural households are more likely to grow their food; thus, rising food prices during the blockade may not significantly impact them.

*Table 3: RD effect of the blockade on chid mortality.*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Urban Sample | | | Rural Sample | | |
| Age group | Neonatal (<1 month) | 1 to 11 months | 12 to 23 months | Neonatal (<1 month) | 1 to 11 months | 12 to 23 months |
| RD effect | 0.047** | -0.001 | 0.000 | -0.003 | 0.000 | -0.001 |
| | (0.022) | (0.002) | (0.000) | (0.030) | (0.000) | (0.002) |
| Bandwidth | 10.999 | 9.002 | 18.723 | 7.666 | 11.226 | 11.338 |
| Observations | 2,615 | 28,440 | 30,902 | 1,991 | 21,352 | 23,072 |

*Source: Authors' calculation from the birth record of 2016 NDHS data. Notes: Coefficients are the RD point estimates of mortality changes at the cutoff (onset of the blockade) obtained from a nonparametric RD estimation. Estimations are based on MSE-optimal bandwidth with triangular kernel weights. Standard errors in the parenthesis are clustered at the 2016 NDHS primary sampling unit. Estimates are interpreted as the proportion of deaths. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.*

## POSSIBLE CHANNEL

So far, we have revealed that the blockade adversely affected neonatal mortality. We now turn into a discussion to elucidate possible mechanisms behind the detrimental impact. We begin by clarifying the possible influence on mortality estimates due to access to medical care. As illustrated in Figure (1), the acute gasoline shortage during the blockade may have adversely affected women's access to health facilities. We thus estimate Equation (2.2) employing access to perinatal and intranatal care as the dependent variables. However, we did not analyse the impact on postnatal care to avoid an endogeneity issue. For example, receiving postnatal care may be affected by neonates' health status; the more severe their health, the more likely they will receive postnatal care.

Table (4) shows the impact of in-utero exposure to the blockade on the probability of receiving perinatal care from a doctor (Columns 1 and 2) or any medical professional (Columns 3 and 4). The results indicate that in-utero exposure did not affect the accessibility to prenatal care. Disaggregating the foetal period into trimesters does not change the results (Columns 2 and 4).[26] The estimation result in Column 5 shows that the blockade did not affect the probability of giving birth at a hospital or clinic. We also estimate the blockade's impact on Caesarean births since Caesarean deliveries may decline if petroleum shortages deteriorate medical supplies and healthcare access. However, Column 6 show no evidence that Caesarean deliveries declined during the blockade (Column 6).

[26] Note that the sample size in Columns 1 to 4 is less than that in the primary analysis due to missing responses on whether respondents received prenatal care. If women losing their babies are reluctant to respond to the questions about perinatal care, the point estimates may contain upward bias. We observe a negative correlation (r: 0.143 at 1% statistical significance) between child mortality with reporting access to perinatal care.

*Table 4: Impact of the blockade on access to medical care.*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Prenatal care | | | | Intranatal care | |
| Dependent variables: | Doctor | | Other health workers | | Institutional Delivery | C-section |
| Mean (std. dev.) | 0.511 (0.500) | | 0.945 (0.228) | | 0.656 (0.475) | 0.118 (0.323) |
| Exposed to blockade one month or more | | | | | | |
| Fatal stage | 0.073 | | 0.041 | | | |
| | (0.060) | | (0.044) | | | |
| 1st trimester | | -0.051 | | -0.022 | | |
| | | (0.113) | | (0.039) | | |
| 2nd trimester | | 0.067 | | 0.005 | | |
| | | (0.074) | | (0.039) | | |
| 3rd trimester | | 0.050 | | 0.052 | | |
| | | (0.086) | | (0.054) | | |
| Delivery during blockade | | | | | -0.050 | -0.014 |
| | | | | | (0.070) | (0.046) |
| Observations | 2,323 | 2,323 | 2,323 | 2,323 | 2,840 | 2,840 |
| R-squared | 0.871 | 0.572 | 0.512 | 0.512 | 0.530 | 0.476 |

*Source: Authors' calculation from the birth record of 2016 NHDS data using Equation (2.2). Note: Standard errors in the parenthesis are clustered at NDHS primary sampling units. An observation is a woman pregnant within five years preceding the survey. Estimates are interpreted as the proportion of women receiving prenatal and intranatal care. Blockade months correspond to the first day of Kartik 2072 to the end of Paush 2072 in the Bikram Sambat calendar. All estimates are based on the specification (i.e. adjusted for district-month fixed effects, district-year fixed effects, and maternal and household characteristics) outlined in the second column of Table (2). Significance levels: $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1.*

Next, poor maternal health during the blockade may have contributed to neonatal mortalities. Figure (3) implies that price sore may have deteriorated pregnant women's nutrition. Similarly, the fuel shortage forced households to cook with firewood (Acharya and Adhikari 2021), which may have harmed foetal growth during the blockade. Using birthweight and pregnancy loss as the dependent variables in Equation (2.2), we explore the possible influence on maternal health. If the blockade worsens women's health, neonatal birth weight may decrease, whereas the prevalence of pregnancy loss may increase.

Columns 1 to 3 of Table (5) report the estimated impact on birth weight. The results provide no statistically significant evidence of the impact on low birth weight, implying no adverse health effect of the increased commodity price on pregnant women. However, these findings should be interpreted cautiously due to the missing response on neonatal birth weight in the NDHS data (also see [Singh, Ueranantasun, and Kuning 2017]). It is important to note that our dataset shows a correlation coefficient of 0.105 (at 1% statistical significance) between missing birthweight and neonatal death. The non-random missing response may indicate upwardly biased estimates.

Then, we move on to the relationship between blockade and pregnancy loss. Table (5) shows that the increased neonatal deaths during the blockade were not due to poor maternal health. Unfortunately, the 2016 NDHS does not disclose specific causes for pregnancy loss. Therefore, the estimated effects on pregnancy loss we report here may include spontaneous and elective abortion. Although we need to consider this issue cautiously, the estimation results show no causal linkage between in-utero exposure to the blockade and pregnancy loss, except for exposure during the first trimester (Column 6). The statistically positive impact in the first trimester may reflect that miscarriage is more prevalent in the early stages of pregnancy.

*Table 5: Impact of the blockade on birth weight and pregnancy loss.*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variables: | Log birthweight | | | Pregnancy loss | | |
| Mean (std. dev.) | 7.981 (0.221) | | | 0.247 (0.431) | | |
| Delivery during blockade | 0.034 | | | -0.004 | | |
| | (0.038) | | | (0.053) | | |
| Exposed to blockade one month or more | | | | | | |
| Fatal stage | | -0.020 | | | 0.026 | |
| | | (0.020) | | | (0.040) | |
| 1st trimester | | | 0.041 | | | 0.205*** |
| | | | (0.057) | | | (0.060) |
| 2nd trimester | | | -0.029 | | | 0.013 |
| | | | (0.037) | | | (0.067) |
| 3rd trimester | | | -0.028 | | | -0.070 |
| | | | (0.040) | | | (0.051) |
| Observations | 2,031 | 2,031 | 2,031 | 3,773 | 3,773 | 3,773 |
| R-squared | 0.496 | 0.496 | 0.497 | 0.460 | 0.460 | 0.465 |

*Source: Authors' calculation from the birth record of 2016 NHDS data using Equation (2.2). Note: Standard errors in the parenthesis are clustered at NDHS primary sampling units. An observation is a woman pregnant within five years preceding the survey. Estimates are interpreted as the proportion of pregnancy loss in Columns 4 to 6. Blockade months correspond to the first day of Kartik 2072 to the end of Paush 2072 in the Bikram Sambat calendar. Pregnancy loss includes abortion and stillbirths. All estimates are based on the specification (i.e. adjusted for district-month fixed effects, district-year fixed effects, and maternal and household characteristics) outlined in the second column of Table (2). Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

Finally, we examine the influence of mother-specific unobserved characteristics by including mother-fixed effects. Table (6) shows that controlling mother-specific unobserved heterogeneity by fixed effects eliminates the blockade's impact, implying that mothers with a particular characteristic tend to lose their newborns when exposed to the blockade (Column 1). We include interaction terms between the blockade dummy and mother/household characteristics in Equation (2.2) to illustrate the determinants of neonatal deaths. More specifically, we employed a dummy for the first and second quintiles of the 2016 NDHS wealth index (Column 2), a dummy for mothers with no formal education (Column 3), and a dummy for mothers delivered at risky maternal ages (Columns 4 and 5).

The results show that women in the first and second wealth quantiles and those without formal education had no significant impact on neonatal mortality due to the blockade (Columns 2 and 3). On the other hand, neonatal deaths were associated with mothers giving birth at a risky age during the blockade.[27] The point estimate in Column 4 suggests that mothers delivering at risky maternal age during the blockade are 34% (i.e. 38.2 – 4.2) more likely to lose their children than those delivering at non-risky maternal age at regular periods. Furthermore, the blockade had a statistically significant negative effect on neonatal mortality when the mother was older than the threshold ages (Column 5).

While pregnancies at risky maternal ages are associated with preterm birth or lower

[27] Note that the total number of women giving birth at risky maternal ages during the blockade was 50. Therefore, we cannot make a bold claim about the result in Columns 4 and 5. It might be possible that these mothers had medical difficulties in childbearing.

birthweight (Fall et al. 2015), we reinvestigate the impact on birthweight with the exact specifications in Columns 4 and 5. The estimated impacts of the blockade dummy and interaction terms are negative after controlling mother-fixed effects (Columns 6 and 7). Although the coefficients on interaction terms are not statistically significant, we observe a significant difference when comparing deliveries at risky maternal ages during the blockade and non-blockade periods (Column 7). The null hypothesis that the sum of two coefficients ($\beta_1 + \beta_2 = 0$) equals zero is rejected at the 10% significance level. If the mother's age was two years apart from the threshold ages, newborns of mothers who delivered at risky ages during the blockade tend to have about 23.7% ( i.e. $(yN - yR)/yN = 1 - \exp[\ln(yR/yN)] = 1 - \exp[(-0.048) + 2 \times (-0.111)]$) less birth weight than those of mothers who delivered at typical ages in non-blockade periods. Thus, our results imply that the increased neonate mortality during the blockade period was attributed to deliveries at risky maternal ages. Furthermore, medical treatments for neonates with lower birth weights may have been insufficient due to a lack of petroleum and medicine supplies.

*Table 6: Further checks on the blockade's impact on neonatal deaths.*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dependent variables: | Died < 1 month | | | | | Log birthweight | |
| Mean (std. dev.) | 0.016 (0.124) | | | | | 7.981 (0.221) | |
| Blockade ($\beta_1$) | 0.027 | -0.014 | 0.025 | -0.042 | -0.046 | -0.059 | -0.048 |
| | (0.054) | (0.036) | (0.067) | (0.042) | (0.042) | (0.058) | (0.057) |
| Interaction term ($\beta_2$): Blockade X | | | | | | | |
| Poor dummy (1st-2nd wealth quantile) | | 0.093 | | | | | |
| | | (0.098) | | | | | |
| No formal education | | | 0.008 | | | | |
| | | | (0.077) | | | | |
| Delivery at risky maternal age (<19 & >35) | | | | 0.382$^{*}$ | | -0.182 | |
| | | | | (0.204) | | (0.196) | |
| Deviation from the threshold age of high-risk delivery | | | | | 0.179$^{**}$ | | -0.111 |
| | | | | | (0.086) | | (0.081) |
| F-test: Ho: $\beta_1+\beta_2=0$ | - | 0.631 | 0.307 | 2.998$^{*}$ | 3.737$^{*}$ | 1.633 | 2.814$^{*}$ |
| Observations | 2,840 | 2,840 | 2,840 | 2,840 | 2,840 | 2,031 | 2,031 |
| R-squared | 0.147 | 0.149 | 0.147 | 0.172 | 0.180 | 0.179 | 0.183 |

*Source: Authors' calculation from the birth record of 2016 NHDS data using Equation (2.2). Note: Standard errors in the parenthesis are clustered at NDHS primary sampling units. An observation is a woman pregnant within five years preceding the survey. Estimates are interpreted as the proportion of deaths in Columns 1 to 5. Blockade months correspond to the first day of Kartik 2072 to the end of Paush 2072 in the Bikram Sambat calendar. Coefficients are adjusted for birth year and birth month fixed effects and mother fixed effects. Estimates are based on the primary specification outlined in Equation (2.2) with interaction terms between the blockade dummy and mother/household characteristics. Poor dummy indicates mothers belonging to the first and second wealth quantiles of the 2016 NDHS wealth index. Risky maternal ages involve mothers younger than 19 and older than 35. The threshold ages for high-risk delivery are 19 and 35, respectively. Significance levels: $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1.*

## FUTURE DRECTION

When a landlocked developing country is politically unstable, its coastal neighbours seek to reclaim political and economic dominance over the landlocked nation. The 2015 Nepal-India blockade was an example of a natural political experiment demonstrating how a landlocked country is vulnerable to the vagaries of its primary trading partner. The blockade, followed by fuel and essential supplies shortages, profoundly impacted Nepal. In this paper, we examine the child health impact of the blockade with a focus on child mortality.

Although short-lived, the blockade had an immediate impact on child mortality. Infant deaths rose significantly during the blockade months compared to the non-blockade months. The probability of death within the first 30 days of birth was significantly higher for children born during the blockade months. Our estimation represents a lower bound because of the ongoing countermeasure to protect mother and child health.[28] For instance, the child-level analysis with mother-fixed effects helps us presume that the mortality rate could be more significant when considering at-risk mothers and children.

These findings caution the Madhesi leaders and India's foreign policy establishment, contemplating blockade as a policy tool to accomplish their political interests. As explained in the second section, Madhesi leaders and India, discretely in the background, deemed the blockade to pressure Nepal's mainstream political parties into changing the constitutional provisions affecting Madhesi's identity and representation. However, the blockade was counterproductive for the health and well-being of children. Children exposed to the blockade had higher death probabilities during the neonatal period. It implies that the Madhesi leaders are incentivised to negotiate politically with mainstream political parties to resolve their grievances. Our study also suggests that landlocked developing countries pay attention to protecting their healthcare system while facing cross-border trade disruptions. Landlocked countries must utilise diplomatic channels with their coastal neighbours to maintain humanitarian supplies.

We cast a dim light on the possible causal pathway between blockade and infant mortality. Our results generally hint at the immediate effects on high-risk mothers exposed to the blockade during their first trimester. However, early life exposure to health shocks can influence subsequent human capital formation.[29] Future research may thus focus on the long-term impacts of trade stagnation on human capital formation such as long-term effects on educational attainment.

♦♦♦

[28] For example (Khatri et al. 2016), essential newborn care, treatment of neonatal sepsis, and inpatient care for low-birth-weight babies were some ongoing interventions from the Government of Nepal during the blockade year.

[29] A substantial body of literature has documented the profound influence of early-life environment and health conditions on socioeconomic outcomes in later life (Bleakley 2010; Currie and Vogl 2013). Furthermore, some studies have demonstrated that improving child health may lead to the better human capital formation (see, for instance, [Case, Fertig, and Paxson 2005; Bleakley and Lange 2009; Ito and Tanaka 2018]).

*"Fire made us human, fossil fuels made us modern, but now we need a new fire that makes us safe, secure, healthy and durable."*

~ Amory Bloch Lovins
Author, Reinventing Fire

# CHAPTER 3 ENVIRONMENT AND HEALTH

## OUTDOOR AIR POLLUTION AND BIRTHWEIGHT: IMPACT OF THE 2015 NEPAL-INDIA BORDER BLOCKADE

Outdoor air pollution is a major environmental risk factor for poor health. Around 4.1 million deaths globally are due to outdoor air pollution, and around 70% of the deaths occur in low- and middle-income countries (GBD Collaborators 2020). Air pollution effects are severe for women and children. For example, perinatal air pollution exposure is associated with low birth weight, which may profoundly impact health education and earnings into adulthood (Oreopoulos et al. 2008; Currie 2011). These negative health consequences applaud the consensus for air pollution measures targeting women and children in developing nations and the urgency of assessing the policy impacts.

Previous studies have taken advantage of natural experiments to examine if environmental policies reduced pollution levels and whether better air quality concurrently benefited child health. For example, Japan's 1992 automobile nitrogen-dioxide legislation reduced foetal mortality by 3.5% while improving air quality in the country's regulated areas (Inoue et al. 2020). Many studies demonstrated comparable gains in child health due to environmental measures such as clean air regulations in the USA (e.g. [DeCicca and Malak 2020; Yang and Chou 2018]) and Asian games initiatives in China (Liu et al. 2022).

Besides environmental policies, special events may reduce pollution levels and improve the health outcomes of those exposed to the event. For instance, strikes at French oil refineries resulted in 75 grams rise in the average birthweight of babies born to mothers exposed to the strike during their first and second trimesters owing to disrupted petroleum supplies (Lavaine and Neidell 2017). Similar improvements in air quality and health outcomes were demonstrated during the COVID-19 lockdown, where human mobility was restricted (Liu, Wang, and Zhang 2021; Perera et al. 2021).

Analogous to this strand of literature, we utilised the Nepal-India border blockade as a natural political experiment to assess the child health effects of improved air quality.[30] Amid the promulgation of Nepal's new constitution in 2015, political demonstrations at the border halted cross-border trade flows, resulting in a nearly four-month-long petroleum scarcity in Nepal (Grocke 2016; Tripathi 2019). Assuming that the blockade exogenously improved air quality, we estimate the health impact by comparing the average birthweight during the blockade to the birthweight after the blockade within the framework of regression discontinuity design (RDD). We then compare the birth outcomes of neonates born around the blockade abolition months (i.e. Jan-Feb 2016) to those born in the same months in the adjacent years, adjusting for births in adjacent months.

While this is not the first quasi-experimental investigation of the effect of improved air quality on human health, it is distinctive in numerous ways. In contrast with the causal effects of environmental policies (Yang and Chou 2018; DeCicca and Malak 2020; Inoue et al. 2020; Liu et al. 2022), we attempted to extend the evidence by utilising a rare political event as a natural experiment. More specifically, we focused on air pollution and child health impacts of an unofficial blockade[31] in a landlocked country. Although our estimates are less conclusive of

[30] Other strand of literature have assessed negative health externalities of sudden increases in air pollution due to accidents, disaster or anthropogenic exogenous events (Beland and Oloomi 2019; Hill 2018; Rangel and Vogl 2019).

[31] Note: Nepal-India border blockade is an exceptional political circumstance arising from political instability in Nepal. The blockade is distinct from other international trade sanctions because it is a non-legally binding bilateral sanction informally imposed against a landlocked country. See chapter 2

the air pollution effects, they offer us caution while relying on administrative birth data and satellite-based pollution data in a data-poor setting. Given that the evidence on exposure to ambient air pollution during different stages of pregnancy is not uniform (e.g. [Li et al. 2020; Gong et al. 2022]), we reveal a negative association between birthweight and in-utero exposure during the first trimester. The results may provide some insights into the possible interactions between political instability, air pollution and timing of in-utero exposure.

