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Hospital Quality, Patient Distance Traveled, and Traffic Fatality

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ABSTRACT

HOSPITAL QUALITY, PATIENT DISTANCE TRAVELED, AND TRAFFIC FATALITY

By

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This dissertation examines policy trade-offs and interventions for two different populations: rural mothers and drivers. Each essay uses a different econometric approach, such as simulation or quasi-experimental econometrics design, to identify trade-offs faced by rural mothers or causal impacts of policy interventions on drivers' behaviors.

The first chapter examines the trade-offs US rural mothers face between hospital quality, and distance traveled to deliver their babies. Over 18 million women of reproductive age live in rural areas. They face substantial difficulties in accessing quality medical care. Thirty-five percent of all US counties have no hospital providing obstetric care. The rural hospital closure phenomenon exacerbates the shortage in maternity care provision, with 181 hospitals closed in rural areas since 2005. One in every ten individuals drives over 100 miles for maternity care. Additionally, rural patients complain about the quality of their community hospitals. As such, many bypass their local hospitals and drive longer distance to reach better quality hospitals. In this chapter, I examine the distance-quality trade-offs faced by rural mothers and how it differs by age, race, education, risk level, and types of insurance coverage. I use the Vital Statistics (birth records) and the American Hospital Association annual surveys over the period 2007-2017 for a total of over 113 million hospital-births matched observations. This chapter helps one evaluate who is harmed and benefited from policy interventions to subsidize, open, or close various types of health facilities, including Neonate Intensive Care Unit (NICU), teaching, public, private, critical access, and sole community hospitals.

In the second chapter, Nicholas Wright and I analyze the causal impacts of handheld devices legislation on traffic fatality. Nowadays, people tap, swipe, and click on their mobile phones more than ever before, with an average of 2,617 clicks per day. There is evidence that 56% of individuals make phone calls, more than 28% utilize social media applications, and about 12% read text messages or emails while operating a vehicle. Using a cell phone while driving increases the risk of an accident five-fold and the total cost of distracted driving on society in 2010 alone was estimated at $123 billion. Policymakers are concerned about this public health crisis. More than 20 states passed regulations that prohibit the use of handheld wireless communication devices, texting, dialing, or emailing while operating a vehicle (handheld bans). This chapter examines the effects of this policy on traffic fatality using the Fatality Analysis Reporting System (FARS) data over the period 2000-2015 and several quasi-experimental econometric approaches, such as Regression Discontinuity and Difference-in-Differences. This chapter has value for at least two reasons: 1) distracted driving is one of the leading causes of traffic injuries and fatalities in the United States; 2) while the number of states with handheld bans have tripled over the last decade, empirical studies have yielded mixed results.

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

To my auntie Rozette Milien and uncle Paul Vilfranche Milien who sent me to school and continued taking care of me after my parents' death at the age of six. This accomplishment would not have been possible without your love and unwavering support. I hope this journey inspires many, including my wife and my daughter.

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Chapter 1

Distance and Quality Trade-Off

1.1 Introduction

In the United States, mothers living in rural areas face substantial difficulties in accessing quality medical care (Douthit et al., 2015, Kozhimannil et al., 2019). Numerous clinical factors such as rising costs of care, low Medicaid reimbursement rates, and competition from urban hospitals adversely impact rural health facilities and the quality of services provided (Roh, Lee, and Fottler, 2008). These conditions, among others, led to the closure of 181 hospitals in rural areas since 2005 (Cecil, 2020). Over 10% of rural women drive more than 100 miles for maternity care (CMS, 2018). Moreover, some patients who access rural hospitals complain about the low quality care and poor reputation of rural hospitals (Taylor and Cosenza, 1999; Liu, Bellamy, and McCormick, 2007). As a result, many rural patients bypass their local facilities for more distant and better quality hospitals (Bronstein and Morrisey, 1991, Premkumar, Jones, and Orazem, 2016).

In this paper, I examine the distance-quality trade-off faced by rural mothers and how it differs by age, race, education, risk level, and types of insurance coverage. This issue matters because 35% of all US counties have no hospital providing obstetric services (Dimes, 2018), and women living in communities with obstetric care shortages have a higher proportion of delivery complications, higher rates of premature births, and greater neonatal care costs than other women (Nesbitt et al., 1990). Longer distances traveled are associated with a reduction in neonatal care utilization and increased risk of neonatal mortality (Målqvist et al., 2010), higher rates of c-section and neonatal hypoglycemia (Robbins et al., 2019), and higher rates of adverse perinatal outcomes (Grzybowski, Stoll, and Kornelsen, 2011, Ravelli et al., 2011).

In a context of obstetric care shortages and hospital closures, access to care is crucial from a policy perspective. The study helps one think about policy trade-offs and interventions, especially by evaluating the willingness to travel additional miles for better hospital quality for different sub-groups of the population. The paper helps one evaluate who is harmed and benefited from policy

interventions to subsidize, open, or close various types of health facilities, including Neonate Intensive Care Unit (NICU), teaching, public, private, critical access, and sole community hospitals.

The theoretical framework of this paper relies on the random utility theory, which posits that mothers will choose the hospital that maximizes their satisfaction considering travel distance and hospital quality. The paper makes the following contributions. First, to my knowledge, it is the first paper to develop a probabilistic approach to evaluate the trade-off between travel distance and hospital quality. Second, I exploit two large datasets (the universe of the birth records in the United States and the near-universe of hospitals) to study the population of rural mothers that was not studied before. The prior literature is mostly focused on Medicare patients (Adams et al., 1991, Tay, 2003). Third, this paper is a national study, while the few papers that investigate this issue are state-specific (Luft et al., 1990, Premkumar, Jones, and Orazem, 2016).

To estimate the trade-off between hospital quality and distance, I use the 2007-2017 Vital Statistics Data Files (birth records) and the American Hospital Association (AHA) annual surveys [1]. Because the restricted version of the birth records that I use do not include the address and the hospital of birth of the patient, I use the centroids of the county of residence and birth occurrence for the analysis. I develop a three-step method to investigate the trade-off of interest. First, I create a dataset that enables me to identify all the counties within a radius of 50 miles of each specific county. I create a ”ghost” county for all the counties whose centroid is outside of the 50 miles. I consider 50 miles as the defined market area after several considerations such as distance traveled distributions and Federal regulations related to the establishment of small rural hospitals such as Critical Access Hospitals (CAH)[2] and Sole Community Hospitals (SCH)[3]. Second, I merge this dataset with the natality files using the county of residence. This step enables me to observe all the potential counties where the individual could give birth (within the 50 miles radius and outside the 50 miles radius through the ”ghost” county). Finally, I merge this dataset with the hospital records to match in hospital characteristics. For the ”ghost” county, I create a single ”ghost”

[1] Our main limitation, a precision concern, comes from the Vital Statistics data that only reports the county of birth and not the birth hospital. One particular strength of the paper is combining two large datasets.

