Three Essays on Pre-natal Experiences and Human Capital Accumulation

by

Romina Tomé

Public Policy Studies
Duke University

Date: _____

Approved:

_____
Marcos De Almeida Rangel, Supervisor

_____
Duncan Thomas

_____
Elizabeth Frankenberg

_____
Jennifer Lansford

Dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy
in Public Policy Studies
in the Graduate School of
Duke University

2020

ABSTRACT

Three Essays on Pre-natal Experiences and Human Capital Accumulation

by

Romina Tomé

Public Policy Studies
Duke University

Date: _____

Approved:

_____
Marcos De Almeida Rangel, Supervisor

_____
Duncan Thomas

_____
Elizabeth Frankenberg

_____
Jennifer Lansford

An abstract of a dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy
in Public Policy Studies
in the Graduate School of
Duke University

2020

Copyright © 2020 by Romina Tomé
All rights reserved

Abstract

This dissertation combines three essays that explore how pregnant women's exposure to social and physical stressors affect human capital at it earliest stage, *in utero*. Informed by theoretical groundwork adopted from medical and epidemiological literature and applying quasi-experimental methods to population-representative data, this work rigorously examines the impact of risk factors for which policy in the form of regulation is the main institutional instrument on newborns' health and survival. I begin with a chapter that evaluates the introduction of alcohol-related policies in a large metropolitan area in Brazil. The staggered adoption over the area permits identifying the positive causal effects of these policies on fetal survival. The second chapter quantifies the adverse effect of air pollution on newborns' health using the meteorological phenomenon of thermal inversion formation to disentangle the impact of pollution from the role of economic conditions. The third chapter investigates the consequences of immigration enforcement in the U.S. on the birth outcomes of *in utero* children for likely unauthorized families.

A mis padres, a quienes les debo todo.

Contents

Abstract **iv**

List of Tables **ix**

List of Figures **xii**

Acknowledgements **xiii**

Introduction **1**

1 Happier Hours? The Impact of Closing Bars Earlier on Fetal Health **4**

1.1 Introduction 4

1.2 Background 8

1.2.1 Alcohol Consumption and Violence in Brazil 8

1.2.2 Dry Laws in the São Paulo Metropolitan Area 10

1.2.3 Potential Impact of Dry Laws on Fetal Health 14

1.3 Data 19

1.4 Empirical Strategy 20

1.4.1 Fertility and Migration Decisions 21

1.4.2 Empirical Model 26

1.4.3 Sample and Summary Statistics 31

1.5 Results 32

1.5.1 Main Results 32

1.5.2 Comparison with Existing Literature 38

1.5.3 Heterogeneity Analysis 40

1.6 Robustness Checks 41

1.6.1 Time Trends . . . 41
1.6.2 Alternative Specifications . . . 44
1.6.3 Full Term Gestation . . . 46
1.6.4 Neighbor Effects . . . 46
1.7 Exploring Potential Mechanisms . . . 48
1.7.1 Alcohol Consumption . . . 48
1.7.2 Drug Use . . . 52
1.7.3 Violence . . . 53
1.7.4 Workplace Accidents . . . 57
1.7.5 Labor Market Outcomes . . . 59
1.7.6 Family Composition . . . 60
1.8 Conclusions . . . 62

2 Health and the City: Urban Congestion and Air Pollution in Brazil 64

2.1 Introduction . . . 64
2.2 Background Regarding Sources of Air Pollution in Brazil . . . 66
2.3 Potential Health Impact of Air Pollution . . . 67
2.4 Data and Summary Statistics . . . 69
2.4.1 Pollution . . . 70
2.4.2 Weather . . . 71
2.4.3 Thermal Inversions . . . 73
2.4.4 Birth Data . . . 74
2.5 Thermal Inversions, Weather Conditions, and Pollution in Brazil . . . 77
2.6 Estimation . . . 83
2.7 Results . . . 86

2.7.1 Effects of Thermal Inversions on Air Pollution 86
2.7.2 Effects of Thermal Inversions on Infant Health 88
2.7.3 Heterogeneity Analysis 94
2.8 Conclusions 95

3 Heightened immigration enforcement impacts U.S. citizens' birth outcomes 102

3.1 Introduction 102
3.2 Potential Effects of 287(g) Programs on Families 105
3.3 Data 107
3.4 Empirical Strategy 110
3.5 Results 112
3.6 Conclusions 118

Conclusion 121

Appendix A: Chapter 1 123

Appendix B: Chapter 2 130

Appendix C: Chapter 3 138

References 139

Biography 155

List of Tables

1.1 Dry Laws by Municipality 13

1.2 Behavioral Responses: Migration 25

1.3 Characteristics of Municipalities 30

1.4 Variable Means 33

1.5 Effects on Birth Outcomes 35

1.6 Effects on Birth Outcomes by Sex of Newborn 37

1.7 Effects on Birth Outcomes by Race 42

1.8 Robustness Checks: Time Trends 43

1.9 Robustness Checks: Alternative Fixed Effects Specifications and Controls 45

1.10 Robustness Checks: Neighbor Effects 47

1.11 Potential Mechanisms: Car Accidents and Alcohol-related Hospitalizations 51

1.12 Potential Mechanisms: Drug Use 53

1.13 Potential Mechanisms: Violence 56

1.14 Potential Mechanisms: Workplace Accidents 58

1.15 Potential Mechanisms: Layoffs in Bars and Restaurants 60

1.16 Potential Mechanisms: Family Composition 61

2.1 Descriptive Statistics: Pollution, Weather Conditions, and Thermal Inversions 72

2.2 Descriptive Statistics: Health Outcomes 76

2.3 Effect of Thermal Inversions on Pollution: Main Results 89

2.4 Effect of Thermal Inversions on Pollution: Alternative Specifications . 90

2.5 Effect of Thermal Inversions on Infant Health 92

2.6 Effect of Thermal Inversion on Health at Birth: Other Outcomes . . . 97

2.7 Thermal Inversion Strength and Infant Health: Tertiles of Strength . 98

2.8 Robustness Checks: Adding Covariates 99

2.9 Covariates and Inversions 100

2.10 Effect of Thermal Inversions on Infant Health: Heterogeneity Analysis 101

3.1 Descriptive Statistics on Maternal Demographics for Births to Foreign and U.S.-born Parents Residing in North Carolina (Mar to Nov, 2005 and 2006) 113

3.2 Descriptive Statistics on Outcomes for Births to Foreign and U.S.-born Parents Residing in North Carolina (Mar to Nov, 2005 and 2006) . . 114

3.3 Difference-in-Difference Estimates by Parental Foreign Origin and Education 116

3.4 Triple-Differences Estimates by Parental Foreign Origin and Education 118

3.5 Triple-Differences Estimates by Parental Foreign Origin and Education - Results by Subsamples of Control Counties 119

A.1 Effects on Birth Outcomes by Mother's Education 126

A.2 Potential Mechanisms: Car Accidents by Race 127

A.3 Potential Mechanisms: Violence by Race 128

A.4 Potential Mechanisms: Family Composition by Race 129

B.1 Descriptive Statistics by Station: Pollutants 134

B.2 Descriptive Statistics by Station: Weather Conditions 135

B.3 Descriptive Statistics by Station: Wind 136

B.4 Effect of Thermal Inversions on Infant Health: 4-weeks Estimates . . 137

C.1 List of Counties Included in the Analysis 138

List of Figures

1.1 Municipalities Adopting Dry Laws in the SPMA 12

1.3 Behavioral Responses: Fertility and Migration 23

1.5 Assignment of Prenatal Exposure to Dry Laws 28

2.1 Relationships Between PM_{10} and Thermal Inversions 80

2.2 Relationships Between Pollutants and Number of Thermal Inversions 81

2.3 Number of Thermal Inversions in a Week and Weather Variables by Calendar Month 82

A.1 Evolution of Homicide Rate in the SPMA (1996-2005) 125

A.2 Distribution of Stillbirth (2002-2005) 125

B.1 Topography of the SPMA 130

B.3 Relationships Between Weather and Thermal Inversions 131

B.4 Relationships Between Air Pollution and Thermal Inversions Counts . 132

B.5 Relationships Between Air Pollution and Thermal Inversions Strength 133

Acknowledgements

This work owes much to the generosity of many who over the years have patiently read, listened to, supported, and fostered my work.

The fact that my academic experience at Duke University has been outstanding is largely due to Marcos A. Rangel, my advisor. His guidance, honesty, and constant encouragement have made this work possible and helped me overcome many moments of self-doubt. Thank you for your invaluable suggestions and comments, for so many econometric lessons, for being so patient, emphatic, and always willing to talk about my research projects. Thank you for your jokes –even those that I still do not understand. I have learned so much from your example. I aspire to be one day the exceptional economist and mentor that you are. In the meantime, I will remember to bring together theory and data in my applied work.

I am very grateful to Duncan Thomas for his intellectual rigor and relentlessly critical eye. His input and feedback has enriched my learning and growth as a researcher. This work has also benefited enormously from the input of Elizabeth Frankenberg, and Jennifer Lansford. Thank you for your guidance and helpful suggestions and comments. Thanks are also due to seminar and conference participants in Sanford Graduate Research Workshop, Duke Economics International Population, Health, and Development Lunch Group, Informed Choices for Equitable Development Working Group, and PAA, LACEA, SEHO, APPAM, MEA, CEA, IAPHS annual meetings for feedback on earlier drafts of this work. I also want to thank Lisa Kukla and Penny Sanders for all the behind-the-scenes work they have done.

The last five years have been enriched by the friendship and intellectual curiosity of many. They are in no particular order: Emily Pakhtigian, Nivedhitha Subramanian,

Emily Rains, Ajenai Clemmons, Maria Laurito, Marwa AlFakhri, Sarah Nolan, Bobby Harris, Kelly Hunter, and Jessica Gagete Miranda. Their contributions through both formal and informal conversations, lunches and coffees, appear throughout this work. A special mention goes to Marwa, Sarah, and Bobby. Thank you for bringing much needed entertainment, social support, laughter, and your warm companionship in coffee shops, workout classes, dinners, and trips.

I want to recognize the profound influence and support I received from mentors and professors outside of Duke. In particular, a special thank you to Irene Brambilla at Universidad Nacional de La Plata, and to Maria Caridad Araujo, Florencia Lopez Boo, and Marta Rubio-Codina at the Inter-American Development Bank. You are a true role model. Thank you also to Lee McAuliffe Rambo. I could not have written this work in English without her lessons. Many friends have also been incredibly supportive during this process strengthening our relationship despite of the distance. Thanks Yada, Ceci, Iva, Marian, Ailin, Mari, Marce, Lina, Carito, and Ronald for everything.

My deepest gratitude is to the endless support of my family. To my partner, Xavier Carnegie, for his untold patience, continuous encouragement, and editorial eye. You have been my rock through all these years. To my parents, Mimi and José, for encouraging me to pursue my dreams and for their unconditional love, unwavering belief in me, and prayers. Nothing of what I have achieved could have been possible without them. This dissertation is dedicated to my parents and in loving memory of my father.

Introduction

The fetal origins hypothesis suggests that health of a mother during pregnancy presages health outcomes in adulthood. This hypothesis has become popular since the pivotal work of David Barker, and advances in various fields beyond the epidemiology underscore the foundational importance of the early years of life since conception. Yet, estimating causal effects of prenatal conditions is challenging because unobserved characteristics of families and their behaviors are correlated with both maternal and fetal health. The essays comprising this dissertation contribute to this discussion by applying state-of the-art econometric methods to population-representative data to rigorously examine the impact of prenatal exposure to risk factors related to the community, intra-household, and physical environments on maternal well-being and the early-life health.

In the first chapter, I explore the impact of alcohol-related polices. These policies have the potential to address not only medical consequences of substance abuse but also related negative behaviors, including driving under the influence, violent conduct, and risky sexual decisions. Such policies may, therefore, influence the social and intra-household environments surrounding pregnant women and children in their wombs. However, little is known about the broader consequences of alcohol-related policies on this portion of the population. This chapter analyzes the staggered adoption of laws that restrict the hours of operation of bars and restaurants in a major metropolitan area in Brazil. Capitalizing on confidential vital registration data, I account for selective fertility and migration to assess morbidity, exploiting within-family variation in prenatal exposure to the laws. My fixed effects difference-in-differences estimates show that reducing alcohol trading hours increases the probability of having a male

child and does not have a statistically significant impact on the incidence of low birth weight and preterm birth. Taken together, these results are compatible with a reduction in fetal deaths. Consistent with this hypothesis, I show that the laws caused increased survival rates of less healthy male fetuses, which helps to explain the null effects on birth outcomes. Results are shown robust to the choice of specification regarding seasonality and time effects. I provide extensive auxiliary evidence detailing potential mechanisms behind my main results.

In the second chapter, I study the effect of air pollution. Congestion of urbanized centers is a trademark accompaniment of economic development across the globe. These economic forces pose a challenge with respect to environmental conditions and, therefore, welfare for populations in those areas. Nevertheless, the evidence to inform policy-makers in the developing world is still scarce. In this chapter, I take advantage of the meteorological phenomenon of thermal inversion which in urban areas arguably exogenously lock pollutants closer to the ground to estimate the causal effects of pollution on infants health at birth. I employ detailed data from birth records around the metropolitan area of São Paulo, Brazil, between 2002 and 2009. Results confirm a positive relationship between thermal inversions and several air pollutants. I find that exposure to inversion episodes during the last three months of pregnancy reduces birth weight and fetal survival, and increases the incidence of preterm births.

The third chapter is a collaborative work with Marcos A. Rangel, Christina Gibson-Davis, and Laura Bellows. We examine how increased Immigration and Customs Enforcement (ICE) activities impact the health of pregnant mothers and their infants in North Carolina around the time Section 287(g) of the Immigration and Nationality Act was first being implemented in the state. Focusing on administrative data between 2004 and 2006, we conduct difference-in-differences and triple-differences case-control regression analysis. Pregnancies are classified by levels of

potential exposure to immigration enforcement depending on parental country of birth and education level. Contrast groups are foreign-born parents residing in non-adopting counties and all U.S.-born non-Hispanics. We find that the introduction of the policy decreases birth weight between 58 and 63 grams with intra-uterine growth effects also manifesting themselves within the first three months of the new regime. These results coexist with a worsening in the timing of initiation and frequency of pre-natal care received. Our findings inform policies that address disadvantages generated before birth and quantify the impact of policies targeting unauthorized populations over their U.S. born children.

Chapter 1

Happier Hours? The Impact of Closing Bars Earlier on Fetal Health

1.1 Introduction

Alcohol abuse is a global public health concern. The consequences of excessive alcohol consumption are numerous and well-known. These include direct effects—such as liver cirrhosis and fetal alcohol syndrome—and indirect repercussions- violence and traffic accidents, for instance. Alcohol-related policies have the potential to mitigate these issues. Apart from having a direct effect on drinkers' health, these policies influence community and intra-household dynamics, leading to subtler effects for families. In particular, pregnant women and children *in utero* are likely to be affected in significant and long-lasting manner. These broader consequences of alcohol-related policies, and of those changing access to alcohol in particular, are mostly unknown by policy makers and have received little attention from the literature. The objective of this article is to help fill that gap.

In this chapter, I study the unanticipated impact on newborns' health of a particular type of alcohol related policy: restricting hours of operation of on-premise outlets.[1] For identification, I exploit the introduction of "dry laws" across municipalities in the São Paulo Metropolitan Area (SPMA), Brazil. These laws were adopted in 18 municipalities following a staggered implementation between 2001 and 2005, and they established mandatory closing hours for bars and restaurants. My empiri-

[1] On-premise outlets are places in which alcoholic beverages are consumed where purchased, such as bars and restaurants. Off-premise outlets refers to settings that sell alcohol for consumption elsewhere (for example, liquor stores, supermarkets, and kiosks).

cal strategy employs this geographic variation in the timing of adoption of the laws using a difference-in-differences approach. Documenting the health consequences of such a change, however, presents clear challenges associated with families' behavioral responses to the laws. I directly address them in my analysis.

I find that *in utero* exposure to curtailing alcohol trading hours does not significantly affect the incidence of low birth weight and preterm birth. These results may lead to conclude that dry laws do not affect fetal health, yet they are also compatible with a reduction in fetal losses. Consistent with this latter, I find that the probability of having a male child—a measure of sex-specific fetal survival—is 17% greater among mothers exposed to the laws during the last 3 months of their pregnancy. This increase in fetal survival seems to be driven by relatively lower fetal deaths among less healthy male fetuses explaining the lack of significant effects on low birth weight and prematurity. Results are robust to the choice of specification and to a variety of alternative checks for time trends, seasonality, and spatial spillover effects. My subsample analysis shows that black mothers are more affected by the introduction of the laws than white mothers. This suggests that these dry laws play a role in reducing the black-white fetal mortality gap.

To address the concern that families may respond to the laws by adjusting their fertility and/or migration decisions and that these behavioral responses may be related to mothers' observed and unobserved characteristics, I capitalize on confidential vital registration data. I identify siblings and account for migration decisions using mothers' residential location before the laws were implemented. Focusing on infants conceived before the introduction of these dry laws, I estimate the effect on health at birth, exploiting within family variation in prenatal exposure to the laws. Because residential location and adoption of dry laws are not random events, my within-family approach has the advantage of eliminating any bias due to unobserved family char-

acteristics that are common across siblings. By using data on siblings, none of whom were exposed to dry laws, I am also able to account for events that coincide with the adoption of these laws. Semi-parametrically, I control for seasonality and time trends that exist across siblings independent of the adoption of the laws.

I leverage additional administrative datasets to investigate a number of possible mechanisms driving my findings. I provide evidence that in the nine-month period after the adoption of the laws, alcohol consumption, assaults, and workplace accidents decrease, and there is no significant substitution for alcohol consumption of other drugs. Effects on these outcomes are found for men and women and across races, with a larger reduction of homicides for black men. Because women are more likely to be victims of violent assaults in the sphere of their domestic relationship while men are more likely to be murdered outside their family residence, my findings suggest that both community and domestic violence decrease as a result of the dry laws. The restriction in hours of operation of bars and the consequent decrease in the rate of assaults and driving under the influence seems to translate into a change in family composition: it is more likely that a woman has a partner when her child is born. While all these channels indicate positive shocks to pregnant women, changes in labor market outcomes for those working at bars or restaurants may have the opposite effect. I show that there is no evidence of changes in layoffs in bars and restaurants after the adoption of the dry laws. Overall, these findings point to alcohol consumption, violence, and changes in family composition as three likely channels affecting newborns' health.

A limited amount of research has examined whether restricting access to alcohol benefits infants' health at birth even though much more is known about the consequences of shocks to the physical and mental health of mothers-to-be on the health of their infants (Barker and Osmond (1986); Almond and Currie (2011) and Almond

et al. (2018) review the economic literature). These studies focus on relaxing the minimum legal drinking age (MLDA) (Fertig and Watson, 2009; Barreca and Page, 2015) and increasing off-premise sales to women under 21 years old (Nilsson, 2017). They find that increasing alcohol availability among young mothers has a small negative effect on children's outcomes at birth. To the best of my knowledge, this study is the first to study the effects of changes in on-premise trading hours on newborns and to examine a change in access to alcohol that affects women across age groups. This chapter also contributes to understanding the role of potential mechanisms that help interpret the effect of alcohol-related policies on health at birth. Unlike other alcohol-related policies targeting access, changing the hours of operations of bars and restaurants presents certain particular characteristics: it affects access to alcohol as well as social gathering and aggressive behaviors that are exacerbated by public intoxication. Thus, this study not only contributes to the literature in economics exploring the "fetal origins" hypothesis, that argues that adverse environmental conditions *in utero* can cause disruptions to development that may exert life-long health effects, and fetal survival but it also relates to the broader literature spanning many disciplines on the impact of maternal stress and violence on newborns' health (for example, Foureaux Koppensteiner and Manacorda (2016), Brown (2018), Currie et al. (2018), and Black et al. (2016)). The overall analysis of the effect of the dry laws implemented in the SPMA also contributes to the debate surrounding alcohol policies. Considering that prenatal experiences can have consequences for outcomes later in life, the potential for long-term improvements in maternal and infant well-being should be considered in the cost-benefit analyses of those policies.

1.2 Background

This study focuses on policies enacted in the early 2000s in Brazil. Two related characteristics of the context that motivated the adoption of dry laws are high consumption of alcohol and high levels of violence. This section describes both of them before introducing the laws in details and summarizing potential links between the laws and fetal health.

1.2.1 Alcohol Consumption and Violence in Brazil

In the early 2000s, as well as in the next decade, alcohol consumption was the main risk factor for ill-health in the Americas, and in particular in Brazil, ranking higher than tobacco, obesity, and lack of sanitation (Rehm and Monteiro, 2005; Shield et al., 2015). The average consumption of pure alcohol per adult in Brazil was 8.6 liters/year in 2000, while the worldwide average consumption was 5.8 liters (Rehm and Monteiro, 2005; Caetano and Laranjeira, 2006).

In Brazil, low social stigma of alcohol is associated with its high consumption. For instance, beer was considered as a "soft drink," and it could be advertised in prime time television slots (Kerr-Correa et al., 2005).[2] Moreover, alcohol was inexpensive, easy to accessy, and there were no general religious restrictions to drinking (Romano et al., 2007; Caetano and Laranjeira, 2006).[3] Lastly, hours of operation of bars were not restricted; they typically remained open 24 hours-a-day and worked on a "last

[2]The federal law that regulates advertising considers beverages with less than 13% of alcohol content as food (Kerr-Correa et al., 2005). Pinsky et al. (2010) show that exposure to alcohol advertising among adolescent and young adults is more prevalent (at least daily) than exposure to any kind of prevention message. Studies reviewed by (Burton et al., 2017) summarize a positive association between exposure to alcohol advertising and an increased likelihood that children will start to drink or will drink greater quantities if they already do.

[3]A 350 ml can of beer costed less than a can of cola or a bottle of milk ($0.20, $0.40, and $0.90, respectively), and access to alcohol was easy even for those below the minimum legal age to buy and drink alcohol, 18 years old (Caetano and Laranjeira, 2006). Regarding religion, according to the 2000 Census, almost 80% of the population in Brazil is Catholic.

client served" basis (Duailibi et al., 2007; Biderman et al., 2010). In sum, advertising, pricing, and availability are factors contributing to a high per capita consumption of alcohol in Brazil (Caetano and Laranjeira, 2006) and have helped to make bars one of the chief environments in which young people socialize (**?**).[4]

Patterns of alcohol consumption in Brazil differ by gender and age (Laranjeira et al., 2007).Adult men are more likely to drink frequently and to drink more than women. Binge drinking is a common practice for 3 out of 10 people and mostly among unmarried men and those with higher incomes (Castroand et al., 2012). Moreover, people under 35 years of age are more likely to drink than those over 35 (Laranjeira et al., 2007). Regarding the incidence of alcohol consumption during pregnancy, there is no a clear consensus. A meta-analysis based on 17 studies published between 2004 and 2014, shows that reported consumption of alcohol during pregnancy varies from 2% to 40% (Lange et al., 2017). The wide range of incidence is explained by the use of diverse measures of drinking, timing of the measure, and selected samples of women. The stigma of the diagnosis of alcoholism, and possible legal repercussions, such as the loss of child custody, contribute to the difficulties in accurately estimating and characterizing alcohol intake during pregnancy (Segre, 2010; Baptista et al., 2017).

A high-level of violence, and, in particular, violence related to alcohol was another concern at the beginning of the twenty-first century in Brazil. In 2000, the mortality rate of intentional injuries was, approximately, 29.6 per 100,000 inhabitants—a rate higher than all other countries in the Americas except for Colombia and El Salvador (WHO, 2004). Specifically, in the SPMA, in the late 90s, around 40,000 people were murdered. That is a monthly rate of 4.7 people per 100,000 inhabitants. For a reference point consider that in New York City the rate was 3.56 in its peak in 1990. Homicides dropped between 2000 and 2005; this drop was much faster around the

[4]In 2001, 50% of the sales of beer were in bars (Ferrari, 2008; Laranjeira et al., 2007).

year 2002. Figure A.1, summarizes trends in homicides over time using data provided by DATASUS.

Unsurprisingly, the level of homicides and alcohol consumption were related (Room et al., 2005; Leonard and Quigley, 2017). For example, in Diadema, a municipality in the SPMA, police reports indicate that 65% of homicides in 1999 were alcohol-related; the victim and/or the perpetrator had alcohol in their bloodstream (Duailibi et al., 2007). Furthermore, 60% of homicides and 45% of complaints about violence against women in this municipality occurred between 11 p.m. and 6 a.m. in neighborhoods with a high concentration of bars (Duailibi et al., 2007). A study requested by the *Secretaria de Segurança Pública do Estado de São Paulo* confirmed that consumption of alcohol was one of the main factors associated with homicides in the SPMA (Moura, 2012).

1.2.2 Dry Laws in the São Paulo Metropolitan Area

As a response to the high rates of crime, different levels of government in Brazil introduced new policies. For instance, federal legislation on firearms possession (the *Lei do Desarmamento*) was implemented in December of 2003, and a system that improved police intelligence at the state level (INFOCRIM) was introduced in January of 2000. Improving public safety was also understood to be the responsibility of the municipalities in the SPMA at that time (Moura, 2012).[5] Aiming to reduce community violence, some municipalities of the metropolitan area implemented laws

[5]During the 1990s, the state government bore the main responsibility for public safety (Moura, 2012). The *Plano Nacional de Segurança*, implemented in 2000, was the first of several initiatives that involved the transfer of resources to municipalities to deal with public security encouraging local governments to be more involved in this area. Yet, the federal government manages police officers.

that limited the time of operation for bars and restaurants.[6] [7]

These dry laws did not attempt to ban alcohol consumption; they focused on the circumstances in which the effect of alcohol could be harmful (Moura, 2012). Overall, they established closing hours for bars and restaurants each night of the week and penalties for non-compliance. Closing hours marginally differ by municipalities: starting at 11 pm or midnight to 5 am or 6 am. Some municipalities also allow special times during weekends and holidays, yet with the latest closing time at 2am. Fees and consequences of failing to comply with the laws vary marginally by municipalities. Penalties increase with the number of relapses, going from a monetary fee up to revocation of the operating license of the establishment. In total, 18 cities in the SPMA adopted dry laws. Figure 1.1 shows their geographical distribution. Implementation of these laws was staggered over time between 2001 and 2005. Table 1.1 lists adoption dates and summarizes closing hours implemented in each municipality.

The adoption of these dry laws implied a significant reduction in the hours of operation of bars and restaurants (by at least 5 hours). In high-income countries, changing hours of operation in on-premises settings by at least 2 hours has been positively associated with changes in excessive alcohol consumption and alcohol-related harms (see Hahn et al. (2010) for a review of several studies). One concern in developing countries is lax enforcement efforts, which may jeopardize potential benefits of the laws. However, an ethnographic study of the dry laws implementation in the

[6] Laws are available at https://leismunicipais.com.br.

[7] High rates of violence against women, especially perpetrated by family members, motivated other initiatives at the federal level. In 2003, the Brazilian government established a new unit, the Secretaria de Políticas para Mulheres (SPM), as part of the Ministry of Women, Family and Human Rights, and in 2005 the first National Plan of Women's Policies was created. A year later, the Maria da Penha law was enacted. As domestic violence became a national priority, there was an expansion of women's police centers (Delegacias Especializadas de Atendimento das Mulheres, DEAMs). These units, primary staffed with women, are a part of the Civil Police focusing on crime against women. Their expansion was pronounced after 2005, and by 2009 there were DEAMs in 500 municipalities around the country (Perova and Reynolds, 2017).

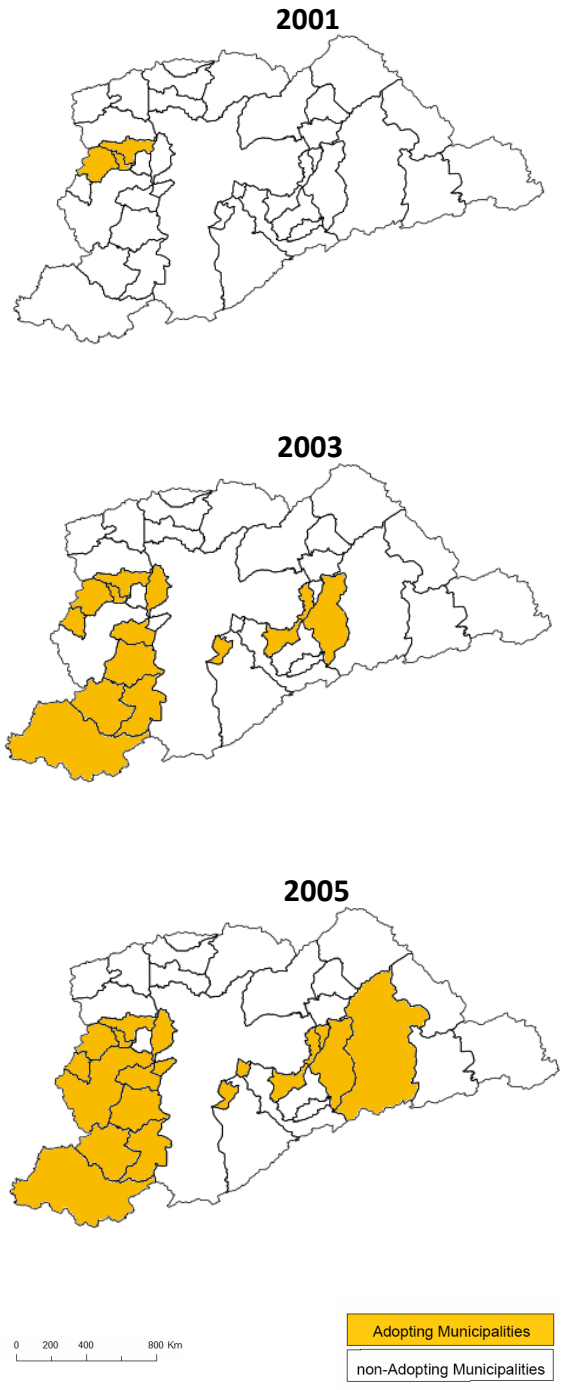


Figure 1.1. Municipalities Adopting Dry Laws in the SPMA

SPMA (Moura, 2012) provides evidence of the coordination and enforcement strate-

gies followed by some municipalities.

Table 1.1. Dry Laws by Municipality

Municipality	Date of Adoption	Closing Hours
Barueri	Mar-2001	11pm-6am all week
Jandira	Aug-2001	11pm-6am all week
Itapevi	Sep-2001	11pm-6am all week
Diadema	Mar-2002	11pm-6am all week
Juquitiba	May-2002	11pm-6am weekdays, 2am-6am Fridays, Saturdays, Sundays, and Holidays
Sao Lourenco da Serra	Jun-2002	11pm-6am all week
Suzano	Jun-2002	11pm-5am all week
Itapecerica da Serra	Jul-2002	11pm-6am all week
Mauá	Jul-2002	11pm-6am all week
Ferraz de Vasconcelos	Sep-2002	0am-6am all week
Osasco	Nov-2002	0am-5am all week
Embu das Artes	Dec-2002	11pm-5am all week
Embu-Guacu	Apr-2003	11pm-6am weekdays, 1am-6am Fridays and Saturdays, 0am-6am Sundays and Holidays
Vargem Grande Paulista	Dec-2003	0am-5am all week
Sao Caetano do Sul	Jul-2004	11pm-6am weekdays, 0am-6am Fridays, Saturdays, Sundays, and Holidays
Poa	Aug-2004	11pm-6am all week
Mogi das Cruzes	Jan-2005	0am-5am all week
Cotia	May-2005	11pm-5am all week

Notes: Municipal laws are available at https://leismunicipais.com.br

It has been documented that dry laws in the SPMA led to a reduction of household alcohol consumption and homicide rates (Biderman et al., 2010). A comparison of municipalities in and outside the SPMA (excluding the city of São Paulo) using the 1995 and 2003 waves of the Brazilian household expenditure survey (*Pesquisa de Orçamento Familiar*) suggests that, on average, households decreased their monthly consumption of beer by R$28 (70% of the average bar consumption) and their monthly consumption of cachaça by R$2.2 (58% of the average bar consumption) (Biderman et al., 2010). The reduction in bar consumption of beer seems to be only partially substituted by consumption of alcohol at stores: households' grocery purchases of beer increased by R$11. A difference-in-differences strategy that compares adopting and non-adopting municipalities between 1999 and 2004 indicates that the rate of

homicides per 100,000 inhabitants dropped by 10% after the introduction of the laws (Biderman et al., 2010). In Section 1.7, I complement these findings by studying the effects of dry laws right after their adoption (to provide a comparable analysis with the timing included in my main analysis) on alternative measures of alcohol consumption and violence desegregated by gender and race. I expand the analysis by looking at other potential channels: workplace accidents, drug use, changes in family composition, and layoffs at bars and restaurants.

1.2.3 Potential Impact of Dry Laws on Fetal Health

Time *in utero* is a critical period in the development of children. A growing body of literature based on the fetal origins hypothesis has shown that perturbations during this period have lifelong "scarring" consequences. Insults during pregnancy may affect fetal health and translate into short and long term consequences (Barker, 1990; Barker and Osmond, 1986; Almond and Currie, 2011; Black et al., 2007; Figlio et al., 2014). *In utero* survival (or selection) may also be affected as a result of environmental stressors (Bruckner and Catalano, 2018). Both "scarring" and selection shape cohort health; they may occur simultaneously but at different points of the gestational distribution of frailty. The relative effect of each is still an open question. In this chapter I focus on both.

Alcohol-related policies in general, and restricting hours of operation of on-premise outlets in particular, may affect fetal health and survival through many channels such as changing alcohol consumption, drug use, and violence. Previous studies have established a link between alcohol-related policies and these pathways (for a comprehensive review of alcohol-related policies see Cook (2007)). For instance, higher alcohol availability through increasing the number of outlets (Anderson et al., 2018; Pridemore and Grubesic, 2013; Gyimah-Brempong and Racine, 2006; McKinney et al., 2009),

extending the time for selling alcohol (Hahn et al., 2010; Heaton, 2012; Grönqvist and Niknami, 2014; Middleton et al., 2010) and providing legal access (Carpenter and Dobkin, 2009, 2015; Carpenter et al., 2016) has been linked to higher level of community violence, partner violence, driving under the influence, and motor vehicle accident mortality. Conversely, studies have shown that limiting hours of operation at on- or off-premise outlets (Marcus and Siedler, 2015; Wilkinson et al., 2016), banning alcohol (Luca et al., 2015) and increasing alcohol taxes (Cook and Durrance, 2013) leads to a reduction of violent crime and violence against women. Changes in the minimum drinking age (Barreca and Page, 2015; Fertig and Watson, 2009) and alcohol availability at off-premise stores (Nilsson, 2017) were also linked to changes in fertility for young mothers. The use of other drugs as a complement or a substitute to alcohol (for instance, cigarettes and marijuana) can also be affected by changing access to alcohol (Fertig and Watson, 2009; Yörük and Yörük, 2011). Alcohol policies and their consequences may affect mothers-to-be directly and, also, indirectly by, for example, modifying complementary smoking, nutritional decisions, and the use of money and time (for example, sleeping more) in a way that also influences newborns' health. Drinking and violence may also affect family composition.[8]

Documenting the causal effects of these stressors on health at birth has been challenging. Some studies have examined the impact of violence, maternal stress, and nutrition on newborn's health. Overall, these articles emphasize the negative effects of these perturbations. I review them in Appendix A, section 3.6. Regarding alcohol consumption, there is substantial evidence documenting the association of heavy maternal drinking during pregnancy with poorer infant health at birth (forexample, a

[8]It is unclear the direction of these changes. On one hand, people may respond to restrictions in alcohol availability by spending less money on alcohol consumption, and purchasing better quality food or sleeping more. Reducing alcohol availability may also reduce traffic fatalities and workplace accidents, and deter family disruption. On the other hand, men may spend more time with their partners increasing the likelihood of domestic violence.

higher risk of preterm delivery and lower weight, early childhood cognitive ability, and behavior (Jones and Smith, 1973; Albertsen et al., 2004; Whitehead and Lipscomb, 2003). Studies also suggest that even low and moderate drinking may be detrimental to newborn's health (Russell, 1991). A critical time period for exposure has yet to be demonstrated. Establishing a causal link of maternal drinking on newborns' well-being has been unfeasible. A randomized-control trial to study maternal drinking is not possible for obvious ethical reasons.

Aiming to understand the effects of alcohol abuse and its consequences on newborns' health, a handful of studies have focused on the overall causal impact of alcohol policies on newborns. These papers have studied changes in the minimum legal drinking age (Fertig and Watson, 2009; Barreca and Page, 2015), off-premise sales of "strong beer" for people under the age of 21 (alcohol content of 5.6% by volume, Nilsson, 2017), and alcohol taxes (Zhang, 2010). They find that easier access to alcohol leads to selection *in utero*. Consistent with the claim that male fetuses are more vulnerable to environmental stressors than females, Nilsson (2017) shows a reduction in the fraction of male births of 7.2 percentage points; Barreca and Page (2015) find a 0.18 percentage point increase in the probability of giving birth to a female child. As expected, based on the effect on fetal survival, results for other birth outcomes are negligible or small (Barreca and Page, 2015; Fertig and Watson, 2009). In addition, a one-cent increase in alcohol taxes decreases the likelihood of low birth weight by 0.1-2.0 percentage points (Zhang, 2010).

Overall, previous studies looking at alcohol access and infants focus on young mothers who live in developed countries. To the best of my knowledge this study is the first study looking at the effects of changes in on-premise trading hours on newborns and the first focusing on a developing country. Unlike the other alcohol policies, changing the hours of operations of bars and restaurants presents some

particular characteristics: it directly affects on-premise alcohol consumption, social agglomeration and potentially aggressive behaviors that are exacerbated by public intoxication and people regardless of their age.