Next, this is one of the few empirical analyses to examine outdoor air pollution and health effects from a developing country's perspective. For example, previous studies from south Asia mostly used household fuel use as a proxy for indoor air pollution to assess impacts on birthweight (Khan et al. 2017; Patel and Chauhan 2020). These studies often measure birth weight based on maternal recall of children's birth size, leading to recall bias, measurement errors and misclassification. Instead, using a cross-sectional household survey, these studies assessed the association between fuel use and birth weight. We attempted to examine the causal effect of improved pollution levels from the blockade using longitudinal data from a tertiary maternity hospital. Contrary to cross-sectional surveys using proxy measures, we used more reliable sources of information for birthweight and pollution measures. The dataset consisted of 72191 births from May 2014 to May 2018, allowing us to compare the birthweight of babies born during the blockade with those born after and the blockade year births with adjacent years.

We begin by providing relevant background on the Nepal-India border blockade and the potential air quality changes in the next section. We then describe our estimation strategies and present the results. We conclude this chapter by reviewing the possible caveats to our estimation strategies and offer directions for future research.

## NEPAL-INDIA BORDER BLOCKADE AND THE AIR QUALITY

In 2015, an unofficial embargo occurred at the Nepal-India border because of ethnic movement in Nepal after Nepal's new constitution was promulgated. A detailed elaboration of the process leading to the blockade was reported in the second chapter. Briefly, the blockade was due to political demonstrations at the Nepal-India borders by the Madhesi ethnic group in response to their dissatisfaction with certain aspects (e.g. citizenship rights and state restructuring) of the new constitution (Grocke 2016; Tripathi 2019).

The blockade lasted between 23 September 2015 and 3 February 2016, disrupting petroleum supplies in Nepal. Approximately five to ten fuel trucks were allowed to cross the border during the blockade days, while the regular supplies were at around 300 fuel trucks per day (Grocke 2016). Petroleum scarcity was noticeably observed in the local market, given that Nepal was almost entirely dependent on India for its petroleum imports. For example, a gasoline station[32] in Kathmandu sold around 250 kiloliters of petroleum during the blockade months (see Figure [5]). However, its monthly sales were around 500 kiloliters during the same months the previous year and 750 kiloliters the year following the blockade. The acute shortage of petroleum affected essential supplies, including mobility and transport.

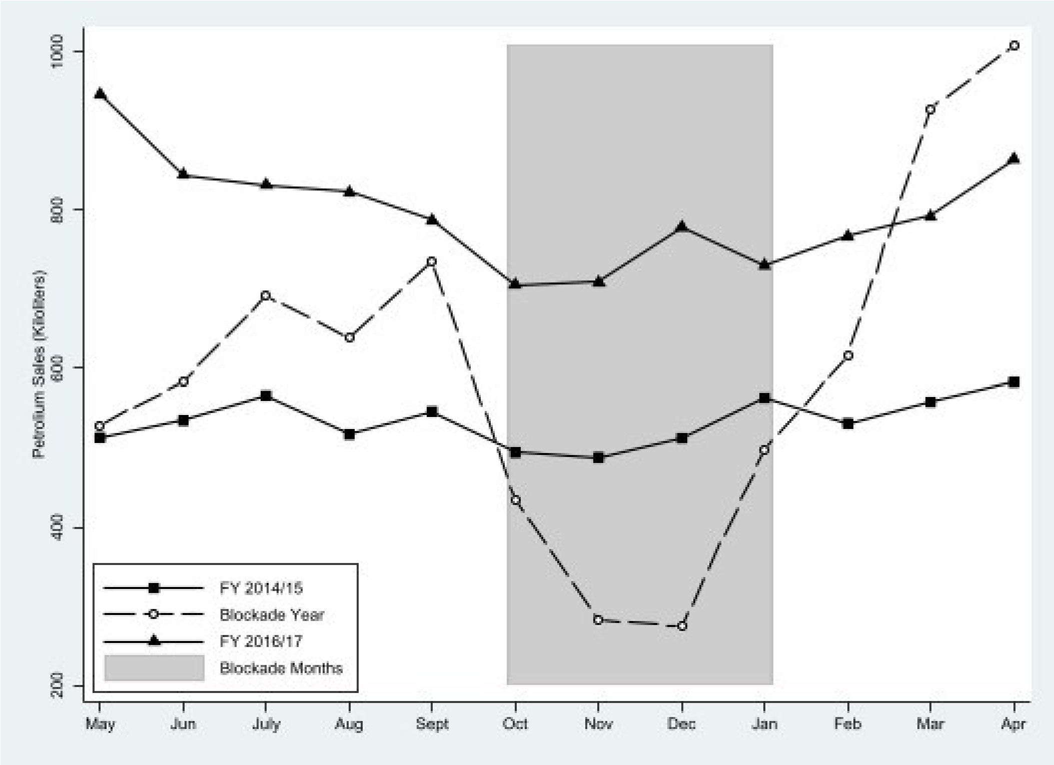
Previous studies have demonstrated a strong positive correlation between human mobility and anthropogenic emissions. For example, a recent analysis of global air quality changes during the Corona Virus Disease (COVID)-19 pandemic revealed around 30 to 60% reductions in the ambient concentration of vehicular pollutants during the lockdown (Sokhi et al. 2021). Analogous to the previous studies (Liu, Wang, and Zhang 2021; Perera et al. 2021;

for further elaborations on the nature of the 2015 Nepal-India border blockade.

[32] Monthly sales data was available from a co-op gasoline station (Sahja Petrol Pump) located in the Kathmandu City. It is one of the largest and most popular petrol station in the Kathmandu Valley.

Sokhi et al. 2021), we assume improved air quality[33] in Nepal during the blockade, given the disrupted petroleum supplies from India. Based on this implicit assumption, we utilise the border blockade as a natural political experiment to study the effect on neonatal birth weight.

*Figure 5: Monthly petroleum sales in Kathmandu, 2014/15 to 2016/17.*




*Source: Authors' drawing from monthly petroleum sales data from Sajha Petrol Pump, Pulchok, Nepal. Note: monthly sales include petrol and diesel from May 2014 to April 2017.*

## STATISTICAL ANALYSIS

The data are from the restricted access birth register of Paropakar Maternity and Women's Hospital (PMWH) in Kathmandu. Individual-level birth registers are poorly maintained and mostly paper-based in Nepal. Thus, we collected the data from the paper-based register of the PMWH, covering the period between May 2014 to May 2018, consisting of 72919 birth records. Eight trained enumerators collected data using the Open Data Kit (ODK) under the supervision of field supervisors and hospital staff. Our choice of hospital data is contingent on longitudinal data with mother and child characteristics that allow us to compare the child's health status between blockade and non-blockade days. Having considered the hospital data, we also intended to minimise issues such as measurement errors and recall biases that may arise from population surveys.

The hospital record contains children's birth data and health status. Hospital nurses record the birth weight and APGAR score of children immediately after the delivery. Similarly, the record also provides maternal characteristics such as the mother's age, place of residence, expected due dates of delivery and delivery types. We utilised this information to estimate the

[33] Figure (11) n Annex 2 show monthly averaged pollutant levels in Nepal from May 2014 to 2018. Note that the ground-level air quality data are unavailable for Nepal until 2018. We used raster data from satellite images to demonstrate air quality trends before, during and after the blockade. The raster data consisted of time-averaged (i.e. monthly) and area-averaged (i.e. Nepal as a whole) values.

effect of the blockade on child health status. We supplemented the longitudinal birth data with other day-level covariates and matched them with the neonate's birth dates. For example, we collected daily Optical Aerosol Depth (AOD) and other meteorological data from the National Aeronautics and Space Administration (NASA) Earth Data. Table (22) in Annex 2 summarises descriptive statistics of major variables used in the analysis.

We estimate the blockade effect on birth weight by comparing births before and after the abolition of the blockade. Health outcomes such as birth weight may not change immediately after exogenous shocks, but they have a long-term influence owing to sustained exposure. For example, children born a day before and after the onset of the blockade may not be different in terms of birth weight, but the effect may only arise after a certain period of in-utero exposure. As a result, the birth weight may not be discontinuous at the beginning of the blockade. We thus focused on local effects at the abolition cutoff date (3 February 2016) instead of the onset cutoff date (23 September 2015).[34] We used various time windows around the cutoff, including 30 days, 60 days, 90 days, 120 days, and 134 days. The econometric model is similar to Borra, González, and Sevilla (2019) and indicated as follows:

$$bwt_{id} = \alpha + \beta blockade_d + \theta f(d) + \gamma_{dow} + \delta X_{id} + \epsilon_{id} \; \forall d \in (d_0 - w^*, d_0 + w^*) \qquad (3.1)$$

where $bwt_{id}$ is the birth weight of a neonate $i$ born on day $d$; $\beta blckade_d$ is the treatment dummy for children born on blockade days. It takes the unity for neonates born before the abolition of the blockade and zeroes otherwise. $\theta f(d)$ represents linear time trend; $\gamma_{dow}$ is the day of week fixed effects; $\delta X_{id}$ is the vector of control variables such as maternal and child health characteristics; $\varepsilon_{id}$ is an unobserved component, and $w^*$ is the time windows around the cutoff. The parameter of interest is the beta coefficient that captures the local average treatment effect of abolishing blockade on neonatal health. If the blockade caused temporary changes in the air quality, we expect the beta coefficient to be greater than zero ($\beta>0$).

During the blockade, the shortage of essential medical supplies may stretch the health services, whereas the surge in deliveries after the abolition may result in overcrowding and decreased quality of obstetric care. In either of the cases, a considerable number of births may be scheduled earlier or later than the due dates, which may cause poor neonatal health outcomes. We estimate a flexibly specified regression of birth timing in Equation (3.2) to check the possible birth scheduling. Equation (3.2) lets us detect missing or excess births around the cutoff if the blockade shifts the births. The econometric model in Equation (3.2) is similar to that of Huang, Zhang, and Zhao (2020) and is represented as follows:

$$births_{dm} = \alpha + \beta_1 Bockade_{dm} + \beta_2 After_{dm} + \gamma_{dow} + \delta X_{id} + \epsilon_d \qquad (3.2)$$

where $births_{dm}$ is the number of births on day $d$ of blockade months $m$; $\beta_1 Blockade_{dm}$ is a binary indicator for births occurring on the last 30 days of the blockade, and $\beta_2 After_{dm}$ is the binary indicator for births occurring on the first 30 of the blockade abolition. The other terms in the Equation are similar to those indicated in the first. The analytic sample in Equation (3.2) consists of all births from 60 to 60 days after the cutoff. So, the December 2015 births are the reference group for $\beta_1 Blockade_{dm}$ and the March 2016 births for the $\beta_2 After_{dm}$. If the blockade displaced the births, we expect the sum of coefficients across the cutoff to be zero ($\beta_1 + \beta_2 = 0$).

As elaborated in the second section, the blockade lasted for a few months (around four months). The naive direct comparison of health outcomes just above and below the cutoff in Equation (3.1) may be biased due to the potential influence of seasonal variations in birthweight. We reexamined the effects on neonatal birthweights utilising an econometric model like a

[34] We checked preliminary RD effects at blockade initiation cutoff using non-parametric RD specification. See Table (23) in Annex 2.

difference-in-difference (DID) method. The econometric model is based on Schulkind and Shapiro (2014) as well as Borra, González, and Sevilla (2019) as follow

$$bwt_{iy} = \alpha + \beta_1 JanFeb_{iy} + \beta_2 JabFeb^{16}{}_{iy} + \phi_{ym} + \gamma_{dow} + \delta X_{iy} + \epsilon_{iy} \quad (3.3)$$

where $bwt_{iy}$ is the birth weight of newborn $i$ born on year $y$, $\beta_1 JanFeb_{iy}$ is the binary indicator for all births occurring in January and February. $\beta_2 JanFeb^{16}{}_{iy}$ is the variable of interest, which takes unity if the births occur on months around the blockade abolition cutoff (i.e., from January 2016 to the end of February 2016) and zeros otherwise. We included December 2015 and March 2016 births, the two adjacent month pairs, as a comparison group. $\phi_{ym}$ is the year-month fixed effects, which nets out any unobserved time trend and seasonal effects that may correlate with the birth weight and birth rates, $\gamma_{dow}$ is the day-of-week fixed effects of isolating weekly patterns in birth rates, $\delta X_{iy}$ is the vector of control variables and $\varepsilon_{iy}$ is the unobserved component. Therefore, $\beta_2$ captures before-after differences (i.e., blockade and after blockade births) in birthweight compared to the birthweight of babies born in the same months of the non-blockade year. The beta coefficient ($\beta_2$) is adjusted for linear trend by including a pair of adjacent months (December and March) similar to January and February. We thus assume that the unobserved characteristics, such as seasonality, are less likely to confound the observed differences in birthweight between January-February 2016 and other adjacent months.

## IMPACTS ON CHILD HEALTH

Table (7) estimates the impact of the blockade on birthweight. We report the estimation result for 134 days (the entire blockade period) before and after the cutoff based on Equation (3.1). We narrowed the windows from 134 to 120 days, 90 days, 60 days, and 30 days. The table suggests lower birth weight for children born before the cutoff, but the results are only statistically significant for children born 30 days and 60 days before the cutoff than those born after the cutoff. Children born 60 days before the cutoff are 73.487 grams lower in their birth weight (Column 4), whereas 76.663 grams lower for children born 30 days before the cutoff (Column 5). The table indicates lower birth weight for children as they approach the cutoff, thus suggesting that children exposed to the blockade for a more extended period tend to have poor health outcomes.

### *EFFECT OF THE BLOCKADE ON BIRTH TIMING*

We report the estimation results for Equations (3.1) and (3.2) in Table (8) to understand the effect of the blockade on birth timing. Column 1 reports the result from Equation (3.1) with no statistically significant excess or fewer births during the last 30 days of the blockade. In Column 2, we report the result from Equation (3.2), where we flexibly specified the regression of birth timing. The result suggests a birth deficit during the blockade, whereas a surplus after the blockade. However, the coefficients are not statistically significant. Aggregating the two coefficients generate approximately 1.612 missing births for the last 30 days of the blockade. The difference is not significantly distinguished from zero (F-test), suggesting no possible birth scheduling at the cutoff.

*Table 7: Effect of the blockade on birthweight.*

| | (1)<br>-/+ 134 Days | (2)<br>-/+ 120 Days | (3)<br>-/+90 Days | (4)<br>-/+ 60 Days | (5)<br>-/+ 30 Days |
|---|---|---|---|---|---|
| Mean Birthweight | 2,917.063 | 2,915.148 | 2,916.700 | 2,912.816 | 2,912.033 |
| Std. dev. | (526.198) | (2,915.148) | (521.066) | (525.621) | (507.213) |
| Blockade | -42.363 | -34.247 | -37.500 | -73.487** | -76.663** |
| | (29.434) | (29.755) | (31.013) | (34.409) | (33.782) |
| Adj. R-squared | 0.205 | 0.210 | 0.201 | 0.191 | 0.170 |
| Observations | 11,099 | 9,959 | 7,580 | 4,932 | 2,440 |

*Source: Authors' calculations from Equation (3.1). Note: Standard errors are clustered by birthdates and shown in the parenthesis. An observation is a newborn. The blockade ended on 3 February 2016 (cutoff). The variable of interest is whether a baby was born before the cutoff. Each column represents the estimation results based on samples around 30 days, 60 days, 90 days, 120 days, and 134 days from the cutoff. Coefficients are adjusted for year-months, day-of-the-week fixed effects and other covariates. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$*

*Table 8: Effect of the blockade on birth timing.*

| | (1)<br>Total Births | (2)<br>Total Births |
|---|---|---|
| Mean | 46.875 | 47.474 |
| Std. dev. | (6.753) | (6.247) |
| Blockade | 0.876 | - |
| | (3.119) | |
| Before Blockade | - | -0.617 |
| | | (2.254) |
| After Blockade | - | 2.229 |
| | | (1.866) |
| Adj. R-squared | 0.333 | 0.234 |
| F test $H_o$: $\beta_{Before} = \beta_{After}$ | - | 2.310 |
| Observations | 2,449 | 4,955 |

*Source: Authors' calculations from Equation (3.1) and Equation (3.2). Note: Standard errors are clustered by birthdates and shown in the parenthesis. An observation is a newborn. The blockade ended on 3 February 2016 (cutoff). Coefficients are adjusted for year-months, day-of-the-week fixed effects and other covariates. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.*

*EFFECTS ON CHILD HEALTH OUTCOMES*

Table (9) presents the estimation results from Equation (3.3). In Column 1, we find no significant effect of the blockade on birth weight, although the neonates born during the blockades were 0.867 grams lower in their birth weight. The proportion of children born with low birth weight is not statistically significant either. Columns 3 and 4 reveal results for the APGAR score; again, we do not the significant results. These results are consistent with those reported in Table (8), suggesting no birth scheduling during the blockade and, thus, no significant impacts on child health.

*Table 9: Effect of the blockade on child death.*

| | (1) Birthweight | (2) Low Birthweight | (3) APGAR | (4) Low APGAR |
|---|---|---|---|---|
| Mean | 2,936.007 | 0.057 | 7.592 | 0.125 |
| Std. dev. | (515.430) | (0.233) | (1.206) | (0.331) |
| Jan-Feb 2016 Births | -0.867 | -0.003 | 0.025 | -0.002 |
| | (17.718) | (0.012) | (0.030) | (0.007) |
| Adj. R-squared | 0.166 | 0.103 | 0.578 | 0.236 |
| Observations | 15,260 | 15,260 | 15,319 | 15,319 |

*Source: Authors' calculations from Equation (3.3). Note: Standard errors are clustered by birthdates and shown in the parenthesis. An observation is a newborn. The blockade ended on 3 February 2016 (cutoff). The variables of interest are the dummy for 30 days before the cutoff and a dummy for 30 days after the cutoff. Coefficients are adjusted for year-months, day-of-the-week fixed effects, and other covariates Columns 2 and 4 represent the proportion. Low birth weight <2500 grams and low APGAR <7. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

### *Heterogenous effects*

The preceding section demonstrated that the blockade had no significant impact on birth timing and, thus, no impact on child health outcomes. We now look at whether birth weight differs by maternal and child health characteristics for births during the end of the blockade (January and February 2016) compared with births in the same months of non-blockade years. We thus estimated Equation (3.3) for heterogeneous effects. We interact dummies for various mother and child characteristics with the indicator variable ($JanFeb^{16}_{iy}$) in Equation (3.3). The reduced form specification takes the following form.

$$birthweight = MCH1 + MCH2 + MCH1 * JanFeb^{16} + MC2 * JanFeb^{16} \quad (3.4)$$

Here, the outcomes are the birth weight. *MCH1* and *MCH2* denote maternal and child characteristics. For example, *MCH1* and *MCH2* correspond to male and female births as child demographic characteristics. They also reflect adult and younger mothers as a maternal demographic characteristic. We remove intercept, subscripts, constant and other features from Equation (3.3) for simplicity. The beta coefficients on interaction terms represent each group's blockade effects on birth weight. We should reject the null hypothesis that the coefficients are identical across the two groups if the blockade had a differential impact on mother-child characteristics.