[2] Rural hospitals of no more than 25 beds located more than 35 miles away from another hospital.

[3] Rural hospitals of no more than 50 beds located up to 50 miles away from another hospital.

hospital to which I impute the average hospital characteristics. This yields a total of 113,488,826 birth-hospital matched observations for a total of 6,039,936 rural residing mothers who gave birth between 2007 and 2017. The average mother has 18 hospitals in her choice set, plus the ghost county.

The restricted Vital Statistics data only reports the county of birth and not the actual hospital that the mother chose. If I knew the selected hospital, then a simple conditional logit (fixed-effects logit model) would suffice to estimate the model. To address this issue, I use a maximum likelihood estimation procedure that aggregates the probability of choosing any hospital within a county as the sum of the probabilities of selecting each hospital within the county. During 2007-2017, 55% of all rural counties had only one hospital, and for mothers choosing such a county, the probability of that county is the same as a conditional logit formulation.

The numerical estimation of the maximum likelihood is identified by variations in hospital features (size, demand, cost, number of maternity care providers with admitting privileges[4], number of registered nurses, etc.), community characteristics (economic conditions, Medicaid expansion, obstetric reimbursement rates, Certificate of Need Laws, immigrant childbirth-friendly policies), and choice set features (bypass phenomenon i.e., going beyond the defined market area). Given that this is a within-individual framework, all factors that stay constant within individuals across hospitals (e.g., age, education, etc.) will not impact the choice model unless they are interacted with hospital characteristics. This random utility framework models utilities that capture the desirability of hospital alternatives. It is rooted in the utility maximization theory, and this allows me to use a compensatory or trade-off interpretation for the estimated impacts of the explanatory variables.

Specifically, to estimate the trade-off of interest, I apply spherical distances[5] to proxy for road

[4]The maternity care providers with admitting privileges are obstetricians and gynecologists, geriatrics, family practice doctors, general internal doctors, and general practitioners who can admit patients to specific hospitals. In rural communities, those doctors are generally the ones who provide obstetric care (Dimes, 2018).

[5]Ideally, I would need the actual time traveled by a given rural mother. To get this metric, I would need the total distance traveled (in miles) divided by the speed (in miles per hour). The exact addresses of patients and hospitals and the speed (in miles per hour) are unknown in the data. Therefore, I use the spherical distance measure as a proxy for actual travel times.

distance or actual travel times. Spherical distances characterize the shortest distance between two points (the centroid of the county of residence and a potential birth occurrence county). Given that hospital quality is a multifaceted object, I employ several different measures of quality such as hospital accreditation by the Joint Commission[6], an indicator for Neonate Intensive Care Units (NICU), number of obstetric beds and bassinets (beds for babies), hospital teaching status, and indicators for public and non-profit hospitals. These hospital quality indicators refer to the process of care, unlike other hospital quality metrics used in the literature, such as mortality rates and readmission rates. These latter pose selection issues and necessitate an adjustment in differences in case-mix, which is controversial (Jencks, Brook, Iezzoni, et al., 1987).

The results show that all the race groups such as Blacks, Hispanics, Whites, and other-race (e.g., Asian) dislike traveling far to give birth. Highly educated, married, and young mothers would be willing to travel longer distances for additional obstetric beds than their respective counterparts. The findings for highly educated and married are consistent with our expectations considering that these individuals generally have higher household incomes than their counterparts, and longer distances are synonymous with higher out-of-pocket transportation costs for a rural pregnant mother. Moreover, uninsured individuals show greater willingness to travel additional miles for bassinets (beds for babies) than other insurance coverage groups.

The results also find that rural mothers are willing to travel further to higher-quality hospitals as measured by obstetric beds, bassinets, hospitals accredited by the Joint Commission, NICU (especially high-risk[7] mothers), public and non-profit hospitals. A high-risk patient, as defined here, is a mother that is 35 years and older and has had at least one previous c-section, or that is 44 years and older, or that has a current plural birth (e.g., twins, triplets). I use another risk definition that also includes patients below 17 years and patients that have pre-pregnancy risk factors following Bladder, 2000 and Mayo-Clinic, 2020. While low-risk rural mothers prefer the characteristics of non-NICU over NICU hospitals, high-risk mothers are willing to travel 3.08 (or

[6]Founded in 1951, the Joint commission is the oldest and the reference in terms of hospital quality and safety standards. The accreditation is not necessarily specific to maternity care.

[7]High-risk for birth complications. For simplicity, I only have one risk group.

9.5% relative to the mean) more miles for any hospitals with NICU. Hence, hospitals with NICU may represent an emergency plan for high-risk mothers.

The results also suggest that rural mothers are less likely to deliver their babies at teaching hospitals. They are willing to drive several miles away, or to accept a disamenity or a cost factor, to avoid going to major teaching hospitals. This finding is consistent for all the age categories, racial groups, education levels, marital statuses, and insurance coverage groups. As such, rural mothers are less likely to go to teaching hospitals for delivery purposes. Albeit surprising, there are at least two reasons for this finding. First, very few rural providers can directly admit patients in teaching hospitals; this reduces the likelihood that rural patients will choose those institutions for delivery. Second, some patients are less likely to choose teaching hospitals because of the fear associated to the fact that the attending physician may be a resident and not a senior doctor (Mishori, 2003).

Furthermore, the results show that rural mothers value hospitals accredited by the Joint Commission. Regarding hospital ownership, rural mothers value the characteristics of both public and non-public hospitals more than they value private for-profit hospitals. However, patients dislike small community medical facilities such as critical access and sole community hospitals[8]. Overall, the results are consistent across all age categories, racial groups, education levels, and insurance coverage groups. The study's main conclusions are robust to several specification checks, including performing a choice set expansion and contraction of radius of several miles, removing transfer patients, using lagged hospital quality measures, and adjusting the definition of risk high-patients.

Our results are directly relevant to policy debates on 1) the necessity to invest in the expansion of health care facilities in rural areas and 2) the necessity to invest in improving the quality of care provided in rural communities. Both of these debates pertain also to Certificate of Need Laws (CON Laws). These policy recommendations can have tremendous impacts on the 18 million women of reproductive age living in rural areas.