The impact of alcohol policies on fetuses' health may be captured in three widely studied markers: low birth weight (less than 2,500 grams or 5.5 pounds), prematurity (less than 37 weeks of gestation), and male sex (a measure of selection *in utero* given that male fetuses are less resilient to poor early conditions than females). Low birth weight reflects intrauterine nutritional status and has been associated with outcomes at various stages of the life cycle such as lower test scores (Currie and Hyson, 1999), lower educational attainment (Currie and Moretti, 2007), adult health issues including diabetes and heart diseases (Barker, 1995), and labor market outcomes (Black et al., 2007). The fetus develops in mass and weight mostly during the second half of pregnancy; external shocks during that time may have larger effects on birth weight. Prematurity (being born too early) and intrauterine growth retardation (IUGR, growing too slowly in the womb) are the two main causes of low birth weight.[9]

Preterm birth is the leading cause of infant mortality and morbidity, and it increases the risk of reduced cognitive test scores (Bhutta et al., 2002). Multiple mechanisms are thought to interact to cause preterm labour, including infection or inflammation, uteroplacental ischaemia or hemorrhage, uterine overdistension, and stress (Goldenberg et al., 2008).[10] At least 25-40% of preterm births may be associated

[9]The diagnosis of IUGR is assigned to infants with a birth weight below the 10th percentile for gestational age. Infants' growth is restricted *in utero* due to maternal, fetal or placental pathology. IUGR determinants include factors such as maternal undernutrition, and infections that prevent normal circulation across placenta precluding the fetus to receiving nutrients and oxygen (Alderman, 2006). I do not explore the effects of dry laws on IUGR because the date of conception or exact gestational age to backdate conception is not available in the data I use.

[10]The exact mechanism by which prenatal stress affects fetuses is still unclear. Heightened cortisol levels may affect the fetal physiology (Barbazanges et al., 1996); maternal hormonal imbalance stimulates the placenta to produce corticotrophin-releasing hormones that may jeopardize the central nervous system and brain development (Majzoub and Karalis, 1999; Meaney et al., 1996). Additionally, stress reactivity may constrain uteroplacental blood flow, reduce nutrient transmis-

with intrauterine infections (Kramer et al., 2012), but it is not clear when the infection happens (Goldenberg et al., 2000). Maternal risk factors include multiple gestation, substance abuse, maternal malnutrition, age below 20 or above 35 years old, low socioeconomic and educational status, and black race (Goldenberg et al., 2008).

Sex ratio in live births (or secondary sex ratio) is the ratio of males to females at birth. This is another measure of fetal health. Specifically, it indicates sex-specific selection *in utero*. Male and female fetuses have different rates of survival, and male fetuses are more at risk than females (Trivers and Willard, 1973; Wells, 2000; Kraemer, 2000). Through gestation, deaths are male-biased at the beginning and later in the pregnancy (Orzack et al., 2015). Female fetuses suffer selection associated with chromosomal and genetic abnormalities; males fragility signals as small for gestational age, and it may grow stronger as gestation proceeds (Bruckner and Catalano, 2018). The selection *in utero* against males seems to disproportionately affect frail fetuses (Bruckner and Catalano, 2018). Maternal stressors (e.g. prenatal alcohol exposure, stress, and poor nutrition) affect male fetuses' survival more negatively as compared to females (May et al., 2017; Almond and Mazumder, 2011; Catalano et al., 2006; Sanders and Stoecker, 2015). Mechanistic causes of male-specific fetal loss remain poorly established (Bruckner and Nobles, 2013).

sion to the fetus, and induce fetal growth restriction (Teixeira et al., 1999). Lastly, corticotrophin-releasing hormones also play a role in initiating labor, increasing the chance of premature births (Hobel et al., 1999; McLean and Smith, 1999). Stress may also lead to poor nutrition and coping behaviors such as smoking and reduction of preventive or curative health services, exacerbating negative conditions for fetal development.

1.3 Data

Data are individual-level birth records provided by the Brazilian Ministry of Health's Usage Information System, DATASUS, for all births in the SPMA. These records have a high coverage rate that exceeds 98% in the mid-2000s (Jorge et al., 2007).

The study focuses on singleton children born in hospitals between 2002 and 2006 in 37 municipalities in the SPMA. My analysis looks at the period between 2002-2006 because these are the years covered by identified data. I exclude from the analysis two municipalities in the SPMA, Taboao da Serra and São Paulo City because of their previous attempts to establish laws similar to the dry laws I study. The laws implemented in these municipalities had a different goal (noise control) and a relative lack of enforcement (Cardoso, 2017).

The vital registration system in Brazil provides information about newborns' health, race, their mothers' age, marital status, education and parity, municipality and date of delivery, and municipality of residence.[11] I include in the analysis births with non-missing information on the date of birth, municipality of residence, birth weight, and gestational length. Moreover, I exclude observations that correspond to birth with less than 27 gestational weeks because they have much worse outcomes than other infants.[12] The full sample of all births consists of 548,666 infants.

Because I use specifications with mother fixed effects, I include in my analysis sample mothers with at least two births between 2002 and 2006. To identify siblings, I use mother and father's complete names. I confirm the siblings matches using information on each child's exact birth date, mothers' age and parity at each birth. Moreover, I restrict the analysis to mothers who give birth to up to three children between 2002

[11] Race is coded only for children. I use that to proxy for mother's race despite the fact that mixed race families are widespread in Brazil.

[12] These restrictions result in dropping less than 1% of the observations and do not have any impact on my findings.

and 2006. The resulting sample of all siblings consists of 57,963 newborns.

I study the impact of the dry laws on three main outcomes: low birth weight (birth weight below 2,500 grams), prematurity (<37 gestational weeks), and the probability of giving birth to a male child. These outcomes provide information about fetal health and fetal loss. Like other natality data, Brazilian birth certificates do not include the date of conception and code gestational age at birth in categories. Thus, I assign exposure timing based on a nine-month conception period prior to the date of birth.

1.4 Empirical Strategy

I estimate the effect of a restriction in trading hours of bars and restaurants on children's outcomes at birth. Comparing newborns' outcomes for women who reside in an adopting municipality while pregnant to those for women who were not exposed to the dry laws while pregnant may lead to inconsistent estimates of the effect of the introduction of the laws on health at birth. Adoption of dry laws was not random; families' location of residence is not random as well. Moreover, families' decisions about whether and when to have a child and their place of residence may be affected by the adoption of dry laws. Unobserved characteristics of families may be correlated with their responses in terms of fertility and migration, and their newborns' health. Considering these potential behavioral responses, I first describe their relevance in this setting, and building on these results I construct my empirical strategy. I discuss alternative specifications addressing potential threats to identification and other robustness checks in Section 1.6.

1.4.1 Fertility and Migration Decisions

The adoption of dry laws may influence families' fertility behavior. Previous research has shown that changes in access to alcohol affect risky sexual activity, unintended pregnancies, and birth rates (Dee, 2001; Fertig and Watson, 2009). Families may also respond to the introduction of these laws by moving towards or away from an adopting municipality, akin to a "voting with your own womb" model of public goods provision. These behavioral responses may lead to changes in parental composition of births. Those responding to introduction of the laws may differ from other parents in characteristics that are also related to fetal health, and estimates of the effect of the dry laws on health at birth would be biased. Differential behavioral responses have been documented as a consequence of hurricanes and community violence (Currie and Rossin-Slater, 2013; Brown, 2018).

Employing the full sample of births aggregated at the municipality-week level, I examine whether the introduction of dry laws in the SPMA affects fertility and migration decisions by estimating the following regression:

$$Log(Births)_{jwmy} = \sum_{t=1}^{36} \theta_t DL(t \text{ months before birth})_{jwmy} + \gamma_{ym} + \gamma_j + \epsilon_{jwmy} \quad (1.1)$$

where cohort size is measured by the logarithm of the number of births in week w in month m of year y among women who reside in municipality j ($Log(Births)_{jwym}$). To explore the change in parental composition, I consider three additional outcomes: the proportion of the total number of children in week w who are born to black women, mothers with 11 or fewer years of education, or mothers aged less than 20 years old. γ_{ym} are year-by-month fixed effects, and γ_j are municipality of residence at birth fixed effects. Regressions are weighted by the number of newborns in each municipality-week cell.

$DL(t \text{ months before birth})_{jwym}$ are indicator variables that take value 1 if t months before week w a dry law was adopted in municipality j. I include 36 indicators and use data for the period 2002-2009 to cover up to 36 months after the adoption of the laws in adopting municipalities. Estimates recover a combination of biological (*in utero* mortality and prematurity) and behavioral (fertility and migration) effects of the dry laws.[13] Since the estimators for the coefficients in a month-by-month specification are noisy, I present in Figure 1.3 a linear combination (average) of three adjacent coefficients. That is, the figure reproduces for every t the estimate for the quantity represented by: $(t + t\text{+}1 + t\text{+}2)/3$.

Graph (a) in Figure 1.3 shows the impact on cohort size; graphs (b) to (d) present effects on parental composition (mother is black, lower educated, and less than 20 years old, respectively). Overall, these figures indicate that the number of births does not change in the first year and a half after the adoption of the laws in adopting municipalities compared to non-adopting ones, and it declines after that. That change is accompanied by a reduction in the proportion of newborns whose mothers are black and lower educated. The adoption of the laws does not affect the composition of mothers in term of their age. These findings suggest that not accounting for selective fertility and migration would lead to a biased estimation of the effect of restricting hours of operations of bars and restaurants on health at birth. The direction of the bias is not clear. On one hand, being black and less educated may indicate low socio-economic status and worse health related outcomes. On the other hand, these mothers may be more affected by the dry laws.

A way to remove selective fertility effects is to focus on infants who were conceived before the adoption of the dry laws. I further explore behavioral responses (and

[13] Fertility responses include planning but also any biological changes on conception. Stress decreases sperm motility and the frequency of coitus and, therefore, conception (See Catalano et al. (2005) for a review of the literature).

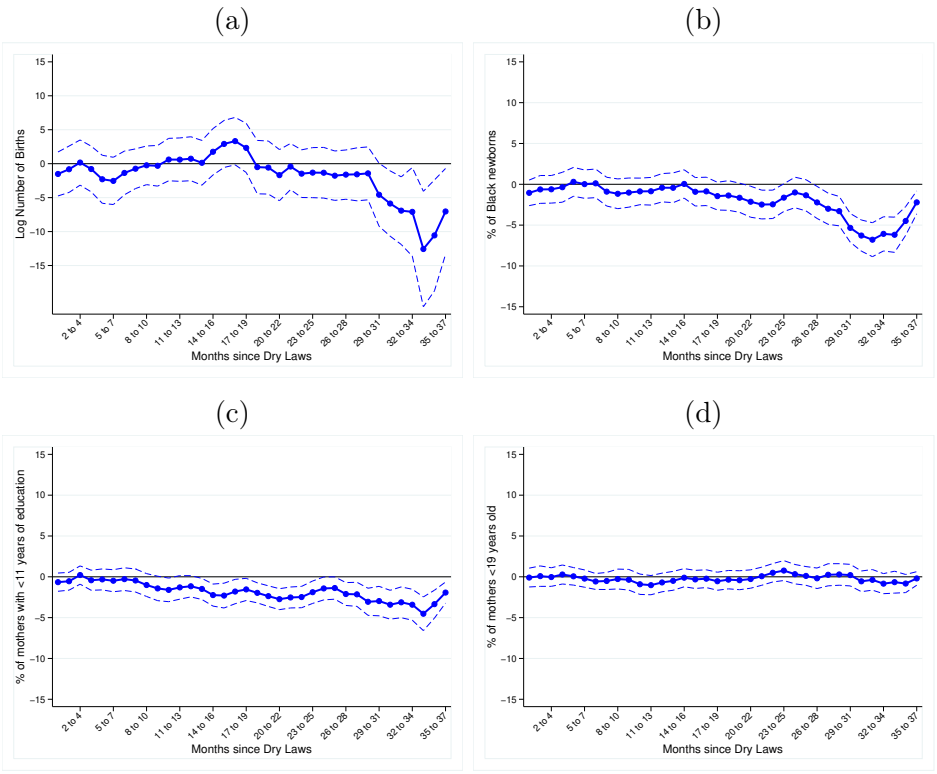


Figure 1.3. Behavioral Responses: Fertility and Migration

Notes: Rolling three month averages of the impacts of the dry laws on (a) cohort size, (b) proportion of births who are black, (c) proportion of births of low educated mothers (0-11 years of education), and (d) proportion of births of young mothers (less than 20 years old). Observations are at the municipality-week level. Cohort size is measured by the logarithm of the weekly number of births in each municipality. Dashed lines indicate confidence intervals.

biological effects) by looking at children born up to 38 weeks after the laws were introduced and estimating the following model,

$$Move_{fjym} = \delta_1 DL_{fjym} + \delta_2 DL_{fjym} * X_{fjym} + \delta_3 X_{fjym} + \gamma_{ym} + \gamma_j + \epsilon_{fjym} \qquad (1.2)$$

where $Move_{fjym}$ is the probability that a mother f moves to another municipality between births and DL_{fjym} indicates whether a dry law was adopted in her municipality of residence during her last pregnancy in the period 2002-2006 and 0 if a mother was not exposed to the laws. To construct these variables, I restrict the analysis to the

sample of siblings excluding mothers who were exposed to the laws during their first pregnancy in the period of analysis. The model includes municipality fixed effects (γ_j), year-by-month of birth fixed effects (γ_{ym}), and a vector (X_{fjym}) with the following controls: mother's age (20-24, 25-29, 30-34, or at least 35 years old), education (12 or more years), race (white or other), and parity. The omitted categories are mother is less than 20 years old, mother's education is below 12 years, and black. To asses differential responses by maternal characteristics, I also add to the model the interaction between the vector of controls and the indicator for exposure to the laws.

Estimates from Eq. (1.2) recover behavioral effects related to migration as well as biological-related effects. Table 1.2 presents these results. The model in column (1) only includes the indicator for dry laws, in column (2) the model adds fixed effects and controls, and the last column presents the complete model including interaction terms. The table suggests some evidence of selective migration (or biological effects) as a response to the dry laws. For instance, the likelihood of moving decreases for more highly educated mothers compared to those with lower education when they are exposed to the laws. Migration decisions seem to vary by age too: mothers aged 20-24 are more likely to move between pregnancies than younger mothers, yet the likelihood decreases and is not statistically significant for older mothers.

To deal with the endogeneity of maternal location, I define timing and length of exposure to dry laws based on the municipality where a mother resides when her first child in my sample was born. This is measuring the child's prenatal exposure to the laws as if the mother had remained in the municipality she reported in her first pregnancy. For those mothers who were exposed to the laws during their last pregnancy in my period of analysis (or were not exposed to them), their first municipality of residence was defined before the introduction of the laws. In this way, while a mother's current location might be endogenous, her residence during her first

Table 1.2. Behavioral Responses: Migration

	Move between pregnancies		
	(1)	(2)	(3)
Dry Laws during second pregnancy	-0.005	-0.005	-0.013
	(0.025)	(0.022)	(0.032)
Dry Laws * Mother's age 20-24			0.033*
			(0.017)
Dry Laws * Mother's age 25-29			0.036
			(0.024)
Dry Laws * Mother's age 30-34			0.019
			(0.024)
Dry Laws * Mother's age 35+			-0.003
			(0.031)
Dry laws * Mother is white			-0.001
			(0.012)
Dry Laws * Mother is other race			-0.075
			(0.049)
Dry Laws * Mother's education $\geq$ 12 years			-0.030*
			(0.015)
Dry Laws * Parity			-0.001
			(0.008)
Municipality FE		X	X
Year-Month FE		X	X
Controls		X	X

Notes: The period of analysis is 2002-2006 (including up to 9 months after the introduction of dry laws in adopting municipalities). Observations include siblings if only the youngest one was prenatally exposed to the dry laws (N=37,413). The dependent variable is an indicator that takes value 1 if a woman moves to another muncipality between pregnancies. Each column indicates a regression. Regressions in columns (2) and (3) include municipality fixed effects, year-by-month of birth fixed effects, and the following controls: mother's age, age squared, education, race, and parity. Omitted categories include mother is at most 19 years old, black and mother's education is below 12 years. Exposure is measured at children's municipality of residence at birth. Standard errors (in parenthesis) are clustered at the municipality level. * significant at 10% level

pregnancy is controlled for by the inclusion of mother fixed effects. Exposure to the dry laws should be exogenous to any particular mother, and its effect is identified only by the timing of the adoption.

1.4.2 Empirical Model

To estimate the effect of being prenatally exposed to a reduction in the hours of operations of bars and restaurants, I employ a difference-in-differences approach that exploits two sources of variation: the staggered implementation of these dry laws and the fact that only a set of municipalities in the SPMA adopted them. I compare infant health outcomes of those born before and after the adoption of dry laws in adopting municipalities to infants who were born in municipalities that did not implement them. The underlying assumption is that changes in children's outcomes in non-adopting municipalities are good predictors of what would happen in adopting municipalities in the absence of the dry laws.

As mentioned in the preceding section, I focus on children who were conceived before the adoption of the laws to limit the potential for time-varying changes in fertility decisions. I address selective migration by defining exposure based on mothers' pre-laws location of residence. I include mother fixed effects in my model to difference out any time-invariant characteristics of the mother, or family background more generally, that could bias the results.

My regression model is defined as follows,

$$\begin{aligned} y_{ifym} =& \beta_0 \\ &+ \beta_1 \, DL(\textit{1 to 3 months before birth})_{ifym} \\ &+ \beta_2 \, DL(\textit{4 to 6 months before birth})_{ifym} \\ &+ \beta_3 \, DL(\textit{7 to 9 months before birth})_{ifym} \\ &+ \mathbf{X}'_{ifym} \, \beta_4 + \gamma_f + \gamma_{ym} + \epsilon_{ifym} \end{aligned} \tag{1.3}$$

for each child i, borne by mother f, in year y, and month m. y_{ifym} is the birth

outcome of interest such as an indicator for low-birth weight, preterm birth, or male child. The vector of maternal and children's characteristics, $\mathbf{X}_{ifym}$, includes dummies for birth order (1st, 2nd, 3rd or higher order, missing), mother's age at birth and age squared. Year-by-month of birth fixed effects, γ_{ym}, remove common time effects and common seasonality. γ_f indicates mother's fixed effect. Robust standard errors are clustered at the mother level.

Following the literature of *in utero* exposure, I focus on three periods of gestation, akin to trimesters. To identify a child's timing of prenatal exposure to a dry law, I use his date of birth and count backwards up to 38 lag weeks because Brazilian birth certificates preclude from backdate conception. Thus, I define my main explanatory variables as time before birth. Specifically, the indicator $DL(\textit{1 to 3}$ months before birth$)_{ifym}$ is equal to 1 if a dry law was adopted 1 to 3 months before child i was born in the municipality where his mother resides when she had her first child in 2002-2006. Similarly, $DL(\textit{4 to 6}$ months before birth$)_{ifym}$ and $DL(\textit{7 to 9}$ months before birth$)_{ifym}$ indicate exposure to a dry law 4 to 6 and 7 to 9 months before birth, respectively. To make it clear, Figure 1.5 displays the definition of prenatal exposure to a law based on the month of birth after the adoption of the law. Unlike studies looking at the impact of fluctuating shocks such as air pollution or homicide rates that measure effects at a point during the pregnancy, these coefficients recover the effect of both length and timing of exposure to the laws.

If I assume that conception occurred 9 months prior to birth, β_1, β_2 and β_3 would indicate exposure to a dry law from the third, second and first trimester of pregnancy, respectively. However, I refrain from referring to pregnancy trimesters. Because the timing and length of pregnancy are endogenous, backdating exposure introduces an obstacle: infants exposed to the laws long before birth would be positively selected because they were not born prematurely, and some premature children would not have

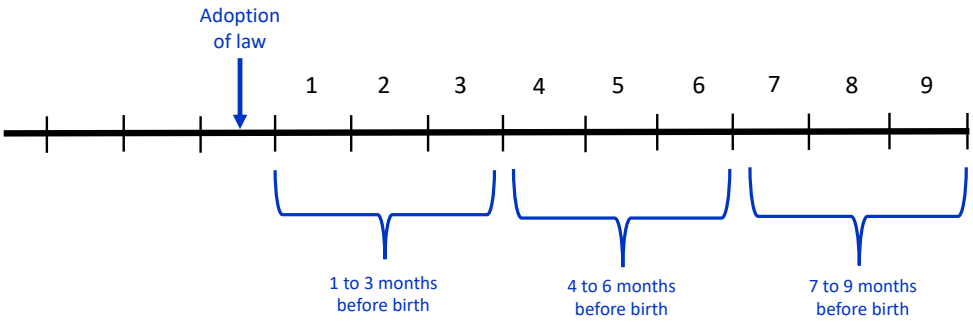


Figure 1.5. Assignment of Prenatal Exposure to Dry Laws

Notes: Figure describes assignment of the timing of prenatal exposure to a dry law based on the month of birth after the adoption of it. Infants born 1 to 3 (4 to 6 or 7 to 9) months after the adoption of the laws were exposed 1 to 3 (4to 6 or 7 to 9) months before births.

been conceived yet. Selection would bias estimates of the impact of dry laws that were adopted long before birth. Considering that, coefficients β_1 are more interpretable than the estimate of β_2 and β_3; I emphasize estimates related to the last 3 months of pregnancy. I further explore this estimation hurdle with some robustness checks in Section 1.6.

Specifically, β_1, β_2 and β_3 capture the average causal effect on newborns who were *in utero* when a dry law was implemented compared to their siblings who were not prenatally exposed to these laws.[14] Since my regression model estimates the reduced form impacts on all children who reside in the SPMA, these estimates are interpreted as an intention-to-treat effect (ITT). Because I cannot disentangle all alternative explanations associated with the effect of a restriction in alcohol availability on new-

[14]Recent research on difference-in-differences settings with staggered adoption has been developed in the last years (Athey and Imbens, 2018; Goodman-Bacon, 2018; Borusyak and Jaravel, 2016; Abraham and Sun, 2018; Chaisemartin and D'Haultfoeuille, 2018). Overall, these working papers conclude that the difference-in-difference estimator is a weighted average of different estimators that compare timing groups to each other. In a single-coefficient specification, heterogenous treatment effects over time or anticipatory behaviors would lead to negative weights in this average. In that case, specifications such as event-studies may be more appropriate.

borns' health, the estimated ITT effects should be interpreted as the full impact of the introduction of dry laws that reduce hours of operations for bars and restaurants. The list of potential channels includes, but is not limited to, maternal drinking, victimization or stress related to violence, reallocation of income or time, and smoking or exposure to secondary smoking. This reduced form analysis is common to most studies on the effects of early shocks in life. I explore subsidiary datasets to provide suggestive evidence of some of the channels that may explain the effect of the adoption of dry laws in the SPMA on health at birth in Section 1.7.

Regarding the expected signs of β_1, β_2 and β_3, the following hypotheses arise from biological studies (see review in Section 1.2.3). If the outcome of interest is low birth weight or preterm birth, I expect that they would be negative if dry laws improve health at birth. If the adoption of dry laws leads to an improvement in fetal survival, I would expect them to be positive or small, indicating a greater survival of male fetuses who are particularly more fragile to shocks *in utero*. In the latter case, the regression of male birth would lead to positive coefficients as well. Establishing hypotheses about the relative size of these coefficients for each outcome is hard because they would depend on a combination of biology (for example, effect dampened during pregnancy vs. infection) and selection (fetal survival and by design).

As previously noted, implementation of dry laws was decided at the local level, and only some municipalities in the SPMA introduced them. It is reasonable to expect that dry laws' adoption may be related to municipality characteristics that might also be associated with children's birth outcomes. Because I control for family fixed effects, it is not necessary that the adoption of dry laws remain unrelated to municipality characteristics. However, it is still useful to understand the determinants of the adoption of these laws across municipalities. Table 1.3 summarizes municipalities' characteristics using data from the Instituto Brasileiro de Geografia

Table 1.3. Characteristics of Municipalities

	Adopting Municipalities (1)	Non-adopting Municipalities (2)
A. Population (%)		
Male	0.49	0.50
Male 15-45 years old	0.29	0.29
Female 15-45 years old	0.34	0.33
White	0.60	0.64
Black	0.35	0.31
Literate	0.81	0.80
B. General characteristics		
Population Density (per km^2)	3903.4 [SD=3758.4]	1781.4 [SD=2519.1]
GDP per capita ($x1000\$R$)	16.6 [SD=17.6]	11.9 [SD=6.9]
Infant Mortality Rate (per 100,000)	179.3 [SD=368.6]	166.1 [SD=355.4]
Homicide rate (per 100,000)	3.2 [SD=2.6]	2.3 [SD=2.3]
Hospital admissions related with traffic accidents (per 100,000)	13.9 [SD=13.6]	13.7 [SD=20.3]
Hospital admissions related with alcohol consumption (per 100,000)	1.6 [SD=2.6]	1.3 [SD=2.1]
Hospital admissions related with drug use (per 100,000)	0.04 [SD=0.19]	0.06 [SD=0.29]
Average number of employed individuals in bars and restaurants	586.8 [SD=688.1]	620.0 [SD=1148.8]
C. Mayor (%)		
Centre-left	0.31	0.33
Centre	0.56	0.51
Centre-right	0.10	0.14
Right	0.03	0.00
Mayor is male	0.94	0.92
D. Municipalities		
Proportion of neighboring municipalities adopting dry laws	0.35	0.15
Number of municipalities	18	19

Notes: The period of analysis is 2002-2006. See Table 1.1 for the list of municipalities adopting dry laws.

e Estatistica (IBGE) and DATASUS. There appears to be no substantial differences in terms of population composition, the average number of people employed by bars and restaurants, and the mayor's characteristics between adopting and non-adopting municipalities. There are, however, other notable differences between them. Municipalities implementing dry laws are characterized by a higher population density, GDP per capita, infant mortality, and homicide rate, and a slightly higher rate of hospital admissions related to traffic accidents, alcohol consumption and drug use

compared to the control group.[15] This confirms intuition, since the implementation of dry laws was motivated by high levels of violence. One may still be concerned that the determinants of adoption of the laws may be correlated with underlying trends in newborns' health. In Section 1.6, I run several specification checks to provide support to my identification strategy.

1.4.3 Sample and Summary Statistics

Once municipalities adopted dry laws their enforcement was uninterrupted. Considering this, a group of siblings is included in my analysis sample if their mother was exposed to a dry law only during her last pregnancy between 2002 and 2006. That is, the oldest sibling was not prenatally exposed to a dry law and the youngest one was exposed to a law. I also include in my analysis siblings who were both not prenatally exposed to a dry law, or, in other words, their mother was never exposed to the laws when she was pregnant.[16] After these conditions are applied, the main sample consists of 31,442 newborns.

Table 1.4 presents means for main variables in the birth dataset. Columns (1) and (2) show averages for all newborns, column (3) focuses on the sample of siblings whose mother was exposed to dry laws only during her last pregnancy, and the last two columns include siblings who were both not prenatally exposed to dry laws. I divide the samples of children in adopting or non-adopting municipalities based on their mother's first municipality of residence in the sample.

[15] I employ the 3-digital International Classification of Diseases 10th Edition (ICD-10) constructed by the WHO to identify homicides (codes X85-Y09) and hospital admissions related to traffic accidents (V20-V98), alcohol consumption (X45, Y15, and T51), and drug use (X42, X62, and Y12). A further description of how the homicide and hospital admission rates are constructed can be found in Section 1.7.

[16] This controls for events that occurred at the same time as the adoption of the laws and affect children in adopting and non-adopting municipalities. Moreover, it increases power in identifying coefficients on variables in the regression model other than the main indicator variables.

Children are more likely to be white and their mothers are slightly more likely to have 12 or more years of education in adopting than in non-adopting municipalities. Comparing samples shows that newborns are less likely to be white in the siblings sample than in the sample of all newborns.[17] Moreover, mothers are on average less educated in the siblings sample relative to the sample of all children. When interpreting my results, it is necessary to have these sample characteristics in mind.

In my main sample of analysis that combines siblings who are prenatally exposed and non-exposed, about half of these newborns are male and white. Their mothers are, on average, 25 years old, and more than half of them have a high school education. Married mothers represent 34% of the sample. Newborns weigh, on average, 3.2 kg. (7 pounds), and 68 per 1,000 of them are born with low birth weight. On average, children are born with 39 weeks of gestation; about 5% of them are born preterm.

1.5 Results

1.5.1 Main Results

Table 1.5 shows estimates from specifications that build up to Eq. (1.3). Panel A presents estimates of the universe of births in the SPMA that do not account for migration decisions. Regressions in this panel include municipality of residence at birth fixed effects, year-by-month of birth fixed effects, and mother's characteristics. Municipality fixed effects pick up direct effects of predetermined factors of the municipalities such as differences in the number of hospitals prior to the laws. The difference-in-differences approach controls for unobserved differences both between infants born in different months and years as well as between children from adopting and non-adopting municipalities.

[17] I combine children who are "preta" or "parda" as black.

Table 1.4. Variable Means

Municipalities /	All births		Mother is only exposed during last pregnancy	Mother is never exposed	
	Adopting	non-Adopting	Adopting	Adopting	non-Adopting
	(1)	(2)	(3)	(4)	(5)
A. Infant Demographics					
Male	0.51	0.51	0.53	0.52	0.51
White	0.60	0.48	0.44	0.45	0.42
Black	0.23	0.20	0.43	0.38	0.29
B. Maternal Demographics					
Age	25.9 [SD=6.4]	26.4 [SD=6.5]	24.9 [SD=5.9]	24.4 [SD=5.9]	25.1 [SD=5.9]
0-11 years of education	0.38	0.33	0.41	0.45	0.40
12 or more years of education	0.62	0.67	0.59	0.55	0.60
Mother's edu is missing	0.05	0.03	0.03	0.03	0.02
Married	0.39	0.38	0.32	0.31	0.34
Marital status is missing	0.02	0.01	0.01	0.01	0.01
C. Birth outcomes					
Birth weight (in grams)	3162.6 [SD=482.4]	3168.5 [SD=483.2]	3171.9 [SD=469.5]	3128.8 [480.4]	3173.1 [SD=471.7]
Low Birth Weight (per 1,000)	74.56	73.19	66.46	82.09	68.79
Preterm (per 1,000)	55.30	65.21	46.37	58.95	60.10
Late preterm(per 1,000)	49.42	59.81	43.28	54.55	55.77
Extreme preterm (per 1,000)	5.88	5.40	3.09	4.41	4.33
Number of births	56,325	492,341	1,878	1,755	27,809
Number of mothers			926	873	13,674

Notes: The period of analysis is 2002-2006. The "All births" sample does not include births with missing information and those born with less than 27 weeks of gestation. Columns (3) includes a group of siblings if the oldest one was born before the adoption of a dry law and the youngest one was prenatally exposed by the laws. The last two columns show the sample of siblings who were both not exposed to dry laws. Newborns are divided in adopting or non-adopting municipalities based on the residence of their mother at her first pregnancy in the data. Preterm, late preterm, and extreme preterm indicate less than 37 gestational weeks, 32 to 26 gestational weeks, and less than 32 gestational weeks respectively.

To reduce computational demands, I collapse individual observations to the date and municipality level. I weight regressions by the number of births in each municipality-date cell. Robust standard errors are clustered at the level of municipality of residence at birth. Following Cameron et al. (2008), because there are a few clusters in my analysis, I provide p-values based on a wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis.

Estimates in panel A suggest that prenatal exposure to dry laws has a nonsignificant and economically small effect on the likelihood of low birth weight, preterm birth, and male child regardless of the time *in utero*.

I replicate the analysis using the sample of siblings in panel B. To define this sample, I only include siblings if the youngest was prenatally exposed to the laws and the oldest was not. Exposure is defined based on child's municipality of residence at birth. Regressions in panel C are built similarly, using mother's first municipality of residence in my sample to assign exposure, define location fixed effects, and the level of clustering of standard errors. Using the sample of siblings (panel B), estimates are slightly larger compared to employing all births (panel A). The differences in maternal characteristics between these samples might explain the change in estimates. In the sample of siblings, mothers are on average of lower socioeconomic status (less educated and more likely to be black); they seem to benefit more from the dry laws, as I show in the heterogeneity analysis. When I fix location to define exposure and control for migration behavior (panel C), estimates are larger for those children born within 3 months of the adoption of the laws. Panel C shows that the dry laws have a significant positive effect on the probability of having a male child of 13% (an effect of 66 per 1000 on a base of 514 per 1000). This larger effect compared to panel B suggests a downward bias when migration is not taken into account as a response to the adoption of the laws that may be explained by less educated mothers being both more likely

Table 1.5. Effects on Birth Outcomes

(x1000)	Low Birth Weight	Preterm Birth	Male Child
	(1)	(2)	(3)
A. All children - municipality fixed effects			
Dry Law 1 to 3 months before birth	-2.48	-1.83	-5.77
	(1.91) [0.178]	(2.55)[0.574]	(5.136) [0.238]
Dry Law 4 to 6 months before birth	1.45	-5.43	-2.53
	(3.12) [0.71]	(2.71) [0.119]	(6.87) [0.713]
Dry Law 7 to 9 months before birth	-1.44	-0.72	4.35
	(2.87) [0.317]	(2.69) [0.891]	(5.93)[0.495]
B. Sibling sample - municipality fixed effects			
Dry Law 1 to 3 months before birth	-5.60	-0.40	8.76
	(7.96) [0.495]	(5.64) [0.990]	(18.35) [0.772]
Dry Law 4 to 6 months before birth	-20.09	-19.26**	19.93
	(8.69) [0.178]	(5.55) [0.020]	(16.19) [0.218]
Dry Law 7 to 9 months before birth	-7.01	0.901	29.82
	(7.03) [0.317]	(7.25) [0.931]	(20.97) [0.277]
C. Sibling sample - municipality fixed effects and mother's first residence used as exposure location			
Dry Law 1 to 3 months before birth	-14.11	-11.46	65.91*
	(23.29) [0.931]	(9.03) [0.475]	(13.08) [0.059]
Dry Law 4 to 6 months before birth	-6.597	-17.36	8.564
	(11.33) [0.594]	(9.22) [0.139]	(32.73) [0.772]
Dry Law 7 to 9 months before birth	-12.18	7.87	21.80
	(9.62) [0.218]	(16.14) [0.733]	(32.43) [0.436]
D. Sibling sample - mother fixed effects and mother's first residence used as exposure location			
Dry Law 1 to 3 months before birth	-4.83	7.53	91.80**
	(21.30)	(18.10)	(45.69)
Dry Law 4 to 6 months before birth	-17.10	-26.07	47.01
	(18.45)	(16.51)	(42.15)
Dry Law 7 to 9 months before birth	9.04	16.94	8.03
	(17.69)	(16.40)	(37.72)
E. Sibling sample - mother fixed effects and mother's first residence used as exposure location			
Dry Law 1 to 9 months before birth	-3.48	0.110	44.40*
	(11.22)	(10.09)	(24.51)
Ymean	69.68	57.57	514.09

Notes: Each column in each panel indicates a separate regression. All specifications include year-by-month of birth fixed effects. Panel A are estimates of the universe of births in the SPMA collapsed to the date and municipality of residence at birth level (N=40,550). Regressions are weighted by the number of births in each cell. Estimates are obtained similarly in panel B and C but considering the sample of analysis (restricted to siblings whose mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed at all). Regressions in panel A and B include municipality of residence at birth fixed effects; in panel C specifications use mother's first municipality of residence to define location fixed effects. In panel A to C, specifications also control for mother's characteristics including age at birth, age squared, dummies for education level at birth, and race. Robust standard errors (in parentheses) are clustered at the municipality level, and wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (249 replications) are reported in brackets. Regressions in panel D and E use individual-level data (N=31,442) and include mother fixed effects and controls as described in Eq. (1.3). Standard errors (in parentheses) are clustered at the mother level. Exposure is measured at the municipality of residence at birth in panels A and B and at the child's mother's first municipality of residence in panels C, D, E. ** significant at 5% level, * significant at 10% level.

to move (Table 1.2) and more likely to be affected (as I show in Table A.1).

Regressions in panel D and E use individual level data and, following Eq. (1.3), include mother fixed effects. In column (1) estimates suggest a drop in the incidence of low birth weight of 7% after the adoption of dry laws for those children exposed 1 to 3 months before birth (an effect of 5 per 1000 on a base risk of 70 per 1000). Yet, these estimates are statistically insignificant. Column (2) shows that the effect on preterm birth is positive and non-significant in the last gestational period.

The last column on panel D focuses on the sex ratio (share of males at birth) and supports the evidence that the enforcement of dry laws improves newborns' health. Adoption of dry laws leads to a higher statistically significant probability of giving birth to a male child. The share of males is 17% larger among infants exposed to the laws during the last 3 months before birth (an effect of 92 per 1000 compared to a base risk of 514 over 1000). To further understand how economically significance this effect is, Figure A.2 presents the distribution of stillbirths (fetal death between 6 to 9 months of gestation) between 2002 and 2006. Pregnancy loss in the first 24 weeks of gestation is more common than later in the pregnancy, yet stillbirths are significant. During the period of analysis, there were 7 stillbirths per 1000 births (4 males and 3 females)—that is 6,268 stillbirths in total. About two-third of the stillbirths occur after week 27 or in the last 3 months of the pregnancy.

The positive and smaller estimate on the probability of having a male child found for the group of children exposed to the laws longer before birth may be explained by a combination of selection and biology. The sex ratio may decrease in the first week after conception, increase early in pregnancy, and decrease later (from week 28) due to excess male mortality (Orzack et al., 2015). Assuming no selection, the effect on the sex ratio at birth from the first trimester would be smaller than that from later in the pregnancy based on the expected reduction in female mortality early in

the pregnancy. The change in the results in panel D compared to estimates in panel C suggests an underestimation of the effects of dry laws when an analysis fails to control for unobserved heterogeneity between households. In panel E, estimates show the effects when the whole prenatal period is taken together. Overall, the effects of exposure at any time *in utero* are small for low-birth weight and preterm birth (5% and 0%, respectively), and the likelihood of having a male child increases by 8.6%.[18]

Table 1.6. Effects on Birth Outcomes by Sex of Newborn

	Low Birth Weight (per 1,000)		Preterm Birth (per 1,000)	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
A. By "trimesters"				
Dry Law 1 to 3 months before birth	12.43	-17.32	66.90*	25.51
	(37.78)	(55.38)	(34.56)	(43.22)
Dry Law 4 to 6 months before birth	-4.51	2.78	18.22	-55.53
	(38.99)	(41.37)	(32.37)	(39.94)
Dry Law 7 to 9 months before birth	11.61	-9.40	6.29	-9.77
	(30.63)	(30.77)	(23.63)	(33.43)
B. All time in utero				
Dry Law 1 to 9 months before birth	6.97	-7.05	26.98	-17.19
	(20.60)	(24.46)	(17.32)	(23.07)
Ymean	59.73	79.39	58.68	58.08

Notes: Each column in each panel indicates a regression. Sample includes siblings if their mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed at all during that period. Data are restricted to mothers who have at least two children of the same sex (Male newborns=8,524; Female newborns=7,681). Regressions include mother fixed effects, year-by-month of birth fixed effects, and controls (mother's age at birth,aged squared, and dummies for birth order). Standard errors (in parentheses) are clustered at the mother level. * significant at 10% level.