The regression result from Columns 1 to 3 reveals no systematic differences in birth weight by maternal age, place of residence and neonates' gender. Column 4 shows a lower birth weight of 6.676 grams for caesarean births, with F statistics suggesting systematic differences in birth weight between normal and caesarean births. It indicates that women needing emergency obstetric care during the blockade may have undergone caesarean delivery leading to lower birth weight.

In columns 4 and 5, we report the heterogeneous effects of exposure to the blockade and the pregnancy status. While the results are not significant for the months of exposure, we observed a significantly lower birth weight of 37.098 grams for women experiencing blockade in their first trimester. The F statistics also suggest that the effect is systematically different from those observed among women experiencing blockade in their third trimester. The finding indicates a lack of weight gain among women exposed to the blockade during their first trimester. As suggested in the medical literature, failure to gain maternal weight during the first trimester may have resulted in lower birthweight babies.

*Table 10: Heterogenous effects of the blockade on birthweight*

| | (1)<br>Mother's Age | (2)<br>Place of Residence | (3)<br>Child's Gender | (4)<br>Delivery Types | (5)<br>Blockade Exposure | (6)<br>Pregnancy Stages |
|---|---|---|---|---|---|---|
| Birthweight (Jan-Feb 2016 Births) | | | | | | |
| X Adult | -0.848<br>(1.468) | | | | | |
| X Risky age | -1.160<br>(6.954) | | | | | |
| X Kathmandu | | 2.450<br>(2.155) | | | | |
| X Outside | | -1.976<br>(1.488) | | | | |
| X Male | | | -1.037<br>(2.026) | | | |
| X Female | | | -0.816<br>(1.628) | | | |
| X Normal | | | | 2.471<br>(1.554) | | |
| X Caesarean | | | | -6.676$^{***}$<br>(1.819) | | |
| X < 4 Months | | | | | -2.020<br>(1.536) | |
| X > 4 Months | | | | | -1.584<br>(1.512) | |
| X First Trimester | | | | | | -37.098$^{**}$<br>(17.875) |
| X Third Trimester | | | | | | -1.092<br>(1.562) |
| Adj. R-squared | 0.179 | 0.179 | 0.179 | 0.179 | 0.179 | 0.179 |
| F test (Ho: $\beta_3=\beta_4$) | 0.002 | 4.956$^{**}$ | 0.002 | 15.03$^{***}$ | 0.572 | 3.931$^{**}$ |
| Observations | 29,594 | 29,594 | 29,594 | 29,594 | 29,594 | 29,594 |

*Source: Authors' calculations from Equation (3.4). Note: Standard errors are clustered by birthdates and shown in the parenthesis. An observation is a newborn. The variables of interest are the interaction terms from Equation (3.4). Coefficients are adjusted for year-months, day-of-the-week fixed effects and other covariates. Significance levels: $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1*

## DISCUSSION AND CONCLUSION

We examined the effect of the 2015 Nepal-India border blockade on birthweight using longitudinal birth records from a national maternity hospital in Nepal. We observed an adverse effect of the blockade on birth weight and child health outcomes. The estimated effects are contrary to the existing studies that examined the causal effects of better air quality on neonatal health outcomes (Lavaine and Neidell 2017; Yang and Chou 2018; DeCicca and Malak 2020; Inoue et al. 2020; Liu et al. 2022). Our study hinges on the expectation that children born during blockade months have better health outcomes due to better air quality during the blockade. However, the estimated effects are rather adverse. The results were sensitive to the choice of comparison window in the main specification using the RDD and insignificant when estimated with Equation (3.3). We discuss the results concerning some of the limitations of our study in this section.[35]

The blockade resulted in an acute petroleum shortage, but the widespread use of polluting fuel for outdoor cooking may have kept pollution levels stable. Figure (5) in indicates a sharp decline in petroleum supplies while the average pollution levels remained steady over the blockade months (Figure [11] in Annex 2). The blockade-induced supply chain disruption pushed households to adjust their energy consumption behaviour. As a result, many urban households resorted to polluting fuels like firewood and coal for cooking (Acharya and Adhikari 2021). Usually, the firewood cooking was outdoors, with many households sharing the open space to set up their fires (Grocke 2016).[36] Anecdotal evidence also indicates persistent traffic congestion in Kathmandu Valley a month into the blockade owing to the illicit petroleum market and rampant supplies (Rinck and Adhakari 2016).

Next, the satellite-based AOD data is a proxy to measure changes in air quality. While satellite data is readily accessible and has adequate spatial coverage, it is prone to measurement errors. Satellite data may be biased due to potential errors such as the misclassification of clouds as haze. The other concern is that missing data due to low signal and invalid pixels may limit the use of AOD data for spatiotemporal analysis (Wei et al. 2020; Zhang et al. 2021). Similarly, seasonality in particulate pollution, such as agricultural residue burning during October and November in Nepal and north India, may have confounded the actual pollution levels during the blockade. Thus, AOD data may not be a reliable measure of pollution unless supplemented with air pollution data from atmospheric monitoring stations (Shaw and Gorai 2020). The lack of air quality data from atmospheric monitoring stations limited us to relying on satellite-generated AOD data. The actual air quality changes would have been evident if the atmospheric monitoring data were available.

Similarly, we observed no apparent discontinuity in birth weight before and after the blockade (Figure [12] in Annex 2). The consistent birthweight across the cutoff may be due to data heaping. Although hospital records provide rich information on maternal and child health status, data heaping is ubiquitous for birthweight. A multi-country study of hospital birth records from low-income countries suggests significant data heaping in birthweight around 2500 and 3000 grams and birthweight rounding to the nearest 100 grams (Kong et al. 2021). We found a similar tendency in our dataset, rounding the birthweights to the decimal place of zeros or five. Around 23% of children in our dataset were reported as having their birthweight strictly at 2500 grams and 3000 grams. As reported in previous studies (Day et al. 2020; Kong et al. 2021), the inconsistent use of digital and analogue weighing scales may have further biased the birthweight data.

Our empirical strategy also suffered several critical limitations while using birthdate as a running variable in the RDD. First, the blockade may not immediately affect the air quality and the neonatal birth weight. Even if the blockade truly affected air quality and birthweight, the outcome is not discontinuous at the cutoff if the effect is not instantaneous (Lee and Lemieux 2010). We focused on the abolition cutoff to mitigate the extended impact of the blockade, but the results are less conclusive. Next, the endogenous variables (e.g., AOD and birthweight) follow the time-series properties of the data-generating process;[37] as a result, the canonical RDD assumptions are often violated (Hausman and Rapson 2017). We estimated the

[35] Rich (2017) provides a comprehensive review of the challenges of utilizing natural experiments while assessing the health benefits of lower pollution levels.

[36] Despite restricted human mobility, ambient particulate matter ($PM_{2.5}$) concentration remained higher during the COVID-19 lockdown owing to secondary aerosol emissions from domestic and agricultural biomass burning (Sokhi et al. 2021).

[37] For example, average birthweight follows seasonality due to temporal variations in climatic conditions, pollution levels, labour supplies production, and food availability (Chodick et al. 2009).

treatment effects by comparing the average birthweight of children born just above and below the cutoff within a small comparison window, assuming everything else (observables and unobservables being equal) except the treatment.[38] However, the unobservable may be correlated with the running variable due to the time-series nature of the endogenous variables. The time-series endogenous variables may also generate time-varying and dynamic treatment effects (Hausman and Rapson 2017). We attempted to elucidate some of these issues in Equation (3.3) by comparing the average outcome in the blockade months with the same months of non-blockade years. The estimated effects, however, showed no specific impact on child health outcomes.

Furthermore, blockades may have affected birthweight through channels other than air pollution. The petroleum shortage was so acute and noticeable that households may have changed their behaviour, which could confound the effect on birthweight. For example, the blockade may have changed the frequency of perinatal check-ups, access to health services, changes in income, alteration of dietary habits and stress[39]. However, the hospital data does not provide us with all the details on the socioeconomic aspects of women to rule out these channels more comprehensively.

These limitations highlighted that the estimated effects were biased and less conclusive due to methodological issues such as data heaping and time-series properties of the data. Similarly, we could not demonstrate the changes in air quality during the blockade. Further research is thus needed to examine the effect of better air quality on neonatal health outcomes. Similar studies may exploit the COVID-19 lockdown as a natural experiment to investigate the causal impact on neonatal health outcomes due to temporary changes in air quality during the lockdown period. The Government of Nepal started monitoring air quality through atmospheric stations in different parts of the country in 2018. So, future studies may benefit from combining AOD data with the air quality data available from atmospheric monitoring stations to accurately quantify the air quality changes and the concurrent health effects. Studies may also consider multiple hospitals, at least in Kathmandu valley, to ensure external validity.

♦♦♦

[38] In our RDD setup, blockade abolition date (3 February 2016) is the cutoff, and a child's birthday relative to the cutoff (time) is the score. Blockade days are the treated units and days after the cutoff are the comparison units.

[39] See Duncan, Mansour, and Rees (2017) for an exceptional example of how in-utero exposure to an exogenous event may trigger health behaviours detrimental to neonatal health outcomes. The authors revealed that low birth weight was associated with emotions that encouraged women to increase alcohol and tobacco use during a sporting event.

*"Our goddess of the moon is gifted with magic, with power over the dead. She could banish the dreams, if she wished. She did not."*

~ Madeleine Miller
Writer, The Song of Achilles

# CHAPTER 4 CULTURE AND HEALTH

## DOES CULTURAL BELIEFS AFFECT BIRTH TIMING AND CHILD HEALTH?

Apart from medical reasons, parental awareness of socio-economic aetiology influences the birth decision and thus their choice of health services. Parents may decide to deliver their baby earlier or later than the expected date in response to various economic enticements. For example, women in China choose caesarean birth in August rather than September to fulfil the school entrance age so that their kids may attend school and join the labour market at a younger age (Huang, Zhang, and Zhao 2020). Many studies conclusively demonstrate similar responses in various instances, including tax benefits in the US (Dickert-Conlin and Chandra 1999; Schulkind and Shapiro 2014), parental leave benefits in Germany (Neugart and Ohlsson 2013), and child benefits in other similar countries (Borra, González, and Sevilla 2016, 2019; Gans and Leigh 2009; Jürges 2017; Tamm 2013).

Cultural and superstitious beliefs may also motivate parents to schedule their childbirth early. For instance, in Taiwan, the lunar month of July has much lower birth rates because of the superstition surrounding the Ghost Month (Halla, Liu, and Liu 2019), whereas much higher caesarean deliveries in China during dragon Zodiac year due to beliefs associated with the auspiciousness of the dragon year (Huang et al. 2021). Studies from developed countries show similar birth patterns due to their beliefs around auspicious and inauspicious dates (Antipov and Pokryshevskaya 2020; Gans and Leigh 2012; Levy, Chung, and Slade 2011).

Previously published studies nevertheless represent a comparable cultural setting. Cultural beliefs regarding birthing decisions demonstrated in the existing studies may not necessarily hold on to another cultural context. For instance, early birth scheduling due to rare cultural events such as the dragon year or the leap year does not commonly persist in South Asian countries. Thus, cultural relativism asserts that no culture possesses absolute standards for assessing the behaviours of another culture, but every culture should apply such judgment to its members and their activities (Hofstede, Hofstede, and Minkov 2010). Grounded on this notion of cultural relativism, we intend to provide evidence for a new cultural context. Our specific focus is on Nepal, a South Asian country.

Although there is a rich literature on the influence of culture on pregnancy and childbirth, studies from Nepal mainly took a qualitative approach to describe the culture-health nexus (e.g. [Paudel et al. 2018; Sharma et al. 2016]). Studies employing a quantitative approach are limited to analysing the association between sociocultural determinants and health behaviours, such as health care use (e.g. [Acharya and Paudel 2021]). However, no previous studies empirically examined whether and how sociocultural factors such as cultural beliefs influence birthing decisions and the choice of obstetric services in Nepal. Moreover, the current studies are primarily from cross-sectional household surveys with insufficient information on longitudinal variations in birth rates to quantify such effects.

This study examined the causal effect of cultural beliefs on childbirth decisions. In so doing, we utilise beliefs around the auspiciousness of certain lunar days, especially the full moon. Selecting an auspicious date for important life events is a common practice in Nepal. This practice may also exist for preferring specific days for births. Anecdotal evidence suggests that women may even ask for a caesarean birth to ensure auspicious days for their delivery (Acharya and Paudel 2021). For example, a hospital-based study reports that 19.4% of the caesarean births in a tertiary care hospital were on maternal request for date-of-birth selection (Shrestha, Saha, and Mahato 2021). Although no previous studies confirmed this behaviour, pregnant women may seek guidance from astrologers or fortune-tellers about the best delivery times (Sharma 2016). Women may also refer to the Vikram Sambat Calendar (also known as *patro*)[40], which offers a general guideline on auspicious and inauspicious days based on the lunar cycle and lunar days. Usually, the waxing phase of the moon and the full moon day is considered auspicious (henceforth the full moon day belief). We briefly elaborate on the Bikram Sambat Calendar and the cultural belief in the next section. Pregnant women may try to give births on or before the full moon day instead of days after the full moon.

Using daily birth data from May 2014 to April 2018, we employed a Regression Discontinuity Design (RDD) to confirm our prediction of intentional shifting of birthdates around the full moon days. We collected information from 72919 birth records from a tertiary maternity hospital in Kathmandu. The birth record included exact birth data and other covariates to compare the daily variations in birth counts relative to full moon days.

This paper extends the existing studies on the effects of birth timing and health outcomes due to sociocultural beliefs. Ours is one of the first empirical studies investigating how parental willingness to prefer certain lunar days, associated with cultural beliefs and symbolism of lunar phases, is reflected in birth statistics from Nepal. Based on a setting that birth dates relative to full moon days closely resemble randomised assignment with binary treatment (if a child was born before or on the full moon day), we examine the short-term effects on birth timing. We then analyse the health consequences of timing manipulation. We show that the cultural belief associated with the auspiciousness of the full moon caused significantly higher caesarean births on or before the full moon day and fewer births after the full moon. We also reveal that the weight of babies born on or before the full moon days is significantly lower for high-risk pregnancies and significantly higher for low-risk pregnancies.

The overall construction of this paper is organised into five parts, including this introductory section. The following section briefly elaborates on the Bikram Sambat Calendar and the full moon day belief. The third section elaborates on the dataset with a detailed description of our empirical approach. Section four presents the results regarding the effect on birth timing, health consequences and the subsample analysis of low-risk and high-risk births. The last section is a discussion and conclusion with direction for future research.

## THE FULL MOON DAY BELIEF

Auspiciousness is a widely held cultural belief in Nepal. One aspect of auspiciousness in Nepali culture is related to the planetary positions at an individual's birth. People believe that the day and hours on which they are born determine their overall wellbeing, happiness, and fate in life (Gray 1979). The routine chores of Nepali people thus often involve finding an auspicious period for important life events such as marriage, birth, rituals, business ventures and building a house (Gray 2011).[41] The practice is usually based on the Bikram Sambat calendar that defines the lunar cycle and gives guidelines for evaluating the planetary effect on people's daily lives.

Bikram Sambat defines lunar months and days using a regular cycle of the moon's phases. It usually contains 12 lunar months in a year, and each month represents a period between two full moon days, distributed into 30 lunar days known as *tithis* (Sanford 2013). A lunar day is a time it takes the moon to extend its longitude by 12 degrees above the Earth's longitude. So, a lunar day lasts between 20 to 27 hours, each day having irregular hours.[42] The

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[40] Bikram Sambat, the ancient Hindu calendar, has been the official calendar of Nepal since 1901. It runs 56.7 years ahead of the Gregorian calendar. Bikram Sambat new year begins in mid-April and extends to the next mid-April.

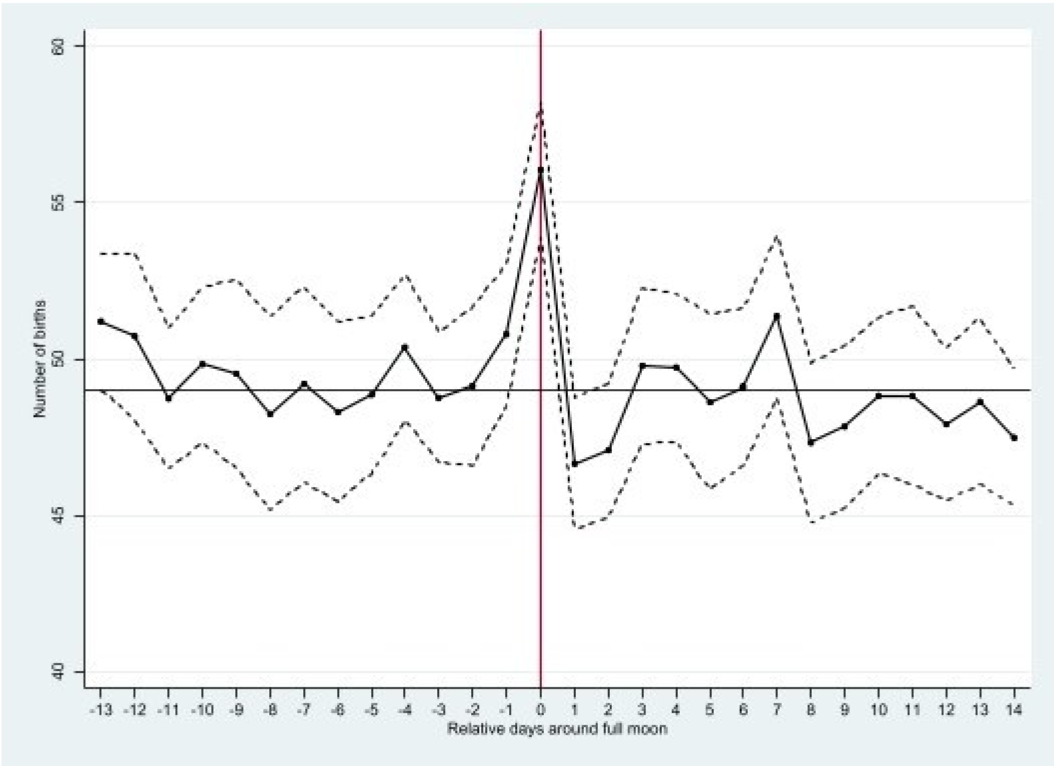
[41] "Astrologers hold sway over Nepal's political leaders." (France 24, December 5, 2017).