The remainder of the study is organized in this order. Section II presents the literature review,

[8]Critical Access and Sole Community Hospitals represent over 41% of all the hospitals closed in rural areas since 2005.

section III covers the conceptual framework, section IV describes the data sources. Last, sections V to VIII exhibit respectively the model specification, the empirical results, the robustness checks, and a discussion of the findings.

1.2 Literature review

1.2.1 Hospital Choice Process

Understanding patients' hospital choices requires at least some comprehension of individuals' decisions to seek care in the first place. Andersen et al., 1968 lays the underpinning of individual behavioral processes underlying the hospital choice model. While the Andersen model is not directly related to hospital choice, it provides a suitable mechanism to describe an individual's decision to seek care. Andersen et al., 1968 prescribes the following three sets of determining factors to a person's decision to seek medical care: 1) medical needs factors such as the perceived health status of an individual, the nature of his/ her medical conditions; 2) predisposing factors (age, education, and race) that affect an individual's marginal tendency toward seeking medical care; and 3) enabling factors to determine individual access to care such as income, insurance coverage, physicians' access, etc... As for choosing a specific hospital, the study by Porell and Adams, 1995 claims that there is little consensus regarding the step-by-step process of determining how individuals choose a particular hospital. This study helps to fill this gap.

In general, a patient chooses a doctor who has admitting privileges in some set of hospitals. Considering the person's insurance and possible complications, the doctor decides on which hospital to treat the patient. In some cases[9], the treating physician may have affiliations to several hospitals. Garnick et al., 1987 and Luft et al., 1990 argue that diagnosing physicians dominate the hospital selection decision for patients, while studies by McGuirk and Porell, 1984 and Morrisey, Sloan, and Valvona, 1988 assert that patients play the critical role in terms of hospital selection after evaluating distance traveled, perceived quality of the hospital, as well as physicians affiliations

[9]In other cases, a patient chooses a given hospital and finds a doctor that has affiliations in that specific hospital. This is more so for patients who have knowledge about the US health system (Trisha, 2020).

to specific hospitals. To this, Porell and Adams, 1995 emphasizes that it doesn't matter whether patients or physicians make the decision, as long as the relative preferences are expressed in terms of hospital characteristics, distance, quality, and price. The true preferences should be reflected in systematic hospital choice patterns, although particular constraints might hinder some of that.

The hospital choice is often made one or several months before childbirth. In most cases, childbirth is a non-emergency care event, where a mother, her physician, and her lay network (family and friends) prepare for the birth event. In these cases, the mother delivers the baby in a hospital that was already chosen by the physician considering the patient's or partner's insurance, the patient's preferences, and the physician's preferences and hospitals' affiliations. However, in some cases, labor can be unexpected, which may constrain women's choices regarding the hospital where they deliver. Under the Emergency Medical Treatment & Labor Act, signed by Congress in 1986, hospitals are required to provide emergency care to patients regardless of their ability to pay. So, in emergency situations, the mother is more likely to go to the nearest hospital.

1.2.2 Hospital Choice and Distance

The earliest strand of the hospital choice literature focused on distance decay, which emphasized that more considerable distances tend to decrease hospital utilization. The ”distance decay” literature is also in line with the first law of geography, according to which near things have more association than things that are remotely dispersed. In general, the model fitted to assess the spatial patterns in hospital utilization was a form of a negative exponential function of distance (Morrill and Earickson, 1968, Morrill, Earickson, and Rees, 1970). An essential finding of this literature was that distance-utilization elasticity was much lower for larger hospitals than smaller hospitals.

Other studies in this literature performed a spatial analysis model used in geography and social physics to predict the flows of hospital admissions based on the economic sizes and distance between the community of residence and hospital localization. Known as the gravity model, its hypothesis postulates that the greater the distance between two points in space, the smaller the spatial interaction between these points. According to the social gravity model, the flow of patients

living in a given community going to a hospital in this area is a positive function of the population mass of the community, a positive function of the hospital's capacity, and a negative function of the patients' travel time.

Another group of studies in the hospital choice literature uses the McFadden (1974) conditional logit model or some other forms of Random Utility Model (RUM). Specifically, the conditional logit choice model used in these studies estimates the probability that a patient chooses a given hospital. Usually, the utility function to calculate this probability represents a linear function of individual characteristics such as age, race, gender, etc., and features of different hospital alternatives. In general, this literature considers distance as a disamenity or a cost factor that brings disutility to the individuals (Luft et al., 1990, Tay, 2003, Chandra et al., 2016). The negative effect of distance on demand is large (Tay, 2003) and it is associated with a significant reduction in the likelihood that a hospital will be chosen. Other non-RUM studies find that increased travel time is associated with reductions in the likelihood of having a checkup by minority children (Currie and Reagan, 2003) while Buchmueller, Jacobson, and Wold, 2006 show that increased distance to the nearest hospital is associated with higher mortality and injuries.

Overall, conventional wisdom, as established by the distance decay approach, gravity model, and Random Utility Models, supports the idea of a strong negative relationship between patient distance traveled and hospital choice. Simply put, distance plays a significant role in the hospital selection process of a patient.

1.2.3 Hospital Choice and Hospital Quality

The literature on hospital quality and quality of care suggests numerous possible dimensions and criteria for defining hospital quality and quality of medical care. A seminal paper, Donabedian, 1966, like Lee, 1933, defines quality as value judgments about the several aspects and dimensions of a process called medical care. Another early and influential study, Klein et al., 1961, explains that the notion of patient care is not a unitary concept and that there will always be an array of criteria to measure the quality of patient care. Even more recently, Chandra et al., 2016 supports the

notion that hospital quality is a multi-dimensional object that is a combination of hospitals' ability to produce good health outcomes, patients' beliefs regarding hospitals' ability to produce good health outcomes, and patients' satisfaction from past experiences. Consequently, conventional wisdom regards hospital quality as a non-unitary and multi-dimensional concept.

A review of the hospital quality literature shows that studies generally use three types of approaches to evaluate hospital quality: patient surveys, hospital input, and hospital output. Patient satisfaction surveys measure patients' experiences of their hospital care. Jencks, Brook, Iezzoni, et al., 1987 argues that there are at least two issues with this approach: 1) there is no consensus on the patient attributes to use to compare hospitals, and 2) the necessity to control for cultural factors such as the person's first language.