A positive effect on the sex ratio at birth would suggest a reduction in fetal losses,

[18] A possible critique to these results is that I test many hypotheses. In that case, using a p-value threshold corrected by the Bonferroni approach may be more appropriate than the standard p-value. It is worth noting that I explore three outcomes, and there are only three tests for the final gestational period that, as mentioned, provide the most interpretable results. In that case, at the 5% level the corrected p-value threshold would be 0.016. The coefficient for sex ratio does not meet this criterion. Because the results are highly correlated, I consider that this threshold is too conservative.

and it may explain the small and non-significant effects in the other two outcomes if the dry laws are helping unhealthier infants to survive. To further explore the fetal selection hypothesis, I stratify the analysis by sex of the newborns. Employing a mother fixed effects model requires that the sample used in these regressions focuses on siblings who are both of the same sex and reduces the sample of analysis. Table 1.6 shows that for male newborns, birth outcomes worsen after prenatal exposure to dry laws: the likelihood of being low birth weight and preterm both increase, and the last one is statistically significant. Conversely, female infants seems to benefit from being prenatally exposed to these laws. Most of these coefficients are imprecise but indicate an interesting pattern.[19] Overall, results point to a relatively lower fetal death rate among unhealthier male fetuses suggesting that dry laws lead to both selection and "scarring" effects. It is worth noting that, while these estimates provide suggestive evidence of the differential effect of the dry laws by sex of the newborns, one should be cautious when interpreting the magnitude of these effects.[20]

1.5.2 Comparison with Existing Literature

The literature on policies affecting alcohol availability and infant health offers only a few estimates of the effect of prenatal exposure to such policies. Fertig and Watson (2009) and Barreca and Page (2015) exploit the variation in MLDA across U.S. states between the 1970s and 1980s and find that MLDA of 18 years is associated with an increase of 4% and 1% in the incidence of low birth weight and a rise of 2.5% and

[19] A comparison of the coefficients for the sample of males and females shows that only the difference between coefficients measuring the effect of exposure 1 to 3 months before birth on preterm is marginally significant ($p\text{-}value$=0.09). Other differences are not statistically significant at conventional levels, likely because of lack of statistical power to detect them.

[20] Alternative ways to further examine these differential effects would be would be looking at stillbirths or the number of live births. This requires considering, for example, monthly live births in each municipality, which is not compatible with my strategy to address selection. Because it does not account for fertility and migration behaviors leading to underestimation of the results, this strategy would not provide unbiased evidence of the effects of the dry laws.

1% in the incidence of preterm birth compared to the mean, respectively. These estimates are significant only in Fertig and Watson (2009), and they indicate the effects at any time in the pregnancy. My estimates for low-birth weight and preterm (panel E) are similar to these findings. These results are also in line with several studies documenting small effects of stress (for instance, from homicides) on birth outcomes (Foureaux Koppensteiner and Manacorda, 2016; Currie and Rossin-Slater, 2013; Black et al., 2016; Simeonova, 2011).

The effects of alcohol availability on sex ratio have also been explored before. Barreca and Page (2015) report that a lower MLDA is associated with a 0.4% higher probability of giving birth to a female child. This change in fetal selection is accompanied by worse birth outcomes for female newborns, or in other words, unhealthier male fetuses do not survive while female ones do. Nilsson (2017) finds a larger effect on sex ratio, 14%, by examining a policy that temporarily and sharply increased access to strong beer for people under the age of 21 in certain regions of Sweden during the late 1960s. The author finds that the effect is concentrated among infants exposed from the first trimester (with an effect of 8.2 percentage points or 16%) and that male newborns are born one week earlier.

The size of my estimate (9.2 percentage points or 17%) conforms with Nilsson (2017); the timing *in utero* when the effect is relevant is different. Similar to Nilsson (2017), a larger effect on fetal death compared to later in the pregnancy has also been documented as a result of a reduced nutritional intake (Almond and Mazumder, 2011). In line with my findings, prenatal exposure late in the pregnancy (after week 20-24) to elements that induce maternal stress have been linked to changes in the ratio of males to females during the terrorist attacks of September 11 (Catalano et al., 2005, 2006), political events (Grech, 2015), and mass layoffs (Catalano et al., 2010). Moreover, for infants born within 3 months after the terrorist attacks, the sex

ratio among those with very low birth weight was lower, and they scored greater than expected in cognitive ability confirming the hypothesis that selection occurred among unhealthier male fetuses (Catalano et al., 2006; Bruckner and Nobles, 2013). Overall, these studies suggest that maternal stress may have a larger role in my findings than changes in nutrition. I show in section 1.7 that changes in stress are likely trigger by changes in violence, workplace accidents, traffic accidents, and family composition.

1.5.3 Heterogeneity Analysis

Estimating the average effect for the group of infants prenatally exposed to a reduction in hours of operation of bars as a whole may conceal important differences in the consequences of the laws across subgroups. In particular, looking at differences by race would be interesting for two reasons. First, studies report that black women have a higher incidence of low birth weight and are at a higher risk of preterm delivery relative to women from other racial or ethnic groups (Valero De Bernabé et al., 2004; Goldenberg et al., 2008). These differences are evident in the comparison of mean levels for mothers in each group (reported at the bottom of Table 1.7): the rates of low birth weight and preterm birth are 10 and 6 percentage points higher for black children, respectively. Second, if the effect of dry laws is stronger among lower socio-economic status mothers, race may be serving as a proxy in these cases to understand differential effects.

In Table 1.7, I report results of estimating Eq. (1.3) in two subsamples: white and black children. Estimates show that effects are larger for black infants compared to white newborns. Specifically, most of the increase in the likelihood of giving birth to a male child in the last gestational period concentrates in the former group.[21] The analysis by gender suggests that black male infants are, on average, more likely to be

[21]The difference between estimates is statistically significant (p-value=0.02).

low birth weight and preterm after the adoption of the dry laws. These larger effects for black newborns conform with previous studies on MLDA (Fertig and Watson, 2009; Barreca and Page, 2015). Overall, my findings indicate that the adoption of the dry laws helps to reduce the black-white fetal mortality gap.

To further explore differential effects by mother's characteristics, I estimate my main equation by mother's educational level. Maternal education has also been linked to birth outcomes (Goldenberg et al., 2008). As expected considering the association between race and education in Brazil, dry laws have a larger impact on infants whose mothers are less educated compared to those with more educated mothers (Table A.1).

1.6 Robustness Checks

The broad takeaways from Tables 1.5 and 1.6 are that prenatal exposure to closing bars earlier causes a reduction in fetal deaths among unhealthier male fetuses. I show in this section that these results are robust to different aspects of my empirical strategy.

1.6.1 Time Trends

My difference-in-differences approach identifies the effect of the adoption of dry laws under the assumption that in absence of these laws, birth outcomes would follow the same trends in adopting and non-adopting municipalities. A concern is that differential time trends in the outcomes of interest between adopting and non-adopting municipalities could be captured in my results. To examine this concern, I augment my model with lags of the dry laws indicators following an even study design. I include five indicators that each take a value of one if a child was born in one of the

Table 1.7. Effects on Birth Outcomes by Race

	Low Birth Weight (per 1,000)			Preterm Birth (per 1,000)			Male Child (per 1,000)
	All	Male	Female	All	Male	Female	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. White Children							
Dry Law 1 to 3 months before birth	-35.30	-38.48	-23.76	4.67	64.25	-20.59	38.43
	(31.19)	(48.79)	(88.00)	(24.14)	(40.10)	(67.69)	(71.75)
Dry Law 4 to 6 months before birth	0.618	61.07	-33.00	-20.60	73.90	-54.96	69.84
	(27.06)	(63.50)	(51.14)	(23.87)	(54.50)	(51.35)	(64.19)
Dry Law 7 to 9 months before birth	-18.16	-67.87	25.99	-29.08	-76.42	9.71	134.90**
	(31.28)	(48.33)	(47.76)	(30.09)	(48.61)	(59.78)	(62.83)
Ymean	68.87	61.52	76.49	55.85	57.13	54.53	508.77
B. Black Children							
Dry Law 1 to 3 months before birth	17.33	119.5**	-66.15	3.79	73.69	9.26	176.10***
	(31.20)	(53.70)	(67.19)	(28.25)	(56.53)	(24.67)	(68.33)
Dry Law 4 to 6 months before birth	-34.07	-112.20	61.88	-29.96	-53.71	-35.36	79.84
	(32.14)	(58.90)	(80.22)	(27.26)	(47.37)	(72.89)	(69.61)
Dry Law 7 to 9 months before birth	16.87	22.18	-33.35	55.56**	47.98	-8.69	-70.74
	(29.01)	(50.25)	(61.53)	(26.14)	(37.44)	(56.10)	(60.44)
Ymean	77.35	66.58	88.68	62.15	61.89	62.42	513.07

Notes: Sample includes siblings if their mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed at all during that period. Panel A includes 10,830 observations (2,882 in regressions including only male infants and 2,614 for those with only females). Sample in panel B contains 8,626 observations (2,384 and 2,163 for regressions including only male or female newborns). Each column indicates a regression. All regressions follow Eq. (1.3) and include mother fixed effects, year-by-month of birth fixed effects, and controls. In columns (1) and (4), regressions also control for whether the child is male. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

five quarters before dry laws were adopted. Compared to indicators in Eq. (1.3), a child born one quarter after a dry laws was adopted means that she was prenatally exposed to the laws up to 3 months before birth. Table 1.8 shows that, consistent with the parallel trends assumption, there is little evidence that birth outcomes improve in the years leading up to the introduction of the laws.

To account for different underlying trends in infants' potential birth outcomes depending on the pre-laws characteristics of the municipality, I add interaction terms between time fixed effects and a set of municipalities' characteristics before the in-

Table 1.8. Robustness Checks: Time Trends

	Low Birth Weight (x1000) (1)	Preterm Birth (x1000) (2)	Male Child (x1000) (3)
Births			
5 quarters before Dry laws	16.04	19.48	-54.30
	(24.65)	(22.19)	(51.06)
4 quarters before Dry laws	-3.39	-48.13***	13.53
	(18.54)	(18.16)	(48.04)
3 quarters before Dry laws	11.51	4.85	-2.68
	(21.81)	(20.68)	(45.86)
2 quarters before Dry laws	-23.93	-1.78	-2.39
	(25.85)	(22.58)	(50.13)
1 quarters before Dry laws	7.88	-29.45	-75.08
	(25.01)	(21.53)	(52.27)
1 quarter after Dry laws	-5.66	2.41	83.79*
	(21.49)	(18.79)	(46.40)
2 quarters after Dry laws	-15.53	-32.61*	41.59
	(19.46)	(16.99)	(44.16)
3 quarters after Dry laws	9.53	12.00	2.30
	(18.72)	(17.48)	(40.92)

Notes: Sample includes siblings if their mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed at all (N=31,442). Each column indicates a regression. All regressions follow Eq. (1.3) and include mother fixed effects, year-by-month of birth fixed effects, and controls. *** significant at 1% level, ** significant at 5% level, * significant at 10% level/

troduction of the laws. These characteristics include population density, GDP per capita, mayor's political party, and homicide rates. Estimates from this specification (panel A in Table 1.9) conform well to the results from the main specification (panel D in Table 1.5) suggesting that differential preexisting underlying trends are unlikely to be a key driver of the overall results. Findings in Table 1.9 also show a statistically significant reduction on prematurity for infants exposed to the laws 4 to 6 months before.[22] This effect is related to a decrease in the likelihood of late-preterm birth

[22]For the main specification this estimate has a p-value of 0.1.

(32-36 weeks). Considering that by that time in the gestation weight is on average between 2000 and 2400 grams, it is not surprising that the effect is not reflected in the probability of low birth weight.

1.6.2 Alternative Specifications

My preferred specification using mother and time fixed effects controls for time-invariant characteristics of the mother and family background, and common seasonality and time changes. One concern might be whether location-specific seasonality (for example, holidays or celebrations) and location-specific trends (such as investment in health services or initiatives to reduce domestic violence) are confounding my results.

I address these concerns by estimating alternative regressions that include interaction terms between mother's first municipality of residence and time fixed effects. To address municipality-specific seasonality effects on birth outcomes, regressions in panel B of Table 1.9 replace year-by-month fixed effects with municipality interacted with month of birth fixed effects and year of birth fixed effects. In panel C, I substitute year-by-month of birth fixed effects with a municipality-specific quadratic time trend and month of birth fixed effects. If newborns' health would have improved in adopting municipalities even without the introduction of dry laws, my main findings may reflect these unobserved trends and the inclusion of a municipality time trend would attenuate my estimates. Overall, results in panel B and C confirm my main finding: exposure to the dry laws in the last gestational period leads to an increase (of about 17%) on the probability of having a male child.

Table 1.9. Robustness Checks: Alternative Fixed Effects Specifications and Controls

	Low Birth Weight (x1000) (1)	Preterm Birth (x1000) (2)	Male Child (x1000) (3)
A. Municipality's characteristics x Time fixed effects			
Dry Law 1 to 3 months before birth	-17.36	-27.24	126.60**
	(29.14)	(26.88)	(60.37)
Dry Law 4 to 6 months before birth	-25.53	-61.99**	81.46
	(29.30)	(26.41)	(55.81)
Dry Law 7 to 9 months before birth	3.15	-15.90	109.70*
	(27.27)	(23.62)	(57.05)
B.Municipality specific seasonality + Year fixed effects			
Dry Law 1 to 3 months before birth	-23.48	10.49	91.84*
	(22.23)	(18.83)	(49.74)
Dry Law 4 to 6 months before birth	-15.27	-30.82*	35.17
	(19.79)	(17.72)	(46.88)
Dry Law 7 to 9 months before birth	19.87	20.71	1.19
	(24.01)	(21.62)	(46.26)
C.Municipality quadratic trend + Month fixed effects			
Dry Law 1 to 3 months before birth	-8.38	4.32	104.70**
	(20.93)	(17.64)	(44.74)
Dry Law 4 to 6 months before birth	-19.50	-32.39**	47.11
	(17.93)	(16.08)	(41.28)
Dry Law 7 to 9 months before birth	5.89	9.72	11.66
	(17.39)	(16.11)	(36.94)
D. Full term infants (> 37 weeks)			
Dry Law 1 to 3 months before birth	-6.03		100.30**
	(18.24)		(47.83)
Dry Law 4 to 6 months before birth	-8.54		38.62
	(14.69)		(44.31)
Dry Law 7 to 9 months before birth	-0.885		12.79
	(13.79)		(39.18)
Ymean	69.67	57.54	514.04

Notes: Sample includes siblings if their mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed at all (N=31,442). Each column indicates a regression. All regressions include mother fixed effects and controls as described in Eq. (1.3). Standard errors (in parentheses) are clustered at the mother level. Regressions in panel A include interactions between year-by-month of birth fixed effect and municipality's characteristics before the adoption of dry laws. In panel B, I control for municipality-month of birth and year fixed effects; in panel C, I include month of birth fixed effects and municipality-specific quadratic trends. In the last panel, regressions follow my main model using year-by-month fixed effects and focus on a subsample of newborns who were born after 37 weeks of gestation (N=28,174). ** significant at 5% level, * significant at 10% level.

1.6.3 Full Term Gestation

To define whether and for how long a child was prenatally exposed to dry laws, I count backward from the date of birth. This strategy performs better for those children born within 1 to 3 months after dry laws enforcement. However, it may indicate that some children born more than 6 months after the adoption of the law were prenatally exposed to dry laws when actually they were born prematurely. Panel E in Table 1.9 replicates my estimation using only children who were born "full term" (after 36 weeks of gestation). While this sample is selected, it shows that the effect on sex ratio is consistent even within this group of newborns.

1.6.4 Neighbor Effects

Considering that not all municipalities in the SPMA adopted dry laws, displacement might occur. Shifting bar drinking to non-adopting municipalities would challenge my inference because non-adopting municipalities might be affected by the laws. For instance, increasing the number of people from adopting municipalities who go to bars in non-adopting municipalities would increase agglomeration and, likely, levels of violence. In that case, displacement would lead to a downward bias in my estimates.

Anecdotal evidence suggests that displacement might not be an issue. One might expect that most regular clients visiting bars live in the immediate neighborhood (Laranjeira and Hinkly, 2002). Also, it is worth noting that public transportation connecting municipalities is not available during the window of time that bars close. For instance, the lines of the Metro São Paulo and the metropolitan train close at midnight. Some buses run after midnight, but with a very low frequency.

I evaluate neighbor effects in different ways in Table 1.10. First, I explore the effect of the intensity of neighbor adoption on birth outcomes for municipalities in

Table 1.10. Robustness Checks: Neighbor Effects

	Low Birth Weight (x1000)	Preterm Birth (x1000)	Male Child (x1000)
	(1)	(2)	(3)
A. "Control" municipalities			
Number of neighboring adopting munic.	-21.40	-18.41	-53.82
	(22.74)	(22.36)	(50.00)
B. Controlling for % neighboring adopting municipalities			
Dry Law 1 to 3 months before birth	-3.93	7.02	90.91**
	(21.21)	(18.11)	(45.72)
Dry Law 4 to 6 months before birth	-17.01	-26.64	45.78
	(18.47)	(16.53)	(42.17)
Dry Law 7 to 9 months before birth	8.70	16.54	7.085
	(17.62)	(16.39)	(37.72)
Proportion of neighboring municipalities	-18.37	-15.59	-38.22
adopting laws	(22.04)	(21.43)	(48.18)
C. Municipalities with Population > 100,000			
Dry Law 1 to 3 months before birth	-4.46	8.06	86.66*
	(22.48)	(18.62)	(49.18)
Dry Law 4 to 6 months before birth	-8.03	-17.64	76.80*
	(19.18)	(16.69)	(45.26)
Dry Law 7 to 9 months before birth	11.10	21.20	39.11
	(19.09)	(18.48)	(40.62)

Notes: Sample includes siblings if their mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed at all. Each column indicates a regression. All regressions include mother fixed effects and controls as described in Eq. (1.3). Standard errors (in parentheses) are clustered at the mother level. In panel A, specifications include infants born in non-adopting municipalities and those born in adopting municipalities before the introduction of dry laws (N=29,616). Panel B considers all newborns in my main analysis sample (N=31,442). Panel C restricts the analysis to municipalities with more than 100,000 inhabitants (N=27,056). ** significant at 5% level, * significant at 10% level.

the "control" group. This sample includes infants in non-adopting municipalities and adopting municipalities before adoption. The main explanatory variable is the proportion of neighboring municipalities adopting dry laws. Panel A shows that the effects are statistically insignificant. Adding this measure of neighbor adoption as a control in my main specification confirms the lack of spillover effects. Panel B shows that estimates are robust to this inclusion. Further evidence is provided in the last

panels in Table 1.10 that restrict the analysis to large municipalities where it would be more costly to go to another municipality to drink. Panel C uses population to define large cities (above 100,000 inhabitants) and shows that estimates are robust to these modifications.

1.7 Exploring Potential Mechanisms

Previous sections show that adoption of dry laws has a positive effect on children's outcomes at birth. In this section, I explore some mechanisms that might explain the effects of dry laws on health at birth: alcohol consumption, drug use, violence, workplace accidents, labor market outcomes, and changes in family composition when the child is born. To build a parallel argument with effects on health outcomes, I explore the impact of the adoption of dry laws on these channels up to 9 months after adoption and examine outcomes by gender. While these mechanisms do not exhaust all possible underlying channels, they represent a set of the most salient ones.

1.7.1 Alcohol Consumption

Suggestive evidence of an impact on alcohol purchases after the introduction of the dry laws has been documented by Biderman et al. (2010). The authors compare households' expenditures on beer and cachaça aggregated at the SPMA level (excluding the city of São Paulo) in 1995 and 2003 and find a significant reduction over time (58% and 70% of the average bar consumption in cachaça and beer, respectively). To complement these findings, I study how the adoption of dry laws affect alcohol consumption in the 9 months following their adoption and examine outcomes by gender.

Data on alcohol consumption desegregated at the municipality level are not avail-

able; I proxy for it using two measures. First, I examine traffic-related deaths. A large proportion of traffic accidents are related to alcohol abuse, so a reduction in traffic-related deaths would provide evidence of a decrease in alcohol consumption. Yet, they do not measure alcohol abuse directly. It is also worth noting that closing bars earlier may lead to people driving to bars in another locations. If that occurs the adoption of dry laws would increase traffic-related deaths. To explore that, I use data recorded by the Department of Public Health Information (DATASUS). These were identified as the codes V20 to V89 ("Transport accidents") of the 3-digital International Classification of Diseases 10th Edition (ICD-10) constructed by the WHO.[23]

Second, I consider hospitalization rates due to alcohol consumption. Data correspond to admissions to hospitals. These admissions are charged to the public health care system and recorded by the Ministry of Health's Hospitalization Records (SIH-DATASUS).[24] Alcohol-related hospital admissions correspond to ICD-10 codes X45 ("Accidental poisoning by and exposure to alcohol"), Y15 ("Poisoning by and exposure to alcohol"), T51 ("Toxic effect of alcohol"), following Marcus and Siedler (2015) and Nakaguma and Restrepo (2018).[25]

I compute rates of death or hospital admissions per 100,000 inhabitants caused by traffic accidents or alcohol consumption using records from the period 2002-2006 for men and women aged 15-49 years old. To deal with many zeros in these outcomes, I transform hospital admissions per 100,000 inhabitants with the inverse hyperbolic sine. This can be interpreted in exactly the same way as a standard logarithmic transformation (as approximating percent changes). But unlike a log variable, the

[23]Specifically, I follow Nakaguma and Restrepo (2018) and include codes V02-V04, V09, V12-14, and V19-V89.

[24]The proportion of the population with private health insurance was 25% in 2003, and about 30% of the admissions to hospitals correspond to private ones (Instituto Brasileiro de Geografia e Estatística, 2010).

[25]Marcus and Siedler (2015) also include code F10 ("Mental and behavioral disorders due to alcohol use"). This code was not reported for the full sample of municipalities in my analysis.

inverse hyperbolic sine is defined at zero (see Burbidge et al. (1988) and MacKinnon and Magee (1990)). Along this section, outcomes are constructed in a similar way unless otherwise noted.

I estimate the following model using observations at the municipality-month level and weighting observations by population level in each cell,[26]

$$y_{jym} = \omega_0 + \omega_1 \, DL(\textit{1 to 9 months before hospit.})_{jym} + \gamma_j + \gamma_{ym} + \epsilon_{jym} \tag{1.4}$$

for each municipality j in year y, and month m. $DL(\textit{1 to 9 months before hospital.})_{lym}$ is a binary variable that indicates that a dry law was adopted in municipality j 1 to 9 months before the hospital admissions in month m and year y. Alternatively, I replace the main binary variable by three binary variables that indicate each of the three quarters after the adoption of the laws. The model also includes year-by-month fixed effects, γ_{ym}, to remove common time effects and common seasonality and municipality fixed effects, γ_j, to control for common location specific characteristics. Robust standard errors are clustered at the municipality level and wild cluster-bootstrap p-values are calculated because of the small number of clusters.

Table 1.11 presents the estimated coefficients from Eq. (1.4) for men and women separately. Columns (1) to (3) in panel A show that the rate of death related to motor-vehicles crashes decreases by 12% after the adoption of the laws, and this effect is driven mostly by a decrease of the rate of men who die in traffic-accidents (11%). The impact of the dry laws on traffic-related deaths is slightly larger in the first two quarters after their adoption compared to the third quarter (panel B).

Using information about race provided by vital statistics data (but not available for hospital admissions), I show that findings are quite similar for whites and blacks

[26] I extrapolate population growth comparing annual measures of population to get a proxy of the monthly population.

Table 1.11. Potential Mechanisms: Car Accidents and Alcohol-related Hospitalizations

	IHSF Rate of traffic-related deaths (per 100,000 inhabitants)			*IHSF* Rate of alcohol-related hospital admissions (per 100,000 inhabitants)		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
A. Up to 9 months after adoption of the laws						
Dry Law 1 to 9 months before	-0.118*	-0.110*	-0.037*	0.052	0.036	0.004
	(0.081)[0.089]	(0.064)[0.091]	(0.047)[0.090]	(0.029)[0.175]	(0.024)[0.199]	(0.004)[0.382]
B. By quarters after the adoption of the laws						
Dry Law 1 to 3 months before	-0.117	-0.059	-0.085*	0.047	0.029	0.01
	(0.138)[0.425]	(0.122)[0.636]	(0.047)[0.092]	(0.035)[0.220]	(0.022)[0.222]	(0.009)[0.399]
Dry Law 4 to 6 months before	-0.189*	-0.200***	-0.023	0.034	0.030	0.001
	(0.100)[0.096]	(0.068)[0.008]	(0.078)[0.816]	(0.033)[0.446]	(0.033)[0.471]	(0.002)[0.485]
Dry Law 7 to 9 months before	-0.039	-0.050	-0.014	0.067	0.043	0.001
	(0.104)[0.703]	(0.101)[0.622]	(0.064)[0.841]	(0.037)[0.163]	(0.031)[0.127]	(0.002)[0.824]
Rate mean	0.991	0.821	0.169	0.055	0.034	0.007

Notes: Sample includes observations at the municipality-month level (N=1,541). Dependent variables are deaths or hospitalization rates per 100,000 inhabitants transformed by an inverse hyperbolic sine function. Each column in each panel indicates a regression. All specifications include municipality fixed effects and year-by-month fixed effects. Regressions are weighted by population in each cell. Robust standard errors (in parentheses) are clustered at the municipality level, and wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (249 replications) are reported in brackets. The last row in the table shows the average monthly rate of deaths or hospital admissions per 100,000 inhabitants. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

(Table A.2). Columns (4) to (6) show that there is no statistically significant effect on hospital admissions related to alcohol consumption. Estimates are small (4% for men and 0.4% for women), and even though they are positive, they indicate a change over a low monthly average rate (0.05 individuals per 100,000 inhabitants). Overall, estimates in Table 1.11 provide suggestive evidence of the effectiveness of the dry laws on reducing alcohol consumption in the months following their adoption.

1.7.2 Drug Use

The laws restricting hours of operation of bars may have a direct effect on illicit drug use; it may also impact drug use indirectly through their effect on alcohol consumption. Illicit drugs and alcohol could act as substitutes or complements (Williams et al., 2004; Crost and Guerrero, 2012). If they are substitutes, the adoption of the dry laws may increase the use of drugs; if drugs and alcohol are complements, drug use may decrease after these laws are implemented. Changes in illicit drug use may affect child health directly through maternal use (Gunn et al., 2016), as well as indirectly through changes in violence, including intimate partner violence (Norström and Rossow, 2014; Ally et al., 2016).

I use drug-related hospital admissions in the period 2002-2006 to measure the impact of the adoption of the laws on drug use. I follow Marcus and Siedler (2015) and Nakaguma and Restrepo (2018) in relying on the codes X42, X62, and Y12 in the ICD-10 (accidental, intentional, or by other intent "poisoning by hallucinogens", respectively).[27]

Table 1.12 shows results from the estimation of Eq. (1.4). Estimates in panel A and B are statistically insignificant and economically small for both men and women.

[27]Hallucinogens include cannabis (derivatives), cocaine, codeine, heroin, lysergide (LSD), mescaline, methadone, morphine, and opium (alkaloids). Codes related to mental and behavioral disorders (F11, F12, F14, and F16) are not available for the sample of municipalities in this analysis.

Overall, these estimates do not provide evidence to suggest drugs (hallucinogens) are being substituted as alcohol use declines.

Table 1.12. Potential Mechanisms: Drug Use

	IHSF Rate of drug-related hospital admissions (per 100,000 inhabitants)		
	All	Men	Women
	(1)	(2)	(3)
A. Up to 9 months after adoption of the laws			
Dry Law 1 to 9 months before hospit.	0.014	0.002	0.007
	(0.008)[0.110]	(0.005)[0.514]	(0.006)[0.316]
B. By quarters after the adoption of the laws			
Dry Law 1 to 3 months before hospit.	-0.002	0.003	-0.002
	(0.013)[0.912]	(0.006)[0.567]	(0.002)[0.221]
Dry Law 4 to 6 months before hospit.	0.036	-0.001	0.021
	(0.019)[0.121]	(0.007)[0.948]	(0.015)[0.221]
Dry Law 7 to 9 months before hospit.	0.005	0.007	-0.001
	(0.015)[0.723]	(0.006)[0.236]	(0.004)[0.854]
Rate mean	0.026	0.014	0.005

Notes: Sample includes observations at the municipality-month level (N=1,541). Dependent variables are hospitalization rates per 100,000 inhabitants transformed by an inverse hyperbolic sine function. Each column in each panel indicates a regression. All specifications include municipality fixed effects and year-by-month fixed effects. Regressions are weighted by population in each cell. Robust standard errors (in parentheses) are clustered at the municipality level, and wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (249 replications) are reported in brackets. The last row in the table shows the average monthly rate of hospital admissions per 100,000 inhabitants. * significant at 10% level.

1.7.3 Violence

Another potential mechanism that may explain my findings in newborns' health is a change in violence. Closing bars earlier may reduce social gatherings and associated violent behaviors, as well as violence related to alcohol consumption in the community or within households (Graham and Wells, 2003; Room et al., 2005; Langenderfer, 2013; Leonard and Quigley, 2017). Violence may affect women in many ways. Women are likely to be victims of violence and to suffer mental and physical stress from living in a violent context. Both victimization and stress, have been linked to poor health

at birth (see Appendix 3.6 for a summary of the literature). Violence may also affect family composition and maternal well-being. For example, violence may result in a wife losing her husband or divorcing him. Thus, a reduction in violence may lead to better health at birth.

I use three data sources: death records, hospital admissions, and police reports for the years 2002-2006. I employ codes X85 to Y09 ("Assaults") in the ICD-10 to classify deaths as homicides and hospitalization admissions caused by violence (not resulting in death in hospital).[28] I focus on police reports for rape collected by the *Secretaria da Segurança Pública* of the São Paulo State. Data include men and women aged 15-49 years old. An advantage of using homicide data is avoiding underreporting and self-reporting bias. Yet, it only captures the most violent form of assaults. That said, a decline in homicides after the adoption of dry laws may broadly track the lesser forms of violence. I complement the analysis using hospital admissions due to assaults and reports of rape.

Considering that in Brazil victims of homicide are most likely to be young (20-39 years old), male, and black (Murray et al., 2013), I estimate separate models by race and gender. Breaking the analysis of violence down by gender provides insights about community and domestic violence. The former is more frequently related to men; the latter is more common among women. Men are more likely than women to be murdered outside their family residence, where intimate partner violence is more likely to take place (Waiselfisz, 2012). Indeed, for every 10 women being murdered in Brazil, 7 are killed in the sphere of their domestic relationships (AGENDE, 2007). Female victimization is more likely when partners are drunk.

Table 1.13 reports the estimated coefficients, standard errors, and wild-bootstrap p-values from several regressions following Eq. (1.4). Columns (1) to (3) in the

[28] Using data on hospital emergency departments would be ideal, but this information is not available for the period analyzed in my analysis.

top panel indicate that homicide rates reduce by 15% in municipalities adopting dry laws in the period of 9 months after this adoption compared to non-adopting municipalities; the reduction corresponds mostly to homicides of men. The estimate on homicide rates is slightly larger for white men than black men, and estimates for black women are larger compared to white women (Table A.3). Considering that the mean average rate of homicides is larger within blacks, the overall effect indicates that blacks benefit more by the introduction of the laws. Less extreme cases of violence measured by hospitalizations seem unaffected by the dry laws (columns 4 and 5), yet the last column in the table suggest that the rate of reports of rape decrease.

Overall, these estimates point to two important findings. First, the adoption of dry laws in the SPMA reduces community violence mostly among black men. Second, assaults of women also decrease after the adoption of these laws suggesting a reduction of domestic violence, yet this effect is less precisely estimated. Previous studies have found mixed effects of policies changing bars' closing hours, and most of these studies have focused on extensions on the hours of operation of these outlets such as the United Kingdom's Licensing Act 2003 that eliminated mandatory pub closing hours (Hahn et al., 2010; Gruenewald, 2011; Popova et al., 2009; Rossow and Norström, 2012). My findings provide additional evidence of the effects of alcohol-related policies regulating availability and, in particular, of a significant reduction of hours of operations of bars.[29]

[29]Policies that restrict closing hours for on-premise outlets may raise two concerns. First, public disorder and violent encounters may result from numerous intoxicated individuals leaving bars simultaneously. Second, policies would be ineffective if individuals displace their drinking to other times or if they lead to binge drinking. While there is no empirical evidence that consistently provides support for these concerns, still some characteristics of the Brazilian context should be noted to reinforce their potential insignificant role in this study. Unlike many cities where policies changing on-premise trading hours have been studied, bars in the SPMA are widespread. Moreover, even though individuals may adjust the time they visit a bar or how fast they drink, the restriction imposed in Brazil limits, at least, popular gatherings at bars after soccer matches that end at 11.30pm.

Table 1.13. Potential Mechanisms: Violence

	IHSF Rate of homicides			*IHSF* Rate of hospital admissions caused by violence		*IHSF* Rate of rape by police reports
	All	Men	Women	Men	Women	All
	(1)	(2)	(3)	(4)	(5)	(6)
A. Up to 9 months after adoption of the laws						
Dry Law 1 to 9 months before	-0.150**	-0.142**	-0.014	0.008	0.094	-0.112
	(0.053)[0.011]	(0.057)[0.016]	(0.031)[0.659]	(0.127)[0.948]	(0.089)[0.358]	(0.080)[0.210]
B. By quarters after the adoption of the laws						
Dry Law 1 to 3 months before	-0.126	-0.131	0.017	-0.005	0.195	-0.084
	(0.104)[0.256]	(0.111)[0.277]	(0.046)[0.732]	(0.152)[0.975]	(0.102)[0.114]	(0.123)[0.539]
Dry Law 4 to 6 months before	-0.150	-0.143	-0.004	-0.001	0.038	-0.299***
	(0.090)[0.126]	(0.099)[0.190]	(0.047)[0.953]	(0.129)[0.992]	(0.079)[0.661]	(0.103)[0.007]
Dry Law 7 to 9 months before	-0.153	-0.128	-0.066*	0.01	0.034	-0.155
	(0.124)[-.268]	(0.122)[0.344]	(0.034)[0.080]	(0.159)[0.966]	(0.114)[0.805]	(0.146)[0.327]
Rate mean	2.25	2.12	0.130	1.45	0.237	0.896

Notes: Sample includes observations at the municipality-month level (N=1,541). Dependent variables are homicide, hospital admissions or police report rates per 100,000 inhabitants transformed by an inverse hyperbolic sine function. Each column indicates a regression. All specifications include municipality fixed effects and year-by-month fixed effects. Regressions are weighted by population in each cell. Robust standard errors (in parentheses) are clustered at the municipality level, and wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (249 replications) are reported in brackets. The last row in the table shows the average monthly rate of homicides, hospital admissions, or police reports per 100,000 inhabitants. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

1.7.4 Workplace Accidents

Closing bars earlier may reduce work injuries. Reducing public intoxication, social gatherings, and aggressive behaviors may decrease the risk of people working at bars to be injured by violent incidents. It may also lead to fewer workplace accidents for workers in other sectors because, for example, if they go to sleep earlier or drink less alcohol and they can concentrate better during the work hours. It has been documented that workplace accidents reduce after the passing of laws that restrict bars' hours of operation in Spain (Martin Bassols and Vall Castello, 2018). Overall, decreasing injuries at work would be a positive shock for families.

I first examine the rate of deaths classified as workplace accidents. To further explore the differential effects by economic sectors, I complement the analysis using data from the Brazilian Annual Social Information Report *(Relação Anual de Informações Sociais, RAIS)*, compiled by the Ministry of Labour (MTE). These records depict the Brazilian formal market. Indeed, MTE estimates that RAIS is annually declared by 98% to 99% of officially existing firms. Data are compulsorily provided by public and private businesses on December 31st of the previous year. They include employees' demographic and occupational characteristics, as well as information about job destruction across the year. The outcome of interest is the monthly number of workplace accidents at bars and restaurants and in other sectors such as construction and industry. Workplace accidents correspond to death or retirement due to workplace accident that resulted in a disability (codes 62 and 73).

Table 1.14 presents results of estimating Eq. (1.4) weighting observations by the annual employed population in each municipality. The introduction of dry laws leads to a decrease of deaths at work that is driven by the effect on men (columns 1 to 3). This effect is statistically significant in the first three months after the adoption of the laws, but it reduces over time (panel B). The rate of workplace accidents decreases

Table 1.14. Potential Mechanisms: Workplace Accidents

	IHSF Rate of workplace accidents (per 100,000 employed individuals)					
	Any Death			Bars/restaurants	Construction	Industry
	All	Men	Women	All	All	All
	(1)	(2)	(3)	(4)	(5)	(6)
A. Up to 9 months after adoption of the laws						
Dry Law 1 to 9 months before accident	-0.005	-0.021	0.026	0.030	-0.002	-0.049
	(0.032)[0.899]	(0.019)[0.320]	(0.023)[0.440]	(0.029)[0.646]	(0.053)[0.984]	(0.102)[0.639]
B. By quarters after the adoption of the laws						
Dry Law 1 to 3 months before accident	-0.055**	-0.058***	0.003	0.059	-0.048**	-0.204**
	(0.021)[0.044]	(0.018)[0.009]	(0.004)[0.466]	(0.051)[0.682]	(0.018)[0.040]	(0.068)[0.025]
Dry Law 4 to 6 months before accident	0.007	-0.056***	0.064	-0.032	-0.024	0.043
	(0.066)[0.992]	(0.014)[0.000]	(0.059)[0.472]	(0.028)[0.270]	(0.016)[0.156]	(0.179)[0.858]
Dry Law 7 to 9 months before accident	0.056	0.052	0.004	0.064	0.081	0.016
	(0.043)[0.297]	(0.041)[0.364]	(0.006)[0.491]	(0.052)[0.596]	(0.144)[0.893]	(0.141)[0.916]
Rate mean	0.067	0.064	0.003	0.010	0.052	0.285

Notes: Sample includes observations at the municipality-month level (N=1,541). In the top panel, dependent variables are layoff rates per 100,000 employed individuals transformed by an inverse hyperbolic sine function; in the bottom panel, dependent variables indicate workplace accident rates per 100,000 employed individuals transformed by an inverse hyperbolic sine function. Each column indicates a regression. All specifications include municipality fixed effects and year-by-month fixed effects. Regressions are weighted by the employed population in each cell. Robust standard errors (in parentheses) are clustered at the municipality level, and wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (249 replications) are reported in brackets. The last row in the table shows the average monthly rate of workplace accidents per 100,000 inhabitants.*** significant at 1% level, ** significant at 5% level.

for those who work in the construction or industry sectors (columns 5 and 6), but it has a non-significant effect within bars and restaurants (column 4). Overall, these findings suggest that closing bars earlier reduced workers' risk at work likely because of less alcohol consumption or more hours of sleep. Moreover, it is worth noting that the analysis is based on extreme accidents. The likelihood of smaller injuries may also decrease with the adoption of the laws.