[42] Bikram Sambat adjusts tropical solar days and sidereal years to create a civic solar calendar. It involves the deletion or repetition of lunar days according on intricate Vedic criteria, and the inclusion of a leap lunar month every 2.7 years.

lunar days are grouped into two fortnights known as *paksha*. The first 15 days of a lunar month is a dark fortnight (*Krishna paksha*), representing the moon's waning phase. The second half of the lunar month is the waxing phase of the moon (known as *Sukla paksha*), during which the moon becomes brighter and ends in the full moon day.

The lunar months and lunar days are utilised for determining auspicious dates for rituals. Generally, the waxing phase of the lunar month and the full moon days is considered auspicious (Sanford 2013). During the waxing phase of the moon, the light falling from the moon is believed to bring good luck, happiness and contribute to health and wellbeing. Many ethnic and religious groups share similar inherent thoughts. For example, most Hindu festivals and important Buddhist rituals fall on the full moon day. Similarly, Nepal's national flag bears the emblems of a crescent moon, which is symbolised as peace and prosperity (Bordeleau 2014; Kızılçaoğlu 2014). The inherent beliefs on the moon and the lunar days are also corroborated in Figure (6), which shows excess births around the last few days of the waxing phase of the full moon and fewer births just after the full moon days. We utilise the social beliefs about the auspiciousness of full moon days to study its effect on birth timing.

*Figure 6: Distribution of births by days relative to full moon days.*




*Source: Authors' drawing using the data file from the birth record of the maternity hospital (2014-2018). Note: Relative days are the child's birth dates from the full moon days as indicated in the Vikram Sambat calendar.*

## DATA AND STATISTICAL ANALYSIS

Our primary data source is the restricted access 2014-2018 Confinement Book of the Paropakar Maternity and Women's Hospital (henceforth Maternity hospital).[43] The paper-based confinement book provides information on all births in the hospital during this period, as recorded by the hospital nurse during delivery. These data include maternal and neonatal health information, including birth date and time. Table (24) in Annex 3 summarises the information included in the confinement book. We retrieved 72919 birth records from 29th April 2014 to 14th May 2018 and created a unique dataset for further analysis. The confinement book was tailored to an electronic form using the Open Data Kit (ODK) tool.[44] Trained enumerators collected data on Android devices through the ODK App.

We consider the tertiary hospital due to the quality of longitudinal data. The study sample covers four years of data, which allows us to compare the birth rates across forty-eight lunar cycles. We also intended to minimise biases and measurement errors such as deliberately faking birth dates due to recall biases and ensuring that births occurring before and after the full moon days are correctly identified for comparison. For example, our estimation depends on the preciseness of birth dates in the dataset. Manipulation of birth dates in the confinement book might be a source of bias, although this is less likely. After the delivery, nursing staff immediately records the exact birthdate in the register, later verified by a head nurse and approved by an obstetrician before birth certificates are issued. Furthermore, biases might arise while retrieving the data electronically.

Similarly, we customised the ODK with data validation rules to avoid entry errors. Eight enumerators were provided with data entry manuals and trained using the ODK. Two field supervisors concurrently performed data quality assessments during the data entry phase. With these quality control measures, we assume almost negligible errors in the birth dates. Daily data on other covariates were obtained from various sources such as average daily temperature, rainfall and extreme weather events from the Government of Nepal, Department of Meteorology and Hydrology and political event data from the Armed Conflict Location and Data (ACLED) (Raleigh et al. 2010). Table (25) provides summary statistics of the major variables used in the study.

Our empirical strategy is based on quantitative causal inference analysis, which compares observed results with the counterfactual. In other words, we intend to estimate what would have happened to the full moon births (a.k.a. observed results) if these births were not full moon births (a.k.a. counterfactual), holding everything else constant. The fundamental problem of causal inference analysis is to estimate the counterfactual because birth can either be on auspicious lunar days or regular days but not on both days at once. We used the regression discontinuity design (RDD) to estimate the counterfactual and compare it with the full moon births.

In RDD, all units get a score, and those scoring higher than a predetermined cutoff receive treatment, while units below the cutoff are the control group. This treatment assignment rule compares units above and below the cutoff to assess causal treatment effects (Cattaneo, Titiunik, and Vazquez-Bare 2020). In our context, a day is an observation, full moon days are the cutoff, and a child's birthday is the score relative to the full moon day.

RDD assumes a small comparison window around the cutoff in which the treatment

[43] PMWH is the only tertiary level public hospital for specialized obstetrics services in Nepal. Around 15000 women from all parts of Nepal give birth to their babies in this hospital annually (see http://www.pmwh.gov.np).

[44] ODK is an off-grid data collection and management tool widely used in household survey, clinical trials and disease surveillance (Tom-Aba et al. 2015).

and comparison units are similar on average in observed and unobserved characteristics, except that some units are auspicious lunar days whereas others are regular days. Then, the effects of full moon day beliefs are estimated by comparing units above and below the cutoff within the comparison window. The RDD estimates allow us to infer that any differences in the birth count before and after the full moon are due to the auspiciousness of full moon days holding everything else (e.g. seasonality, maternal characteristics, and other unobservable) constant on average.

We collapsed the 72919 birth records into daily births count corresponding to 1477 days. We ascribed the lunar calendar with the child's birth date based on the review of past years calendars. We assume that all births occurring on or before the cutoff are due to the full moon day belief, but those after the cutoff are not. We examine the effect of full moon day belief by estimating the variations in daily birth counts around the full moon days. The statistical model closely follows Huang, Zhang, and Zhao (2020), with the following functional form:

$$Births_{dt} = \alpha + \beta BeforeFM_d + \phi_{ym} + \gamma_d + \theta_{ld} + \delta X_{dt} + \varepsilon_{dt} \quad (4.1)$$

where $Births_{dt}$ is number (or log number) of births on calendar day $d$ and year $t$, $BeforeFM_d$ is the binary indicator taking the value of one if the birthday is on or before the full moon days, $\phi_{ym}$ is a set of dummy variables for each month and year, $\gamma_d$ is dummy variables for each day of the week, $\theta_{ld}$ is dummy variables for each lunar day, $\delta X_{dt}$ is a vector of control variables[45], and $\varepsilon_{dt}$ is the error term. Here, full moon day is the cutoff, so $BeforeFM_d$ in Equation (4.1) takes unity for births occurring on or before the full moon days. It measures additional full moon births relative to days after the full moon. We expect a positive coefficient ($BeforeFM_d>0$) if women strategically shift their birth and bring delivery forward. In our dataset, women give births around 2.76 days earlier than their expected delivery date. Therefore, we normatively select the three-day window across the cutoff, assuming that the birth can be shifted around three days. We vary the window length from three days to six days to check the sensitivity of the coefficients. The primary specification in Equation (4.1) contains a linear trend of birthdays across the full moon day cutoff as standard regression discontinuity specification.[46]

We estimate a birth timing model with a more flexible specification. The sample contains all births from plus six days to minus five days of the cutoff. We replaced the binary indicator in Equation (4.1) with dummies for the last six days of the full moon and the first six days after the full moon. The model takes the following form:

$$Births_{dt} = \alpha + \beta_1 Before_d + \beta_2 After_d + \phi_{ym} + \gamma_d + \theta_{ld} + \delta X_{dt} + \varepsilon_{dt} \quad (4.2)$$

$Before_d$ equals one for births occurring on the last three days of the full moons and zeroes otherwise. Similarly, $After_d$ equals one for births occurring on the first three days after the full moons and zero otherwise. The reference group are minus five days to minus three days of the full moon and four days to six days after the full moon. If the full moon day affects women who would otherwise give birth in the first three days after the full moon but shifted their births

[45] Daily average temperature, rainfall, extreme weather event, holiday violent political events, earthquake and blockade are the control variables. We used the ACLED definition (Raleigh et al. 2010) to create a dummy variable for politically violent events. The earthquake dummy takes unity for days between 25 April 2015 and 12 May 2015, and the blockade dummy for the blockade period.

[46] We checked linear, quadratic, and cubic trends of birthdays across the cutoff; however, the estimated results are almost similar. We thus included linear trends and excluded these higher-order polynomials throughout the paper. We included first-order lag of birth counts to account for unobservable such as service arrangements and staffing.

to the last three days of the full moon, beta coefficients immediately across the threshold would add up to zero.

## BIRTH TIMING AND DELIVERY TYPES

Table (11) estimates the impact of full moon day cutoff on birth and delivery types. We report estimation results for two samples, widening the windows from 3 days to 6 days before and after the full moon day cutoff based on Equation (4.1). We expect most birth scheduling to occur immediately around the full moon days. The table suggests around 11 (20.4%)[47] and 12 (24.2%) additional daily births in the last three days and six days of full moon days. We find no significant changes in the normal births during three days of full moon day cutoff, whereas six (19.7%) additional normal births during six days of the cutoff. We observed seven (37.3%) extra caesarean births three days before the full moon day. The coefficient reduces to five (27.4%) additional caesarean births six days before the full moon day. Table (11) provides some indication that the full moon day cutoff changes delivery types.[48] For example, caesarean births account for more than two-thirds of the total additional births in three days of full moon day cutoff, but it accounts for less than half of the total additional births in the six-day window. Figure (13) in the Annex 3 shows the additional daily births on or before the full moon days.

*Table 11: Effect of full moon day belief on deaths and delivery types.*

| | Total Births | | Normal Births | | Caesarean Births | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | +/- 3 Days | +/- 6 Days | +/- 3 Days | +/- 6 Days | +/- 3 Days | +/- 6 Days |
| Before full moon | 10.763*** | 12.420*** | 3.265 | 6.619** | 7.497*** | 5.801*** |
| | (2.684) | (2.684) | (2.514) | (3.181) | (0.925) | (1.730) |
| Daily mean | 49.91 | 49.58 | 31.34 | 31.68 | 18.57 | 17.90 |
| R-squared | 0.433 | 0.386 | 0.352 | 0.296 | 0.842 | 0.646 |
| Observations | 288 | 576 | 288 | 576 | 288 | 576 |

*Source: Authors' calculation from Equation (4.1). Notes: Robust standard errors in parentheses. An observation is a day. Full moon days are the cutoff. Columns 1, 3 and 5 represent the comparison window of three days before and after the full moon days cutoff, whereas Columns 2, 4 and 6 represent a comparison window of six days. Coefficients are adjusted for year-month, day of the week, lunar days dummies and other covariates. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

## DYNAMICS OF BIRTH TIMING

We report estimation results for Equation (4.2) in Table (12) to understand the dynamics of birth timing. The first Column suggests a surplus of 2.067 (3.8%) births in the last three days of full moon day and a deficit of 2.264 (4.5%) births in the three days after the full moon. Coefficients in Column two suggest a small deficit in normal births above the cutoff and a

[47] The percentage reported in the parentheses is obtained when the log number of births is used in Equation (4.1) and Equation (4.2). Logging the dependent variable would allow us to interpret the result as β∗100% change (increase/decrease) in births with the explanatory variable. We omit the log numbers from Tables (4-1) and (4-2) to minimise numerical confusion.

[48] Note that we cannot entirely separate birth timing due to changes in delivery methods (birth switching) or delivery timing alone (birth shifting). The overall effect of full moon day cut-off on the prevalence of cesarean births is a proxy to compare if the full moon day increased the overall cesarean sections and decreased the normal births.

small surplus below the cutoff. However, results in the first two columns are not statistically significant to rule out if the coefficients immediately across the threshold would add up to zero. Column 3 suggests a 3.051 (13.7%) surplus of cesarean births in the last three days and a deficit of 2.724 (16.6%) births in the three days after the full moon. Aggregating the two coefficients generates approximately 0.327 (2.9%) missing caesarean births for the last three days of the full moon, but this difference is not statistically different from zero. It displays a clear shifting trend in caesarean births. Due to the full moon day belief, women who would otherwise have given births in the first three days after the full moon might have undergone caesarean delivery to shift the birth before the threshold.

*Table 12: Dynamic of birth timing due to the full moon day belief.*

| | (1)<br>Total Births | (2)<br>Normal Births | (3)<br>Caesarean Births |
|---|---|---|---|
| -2 to 0 days of full moon | 2.067 | -0.983 | 3.051*** |
| | (2.808) | (2.583) | (0.627) |
| +1 to +3 days of full moon | -2.264 | 0.460 | -2.724*** |
| | (2.473) | (2.434) | (0.767) |
| R-squared | 0.388 | 0.297 | 0.658 |
| F test Ho: $\beta_1 = \beta_2$ | 1.35 | 0.17 | 35.51*** |
| Observations | 576 | 576 | 576 |

*Source: Authors' calculation from Equation (4.2). Note: Robust standard errors in parentheses. An observation is a day. Full moon days are the cutoff. Coefficients are adjusted for year-month, day of the week, lunar days dummies and other covariates. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

## MOTHER AND CHILD CHARACTERISTICS

The preceding section demonstrated that the full moon day belief resulted in a significant number of births being shifted from three days after the full moon to the last three days of the full moon, with caesarean births accounting for most of the manipulation. We now look at if maternal and child characteristics differ across days within the vicinity of the threshold. To make the comparison more accessible, we estimate Equation (4.1) for heterogeneous effects. We interact dummies for various mother and child characteristics with the indicator variable. The reduced form specification takes the following form:

$$\log(births) = \beta_1 mch1 + \beta_2 mch2 + \beta_3 mch1 * BeforeFM + \beta_4 mch2 * BeforeFM \quad (4.3)$$

where the outcomes are the natural log number of births, $mch1$ and $mch2$ are the maternal and child characteristics[49], $BeforeFM$ is the indicator variable. We remove intercept, subscripts, constant and other features from Equation (4.1) for simplicity. The additional births occurring three days before the full moon to three days following the full moon are represented by coefficients ($\beta_3$ and $\beta_4$) on the interaction terms for each group. We should reject the null hypothesis that the coefficients are identical across the two groups if these traits differ systematically across the threshold.

The regression results from Column 1 of Table (13) depicts 1.98% of additional male

[49] For example, mch1 and mch2 correspond to male and female births, which is a child demographic characteristic. They also reflect younger and older women when it comes to mother's age, which is a maternal demographic characteristic. Due to a lack of information on mother-child characteristics in our dataset, we were unable to evaluate a variety of socio-demographic variables such as maternal education, income, and employment.

births and 1.97% of additional female births. The F-statistics reveal no systematic difference between male and female births suggesting no gender preference around the full moon day cutoff. Column 2 shows fewer higher-order births across the cutoff; however, we cannot reject the equality of the coefficients that the mothers with first and higher-order births are systematically different. We find no significant difference between the coefficients by mother's age (Column 3) and place of residence (Column 4). Overall, as indicated in Table (13), there are no significant changes in mother and child characteristics across the threshold. The finding offers little insight into the factors influencing birth timing.

*Table 13: Mother and child characteristics around the fell moon days cutoff.*

| | (1) Gender | (2) Birth Order | (3) Mother's Age | (4) Place of Residence |
|---|---|---|---|---|
| Before full moon | | | | |
| X boy | 0.0198*** | | | |
| | (0.0005) | | | |
| X girl | 0.0197*** | | | |
| | (0.0004) | | | |
| X first birth | | -0.0011 | | |
| | | (0.0007) | | |
| X higher order | | -0.0017** | | |
| | | (0.0007) | | |
| X teenage mother | | | 0.0125 | |
| | | | (0.015) | |
| X older mother | | | 0.0006 | |
| | | | (0.019) | |
| X Kathmandu valley | | | | -0.0013** |
| | | | | (0.0006) |
| X out of the valley | | | | -0.0014*** |
| | | | | (0.0005) |
| F test (H0: β3 = β4) | 0.054 | 0.478 | 0.202 | 0.126 |
| R-squared | 0.990 | 0.984 | 0.457 | 0.989 |
| Observations | 288 | 288 | 288 | 288 |

*Source: Based on Authors' calculation from Equation (4.3). Note: Robust standard errors in parentheses. An observation is a day. Full moon days are the cut-off. Coefficients are adjusted for year-month, day of the week, lunar days dummies and other covariates. F test determines the equality of interaction terms. The dependent variable is the daily log number of births. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

## CHANGES IN GESTATIONAL LENGTH

Another concern is how far along in the pregnancy the deliveries are planned? We analyse the number of births by the gestational length in weeks (birth term). We also consider the average gestational length.[50] Mean gestational age is around two days shorter for children born in the last three days of full moon day (Column (1) in Table (14). The decline is

[50] Gestational age was based on women's recall of her last menstruation. It is the duration between the delivery date and the first day of last menstruation. Note that the birth register does not include detailed screening tests (ultrasonography) to estimate gestational age clinically.

statistically and clinically significant relative to a mean gestational age of about 277 days. Short gestational length may lead to adverse health outcomes. We report the impact on health outcomes in the subsequent section.

Column 2 to Column 6 reports results for births composition by pregnancy term. We detect that the most significant changes happen during 39 to 40 weeks (4.74 additional births) and then at 37 to 38 weeks. The rise in births above the threshold (Table [11]) is mainly attributable to increased full-term births. Prediger et al. (2020) show that 37 to 40 weeks of gestation is relatively safe for women to undertake elective caesarean delivery. Thus, the full moon day belief may have affected the timing of cesarean births nearer to the 37-40 weeks of gestation. Although statistically insignificant, preterm, late-term, and post-term births increase in the last three days of the full moon day.

*Table 14: Effect of full moon day on term length.*

| | (1)<br>Mean<br>Gestational<br>Age | (2)<br><37<br>Weeks | (3)<br>37-38<br>Weeks | (4)<br>39-40<br>Weeks | (5)<br>41-42<br>Weeks | (6)<br>>42<br>Weeks |
|---|---|---|---|---|---|---|
| Before full moon | -1.993*** | 1.447* | 3.823*** | 4.740*** | 0.109 | 1.249 |
| | (0.123) | (0.760) | (1.467) | (1.815) | (1.303) | (0.967) |
| Daily mean | 277.511 | 3.045 | 8.913 | 22.43 | 7.153 | 2.997 |
| R-squared | 0.339 | 0.160 | 0.474 | 0.307 | 0.317 | 0.198 |
| Observations | 288 | 288 | 288 | 288 | 288 | 288 |

*Source: Based on Authors' calculation from Equation (4.1). Notes: Robust standard errors in parentheses. Each Column is a separate regression from Equation (4.1). An observation is a day. We include a linear trend of birthdays across the threshold as a standard RD specification. Full moon days are the cutoff. Columns (2) to (6) are the proportion of births in the gestational weeks. The dependent variables are mean gestational age in days, the proportion of preterm birth (<37 weeks), early-term birth (38-39 weeks), full-term birth (39 40), late-term birth (40-41) and post-term birth (>42 weeks). Column (1) is weighted by the number of birth counts. Coefficients are adjusted for year-month, day of the week, lunar days dummies and other covariates. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

## HEALTH OUTCOMES

We estimate a version of Equation (4.1) to look at the impacts on maternal and child health outcomes. Our dataset contains maternal and child health conditions at birth, such as low birth weight, low APGAR scores, stillbirth, and maternal postpartum haemorrhage. These outcomes are represented by the proportion of births with maternal and child health conditions on each date of birth. We also consider mean birthweight and mean APGAR score on each date of birth.