Studies that used the outcome approach to gauge hospital quality[10] tend to utilize variables such as perinatal mortality (NYAM, 1955; Shapiro et al., 1960) and surgical fatality rates (Lipworth, Lee, and Morris, 1963). More recent studies also relied on outcomes such as risk-adjusted 30-day survival rates (Chandra et al., 2016), risk-adjusted readmission rates as a proxy for medical errors and inappropriate discharge (Chandra et al., 2016; Jencks, Williams, and Coleman, 2009, Axon and Williams, 2011), as well as severity-adjusted mortality and complication rates (Luft et al., 1990).

Donabedian, 1966 explains that there are many drawbacks to using outcomes as a dimension of quality of care. One concern is whether the outcome utilized is the relevant measure. The survival rate in a non-fatal situation is a scenario where the measured criterion to assess medical care may not be relevant. The study further argues that even in a context where the outcomes chosen to evaluate hospital quality are relevant, several other limitations must be reckoned with. First, other medical service determinants may affect the outcome. Second, sometimes it may take decades

[10] Given that the quality measures have not been standardized, several organizations such as the Centers for Medicare and Medicaid Services (CMS), the American Hospital Association, the Joint Commission on Accreditation of Healthcare Organizations (JCAHO), and many other partners have joined forces to create the Hospital Quality Alliance (HQA). Under this joint effort, hospital facilities accept to provide the CMS indicators of quality of care on several conditions such as acute myocardial infarction, congestive heart failure, and pneumonia (Jha et al., 2005). Since November 2004, when HQA data became available, many studies have analyzed patient's perceptions of hospital care.

before relevant effects are present. Thirdly, often times, medical care measures are not clearly defined and can be difficult to compare.

Tay, 2003 also argues that there are several problems with patient outcomes as quality indicators. Patient outcomes are noisy, especially for low-patient volume hospitals. There may also be a selection bias problem where better hospitals attract the patients with the worse health outcomes. Patients tend to sort across hospitals as a function of the severity of their illnesses. As such, the bias dimension is heterogeneous in the severity of the disease, with severely ill patients seeking hospitals that provide more intensive treatments. Tay, 2002 also explains that this bias may lead to systematic differences in patient health outcomes across hospitals. If lower quality rural hospitals tend to only attract and admit less ill and low-risk patients, the quality of care they actually produce is likely to be lower than that implied by the average mortality or readmission rates of their patients.

Jencks, Brook, Iezzoni, et al., 1987 argues that the outcome approach to defining hospital quality relies on case-mix measurement and adjustment; this refers to controlling for factors such as patient's age, sex, diagnosis-related group (DRG) mix, and the severity of patients' illnesses and/or functional status. It can be quite controversial. Jencks, Brook, Iezzoni, et al., 1987 reports that case-mix differences account for 44 to 50% of interhospital variation in mortality associated with hospitalization. The study argues that no amount of case-mix or illness severity adjustment will compensate for the limitations imposed by outcomes measures of hospital quality. Using variables such as mortality rates, readmission rates, or even risk-adjusted survival rates as hospital quality measures are likely to be biased by factors such as triage, recruitment of easy cases, and transfer of severely ill patients.

Contrary to the output approach, the input framework refers to the process of care and the setting in which it takes place (Donabedian, 1966). This method considers the size, structure, administrative processes, adequacy of facilities and equipment, the personal and medical staff's competence level, etc. By and large, this framework relies on the assumption that if a hospital operates under the proper settings and instrumentalities, the provision of quality care will be

automatic. Donabedian, 1966 argues that one limitation of this approach is that the association between structure and process or structure and outcome is not clearly established. Poor leadership or governance with high-quality inputs, for example, may produce poor outcomes.

1.2.4 Quality Metrics Used In This Paper

This study employs the input approach to measure hospital quality for three reasons. First, a national comprehensive[11] patient satisfaction survey that investigates patients' past maternity experiences is not available for my study period. Second, the output approach is contaminated by selection issues and is quite controversial (Jencks, Brook, Iezzoni, et al., 1987). As such, my quality measures are related to the process of care or the settings in which medical care is provided.

My first quality metric is a dummy variable that captures whether a potential hospital of birth provides Neonate Intensive Care Unit (NICU) services. Given the possibility of unexpected factors such as prematurity[12], Respiratory Distress Syndrome (RDS) [13], infection[14], hypoglycemia[15], maternal chorioamnionitis [16], and even re-admission, expectant mothers generally value hospitals that also provide intensive care. For infants born in a non-NICU setting, when there is an emergency that requires the skills and specificities of a NICU, the newborns are transferred to a NICU. Hence, there may be additional stress for the mothers related to the process of moving the child to a hospital with NICU. Consequently, the existence of a NICU in a hospital facility may change patients' perceptions regarding the hospitals' ability to produce good pregnancy and neonatal care. Existing literature suggests that parents value a NICU's presence in a health facility (Cleveland, 2008).

My second quality measure accounts separately for the number of obstetric beds and bassinets

[11]The closest to what I would need is the Hospital Consumer Assessment of Healthcare Providers and Systems Survey (HCAHPS), developed by Centers for Medicare & Medicaid Services (CMS) and the Agency for Healthcare Research and Quality (AHRQ). The questions are not specifically related to maternity care experiences.

[12]Prematurity is when the baby is born too early (less than 37 weeks of gestation).

[13]RDS is a prevalent respiration issue in babies that is due to immature lungs. Oxygen machines, breathing tubes, or ventilators are generally needed to help infants overcome this respiratory problem.

[14]Infection or sepsis is a widespread cause of neonatal death.

[15]Low blood sugar level

[16]Maternal inflammation of the placenta or the umbilical cord that increases the risk of the baby to have an infection.

(beds for babies) in a potential hospital of birth. This metric is a proxy for the health facility's size, which is generally perceived as a signal of quality by patients (Boscarino, 1988). My third quality measure is an indicator for whether a potential hospital of birth receives an accreditation from the Joint Commission. In general, Medicare-participating hospitals need to follow a set of Conditions of Participation (CoP) rules, which guarantee a minimum safety level. Under the CoPs, the requirements are authenticated through accreditation and certification (Moffett, Morgan, and Ashton, 2005). But, specifically, for non-emergency care such as childbirth, where a mother and her lay network (family and friends) tend to do online shopping for a hospital to deliver[17], it is likely that an accredited hospital will be perceived as a signal of good quality by expectant mothers. Besides, if the choice was made by a physician that has admitting privileges in a given hospital, it is also likely that this facility is accredited.