1.7.5 Labor Market Outcomes

Dry laws may affect labor market outcomes: bars' owners may react to the reduction in trading hours by cutting the number of employees. Losing a job would induce stress and reduce the household's income affecting mothers and their children in the opposite way than the other channels discussed in this section.

To explore whether dry laws affect labor market outcomes, I examine the monthly number of employees being fired from bars and restaurants using RAIS data. I consider two codes of the National Classification of Economic Activities (CNAE) to identify bars' and restaurants' employees: 55212 ("Full-service restaurants, bars, and beverage outlets") and 55220 ("Bars without full-service"). Contract termination corresponds to codes 10, 11, 12, 20, and 21.

Estimates in Table 1.15 show that the adoption of dry laws does not affect the rate of layoffs at bars and restaurants in the 9 months after the adoption. Findings are consistent for men and women and across quarters after the adoption of the dry laws. These results confirm that the dry laws do not result in higher stress, lower income, and related poor maternal nutrition as a consequence of reducing jobs at bars and restaurants.

Table 1.15. Potential Mechanisms: Layoffs in Bars and Restaurants

	IHSF Rate of layoffs (per 100,000 employed individuals)		
	All	Men	Women
	(1)	(2)	(3)
A. Up to 9 months after adoption of the laws			
Dry Law 1 to 9 months before layoff	0.122	0.077	0.105
	(0.109)[0.372]	(0.074)[0.341]	(0.106)[0.396]
B. By quarters after the adoption of the laws			
Dry Law 1 to 3 months before layoff	0.162	0.143	0.102
	(0.123)[0.262]	(0.105)[0.223]	(0.127)[0.475]
Dry Law 4 to 6 months before layoff	0.206	0.091	0.166
	(0.132)[0.133]	(0.084)[0.280]	(0.137)[0.236]
Dry Law 7 to 9 months before layoff	-0.001	-0.004	0.048
	(0.105)[0.994]	(0.117)[0.970]	(0.125)[0.710]
Rate mean	55.04	26.72	28.32

Notes: Sample includes observations at the municipality-month level (N=1,541). Dependent variables are rates of layoff in bars and restaurants per 100,000 employed individuals transformed by an inverse hyperbolic sine function. Each column indicates a regression. All specifications include municipality fixed effects and year-by-month fixed effects. Regressions are weighted by the employed population in each cell. Robust standard errors (in parentheses) are clustered at the municipality level, and wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (249 replications) are reported in brackets. The last row in the table shows the average monthly rate of layoffs per 100,000 employed individuals.

1.7.6 Family Composition

Worse newborns' health has been linked to single marital status and stress related to family changes (Goldenberg et al., 2008; Black et al., 2016; Persson and Rossin-Slater, 2018). Adopting dry laws may affect infants' health at birth through changes in family composition. For instance, less alcohol consumption, and/or domestic violence may lead to a reduction in the likelihood of a divorce or an increase in the likelihood of marriage. Moreover, one would also expect that reducing traffic accidents and death due to assaults may also affect the family composition leading to less stress and loss of sources of income.

To explore this channel, I estimate Eq. (1.3) using mothers' marital status at birth as the main outcome. My main outcome of interest is a binary variable that

Table 1.16. Potential Mechanisms: Family Composition

	When her child is born	
	Mother has a partner (x100)	Missing information (x100)
	(1)	(2)
A. Up to 9 months after adoption of the laws		
Dry Law 1 to 9 months before birth	3.54**	-0.37
	(1.57)	(0.46)
B. By quarters after the adoption of the laws		
Dry Law 1 to 3 months before birth	9.78***	-1.10
	(2.78)	(1.12)
Dry Law 4 to 6 months before birth	4.20	0.72
	(2.69)	(0.62)
Dry Law 7 to 9 months before birth	-1.43	-0.76
	(2.46)	(0.59)
Ymean	52.35	1.17

Notes: Sample includes siblings if their mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed at all (N=31,442). Dependent variables are multiplied by 100. Each column indicates a regression. All specifications include mother fixed effects and year-by-month fixed effects. Standard errors are clustered at the mother level. *** significant at 1% level, ** significant at 5% level.

take value one if a mother has listed a partner when her child is born. Because consensual unions are common in Brazil, I consider those who are married as well as those in a more informal union. Table 1.16 shows that there is a 7% increase in the likelihood that a mother has a partner when her child is born after the adoption of dry laws. The analysis by quarters after the adoption of the laws suggest that the effect is larger in the first three months. Effects are slightly larger for black mothers compared to white mothers (11% and 9%, respectively; Table A.4). These differential effects by race coincide with an increase of 5% to 12% on missing father's information for black mothers relative to white mothers when lower minimum drinking ages are established (Fertig and Watson, 2009).[30] Taken together, my findings highlight the effects of laws adopted in the community on intra-household dynamics.

[30]It is worth noting that the authors recover a combination of a direct effect on family composition and the effect on it by differential fertility decisions.

1.8 Conclusions

This chapter documents the unanticipated impact of restricting hours of operations of bars and restaurants on human capital accumulation at its earliest stage. I exploit the exogenous source of variation provided by the staggered implementation of laws in the SPMA. Employing confidential vital registration data, I account for families' behavioral responses (fertility and migration) and compare infants prenatally exposed to the laws with their earlier-born siblings who were not exposed to the dry laws.

My precise and robust difference-in-difference estimates show that curtailing access to drinking reduces fetal death among unhealthier male fetuses. Effects are larger for black infants compared to white infants. Based on the extensive literature documenting the longer-term consequences of fetal experiences (Almond and Currie, 2011) and, in particular, evidence presented by Nilsson (2017), my findings highlight the importance of including the potential for improving maternal and infants' well-being as part of the debate about alcohol-related policies. They also emphasize the importance of exploring not only the intensive but the extensive margin of fetal outcomes. As this chapter shows, examining only fetal health outcomes may lead to the wrong conclusion when there is a change in survival.

Leveraging additional data, I document that alcohol consumption, exposure to violence, and family composition cannot be ruled out as potential mechanisms explaining the link between the implementation of dry laws and health at birth. Moreover, I find no evidence supporting changes in layoffs in bars and restaurants as a consequence of the dry laws. Beyond that, however, my findings are not able to shed light on which physiological channels may be at work and the relative importance of each one.

This article offers insights to the discussion about the effectiveness of alcohol-related policies and calls attention to their impact on within household dynamics. It

serves as an example of an intervention that improved families' well-being. Considering that Brazil's drinking culture is similar in many aspects to that in Australia, the United States, and some countries in Europe, my findings would be useful to policymakers worldwide.

Chapter 2

Health and the City: Urban Congestion and Air Pollution in Brazil

2.1 Introduction

Congestion of urbanized centers is a trademark of economic development across the globe. These economic forces pose a challenge with respect to environmental conditions and, therefore, welfare for populations in those areas. While empirical evidence has helped shape environmental regulation in developed nations, there is a scarcity of evidence to inform policy-makers in the developing world. The level of air pollution is higher and population's health is worse in the latter than in the former preventing to extrapolate the results found in richer countries, and poorer nations could benefit from further knowledge of potential nonlinear effects of pollution.

In this chapter, I focus on air pollution and undertake a causal inference study of its impacts on the health of infants *in utero* in a metropolitan area of Brazil. The identification of the causal effect of air pollution on health is challenging for many well known reasons, such as families self-selection into residential location and avoidance behaviors, and the link between economic activity and pollution. To address these concerns, I take advantage of the meteorological phenomenon of thermal inversion which in urban areas arguably exogenously locks pollutants closer to the ground. Relevant for my identification strategy, the formation of thermal inversions do not have any direct effect on health conditional on weather conditions.

My empirical strategy consists in two parts. I focus first on understanding the effect of thermal inversion on air pollution. Using data on concentrations of five

pollutants and weather conditions between the years 2001 and 2009, I explore how the concentration of pollutants increases with the frequency of inversion episodes. I find that a one standard deviation (SD) increase in the number of thermal inversions in the preceding week increases the average concentration of particulate matter under 10 micrometers (PM_{10}), carbon monoxide (CO), and sulphur dioxide (SO_2) by 2% of their average weekly concentration. I also show that the frequency of stronger inversions leads to larger accumulation of pollutants.

My second estimation looks at the link between inversion episodes and health at birth. The lack of high quality air pollution and vital data in developing countries has contributed to the scarcity of evidence for these nations. By employing detailed data from vital records around the São Paulo Metropolitan Area (SPMA) I detail cumulative effects of air pollution on infants' health at birth. My model parametrically accounts for potential effects of weather and seasonality on health.

I find that exposure to thermal inversion episodes harms health at birth. Specifically, a SD increase in the number of inversions per week occurring in the last 13 weeks of gestation leads to a decrease in birth weight of 34.5 grams, an increase of 16% in the incidence of low birth weight, and a raise of 50% in the incidence of very low birth weight. These effects are partly explained by a reduction in gestational length: the incidence of preterm and very preterm births increases significantly with the number of inversion episodes. The same increase in the frequency of inversions leads to a 18.5% reduction in birth cohort size that affects both male and female fetuses. I do not find an effect of thermal inversion formation on morbidity outside the womb or in earlier periods of gestation.

My work builds on Arceo et al. (2016) and Jans et al. (2018). The former paper studies the contemporaneous effect of pollution on infant mortality in Mexico City using thermal inversions as an instrument for pollution. The latter employs thermal

inversions to study infants' respiratory problems in Sweden. I complement these studies by examine the effects of prenatal exposure to inversion episodes on birth outcomes. Because health early in life is a predictor of future outcomes such as health, earnings and education (Barker and Osmond, 1986; Almond and Currie, 2011), my results suggest lasting negative consequences of air pollution on new generations and that policies oriented to reduce pollution are needed.

2.2 Background Regarding Sources of Air Pollution in Brazil

The SPMA consists in 39 municipalities that extends over 8.000 km^2. More than 19.7 million inhabitants reside there in 2009 making it one of the ten largest urban conglomerates in the world. The municipality of São Paulo, with 11 million people, is in the center of this dynamic urban area.

The number of vehicles increased significantly between 2001 and 2009: from 6 millions to 9.7 millions (CETESB, 2002, 2010). That represents an increase in the number of vehicles per 1,000 inhabitants from 331 to 492. While this is lower than some cities in the United States, it is still higher than most other Latin American cities and contributes to a mobility crisis; São Paulo's infrastructure was not built to accommodate such a large number of private vehicles. For instance, during the first decade of this century, the daily average traffic jams during rush hours in the city of São Paulo has been 118 kilometer (112 miles), and cars circulate during rush hour with an average speed of 19.30 km/h (Rolnik and Klintowitz, 2011). As a result, people waste almost one month each year trapped in traffic.

Considering the high level of congestion in the SPMA, it is not surprising that traffic is one of the main sources of pollution. Indeed, in 2009, motor vehicle emissions were responsible of 97% of the emissions of carbon monoxide (CO), 96% of

nitrogen oxides (NO_x), 40% of particles matter under 10 micrometers (PM_{10}), 97% of hydrocarbons (HC), and 32% of sulfur dioxide (SO_2) (CETESB, 2010).[1] Hydrocarbons, NO_x, and other volatile organic compounds contribute to the formation of ozone (O_3) by reacting with sunlight (Kinney et al., 2011). Moreover, a comparison of the emissions of CO and NO_x during weekdays and weekends shows peak of them at rush hour in the morning and evenings; O_3 concentrations behave differently with higher concentrations during the afternoon between 2 pm and 3 pm given the solar radiation at that time (Carvalho et al., 2015).

Other sources of pollution in this area are industrial processes, waste burning, and handling and storage of fuels (CETESB, 2002). Industrial processes account for the rest of the emissions of CO, HC, NO_x, and SO_2. In the case of PM_{10}, secondary aerosol formation and resuspension of soil particles, are a bigger contribution to emissions than industrial processes (CETESB, 2010).

In the SPMA, meteorological conditions are unfavorable to the dispersion of pollutant: during winters, there are frequent low altitude thermal inversions, and during other seasons, strong solar radiation occurs. Moreover, the topography of the SPMA contributes to the deterioration of air quality considering that the area is formed by floodplain surrounded by mountains in the north and northwest (as Figure B.1 shows), receiving predominant winds from the ocean at southeast (Ribeiro and de Assuncao, 2001).

2.3 Potential Health Impact of Air Pollution

Urban air pollution has become one of the most critical issues in metropolitan cities everywhere. The rapid growth of urban transportation and industries that character-

[1] Overtime, the contribution of vehicle emissions does not change significantly for CO, NO_x, HC, and PM_{10}; the contribution was higher for SO_2 in 2001 reaching 56% (CETESB, 2002).

ized the urbanization in many developing countries makes the environmental problems much greater. While these processes are positively related to economic activity, they also introduce a health burden. So, how exposure to air pollution could affect health at birth?

Pollution may affect infants health while in the womb and after birth. Before birth, exposure to air pollution may deteriorate fetal development through the effect on the mother's health that reduces nutrients required to development. It may also have a direct effect on infants if it crosses the protective barrier of the placenta. Consequences of such exposure may include interrupted placental development, fetal growth restriction, higher likelihood of a preterm delivery, heart anomalies, and lower weight at birth. After birth, air pollution may cause several health issues such as respiratory and cardiac troubles. These later effects may be influenced by the effects before birth that make children more vulnerable, yet there is not a clear understanding of the functional form or magnitude of these interactions.

A growing body of the economic literature has shown that pre- and post-natal exposure to urban air pollution has an adverse effect on infants' health at birth and infant mortality (e.g.,Chay and Greenstone, 2003; Currie et al., 2009; Currie and Walker, 2011; Knittel et al., 2015). Prenatal exposure to air pollution also has consequences after birth, for instance, on performance at school (Sanders, 2012). While these studies provide evidence of the effects on health in developed countries, their implications for urban air pollution in developing countries remain unclear. In particular, the differences in air pollution levels and population's health between developed and developing nations restrict the extrapolation of results from the former.

In developing countries, epidemiological studies have shown a correlation between air pollution and health (see, for example, Gouveia et al. 2004, Lin et al. 2004, Borja-Aburto et al. 1997). These cross-sectional articles fail to account for several sources

of confounding effects. First, families may self-select into residential location. If those who prefer to live in cleaner places are also healthier, wealthier, or have greater access to health care, and/or make higher quality investment in their children, the results will be biased upward. Second, the estimates would be biased downward if families adopt avoidance behaviors when they face environmental risks. Third, air pollution may be correlated with economic activity, and the latter also may affect infant health directly.

One of the few papers using applied microeconometric techniques in a developing country, instruments air pollution with thermal inversion episodes in Mexico City (Arceo et al., 2016). Authors link the number of thermal inversions per week, weekly pollution levels, and contemporary infant mortality. They show that increasing PM_{10} or CO by 1% raises infant mortality by 0.42% or 0.23%, respectively. My work builds on this study. As an extension, I explore the effects of pollution on fetal development. This analysis will allow to understand how the effects after birth differ depending on the level of exposure before birth. Moreover, even though I also use thermal inversions to address the well known concerns related to estimating the effects of air pollution on health, I take a more conservative approach to minimize the difficulties that the lack of data on all pollutants brings. Similar to Jans et al. (2018), I employ inversion episodes in an intention-to-treat analysis.[2]

2.4 Data and Summary Statistics

To study the effects of exposure to air pollution on health at birth, I combine several datasets. The following sections describes each data source. I include in my analysis

[2]Other studies in developing countries providing causal effects of pollutants on health at births are Rangel and Vogl (2018) and Bharadwaj et al. (2017). The first studies pollution related to sugarcane harvest fires in Brazil; the latter focuses on CO (and related pollutants) in Chile.

the universe of births in the SPMA between 2002 and 2009. To being able to analyze the time *in utero* for all of them, I examine air pollution and thermal inversions in the period from April, 2001 to December, 2009.

2.4.1 Pollution

Air pollution data come from air monitoring stations operated by the State of São Paulo's environmental agency *Companhia Ambiental do Estado de São Paulo* (CETESB). I study five pollutants: particulate matter under 10 micrometers (PM_{10}), carbon monoxide (CO), ozone (O_3), nitrogen oxides (NO_x), and sulphur dioxide (SO_2). For the period 2001-2009, PM_{10} data are available for 25 stations; CO is collected in 16 stations; O_3 data are available in 15 stations; NO_x data are drawn from 12 stations; and SO_2 information is available for 8 stations.

Data are organized in hourly observations of pollution concentrations at the station level. For all pollutants except CO, I calculate daily average concentration of pollutants for every day with at least 8 hours of raw data. For CO, I use the hourly observations to calculate the maximum daily 8-hour average. Then, for all pollutants, I compute rolling week averages. That is, for each date t, I take the average of the daily measure for the week from $t-6$ to t.

I present in the top panel of Table 2.1 descriptive statistics of the concentration of pollutants (statistics by station can be found in Table B.1). The mean level of pollution in the SPMA is higher than in California and Sweden but lower than in Mexico City based on levels of PM_{10}, CO, and SO_2. For instance, the average level of PM_{10} in the Brazilian data reaches 42 $\mu g/m^3$ whereas that mean in California and Sweden is below 30 $\mu g/m^3$ (Knittel et al., 2015; Jans et al., 2018) and it is around 67 $\mu g/m^3$ in Mexico City (Arceo et al., 2016). However, the mean levels of O_3 in the SPMA and Mexico City are similar (around 33 $\mu g/m^3$).

2.4.2 Weather

Weather covariates are key elements in the analysis because they are directly related to thermal inversions, economic activity, and health. Temperature, relative humidity, and wind data are collected by CETESB. Rainfall data come from the São Paulo department of water and energy DAEE -*Departamento de Águas e Energia Elétrica*. I convert daily hourly observations of all weather variables (except wind) into daily averages if there are at least 8 hours of raw data for that date. Then, I compute rolling week averages from daily interpolated data.

For wind, I consider the daily prevailing wind as the measure of daily summary. From raw data that are coded as angles in degrees (0 indicates wind from due North, and 180 corresponds to wind from due South), I define the daily prevailing wind as the sector of the wind rose that contains the most frequent occurrences, starting from the sector centered at due north and then increasing in intervals of 10 degrees.

Not all the CETESB stations that collect air pollution data, recover weather data too, and some stations that provide weather data do not measure the concentration of pollutants. For those stations collecting only pollution data, I construct weighted daily averages of weather covariates employing data from stations within a radius of 20 miles. I use the inverse of the distance between stations as weights.

In panel B of Table 2.1, I present mean and standard deviation of weather covariates. These statistics are based on observations measured at the 25 stations that collect PM_{10} data. Statistics do not change significantly when I look at fewer stations such as the ones measuring other pollutants. Tables B.1 and B.3 show descriptive statistics by station confirming that.

Table 2.1. Descriptive Statistics: Pollution, Weather Conditions, and Thermal Inversions

	Mean	SD	Observations	Stations	Dates
Panel A. Pollutants					
Particulate Matter (PM_{10}) ($\mu g/m^3$)	42.2	17.3	57,162	25	2,286
Carbon Monoxide (CO) (ppm)	1.3	0.6	34,416	16	2,151
Ozone (O_3) ($\mu g/m^3$)	33.5	12.6	34,359	15	2,291
Nitrogen Oxides (NO_x) (ppb)	79.6	57.6	21,394	12	1,783
Sulphur Dioxide (SO_2) ($\mu g/m^3$)	11.4	5.9	14,421	8	1,803
Panel B. Weather variables					
Mean Temperature ($Celsius$)	20.3	2.7	57,162	25	2,286
Max Temperature ($Celsius$)	25.8	3.2	57,162	25	2,286
Min Temperature ($Celsius$)	16.5	2.7	57,162	25	2,286
Mean Relative Humidity (%)	76.3	8.3	57,162	25	2,286
Max Relative Humidity (%)	92.4	6.2	57,162	25	2,286
Min Relative Humidity (%)	52.5	11.3	57,162	25	2,286
Rainfall ($millimetre$)	4.1	4.7	57162	25	2,286
Percent of winds originating from octant...					
Wind NNE	9.8	7.2	57,162	25	2,286
Wind ENE	13.4	7.6	57,162	25	2,286
Wind ESE	15.6	10.7	57,162	25	2,286
Wind SSE	28.6	14.3	57,162	25	2,286
Wind SSW	7.2	6.0	57,162	25	2,286
Wind WSW	2.4	2.2	57,162	25	2,286
Wind WNW	7.5	7.1	57,162	25	2,286
Wind NNW	10.2	8.2	57,162	25	2,286
Missing or Without prevailing wind	5.2	10.0	57,162	25	2,286
Panel C. Thermal inversions					
At least 1 inversion (%)	71	45	3,197	1	3,197
Number of inversions	4.6	3.1	3197	1	3,197
At least 1 inversion below 1.3 km (%)	66	47	3,197	1	3,197
Number of inversions below 1.3 km	2.2	2.1	3,197	1	3,197
Strength of inversion ($Celsius$)	1.3	1.2	3,197	1	3,197
Height of inversion ($meter$)	148	112	3,197	1	3,197
At least 1 date with missing data (%)	29	45	3,197	1	3,197
Number of missing data dates	0.5	1.1	3,197	1	3,197
Weeks with non-missing measures of inversions					
At least 1 inversion (%)	99	5.1	2,278	1	2,278
Number of inversions	6.5	1.1	2,278	1	2,278
At least 1 inversion below 1.3 km (%)	93	25	2,278	1	2,278
Number of inversions below 1.3 km	3.2	1.8	2,278	1	2,278
Characteristics of inversions below 1.3 km					
Strength of inversion ($Celsius$)	1.0	0.9	2,278	1	2,278
Height of inversion ($meter$)	102	75	2,278	1	2,278
Number of inversions High Δ Temp	1.4	1.5	2,278	1	2,278
Number of inversions Medium Δ Temp	0.9	0.9	2,278	1	2,278
Number of inversions Low Δ Temp	0.8	0.8	2,278	1	2,278

Notes: Observations are at station-date level; they are measures over the preceding week. In the top part of panel C, for weeks with missing information about inversions in a date, variables are coded as 0.

2.4.3 Thermal Inversions

To identify thermal inversion episodes I use vertical temperature profile of data collected by CETESB and the University of Wyoming. Balloons are launched twice each day, at 12 UTC and 00 UTC (10 am and 10 pm of the night before in Brazil, respectively); they gather temperature, humidity, and wind data as they ascend through the troposphere.

I use all diurnal data to find boundary layer inversions every time a temperature at a given altitude was warmer than the temperature at an altitude below it. The opposite non-monotonic temperature gradient allows me to identify the top of an inversion. The station collecting thermal inversion data is located at 731 meters above sea level, so I ignore inversion episodes that occurred below that level.[3] More than one inversion can be found each day at different altitudes; I focus for each date on the inversion that occurs closer to the ground. I construct for each date rolling week counts of inversion episodes between that date and the previous six dates. In less than 7.2% of the dates considered in the analysis, thermal inversion data were not collected. This means that in 29% of the weeks there is at least one date without inversion data, and they represent on average less than a date per week (Table 2.1, panel C). I take a conservative approach to deal with these missing values: I assign a zero to each rolling week with at least one date with missing information, and I control semi-parametrically in my specification for the number of missing values in each week.

At least one inversion episode occurs almost every day (Table 2.1, bottom part of panel C), and, on average, three days every week inversions occur close to the ground. I classify inversions as being close to the ground when they occur below 1.331 meters

[3]In my period of analysis, inversions with a base below 731 meters are less than 4% of the inversions. Results only change marginally when I leave them in my sample.

from the sea level. The 1.3 km limit is based on the fact that the station collecting inversion data is located 731 meters above sea level and that inversions that occur closer to the ground generally control ground-level pollutant concentrations (Abdul-Wahab et al., 2004). I follow previous papers in the literature and use 600 meters above the ground to characterize these inversions (Jans et al., 2018).[4]

Inversion strength is defined by the difference in temperature between the top and the base of the inversion. Higher positive values correspond to stronger inversions. I define three levels of strength (high, medium, and low) based on the tertiles of the distribution of temperature change.[5] Data show that the average temperature differences between the two air layers is 1 Celsius. On average, there are between one and two strong inversions closer to the ground per week. Inversion depth is measured by the height of the inversion. The average depth is 0.12 km.

2.4.4 Birth Data

I study singletons born in hospitals in the SPMA between 2002 and 2009. Data come from individual-records in DATASUS, the Brazilian Ministry of Health's Usage Information System. Brazil vital registration system provide a complete coverage of all birth. These records have a high coverage rate that exceeds 98% in the mid-2000s (Jorge et al., 2007). They provide information about newborns' health, their mothers' characteristics, and location of residence.

To link daily rolling weeks of weather and pollution data to vital records, I use information about location of residence available in vital records. I approximate local conditions by averaging location-level weather and pollution in each date. I proceed in three steps based on the information available. First, for newborns whose

[4]Section 2.5 presents further evidence supporting this choice.

[5]Strength values used to define tertiles are 1 and 2.2 Celsius.

mothers reside in the São Paulo City and I recover their district of residence, I use contour data from *Centro de Estudos da Metropole* (CEM/USP) to identify the district polygon centroid. Second, for infants in the capital with unidentified district, I use the weighted polygon centroid employing 2000-Census population as weights for each district listed on the CEM/USP data. Third, for newborns who reside at birth in municipalities outside the São Paulo City, I use the weighted centroid using all districts within that municipality and 2000-Census population as weights.

I identify 135 locations of residence (97 districts in the capital and 38 municipalities outside of it). Then, I calculate the distance between a centroid and each monitor station using latitude and longitude location data. I employ locations that have a weather station within 20 miles in my analysis. That results in 123 locations of residence. Finally, I compute a weighted average of the weather variables and pollutants concentration weighting the data measured in each station by the inverse of the distance from the station to the centroid.

Outcomes of interest are birth weight and prematurity. Weight at birth is measured continuously in grams and as indicators for low or very low birth weight ($<$2,500 grams or $<$1,500 grams, respectively). Gestational age at birth is coded in categories in the Brazilian birth certificates. I define indicators for preterm ($<$37 gestational weeks) and very preterm ($<$32 gestational weeks) births. To reduce computational demands, I collapse observations at individual level by date of birth and location of residence. The final sample includes 316,970 location-date cells.

In Table 2.2, I show descriptive statistics of the health outcomes. Newborns weight on average 3.2 kilograms. 8 per 100 infants are low birth weight, and 1 in 100 weight at birth less than 1,500 grams. Prematury is found in almost 7 out of 100 newborns; 1 in 100 infants are very premature. I also include in panel B of Table 2.2 the mean and standard deviation of the covariates I use in my empirical strategy; they coincides

Table 2.2. Descriptive Statistics: Health Outcomes

	Mean	SD	Min	Max	Observations	Weights
Panel A. Health outcomes						
Birth weight	3151.6	214.2	1085	4560	326,280	16,477,732
Low birth weight (*per 100*)	8.2	11.1	0.0	100	326,280	16,477,732
Very low birth weight (*per 100*)	1.22	4.4	0.0	100	326,280	16,477,732
Preterm (*per 100*)	7.2	10.7	0.0	100	326,280	16,477,732
Very preterm (*per 100*)	0.4	2.7	0.0	100	326,280	16,477,732
APGAR score minute 1	8.2	1.5	0	10	326,280	16,477,732
APGAR score minute 5	9.3	1.3	0	10	326,280	16,477,732
Panel B. Weather Covariates						
Mean Temperature (*Celsius*)	20.4	2.6	12.2	27.7	326,661	16,477,732
Max Temperature (*Celsius*)	25.8	3.1	15.1	34.5	326,661	16,477,732
Min Temperature (*Celsius*)	16.6	2.7	7.7	23.1	326,661	16,477,732
Mean Relative Humidity (*percentage*)	76.0	7.1	50.8	99.2	326,661	16,477,732
Rainfall (*millimetre*)	4.1	4.4	0.0	36.1	326,661	16,477,732
Percent of winds originating from octant...						
Wind missing	5.6	7.3	0.0	68.3	326,661	16,477,732
Wind NNE	9.8	5.6	0.0	46.4	326,661	16,477,732
Wind ENE	12.8	5.8	0.0	51.0	326,661	16,477,732
Wind ESE	15.7	7.2	0.1	66.6	326,661	16,477,732
Wind SSW	6.6	4.0	0.0	35.7	326,661	16,477,732
Wind WSW	2.4	1.7	0.0	21.8	326,661	16,477,732
Wind WNW	8.3	6.1	0.0	64.9	326,661	16,477,732
Wind NNW	10.2	7.6	0.0	48.3	326,661	16,477,732
Number of inversions below 1.3 km	2.3	2.1	0.0	7.0	326,661	16,477,732
At least 1 date with missing data	27.7	44.8	0.0	100.0	326,661	16,477,732
Number of missing data dates	0.5	1.2	0.0	7.0	326,661	16,477,732
Panel C. Maternal and infant demographics						
Male (*per 100*)	51.2	20.3	0.0	100	326,280	16,477,732
White (*per 100*)	54.7	25.3	0.0	100	326,280	16,477,732
Black (*per 100*)	24.1	20.2	0.0	100	326,280	16,477,732
Education 12+years (*per 100*)	17.4	12.8	0.0	100	326,280	16,477,732
Married/Committee relationship (*per 100*)	45.7	15.2	0.0	100	326,280	16,477,732
<25 years old	42.5	9.5	0.0	100	326,280	16,477,732
25-35 years old	47.9	8.3	0.0	100	326,280	16,477,732
>35 years old	9.7	4.9	0.0	100	326,280	16,477,732
First born	69.4	11.9	0.0	100	326,280	16,477,732
Higher parity	30.5	11.9	0.0	100	326,280	16,477,732

Notes: Observations are at location-date level; they are measures over the preceding week.

with those in Table 2.1.

2.5 Thermal Inversions, Weather Conditions, and Pollution in Brazil

Thermal inversion layers occur when temperature increases with altitude, resulting in a mass of hot air on top of a mass of cold air. Normally, temperature in the troposphere decreases with height. During an inversion, the coldest, densest air is at the surface and its density steadily decreases with increasing height. The stable stratification of air resists uplifting of the particles from surface to atmosphere because layer acts like a cap in which vertical air movement almost remains nil and pollutants are trapped close to the ground (Rendón et al., 2014; Jans et al., 2018).

The formation of inversions is more common during nights when cold ground temperatures cool the air that is closer to the ground creating warm air over cold air (Jacobson, 2005). Roughly speaking, the diurnal cycle of inversions starts after sunset when sunlight intensity is negligible, temperature decreases, and ground (or initial elevated) inversions form (Sadar, 2018). During night, when temperature decreases, these inversions strengthen. They are strongest at sunrise when surface temperature begins to increase from overnight minimum, and they become weaker once the warmer ground warms the air. By midday inversions are completely burned off. Wind speed and rain also contribute to weaken inversions (Czarnecka et al., 2019). Because the dynamics of these inversions are driven by the heating of the surface, episodes become more frequent (and last longer) in winter because day lengths are shorter, sun angles lower, and surfaces wet or frozen; summer speeds up the process of breaking inversions (Kikaj et al., 2019).

The shape of the landscape has a role in the formation, lifetime, and intensity of inversion (Rendón et al., 2014). In valleys, denser and heavier cool air flow down the slopes and settler under the warm air leading to stronger effects. Inversions can be produced when a layer of cool air descends through a layer of hot air from vertical air

movements or from horizontal movements of air at different temperatures (Jacobson, 2005).

There are two factors of the formation of inversions that are worth noting for my study. First, the formation of thermal inversion is independent of the current economic activity (including industrial activity and traffic) conditional on weather variables. In the longer run, urbanization influences the heating of the surface and wind, and it may affect both formation and breakup of inversion episodes (Rendón et al., 2014). Yet, these changes take many years. Lima and Magaña Rueda (2018) study urbanization and climate change in the SPMA for the period 1930-2015 and show that the average of maximum temperature increases up to 1.1 $Celsius$ every 20 years.

Second, the meteorological factors associated with thermal inversions (e.g., temperature) may directly impact health; similarly, air pollution affects health. However, conditional on weather conditions and air pollution, thermal inversions do not represent a health risk by themselves. It is the accumulation of pollution that occurs when there is an inversion episode that is likely to be risky. Then, the effect on health may be larger when inversions lock pollutants closer to the ground and when they are stronger leading to stronger locking of pollutants.

I proceed next to characterize thermal inversion episodes in the SPMA. Figure 2.1 shows that there is a higher accumulation of pollutants when more thermal inversions occur in a week. Density plots correspond to all inversions (graph a), inversions within 1.3 km from the ground (graph b), and strong inversions (in the top tertile of the change in temperature) closer to the ground (graph c). Each graph shows the distribution of the concentration of PM_{10} for weeks with 0 to 7 inversions (thicker solid lines in black and blue, respectively). This distribution is centered at a higher level for weeks with more inversions. For inversions closer to the ground and stronger

ones, the difference between the means of these distributions is larger. Figure 2.2 confirms that the distribution of the accumulation of CO, SO_2 and NO_x for weeks with different number of inversions within 1.3 km from the ground present a similar pattern than for PM_{10}. This pattern is not seen for O_3 a pollutant that is affected in a different way than other pollutants by sunlight and weather conditions.

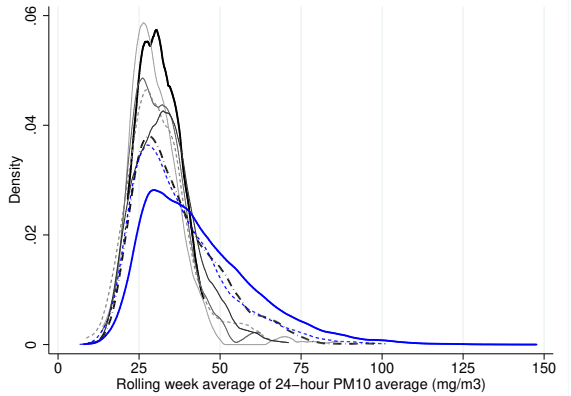
The seasonality patterns associated with thermal inversions are clearly illustrated in Figure 2.3. For each calendar month, this figure shows the average number of inversions that occurs up to 1.3 km from the ground in a week (bars), and it compares the count with the weekly average of weather variables: ground temperature, relative humidity, and rainfall. Weather conditions and the frequency of thermal inversion episodes are negatively correlated. During the spring (October to December) and the summer (January to March), the average weekly temperature, humidity, and rainfall are higher than during the fall (April to June) and the winter (July to September), while the average number of weekly inversions is lower during warmer seasons than the colder ones.[6]

I complement this analysis by plotting locally weighted regressions (LOWESS) of predicted number (or strength) of inversions on weather variables (Figure B.3). Counts of inversions in a week are predicted using regressions that include weather controls, year fixed effects, and week-of-the-year fixed effects.[7] Inversion strength is predicted in a similar regression that also controls for the count of inversions to identify the additional role of strength over frequency. Values for weather variables are trimmed above the 95th percentile and below the 5th percentile. Graphs jointly illustrate an inverted-U-shaped relationship between ground temperature and the number of inversions per week (first three graphs from left to right on the top panel).

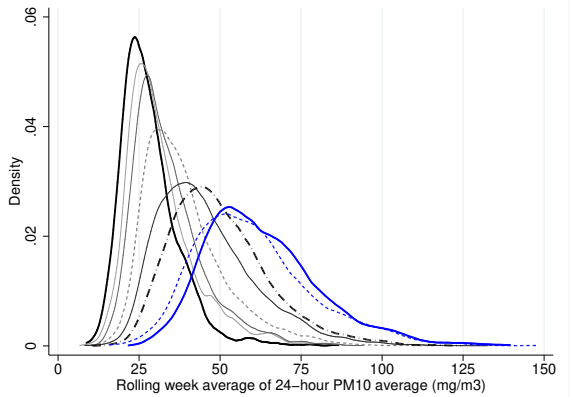
[6]During warmer days the average of the lower inversion's base height is higher, and they are more likely to occur above 2km from the ground.

[7]See Section 2.7.1 for a discussion of the model used in these regressions.

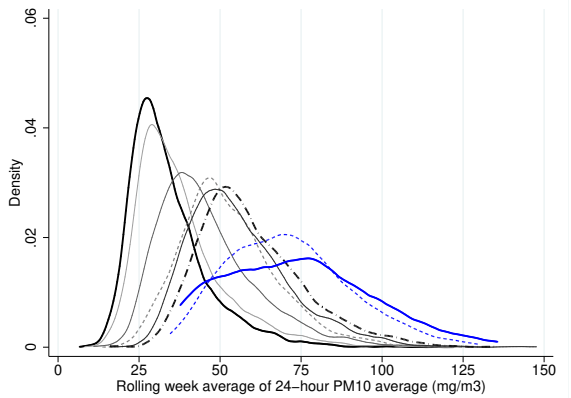
(a) All inversions



(b) Inversions below 1.3 km



(c) Inversions below 1.3 km -High ΔTemp



7-days count of inversions

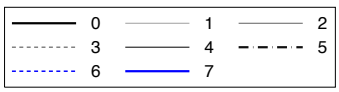


Figure 2.1. Relationships Between PM_{10} and Thermal Inversions

Notes: Figures plot the kernel density of rolling week averages of 24-hour PM10 average for all inversions (figure a), inversions that occur close to the ground (figure b), and stronger inversion (figure c). In each figure, plots show densities for weeks with 0 to 7 daily inversions.