Consistent with the findings from the gestational length, Column 1 in Table (15) suggest poor health outcomes for children born before the cutoff with 100 grams lower in birth weight. Column 2 indicates statistically significant lower APGAR scores for children born before the cutoff, but the decline is not clinically meaningful. The proportion of low-birthweight children (Column 3) is also significantly higher. Although statistically insignificant, Table (15) suggests a higher proportion of other adverse health outcomes before the cutoff.

*Table 15: Effect of full moon day on maternal and child health outcomes.*

| | (1)<br>Mean<br>Birthweight | (2)<br>Mean<br>APGAR | (3)<br>Low Birth<br>Weight | (4)<br>Low<br>APGAR | (5)<br>Stillbirth/<br>Miscarriage | (6)<br>Postpartum<br>Haemorrhage |
|---|---|---|---|---|---|---|
| Before moon | -99.506*** | -0.02*** | 0.037** | 0.011 | 0.009 | 0.015* |
| | (3.364) | (0.008) | (0.018) | (0.015) | (0.007) | (0.008) |
| Daily mean | 2937.012 | 7.741 | 0.127 | 0.043 | 0.014 | 0.014 |
| R-squared | 0.193 | 0.646 | 0.118 | 0.181 | 0.168 | 0.153 |
| Observations | 288 | 288 | 288 | 288 | 288 | 288 |

*Source: Based on Authors' calculation from Equation (4.3). Notes: Robust standard errors in parentheses. Each Column is a separate regression from Equation (4.1). An observation is a day. We include a linear trend of birthdays across the threshold as a standard RD specification. Full moon days are the cutoff. Columns (3) to (6) are the proportion of births. Low birth weight: <2500 gram and low APGAR: <7. Column (1) and Column (2) is weighted by the number of birth counts. Coefficients are adjusted for year-month, day of the week, lunar days dummies and other covariates. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

## EFFECTS BY PREGNANCY RISK

While high-risk mothers may be medically prescribed for early delivery, low-risk mothers may afford caesarean delivery over normal births during the full moon days. The nature of pregnancy thus possibly biases the actual effect of full moon day on birth timing and health outcomes. We rerun the primary analysis on births to women with low-risk and high-risk pregnancies[51] to elucidate this issue. Consistent with the overall sample, Table (16) suggests a significant rise in births for low-risk mothers during the full moon days. The additional births are almost entirely driven by caesarean delivery. Panel B shows the result for high-risk pregnancy, suggesting no significant effect on the overall births. As expected, it appears that the delivery methods for high-risk pregnancies shift from normal to caesarean births due to medical concerns.

*Table 16: Effect of full moon day belief on the births and delivery types by pregnancy risk.*

| | (1)<br>Total Births | (2)<br>Normal Births | (3)<br>Caesarean Births |
|---|---|---|---|
| | | A: Low-Risk Pregnancy | |
| Before full moon | 7.962*** | 2.830 | 5.132*** |
| | (2.494) | (2.133) | (1.162) |
| Daily mean | 36.86 | 24.00 | 12.86 |
| R-squared | 0.431 | 0.362 | 0.621 |
| | | B: High-Risk Pregnancy | |
| Before full moon | 1.692 | -2.244 | 3.936*** |
| | (1.942) | (1.589) | (1.158) |
| Daily mean | 19.99 | 11.75 | 8.247 |
| R-squared | 0.317 | 0.283 | 0.396 |
| Observations | 288 | 288 | 288 |

*Source: Authors' calculation from Equation (4.1). Notes: Robust standard errors in parentheses. An observation is a day. Full moon days are the cutoff. Coefficients are adjusted for year-month,*

[51] Our dataset lacks comprehensive information to categorize high-risk and low-risk pregnancies accurately. Low-risk pregnancy is defined as a woman aged 18 to 35 years and a gestational duration of 259 to 286 days.

*day of the week, lunar days dummies and other covariates. Low-risk pregnancy is defined as a woman aged 18 to 35 years and a gestational duration of 259 to 286 days. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

Table (17) shows better health outcomes for low-risk mothers, whereas worsening health effects for high-risk mothers. High-risk births are significantly shorter in gestational age by four days, given that the prevalence of preterm births around the three days of the full moon is 20%. Although statistically significant, the decline in gestational age (by less than a day) among low-risk births is not clinically meaningful. Low-risk births are around 74 grams heavier in their birth weight, while high-risk births are around 341 grams lower. Similarly, a significantly higher proportion of babies with very low birth weight and low APGAR score for high-risk pregnancies, whereas the proportions are not statistically significant for low-risk pregnancies.

*Table 17: Effects of full moon day belief on health outcomes by pregnancy risk.*

| | (1) Mean Birthweight | (2) Mean APGAR | (3) Low Birth Weight | (4) Low APGAR | (5) Stillbirth | (6) Postpartum Haemorrhage |
|---|---|---|---|---|---|---|
| | A: Low-Risk | | | | | |
| Before full moon | 74.261*** | 0.044*** | 0.009 | -0.010 | 0.002 | 0.009 |
| | (4.864) | (0.010) | (0.017) | (0.015) | (0.004) | (0.008) |
| Daily mean | 2958.097 | 7.778 | 0.111 | 0.036 | 0.004 | 0.013 |
| R-squared | 0.236 | 0.615 | 0.181 | 0.181 | 0.134 | 0.202 |
| | B: High-Risk | | | | | |
| Before full moon | -341.317*** | -0.156*** | 0.078** | 0.052** | 0.027 | 0.025* |
| | (12.727) | (0.016) | (0.035) | (0.022) | (0.017) | (0.015) |
| Daily mean | 2888.710 | 7.656 | 0.161 | 0.056 | 0.034 | 0.016 |
| R-squared | 0.143 | 0.430 | 0.100 | 0.158 | 0.180 | 0.104 |
| Observations | 288 | 288 | 288 | 288 | 288 | 288 |

*Source: Authors' calculation from Equation (4.1). Notes: Robust standard errors in parentheses. An observation is a day. Full moon days are the cut-off. Coefficients are adjusted for year-month, day of the week, lunar days dummies and other covariates. Columns 3-6 are the proportion of births. Low birth weight: <2500 gram and low APGAR: <7. Column 1 and 2 is weighted by the number of birth counts. Low-risk pregnancy is defined as a woman aged 18 to 35 years and a gestational duration of 259 to 286 days. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

## DISCUSSION AND CONCLUSION

Based on the 2014-2018 birth records of a maternity hospital in Nepal, this study demonstrates parents' response to their birthing decisions according to social beliefs, which caused around 11 additional births (20.4%) to occur on or before the full moon days. We also find that most of these additional births are due to cesarean delivery, with a 13.7% surplus during the full moon days and a deficit of 16.6% after the full moon. The estimated effect observed in this study is remarkably higher than those from previous studies related to social beliefs (Almond et al. 2015; Huang et al. 2020; Huang et al. 2021) whereas almost consistent with studies related to economic incentives (Tamm 2013; Gans and Leigh 2012; Dickert-Conlin and Chandra 1999; Borra, González, and Sevilla 2016). While economic incentives, such as tax benefits for eligible parents, may be associated with an opportunity cost that possibly confounds birth timing behaviour and health outcomes, social beliefs induced behaviours are less subject to such confounding biases. It is thus somewhat surprising to get the higher effect in response to full moon day beliefs. This section outlines the potential caveat to our estimated effect and then discusses the findings from the demand and supply-side perspectives.

A potentially important issue to the effect of full moon day on birth timing is that the lunar phases have been often postulated to trigger imminent births. Therefore, spontaneous births are presumed to be higher on full moon days. Here, we allude to recent medical literature convincingly refuting such views (Marco-Gracia 2019; Morales-Luengo et al. 2020). It is also documented in South Asia (Bharati et al. 2012). Our analysis further clarified that cesarean births entirely drove the additional full moon births with insignificant results for spontaneous births. The study findings support the conclusion that the additional full moon births are not naturally triggered but shifted on or before the full moon days due to the belief around the auspiciousness of the lunar days.

The study findings show that all other conditions (such as seasonality and mother characteristics) being the same, full moon day caused an increase in the birth only through the caesarean delivery. These findings reflect the desire of Nepali families to have their children born on or near a full moon day through caesarean delivery. The auspiciousness belief is so widespread in Nepali communities that parents may prefer to consider caesarean deliveries on certain lunar days. Such preferences are often anecdotally stated. For example, Shrestha, Saha, and Mahato (2021) speculated that every one in four caesarean deliveries is due to parental preferences for specific dates. Previous studies suggest a higher probability of cesarean births among Nepali women from a better socio-economic background (Acharya and Paudel 2021). We separately estimated our main specification to check if maternal and child characteristics differ systematically across the full moon day cutoff and observe no possible heterogeneity (see Table [13]). However, the lack of a range of sociodemographic variables in the dataset renders the conclusion less convincing.

These findings also indicate that parents in Nepal seem less likely to plan conception but instead depend on caesarean delivery to schedule birth on auspicious lunar days. For instance, Huang et al. (2021) previously demonstrated excess spontaneous births due to couples consciously planning their pregnancy to give birth on the auspicious Dragon year. While the Dragon year is so rare (it occurs every 12 years) that couples may expect to plan their conception consciously, full moon days are too frequent to consider. It is thus possible that we obtained somewhat higher caesarean rates in response to cultural beliefs than those reported in similar studies from another cultural context (Almond et al. 2015; Huang et al. 2020; Huang et al. 2021). Although the beliefs persisted for years prior to caesarean deliveries,[52] the practice is probably common recently. The increasing caesarean rates in Nepal may justify such possibilities (Acharya and Paudel 2021; Dhakal-Rai et al. 2019).

Our study revealed a significantly higher prevalence of caesarean births on days near the full moon. From a supply-side perspective, the surge in deliveries may result in overcrowding and poor-quality services during the full moon days, affecting maternal and child health outcomes. However, the maternity hospital is a tertiary public hospital with adequate human and technical resources. It is equipped with 415 beds, 60% allocated for obstetrics services, and employs 54 doctors, 172 nurses and around 40 paramedics. It also offers after-hours Extended Health Services (EHS) for women willing to pay for specialised care in outpatient and inpatient services. Furthermore, the hospital is accredited under the government-

[52] For instance, Nepali culture (especially the Hindus) consider the sixteenth day of women's monthly cycle the most and the fourth day the least auspicious for conceiving. Similarly, astrological beliefs consider the eleventh to fifteenth lunar days least auspicious for coitus (Pandey 1969, 51-4). These cultural norms interplay with the timing of births around the full moon, but assessing the interaction was not feasible in this study.

funded National Delivery Incentive Program. It receives case-based payment (around USD 70) for caesarean births (FHD 2012). Thus, the hospital may not have reached its limit to provide health services even with the additional caesarean births during the full moon days.

Our subsample analysis shows a significant rise in overall births for low-risk and high-risk mothers due to caesarean delivery. Consistent with the previous literature (Huang, Zhang, and Zhao 2020), the result for low-risk mothers indicates possible self-selection for caesarean delivery. Low-risk mothers may have requested elective caesarean births through the EHS in response to cultural beliefs.[53] Caesarean births among high-risk mothers may be somewhat related to medical reasons. It could also be possible that cultural beliefs may have furnished providers' ulterior socio-economic impetus (see, e.g., [Dhakal-Rai et al. 2021, 6-7]) for prescribing caesarean delivery. These interpretations should be cautiously considered for two important issues. First, our dataset does not detail women's medical history to effectively determine high-risk and low-risk mothers. Next, the secondary data does not include socioeconomic and service arrangement statistics to quantify the possible factors influencing decisions for caesarean delivery on specific lunar days.

Together with these findings, our study raised intriguing concerns regarding healthcare decision-making. For example, healthcare options may be constrained or facilitated within the institutional setup and milieus (e.g., fear and suspicion of the service quality) in which women receive the services (McAra-Couper, Jones, and Smythe 2012). We are unable to demonstrate if (and how) these contextual characteristics influence mothers' decisions for caesarean delivery. Our dataset does not provide detailed information on whether the caesarean delivery is medically indicated or elective to distinguish births that would have been by using EHS on dates around the full moon days. Neither the dataset includes detailed information on women's socio-economic characteristics to gauge these effects in detail. The paucity of birth registries (i.e. inefficient, vital registration system and poorly maintained paper-based hospital records) limit our analysis to use data from a public tertiary hospital.

Our analysis explicitly examined if widely held cultural beliefs drive specific types of services on certain lunar days. Built on the theoretical underpinning of our identification strategy (the RDD), we consider that contextual factors should similarly affect healthcare choices on auspicious and regular lunar days. Instead, this study emphasises the need for parents' education on safe birthing practices and maternity care. It also alerts us that scheduling births for nonmedical reasons, such as cultural beliefs, is biased toward welfare loss for mothers and children.

Despite using the longitudinal data, the generalisability of the study findings is subject to certain limitations. For example, the paucity of population-level longitudinal birth data restricted us to focus on a single hospital. The dataset consists of 72,919 births, with 35% being caesareans. It represents a small subset of births in Nepal compared to the national estimate, whereas it has a higher caesarean rate than a recent demographic survey (MoH, NewERA, and ICF 2017). The study sample consists of births from a tertiary level maternity hospital that tends to provide advanced obstetrics treatment. It is not surprising that the caesarean rate generally exceeds the national average in a large hospital. For example, the caesarean rates in Nepal's large hospitals ranged from 18.9% to 50.9% between 2013 and 2016 (Dhakal-Rai et al. 2019). The hospital-based proportions imply women's inclination toward certain medical interventions while visiting the tertiary hospital. We thus propose a cautious comparison of our results with population estimates.

Although our study reflects the birth trend of tertiary hospitals, it may not be generalised

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[53] Prediger et al. (2020) show that 37 to 40 weeks of gestation is relatively safe for women to undertake elective caesarean delivery. Thus, these mothers were possibly medically safe to undergo elective caesarean delivery.

to the entire population. Similarly, the study findings may not be generalised to women seeking services from private health facilities. Caesarean births in private hospitals are documented to be higher than those in public hospitals (Acharya and Paudel 2021), and doctors in private hospitals are speculated to have higher economic benefits for conducting elective cesarean deliveries (Brunson and Tamrakar 2018). An excellent avenue for future research is to examine whether similar effects of birth timing exist for women giving birth in private hospitals. Future research could then distinguish the effect of the supply channel (provider choice) versus the parent's choice in driving the timing of births.

♦♦♦

*"He who has health, has hope; and he who has hope, has everything."*

~ Thomas Carlyle
Essayist, Historian & Philosopher

# CHAPTER 5 GENERAL DISCUSSION

## SUSTAINABLE DEVELOPMENT AND HEALTH: REVISITING THE NEXUS

Thus far, the dissertation conceived that better health outcomes in early life are the keys to sustainable development. Based on the propositions outlined in the Social Determinants of Health (SDH) literature, the dissertation considered health as a product of interaction between macro factors and the individual's biological and behavioural characteristics. Thus, the study opted to follow a causal inference approach to examine the impacts of economic, environmental and social factors (i.e. the three pillars of sustainable development) on child health outcomes in Nepal using the cross-sectional data available from the population survey and the hospital-based longitudinal data. The empirical analysis led to three analytical chapters, which presented the health impacts of economic, environmental and social circumstances representing the Nepal-India border blockade, air pollution and cultural beliefs. This chapter follows the findings from the analytical chapters to appraise and interpret the empirical patterns within the context of pertinent theories and existing literature. Then the chapter concludes with a brief explanation of the implication of findings for future research and practice

## THE THREE PILLARS AND HEALTH EFFECTS

The "three pillars" perspectives suggest that health is a form of the human capital necessary for sustainable development (Ríos-Osorio, Salas-Zapata, and Ortiz-Lobato 2012). Thus, the concept of upstream determinants, as elaborated in the SDH framework (CSDH 2008; Solar and Irwin 2010), accounts for women's and children's vulnerability to adverse socioeconomic and environmental circumstances. Grounded on these conceptual stances, the first analytical chapter estimated the effect on child mortality due to trade stagnation during the Nepal-India border blockade. The trade stagnation represents critical instances of political instability and the consequent economic hardship in Nepal. The overall result suggests a higher neonatal death rate for children exposed to trade stagnation during the first trimester of their perinatal period and those born to mothers with risky maternal ages. The result indicates that older children and adults are less sensitive to blockade exposure. Consistent with previous studies (Baten and Wagner 2003; Doerr and Hofmann 2022; Friedman and Schady 2013), exposure to macroeconomic crises, such as trade stagnation, in early life manifests in child mortality and poor health outcomes in adulthood.[54] These impacts could also translate into other aspects of human capital formation, such as education attainment (Baten and Wagner 2003; Sharma 2022), mental health (Guerra and Eboreime 2021; Parmar, Stavropoulou, and Ioannidis 2016), economic success (Markovits, Boer, and van Dick 2014), and resilience (Mohseni-Cheraghlou 2016).

The second analytical chapter focused on the environmental pillar of sustainable development by examining the impact of outdoor air pollution on birth weight. The study assumed that the trade stagnation due to the Nepal-India border blockade resulted in reduced petroleum supplies and decreased ambient pollution levels. Contrary to the existing literature revealing positive health externalities of improved air quality (Lavaine and Neidell 2017; Liu, Wang, and Zhang 2021; Yang and Chou 2018), this study revealed negative but statistically insignificant health impacts. There are two possible explanations for the observed discrepancy. First, despite the petroleum shortage, ambient pollution was retained during the blockade months due to households switching to firewood cooking and the illicit petroleum market in

[54] See Naik et al. (2019) for an extensive review of evidence on public health impacts of macroeconomic circumstances and policies.

urban areas of Nepal (Acharya and Adhikari 2021; Rich 2017). As a result, the proxy air quality measures (i.e. Aerosol Optical Depth) may have been confounded with secondary aerosols. Next, inconsistency in the measurement of health outcomes (e.g. birth weight) could have resulted in dubious estimates. Although the results are statistically insignificant, they illustrate the possible depletion of natural resources during the economic crisis and that the economy-environment nexus may lead to poor health. Furthermore, the analytical chapter raised intriguing questions regarding the extent to which political instability and the consequent trade stagnation might have contributed to climate change vulnerability in Nepal.