Finally, the fourth quality measure captures patients' valuation of teaching hospitals. These facilities have the reputation of highly specialized (Levin, Moy, and Griner, 2000) and high-quality care (Boscarino, 1992). They also tend to use cutting-edge technology to do state-of-the-art research to develop new treatments and cures. They have a wide range of interns and residents and are generally affiliated with a medical school, which can also increase patients' perception about their ability to provide good health outcomes. Consequently, patients are likely to value them as producers of good quality medical care when choosing.

The four measures of quality considered in this study capture different aspects of quality of care. In general, they represent a mixture of a hospital setting and its ability to produce good medical services. They are likely to influence patients' beliefs about hospitals' capabilities to have good health outcomes.

[17] Anhang Price et al., 2014 provides suggestive evidence that patients consult hospital rankings a year before their visits.

1.3 Conceptual Framework

1.3.1 Theoretical Model

This section presents a basic utility framework that relies on the Utility Maximization Theory and the idea that the expectant mother chooses the hospital for delivery to maximize utility. Each patient faces a choice among $h = 1, 2, ..., H$ hospital alternatives and derives satisfaction from the medical care received from each possible hospital choice. A mother may have perceptions about the quality of a hospital, attitudes regarding the importance of the quality, preferences among specific hospital alternatives, and a protocol to maximize preferences considering the direct out-of-pocket transportation costs and the opportunity cost of travel time (McFadden, 1986). Specifically, the model is built under the following assumptions:

1. Expectant mothers have preferences over various hospital alternatives and will choose the one that maximizes their satisfaction.

2. Expectant mothers' incomes and time are limited. They face a budget constraint[18] and a time constraint.

3. A mother's choice set contains several hospital alternatives.

4. Mothers form beliefs about the quality of care that they are likely to receive in each possible hospital alternatives.

5. Mothers are aware of the distance in miles to travel from their residence to a given hospital choice.

6. Whether the mother or a referring physician chooses the hospital for the patient does not really matter[19]. When physicians choose, they take mothers' preferences into account in

[18]The money constraint may not be significant for most rural mothers because Medicaid/CHIP covers births for women up to some high-income levels depending on the state and its Medicaid expansion status. Also, emergency Medicaid pays for labor and delivery for non-citizens who do not otherwise qualify for Medicaid.

[19]Almost all papers in the hospital choice literature make this assumption. Porell and Adams, 1995 adds it doesn't matter whether patients or physicians make the decision.

their hospital choice process.

7. A mother's place of residence is exogenous, meaning that mothers do not self-select their place of residence due to obstetric care providers in a community.

Given that every obstetric service provided by the chosen hospital has a price tag which depends on the person's insurance network, expectant mothers select the hospital that maximizes their expected utility across all alternatives considering their limited incomes. In general, patients formulate their demand for obstetrics care from a given hospital h based on their characteristics such as medical needs, perceived health status, predisposing factors (age, education, race, etc.), and enabling factors (income, insurance coverage, etc.).

A patient's utility can be expressed[20] as follows:

$$U_{ih} = V_{ih}(quality_h, distance_{ih}),$$

with i being the patient and h a particular alternative. The marginal utilities will depend on the functional form chosen to represent the mothers' preferences. As such, any monotonic transformation of utility is also going to affect the marginal utilities and its interpretation. For a more consistent interpretation, I will refer to the ratio of marginal utilities. For example, if I consider a change in the health care consumption bundle ($\Delta quality_h$, $\Delta distance_{ih}$) such that the mother is kept on the same indifference curve, then the change in utility resulting from an increase in quality must be exactly offset by the decrease in utility resulting in additional distance traveled. To the extent that,

$$MUquality_h * \Delta quality_h + MUdistance_{ih} * \Delta distance_{ih} = 0$$

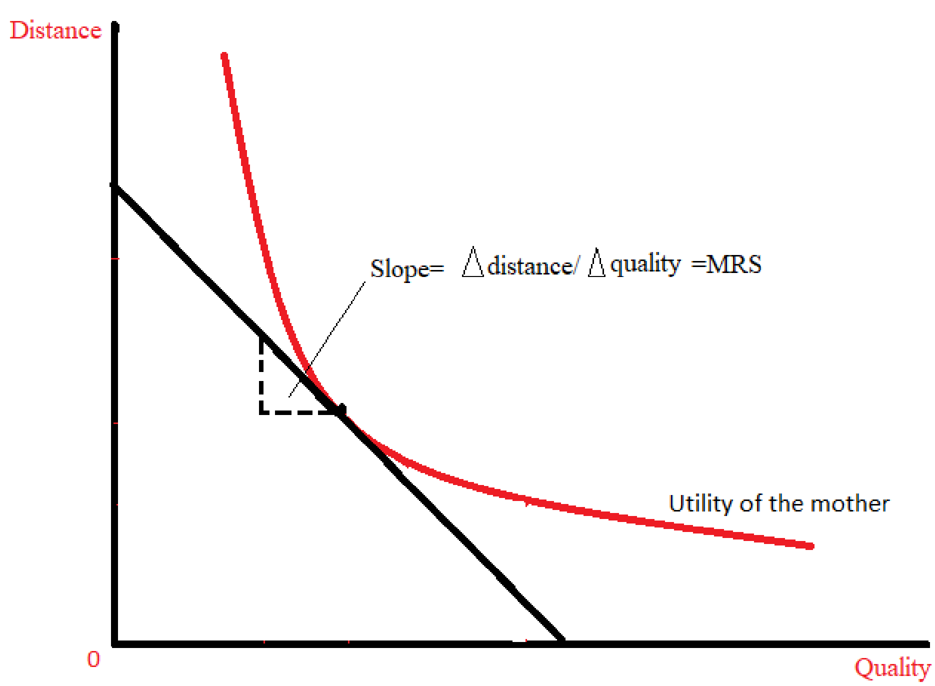
[20]For simplicity, in this conceptual framework, I only use quality and distance as determinants of the utility. However, in the model, I adopt a more comprehensive approach.

$\Rightarrow$ which is equivalent to:

$$MRS = \frac{\Delta distance_{ih}}{\Delta quality_h} = -\frac{MUquality_h}{MUdistance_{ih}}.$$

The ratio of the two marginal utilities is the Marginal Rate of Substitution (MRS) between quality and distance. It measures the rate at which the mother is willing to substitute distance for quality. Because the mother is on the same indifference curve (Figure 1), the disutility resulting from an increase in distance traveled is exactly offset by the utility resulting from an increase in quality. For example, in the case of obstetric beds as an indicator of quality, if the MRS=-2, the mother will be willing to give up 2 miles of distance for every 1 additional obstetric bed.