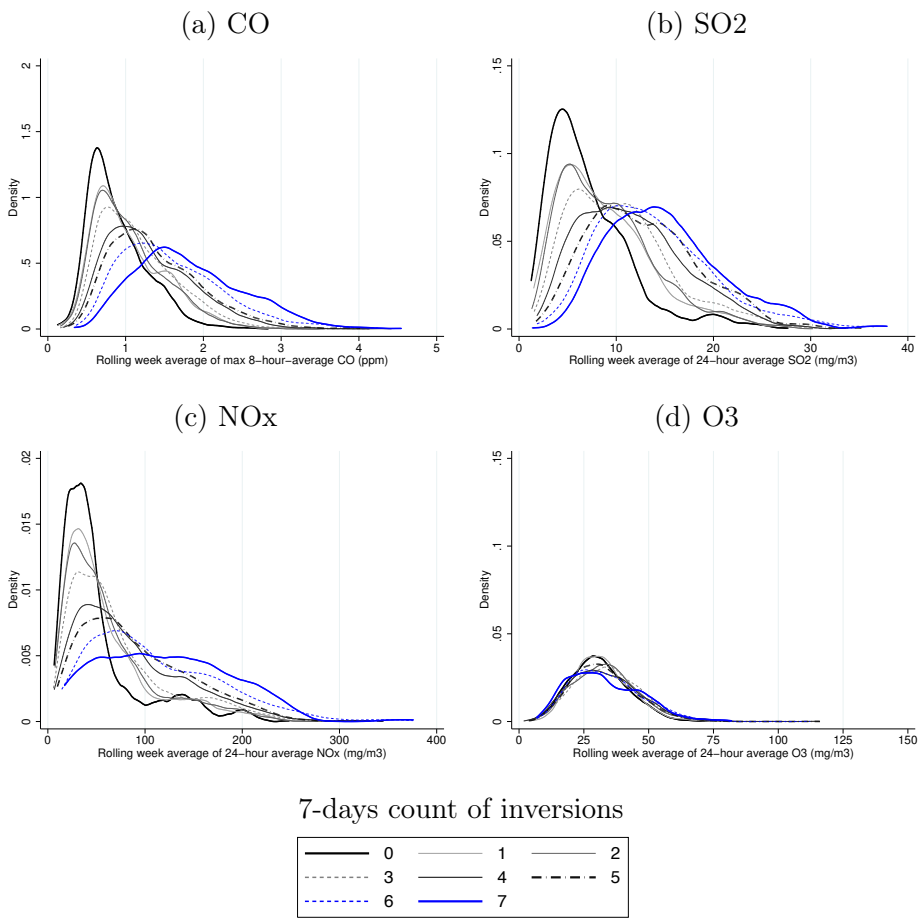


Figure 2.2. Relationships Between Pollutants and Number of Thermal Inversions

Notes: Figures plot the kernel density of rolling week averages of pollutants. In each figure, plots show densities for weeks with 0 to 7 daily inversions that occur within 1.3 km from the ground.

A similar relationship is seen for average inversion strength (fourth graph in the top panel). The number and strength of inversions reduce as relative humidity increases (center panel); inversions and rainfall show a U-shape relationship (bottom panel).

Similarly, I use LOWESS to explore potential non-linearities between thermal inversions' characteristics and (predicted) concentration of pollutants. The top panel of Figure B.4 confirms a sharp reduction in the average concentration of pollution

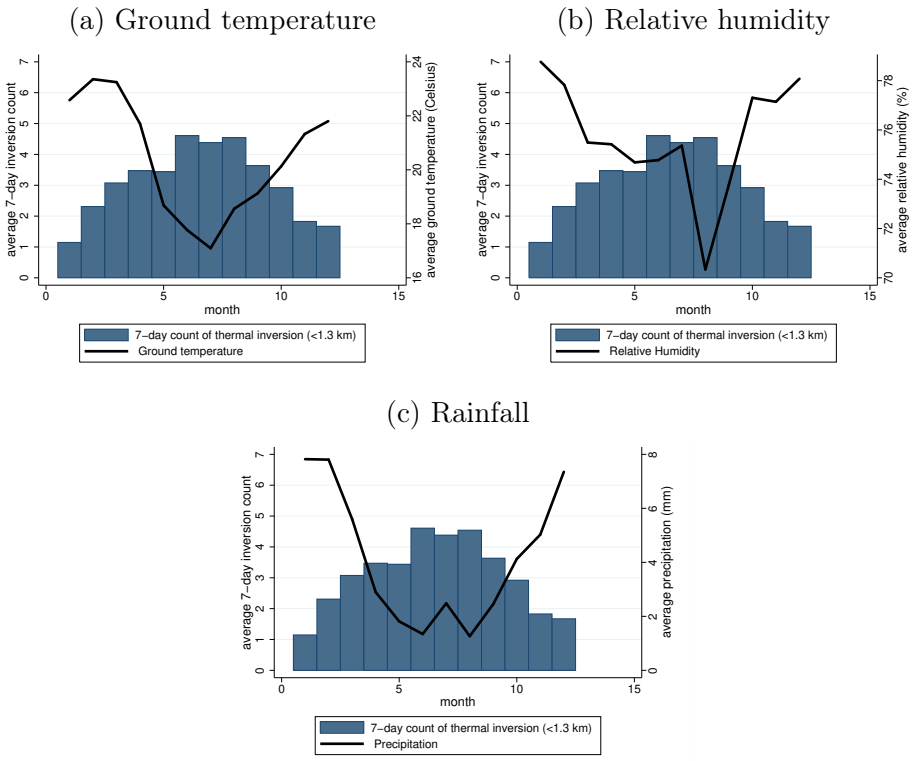


Figure 2.3. Number of Thermal Inversions in a Week and Weather Variables by Calendar Month

Notes: Figures plot average of the number of thermal inversions in a week and average weather in a week by calendar month. Inversions indicate those that occur below 1.3 km.

when inversions occur at a higher distance from the ground. Specifically, a steeper slope in the accumulation of pollutants is seen for inversions that has a base between 1km and 2km from the ground compare to the slope of that relationship for more elevated inversions. Based on these figures, as well as on the relationships shown in Figure 2.1, I focus on inversions that occur closer to the ground ($< 1.3km$) in my analysis.

Panel b of Figure B.4 shows that the concentration of pollution increases almost linearly with the number of inversions closer to the ground; panel a of Figure B.5

suggests that the relationship between concentration of pollutants and inversions strength seems somehow linear too. As expected, these patterns are different for O_3.

2.6 Estimation

I aim to estimate the effect of air pollution on infant health at birth. Selection into residential location, avoidance behaviors, and the correlation between air pollution and economic activity prevent the estimation of unbiased effects from the simple study of pollution and health cross-sectional data. Furthermore, the link between thermal inversions and pollutants that are not in my data limits the interpretation of the effects of each particular pollutant using inversions as an instrument of it. In other words, many pollutants are locked in an inversion episode. These pollutants may be correlated with each other, and they may also have a direct effect on infants' health or economic activity. So, the unobservables in a second-stage regression for an instrumental variables estimation would be correlated to the outcome and to the instrument.

In order to avert these concerns, I first formally test the pollution effects of thermal inversion episodes, which is akin to a first stage relationship. Specifically, I explore the effect of the frequency of inversions on the concentration of pollutants and the role of inversions strength in that. My second estimation links thermal inversions to health outcomes. In the last one, I estimate an intention-to-treat effect.

Using observations at the station (s)-date (t) level, I estimate alternative versions of the following model,

$$Poll_{st} = \beta_0 + \beta_1 \, Inv_t + X'_{st} \, \gamma + \mu_{ws} + \mu_y + \epsilon_{st} \tag{2.1}$$

where $Poll_{st}$ is an average daily pollution concentration from date $t-6$ to t measured by station s. I explore five pollutants: PM_{10}, CO, O_3, NO_x, and SO_2. Inv_t indicates the number of inversion episodes that occurred from date $t-6$ to t. As I discussed in Section 2.5, estimates of non-parametric models suggest that the concentration of pollutants increases almost linearly with the number of inversions (with the only exception of O_3). To ease the interpretation of the effects, I transform the 7-days counts to be expressed in standard deviation (SD) units based on the weekly count of inversions in the sample.

Considering the relationship between weather conditions and thermal inversion formation, I control for them in a flexible way. The vector of covariates, X_{st}, includes cubic polynomials in weekly average temperature, weekly maximum temperature, weekly minimum temperature, weekly average humidity, direction of wind (fixed octants), and weekly average rainfall. It also includes an indicator for having at least one date between $t-6$ and t with missing inversion data and a count of the number of dates in that week without inversion data. μ_{ws} are station-by-weak of the year fixed effects that control for location-specific seasonality effects within each location; μ_y are year fixed effects, that account for common changes over time. I use two-way clustering, and standard errors are clustered at the station and date level to account for potential spatial correlation and common weather trends.

To explore the role of the strength of inversions, I add to Eq. (2.1) the average strength of inversions that occurred from date $t-6$ to t. Alternatively, I replace Inv_t with three variables that indicate the number of "high", "medium", and "low" strength inversions.

I study the effects of prenatal exposure to air pollution on health at birth employing specifications similar to Eq. (2.3). Following Rangel and Vogl (2018), I use a distributed lag model on data aggregated at the location and date of birth level.

Specifically, because I cannot backdate conception from Brazilian birth records, I use lags for 38 weeks from the birthdate and assume 9-month pregnancies. For births in location of residence l on date t, I estimate,

$$H_{lt} = \sum_{s=0}^{38} \beta_s Inv_{t-7s} + \sum_{s=0}^{38} X'_{l,t-7s}\ \gamma_s + \mu_{wl} + \mu_y + \epsilon_{lt} \tag{2.2}$$

where H_{lt} is an average birth outcome for births in location l on date t. Inv_{t-7s} are the count of thermal inversion episodes for the week leading up to date $t-7s$, where s is measured in weeks and t is measured in days. Alternatively, I replace this count with the count of different strength inversions to explore potential non-linearities in the effect of inversion on health. The vector of covariates is defined as in Eq. (2.1) for each one of the lagged weeks. I also control for location-by-week of the year fixed effects and year of birth fixed effects. Regressions are weighted by the number of births in each location-date cell.

From Eq. (2.2) I recover many week-specific estimates, yet they might be individually imprecise and unwieldy to report (Almon, 1965; Sargan, 1980). Thus, I report 13-week sums of the coefficients that correspond to the last, second-to-last, and third-to-last periods before birth —alike trimesters of gestation,

$$\beta_{\{\underline{T},\bar{T}\}} \equiv \sum_{s=\underline{T}}^{\bar{T}} \beta_s \tag{2.3}$$

where the periods $\{\underline{T},\bar{T}\}$ are $\{0,12\}$, $\{13,25\}$, and $\{26,38\}$. Each of these coefficients indicates the effect of a SD increase in the number of thermal inversions per week during periods of approximately three months. Defining exposure backwards from the date of birth is not ideal: the timing and length of pregnancy are endogenous. For some premature infants, the 38-weeks period might include weeks before their

conception. Moreover, those who are exposed to inversions since early in their time *in utero* might be positively selected because they were not born prematurely. Thus, estimates from the first three months of pregnancy should be interpreted carefully.

The key identifying assumption to get consistent estimates of β_s is that conditional on the weather controls and set of time and location fixed effects included in my model, no other unobserved factor influences both inversions and health at birth. Weather conditions is related to both of them, so I include them in my model. Moreover, I control for location-specific seasonality of birth and inversions by including location-by-week of the year fixed effects. I show in Section 2.7.1 that this model yields conservative estimates compared to controlling for common seasonality across locations.

2.7 Results

2.7.1 Effects of Thermal Inversions on Air Pollution

Table 2.3 presents findings from estimating Eq. (2.1). Panel A (column 1) shows that one SD increase in the number of thermal inversions in the preceding week increases the average concentration of PM_{10} by 0.9 $\mu g/m^3$ in that week. That is an increase of 2% of the average weekly concentration of PM_{10} in the sample. The accumulation of CO and SO_2 also increases by 2% of the weekly average (columns 2 and 3). For NO_x and O_3 (columns 4 and 5), I observe a positive effect on their accumulation of the number of inversions, yet that is statistically insignificant and economically smaller.

I confirm that the accumulation of pollutants also increases with the strength of inversions in panels B and C. In the first panel, I add to my main specification a continuous measure of the strength of inversions (i.e. change in temperature between

the bottom and top layers). I find that a SD increase in the average strength of inversions within a week, leads to a 1.3 $\mu g/m^3$ higher PM_{10} concentration (3% of the weekly average) and raises CO concentration by 22 *ppm* (2% of the weekly average). Concentrations of the other pollutants also increase with an extra inversion in a week, yet these effects are statistically no significant–ranging from a 2% in SO_2 to a 0.07% increase in O_3.

In panel C, estimates come from a regression that replaces the total 7-days count of inversions with three counts over the same period for high, medium, and low strength inversions. Increasing the number of inversions with the highest strength leads to statistically significant larger concentration of PM_{10} and NO_x than increasing the number of medium and low strength inversions. The CO concentration increases similarly with the number of inversions in the third and second tertile of the strength distribution; this increase is larger than for less intense inversions. For SO_2 and O_3, I find that the different levels of strength do not affect pollution in a different intensity.

The other measure that characterizes inversions is their height. To explore how inversion depth affects pollution concentration, I add the weekly average inversion height as another covariate in Eq. (2.1). The bottom panel of Table 2.3 shows no evidence of a differential impact of the average inversion depth on the concentration of pollutants holding their frequency constant.

Table 2.4 presents robustness checks using specifications with different sets of weather controls and fixed effects. First, I add 12-weeks lags of inversions and weather conditions to Eq. (2.1) to evaluate whether conditions in the past weeks influence concentration of pollution in the week of analysis (panel A). I find that estimates do not change as much. Second, I include fewer temperature controls by dropping minimum and maximum temperature from my preferred specification (panel B); estimates are larger for all pollutants compared with my preferred specification, including for O_3.

The last two specifications include time and location fixed effects following Jans et al. (2018) and Arceo et al. (2016), respectively, rather than those used in Eq. (2.1). In panel C, I use year-by-month fixed effects, day-of-week fixed effects, and station fixed effects; in panel D, I control for bimonthly x station fixed effects, station fixed effects, and location-specific week of the year linear trend. I find that my main specification (Table 2.3, panel A) yields smaller effects than with Jans et al. (2018)'s model and similar effects than with Arceo et al. (2016)'s specification.

Overall, my model yields more conservative estimates than other specifications presented in Table 2.4. Compared to estimates of the accumulation of pollutants due to inversion formation in other papers looking at Sweden or Mexico (Jans et al., 2018; Arceo et al., 2016), my estimates are on the lower tale of the distribution of effects as well.

2.7.2 Effects of Thermal Inversions on Infant Health

Table 2.5 presents estimates from Eq. (2.2). I report full-sample estimates of the 13-week sums of coefficients for each of the birth outcomes. I find that a SD increase in the number of thermal inversions per week occurring in the last 13 weeks of gestation leads to a reduction in birth weight of 34.5 grams (column 1). For 13-25 and 26-38 weeks before birth, an increase in the number of inversions have statistically insignificant and economically small effects, although their differential effects might be related to concerns about selection form prematurity.[8]

The impact on birth weight is actually felt at the lower tail of the birth weight distribution: an additional SD of the number of inversion per week in the last period of gestation increases the incidence of low birth weight ($<$ 2,500 grams) by 16% (an

[8]Table B.4 shows week-specific estimates aggregated in 4-week sums that correspond to month of pregnancy. Results are less precisely estimated than when I aggregate 13 weeks, yet they show that more inversion episodes in any of the last months of the gestation have negative effects on newborns' health.

Table 2.3. Effect of Thermal Inversions on Pollution: Main Results

	PM_{10} ($\mu g/m^3$) (1)	CO (ppb) (2)	SO_2 ($\mu g/m^3$) (3)	NO_x (ppb) (4)	O_3 ($\mu g/m^3$) (5)
Panel A					
Count of inversions	0.933***	23.101***	0.257**	1.060	0.023
	(0.226)	(7.581)	(0.101)	(0.717)	(0.174)
Panel B					
Average strength (ΔTemp)	1.250***	22.396**	0.120	1.342	0.023
	(0.288)	(8.162)	(0.079)	(0.914)	(0.229)
Panel C					
Count "high" ΔTemp	1.695***	35.228***	0.341**	2.288*	-0.034
	(0.281)	(10.245)	(0.104)	(1.069)	(0.215)
Count "medium" ΔTemp	0.104	28.999***	0.158	0.313	-0.255
	(0.332)	(9.546)	(0.218)	(0.812)	(0.270)
Count "low" ΔTemp	0.227	-15.522	0.199	-0.976	0.575
	(0.322)	(10.400)	(0.194)	(0.724)	(0.346)
P-value Diff."high"-"medium"	0.00	0.60	0.39	0.03	0.49
P-value Diff. "high"-"low"	0.00	0.00	0.49	0.05	0.13
Panel D					
Average depth (ΔHeight)	0.367	-9.855	-0.058	-1.137	0.025
	(0.316)	(10.489)	(0.128)	(1.004)	(0.186)
WOY x District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weather Controls	Yes	Yes	Yes	Yes	Yes
Observations	57,162	34,416	14,420	21,393	34,359
Ymean	42.17	1260.41	11.44	79.59	33.46
Ysd	17.31	589.34	5.87	57.6	12.57
N Station	25	16	8	12	15
N Date	3197	3197	3197	3197	3197

Notes: Each panel correspond to a separate regression. Observations are at station-date level; they are measures over the preceding week. The sample corresponds to the period April 2001 to December 2009. All regressions control for an indicator of having missing information data over the preceding week, the number of dates with missing data in that week, and weather controls (cubic polynomials in mean temperature, maximum temperature, minimum temperature, humidity, direction of wind, and rainfall). In all specifications I include year fixed effects and week of the year x station fixed effects. Regressions in panel B and D also control for the number of inversions during the preceding week. Two-way clustering is employed; standard errors are clustered at station and date level. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Table 2.4. Effect of Thermal Inversions on Pollution: Alternative Specifications

	PM_{10} ($\mu g/m^3$) (1)	CO (ppb) (2)	SO_2 ($\mu g/m^3$) (3)	NO_x (ppb) (4)	O_3 ($\mu g/m^3$) (5)
Panel A. Lags of weather conditions					
Count of inversions	1.198***	21.959**	0.235	1.052	0.065
	(0.201)	(8.519)	(0.131)	(0.637)	(0.131)
Panel B. No min/max temperature					
Count of inversions	1.994***	30.619***	0.398***	1.952**	1.449***
	(0.267)	(7.825)	(0.098)	(0.693)	(0.222)
Panel C. Year-by-month FE, day-of-week FE, and location FE					
Count of inversions	2.469***	53.169***	0.456***	3.371***	1.445***
	(0.265)	(8.278)	(0.101)	(0.775)	(0.201)
Panel D. Bimonth x location FE, location FE, and location-specific week of by year trend					
Count of inversions	0.967***	22.435**	0.151	1.957**	-0.348
	(0.289)	(10.276)	(0.110)	(0.852)	(0.219)
Observations	57,162	34,416	14,420	21,393	34,359
Ymean	42.2	1260.4	11.4	79.6	33.5
Ysd	17.3	589.3	5.9	57.6	12.6
N station	25	16	8	12	15
N date	3,197	3,197	3,197	3,197	3,197

Notes: FE=fixed effects. Each panel correspond to a separate regression. Observations are at station-date level; they are measures over the preceding week. The sample corresponds to the period April 2001 to December 2009 and 123 locations of residence. Each panel presents a modified version of my main specification in Eq. (2.1) in each panel. In panel A, I add 12-weeks lags of weather conditions in my preferred specification; in panel B, I drop min/max temperature controls. The last two panels use different sets of fixed effects and my main set of weather controls. Panel C includes year-by-month fixed effects, day-of-week fixed effects, and station fixed effects; part D controls for bimonth-station fixed effects, station fixed effects, and a station-specific week of the year linear trend. Two-way clustering is employed in all regressions; standard errors are clustered at station and date level. *** significant at 1% level, ** significant at 5% level.

effect of 1.3 per 100 on a base risk of 8.1 per 100) and raises the incidence of very low birth weight (< 1,500 grams) by 50% (an effect of 0.6 per 100 on a base level of 1.2 per 100, columns 2 and 3). Columns 4 and 5 show that the effect of inversions on birth weight might be explained in part by their influence on the length of gestation. Rates of preterm birth (<37 weeks) and very preterm births (< 32 weeks) increases by 1.2 and 0.4 per 100 on a risk base of 7.3 and 0.4 per 100, respectively. Overall, these

negative effects highlight that the concentration of pollutant caused by the formation of inversions deteriorates newborns' health.

In the first three columns of Table 2.6, I explore the role of shorter pregnancies on the effects on birth weight. Two main causes of low birth weight are prematurity (being born too early) and intrauterine growth retardation (growing too slowly in the womb). To do that, I estimate Eq. (2.2) adding controls for gestational length. While the estimates do not change much in this specification, the effects on low birth weight and very low birth weight are significant at 5% level rather than 1% when controlling for gestational length. Shorter pregnancies do not account for the effect on birth weight.

I explore the impact of thermal inversion episodes on other birth outcomes in the rest of Table 2.6. Columns 4 and 5 present estimates for APGAR score at minute 1 or at minute 5. These scores are measures that summarize of how well a newborn tolerate delivery and conditions outside the womb, respectively. I show that increasing the number of inversions does not have a statistically significant effect on these scores.

Does thermal inversion formation affect fetal survival? I answer these question by studying the effect on the number of live births. This approach provides a proxy for survival *in utero* and has been used in previous studies (for example, Jayachandran (2009); Rangel and Vogl (2018)).[9] Columns 6 to 8 present estimates for the total

[9]Using the number of births as a broader proxy for survival relies on the assumption that the formation of thermal inversions (conditional on weather conditions) is unrelated to the number of conceptions. It allows to overcome two drawbacks related to using reported stillbirths. First, only fetal death at 20 or more weeks of gestation are required to be reported as fetal death. Losses prior to 20 weeks are classified as miscarriages, and reliable data on miscarriages is rarely available. A change in fetal health would be underestimated because stillbirths may be underreported and, even if all occurrences are correctly observed, it would only account for losses later in a pregnancy. Second, a shock may move the distribution of fetal losses to cross the 20 weeks threshold (Sanders and Stoecker, 2015). Then, a negative health shock may be followed by an decrease in the number of reported fetal deaths because some of previously reported fetal deaths do not survive until the reporting threshold, even though this would not indicate an improvement in fetal health. A negative shock may also raise the occurrence of fetal deaths by influencing fetal health and *in utero* surviving. Disentangling the magnitude of these effects seems implausible making the sign

Table 2.5. Effect of Thermal Inversions on Infant Health

	Birth weight (grams) (1)	Low birth weight (x100) (2)	Very low birth weight (x100) (3)	Preterm birth (x 100) (4)	Very premature (x100) (5)
Count inversions weeks t and t-12	-34.522***	1.269***	0.577***	1.226***	0.398***
	(8.879)	(0.353)	(0.169)	(0.412)	(0.082)
Count inversions weeks t-13 and t-25	2.342	-0.155	0.107	0.581	0.059
	(8.402)	(0.450)	(0.157)	(0.412)	(0.089)
Count inversions weeks t-26 and t-38	-8.256	-0.096	0.084	0.155	0.143*
	(7.243)	(0.381)	(0.167)	(0.392)	(0.083)
WOY x District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weather Controls	Yes	Yes	Yes	Yes	Yes
Observations	316,970	316,970	316,970	316,970	316,970
Ymean	3151.6	8.2	1.2	7.2	0.4
Ysd	214.2	11.1	4.4	10.7	2.7

Notes: WOY=week of he year. Observations are at location-date level and are weighted by the size of the local birth-cohort in that location-day. Estimates are 13-week sums of coefficients on weekly counts of thermal inversions. Coefficients indicate the effect of one SD increase in the weekly number of inversions over the corresponding window of time. All regressions control for an indicator of having missing information data over the preceding week and the number of dates with missing data in that week. Weather controls include cubic polynomials in mean temperature, maximum temperature, minimum temperature, humidity, direction of wind (fixed octants), and rainfall. Two-way clustering is employed; standard errors are clustered at location and date level. *** significant at 1% level, * significant at 10% level.

number of live births, male births, and female births, respectively. Because there are zero male or female births for some municipality-date (in around 10% of the cells), I transform this outcome with the inverse hyperbolic sine; I report the mean and standard deviation of the number of births per municipality-birthdate at the bottom of Table 2.6.[10]

I find that a SD increase in the number of thermal inversions per week occurring in any of the 13 weeks periods decreases the number of live births. Specifically, for

of the bias unknown.

[10]As described in Chapter 2, the inverse hyperbolic sine can be interpreted in exactly the same way as a standard logarithmic transformation (as approximating percent changes), and it is defined at zero.

the last gestational period, there is a 18.5% reduction in the number of live births. Compared to the average number of births, this estimate indicates that one less infant is being born on per municipality-date. This effect is seen for both male and females infants. Overall, I find evidence that thermal inversion formation increases fetal death. The last two columns in Table 2.6 show that prenatal exposure to thermal inversions also have a statistically significant effect on mortality within a week after birth.

Next, I analyze how the different levels of inversion strength affect health at birth in Table 2.7. I find that the number of "high", "medium" and "low" strength inversions during the last 13 weeks of the gestational period have a similar effect on most health outcomes. The only exception is preterm birth that seems to be affected more by "high" strength inversions. Potential explanations for these patterns include the fact that inversion strength affects different pollutants in different ways (with a clear differential increase when more "high" occur compared to other inversions only for PM_{10} and CO), there may be threshold in the level of pollution that affects birth outcomes that is reached even for "low" strength inversions, and the potential for interactive effects when inversions with different level of strength occur during the same week.

Table 2.8 shows that my main results are robust to adding covariates to the specification. Regressions control for municipality-birthdate averages of maternal characteristics (education, age, and marital status), parity (first born and higher parity), and infants' race. In Table 2.9, I use child and maternal characteristics as outcomes to show that they are not deferentially related to the formation of inversions. I only find a marginally significant effect on the average of children who are white or black.

To place the magnitude of my estimates in the literature, I translate my results into the "effect" of increasing one unit of a pollutant on birth weight. Because thermal

inversion episodes affect the concentration of different pollutants and they correlate with each other, I refrain form referring to this quantity as the effect of each pollutant on health at birth. Adding lags in Eq. (2.1), I estimate that in a period of 13 weeks, an additional SD in the number of thermal inversions per week increases PM_{10} by $3.9\mu g/m^3$ (SD=1.7) and CO by $0.11ppm$ (SD=36.4). Combining that with my main results on Table 2.5, I calculate a 8.8 grams (314 grams) reduction in birth weight per unit of PM_{10} (CO) averaged over the final three months of pregnancy.

While the evidence on the effect of pollution on health at birth is growing, many of these studies focus on infant mortality and only a few papers that look at health at birth offer a measure of the effect in terms of unit of pollutants. For instance, Currie et al. (2009) study the influence of pollution in health at birth for mothers in New Jersey over the 1990s using a model that controls for place-by-season and family characteristics constant over time. They find a reduction of birth weight of 0.4 gram per unit of PM_{10} during the last trimester of gestation. Rangel and Vogl (2018) quantifies the effect of prenatal exposure to smoke from sugarcane harvest fires employing changes in fire location and wind direction. Authors show that during the last three months of gestation, a SD increase in fires per week in the upwind reduces birth weight by 97 grams, that translates into a 5.2 gram decrease in birth weight per unit of PM_{10}. Bharadwaj et al. (2017) use mother fixed effects models and control for air quality alerts in Chile to estimate reductions of 16 grams per unit of CO *ppm*. All in all, my estimates are large compared to the literature.

2.7.3 Heterogeneity Analysis

Because health at birth differ by mothers and infants' characteristics and the accumulation of pollution may differ by the SES of different locations, I explore the effect of thermal inversion formation across subgroups of population.Table 2.10 presents

my results. Column 1 and 2 show results of regressions looking at male and female infants, respectively. Overall, males experienced worse effects on their health at birth compared to females. For instance, I find that thermal inversions during the last period of gestation lead to a reduction on birth weight for males that almost double the effect on females (a SD increase in the number of inversions per week reduces birth weight by 60 grams in males and 32 grams fro women). Considering that inversions affect males survival—and likely fetuses in the lower tail of the health distribution are those affected— my finding on males' fetal health highlights that the concentration of pollutants through inversions also deteriorate significantly the health of those born alive.

When using mothers' characteristics to stratify the analysis, I find that inversion formation has a larger effect for young mothers (below 25 years old) compared to older ones (columns 3 to 5). Larger effects were also expected for mother who are above 35 years old because they are more likely to have riskier pregnancies, yet I cannot ruled that out considering that the number of observations is much smaller than for the other groups of mothers and the weeks used in the analysis of these groups may differ. Lastly, columns 6 and 7 suggest that higher educated mothers may be more affected by the formation of thermal inversions.

2.8 Conclusions

Urban air pollution is one of the most critical issues worldwide. Growth in urban transportation and congestion are key elements behind that. A growing number of studies focused on developed countries have shown that prenatal exposure to air pollution harms health at birth and increases infant mortality. Central differences between developed and developing countries limit the understanding of consequences of

air pollution in the latter based on results from richer countries. Thus, the magnitude and scope of the impact of exposure to air pollution on infants health in less-developed countries still remain unclear.

In this chapter, I use data from one of the largest urban conglomerate, the Metropolitan Area of São Paulo. Taking advantage of the meteorological phenomenon of thermal inversion which arguably exogenously locks pollutants closer to the ground, I find that exposure to inversion episodes during the last three months of gestation decreases birth weight, increases the chances of prematurity, and affects fetal survival. These results are strongly significant. Overall, this results suggest that air pollution harms human capital in its earliest stage —*in utero*— and it may has lasting negative consequences on new generations.

Table 2.6. Effect of Thermal Inversion on Health at Birth: Other Outcomes

	Controlling for gestational length			APGAR score		IHSF(Live births)			Neonatal Mortality (x 100)	
	Birth weight	Low birth weight	Very low birth weight	1 minute	5 minute	All	Males	Females	1st-day	1st-week
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Count inversions weeks t to t-12	-37.019***	1.006**	0.579**	-0.025	-0.003	-0.185***	-0.173***	-0.156***	0.019	0.229**
	(9.730)	(0.425)	(0.222)	(0.063)	(0.063)	(0.039)	(0.039)	(0.038)	(0.070)	(0.105)
Count inversions weeks t-13 and t-25	11.396	-0.846*	-0.050	-0.065	-0.079	-0.119***	-0.129***	-0.094**	0.035	0.169
	(9.218)	(0.504)	(0.203)	(0.133)	(0.138)	(0.040)	(0.040)	(0.038)	(0.067)	(0.103)
Count inversions weeks t-26 and t-38	-9.198	-0.197	-0.061	-0.088	-0.065	-0.154***	-0.130***	-0.147***	0.038	0.124
	(8.029)	(0.430)	(0.183)	(0.096)	(0.097)	(0.041)	(0.040)	(0.038)	(0.063)	(0.118)
WOY x District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	316,970	316,970	316,970	316,970	316,970	316,970	316,970	316,970	316,970	316,970
Ymean	3155.9	8.1	1.2	8.3	9.3	7.4	3.8	3.6	0.04	0.1
Ysd	281.1	14.5	5.8	1.9	1.6	7.8	4.2	4.2	0.5	0.8

Notes: WOY=week of he year. IHSF=Inversion hyperbolic sine function. Observations are at location-date level and are weighted by the size of the local birth-cohort in that location-day. Estimates are 13-week sums of coefficients on weekly counts of thermal inversions. Coefficients indicate the effect of one SD increase in the weekly number of inversions over the corresponding window of time. All regressions control for an indicator of having missing information data over the preceding week and the number of dates with missing data in that week. Weather controls include cubic polynomials in mean temperature, maximum temperature, minimum temperature, humidity, direction of wind (fixed octants), and rainfall. I also control for mother's age, education, marital status and parity. Two-way clustering is employed; standard errors are clustered at location and date level. Ymean and Ysd for columns 1 to 3 present statistics for the number of live births (before being transformed with the inverse hyperbolic sine). *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Table 2.7. Thermal Inversion Strength and Infant Health: Tertiles of Strength

	Birth weight (grams) (1)	Low birth weight (x100) (2)	Very low birth weight (x100) (3)	Preterm birth (x 100) (4)	Very premature (x100) (5)
Count "high" ΔTemp ...					
weeks t to t-12	-36.563***	1.147**	0.622***	1.585***	0.393***
	(10.776)	(0.465)	(0.188)	(0.549)	(0.106)
weeks t-13 and t-25	-7.464	-0.493	0.410**	0.862	0.089
	(10.684)	(0.554)	(0.203)	(0.534)	(0.124)
weeks t-26 and t-38	-9.896	-0.196	0.084	0.042	0.181
	(9.218)	(0.506)	(0.210)	(0.536)	(0.110)
Count "medium" ΔTemp...					
weeks t to t-12	-29.794**	1.873***	0.276	0.715	0.384**
	(13.994)	(0.643)	(0.292)	(0.751)	(0.152)
weeks t-13 and t-25	10.997	-0.114	-0.156	0.627	0.099
	(12.523)	(0.710)	(0.283)	(0.640)	(0.151)
weeks t-26 and t-38	-2.849	0.044	-0.100	0.269	-0.006
	(12.195)	(0.658)	(0.247)	(0.684)	(0.152)
Count "low" ΔTemp...					
weeks t to t-12	-30.553*	1.257	0.811***	0.866	0.459***
	(15.765)	(0.835)	(0.297)	(0.756)	(0.158)
weeks t-13 and t-25	-5.651	1.046	0.162	0.527	-0.050
	(17.422)	(0.861)	(0.371)	(0.855)	(0.194)
weeks t-26 and t-38	-13.000	-0.133	0.425	0.259	0.212
	(17.373)	(0.755)	(0.361)	(0.791)	(0.214)
WOY x District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weather Controls	Yes	Yes	Yes	Yes	Yes
Observations	316,970	316,970	316,970	316,970	316,970
Ymean	3151.6	8.2	1.2	7.2	0.4
Ysd	214.2	11.1	4.4	10.7	2.7

Notes: WOY=week of he year. Each column indicates a separate regression. Observations are at location-date level and are weighted by the size of the local birth-cohort in that location-day. Estimates are 13-week sums of coefficients on weekly counts of thermal inversions. Coefficients indicate the effect of one SD increase in the weekly number of inversions over the corresponding window of time. Outcomes are expressed as per hundred. All regressions control for an indicator of having missing information data over the preceding week and the number of dates with missing data in that week. Weather controls include cubic polynomials in mean temperature, maximum temperature, minimum temperature, humidity, direction of wind (fixed octants), and rainfall. Two-way clustering is employed; standard errors are clustered at location and date level. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Table 2.8. Robustness Checks: Adding Covariates

	Birth weight (grams) (1)	Low birth weight (x100) (2)	Very low birth weight (x100) (3)	Preterm birth (x 100) (4)	Very premature (x100) (5)
Count inversions weeks t to t-12	-25.703***	0.862**	0.455***	0.901**	0.335***
	(8.808)	(0.353)	(0.166)	(0.410)	(0.080)
Count inversions weeks t-13 and t-25	7.893	-0.414	0.017	0.426	0.018
	(8.575)	(0.467)	(0.163)	(0.403)	(0.090)
Count inversions weeks t-26 and t-38	-0.342	-0.408	-0.028	-0.116	0.091
	(7.107)	(0.387)	(0.164)	(0.397)	(0.084)
WOY x District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weather Controls	Yes	Yes	Yes	Yes	Yes s
Covariates	Yes	Yes	Yes	Yes	Yes
Observations	316,970	316,970	316,970	316,970	316,970
Ymean	3151.6	8.2	1.2	7.2	0.4
Ysd	214.2	11.1	4.4	10.7	2.7

Notes: WOY=week of he year. Observations are at location-date level and are weighted by the size of the local birth-cohort in that location-day. Estimates are 13-week sums of coefficients on weekly counts of thermal inversions. Coefficients indicate the effect of one SD increase in the weekly number of inversions over the corresponding window of time. All regressions control for an indicator of having missing information data over the preceding week and the number of dates with missing data in that week. Weather controls include cubic polynomials in mean temperature, maximum temperature, minimum temperature, humidity, direction of wind (fixed octants), and rainfall. I also control for mother's age, education, marital status, and parity. Two-way clustering is employed; standard errors are clustered at location and date level. *** significant at 1% level.

Table 2.9. Covariates and Inversions

	Education: 12+ years	Married/ Committed relationship	Age: <25 years old	Age: 25-35 years old	Child is first born	Child is White	Child is Black
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Count inversions weeks t to t-12	-0.985	-1.044	0.490	-0.135	-0.253	-2.743**	2.321**
	(0.872)	(0.759)	(0.423)	(0.346)	(0.603)	(1.317)	(1.070)
Count inversions weeks t-13 and t-25	0.488	-2.101**	-0.192	0.229	-0.535	2.560	1.891*
	(0.886)	(0.959)	(0.459)	(0.359)	(1.059)	(1.649)	(1.134)
Count inversions weeks t-26 and t-38	1.806	1.386	-0.144	0.168	-0.400	-0.533	2.321**
	(1.127)	(0.988)	(0.562)	(0.418)	(0.713)	(1.465)	(1.117)
WOY x District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	316,970	316,970	316,970	316,970	316,970	316,970	316,970
Ymean	22.4	46.9	39.7	49.6	71.5	54.7	24.1
Ysd	18.1	16.1	12.7	11.1	12.6	25.3	20.3

Notes: WOY=week of he year. Observations are at location-date level and are weighted by the size of the local birth-cohort in that location-day. Estimates are 13-week sums of coefficients on weekly counts of thermal inversions. Coefficients indicate the effect of one SD increase in the weekly number of inversions over the corresponding window of time. All regressions control for an indicator of having missing information data over the preceding week and the number of dates with missing data in that week. Weather controls include cubic polynomials in mean temperature, maximum temperature, minimum temperature, humidity, direction of wind (fixed octants), and rainfall. Two-way clustering is employed; standard errors are clustered at location and date level. ** significant at 5% level, * significant at 10% level.