The third analytical chapter delved into the social dimension of sustainable development. While the social pillar of sustainable development is equivocally stated and flexibly interpreted (Boström 2012),[55] this study exclusively examined it from the perspective of cultural beliefs about the auspiciousness of full moon days in Nepal. By considering cultural beliefs as a social dimension, this study demonstrated the influence of social elements like cultural beliefs, values and practices in shaping health and human capital formation. The study findings reveal the tendency of Nepali parents to depend on cesarean delivery for cultural reasons. It also suggests lower birth weight among children born to mothers opting for caesarean delivery on full moon days. As affirmed by the economic literature (Currie 2011; Oreopoulos et al. 2008), low-birth-weight babies may impede sustainable development since they are less likely to fulfil their educational attainment and consequently less likely to contribute to economic development. Consistent with the previous studies (e.g. [Bhalotra and Cochrane 2010; Bhaskar and Gupta 2007])[56], giving birth by cesarean delivery owing to cultural beliefs is another possible example of a sustainability problem arising from the irrational use of medical resources for non-medical purposes.

## HEALTH DETERMINANTS BEYOND THREE PILLARS

The study revealed that health in early life is embedded in economic, environmental and social factors. Due to their theoretical compatibility with the three pillars of sustainable development, this study focused only on economic, environmental and social dimensions. This study also took a pragmatic position in that these determinants are relatively feasible to employ in an empirical context.[57] However, the number of social determinants in public health literature is ever more emerging than before. It includes a wide range of variables, from the most dominantly cited non-medical factors, such as housing, employment and social support, to the recent extension like social media, democracy, conflict and immigration (Islam 2019; Krumeich and Meershoek 2014). The emerging determinants help us understand how health and sustainable development is shaped by factors other than those analysed in this study. For

[55] Boström (2012) provides a comprehensive summary of conceptual meanings of social sustainability that emerged in the sustainability literature and their possible interpretation in the context of three pillars of sustainable development.

[56] For example, cultural values placed on the male child in India encourage parents to undergo perinatal sex diagnosis using an ultrasound scan. The availability of medical technology resulted in female foeticides and a male-biased population ratio with consequences on the marriage market, female labour force and population growth (Bhalotra and Cochrane 2010; Bhaskar and Gupta 2007).

[57] Note: Krumeich and Meershoek (2014) argue that some social determinants are less feasible to assess for their health impacts. They elaborate that the challenges are inherent in measurement issues for some determinants while context-specific characteristics of some determinants for others. Glass et al. (2013) argue that causal impacts for some critical social determinants (e.g. social class, gender and ethnicity) may be difficult to quantify using a counterfactual framework since these characteristics cannot be manipulated in a hypothetical experiment. Kaufman and Cooper (1999) describe the circumstances under which assessing the causal impacts of social determinants is plausible.

instance, Phadera (2021) provides compelling evidence of the intergenerational effects of conflict on child health and survival. Comparably, McDool et al. (2020) demonstrate the effect of internet use and social media on children's psychological wellbeing. Nevertheless, the broad and expanding number of social determinants is so exhaustive that they may add ambiguity in translating the SDH model into sustainable development policies and practices (Islam 2019; Krumeich and Meershoek 2014).

In a similar vein, the "three pillars" model of sustainable development is consistently emerging. The three-pillar framework was conceptualised in response to the ecological and social criticism of economic development on the one hand and the pursuit of economic growth to address societal and environmental problems on the other (Purvis, Mao, and Robinson 2019). In recent years, studies have gestated other additional pillars such as governance (Crowther, Seifi, and Wond 2019), peace (Sharifi and Simangan 2021), culture (Sabatini 2019), and food security (Guiné et al. 2021). All these factors are critical determinants of health. Thus, future studies may reveal how these elements interact with health and sustainable development. It should also be noted that SDH and the three-pillar paradigm, due to their evolving dimensions, are context-dependent, necessitating an explicit explanation of how they are conceived for any rigorous application in the research.

## CAUSALITY OF SOCIAL DETERMINANTS

This study used population-based cross-sectional survey data and hospital-based longitudinal data to estimate the causal effects of the three specific social determinants on maternal and child health. The analysis was designed such that the observational data emulate a hypothetical randomised experiment (Glass et al. 2013).[58] In other words, assuming that the socioeconomic and environmental actions or events (i.e. trade stagnation, changes in air quality and birthing decisions) were as if randomly allocated, the observational data was used to contrast the average health outcomes among mothers and children exposed to the event (a.k.a. treatment group) with those unexposed. In this process, the study mainly used methodological assumptions from the Regression Discontinuity Design (RDD) that allowed the researcher to assume that the exposure to socioeconomic and environmental circumstances was randomly determined based on a score relative to a cutoff (Lee and Lemieux 2010).

While the RDD is a robust approach to mitigate the likely observed and unobserved confounders inherent in the observational data, it may yield biased estimates depending on the setup on which it is used (Hausman and Rapson 2017; Lee and Lemieux 2010). For example, the second analytical chapter revealed biased results while examining the impact of air quality on birthweight within the RDD framework with time as a running variable. Consistent with Lee and Lemieux (2010), it could be possible that there was no discontinuity at the cutoff because the birthweight impacts were not immediate to air pollution exposure. Similarly, the time-series properties of the outcome variables and the running variable may also have confounded the results (Hausman and Rapson 2017). Given that the health impacts of most social determinants manifest over a lengthy time (Braveman, Egerter, and Williams 2011; Krumeich and Meershoek 2014; Palmer et al. 2019), the second analytical chapter urges the SDH studies to assess the methodological rigours of RDD while studying the causality.

According to Palmer et al. (2019), the interaction between upstream social factors and the downstream individual traits (e.g. income, education and social class) mediates the causal effects of upstream determinants. This study could not untangle the possible interaction

[58] Note: This study followed the counterfactual framework to estimate the causal effects. In the counterfactual framework, causal inferences are possible when the distribution of health outcomes among those who get the intervention is expected to be identical to the distribution that would have been observed had there been no intervention.

between these social characteristics due to the secondary data used for the analyses. Some variables also suffered measurement errors and misclassifications. These limitations corroborate those from the SDH literature that discusses complexities in assessing causal effects of social determinants (Braveman, Egerter, and Williams 2011; Krumeich and Meershoek 2014; Palmer et al. 2019). Similarly, the impacts of social determinants are context-dependent (Krumeich and Meershoek 2014). For example, the empirical patterns that emerged in the second analytical chapter illustrated that poor air quality, despite reduced petroleum supplies, was instituted on the local realities that the petroleum shortage pushed households to switch to polluting fuels (Acharya and Adhikari 2021), leading to a unique pathway that may have determined neonatal birthweight. Indeed, these findings highlight the need to assess local realities while undertaking the causal analysis of interventions or actions that address the socioeconomic and environmental causes of poor health outcomes.

## CONCLUSION

This study examined the impact of socioeconomic and environmental circumstances on maternal and child health in Nepal. Based on the three pillars of the sustainable development framework, the study revealed that child survival and health outcomes are affected by non-medical causes such as economic instability, environmental change and cultural beliefs. The three pillars framework provided an integrated approach to explore and understand determinants of health from non-medical perspectives. Health and wellbeing from the three pillars of the sustainable development framework are based on the understanding that human health depends on a flourishing economy, ecological system and sociocultural factors. However, as revealed in the analytical chapters, the discrepancy in either of these three pillars may constrain maternal and child health. By considering border blockade, air quality changes and birthing decisions related to cultural beliefs as examples of the three pillars of sustainable development, this study demonstrated a supplementary approach to exploring and understanding non-medical causes of poor health outcomes. For example, while the existing framework, such as planetary health (Whitmee et al. 2015), focuses on economic and ecological causes of poor health, this study offered additional insights that other non-medical causes, such as cultural beliefs, are equally crucial for health-related sustainable development.

Thus, the study indicates the importance of multisectoral approaches, such as healthy public policies,[59] to address the social determinants of poor health outcomes. As discussed in the previous chapters, social elements such as trade supplies, quality air, and health services are the critical resources available in a society that may allow its citizens to enjoy good health (Islam 2019). It should be no surprise that public policies control access to these assets. In the first analytical chapter, it was elaborated that the trade stagnation, and thus the child mortality, in Nepal was due to its internal political climate and the foreign policy of its neighbour. Similarly, examples of polluting fuels and the illicit petroleum market revealed in the second analytical chapter reflect another set of public policies (e.g. energy policy) contributing to poor child health. So does the provision of maternity services for non-medical reasons in the third chapter. It is, therefore, apparent that public policy is a more fundamental determinant than any of the other social determinants discussed in the preceding chapters. Indeed, policymakers are incentivised to tradeoff unhealthy decisions with healthy alternatives.

The study also indicates the necessity to invest in early childhood development. Child mortality has enormous economic consequences for the family and society. The cost of policy

[59] Note: Healthy public policy is a public health approach where health and equity are placed at the centre of public policy areas (e.g. education, environment, housing, and trade). It aims to create a supportive environment to enable people to live healthy and productive lives (Nutbeam and Kickbusch 1998).

inaction for child development is estimated to surpass 10% of the GDP, whereas the value of an additional year of life is equivalent to 1.5 times the GDP per capita (Clark et al. 2020). Furthermore, ensuring child health is a widely accepted precursor for social justice. For example, children with adverse health conditions in their early life are at risk of poor education and income trajectories, consequently diminishing social equities. Thus, the findings from the three analytical chapters hint that neither the health-related SDGs nor the principle of "leaving no one behind" are realistic unless child survival and early life development are ensured.

The causal inference analysis employed in this study illustrated that health is central to the three pillars framework. To put it more simply, the economic, social and environmental pillars are the independent but interconnected health domains. Their interaction must be regulated to achieve child health and sustainable development. Despite using the causal inference analysis, the study could not escape from limitations in a few instances. For example, the lack of air quality data from the monitoring stations in Nepal resulted in biased estimates and less conclusive results in the second analytical chapter. Similarly, findings from the third analytical chapter may not be generalised to the entire population of Nepal. While child mortality and morbidity data are extremely limited in Nepal, this study attempted to mitigate the limitation using cross-sectional and longitudinal data. The insights gained from this study may assist future studies that use observational data to assess the impacts of economic and environmental policies on health and human capital.

Similarly, future studies may also examine the impacts of economic, social and environmental policies and actions on other aspects of sustainable development, such as childhood education, health equity and human capital formation. The purpose of such studies would be threefold. First, the studies would approach the impact assessment from life course and intergenerational perspectives since childhood experiences of socioeconomic and environmental circumstances manifest in adulthood and the subsequent generations. Then, the studies might consider local characteristics and processes underlying causal pathways. Studies would also mitigate the limitations of observational data, as highlighted in this study.

Another area of research would be exploring the operational aspects of the social determinants and sustainable development models. As revealed in the previous section, the SDH concepts and the three-pillar framework are increasingly becoming ambiguous due to their flexibility in interpreting these concepts from multiple dimensions. Thus, a better understanding of these concepts must be developed if the health and sustainable development discourse is to be moved forward. Qualitative studies may enhance the utility of these concepts by clarifying the theoretical basis and embedded ideologies in them.

In terms of direction for future practices, several courses of action are desirable for ensuring child health and sustainable development in Nepal. Continued efforts are needed from the Government of Nepal on children's health and wellbeing owing to the high benefit-cost ratio of economic investment in the health and education sector. Then, there is a need to create a policy climate that encourages nurturing and caring for young children. It involves policies that address the upstream determinants of health, such as affordable housing, free primary education, income tax credits, social security programs, environmental sanitation and job creation. These policies are often outside the scope of the health sector. As a result, the sectoral ministries should create an overarching agency to coordinate their policy actions across sectors. In the Nepalese context, some of these policies (e.g. school mean program) are already at their implementation stage. Therefore, there is a definite need to assess the causal impact of these policies on child health and other areas of human capital formation.

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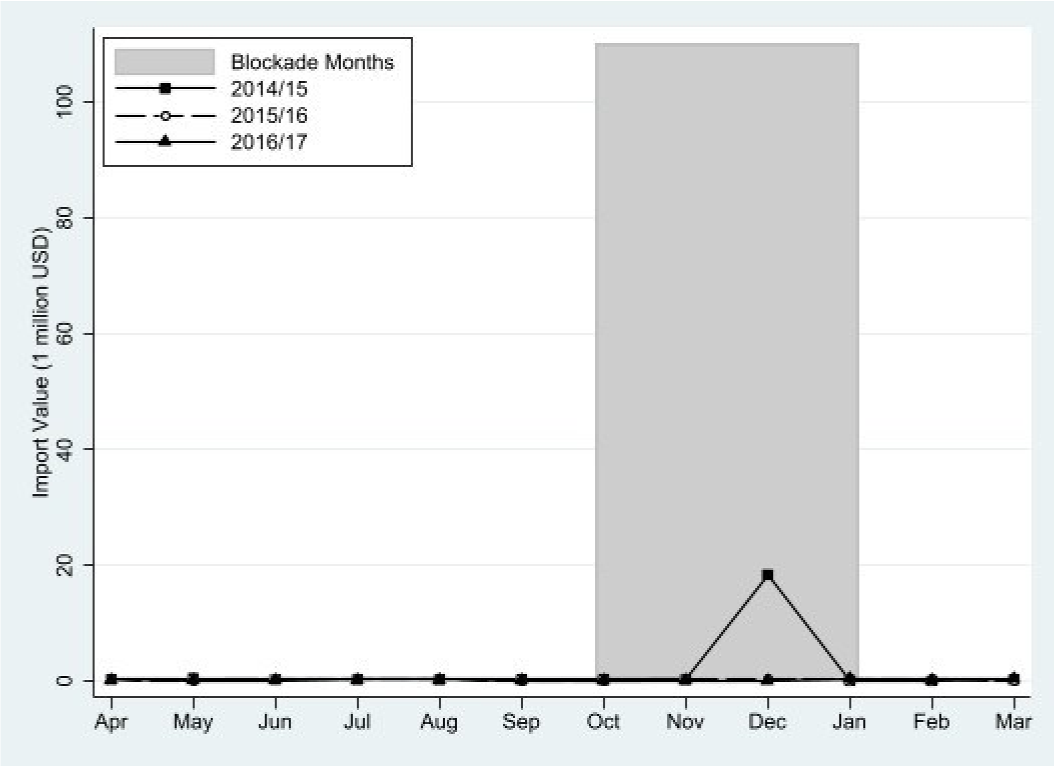
# ANNEX

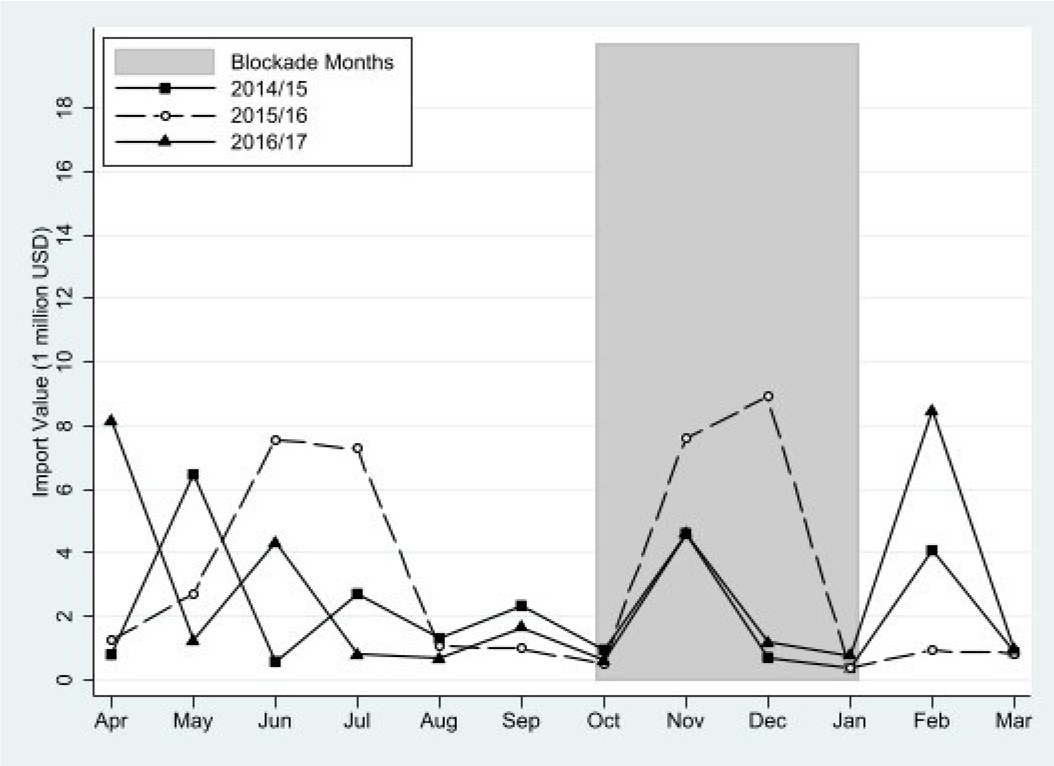
## ANNEX 1: SUPPLEMENTARY ANALYSIS FROM CHAPTER 2.