Figure 1: Indifference Curve



Notes: Author's analysis using the utility maximization theory. A mother's utility is a function of the distance traveled to the hospital and the quality of care provided by the hospital. The marginal rate of substitution (MRS) is the rate at which the mother is willing to substitute distance for quality.

The conceptual framework yields the following testable predictions:

1) **On average, mothers are less likely to choose hospitals that are further away, ceteris paribus.**

2) **On average, mothers will choose hospitals with better quality, ceteris paribus.**

1.3.2 Hospitals Mostly Found in Rural Areas

Critical Access Hospitals (CAH)

The Centers for Medicare and Medicaid Services designate Medicare-participating hospital facilities as Critical Access Hospitals if they meet specific criteria, such as being located in a rural area of either more than 35-miles from other hospitals, reporting no more than 25 beds (either inpatient or swing beds), maintaining an average length of stay of 96 hours or less for acute care patients, and providing 24/7 emergency services. Those hospitals account for two-thirds of all rural hospitals and may be the first access to care for many rural areas mothers.

Sole Community Hospitals (SCH)

In 1983, Congress created the SCH program to bolster rural hospitals and provide care to individuals living in very remote areas where travel, weather conditions, and the absence of health facilities may represent a significant barrier to access care. According to the Title 42 of the 1983 Federal Regulations, to be designated as SCH, a hospital must either be more than 50 miles away from other hospitals or less than 50 miles but was not accessible to patients due to some topographical or weather conditions for an extended period. Since topography is not uniformly distributed in the country, Sole Community Hospitals represent a source of care that some mothers can acquire at a reasonable distance.

Public and Private For-Profit Hospitals

Public hospitals are grouped into Federal (e.g., Navy Hospital) and non-Federal Hospitals (e.g., state Hospital, Hospital district). Those health facilities are generally either partly or fully funded by public funds. The majority of those institutions accept nearly everyone, regardless of insurance status. While public hospitals are usually more affordable and larger, private or for-profit hospitals tend to provide more personalized care due to doctors and nurses overseeing fewer patients per

person.

Non-Profit Hospitals

Non-Profit Hospitals are charity institutions that do not pay taxes in exchange for meaningful contributions to their communities. They are generally less expensive than investor-owned for-profit institutions. During 2007-2017, about 50% of US hospitals are non-profit institutions.

1.4 Data

1.4.1 Data Sources and Manipulation

To evaluate the trade-off between hospital quality and distance, I use the following two data sources: the 2007-2017 U.S. Linked live birth and infant death certificates data and the 2007-2017 American Hospital Association (AHA) Annual Survey data. To compute the distance[21] measures, I obtain latitude and longitude coordinates for residential and birth occurrence counties from several commercially available geographic files. I identify rural mothers using county FIPS codes and the rural definition adopted by the Office of Management and Budget (OMB). I consider the 2013 Rural-Urban Continuum Codes of four or more [22] as being rural.

I make use of the natality and period linked birth-infant-death data from the National Center for Health Statistics for mothers' information. This is an administrative data set that provides birth certificates for all births in the United States. It is the best available source of information for a national study on a mother's choice of birthplace analysis and is widely utilized in the maternal and infant health literature. While the dataset identifies the county of residence and birth occurrence[23],

[21] I use spherical distances, which represent the shortest distance between two points. These measures are calculated using the haversine law and are correct to within about 0.5%(NAVY, 1987). Ideally, I would want to use the actual distance traveled by the patient. Due to data limitation, I use this spherical distance measure as a proxy.

[22] The 2013 Rural-Urban Continuum Codes use the degree of urbanization and adjacency to a metro area to classify non-metropolitan counties (Economic Research Service, U.S Department of Agriculture). The official classification scheme is made of three metro and six non-metro categories.

[23] For about 30% of women, the county of residence and birth occurrence is different. There are mainly three reasons why this may be the case. First, it may be that the county of living has never had a hospital, whether providing obstetric care or not (March of Dimes (2018)). Second, it may also be the case that the county of residence had one or several hospitals closed prior to the birth occurrence period. Finally, it may happen that the women decided to bypass their county hospitals to go to another county, as it is often the case in rural areas (White and Morrisey, 1998).

the restricted version of the data that I use does not have mothers' and hospitals' addresses. Given that the natality data does not provide information about the hospital of birth, I use the American Hospital Association (AHA) annual surveys to link the mothers to a potential hospital[24]. The AHA is a survey of the complete universe of U.S. hospitals with a 80% response rate. For non-responding hospitals[25], the AHA uses an estimation process to impute missing statistical values. This survey represents one of the most comprehensive and credible sources of information about hospital facilities and is widely used in the hospital literature. This data provides the complete address of nearly all hospitals in the U.S. (whether they answer the survey for a particular year or not).

Besides the natality and AHA data, I create a separate dataset that enables me to identify all the counties whose geographic center are within a radius of 50 miles of the centroid of each specific county. A "ghost" county[26] was also created for all the counties outside of the 50 miles defined market area. This dataset was then merged with the natality files using the county of residence. Upon merging, for each individual, I am able to observe all the potential counties where the individual could give birth (within the 50 miles radius plus outside the radius through the ghost county). I can identify the actual county of birth of occurrence from the natality data, along with the other potential counties where the birth could have occurred. I use 50 miles as my defined market area for the hospital choice framework after several considerations such as distance traveled distributions, and Federal Regulations[27] related to the establishment of community hospitals such as Sole Community Hospital (SCH) and Critical Access Hospital (CAH). Below, I also use several other market areas to corroborate my results. The data shows that traveling across the entire continent to give birth is a rare event[28].

[24]In rural areas, over the study period, there are 98% hospital births, 1.5% home births, and 0.50% births coming from freestanding birth center. So, a non-hospital birth is a rare event. In a specification check, I kept only the hospital births and the results did not change.