Table 2.10. Effect of Thermal Inversions on Infant Health: Heterogeneity Analysis

	Child		Mother's age			Mother's education	
	Male	Female	<25	25-35	>35	<12 years	≥12 years
Count of inversions	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Birth Weight (grams)							
weeks t to t-12	-60.301***	-31.924**	-47.895***	-18.615	1.644	-38.345***	-50.346*
	(14.155)	(14.929)	(15.953)	(13.772)	(54.428)	(10.652)	(26.818)
weeks t-13 and t-25	-13.623	13.625	-9.549	16.851	25.597	13.641	-32.595
	(14.231)	(15.737)	(15.042)	(14.241)	(54.861)	(10.748)	(28.679)
weeks t-26 and t-38	-15.599	-6.345	-19.626	13.471	-14.127	-1.803	23.898
	(12.951)	(14.167)	(15.947)	(12.463)	(54.915)	(8.198)	(22.704)
Ymean	3204.51	3096.5	3113.54	3185.9	3143.8	3152.6	3161.6
Panel B. Low birth Weight (x100)							
weeks t to t-12	1.756**	1.717**	1.308	0.680	0.320	1.237***	3.215**
	(0.703)	(0.785)	(0.832)	(0.650)	(3.023)	(0.457)	(1.373)
weeks t-13 and t-25	-0.200	-0.557	0.485	-1.337**	-2.209	-0.592	2.869*
	(0.616)	(0.755)	(0.838)	(0.659)	(2.960)	(0.560)	(1.515)
weeks t-26 and t-38	-0.082	-0.128	0.724	-0.386	-1.083	-0.153	-1.892
	(0.671)	(0.758)	(0.903)	(0.646)	(3.280)	(0.457)	(1.234)
Ymean	7.5	8.9	8.9	7.2	9.9	8.3	6.9
Panel C. Very low birth Weight (x100)							
weeks t to t-12	0.646**	1.008***	1.026***	-0.114	1.034	0.488**	0.686
	(0.270)	(0.293)	(0.373)	(0.294)	(1.160)	(0.205)	(0.534)
weeks t-13 and t-25	0.139	0.429	0.494	0.049	0.053	0.074	0.362
	(0.259)	(0.305)	(0.331)	(0.250)	(1.329)	(0.187)	(0.466)
weeks t-26 and t-38	0.318	0.299	0.457	-0.082	0.033	0.053	0.003
	(0.262)	(0.285)	(0.370)	(0.240)	(1.203)	(0.194)	(0.479)
Ymean	1.2	1.2	1.3	1.1	1.7	1.2	1
Panel D. Preterm birth (x100)							
weeks t to t-12	2.626***	1.720**	1.928**	1.034*	0.547	1.084**	2.841*
	(0.638)	(0.694)	(0.846)	(0.619)	(2.921)	(0.509)	(1.510)
weeks t-13 and t-25	0.463	0.503	0.789	0.032	-0.542	0.083	3.560**
	(0.687)	(0.612)	(0.816)	(0.664)	(2.877)	(0.501)	(1.472)
weeks t-26 and t-38	0.658	0.992	1.473*	-0.508	1.614	0.417	-0.688
	(0.672)	(0.650)	(0.763)	(0.653)	(2.840)	(0.474)	(1.330)
Ymean	7.4	7.0	7.5	6.6	9.2	7.1	7.5
Panel E. Very premature (x100)							
weeks t to t-12	0.521***	0.383**	0.477**	0.122	0.070	0.386***	0.265
	(0.150)	(0.178)	(0.218)	(0.151)	(0.579)	(0.101)	(0.318)
weeks t-13 and t-25	0.466***	-0.008	0.228	0.161	-0.009	0.050	0.143
	(0.152)	(0.186)	(0.196)	(0.171)	(0.670)	(0.106)	(0.277)
weeks t-26 and t-38	0.196	0.148	0.232	-0.086	-0.607	0.094	0.047
	(0.137)	(0.164)	(0.208)	(0.135)	(0.667)	(0.101)	(0.277)
Ymean	0.5	0.4	0.5	0.4	0.5	0.4	0.3
Observations	162,606	154,364	119,760	162,124	34,805	230,036	79,234
WOY x District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: WOY=week of he year. Observations are at location-date level and are weighted by the size of the local birth-cohort in that location-day. Estimates are 13-week sums of coefficients on weekly counts of thermal inversions. Coefficients indicate the effect of one SD increase in the weekly number of inversions over the corresponding window of time. All regressions control for an indicator of having missing information data over the preceding week and the number of dates with missing data in that week. Weather controls include cubic polynomials in mean temperature, maximum temperature, minimum temperature, humidity, direction of wind (fixed octants), and rainfall. Two-way clustering is employed; standard errors are clustered at location and date level. *** significant at 1% level, **significant at 5% level, * significant at 10% level.

Chapter 3

Heightened immigration enforcement impacts U.S. citizens' birth outcomes

Joint with Marcos A. Rangel, Christina Gibson-Davis, and Laura Bellows.[1]

3.1 Introduction

Over the last decade, immigration enforcement impetus has increased dramatically in the U.S. interior, with increasing numbers of individuals experiencing detention and removal. (Gonzalez-Barreca and Krogstad, 2016; Capps et al., 2018). Beyond physical removals, this increased activity by Immigration and Customs Enforcement (ICE) has greatly increased the psychological, social, and economic stresses faced by immigrant families, and had deleterious effects on physical health (Chaudry, 2011; Brabeck et al., 2014). These in turn are likely reinforced by increased location-specific anti-immigrant sentiment in the overall population in general and amongst law enforcement officers in particular (Arbona et al., 2010; Capps et al., 2011; Nguyen and Gill, 2010, 2015; Rhodes et al., 2015; Chaudry, 2011). Pregnant women may be particularly vulnerable to these stresses, insofar as mothers-to-be, and their fetuses, are vulnerable to the hardening of the environment in which they live. Given that conditions during pregnancy partly determine adult health and well-being (Currie, 2011; Shonkoff et al., 2009), ICE activity experienced during gestation may have long-term consequences for the health and well-being of generations of U.S. citizens.

[1]Author contributions: R.T. and M.R. designed research, performed research, analyzed data, and wrote the paper. C. G-D. and L.B. designed research and wrote the paper.

We contribute to the literature by carefully analyzing the causal impact of increases in immigration enforcement by local agencies on the health of newborns of (likely) undocumented immigrant mothers in Mecklenburg County, North Carolina (home to the most populous city in the state, Charlotte). Using additional outcome variables we also investigate one potential mechanism behind our results: behavioral changes regarding health-care utilization. We are the first in this literature to emphasize that immigration enforcement and birth outcomes need to be studied by considering the family-level rather than individual-level assignment of undocumented status.

We focus on North Carolina for a number of reasons. First, ICE activities in Mecklenberg closely mirror the current policy approach of forceful anti-immigrant activities, and serves as a case study of the consequences of an aggressive approach to immigrant detention and removal. Second, the state has become a "new destination" for immigrants, with a 13-fold increase in the number of unauthorized aliens between 1990 and 2014. With an estimated population of 350,000 in 2014, the state is thought to have the eighth largest unauthorized population in the country (Passel and D'vera, 2016). Third, because immigrant communities are newer, support networks in North Carolina are most likely distinct in terms of size and effectiveness from those traditional migration destinations like Arizona, California, and Texas. We believe this has important ramifications in terms of lower social capital more generally, but more specifically in the form of racial profiling and directed police action.

Embedded in this context, Mecklenburg was the first county in North Carolina (and first east of Arizona) to sign a 287(g) agreement with ICE on February 27, 2006. From there, the program was expanded to other communities in the South (the then-sheriff of Mecklenburg went on to lead ICE's Office of State and Local Coordination). These programs were first authorized as part of the 1996 Illegal Immigration Reform

and Immigrant Responsibility Act, but no specific action was implemented until 2002 (Capps et al., 2011). In 287(g) programs, local law officers are deputized to act as ICE agents. Through these partnerships, local law offices have the authority to approach suspected unauthorized individuals, and if necessary, begin deportation proceedings. Local agencies can follow a task force, jail, or hybrid model in which individuals are detained at the point of arrest, after being booked at jail or in both places, respectively. However, particularly in early years of 287(g) programs, little practical distinction likely existed between models: police reports suggest that law enforcement in some counties with jail models question individuals about immigration status prior to arrest (Nguyen and Gill, 2010, 2015). Importantly, local agencies and officers generally maintain full discretion over enforcement priorities.

Mecklenburg followed a jail enforcement model that did not specifically target serious criminal offenders but instead identified as many unauthorized immigrants as possible (Capps et al., 2011). Data from Syracuse University's Transactional Records Access Clearinghouse (TRAC) project indicates that 39% of the detainers issued in the state between 2003 and 2009 were originated in Mecklenburg. More than 84% of the detainers in that county were for minor violations of the law (e.g., traffic related), and 96% of those detained were Hispanic. Mecklenburg thus serves as a case study of a universal approach to immigration enforcement, as opposed to the targeting promoted later in the Obama administration, which focused on more serious criminal offenders (Capps et al., 2018). 287(g) programs are favored by the Trump administration, and in response to an executive order signed by the President upon taking office, have doubled in number since 2018. Currently 82 jurisdictions, located mostly in southeast states or states along the southern US border, have active 287(g) programs. The Mecklenburg experience we investigate better compares with the current state of affairs in terms of political polarization and urban-rural divide, aggressive policy and

anti-immigration sentiment in most locations across the United States.

The impact of 287(g) programs on maternal and infant health is unknown. Extant knowledge on ICE activity and newborn health has been based on studies that examined an immigration raid (Novak et al., 2017) or state-specific legislation that was never enacted (Torche and Sirois, 2019). These studies suggest that ICE activity can have deleterious effects on birth weight, and lead to an increase in the incidence of low birth weight births. However, these studies were limited insofar as they were not concerned with specific ICE policies that favor universal enforcement and aggressive police action. As argued above, we see the context we study as a more appropriate description of the state of affairs in terms of the federal government's rhetoric motivating ICE activities across the United States at the time of this writing. Moreover, these studies concentrated on birth outcomes (specifically birth weight) but did not consider how immigration enforcement alters pregnant women's behaviors, such as changes in prenatal care use.

3.2 Potential Effects of 287(g) Programs on Families

Our focus on birth outcomes is warranted given an extensive literature linking fetal development and maternal stress (de Weerth and Buitelaar, 2005). Specifically, pregnant women respond to external stressors by a quick increase followed by decrease of glucocorticoids (cortisol). Then, abnormal stress reactivity translates itself into a larger increase and/or slower decrease in cortisol levels. These abnormal responses have programming effects on the behavioral and physiological development of the children affected while *in utero* (Mulder et al., 2002).

In addition to aforementioned physiological effects, mothers may also react to stress by negatively altering their pregnancy behaviors (Dole et al., 2003; Hoffman and

Hatch, 1996). In the case of immigration enforcement, they may be less likely to seek out prenatal care (or receive it adequately) because of fear of ICE activities (Rhodes et al., 2015; Hagan et al., 2003; Arriaga, 2017). Mothers-to-be may also respond to stress by adopting maladaptive coping mechanisms, such as increasing cigarette consumption (Yang et al., 2013; Lobel et al., 2008). These pregnancy behaviors are important outcomes in their own right but also have well established and serious implications for the health and well-being of the fetus Hoffman and Hatch (1996); Woods et al. (2010).

Moreover, immigration enforcement may impact maternal support networks (including their own marriage) or neighborhood social capital, either because of increases in removals or because increased ICE activities change community dynamics. Among Hispanics, ICE policies and programs intensify fear and distrust of police, lead to under-reporting of crime in those locales, compromise safety and security, and lead to a greater exploitation of employees (Capps et al., 2011; Nguyen and Gill, 2010, 2015). Communities that partner with federal authorities on immigration enforcement may also be locales that have anti-immigrant policies, experience an uptick in hate crimes, or otherwise signal that the community is hostile to immigrants.

Finally, increases in immigration enforcement may impact labor market participation and household economic conditions. Families experiencing a detention or removal typically lose family income (Dreby, 2012; Koball et al., 2015), but immigration enforcement generally may reduce employment rates, particularly amongst low-educated (Hispanic) men (East et al., 2018). In contrast, obtaining documentation (and therefore reducing or eliminating the risk of removal) leads to increases in employment and income (Pope, 2016). Under conditions of increased immigration enforcement, for example, Hispanic families are less likely to receive public benefits (Watson, 2014; Vargas and Pirog, 2016; Potochnick et al., 2017). For both these

reasons, increased enforcement activity may restrict pregnant women's access to economic resources during pregnancy, possibly affecting access to nutrition and health care.

A growing literature on the association between immigration enforcement activity and maternal and fetal health has suggested negative repercussions of immigration enforcement. In Iowa, Hispanic mothers who were pregnant during an immigration raid were also more likely to give birth to a child with low birth weight (Novak et al., 2017). Focus-group interviews with 83 mothers in North Carolina indicated that the introduction of the same 287(g) programs resulted in a decrease in the likelihood that Hispanic mothers received adequate prenatal care due to reduced trust of medical/nursing staff (Rhodes et al., 2015). Using quasi-experimental methods similar to ours, Florencia Torche and Catherine Sirois estimate that the signature of the Arizona Senate Bill 1070 into law reduced birth weight by 15 grams among Latina women residing in that traditional immigrant destination. These effects were observed even though the law was never put into place (since it was blocked by legal challenges) (Torche and Sirois, 2019). Unlike that, the context for our study provides a more appropriate description of the current state of affairs in terms of the federal government's rhetoric motivating ICE activities across the United States.

3.3 Data

We use long-form birth certificate data available from the North Carolina Detailed Birth Records (NCDBR) database. The NCDBR is the universe of North Carolina births and includes information on parental demographics, maternal health, use of prenatal care, infant health, and geographic identifiers (including parents' county of residence and country of birth).

Our sample of analysis was constructed in two steps. In the first step, we limited the sample to births in counties that met two restrictions: 1) at least 5% of births between 2004 and 2005 were to foreign-born mothers; and 2) at least one birth in every month observed was to a foreign-born mother. Of North Carolina's 100 counties, 47 counties met these restrictions (listed on SI Appendix, Table C.1). Counties reflect the mother's county of residence and not where the birth occurred. Second, among those 47 counties, we limited the sample to births that occurred in the nine months immediately after 287(g) was implemented (March – November of 2006), and to births that occurred during the same time frame one year earlier (March – November of 2005). As the bulk of sample focuses on fetuses conceived prior to the implementation of the 287(g) program, our findings are shielded from potential behavioral responses in the form of fertility decisions.

The NCDBR does not contain information on documentation status (we are not aware of any large administrative data on births that does). We define exposure using a combination of nativity of both parents listed on the certificate. Departing from previous contributions (Rhodes et al., 2015; Potochnick et al., 2017; Torche and Sirois, 2019), we make careful use of parents' education and birth location to highlight what we consider to be a greater exposure to immigration enforcement activities. Immigration enforcement is targeted both in terms of education and in terms of gender: Less educated immigrants, and men in particular, are the groups most likely to experience (fear of) removal (Golash-Boza and Hondagneu-Sotelo, 2013). By expanding our definition to include origin and education information for both the mother and the father, we place a primacy on the exposure of families (and not just women) to ICE's activity. This family-level exposure more accurately reflects the experience of immigrant families and the babies they have conceived.[2]

[2]Only 6% of lower educated foreign born mothers do not list fathers' information in the birth certificate.

Our sample of the targeted population is composed of singleton live births to foreign-born parents distributed across two groups we deem exposed to the policy in different degrees: those in which parents have less than high-school education (n=12,588); and those in which parents have high-school or more (n=8,397). We complement our analyses with the empirical examination of a comparison group based on 97,489 live births to non-Hispanic U.S.-born parents during the same period and within the same locations studied.

We have two sets of outcomes: child health at birth and access to care. The first group of outcomes includes birth weight, and indicators for prematurity (birth before 37 weeks of gestation) and small-for-gestational age. Birth weight is measured both continuously in grams and using an indicator for low birth weight (<2,500 grams). Small for gestational age is defined as membership in the smallest decile of birth weight, by week of gestation, calculated using nationally-defined curves for fetal growth (Oken et al., 2003).

We also look at changes in maternal behaviors related to adequacy of prenatal care utilization. We measure it through the Kotelchuck index (Kotelchuck, 1994), a dichotomous indicator of whether a woman initiated care prior to the fourth month of pregnancy and received at least 50% of recommended visits.[3]

We controlled in our analysis for whether the child is female and whether the child is first born. The following maternal characteristics were also included: indicators for mother's age, maternal race/ethnicity, and whether she was formally married at the time her child's birth was registered. As noted above, we use maternal education to stratify our working samples.

[3]While examining smoking and alcohol consumption measures would be interesting to understand substance use during pregnancy, these outcomes are extremely rarely in our population of interest.

3.4 Empirical Strategy

Our causal inference model compares outcomes for likely unauthorized parents—lower educated foreign-born parents—in Mecklenburg County before and after the 287(g) program implementation with those of parents from the same demographic group but who reside in non-adopting counties around the same time. In this case-control quasi-experimental analysis, we employ a difference-in-differences regression approach. Formally, we consider alternative versions of the following difference-in-difference model:

$$Y_{ict} = \beta_0 + \beta_1 Meck_{ic} + \beta_2 After_{it} + \beta_3 Meck_{ic} After_{it} + u_{ict} \tag{3.1}$$

where Y_{ict} indicates an outcome for individual i born in county c at time t. $Meck_{ic}$ is an indicator that takes value 1 if child i is born in Mecklenburg County and 0 if she is born in another county. $After_{it}$ takes value 1 if child i is born after Mecklenburg adopted the 287(g) program—-that is, after February 27, 2006—and 0 if she was born before the implementation. Our coefficient of interest is β_3. If, for example, Y is birth weight, β_3 indicates that, relative to those children born in other counties, children born in Mecklenburg weight β_3 more grams after the introduction of 287(g) programs.

The identifying assumption underlying this method is that, in the absence of the 287(g) program, changes in birth outcomes among those most likely affected by increased enforcement within Mecklenburg would be equivalent to changes in birth outcomes amongst similar parents in the rest of the state.

We show that our estimates are robust to the inclusion of county-specific characteristics that do not change over time (captured in county fixed effects) and of year-month fixed effects (which semi-parametrically control for state-wide policies and seasonality). We also provide evidence that these estimates are robust to the in-

clusion of covariates capturing child (gender, birth-order) and maternal characteristics (age, marital status, and race). We control for the race and ethnicity of mothers, as racial/ethnic heterogeneity in birth outcomes is well known. We note, however, that racial/ethnic differences in the level of outcomes cannot bias our estimates, as our research design relies solely on similarities of temporal variation in outcomes across each racial/ethnic subgroup.

P-values are computed using standard error estimates clustered at the county level. Given our large sample sizes, when assessing the statistical significance of our findings we employ more stringent critical values for hypothesis tests using the Bayesian Information/Schwartz criterion (Schwarz, 1978; Leamer, 1978; Deaton, 1997). According to the latter, a coefficient estimate is consider significant when its t-statistic surpasses the square-root of the log-transformed number of observations in the regression model.

In order to address potential coincidental events which may have affected Mecklenburg but not the other counties, we estimate the model for two demographic groups that are unlikely to be affected: births to more educated foreign-born parents and births to non-Hispanic U.S.-born parents. We combine these two exercises into a triple-differences estimation, which allows us to assess the statistical relevance of the differences between these distinct groups. Specifically, we estimate the following model:

$$\begin{aligned} Y_{ict} =& \delta_0 + \delta_1 Meck_{ic} + \delta_2 After_{it} + \delta_3 NEDI_{ic} + \delta_4 Meck_{ic} After_{it} \\ &+ \delta_5 Meck_{ic} NEDI_{ic} + \delta_6 After_{it} NEDI_{ic} + \delta_7 Meck_{ic} After_{it} NEDI_{ic} + \theta_{ict} \end{aligned} \tag{3.2}$$

where $NEDI_{ic}$ takes value 1 if child i's parents are both less educated and foreign-born. Our coefficient of interest is δ_7, which indicates the differential effect of immigration enforcement on these groups of mothers. This is equivalent to take the

difference between the difference-in-difference estimator for two groups of children.

3.5 Results

Descriptive averages for the covariates in Mecklenburg and elsewhere in the state are presented in Table 3.1 for both the period of 9 months immediately after the implementation of the policy (March-November) and the corresponding one in the previous year. Statistics are presented for the groups defined by parental foreign-born status and education (columns 1 to 6). The final three columns of the table present descriptive statistics for the contrast population of U.S.-born parents. Most differences in average characteristics for groups inside and outside Mecklenburg seem to be stable over the two periods under study, with the possible exception of marital status. It is also clear from these statistics that Hispanics are overrepresented within the group of less educated parents among foreign-born parents—95% in Mecklenburg and 97% elsewhere.

In Table 3.2 we turn to descriptive statistics for our outcomes of interest according to parental origin and education. Prior to the implementation of 287(g) program in Mecklenburg county, foreign-born, relative to US-born, parents give birth to children with slightly better outcomes. It is also the case that children born in Mecklenburg do not seem particularly different from those born elsewhere before the policy change. Nonetheless, averages after the increase in enforcement seem to be markedly worse in the adopting county. Table 3.2 also shows that utilization of prenatal care seems worse in Mecklenburg than elsewhere, and these differences mostly emerge after the policy implementation and among foreign-born mothers. These descriptive statistics indicate a deleterious effect of ICE activity on birth outcomes among the population targeted by it.

Table 3.1. Descriptive Statistics on Maternal Demographics for Births to Foreign and U.S.-born Parents Residing in North Carolina (Mar to Nov, 2005 and 2006)

	Births to foreign born parents						Births to US born parents		
	Less educated parents (N=12,588)			More educated parents (N=8,397)			(N= 97,489)		
	Mecklenburg	Other NC	Difference	Mecklenburg	Other NC	Difference	Mecklenburg	Other NC	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: March to November, year 2006 - Post 287(g)									
Maternal age	25.93	26.16	-0.23	29.33	29.47	-0.14	29.36	27.45	1.90
			[0.24]			[0.42]			[0.00]
Mom Hispanic	94.80	96.74	-1.94	42.24	37.70	4.54	-	-	-
			[0.01]			[0.01]			-
Mom White non-Hispanic	1.08	1.09	-0.01	12.78	15.85	-3.07	65.00	75.79	-10.79
			[0.98]			[0.01]			[0.00]
Mom Black non-Hispanic	1.30	0.36	0.94	12.60	12.42	0.18	33.93	22.13	11.80
			[0.01]			[0.88]			[0.00]
Mom less educated	-	-	-	-	-	-	6.87	11.27	-4.40
			-			-			[0.00]
Married at birth	35.54	49.12	-13.58	79.50	81.85	-2.35	72.43	71.05	1.38
			[0.00]			[0.09]			[0.03]
First live birth	31.64	25.32	6.31	39.84	39.72	0.12	44.59	43.41	1.19
			[0.00]			[0.94]			[0.09]
Female child born	49.40	48.75	0.65	50.22	48.65	1.57	48.76	48.61	0.15
			[0.71]			[0.36]			[0.82]
Panel B: March to November, year 2005 - Pre 287(g)									
Maternal age	25.57	25.98	-0.41	29.26	29.22	0.04	29.48	27.60	1.88
			[0.04]			[0.84]			[0.00]
Mom Hispanic	95.28	96.81	-1.53	38.53	38.10	0.43	-	-	-
			[0.05]			[0.81]			-
Mom White non-Hispanic	1.30	1.20	0.10	16.84	17.01	-0.17	66.96	76.25	-9.28
			[0.82]			[0.90]			[0.00]
Mom Black non-Hispanic	0.47	0.30	0.17	14.67	14.07	0.60	32.42	21.54	10.88
			[0.49]			[0.65]			[0.00]
Mom less educated	-	-	-	-	-	-	6.49	11.28	-4.79
					-			-	[0.00]
Married at birth	42.03	49.22	-7.19	79.96	81.97	-2.01	74.73	72.69	2.04
			[0.00]			[0.17]			[0.00]
First live birth	30.81	26.33	4.49	38.95	40.38	-1.43	44.36	42.64	1.72
			[0.01]			[0.43]			[0.01]
Female child born	50.53	48.81	1.72	49.79	48.12	1.67	49.04	48.58	0.46
			[0.35]			[0.37]			[0.52]

Notes: p-values for the differences within brackets computed based on standard-errors clustered at the county level. Less educated foreign parents have less than high-school (or non-reported) education. Other North Carolina (NC) counties correspond to 46 distinct units.

Table 3.2. Descriptive Statistics on Outcomes for Births to Foreign and U.S.-born Parents Residing in North Carolina (Mar to Nov, 2005 and 2006)

	Births to foreign born parents						Births to US born parents		
	Less educated parents (N=12,588)			More educated parents (N=8,397)			(N= 97,489)		
	Mecklenburg	Other NC	Difference	Mecklenburg	Other NC	Difference	Mecklenburg	Other NC	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: March to November, year 2006 - Post 287(g)									
Birth weight (grams)	3,298.07	3,361.04	-62.97	3,312.97	3,323.18	-10.21	3,323.06	3,317.82	5.24
			[0.00]			[0.59]			[0.52]
Low birth weight (%)	5.09	4.49	0.60	5.50	5.05	0.45	6.51	6.90	-0.39
			[0.44]			[0.56]			[0.26]
Small for gestational age (%)	10.62	7.90	2.72	11.71	10.07	1.65	8.51	8.80	-0.29
			[0.01]			[0.13]			[0.46]
Inadequate prenatal care (%)	36.80	23.78	13.02	15.19	9.84	5.36	5.85	6.06	-0.21
			[0.00]			[0.00]			[0.52]
Panel B: March to November, 2005 - Pre 287(g)									
Birth weight (grams)	3,339.55	3,343.18	-3.63	3,307.14	3,324.93	-17.79	3,330.86	3,325.95	4.92
			[0.85]			[0.39]			[0.56]
Low birth weight (%)	4.96	4.92	0.03	5.27	4.91	0.36	6.37	6.91	-0.54
			[0.97]			[0.66]			[0.12]
Small for gestational age (%)	8.50	8.25	0.25	10.85	9.79	1.06	8.31	8.52	-0.21
			[0.81]			[0.35]			[0.59]
Inadequate prenatal care (%)	27.41	24.12	3.29	10.77	10.15	0.62	4.59	5.68	-1.09
			[0.05]			[0.59]			[0.00]

Notes: p-values for the differences within brackets computed based on standard-errors clustered at the county level. Less educated foreign parents have less than high-school (or non-reported) education. Other North Carolina (NC) counties correspond to 46 distinct units.

We formalize these analyses estimating difference-in-differences parameters and their statistical significance. Results are presented in Table 3.3. We find that the reduction in birth weight likely induced by 287(g) ranges from a raw estimate of 59.3 grams (p-value<0.001) among less educated foreign-born parents to 58.0 grams (p-value<0.001) after the full set of control variables are included (columns 1 to 3). Significantly worsened outcomes can also be seen in terms of small for gestational age births (2.3 p.p., p-value<0.001). Focusing on our most stringent specification (column 3), we also detect significant differences in the timely initiation and frequency of prenatal care, which together contribute to an increase of 9.9 percentage points (p-value<0.001) in the incidence of births without adequate prenatal care in Mecklenburg after the ICE policy is activated.

In an effort to examine the robustness of our design we also report (column 4) results from a falsification exercise. We estimate the same empirical model but include births in years 2004 and 2005 and assume that 287(g) was implemented one year before it actually was. According to this exercise, significant results would suggest the existence of differential trends between outcomes in and out of Mecklenburg even before the policy was activated. Reassuringly, we see no effects in this exercise.

In column 5 we turn to the births to a group of more educated parents which are also foreign-born (and who have a foreign-born partner). Interestingly, our estimations indicate that this group also had significant increase in the utilization of inadequate medical care during the prenatal phase (although effects are smaller in magnitude than among the less educated). Yet, birth outcomes seem to be relatively unaffected, which calls attention to the potential protective effects of additional economic resources likely available to the more educated or the fact that these parents are more likely to be documented.

Most importantly, Table 3.3 also reveals that for the births to U.S.-born parents

Table 3.3. Difference-in-Difference Estimates by Parental Foreign Origin and Education

	Births to foreign-born parents					U.S-born parents
	Less educated parents				More educated parents	
	(1)	(2)	(3)	(4)	(5)	(6)
Birth weight (grams)	-59.341***	-57.994***	-58.016***	-13.648	10.876	1.824
	(11.884)	(11.839)	(12.137)	(9.041)	(12.870)	(4.357)
Low birth weight (%)	0.564	0.549	0.423	-0.044	-0.022	0.130
	(0.381)	(0.384)	(0.399)	(0.352)	(0.411)	(0.170)
Small for gestational age (%)	2.472***	2.356***	2.249***	0.143	0.539	-0.133
	(0.659)	(0.654)	(0.674)	(0.626)	(0.599)	(0.189)
Inadequate prenatal care (%)	9.731***	9.833***	9.898***	0.849	4.582***	0.826***
	(1.160)	(1.140)	(1.088)	(1.277)	(1.204)	(0.175)
Month-Year FE		Yes	Yes	Yes	Yes	Yes
County FE		Yes	Yes	Yes	Yes	Yes
Controls			Yes	Yes	Yes	Yes
Falsification/lagged policy enactment				Yes		
Observations		12,588			8,397	97,489
Schwartz		3.1			3.0	3.4

Notes: FE=fixed effects. Each coefficient indicates a separate regression. Standard-errors clustered at the county level in parenthesis. Less educated foreign parents have less than high-school (or non-reported) education. Demographic characteristics are listed in Table 3.1.

there is no detectable negative effects in terms of birth outcomes (column 6), and only a relative smaller effect on prenatal care is found. We conclude that there is no indication that residents of Mecklenburg were all exposed to a worsening set of conditions which influenced birth outcomes. Our estimates indicate that such worsening is restricted to the subpopulation of babies born to foreign-born parents (and in particular those with less educated parents).

We further formalize these contrasts by providing estimates based on triple-difference exercises in Table 3.4. In this formulation we statistically compare difference-in-differences results for alternative subgroups of the population. Our results are stark

and clearly indicate that significant differences emerge between the outcomes of children born to less educated foreign-born parents in Mecklenburg after the policy change. Their experience is in no way mimicked by that of babies born to U.S.-born parents in the same location and around the same time (column 1). Differences also emerge between the less and more educated foreign-born parents (column 2)—which is in line both with the on-the-ground targeting of the policy and the protective role of family resources.

In Table 3.5, we use different subsamples of counties when conducting the analysis. Births in the control group come from counties that adopt 287(g) after 2006 (column 1), counties that apply for 287(g) but were denied the program (column 2), counties that either adopt 287(g) in the future or apply for it (column 3), or counties that never adopt nor try to adopt 287(g) programs (column 4). Our findings are robust to the use of these different control groups. While our results are statistically sound and result from the implementation of state-of-the art quasi-experimental techniques, we (as all other studies in this literature) are still subject to a particular limitation: families can move across space once increased immigration enforcement is implemented (Watson, 2014; Ellis et al., 2016). A priori, however, there is no reason to believe our results would be biased in a particular direction. If individuals moving away from Mecklenburg in response to 287(g) are the most marginalized of the undocumented we would expect to have captured an effect that is smaller (in absolute value) than what would have occurred without their migration. On the other hand, if migration requires economic resources one could imagine an opposite compositional change in our "treatment" populations, which would imply an overestimation of effects. However, we believe that the relative recency of international migration to North Carolina and the consequent limited role for immigrant support networks, helps alleviate concerns particularly in relation to other studies based on data from more traditional

Table 3.4. Triple-Differences Estimates by Parental Foreign Origin and Education

	Less educated foreign vs. US-born parents (1)	Less vs. more educated foreign parents (2)
Birth weight (grams)	-62.568***	-70.101***
	(12.493)	(17.915)
Low birth weight (%)	0.353	0.448
	(0.405)	(0.583)
Small for gestational age (%)	2.368***	1.897**
	(0.685)	(0.800)
Inadequate prenatal care (%)	8.954***	5.458***
	(1.056)	(1.373)
Month-Year FE	Yes	Yes
County-Month FE	Yes	Yes
Controls	Yes	Yes
Observations	110,077	20,985
Schwartz	3.407	3.154

Notes: FE=fixed effects. Each coefficient indicates a separate regression. Standard-errors clustered at the county level in parenthesis. Less educated foreign parents have less than high-school (or non-reported) education. Demographic characteristics are listed in Table 3.1.

immigration-receiving regions across the United States.

3.6 Conclusions

Our study adds to a growing body of research documenting the negative spillover effects of immigration enforcement cited in the introduction; all children affected are U.S.-born and therefore U.S. citizens. Negative impacts on children's early health outcomes have long-term consequences for their later health and economic success (Currie, 2011).

Importantly, we see effects intensify as we narrow our treatment group to par-

Table 3.5. Triple-Differences Estimates by Parental Foreign Origin and Education - Results by Subsamples of Control Counties

	Triple-differences, less educated foreign-born parents versus U.S.-born parents			
	Control counties are future 287(g) adopters	Control counties apply for 287(g) but are denied	Control counties are future adopters or applicants	Control counties are never adopters and non-applicants
	(1)	(2)	(3)	(4)
Birth weight (grams)	-77.215**	-85.404***	-78.820***	-43.236***
	(24.109)	(17.007)	(17.399)	(13.911)
Low birth weight (%)	1.318*	0.507	1.041**	-0.483
	(0.607)	(0.872)	(0.464)	(0.622)
Small for gestational age (%)	2.841*	2.136	2.564**	2.176***
	(1.398)	(1.564)	(1.047)	(0.766)
Inadequate prenatal care (%)	9.406***	7.351**	8.806***	8.915***
	(1.758)	(2.584)	(1.471)	(1.692)
Month-Year FE	Yes	Yes	Yes	Yes
County-Month FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	49,252	28,317	64,344	58,929
Schwartz	3.287	3.202	3.327	3.314

Notes: FE=fixed effects. Each coefficient indicates a separate regression. Standard-errors clustered at the county level in parenthesis. Less educated foreign parents have less than high-school (or non-reported) education. Demographic characteristics are listed in Table 3.1.

ents and children most likely directly impacted by immigration enforcement policies. Effects were larger when both parents were born abroad and have less than a high school education, due to two likely reasons. First, immigrant men partnering with these women are more likely to be targeted by immigration enforcement than those married to more educated immigrant women; mothers may be more aware of this targeting and experience greater levels of stress when fathers are potential targets. Second, even if equally impacted by stress and anti-immigrant sentiment in the location where they reside, more educated women most likely have access to protective economic resources.

Throughout our main analysis we find strong effects of immigration enforcement on the likelihood that mothers obtain adequate prenatal care. Under conditions of

increased immigration enforcement, mothers may be afraid to engage with medical institutions or experience a decrease in resources, leaving them less likely to obtain prenatal care. For example, there are reports in North Carolina of sheriff's deputies waiting outside migrant health clinics (Arriaga, 2017). Although federal policy designates hospitals as sensitive locations, at which immigration enforcement actions are only supposed to be carried out under exigent circumstances, the American Medical Association has called for this designation to be expanded to include spaces within 1,000 feet of any medical treatment or medical facility and to apply under all conditions. Our results substantiate the need for a closer consideration of these remedial options.

Conclusion

The three essays that make up this dissertation explore the impact of different risk factors on health at birth. Exploiting various exogenous sources of variation and leveraging several datasets, I document how policies that affect the social environment of pregnant women, as well as changes in the physical environment, affect their well-being and newborns' health.

Chapter 1 explores the unanticipated impact of restricting hours of operations of bars and restaurants by exploiting the staggered implementation of laws that restrict hours of operations of bars and restaurants in the SPMA. Using confidential vital registration data allows me to account for families' behavioral responses—fertility and migration—and to compare infants prenatally exposed to the laws with their earlier-born siblings who were not exposed to the dry laws. I find that curtailing access to drinking reduces fetal death among unhealthier male fetuses and that alcohol consumption, exposure to violence, and family composition cannot be ruled out as potential mechanisms explaining the link between the implementation of dry laws and health at birth.

Chapter 2 also looks at a population in Brazil but it focuses on the impact of the physical environment. I take advantage of the meteorological phenomenon of thermal inversion –which arguably exogenously locks pollutants closer to the ground– to highlight the effect of air pollution rather than the impact of economic conditions on health at birth. I find that exposure to inversion episodes during the last three months of gestation decreases birth weight, increases the chances of prematurity, and affects fetal survival. Estimates are larger than the other few studies in developing countries and those exploring developed countries.

Chapter 3 documents the consequences of local partnerships with ICE on maternal and newborns' well-being. Exploiting the introduction of the 287(g) program in North Carolina, U.S., this chapter estimates that immigration enforcement decreases birth weight and increases the likelihood of being small for gestational age births. Moreover, there are significant differences in the timely initiation and frequency of prenatal care, with an increase in the incidences of births without adequate prenatal care in Mecklenburg after the ICE policy is activated. Importantly, effects intensify for parents and children most likely directly impacted by immigration enforcement policies These results substantiate the need for a closer consideration of these remedial options.

Together, these chapters contribute to the discussion on the fetal origin hypothesis and highlight the need to include the potential for affecting maternal and infants' well-being as part of the debate of policies that affect social and physical environments. Individually, they offer insights to the discussion about the effectiveness of alcohol-related policies, the need for policies to reduce pollution, and the unanticipated consequences of interior immigration enforcement, respectively.

Appendix A: Chapter 1

Intrauterine Shocks and Health at Birth

While only a few studies have explored the causal impact of alcohol policies on health at birth, there is a growing body of literature that focuses on stressors related to access to alcohol. This section summarizes main findings associated with violence, stress, nutrition, and smoking.

Women are likely to be victims of violence and to suffer mental and physical stress of living in a violent context. A number of articles have established a negative correlation between prenatal assaults and adverse newborns' health (see, for example, Newberger et al. (1992), Petersen et al. (1997), and Ahmed et al. (2006)). Direct effects of blunt trauma to the maternal abdomen include abruptio placentae that may lead to fetal death or early onset of labor. More recently, studies using quasi-experimental methods have shown that being admitted to the hospital for an assault leads to a reduction of 163 grams, on average, on infant birth weight (Aizer, 2011) and reporting an assault at home while pregnant increases rates of very low birth weight (less than 1,500 grams) and very pre-term (less than 34 weeks gestation) births (Currie et al., 2018).

Violence may also affect birth outcomes indirectly through elevated stress levels. Studies in the medical literature have established the role of women's mental stress during pregnancy in the risk of adverse birth outcomes (Wadhwa et al., 2001). In the economics literature, it has been documented that prenatal exposure to local violence leads to a small increase in the risk of low birth weight and prematurity (Foureaux Koppensteiner and Manacorda, 2016). Another set of studies look at a

more extreme level of violence, including drug war, terrorist attacks and wars, and confirm the adverse effect of fear on birth weight (Brown, 2018; Camacho, 2008; Quintana-Domeque and Ródenas-Serrano, 2017; Torche and Shwed, 2015). Excess male fetal deaths has also been documented after the terrorist attacks of September 11, 2001 (Bruckner et al., 2010; Catalano et al., 2005). While maternal stress is a relevant mechanism explaining the results of these studies, it is likely that these extreme events also change communities' economic and social conditions. So, it is reasonable to expect greater effects from these shocks than from milder changes such as dry laws.