*Figure 7: Petroleum imports from countries other than India.*




*Source: Authors' drawing from UN Comtrade database. Note: Imported products depicted in this figure are those with the HS (Harmonized System) code 2709 and 2710 (crude and non-crude petroleum oils, respectively).*

*Figure 8: Pharmaceuticals imports from countries other than India.*




*Source: Authors' drawing from UN Comtrade database. Note: Imported products depicted in this figure are those with the HS (Harmonized System) code 3003 and 3004 (medicaments).*

*Table 18: Impact of the blockade on adult mortality (using the province panel data).*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **A) Urban** | | | | |
| Outcome: Mortality by age group | 20 to 39 years | | >= 40 years | |
| Treatment (blockade period dummy) | 0.037 | 0.077 | 0.196 | 0.016 |
| | (0.057) | (0.067) | (0.717) | (1.378) |
| Province-year fixed effects | Yes | Yes | Yes | Yes |
| Province-month fixed effects | No | Yes | No | Yes |
| Province-month linear trends | No | Yes | No | Yes |
| Observations | 420 | 420 | 420 | 420 |
| R-squared | 0.126 | 0.122 | 0.070 | 0.067 |
| **B) Rural** | | | | |
| Outcome: Mortality by age group | 20 to 39 years | | >= 40 years | |
| Treatment (blockade period dummy) | 0.048 | -0.011 | 0.211 | -0.832 |
| | (0.052) | (0.048) | (0.832) | (1.635) |
| Province-year fixed effects | Yes | Yes | Yes | Yes |
| Province-month fixed effects | Yes | Yes | Yes | Yes |
| Province-month linear trends | No | Yes | No | Yes |
| Observations | 420 | 420 | 420 | 420 |
| R-squared | 0.061 | 0.092 | 0.099 | 0.089 |

*Source: Authors' calculation from the 2016 NHDS data using Equation (2.1). Note: Robust standard errors in the parenthesis. Province-month is an observation (i.e. seven provinces x 60 months). Mortality rates are interpreted as the percentage of deaths. The mortality rate is calculated as the number of deaths of children in the age category c in month m divided by the number of children alive in that age category at the beginning of the corresponding month. Regarding neonatal mortality, the denominator is the number of live births at the beginning of the corresponding month. Blockade months correspond to the first day of Kartik 2072 to the end of Paush 2072 in the Bikram Sambat calendar. The baseline results from Equation (2.1), without province-month linear trends, are in Columns 1 and 4. The rest of the Columns show full specification results with linear trends. Significance levels: $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1.*

*Table 19: Summary statistics of the main empirical variables, province panel analysis.*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Mean | Std. Dev. | Mean | Std. Dev. |
| Province panel analysis: Child mortality | Urban (Obs. = 420) | | Rural (Obs. = 420) | |
| Monthly mortality rate (‰) | | | | |
| under 1 year old | 2.279 | 5.569 | 2.861 | 7.421 |
| 1 to 2 years old | 0.194 | 1.578 | 0.192 | 1.852 |
| Blockade period | 0.050 | 0.218 | 0.050 | 0.218 |

*Source: Authors' calculation from the 2016 NHDS province panel data. Province-month is an observation. Mortality rates are interpreted as the percentage of death.*

*Table 20: Summary statistics of main variables.*

| | Obs. | Mean | Std dev. | Min | Max |
|---|---|---|---|---|---|
| Died within a month | 2840 | 0.016 | 0.124 | 0 | 1 |
| Died within 15 days | 2840 | 0.013 | 0.115 | 0 | 1 |
| Died on the birthday | 2840 | 0.010 | 0.099 | 0 | 1 |
| Blockade dummy (born on the blockade months | 2840 | 0.061 | 0.239 | 0 | 1 |
| Interaction terms: Blockade X | | | | | |
| Poor | 2840 | 0.024 | 0.152 | 0 | 1 |
| Mother with no formal education | 2840 | 0.012 | 0.111 | 0 | 1 |
| Delivery at risky maternal age | 2840 | 0.017 | 0.128 | 0 | 1 |
| Deviation from the threshold age | 2840 | 0.039 | 0.351 | 0 | 8 |
| Female | 2840 | 0.474 | 0.499 | 0 | 1 |
| Mother's years of education | 2840 | 5.774 | 4.322 | 0 | 11 |
| Mother's age | 2840 | 23.784 | 5.065 | 13 | 47 |
| Delivery at health facility | 2840 | 0.656 | 0.475 | 0 | 1 |
| Delivery by Caesarean section | 2840 | 0.118 | 0.323 | 0 | 1 |
| Household wealth index | 2840 | 2.800 | 1.352 | 1 | 5 |
| Perinatal care | | | | | |
| Doctor | 2323 | 0.511 | 0.500 | 0 | 1 |
| Other health workers | 2323 | 0.945 | 0.228 | 0 | 1 |
| Exposed to the blockade | | | | | |
| Foetal stage | 2323 | 0.073 | 0.260 | 0 | 1 |
| 1st trimester | 2323 | 0.074 | 0.262 | 0 | 1 |
| 2nd trimester | 2323 | 0.090 | 0.286 | 0 | 1 |
| 3rd trimester | 2323 | 0.178 | 0.382 | 0 | 1 |
| Log birthweight | 2031 | 7.981 | 0.221 | 6.551 | 8.700 |
| Pregnancy loss | 3773 | 0.247 | 0.431 | 0 | 1 |

*Source: Authors' calculation from the birth record of 2016 NHDS data.*

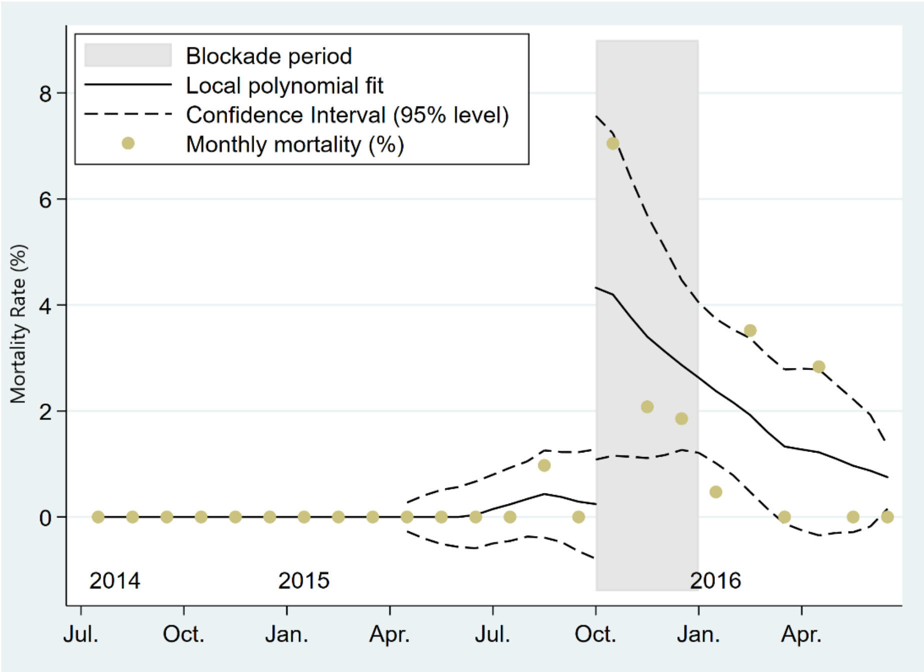
*Table 21: Treatment and placebo effects on infant mortality, province panel data.*

| | (1) | (2) | (3) |
|---|---|---|---|
| **A) Treatment-control comparison** | | | |
| | During the period of blockade (2015/16) | During the same months in the preceding 4 years (2011/12-2014/15) | (1) − (3) |
| Age category | Mean | Mean | Difference |
| Observations | 21 (= 3 mths × 7 provs) | 84 (= 3 mths × 4 yrs × 7 provs) | |
| *Urban* | | | |
| Under 1 year old | 4.713<br>[6.222] | 1.767<br>[5.277 | 2.946**<br>(1.335) |
| 1 to 2 years old | 0.000<br>[0.000] | 0.000<br>[0.000] | 0.000<br>(0.000) |
| *Rural* | | | |
| Under 1 year old | 2.669<br>[5.818] | 2.950<br>[7.255] | −0.281<br>(1.708) |
| 1 to 2 years old | [0.673<br>[3.086] | 0.000<br>[1.000] | 0.673**<br>(0.332) |
| **B) Trends in the preceding year (placebo comparison)** | | | |
| | During the same months in the preceding year (2014/15) | During the same months in the 2nd to 4th preceding years (2011/12-2013/14) | (1) − (3) |
| Age category | Mean | Mean | Difference |
| Observations | 21 (= 3 mths × 7 provs) | 63 (= 3 mths × 3 yrs × 7 provs) | |
| *Urban* | | | |
| Under 1 year old | 0.640<br>[2.022 | 2.143<br>[5.948] | −1.503<br>(1.327) |
| 1 to 2 years old | 0.000<br>[0.000] | 0.000<br>[0.000] | 0.000<br>(0.000) |
| *Rural* | | | |
| Under 1 year old | 2.690<br>[6.836] | 3.037<br>[7.441] | −0.346<br>(1.839) |
| 1 to 2 years old | 0.000<br>[0.000] | 0.000<br>[0.000] | 0.000<br>(0.000) |

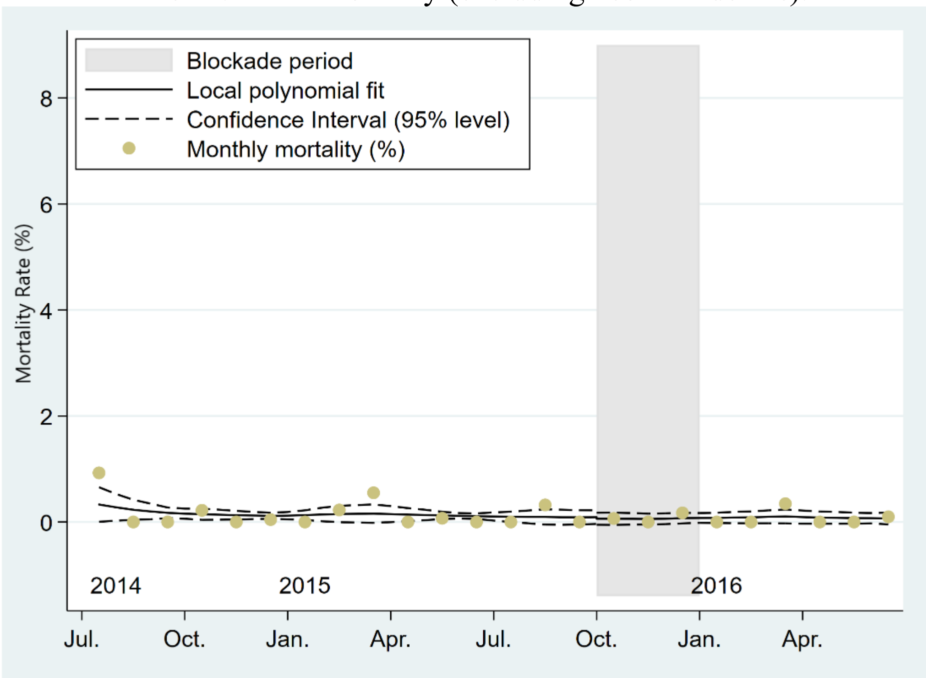
*Source: Authors' calculation from the 2016 NHDS data using Equation (2.1). Note: Standard errors in the small brackets and standard deviations in the square brackets. Province-month is an observation. Mortality rates are interpreted as the percentage of deaths. The mortality rate is calculated as the number of deaths of children in the age category c in month m divided by the number of children alive in that age category at the beginning of the corresponding month. Regarding neonatal mortality, the denominator is the number of live births at the beginning of the corresponding month. All estimates are based on the specification (i.e. adjusted for province-month fixed effects, province-year fixed effects, and province-month linear trend) outlined in the third column of Table (1). Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$*

*Figure 9: Child mortality rates in urban areas.*

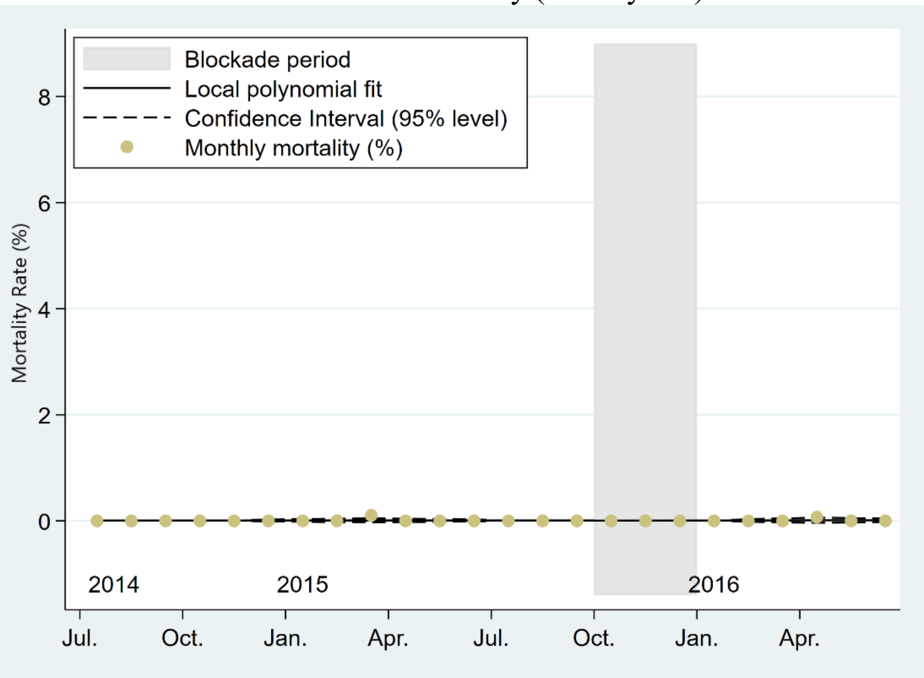
Panel A: Neonatal mortality (within first month of birth).




Panel B: Infant mortality (excluding neonatal deaths).



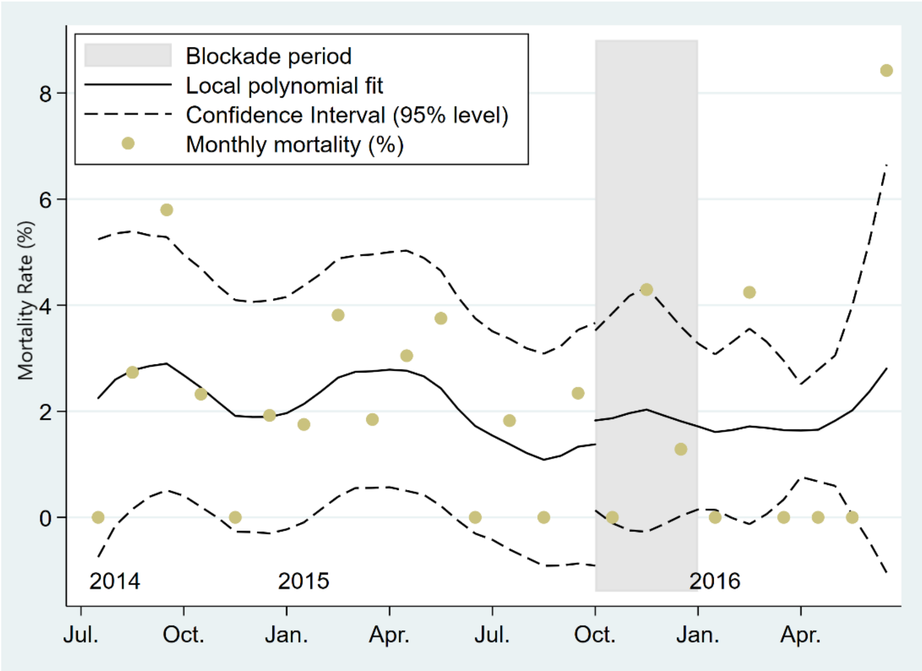
Panel C: Child mortality (1 to 2 years).



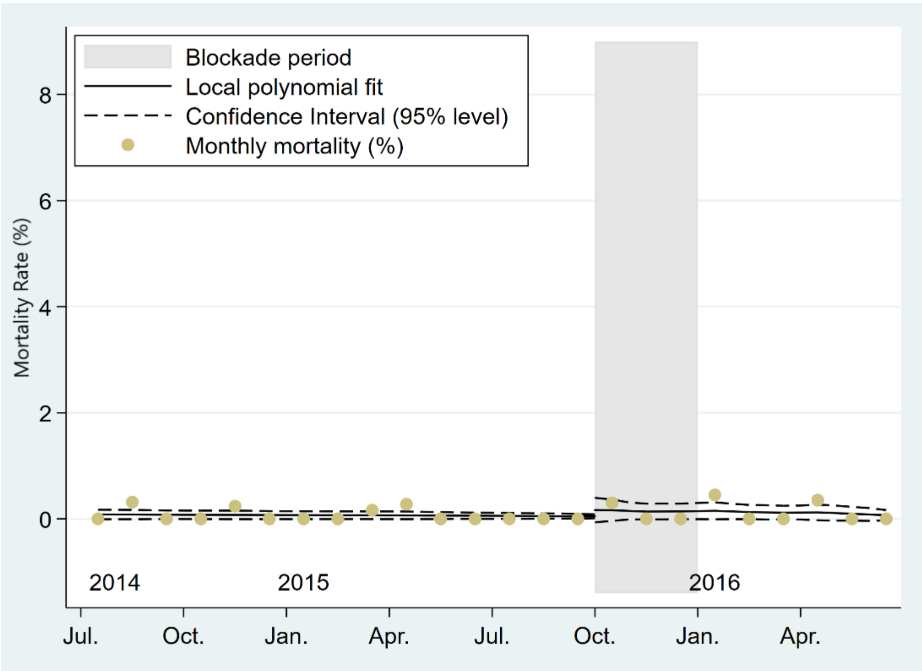

*Source: Authors' drawing from the birth record of 2016 NHDS data. Note: The solid lines represent kernel-weighted local polynomial smooth fits with triangular kernel weights. Dates reported in the 2016 NDHS are based on the Bikram Sambat calendar, which does not correspond precisely with the Gregorian calendar months. The mortality rate for an age category in mid-July is calculated as the number of deaths of children in the age category observed between mid-July and mid-August divided by the number of children alive in that age category at the beginning of the corresponding period. Regarding neonatal mortality in Panel A, the denominator is the number of live births in the period.*

*Figure 10: Child mortality rates in rural areas.*

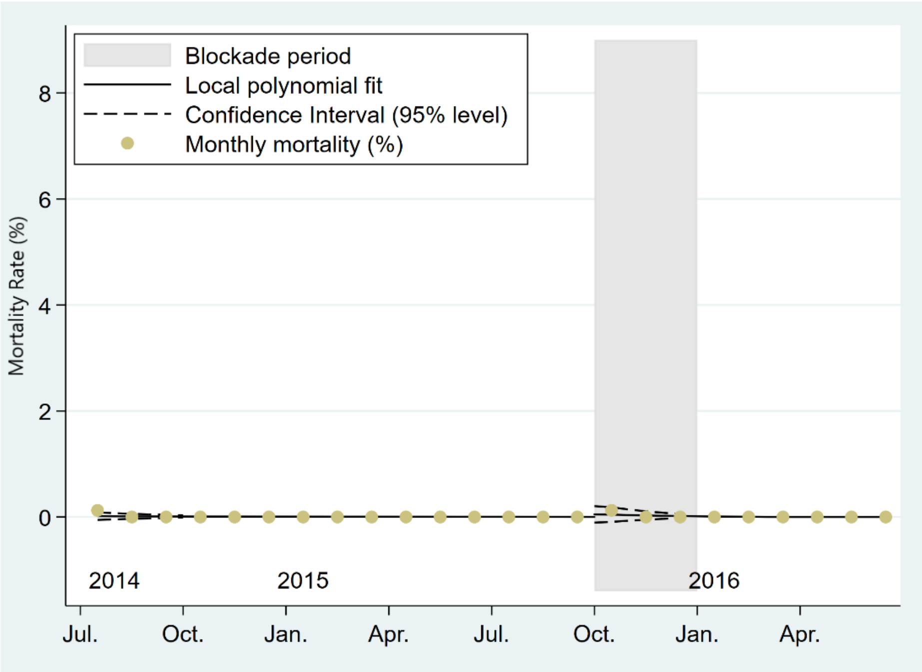
Panel A: Neonatal mortality (within the first month of birth).




Panel B: Infant mortality (excluding neonatal deaths).



Panel C: Child mortality (1 to 2 years).




*Source: Authors' drawing from the birth record of 2016 NHDS data. Note: The solid lines represent kernel-weighted local polynomial smooth fits with triangular kernel weights. Dates reported in the 2016 NDHS are based on the Bikram Sambat calendar, which does not correspond precisely with the Gregorian calendar months. The mortality rate for an age category in mid-July is calculated as the number of deaths of children in the age category observed between mid-July and mid-August divided by the number of children alive in that age category at the beginning of the corresponding period. Regarding neonatal mortality in Panel A, the denominator is the number of live births in the period.*

Equation 2.3: Nonparametric RDD estimation of child mortality.