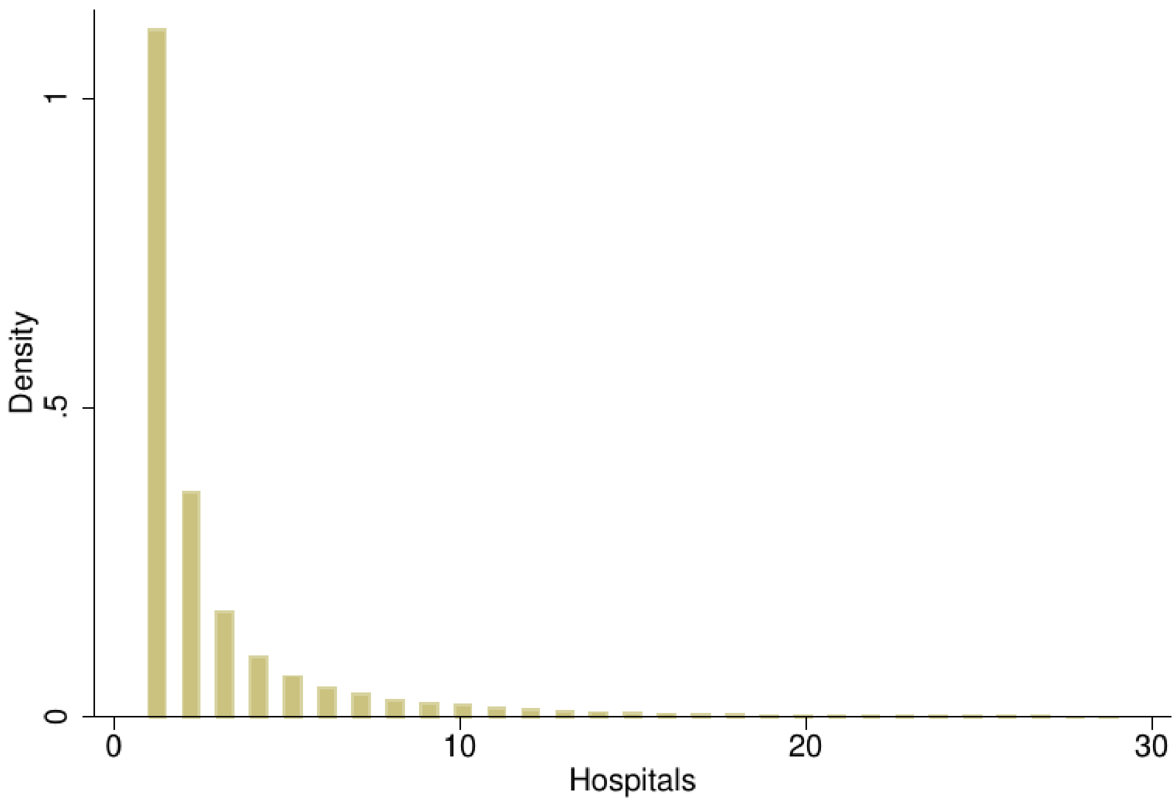
[25]Some responding hospitals have missing values for some of my indicators. See Appendix C for more details about how I deal with those missing values.

[26]The main purpose of the "ghost" county is to reduce computational burden.

[27]Critical Access Hospitals are created through the Balance Budget Act of 1997. They must be located more than 35 miles from another hospital. According to the Title 42 of the 1983 Federal Regulations, a Sole Community Hospital (SCH) must be about 50 miles away from other hospitals.

[28]I understand that a mother may go even beyond 1,000 miles if she has a condition that requires a specialist from

Figure 2: Distribution of the Number of Potential Hospitals per Individual in the County of Birth Occurrence



Notes: Author's analysis using the Vital Statistics (Birth Records) and the American Hospital Association annual surveys. This figure presents the distribution of the number of potential hospitals in the county of birth of the patients. It varies from 1 to 29 hospitals, with the majority of the counties having one hospital.

For each mother, I am able to identify all the potential counties where she could give birth, including the actual county of birth occurrence and the ”ghost” county. This allows me to close the choice set for each mother[29]. I then merge this data to the near universe of hospitals in the

a faraway county. Only 6.7% of the mothers in the dataset go outside of the defined market area of 50 miles to deliver their babies. Some expectant mothers may pick a hospital in a faraway county to deliver because 1) they don't like the characteristics of the hospitals in the market designated area; 2) they have a condition that requires them to see a specialist that maybe even 1000 miles away, or 3) because of some other reasons such as to be near other family members.

[29]In principle, I could allow every hospital to be in every woman's choice set. I do not do that because of computational burden, and simplifying the choice set helps avoid serious computation issues.

United States [30]. For the ”ghost” county, I create a single ”ghost” hospital to which I impute [31] the average hospital characteristics and differentiate it with a dummy variable indicating that it is more than 50 miles from the county of residence. I yield a total of 113,488,826 birth-hospital matched observations, for a total of 6,039,936 rural-residing mothers who gave birth between the period 2007 and 2017. The average number of hospitals in each mother’s choice set is 18 (Figure 2 shows the distribution of hospitals in the county of birth occurrence). Although I only select rural mothers for whom the trade-off between distance and quality is arguably a more interesting phenomenon, they could give birth in any urban county.

To evaluate the trade-off of interest, I employ spherical distances. They represent the shortest distance between the centroid of the county of residence and the centroid of a potential county of birth occurrence. This centroid-based approach (crow-flies) acts as a proxy for actual travel times [32].

I use the AHA survey data to create several hospital quality indicators [33]. I utilize the number of obstetric beds and the number of bassinets (sleeping baby’s beds) in a given hospital. I also consider a dummy variable that takes the value one if a given hospital has at least one Neonate Intensive Care Unit (NICU) bed and zero otherwise. Empirical evidence shows that mothers value NICU because of the possibility of taking care of the baby in case of early delivery, health problems, or the possibility of a difficult birth (Conner and Nelson, 1999). I use dummy variables to identify hospital teaching status, and an indicator that controls for any accreditation received by a given hospital. These multiple aspects of hospital quality enable me to assess which quality factors mothers value when considering a hospital for delivery.

[30] I merge with all types of hospitals and not only obstetric units. One could also suggest using observed demand patterns to create the choice set. One way to do the observed demand patterns approach is to find a benchmark in the literature or limit the choice set to hospitals where, say, at least ten births occurred the year before. The demand pattern approach has a built-in selection problem. Maybe, the other places with zero births were not chosen for delivery because they have low quality. Indeed, some of those places may not have obstetric units, but there is significant evidence of births in other places with no obstetric units. As such, the distance-based approach is better. At least, it solves the selection problem by including all hospitals within a given area, whether good or bad.

[31] Imputing the average in-state hospital characteristics changes very little.

[32] Especially in rural areas, road distances are highly correlated with the crow-flies. The centroid-based approach introduces several measurement errors. I discuss the measurement issues in section 7.6 below.