Fear of violence might also increase the likelihood of negative coping behaviors, such as poor maternal nutrition, smoking, and substance abuse (Moiduddin and Massey, 2008). Understanding the consequences of poor fetal nutrition has been the focus of many studies in different disciplines (see Almond and Currie (2011) for a review of these studies). From research comparing differences in death rates from cardiovascular disease in England and Wales to differences in neonatal mortality (Barker, 1995; Godfrey and Barker, 2000) to studies looking at famines, fasting, or twins (Roseboom et al., 2001; Almond and Mazumder, 2011; Behrman and Rosenzweig, 2004), findings confirm the negative effects of poor maternal nutrition on birth weight and human capital accumulation. Prenatal exposure to cigarette smoke has been linked to a higher probability of low birth weight (Simon, 2016).

Figures and Tables

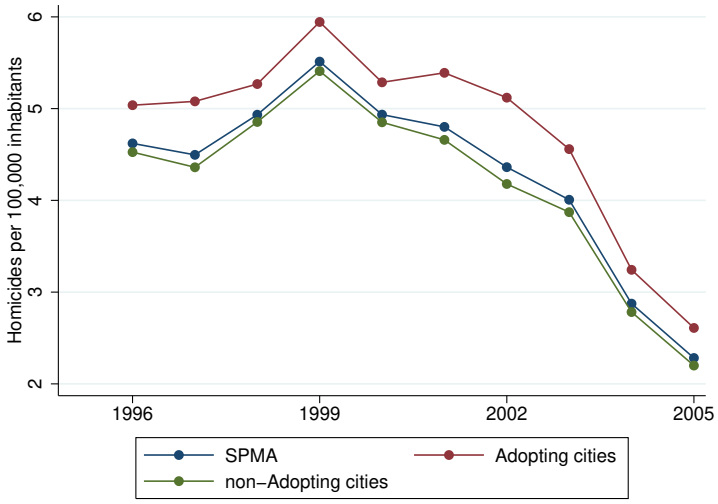


Figure A.1. Evolution of Homicide Rate in the SPMA (1996-2005)

Notes: Non-adopting municipalities correspond to other cities in the SPMA excluding São Paulo City and Taboao da Serra. Source: DATASUS

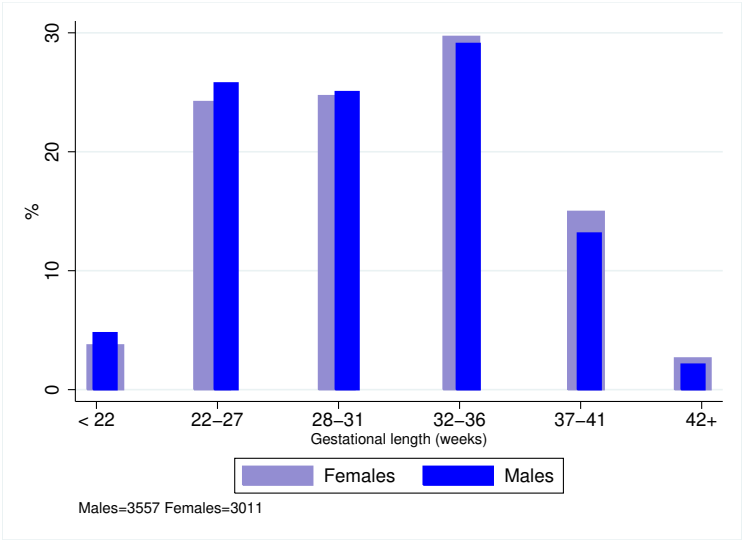


Figure A.2. Distribution of Stillbirth (2002-2005)

Notes: Figure includes stillbirths in the SPMA excluding São Paulo City and Taboao da Serra. Source: DATASUS

Table A.1. Effects on Birth Outcomes by Mother's Education

	Low Birth Weight (per 1,000) (1)	Preterm (per 1,000) (2)	Male (per 1,000) (3)
A. Mother's education: 0-11 years			
Dry Law 1 to 3 months before birth	-21.74	42.27	172.5*
	(44.36)	(38.15)	(92.21)
Dry Law 4 to 6 months before birth	11.58	-2.413	82.40
	(30.63)	(25.33)	(74.01)
Dry Law 7 to 9 months before birth	31.17	52.47*	-0.718
	(34.72)	(29.93)	(70.56)
Ymean	74.9	57.4	504.4
B. Mother's education: 12 or more years			
Dry Law 1 to 3 months before birth	1.347	7.443	53.07
	(27.87)	(24.37)	(64.28)
Dry Law 4 to 6 months before birth	-6.348	-4.001	38.35
	(23.80)	(18.44)	(59.93)
Dry Law 7 to 9 months before birth	16.28	29.55	4.711
	(22.58)	(22.30)	(54.11)
Ymean	67.1	58.4	516.4

Notes: Sample includes siblings if their mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed to these laws at all. Panel A includes 9,913 observations. Sample in panel B contains 16,036 observations. Each column indicates a regression. All regressions include mother fixed effects and controls following Eq. (1.3). *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Table A.2. Potential Mechanisms: Car Accidents by Race

	IHSF Rate of traffic-related deaths (per 100,000 inhabitants)					
	Whites			Blacks		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
A. Up to 9 months after adoption of the laws						
Dry Law 1 to 9 months before	-0.134	-0.092	-0.027	-0.178*	-0.061*	-0.028
	(0.135)[0.356]	(0.086)[0.335]	(0.028)[0.346]	(0.097)[0.096]	(0.041)[0.086]	(0.024)[0.310]
B. By quarters after the adoption of the laws						
Dry Law 1 to 3 months before	-0.154	-0.067	-0.055*	-0.236*	-0.070	-0.044*
	(0.222)[0.512]	(0.149)[0.675]	(0.031)[0.094]	(0.122)[0.077]	(0.046)[0.156]	(0.025)[0.093]
Dry Law 4 to 6 months before	-0.248*	-0.162**	-0.044	-0.175	-0.071	-0.008
	(0.115)[0.071]	(0.075)[0.042]	(0.041)[0.320]	(0.165)[0.371]	(0.056)[0.249]	(0.035)[0.812]
Dry Law 7 to 9 months before	0.022	-0.026	0.013	-0.122	-0.038	-0.0345
	(0.145)[0.884]	(0.098)[0.789]	(0.058)[0.874]	(0.111)[0.314]	(0.056)[0.523]	(0.023)[0.182]
Rate mean	1.20	0.561	0.123	1.01	0.248	0.044

Notes: Sample includes observations at the municipality-month level (N=1,541). Dependent variables are deaths rates per 100,000 inhabitants transformed by an inverse hyperbolic sine function. Each column in each panel indicates a regression. All specifications include municipality fixed effects and year-by-month fixed effects. Regressions are weighted by population in each cell. Robust standard errors (in parentheses) are clustered at the municipality level, and wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (249 replications) are reported in brackets. The last row in the table shows the average monthly rate of deaths per 100,000 inhabitants. ** significant at 5% level, * significant at 10% level.

Table A.3. Potential Mechanisms: Violence by Race

	IHSF Rate of homicides (per 100,000 inhabitants)					
	Whites			Blacks		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
A. Up to 9 months after adoption of the laws						
Dry Law 1 to 9 months before	-0.199**	-0.178**	-0.025	-0.152	-0.154	-0.103
	(0.080)[0.011]	(0.074)[0.029]	(0.070)[0.705]	(0.115)[0.215]	(0.113)[0.180]	(0.077)[0.237]
B. By quarters after the adoption of the laws						
Dry Law 1 to 3 months before	-0.169	-0.162	-0.026	-0.253	-0.237	-0.052
	(0.138)[0.271]	(0.128)[0.249]	(0.082)[0.787]	(0.181)[0.212]	(0.186)[0.265]	(0.090)[0.605]
Dry Law 4 to 6 months before	-0.098*	-0.089	0.037	-0.026	-0.041	-0.096
	(0.0606)[0.082]	(0.075)[0.236]	(0.102)[0.743]	(0.190)[0.909]	(0.191)[0.831]	(0.109)[0.412]
Dry Law 7 to 9 months before	-0.333	-0.275	-0.111*	-0.167	-0.165	-0.163*
	(0.250)[0.274]	(0.252)[0.361]	(0.057)[0.056]	(0.126)[0.211]	(0.121)[0.177]	(0.085)[0.093]
Rate mean	2.713	2.535	0.177	4.204	3.969	0.235

Notes: Sample includes observations at the municipality-month level (N=1,541). Dependent variables are homicide rates per 100,000 inhabitants transformed by an inverse hyperbolic sine function. Each column in each panel indicates a regression. All specifications include municipality fixed effects and year-by-month fixed effects. Regressions are weighted by population in each cell. Robust standard errors (in parentheses) are clustered at the municipality level, and wild cluster-bootstrap percentile-t procedure, imposing the null hypothesis (249 replications) are reported in brackets. The last row in the table shows the average monthly rate of homicides per 100,000 inhabitants. ** significant at 5% level, * significant at 10% level.

Table A.4. Potential Mechanisms: Family Composition by Race

	When her child is born, mother has a partner	
	White Children	Black Children
	(x100)	(x100)
	(1)	(2)
A. Up to 9 months after adoption of the laws		
Dry Law 1 to 9 months before birth	4.85**	4.83*
	(2.39)	(2.66)
B. By quarters after the adoption of the laws		
Dry Law 1 to 3 months before birth	11.29***	12.43***
	(3.84)	(4.82)
Dry Law 4 to 6 months before birth	5.06	6.32
	(4.25)	(4.48)
Dry Law 7 to 9 months before birth	-0.780	-2.11
	(3.69)	(3.99)
Ymean	54.0	42.9

Notes: Sample includes siblings if their mother was exposed to dry laws during her last pregnancy between 2002 and 2006 or she was not exposed at all to these laws. Dependent variables are multiplied by 100. Each column in each panel indicates a regression. Column 1 includes 10,830 observations; column 2 includes 8.626 observations. All specifications include mother fixed effects and year-by-month fixed effects. Standard errors are clustered at the mother level. *** significant at 1% level, ** significant at 5% level, * significant at 1% level.

Appendix B: Chapter 2

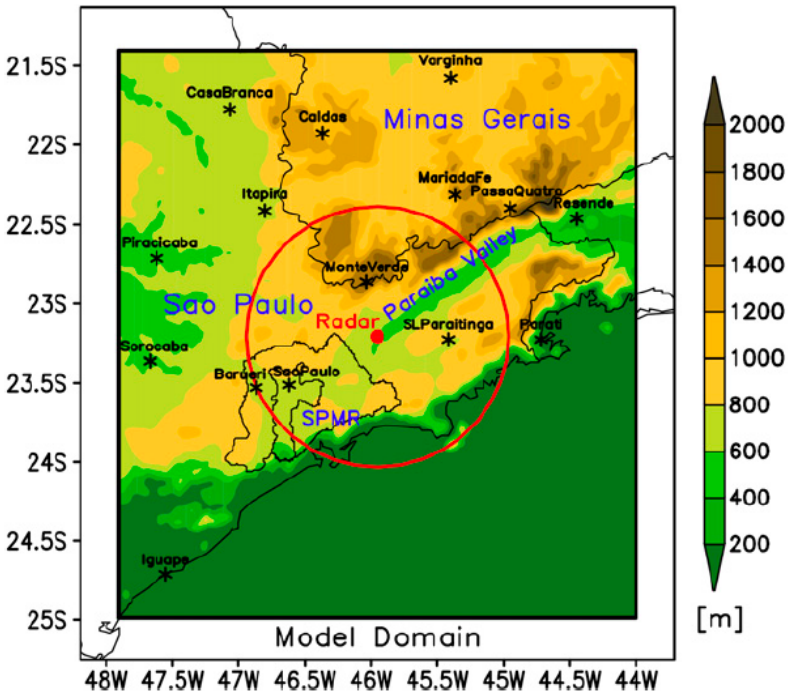
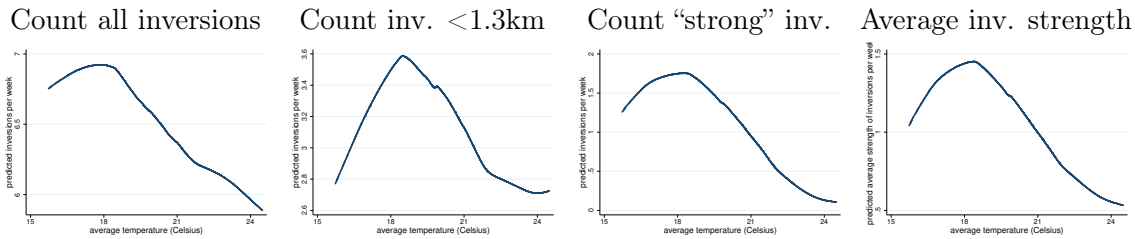


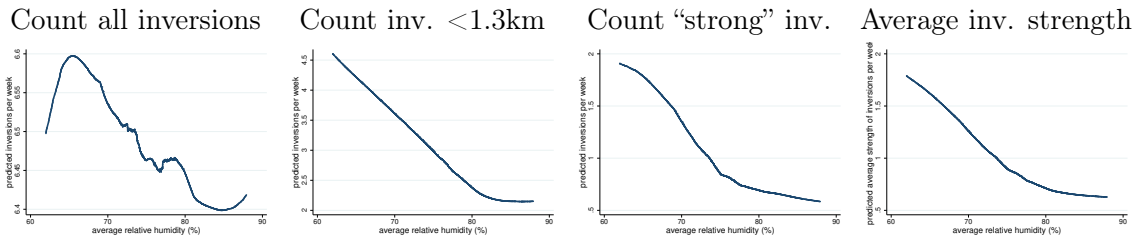
Figure B.1. Topography of the SPMA

Notes: Figure shows the topography of the SPMA and surrounding areas. SPMA is labeled in blue. Reprinted from Vendrasco et al. (2015).

(a) Ground temperature



(b) Relative humidity



(c) Rainfall

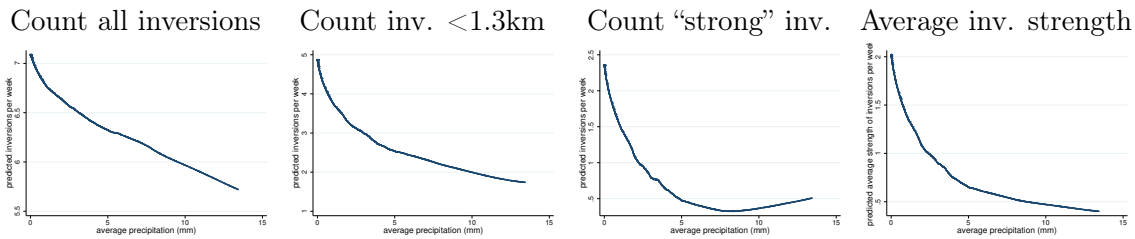
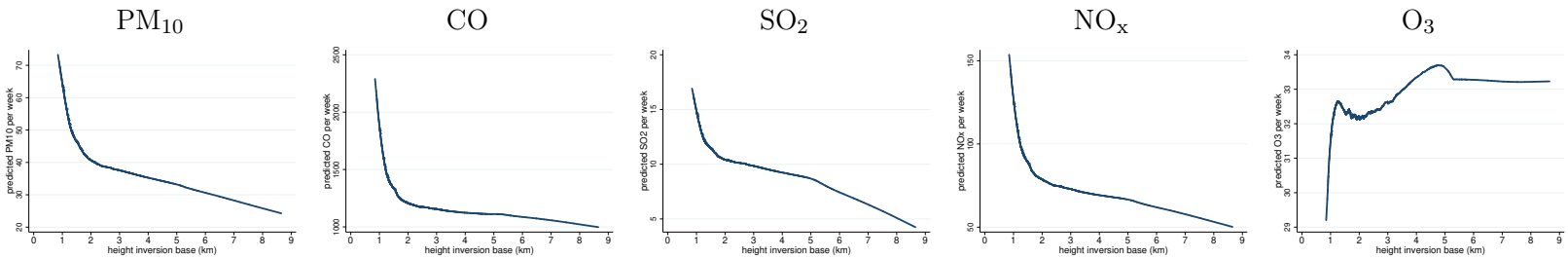


Figure B.3. Relationships Between Weather and Thermal Inversions

Notes: inv.=inversions. Figures plot locally weighted regressions of predicted count or strength of inversions on weather variables. Variables plotted on the y-axis are predicted 7-days count of all inversions (first column of graphs), 7-days count of inversions that occur below 1.3km (second column), 7-days count of "strong" inversion below 1.3km (third column), and 7-days average strength (change in temperature) of inversions below 1.3km (last column). Predicted values are calculated using regressions that include weather controls, year fixed effects, and week-of-the-year fixed effects. On the x-axis, I include 7-days average of ground temperature (top panel of graphs), relative humidity (center panel), and rainfall (bottom panel). Figures do not include values above the 95th percentile and below the 5th percentile for the weather variables.

(a) Height of inversion's base



(b) Count of inversions <1.3km

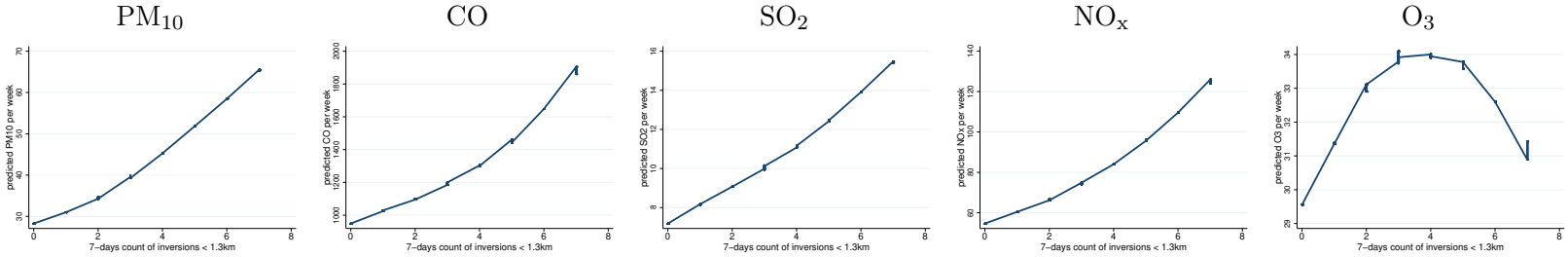


Figure B.4. Relationships Between Air Pollution and Thermal Inversions Counts

Notes: Figures plot locally weighted regressions of predicted concentration of pollutants on inversions' characteristics. Variables plotted on the y-axis are predicted 7-days average of pollution concentration that are calculated using regressions that include weather controls, year fixed effects, and week-of-the-year fixed effects. From left to right I include PM_{10}, CO, SO_2, NO_x, and O_3. On the x-axis, I include 7-days average of height of inversion' base (top panel of graphs), count of inversions below 1.3km (center panel), and average inversion strength for those below 1.3km (bottom panel).

(a) Average inversion (<1.3km) strength

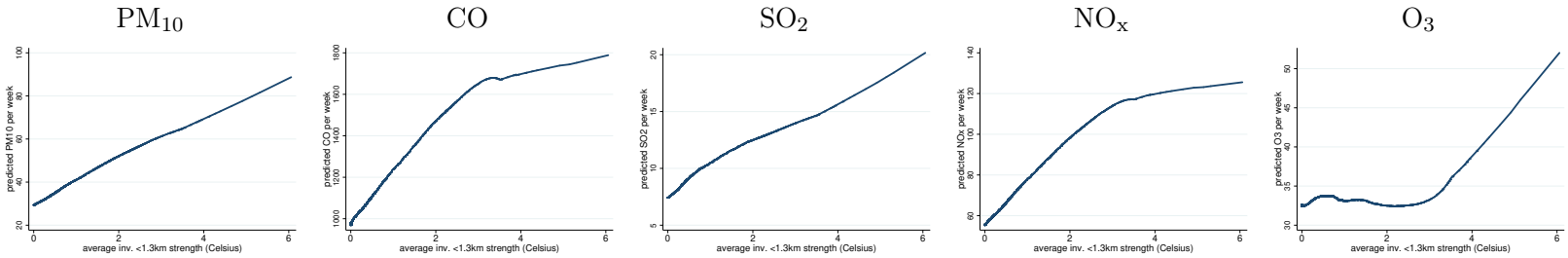


Figure B.5. Relationships Between Air Pollution and Thermal Inversions Strength

Notes: Figures plot locally weighted regressions of predicted concentration of pollutants on inversions' characteristics. Variables plotted on the y-axis are predicted 7-days average of pollution concentration that are calculated using regressions that include weather controls, year fixed effects, and week-of-the-year fixed effects. From left to right I include PM_{10}, CO, SO_2, NO_x, and O_3. On the x-axis, I include 7-days average of height of inversion' base (top panel of graphs), count of inversions below 1.3km (center panel), and average inversion strength for those below 1.3km (bottom panel).

Table B.1. Descriptive Statistics by Station: Pollutants

	PM_{10} ($\mu g/m^3$)		CO (ppm)		NO_x (ppb)		SO_2 ($\mu g/m^3$)		O_3 ($\mu g/m^3$)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
station 1	36.48	12.98	.	.	.	.	.	.	37.31	12.08
station 2	40.16	16.40	0.86	0.38	.	.	.	.	41.36	15.76
station 3	37.83	13.91	.	.	23.40	9.40	.	.	37.49	11.15
station 4	45.37	19.14	1.13	0.42	65.23	31.66	14.21	5.96	28.52	10.50
station 5	52.18	17.22	1.91	0.48	173.53	43.17	15.97	6.03	.	.
station 6	38.13	15.63	0.84	0.31	38.42	22.01	6.61	3.12	34.68	11.63
station 7	54.93	13.93	1.73	0.52	161.50	57.86	.	.	.	.
station 8	39.33	16.05	0.74	0.30	.	.	.	.	32.14	11.20
station 9	37.35	13.59	1.16	0.49	65.46	30.46	10.94	4.95	35.20	11.23
station 10	40.31	12.82	.	.	.	.	.	.	.	.
station 11	38.91	14.26	1.28	0.39	88.39	31.87	9.35	4.40	.	.
station 12	39.15	14.38	.	.	.	.	.	.	34.49	12.16
station 13	46.31	14.63	1.44	0.47	107.24	33.65	16.79	5.11	.	.
station 14	.	.	0.57	0.26	33.91	20.67	.	.	35.74	11.79
station 15	35.87	13.96	.	.	.	.	.	.	29.63	10.50
station 16	31.70	10.70	.	.	.	.	.	.	.	.
station 17	44.04	16.33	0.86	0.31	.	.	.	.	30.38	9.05
station 18	45.24	18.14	1.31	0.52	77.83	48.81	.	.	21.00	8.61
station 19	33.11	10.55	.	.	.	.	.	.	35.11	10.75
station 20	36.73	16.75	0.95	0.36	.	.	.	.	.	.
station 21	42.04	17.17	.	.	.	.	.	.	.	.
station 22	41.58	17.11	1.47	0.63	72.83	39.67	.	.	.	.
station 23	61.37	21.25	.	.	.	.	7.36	2.67	.	.
station 24	54.42	19.96	1.93	0.50	116.66	31.10	10.13	4.78	21.98	6.10
station 25	41.00	16.27	.	.	.	.	.	.	38.51	12.18
station 26	42.54	7.99	1.03	0.17	.	.	.	.	.	.

Table B.2. Descriptive Statistics by Station: Weather Conditions

	Temperature (C°)		Relative Humidity (%)		Rainfall (mm)	
	Mean	SD	Mean	SD	Mean	SD
station 1	20.39	2.62	74.42	7.24	4.23	4.84
station 2	20.22	2.61	74.58	7.29	3.97	4.59
station 3	20.47	2.79	79.10	6.97	3.53	4.53
station 4	20.48	2.61	74.97	7.16	4.42	5.25
station 5	20.10	2.51	76.08	7.60	4.12	4.65
station 6	20.02	2.46	76.56	8.20	4.56	5.38
station 7	20.27	2.61	72.36	8.60	3.89	4.49
station 8	20.47	2.64	76.24	7.08	4.21	4.71
station 9	20.77	2.88	80.41	7.90	3.70	4.54
station 10	20.43	2.61	76.27	7.13	4.36	5.00
station 11	20.29	2.58	72.85	8.31	4.57	5.42
station 12	20.44	2.69	77.45	7.15	3.37	4.26
station 13	20.44	2.58	74.49	7.14	4.70	5.79
station 14	20.27	2.62	72.23	9.58	4.36	5.51
station 15	20.23	2.63	72.74	8.28	4.04	4.52
station 16	20.31	2.89	79.69	7.08	3.93	4.30
station 17	20.34	2.64	75.73	7.05	4.03	4.11
station 18	20.30	2.65	70.83	11.52	4.30	4.86
station 19	20.53	2.84	79.74	7.13	3.96	4.51
station 20	20.68	2.84	79.98	7.52	3.85	4.55
station 21	20.55	2.75	78.53	7.34	3.97	4.80
station 22	20.22	2.70	73.29	8.18	4.11	4.48
station 23	20.32	2.56	77.56	6.71	3.96	4.16
station 24	20.28	2.62	73.19	8.19	3.99	4.35
station 25	20.29	2.57	78.54	6.77	3.89	4.10
station 26	20.64	2.82	79.68	7.38	3.91	4.51
station 27	20.29	2.62	75.64	7.10	3.93	4.36
station 28	20.28	2.62	73.58	7.85	3.76	4.28
station 29	20.33	2.58	77.07	6.76	4.00	4.21
station 30	20.29	2.59	78.53	6.76	3.84	4.08
station 31	20.26	2.65	74.04	7.48	4.02	4.25
station 32	20.28	2.61	72.55	8.78	4.10	4.52
station 33	20.53	2.76	78.55	7.15	3.86	4.53
station 34	20.31	2.56	77.73	6.69	3.75	4.13
station 35	20.34	2.63	77.40	6.72	3.82	3.95

Table B.3. Descriptive Statistics by Station: Wind

	Weekly average of hourly wind direction (%) Winds originating from octant...								
	NNE	ENE	ESE	SSE	SSW	WSW	WNW	NNW	Calm/Missing
station 1	13.7	35.1	19.1	2.0	3.3	11.6	8.7	3.4	
station 2	9.4	12.1	14.5	30.1	7.3	2.5	7.3	10.8	6.0
station 3	13.0	15.1	8.8	29.2	11.1	2.1	5.1	10.1	5.4
station 4	9.0	15.8	17.4	28.5	4.9	2.0	6.4	10.5	5.5
station 5	7.4	13.9	16.8	27.4	8.5	2.5	6.8	9.4	7.2
station 6	5.8	14.6	18.4	25.6	9.6	2.6	6.6	8.7	8.2
station 7	8.2	11.3	17.8	30.3	4.3	2.9	10.3	10.0	4.7
station 8	9.6	18.3	14.1	26.8	7.0	2.4	7.5	8.4	5.9
station 9	13.8	13.4	13.6	29.6	6.0	2.4	6.7	9.8	4.6
station 10	9.5	16.5	15.0	27.8	6.8	2.3	7.0	9.4	5.8
station 11	9.4	11.6	14.6	32.6	5.5	2.7	7.2	10.6	5.8
station 12	11.8	12.2	12.4	27.4	11.5	2.2	4.4	11.8	6.3
station 13	8.8	14.6	17.4	29.0	5.1	2.2	7.0	10.4	5.6
station 14	9.7	9.7	12.5	35.3	4.9	2.9	9.6	10.8	4.8
station 15	7.4	12.5	20.8	27.6	4.3	2.9	10.4	9.7	4.6
station 16	11.6	14.4	13.8	27.7	8.5	2.1	6.3	9.9	5.6
station 17	10.5	12.6	14.1	29.1	8.1	2.4	6.9	10.7	5.6
station 18	11.2	8.4	10.5	38.5	4.6	3.0	6.9	11.6	5.3
station 19	13.3	16.4	5.5	29.8	13.0	2.1	4.6	9.9	5.3
station 20	15.4	12.8	12.2	29.4	7.8	2.1	5.0	10.0	5.4
station 21	12.0	11.7	11.7	26.4	13.4	2.2	3.4	12.6	6.6
station 22	9.0	11.0	14.0	32.3	5.8	2.7	9.7	10.4	5.0
station 23	10.9	16.8	20.4	20.1	4.5	1.9	8.5	10.6	6.3
station 24	6.3	9.1	12.7	34.2	3.6	3.1	19.8	9.1	2.1
station 25	7.9	11.6	25.4	24.7	5.0	1.7	7.6	10.0	6.0
station 26	14.7	13.2	11.3	29.2	8.9	2.1	4.9	10.2	5.5
station 27	10.2	12.5	14.1	29.1	8.3	2.4	6.5	10.9	6.0
station 28	7.0	9.8	13.5	33.3	4.2	3.0	17.1	9.4	2.8
station 29	10.4	16.2	20.9	21.0	4.6	2.0	8.5	10.5	6.1
station 30	8.4	12.1	23.6	25.1	5.5	1.8	7.5	10.0	5.9
station 31	9.3	11.8	14.5	30.8	6.5	2.6	8.9	10.5	5.2
station 32	8.0	10.1	14.2	33.1	4.3	3.0	13.5	9.9	3.8
station 33	12.6	12.9	11.8	28.0	10.6	2.2	4.7	11.1	6.0
station 34	9.4	13.6	21.1	24.5	5.4	2.0	7.9	10.2	5.9
station 35	10.2	13.5	16.8	27.2	6.9	2.2	7.4	10.1	5.5

Table B.4. Effect of Thermal Inversions on Infant Health: 4-weeks Estimates

	Birth weight (grams) (1)	Low birth weight (x100) (2)	Very low birth weight (x100) (3)	Preterm birth (x 100) (4)	Very premature (x100) (5)
Count inversions weeks t to t-3	-8.613*	0.392*	0.165*	0.394*	0.124**
	(5.044)	(0.219)	(0.097)	(0.231)	(0.052)
Count inversions weeks t-4 to t-7	-11.660**	0.291	0.218**	0.561**	0.134***
	(5.369)	(0.245)	(0.094)	(0.235)	(0.051)
Count inversions weeks t-8 to t-11	-11.079**	0.511**	0.127	0.180	0.092*
	(4.306)	(0.222)	(0.098)	(0.257)	(0.054)
Count inversions weeks t-12 to t-15	-5.309	0.005	0.032	0.177	0.041
	(4.303)	(0.242)	(0.082)	(0.223)	(0.057)
Count inversions weeks t-16 to t-19	3.467	-0.228	0.032	-0.069	-0.009
	(4.377)	(0.206)	(0.087)	(0.251)	(0.054)
Count inversions weeks t-20 to t-23	2.459	-0.055	0.044	0.270	0.055
	(4.337)	(0.226)	(0.083)	(0.208)	(0.047)
Count inversions weeks t-24 to t-27	-1.127	0.350	0.032	0.363	0.026
	(4.259)	(0.231)	(0.090)	(0.227)	(0.052)
Count inversions weeks t-28 to t-31	-4.014	0.087	0.149*	0.142	0.014
	(4.458)	(0.207)	(0.088)	(0.212)	(0.052)
Count inversions weeks t-32 to t-35	-1.045	-0.198	-0.111	-0.107	0.010
	(4.297)	(0.235)	(0.085)	(0.208)	(0.049)
Count inversions weeks t-36 to t-39	-6.470	0.013	0.141	0.194	0.142***
	(4.532)	(0.233)	(0.094)	(0.248)	(0.049)
WOY x District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weather Controls	Yes	Yes	Yes	Yes	Yes
Observations	316,970	316,970	316,970	316,970	316,970
Ymean	3151.6	8.2	1.2	7.2	0.4
Ysd	214.2	11.1	4.4	10.7	2.7

Notes: WOY=week of he year. Observations are at location-date level and are weighted by the size of the local birth-cohort in that location-day. Estimates are 13-week sums of coefficients on weekly counts of thermal inversions. Coefficients indicate the effect of a SD increase in the weekly number of inversions over the corresponding window of time. All regressions control for an indicator of having missing information data over the preceding week and the number of dates with missing data in that week. Weather controls include cubic polynomials in mean temperature, maximum temperature, minimum temperature, humidity, direction of wind (fixed octants), and rainfall. Two-way clustering is employed; standard errors are clustered at location and date level. *** significant at 1% level, ** significant at 5% level, * significant at 10% level.

Appendix C: Chapter 3

Table C.1. List of Counties Included in the Analysis

County	All Newborns N	%	Less educated foreign parents	More educated foreign parents	US-born parents
Mecklenburg	20,277	13.0	1,770	2,095	11,460
Future 287(g) adopters					
Alamance	2,774	1.8	380	58	1,641
Cabarrus	3,558	2.3	281	179	2,325
Cumberland	8,011	5.1	58	198	5,192
Durham	5,976	3.8	646	624	2,958
Gaston	3,900	2.5	219	139	2,741
Guilford	8,774	5.6	597	699	5,181
Henderson	1,782	1.1	223	74	1,055
Wake	18,254	11.7	1,745	1,765	10,799
Future applicants for 287(g) that are denied					
Brunswick	1,430	0.9	82	20	1,020
Buncombe	3,883	2.5	223	184	2,643
Columbus	945	0.6	58	7	681
Duplin	1,198	0.8	282	37	572
Iredell	2,909	1.9	153	101	2,131
Lee	1,347	0.9	189	50	716
Randolph	2,560	1.6	300	63	1,674
Surry	1,269	0.8	163	22	824
Union	3,785	2.4	368	221	2,494
Yadkin	673	0.4	85	16	435
Never adopters or applicants					
Beaufort	3,531	2.3	87	53	2,732
Bladen	650	0.4	62	10	402
Burke	1,466	0.9	161	51	970
Catawba	2,890	1.9	197	167	1,883
Chatham	1,094	0.7	219	35	575
Craven	2,290	1.5	52	50	1,658
Davidson	2,759	1.8	176	74	2,036
Forsyth	7,000	4.5	726	347	3,779
Franklin	1,041	0.7	67	26	713
Harnett	2,193	1.4	172	36	1,378
Hoke	1,211	0.8	100	30	728
Johnston	3,405	2.2	428	91	2,178
Lenoir	1,168	0.7	60	14	819
Montgomery	589	0.4	117	20	295
Moore	1,392	0.9	95	19	969
Nash	1,787	1.1	135	27	1,191
New Hanover	3,495	2.2	137	110	2,453
Onslow	4,886	3.1	30	74	3,614
Orange	1,913	1.2	183	198	1,094
Pitt	3,096	2.0	130	89	2,052
Robeson	3,053	2.0	333	49	1,723
Rockingham	1,588	1.0	84	25	1,105
Rowan	2,468	1.6	208	67	1,715
Sampson	1,363	0.9	240	39	682
Stanly	1,011	0.6	46	19	772
Wayne	2,568	1.6	261	52	1,631
Wilkes	1,180	0.8	82	33	864
Wilson	1,545	1.0	178	40	936

References

Abdul-Wahab, S. A., S. Y. Al-Saifi, B. A. Alrumhi, M. Y. Abdulraheem, and M. Al-Uraimi (2004). Determination of the Features of the Low-level Temperature Inversions Above a Suburban Site in Oman Using Radiosonde Temperature Measurements: Long-term Analysis. *Journal of Geophysical Research: Atmospheres 109*(D20).

Abraham, S. and L. Sun (2018). Estimating Dynamic Treatment Effects in Event Studies With Heterogeneous Treatment Effects. *SSRN Electronic Journal*.

AGENDE (2007). Shadow Report of Civil Society to the Sixth National Report of Brazil on the Convention for the Elimination of All Forms of Discrimination against Women (CEDAW) 2001-2005.

Ahmed, S., M. A. Koenig, and R. Stephenson (2006, August). Effects of Domestic Violence on Perinatal and Early-childhood Mortality: Evidence from North India. *American Journal of Public Health 96*(8), 1423–1428.

Aizer, A. (2011). Poverty, Violence and Health: The Impact of Domestic Violence during Pregnancy on Newborn Health. *The Journal of human resources 46*(3), 518–538.

Albertsen, K., A.-M. N. Andersen, J. Olsen, and M. Grønbaek (2004, January). Alcohol Consumption During Pregnancy and the Risk of Preterm Delivery. *American Journal of Epidemiology 159*(2), 155–161.

Alderman, Jere R., H. B. (2006). Reducing the Incidence of Low Birth Weight in Low-Income Countries Has Substantial Economic Benefits. *World Bank Research Observer 21*(1), 25–48.

Ally, E. Z., R. Laranjeira, M. C. Viana, I. Pinsky, R. Caetano, S. Mitsuhiro, C. S. Madruga, E. Z. Ally, R. Laranjeira, M. C. Viana, I. Pinsky, R. Caetano, S. Mitsuhiro, and C. S. Madruga (2016, June). Intimate Partner Violence Trends in Brazil: Data from Two Waves of the Brazilian National Alcohol and Drugs Survey. *Brazilian Journal of Psychiatry 38*(2), 98–105.

Almon, S. (1965). The Distributed Lag Between Capital Appropriations and Expenditures. *Econometrica 33*(1), 178–196.

Almond, D. and J. Currie (2011). Killing Me Softly: The Fetal Origins Hypothesis. *The Journal of Economic Perspectives: A Journal of the American Economic Association 25*(3), 153–172.

Almond, D., J. Currie, and V. Duque (2018, December). Childhood Circumstances and Adult Outcomes: Act II. *Journal of Economic Literature 56*(4), 1360–1446.

Almond, D. and B. Mazumder (2011, October). Health Capital and the Prenatal Environment: The Effect of Ramadan Observance during Pregnancy. *American Economic Journal: Applied Economics 3*(4), 56–85.

Anderson, D. M., B. Crost, and D. I. Rees (2018). Wet Laws, Drinking Establishments and Violent Crime. *The Economic Journal 128*(611), 1333–1366.

Arbona, C., N. Olvera, N. Rodriguez, J. Hagan, A. Linares, and M. Wiesner (2010, August). Acculturative Stress Among Documented and Undocumented Latino Immigrants in the United States. *Hispanic journal of behavioral sciences 32*(3), 362–384.

Arceo, E., R. Hanna, and P. Oliva (2016). Does the Effect of Pollution on Infant Mortality Differ Between Developing and Developed Countries? Evidence from Mexico City. *The Economic Journal 126*(591), 257–280.

Arriaga, F. (2017). Relationships between the Public and Crimmigration Entities in North Carolina: A 287(g) Program Focus:. *Sociology of Race and Ethnicity*.

Athey, S. and G. Imbens (2018). Design-based Analysis in Difference-In-Differences Settings with Staggered Adoption. Technical Report w24963, NBER, Cambridge, MA.