We conducted a nonparametric regression discontinuity (RD) local polynomial estimation with MSE-optimal bandwidth to verify statistical changes in mortality at the onset of the blockade. The RD specification takes the following form:

$$D_{it} = \beta_0 + \beta_1 blockade_t + \beta_2 r(t) + \beta_3 [r(t) * blockade_t] + \upsilon_{it} \ \forall t \in (t_0 - h^*, \ t_0 + h^*) \qquad (2.3)$$

where $D_{it}$ is a binary variable that takes unity if a child $i$ born in time (month) $t$ died within the first month of life and zeros otherwise; $blockade_t$ is the treatment dummy for children born in the blockade period, r($t$) represents a time-trend term, $\upsilon it$ is an unobserved component, $h^*$ is the MSE optimal bandwidth, and $t_0$ represents the running variable (i.e. birth months) cantered to zero around the blockade onset month. The parameter of interest $\beta_1$ captures the blockade's regression discontinuity (RD) effect (i.e. local average treatment effect at the onset of the blockade) on neonatal death. The estimation sample consists of all births from July 2014 to June 2016.

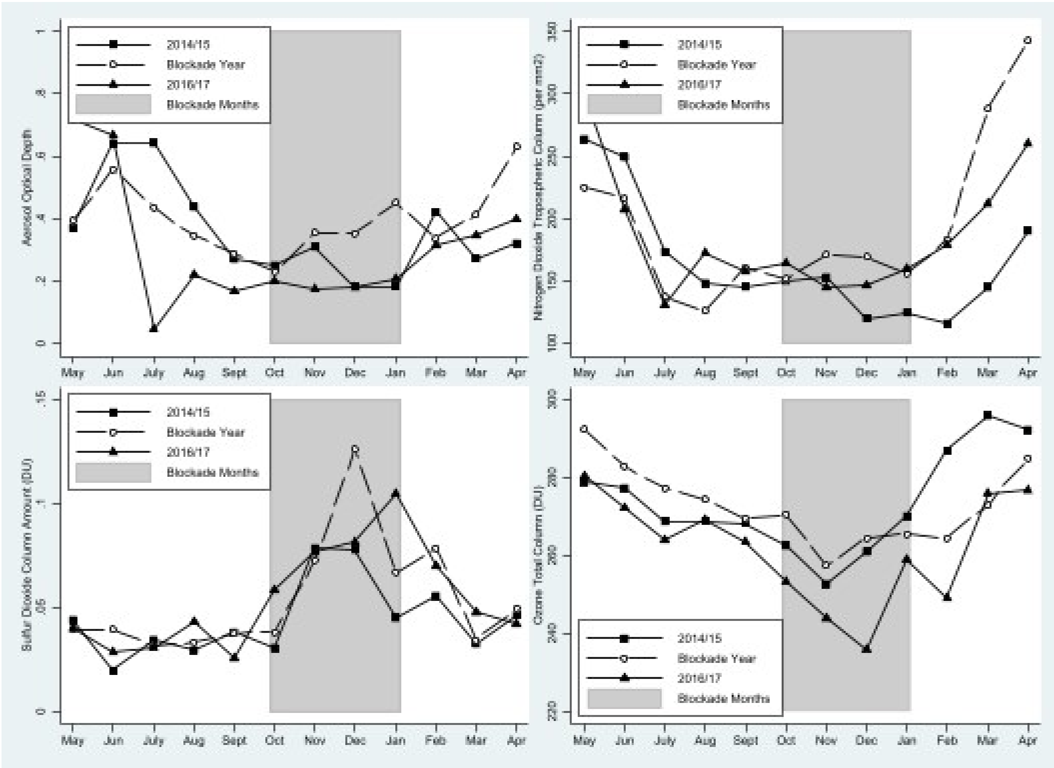
# ANNEX 2: SUPPLEMENTARY ANALYSIS CHAPTER 3.

*Table 22: Descriptive statistics for variables used in Chapter 3.*

| | Before Blockade (1) | | After Blockade (2) | | During Blockade (3) | | Difference |
|---|---|---|---|---|---|---|---|
| | Obs. | Mean | Obs. | Mean | Obs. | Mean | (3) – (1) (2) |
| Birthweight | 23,996 | 2924.019 | 41,967 | 2952.414 | 6,507 | 2900.132 | -41.952$^{***}$ |
| | | (509.494) | | (523.623) | | (520.018) | (6.756) |
| Low birthweight (<2500 gm) | 23,996 | 0.130 | 41,967 | 0.122 | 6,507 | 0.130 | 0.006 |
| | | (0.337) | | (0.327) | | (0.337) | (0.004) |
| APGAR Score | 24,066 | 7.497 | 42,136 | 7.738 | 6,525 | 7.462 | -0.188$^{***}$ |
| | | (1.134) | | (1.261) | | (1.181) | (0.015) |
| | | 0.062 | 42,136 | 0.052 | | 0.063 | 0.008$^{**}$ |
| Low APGAR (< 7) | 24,066 | (0.241) | | (0.222) | 6,525 | (0.244) | (0.003) |
| | | 24.121 | 42,102 | 24.473 | | 24.440 | 0.095 |
| Mother's Age | 24,056 | (4.631) | | (4.679) | 6,522 | (4.705) | (0.061) |
| | | 0.045 | 42,102 | 0.042 | | 0.043 | -0.050$^{**}$ |
| Risky age (<18\|> 35) | 24,056 | (0.207) | | (0.201) | 6,522 | (0.202) | (0.018) |
| | | 0.461 | 42,166 | 0.461 | | 0.467 | 0.006 |
| Female Child | 24,077 | (0.499) | | (0.498) | 6,529 | (0.499) | (0.006) |
| | | 0.333 | 42,143 | 0.440 | | 0.339 | -0.062$^{***}$ |
| Kathmandu Resident | 24,060 | (0.471) | | (0.496) | 6,454 | (0.473) | (0.006) |
| | | 0.504 | 42,133 | 0.490 | | 0.496 | 0.001 |
| First Birth | 24,055 | (0.500) | | (0.500) | 6,524 | (0.500) | (0.006) |
| | | 277.879 | 37,619 | 277.104 | | 275.646 | -1.740$^{***}$ |
| Gestation Length | 21,547 | (14.816) | | (14.887) | 5,859 | (15.246) | (0.208) |
| | | 0.498 | 37,619 | 0.515 | | 0.495 | -0.014$^{**}$ |
| Term Birth (39-40 Weeks) | 21,547 | (0.500) | | (0.500) | 5,859 | (0.500) | (0.007) |
| | | 0.342 | 42,166 | 0.354 | | 0.356 | 0.006 |
| Caesarean Births | 24,077 | (0.474) | | (0.478) | 6,529 | (0.479) | (0.006) |

*Source: Based on Authors' calculations. Note: Standard deviations in the parenthesis. An observation is a newborn. Blockade period was between 23 September 2015 and 3 February 2016. Significance levels: $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0*

*Figure 11: Monthly average pollution levels from May 2014 to April 2017.*



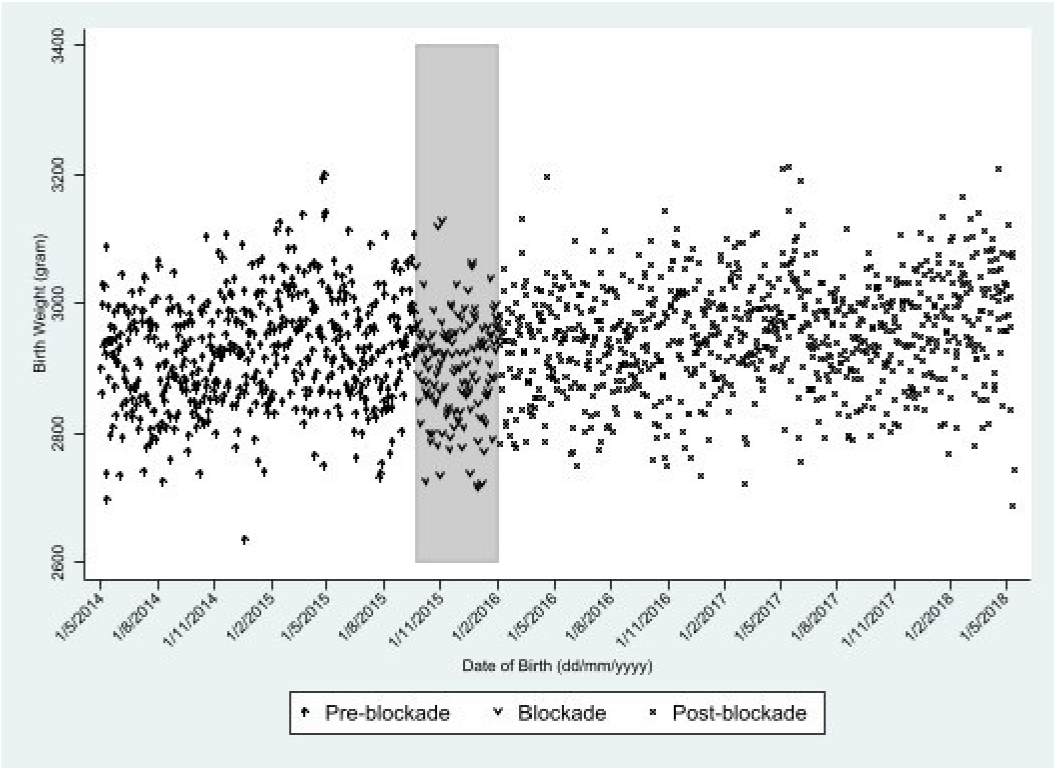

*Source: Authors' drawing based on time-averaged maps from Goddard Earth Sciences Data and Information Services Centre (GES-DISC) of the National Aeronautics and Space Administration (NASA). Note: The figure in the top-left shows the average Optical Aerosols Depth (550nm land only) available through GES-DISC from the Moderate Resolution Imaging Spectroradiometer (MODIS) Aqua at 1-degree spatial resolution. The nitrogen dioxide ($No_2$), sulphur dioxide ($So_2$), and ozone (OZ) data are available through GES-DISC from the Ozone Monitoring Instrument (OMI) Aura at 0.25-degree spatial resolution. Pollution levels for $No_2$ (tropospheric column in molecules per centimetre square), $So_2$ (total column in Dobson Unit) and OZ (total column in Dobson Unit) are shown in the top-right, bottom-left and bottom-right panels, respectively.*

*Table 23: Preliminary RD estimation using blockade onset and abolition cutoffs.*

| | (1) | (2) | (3) |
|---|---|---|---|
| Cutoff 1 (23 Sep 2015) | -37.124 | -37.124 | -37.124 |
| | (-129.19, 75.68) | (-129.19, 75.68) | (-129.19, 75.68) |
| Cutoffs II (4 Feb 2016) | -17.077 | -17.077 | -17.077 |
| | (-130.02, 85.60) | (-130.02, 85.60) | (-130.02, 85.60) |
| Weighted | -30.587 | -30.587 | -30.587 |
| | (-99.18, 49.69) | (-99.18, 49.69) | (-99.18, 49.69) |
| Pooled | -34.921** | -42.572* | -46.815** |
| | (-82.37, -1.76) | (-88.54, 2.72) | (-90.79, -1.12) |
| Effective No of Treated | 6825 | 8806 | 12966 |
| Effective No of Control | 7153 | 9573 | 14492 |
| Bandwidth | 83.826 | 138.354 | 234.527 |
| Polynomial Order | 1 | 2 | 3 |

*Source: Authors' calculation from the birth record of PMWH data. Notes: Coefficients are the RD point estimates of birthweight changes at the onset of the blockade (Cutoff I) and abolition of the blockade (Cutoff II) obtained from a nonparametric RD estimation. Estimations are based on MSE-optimal bandwidth with triangular kernel weights. Standard errors clustered at birthdates. 95%CI in parenthesis. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.*

*Figure 12: Scatterplot of average birthweight, May 2014 to May 2018.*




*Source: Authors' drawing from the birth record of the maternity hospital (2014-2018).*

# ANNEX 3: SUPPLEMENTARY ANALYSIS, CHAPTER 4

*Table 24: Information from the birth register and definitions.*

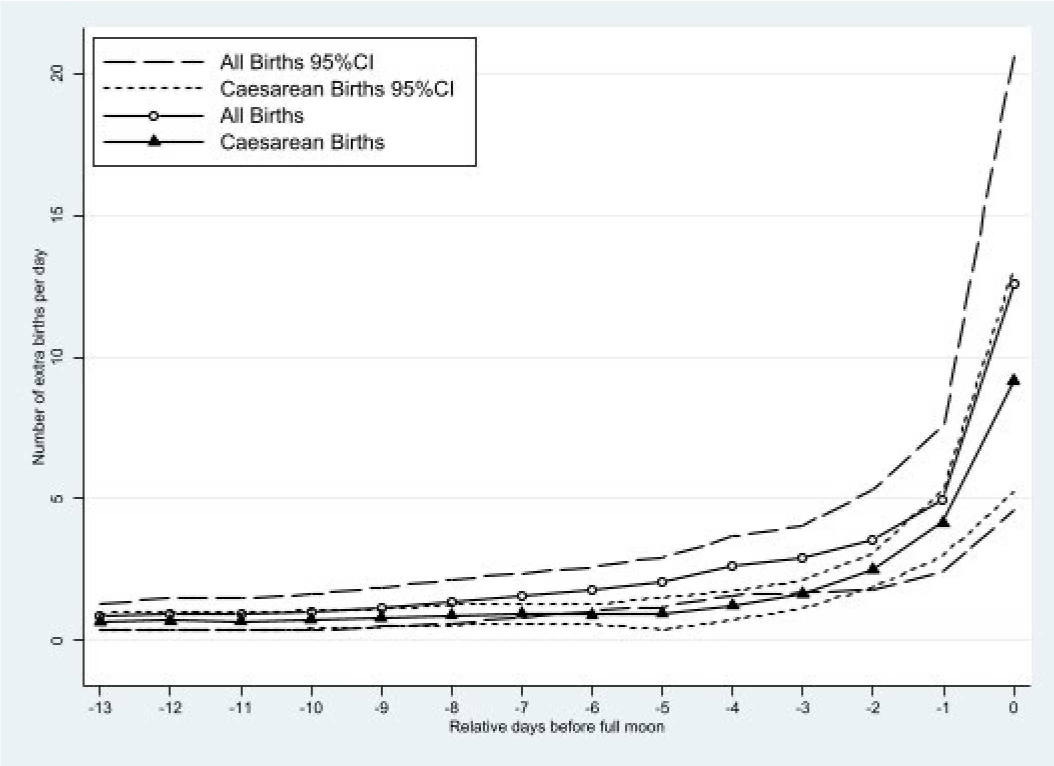
| **Variables** | **Definition** |
|---|---|
| Address | Mother's address (retrieved as outside/inside Kathmandu Valley). |
| Age | Mother's self-reported age (in years) during delivery. |
| Parity | Women's self-reported number of pregnancies during childbirth. |
| EDD | Expected Delivery Date (yy/mm/dd) based on mother's menstrual history. |
| DOD | Neonate's birthdate (yy/mm/dd) recorded by nurses after delivery. |
| Outcome | Physicians confirmed neonatal birth status (alive/dead) after delivery |
| Birthweight | Nurses measured neonate's weight (in grams) immediately after birth. |
| Gender | Male or female child. |
| APGAR | Neonate's health status score (1 to 10) based on breathing, heart rate, skin colour, reflexes, and muscle tone assessment by doctors after delivery. |
| Delivery type | Types of delivery services received (normal/caesarean) |
| Abnormality | Neonates born with physical abnormality (yes/no), recorded by nurses after physicians' post-delivery examination |
| Stillbirth | Foetal death > 20 weeks (yes/no), confirmed by doctors and recorded by nurses after delivery |
| PPH | Postpartum haemorrhage (yes/no) measured by 24-hours maternal blood loss |

*Table 25: Descriptive statistics of variables used in Chapter 4.*

| | Full Sample (N = 1477) | | Analytic Sample (n = 288) | | | | |
|---|---|---|---|---|---|---|---|
| | (1) Total Births | (2) Daily Mean | (3) Total Births | (4) Daily Mean | (5) Before Cutoff | (6) After Cutoff | (7) Difference (5) - (6) |
| *A: Delivery Types* | | | | | | | |
| Normal Births | 47404 | 32.095 | 9027 | 31.344 | 29.604 | 33.083 | -3.479*** |
| Caesarean Births | 25515 | 17.275 | 5348 | 18.569 | 22.382 | 14.757 | 7.625*** |
| *B: Health Outcomes* | | | | | | | |
| Low Birthweight (<2500 gm) | 9104 | 0.125 | 1833 | 0.127 | 0.130 | 0.125 | 0.005 |
| Low APGAR (< 7) | 3020 | 0.042 | 605 | 0.043 | 0.046 | 0.040 | 0.006 |
| Stillbirths | 1084 | 0.015 | 201 | 0.013 | 0.015 | 0.012 | 0.003* |
| Postpartum Hemorrhage | 1031 | 0.014 | 199 | 0.014 | 0.015 | 0.013 | 0.002 |
| *C:Mother-Child Characteristics* | | | | | | | |
| Teenage Mother (< 18) | 1627 | 1.102 | 316 | 1.097 | 1.104 | 1.090 | 0.014 |
| Old Mother (> 35) | 1510 | 1.022 | 303 | 1.052 | 1.028 | 1.076 | -0.049 |
| Male | 39265 | 26.584 | 7708 | 26.764 | 28.014 | 25.514 | 2.500*** |
| Female | 33654 | 22.785 | 6667 | 23.149 | 23.972 | 22.326 | 1.646** |
| Inside Kathmandu | 28805 | 19.502 | 5599 | 19.441 | 20.035 | 18.847 | 1.188 |
| Outside Kathmandu | 43999 | 29.789 | 8766 | 30.438 | 31.903 | 28.972 | 2.93 |
| Low Risk | 44003 | 29.792 | 8617 | 29.920 | 31.118 | 28.722 | 2.396*** |
| High Risk | 21091 | 14.280 | 4208 | 14.611 | 15.174 | 14.049 | 1.125** |
| All Births | 72919 | 49.370 | 14375 | 49.913 | 51.986 | 47.840 | 4.146*** |

*Source: Based on Authors' calculation. Note: Panel B reports proportion of births. An observation is a day. Full moon days are the cutoff. Significance levels: *** p<0.01, ** p<0.05, * p<0.1*

*Figure 13:Additional births per day on and before full moon days.*




*Source: Authors' drawing using the data file from the birth record of the maternity hospital (2014-2018). Note: Relative days are the child's birth dates from the full moon days as indicated in the Vikram Sambat calendar Source: Additional births per day are obtained by dividing the coefficient from Equation (4.1) with the number of days on or before the full moon.*

♦♦♦