Baptista, F. H., K. B. B. Rocha, J. L. Martinelli, L. R. d. S. d. Avó, R. A. Ferreira, C. M. R. Germano, D. G. Melo, F. H. Baptista, K. B. B. Rocha, J. L. Martinelli, L. R. d. S. d. Avó, R. A. Ferreira, C. M. R. Germano, and D. G. Melo (2017, June). Prevalência e Fatores Associados ao Consumo de Alcool Durante a Gravidez. *Revista Brasileira de Saúde Materno Infantil 17*(2), 271–279.

Barbazanges, A., P. V. Piazza, M. Le Moal, and S. Maccari (1996, June). Maternal Glucocorticoid Secretion Mediates Long-term Effects of Prenatal Stress. *The Journal of Neuroscience: The Official Journal of the Society for Neuroscience 16*(12), 3943–3949.

Barker, D. J. (1990). The Fetal and Infant Origins of Adult Disease. *BMJ (Clinical research ed.) 301*(6761), 1111.

Barker, D. J. (1995). Fetal Origins of Coronary Heart Disease. *BMJ : British Medical Journal 311*(6998), 171–174.

Barker, D. J. and C. Osmond (1986). Infant Mortality, Childhood Nutrition, and Ischaemic Heart Disease in England and Wales. *Lancet 1*(8489), 1077–1081.

Barreca, A. and M. Page (2015). A Pint for a Pound? Minimum Drinking Age Laws and Birth Outcomes. *Health Economics 24*(4), 400–418.

Behrman, J. R. and M. R. Rosenzweig (2004). Returns to Birthweight. *The Review of Economics and Statistics 86*(2), 586–601.

Bharadwaj, P., M. Gibson, J. G. Zivin, and C. Neilson (2017). Gray Matters: Fetal Pollution Exposure and Human Capital Formation. *Journal of the Association of Environmental and Resource Economists 4*(2), 505–542.

Bhutta, A. T., M. A. Cleves, P. H. Casey, M. M. Cradock, and K. J. S. Anand (2002). Cognitive and Behavioral Outcomes of School-aged Children Who Were Born Preterm: a Meta-analysis. *JAMA 288*(6), 728–737.

Biderman, C., J. M. P. D. Mello, and A. Schneider (2010). Dry Laws and Homicides: Evidence from the São Paulo Metropolitan Area. *The Economic Journal 120*(543), 157–182.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2007). From the Cradle to the Labor Market? The Effect of Birth Weight on Adult Outcomes. *The Quarterly Journal of Economics 122*(1), 409–439.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2016). Does Grief Transfer across Generations? Bereavements during Pregnancy and Child Outcomes. *American Economic Journal: Applied Economics 8*(1), 193–223.

Borja-Aburto, V. H., D. P. Loomis, S. I. Bangdiwala, C. M. Shy, and R. A. Rascon-Pacheco (1997). Ozone, Suspended Particulates, and Daily Mortality in Mexico City. *American Journal of Epidemiology 145*(3), 258–268.

Borusyak, K. and X. Jaravel (2016). Revisiting Event Study Designs. *SSRN Electronic Journal*.

Brabeck, K. M., M. B. Lykes, and C. Hunter (2014). The Psychosocial Impact of Detention and Deportation on U.S. Migrant Children and Families. *American Journal of Orthopsychiatry 84*(5), 496–505.

Brown, R. (2018). The Mexican Drug War and Early-Life Health: The Impact of Violent Crime on Birth Outcomes. *Demography 55*(1), 319–340.

Bruckner, T. A. and R. Catalano (2018). Selection in Utero and Population Health: Theory and Typology of Research. *SSM - Population Health 5*, 101–113.

Bruckner, T. A., R. Catalano, and J. Ahern (2010). Male Fetal Loss in the U.S. Following the Terrorist Attacks of September 11, 2001. *BMC Public Health 10*, 273.

Bruckner, T. A. and J. Nobles (2013). Intrauterine Stress and Male Cohort Quality: The Case of September 11, 2001. *Social Science & Medicine (1982) 76*(1), 107–114.

Burbidge, J. B., L. Magee, and A. L. Robb (1988). Alternative Transformations to Handle Extreme Values of the Dependent Variable. *Journal of the American Statistical Association 83*(401), 123–127.

Burton, R., C. Henn, D. Lavoie, R. O'Connor, C. Perkins, K. Sweeney, F. Greaves, B. Ferguson, C. Beynon, A. Belloni, V. Musto, J. Marsden, and N. Sheron (2017). A Rapid Evidence Review of the Effectiveness and Cost-effectiveness of Alcohol Control Policies: An English Perspective. *The Lancet 389*(10078), 1558–1580.

Caetano, R. and R. Laranjeira (2006). A 'Perfect Storm' in Developing Countries: Economic Growth and the Alcohol Industry. *Addiction 101*(2), 149–152.

Camacho, A. (2008). Stress and Birth Weight: Evidence from Terrorist Attacks. *American Economic Review: Papers & Proceedings 98*(2), 511–515.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-Based Improvements for Inference with Clustered Errors. *The Review of Economics and Statistics 90*(3), 414–427.

Capps, R., M. Chishti, J. Gelatt, J. Bolter, and A. Ruiz Soto (2018). Revving Up the Deportation Machinery: Enforcement under Trump and the Pushback. Technical report, Migration Policy Institute.

Capps, R., M. Rosenblum, C. Rodriguez, and Chishti (2011). Delegation and Divergence: 287(g) State and Local Immigration Enforcement. Technical report, Migration Policy Institute.

Cardoso, L. (2017). The Politics of Noise Control in São Paulo. *Journal of Latin American Studies 49*(4), 917–945.

Carpenter, C. and C. Dobkin (2009). The Effect of Alcohol Consumption on Mortality: Regression Discontinuity Evidence from the Minimum Drinking Age. *American Economic Journal. Applied Economics 1*(1), 164–182.

Carpenter, C. and C. Dobkin (2015). The Minimum Legal Drinking Age and Crime. *The review of economics and statistics 97*(2), 521–524.

Carpenter, C. S., C. Dobkin, and C. Warman (2016). The Mechanisms of Alcohol Control. *The Journal of Human Resources 51*(2), 328–356.

Carvalho, V. S. B., E. D. Freitas, L. D. Martins, J. A. Martins, C. R. Mazzoli, and M. d. F. Andrade (2015). Air Quality Status and Trends over the Metropolitan Area of São Paulo, Brazil as a Result of Emission Control Policies. *Environmental Science & Policy 47*, 68–79.

Castroand, D. S., Z. M. Sanchez, M. Zaleski, H. N. P. Alves, I. Pinsky, R. Caetano, and R. R. Laranjeira (2012). Sociodemographic Characteristics Associated with Binge Drinking among Brazilians. *Drug and Alcohol Dependence 126*(1-2), 272–276.

Catalano, R., T. Bruckner, J. Gould, B. Eskenazi, and E. Anderson (2005). Sex Ratios in California Following the Terrorist Attacks of September 11, 2001. *Human Reproduction 20*(5), 1221–1227.

Catalano, R., T. Bruckner, A. R. Marks, and B. Eskenazi (2006). Exogenous Shocks to the Human Sex Ratio: The Case of September 11, 2001 in New York City. *Human Reproduction 21*(12), 3127–3131.

Catalano, R., C. E. M. Zilko, K. B. Saxton, and T. Bruckner (2010). Selection in Utero: A Biological Response to Mass Layoffs. *American Journal of Human Biology 22*(3), 396–400.

CETESB (2002). Relatórios de Qualidade do ar no Estado de Sao Paulo. Technical report.

CETESB (2010). Qualidade do ar no Estado de Sao Paulo. Serie Relatorios.

Chaisemartin, C. d. and X. D'Haultfoeuille (2018). Two-way Fixed Effects Estimators with Heterogeneous Treatment Effects. Technical report.

Chaudry, A. (2011). Children in the Aftermath of Immigration Enforcement. *The Journal of the History of Childhood and Youth 4*(1), 137–154.

Chay, K. Y. and M. Greenstone (2003). The Impact of Air Pollution on Infant Mortality: Evidence from Geographic Variation in Pollution Shocks Induced by a Recession. *The Quarterly Journal of Economics 118*(3), 1121–1167.

Cook, P. J. (2007). *Paying the Tab: The Costs and Benefits of Alcohol Control*. Princeton University Press.

Cook, P. J. and C. P. Durrance (2013). The Virtuous Tax: Lifesaving and Crime-prevention Effects of the 1991 Federal Alcohol-tax Increase. *Journal of Health Economics 32*(1), 261–267.

Crost, B. and S. Guerrero (2012). The Effect of Alcohol Availability on Marijuana Use: Evidence from the Minimum Legal Drinking Age. *Journal of Health Economics 31*(1), 112–121.

Currie, J. (2011). Inequality at Birth: Some Causes and Consequences. *American Economic Review 101*(3), 1–22.

Currie, J. and R. Hyson (1999). Is the Impact of Health Shocks Cushioned by Socioeconomic Status? The Case of Low Birthweight. *American Economic Review 89*(2), 245–250.

Currie, J. and E. Moretti (2007). Biology as Destiny? Short- and Long-Run Determinants of Intergenerational Transmission of Birth Weight. *Journal of Labor Economics 25*(2), 231–264.

Currie, J., M. Mueller-Smith, and M. Rossin-Slater (2018). Violence While in Utero: The Impact of Assaults During Pregnancy on Birth Outcomes. SSRN Scholarly Paper ID 3214345, Social Science Research Network, Rochester, NY.

Currie, J., M. Neidell, and J. F. Schmieder (2009). Air Pollution and Infant Health: Lessons from New Jersey. *Journal of Health Economics 28*(3), 688–703.

Currie, J. and M. Rossin-Slater (2013). Weathering the Storm: Hurricanes and Birth Outcomes. *Journal of Health Economics 32*(3), 487–503.

Currie, J. and R. Walker (2011). Traffic Congestion and Infant Health: Evidence from E-ZPass. *American Economic Journal: Applied Economics 3*(1), 65–90.

Czarnecka, M., J. Nidzgorska-Lencewicz, and K. Rawicki (2019). Temporal Structure of Thermal Inversions in Łeba (Poland). *Theoretical and Applied Climatology 136*(1-2), 1–13.

de Weerth, C. and J. K. Buitelaar (2005). Physiological Stress Reactivity in Human Pregnancy—A Review. *Neuroscience & Biobehavioral Reviews 29*(2), 295–312.

Deaton, A. (1997). *The Analysis of Household Surveys: A Microeconometric Approach to Development Policy*. The World Bank.

Dee, T. S. (2001). The Effects of Minimum Legal Drinking Ages on Teen Childbearing. *The Journal of Human Resources 36*(4), 823–838.

Dole, N., D. A. Savitz, I. Hertz-Picciotto, A. M. Siega-Riz, M. J. McMahon, and P. Buekens (2003). Maternal Stress and Preterm Birth. *American Journal of Epidemiology 157*(1), 14–24.

Dreby, J. (2012). The Burden of Deportation on Children in Mexican Immigrant Families. *Journal of Marriage and Family 74*(4), 829–845.

Duailibi, S., W. Ponicki, J. Grube, I. Pinsky, R. Laranjeira, and M. Raw (2007). The Effect of Restricting Opening Hours on Alcohol-Related Violence. *American Journal of Public Health 97*(12), 2276–2280.

East, C. N., P. Luck, H. Mansour, and A. Velasquez (2018). The Labor Market Effects of Immigration Enforcement. *IZA DP No. 11486*, 52.

Ellis, M., R. Wright, and M. Townley (2016). State-Scale Immigration Enforcement and Latino Interstate Migration in the United States. *Annals of the American Association of Geographers 106*(4).

Ferrari, V. (2008). *O Mercado de Cervejas no Brasil.* Mestrado em Economia, Fac. de Administracao, Contabilidade e Economia, PUCRS.

Fertig, A. R. and T. Watson (2009). Minimum Drinking Age Laws and Infant Health Outcomes. *Journal of Health Economics 28*(3), 737–747.

Figlio, D., J. Guryan, K. Karbownik, and J. Roth (2014). The Effects of Poor Neonatal Health on Children's Cognitive Development. *The American Economic Review; Nashville 104*(12), 3921–3955.

Foureaux Koppensteiner, M. and M. Manacorda (2016). Violence and Birth Outcomes: Evidence from Homicides in Brazil. *Journal of Development Economics 119*, 16–33.

Godfrey, K. M. and D. J. Barker (2000). Fetal Nutrition and Adult Disease. *The American Journal of Clinical Nutrition 71*(5 Suppl), 1344S–52S.

Golash-Boza, T. and P. Hondagneu-Sotelo (2013). Latino Immigrant Men and the Deportation Crisis: A Gendered Racial Removal Program. *Latino Studies 11*(3), 271–292.

Goldenberg, R. L., J. F. Culhane, J. D. Iams, and R. Romero (2008). Epidemiology and Causes of Preterm Birth. *Lancet (London, England) 371*(9606), 75–84.

Goldenberg, R. L., J. C. Hauth, and W. W. Andrews (2000). Intrauterine Infection and Preterm Delivery. *New England Journal of Medicine 342*(20), 1500–1507.

Gonzalez-Barreca, A. and J. M. Krogstad (2016). U.S. Immigrant Deportations Down in 2014, but still Near Record High. Pew Research Center.

Goodman-Bacon, A. (2018). Difference-in-Differences with Variation in Treatment Timing. Technical Report w25018, NBER.

Gouveia, N., S. Bremner, and H. Novaes (2004, January). Association between Ambient Air Pollution and Birth Weight in São Paulo, Brazil. *Journal of Epidemiology and Community Health 58*(1), 11–17.

Graham, K. and S. Wells (2003). 'Somebody's Gonna Get Their Head Kicked in Tonight!' Aggression Among Young Males in Bars—A Question of Values? *The British Journal of Criminology 43*(3), 546–566.

Grech, V. (2015). The Secondary Sex Ratio at Birth Was Depressed in Quebec by the Sovereignty Referendums. *Journal of Obstetrics and Gynaecology Canada 37*(5), 405–411.

Gruenewald, P. J. (2011). Regulating Availability: How Access to Alcohol Affects Drinking and Problems in Youth and Adults. *Alcohol Research & Health: The Journal of the National Institute on Alcohol Abuse and Alcoholism 34*(2), 248–256.

Grönqvist, H. and S. Niknami (2014). Alcohol Availability and Crime: Lessons from Liberalized Weekend Sales Restrictions. *Journal of Urban Economics 81*, 77–84.

Gunn, J. K. L., C. B. Rosales, K. E. Center, A. Nuñez, S. J. Gibson, C. Christ, and J. E. Ehiri (2016). Prenatal Exposure to Cannabis and Maternal and Child Health Outcomes: A Systematic Review and Meta-Analysis. *BMJ Open 6*(4).

Gyimah-Brempong, K. and J. Racine (2006). Alcohol Availability and Crime: A Robust Approach. *Applied Economics 38*(11), 1293–1307.

Hagan, J., N. Rodriguez, R. Capps, and N. Kabiri (2003). The Effects of Recent Welfare and Immigration Reforms on Immigrants' Access to Health Care. *The International Migration Review 37*(2), 444–463.

Hahn, R. A., J. L. Kuzara, R. Elder, R. Brewer, S. Chattopadhyay, J. Fielding, T. S. Naimi, T. Toomey, J. C. Middleton, B. Lawrence, and Task Force on Community Preventive Services (2010). Effectiveness of Policies Restricting Hours of Alcohol Aales in Preventing Excessive Alcohol Consumption and Related Harms. *American Journal of Preventive Medicine 39*(6), 590–604.

Heaton, P. (2012). Sunday Liquor Laws and Crime. *Journal of Public Economics 96*(1), 42–52.

Hobel, C. J., C. Dunkel-Schetter, S. C. Roesch, L. C. Castro, and C. P. Arora (1999). Maternal Plasma Corticotropin-releasing Hormone Associated with Stress at 20 Weeks' Gestation in Pregnancies Ending in Preterm Delivery. *American Journal of Obstetrics and Gynecology 180*(1), S257–263.

Hoffman, S. and M. C. Hatch (1996). Stress, Social Support and Pregnancy Outcome: A Reassessment based on Recent Research. *Paediatric and Perinatal Epidemiology 10*(4), 380–405.

Instituto Brasileiro de Geografia e Estatística (2010). *Um Panorama da Saúde no Brasil: Acesso e Utilização dos Serviços, Condições de Saúde e Fatores de Risco e Proteção à Saúde, 2008*. Rio de Janeiro: IBGE.

Jacobson, M. Z. (2005). *Fundamentals of Atmospheric Modeling*. Cambridge University Press.

Jans, J., P. Johansson, and J. P. Nilsson (2018). Economic Status, Air Quality, and Child Health: Evidence from Inversion Episodes. *Journal of Health Economics 61*, 220–232.

Jayachandran, S. (2009). Air Quality and Early-Life Mortality: Evidence from Indonesia's Wildfires. *The Journal of Human Resources 44*(4), 916–954.

Jones, K. L. and D. W. Smith (1973). Recognition of the Fetal Alcohol Syndrome in Early Infancy. *Lancet 302*(7836), 999–1001.

Jorge, M. H. P. d. M., R. Laurenti, and S. L. D. Gotlieb (2007). Análise da Qualidade das Estatísticas Vitais Brasileiras: A Experiência de Implantação do SIM e do SINASC. *Ciência & Saúde Coletiva 12*(3), 643–654.

Kerr-Correa, F., A. Hegedus, A. Sanches, L. Trinca, L. Kerr-Pontes, A. Tucci, and T. Floripes (2005). Differences in Drinking Patterns between Men and Women in Brazil. In I. Obot and R. Room (Eds.), *Alcohol, gender and drinking problems: perspectives from low and middle income countries*. Geneva: World Health Organization.

Kikaj, D., J. Vaupotič, and S. D. Chambers (2019). Identifying Persistent Temperature Inversion Events in a Subalpine Basin Using Radon-222. *Atmospheric Measurement Techniques 12*(8), 4455–4477.

Kinney, P. L., M. G. Gichuru, N. Volavka-Close, N. Ngo, P. K. Ndiba, A. Law, A. Gachanja, S. M. Gaita, S. N. Chillrud, and E. Sclar (2011). Traffic Impacts on PM2.5 Air Quality in Nairobi, Kenya. *Environmental science & policy 14*(4), 369–378.

Knittel, C. R., D. L. Miller, and N. J. Sanders (2015). Caution, Drivers! Children Present: Traffic, Pollution, and Infant Health. *The Review of Economics and Statistics 98*(2), 350–366.

Koball, H., R. Capps, S. Hooker, K. Perreira, A. Campetella, J. M. Pedroza, W. Monson, and S. Huerta (2015). Health and Social Service Needs of U.S.-Citizen Children with Detained or Deported Immigrant Parents. Technical report, Urban Institute and Migration Policy Institute.

Kotelchuck, M. (1994). The Adequacy of Prenatal Care Utilization Index: its US distribution and association with low birthweight. *American Journal of Public Health 84*(9), 1486–1489.

Kraemer, S. (2000). The Fragile Male. *BMJ 321*(7276), 1609–1612.

Kramer, M. S., A. Papageorghiou, J. Culhane, Z. Bhutta, R. L. Goldenberg, M. Gravett, J. D. Iams, A. Conde-Agudelo, S. Waller, F. Barros, H. Knight, and J. Villar (2012). Challenges in Defining and Classifying the Preterm Birth Syndrome. *American Journal of Obstetrics and Gynecology 206*(2), 108–112.

Lange, S., C. Probst, N. Heer, M. Roerecke, J. Rehm, M. G. Monteiro, K. Shield, C. de Oliveira, and S. Popova (2017). Actual and Predicted Prevalence of Alcohol Consumption during Pregnancy in Latin America and the Caribbean: Systematic Literature Review and Meta-analysis. *Pan American Journal of Public Health 41*, e89.

Langenderfer, L. (2013). Alcohol Use Among Partner Violent Adults: Reviewing Recent Literature to Inform Intervention. *Aggression and Violent Behavior 18*(1), 152–158.

Laranjeira, R. and D. Hinkly (2002). Evaluation of Alcohol Outlet Density and its Relation with Violence. *Revista De Saude Publica 36*(4), 455–461.

Laranjeira, R., I. Pinsky, M. Zaleski, and R. Caetano (2007). I Levantamento Nacional sobre os Padrões de Consumo de Alcool na População Brasileira.

Leamer, E. E. (1978). *Specification Searches: Ad Hoc Inference with Nonexperimental Data*. Wiley.

Leonard, K. E. and B. M. Quigley (2017). Thirty Years of Research Show Alcohol to be a Cause of Intimate Partner Violence: Future Research Needs to Identify who to Treat and how to Treat them. *Drug and Alcohol Review 36*(1), 7–9.

Lima, G. N. d. and V. O. Magaña Rueda (2018). The Urban Growth of the Metropolitan Area of Sao Paulo and its Impact on the Climate. *Weather and Climate Extremes 21*, 17–26.

Lin, C. A., L. a. A. Pereira, D. C. Nishioka, G. M. S. Conceição, A. L. F. Braga, and P. H. N. Saldiva (2004). Air Pollution and Neonatal Deaths in São Paulo, Brazil. *Brazilian Journal of Medical and Biological Research 37*(5), 765–770.

Lobel, M., D. L. Cannella, J. E. Graham, C. DeVincent, J. Schneider, and B. A. Meyer (2008). Pregnancy-specific Stress, Prenatal Health Behaviors, and Birth Outcomes. *Health Psychology: Official Journal of the Division of Health Psychology, American Psychological Association 27*(5), 604–615.

Luca, D. L., E. Owens, and G. Sharma (2015, May). Can Alcohol Prohibition Reduce Violence against Women? *American Economic Review 105*(5), 625–629.

MacKinnon, J. G. and L. Magee (1990). Transforming the Dependent Variable in Regression Models. *International Economic Review 31*(2), 315–339.

Majzoub, J. A. and K. P. Karalis (1999). Placental Corticotropin-releasing Hormone: Function and Regulation. *American Journal of Obstetrics and Gynecology 180*(1), S242–246.

Marcus, J. and T. Siedler (2015). Reducing Binge Drinking? The Effect of a Ban on Late-night off-premise Alcohol Sales on Alcohol-Related Hospital Stays in Germany. *Journal of Public Economics 123*, 55–77.

Martin Bassols, N. and J. Vall Castello (2018). Bar Opening Hours, Alcohol Consumption and Workplace Accidents. *Labour Economics 53*, 172–181.

May, P. A., B. Tabachnick, J. M. Hasken, A.-S. Marais, M. M. de Vries, R. Barnard, B. Joubert, M. Cloete, I. Botha, W. O. Kalberg, D. Buckley, Z. R. Burroughs, H. Bezuidenhout, L. K. Robinson, M. A. Manning, C. M. Adnams, S. Seedat, C. D. H. Parry, and H. E. Hoyme (2017). Who is Most Affected by Prenatal Alcohol Exposure: Boys or Girls? *Drug and Alcohol Dependence 177*, 258–267.

McKinney, C. M., R. Caetano, T. R. Harris, and M. S. Ebama (2009). Alcohol Availability and Intimate Partner Violence among US Couples. *Alcoholism, Clinical and Experimental Research 33*(1), 169–176.

McLean, n. and n. Smith (1999). Corticotropin-releasing Hormone in Human Pregnancy and Parturition. *Trends in endocrinology and metabolism 10*(5), 174–178.

Meaney, M. J., J. Diorio, D. Francis, J. Widdowson, P. LaPlante, C. Caldji, S. Sharma, J. R. Seckl, and P. M. Plotsky (1996). Early Environmental Regulation of Forebrain Glucocorticoid Receptor Gene Expression: Implications for Adrenocortical Responses to Stress. *Developmental Neuroscience 18*(1-2), 49–72.

Middleton, J. C., R. A. Hahn, J. L. Kuzara, R. Elder, R. Brewer, S. Chattopadhyay, J. Fielding, T. S. Naimi, T. Toomey, B. Lawrence, and Task Force on Community Preventive Services (2010). Effectiveness of Policies Maintaining or Restricting Days of Alcohol Sales on Excessive Alcohol Consumption and Related Harms. *American Journal of Preventive Medicine 39*(6), 575–589.

Moiduddin, E. and D. Massey (2008). Neighborhood Disadvantage and Birth Weight: The Role of Perceived Danger and Substance Abuse. *International Journal of Conflict and Violence 2*(1), 113–129.

Moura, T. W. d. (2012). Política Pública de Restrição do Horário de Funcionamento de Bares. *6*(2), 352–373.

Mulder, E. J. H., P. G. Robles de Medina, A. C. Huizink, B. R. H. Van den Bergh, J. K. Buitelaar, and G. H. A. Visser (2002). Prenatal Maternal Stress: Effects on Pregnancy and the (Unborn) Child. *Early Human Development 70*(1), 3–14.

Murray, J., D. R. d. C. Cerqueira, and T. Kahn (2013). Crime and Violence in Brazil: Systematic Review of Time Trends, Prevalence Rates and Risk Factors. *Aggression and Violent Behavior 18*(5), 471–483.

Nakaguma, M. Y. and B. J. Restrepo (2018). Restricting Access to Alcohol and Public Health: Evidence from Electoral Dry Laws in Brazil. *Health Economics 27*(1), 141–156.

Newberger, E. H., S. E. Barkan, E. S. Lieberman, M. C. McCormick, K. Yllo, L. T. Gary, and S. Schechter (1992). Abuse of Pregnant Women and Adverse Birth Outcome. Current Knowledge and Implications for Practice. *JAMA 267*(17), 2370–2372.

Nguyen, M. T. and H. Gill (2010). The 287(g) Program: The Costs and Consequences of Local Immigration Enforcement in North Carolina Communities. Technical report.

Nguyen, M. T. and H. Gill (2015). Interior Immigration Enforcement: The Impacts of Expanding Local Law Enforcement Authority. *Urban Studies*.

Nilsson, J. P. (2017). Alcohol Availability, Prenatal Conditions, and Long-Term Economic Outcomes. *Journal of Political Economy 125*(4), 1149–1207.

Norström, T. and I. Rossow (2014). Cannabis Use and Violence: Is There a Link? *Scandinavian Journal of Public Health 42*(4), 358–363.

Novak, N. L., A. T. Geronimus, and A. M. Martinez-Cardoso (2017). Change in Birth Outcomes among Infants Born to Latina Mothers after a Major Immigration Raid. *International Journal of Epidemiology 46*(3), 839–849.

Oken, E., K. P. Kleinman, J. Rich-Edwards, and M. W. Gillman (2003). A Nearly Continuous Measure of Birth Weight for Gestational Age Using a United States National Reference. *BMC pediatrics 3*, 6.

Orzack, S. H., J. W. Stubblefield, V. R. Akmaev, P. Colls, S. Munné, T. Scholl, D. Steinsaltz, and J. E. Zuckerman (2015). The Human Sex Ratio from Conception to Birth. *Proceedings of the National Academy of Sciences 112*(16), E2102–E2111.

Passel, J. and D'vera (2016). Size of U.S. Unauthoriized Immigrant Workforce Stable After the Great Recession.

Perova, E. and S. A. Reynolds (2017). Women's Police Stations and Intimate Partner Violence: Evidence from Brazil. *Social Science & Medicine 174*, 188–196.

Persson, P. and M. Rossin-Slater (2018). Family Ruptures, Stress, and the Mental Health of the Next Generation: Reply. *American Economic Review 108*(4-5), 1256–1263.

Petersen, R., J. A. Gazmararian, A. M. Spitz, D. L. Rowley, M. M. Goodwin, L. E. Saltzman, and J. S. Marks (1997). Violence and Adverse Pregnancy Outcomes: A Review of the Literature and Directions for Future Research. *American Journal of Preventive Medicine 13*(5), 366–373.

Pinsky, I., S. A. R. J. E. Jundi, M. Sanches, M. J. B. Zaleski, R. R. Laranjeira, and R. Caetano (2010). Exposure of Adolescents and Young adults to Alcohol Advertising in Brazil. *Journal of Public Affairs 10*(1-2), 50–58.

Pope, N. G. (2016). The Effects of DACAmentation: The Impact of Deferred Action for Childhood Arrivals on Unauthorized Immigrants. *Journal of Public Economics 143*, 98–114.

Popova, S., N. Giesbrecht, D. Bekmuradov, and J. Patra (2009). Hours and Days of Sale and Density of Alcohol Outlets: Impacts on Alcohol Consumption and Damage: A Systematic Review. *Alcohol and Alcoholism 44*(5), 500–516.

Potochnick, S., J.-H. Chen, and K. Perreira (2017). Local-Level Immigration Enforcement and Food Insecurity Risk among Hispanic Immigrant Families with Children: National-Level Evidence. *Journal of Immigrant and Minority Health 19*(5), 1042–1049.

Pridemore, W. A. and T. H. Grubesic (2013). Alcohol Outlets and Community Levels of Interpersonal Violence: Spatial Density, Outlet Type, and Seriousness of Assault. *Journal of Research in Crime and Delinquency 50*(1), 132–159.

Quintana-Domeque, C. and P. Ródenas-Serrano (2017). The Hidden Costs of Terrorism: The Effects on Health at Birth. *Journal of Health Economics 56*, 47–60.

Rangel, M. A. and T. S. Vogl (2018). Agricultural Fires and Health at Birth. *The Review of Economics and Statistics 101*(4), 616–630.

Rehm, J. and M. Monteiro (2005). Alcohol Consumption and Burden of Disease in the Americas: Implications for Alcohol Policy. *Pan American Journal of Public Health 18*(4-5), 241–248.

Rendón, A. M., J. F. Salazar, C. A. Palacio, V. Wirth, and B. Brötz (2014). Effects of Urbanization on the Temperature Inversion Breakup in a Mountain Valley with Implications for Air Quality. *Journal of Applied Meteorology and Climatology 53*(4), 840–858.

Rhodes, S. D., L. Mann, F. M. Simán, E. Song, J. Alonzo, M. Downs, E. Lawlor, O. Martinez, C. J. Sun, M. C. O'Brien, B. A. Reboussin, and M. A. Hall (2015). The Impact of Local Immigration Enforcement Policies on the Health of Immigrant Hispanics/Latinos in the United States. *American Journal of Public Health 105*(2), 329–337.

Ribeiro, H. and J. V. de Assuncao (2001). Historical Overview of Air Pollution in Sao Paulo Metropolitan Area, Brazil: Influence of Mobile Sources and Related Health Effects. *Transactions on the Built Environment 52*, 10.

Rolnik, R. and D. Klintowitz (2011). (Im)Mobility in the city of São Paulo. *Estudos Avançados 25*(71), 89–108.

Romano, M., S. Duailibi, I. Pinsky, and R. Laranjeira (2007). Alcohol Purchase Survey by Adolescents in Two Cities of State of São Paulo, Southeastern Brazil. *Revista de Saúde Pública 41*(4), 495–501.

Room, R., T. Babor, and J. Rehm (2005). Alcohol and Public Health. *Lancet 365*(9458), 519–530.

Roseboom, T. J., J. H. van der Meulen, A. C. Ravelli, C. Osmond, D. J. Barker, and O. P. Bleker (2001). Effects of Prenatal Exposure to the Dutch Famine on Adult Disease in Later Life: An Overview. *Molecular and Cellular Endocrinology 185*(1-2), 93–98.

Rossow, I. and T. Norström (2012). The Impact of Small Changes in Bar Closing Hours on Violence. The Norwegian Experience from 18 Cities. *Addiction 107*(3), 530–537.

Russell, M. (1991). Clinical Implications of Recent Research on the Fetal Alcohol Syndrome. *Bulletin of the New York Academy of Medicine 67*(3), 207–222.

Sadar, A. J. (2018). The Art and Science of Forecasting Morning Temperature Inversions. *em PLUS*, 4.

Sanders, N. J. (2012). What Doesn't Kill You Makes You Weaker Prenatal Pollution Exposure and Educational Outcomes. *Journal of Human Resources 47*(3), 826–850.

Sanders, N. J. and C. Stoecker (2015). Where Have all the Young Men Gone? Using Sex Ratios to Measure Fetal Death Rates. *Journal of Health Economics 41*, 30–45.

Sargan, J. D. (1980). The Consumer Price Equation in the Post War British Economy: An Exercise in Equation Specification Testing. *The Review of Economic Studies 47*(1), 113–135.

Schwarz, G. (1978). Estimating the Dimension of a Model. *The Annals of Statistics 6*(2), 461–464.

Segre, C. A. d. M. (2010). *Efeitos do álcool na gestante, no feto e no recém-nascido*. Sao Paulo: Sociedade de Pediatria de Sao Paulo.

Shield, K. D., M. Monteiro, M. Roerecke, B. Smith, and J. Rehm (2015). Alcohol Consumption and Burden of Disease in the Americas in 2012: Implications for Alcohol Policy. *Pan American Journal of Public Health 38*(6), 442–449.

Shonkoff, J. P., W. T. Boyce, and B. S. McEwen (2009). Neuroscience, Molecular Biology, and the Childhood Roots of Health Disparities: Building a New Framework for Health Promotion and Disease Prevention. *JAMA 301*(21), 2252–2259.

Simeonova, E. (2011). Out of Sight, Out of Mind? Natural Disasters and Pregnancy Outcomes in the USA. *CESifo Economic Studies; Oxford 57*(3), 403–431.

Simon, D. (2016). Does Early Life Exposure to Cigarette Smoke Permanently Harm Childhood Welfare? Evidence from Cigarette Tax Hikes. *American Economic Journal: Applied Economics 8*(4), 128–159.

Teixeira, J. M., N. M. Fisk, and V. Glover (1999). Association between Maternal Anxiety in Pregnancy and Increased Uterine Artery Resistance Index: Cohort Based Study. *BMJ (Clinical research ed.) 318*(7177), 153–157.

Torche, F. and U. Shwed (2015). The Hidden Costs of War: Exposure to Armed Conflict and Birth Outcomes. *Sociological Science 2*, 558–581.

Torche, F. and C. Sirois (2019). Restrictive Immigration Law and Birth Outcomes of Immigrant Women. *American Journal of Epidemiology 188*(1), 24–33.

Trivers, R. L. and D. E. Willard (1973). Natural Selection of Parental Ability to Vary the Sex Ratio of Offspring. *Science 179*(4068), 90–92.

Valero De Bernabé, J., T. Soriano, R. Albaladejo, M. Juarranz, M. E. Calle, D. Martínez, and V. Domínguez-Rojas (2004). Risk Factors for Low Birth Weight: a Review. *European Journal of Obstetrics, Gynecology, and Reproductive Biology 116*(1), 3–15.

Vargas, E. D. and M. A. Pirog (2016). Mixed-Status Families and WIC Uptake: The Effects of Risk of Deporta on Program Use. *Social Science Quarterly 97*(3), 555–572.

Vendrasco, E., J. Sun, D. Herdies, and C. Angelis (2015). Constraining a 3DVAR Radar Data Assimilation System with Large-Scale Analysis to Improve Short-Range Precipitation Forecasts. *Journal of Applied Meteorology and Climatology 55*.

Wadhwa, P. D., J. F. Culhane, V. Rauh, S. S. Barve, V. Hogan, C. A. Sandman, C. J. Hobel, A. Chicz-DeMet, C. Dunkel-Schetter, T. J. Garite, and L. Glynn (2001). Stress, Infection and Preterm Birth: a Biobehavioural Perspective. *Paediatric and Perinatal Epidemiology 15 Suppl 2*(2), 17–29.

Waiselfisz, J. J. (2012). Mapa da Violencia 2012 - Atualizacao: Homicidio de Mulheres no Brasil. Rio de Janeiro, RJ, Brazil: FLACSO.

Watson, T. (2014). Inside the Refrigerator: Immigration Enforcement and Chilling Effects in Medicaid Participation. *American Economic Journal: Economic Policy 6*(3), 313–338. Publisher: American Economic Association.

Wells, J. C. (2000). Natural Selection and Sex Differences in Morbidity and Mortality in Early Life. *Journal of Theoretical Biology 202*(1), 65–76.

Whitehead, N. and L. Lipscomb (2003). Patterns of Alcohol Use Before and During Pregnancy and the Risk of Small-for-gestational-age Birth. *American Journal of Epidemiology 158*(7), 654–662.

WHO (2004). *Global Status Report on Alcohol 2004*. Geneva, World Health Organization,.

Wilkinson, C., M. Livingston, and R. Room (2016). Impacts of Changes to Trading Hours of Liquor Licences on Alcohol-related Harm: A Systematic Review 2005-2015. *Public Health Research & Practice 26*(4).

Williams, J., R. L. Pacula, F. J. Chaloupka, and H. Wechsler (2004). Alcohol and Marijuana Use Among College Students: Economic Complements or Substitutes? *Health Economics 13*(9), 825–843.

Woods, S. M., J. L. Melville, Y. Guo, M.-Y. Fan, and A. Gavin (2010). Psychosocial Stress During Pregnancy. *American Journal of Obstetrics and Gynecology 202*(1), 61.e1–7.

Yang, S., A. Decker, and M. S. Kramer (2013). Exposure to Parental Smoking and Child Growth and Development: A Cohort Study. *BMC pediatrics 13*, 104.

Yörük, B. K. and C. E. Yörük (2011). The impact of Minimum Legal Drinking Age Laws on Alcohol Consumption, Smoking, and Marijuana Use: Evidence from a Regression Discontinuity Design Using Exact Date of Birth. *Journal of Health Economics 30*(4), 740–752.

Zhang, N. (2010). Alcohol Taxes and Birth Outcomes. *International Journal of Environmental Research and Public Health 7*(5), 1901–1912.

Biography

Romina Tomé was born and raised in Trenque Lauquen, Argentina. She attended the Universidad Nacional de La Plata for her undergraduate studies where she graduated with *highest honors* and earned a BA in Economics in 2010. She also earned a MA in Economics from the same university in 2012 that was financed by a fellowship obtained based on his academic performance. She received her Ph.D. in Public Policy from the Sanford School of Public Policy at Duke University in 2020. She also holds an MA in Economics from Duke University. While studying at Duke, she has been awarded the Fleishman Ph.D. Fellowship and the Brown-Nagin Graduate Fellowship. Her broad research interests include population economics and development economics. Within these fields, her work has focused on human capital, early-life shocks, and policy evaluation.

Before coming to Duke, Romina held research and academic positions at the Inter-American Development Bank, the Argentinean Government, the American Institutes for Research, and the Universidad Católica de La Plata. As part of this journey, she coauthored policy briefs and a book about childcare quality in Latin America. After graduation, she will join the American Institutes for Research as an International Development Economist in Washington DC.