[33] See Appendix C for details about how I deal with missing values

The multiple aspects of hospital quality used in this paper are mostly related to the patient's perceptions of the hospital quality. These factors are likely to play a significant role in the patient's choice of a hospital. In that regard, Chandra et al., 2016 argues that hospital quality is a multidimensional object that is a combination of hospital capacity to produce good health outcomes, patients' beliefs about hospital ability to have good health outcomes, and patients' satisfaction from past experiences. Sixma et al., 1998 suggests that hospital quality, as seen through the patient's eyes, is of paramount importance in hospital choice.

One limitation of this study comes from the Vital Statistics data that only reports the county of birth and not the hospital of birth. A simple conditional logit (fixed-effect logit model) would suffice to estimate the model if the chosen hospital was known. Given this issue, I utilize a different maximum likelihood estimation framework. This approach aggregates the probability of choosing any hospital within a county as the sum of the probabilities of choosing each hospital within the county. During our study period, 55% of all rural counties have only one hospital. For mothers who choose a county with only one hospital, the likelihood function is equivalent to a conditional logit formulation.

1.4.2 Descriptive Statistics

Table 1 presents the descriptive statistics. Over the sample period 2007-2017, the study sample is comprised of 6,039,936 rural mothers for a total of 113,488,826 birth-hospital observations. This yields an average of 18 hospitals per individual located within 50 miles of the centroid of the county of residence. About 7% of the mothers go beyond the 50 miles to deliver their babies, with the majority being white [34]. Half of the rural mothers are between 25 and 34 years, 70% are white, 57% of those mothers are married, 62% of them have more than one child, and 50% of them have more than a high school degree. The average distance between the centroid of the county of

[34]Patients who go beyond 50 miles to deliver their babies are more likely to be patients who necessitate the care from a given specialist. They are also likely to be patients who searched online for specific maternity care providers that fit their preferences, high income, and highly educated patients. A study by Fox and Duggan, 2013 argues that white adults, individuals with households income of $75,000 or more, and those with a college degree or more are more likely than their counterparts to go online to figure out a possible diagnosis. White patients are also more likely to be high-income and highly educated relative to minority patients (Bishaw and Posey, 2016).

Table 1: Descriptive Statistics

	Mean	Standard Deviation
Distance and Quality Metrics		
Distance (in Miles)	32.24	14.75
Obstetric Beds	7.88	13.26
Neonate Intensive Care Unit	0.17	0.37
Bassinets (beds for babies)	8.21	12.3
Accredited Hospitals	0.66	0.46
Major Teaching Hospital	0.024	0.15
Minor Teaching Hospital	0.18	0.38
Public Hospital	0.23	0.42
Non-Profit Hospital	0.49	0.50
For-Profit and Other	0.28	0.41
Critical Access Hospital	0.24	0.41
Sole Community Hospital	0.048	0.21
Hospital Characteristics		
Maternity Providers with Admitting Privileges	40.3	74.9
Registered Nurses	194	326
Individual Characteristics (6,039,936)		
High-risk	0.06	0.23
Before 20 years	0.11	0.31
20-24 years	0.30	0.46
25-34 years	0.50	0.50
35 and beyond	0.09	0.29
White	0.70	0.46
Black	0.09	0.29
Hispanic	0.08	0.27
Married	0.57	0.50
Several Children	0.62	0.48
More than High School	0.49	0.47
Bypasser (Go beyond 50 miles for delivery)	0.067	0.25
Black Bypasser	0.006	0.08
Hispanic Bypasser	0.006	0.08
White Bypasser	0.05	0.21
Community Characteristics		
Medicaid reimbursement rates for Obstetrics Care ($)	1333	266
ACA	0.18	0.38
Certificate of Need Law	0.73	0.43
Birth-Hospital Observations	113,488,826	
Rural Mothers Over the Period	6,039,936	
Average Number of Hospital Per Individual	18	

Notes: Author's analysis using Vital Statistics & American Hospital Annual Surveys Data. Distance traveled represents the shortest distance between the centroid of the county of residence and a potential county of birth occurrence.

residence and a potential county of birth is 32.24 miles[35]. The average number of obstetric beds is about 8 beds and 8.2 bassinets (beds for babies). Over the period, 12% of all the hospitals have at least one neonate intensive care unit bed, and 66% of them are accredited by the Joint Commission. Only 2.4% of those health facilities are major teaching hospitals, while 18% are minor teaching hospitals. About half of them are non-profit hospitals, 23% of them are public hospitals, and 28% are for-profit institutions. A large portion of the hospital facilities over the sample period have a relatively number small of beds. Over 20% of all hospitals are critical access hospitals and 4.8% are Sole Community Hospitals. On average, a hospital has 194 registered nurses and 40 maternity care providers with admitting privileges. However, 50% of all hospitals have fewer than 14 maternity providers with privileges to admit patients.

1.5 Empirical Specification

To estimate the trade-off between hospital quality and distance faced by US rural mothers, I use a birth-hospital probabilistic choice model rooted in Maximization Utility Theory. This approach provides a theoretical justification and the solid empirical ground needed to estimate the model and offer some insights into the role of care quality and distance trade-offs in the mother's hospital choice.

1.5.1 Model specification

The empirical analysis treats some aspects of individual preferences not observed by the researcher as random. The model is estimated using a maximum likelihood approach. Specifically, the utility of choosing a particular hospital for delivery is modeled as:

$$U_{iht} = V_{iht} + \varepsilon_{iht} \quad (1)$$

[35]This average includes the ghost county. Longacre et al., 2020 found that the average distance traveled for rural patients was 40.8 miles away and that their nearest hospital was about 22 miles away.

with

$$V_{iht} = W_{iht}\alpha + \beta Z_{it} * H_{ht} + \gamma H_{ht} + \lambda X_{sct}$$

where $i = 1, ..., N$ is the rural expectant mother, with N being the total number of mothers; $h = 1, ..., H_{it}$ is the hospital where patient i could deliver the baby, with H_{it} being the total number of hospital alternatives in the mother's choice set; and t being the birth year. Additionally, the V_{iht} (explained component of the utility) captures the desirability of hospital alternatives. These utilities are a linear function of hospital-specific attributes (distance and quality metrics, W_{iht}), interaction terms between hospital features and individual characteristics ($Z_{it} * H_{ht}$), hospital characteristics (H_{ht}) and community features (X_{sct}). Given that this approach is a within-individual framework, all factors that do not vary within individuals across hospitals will not impact the choice of hospital. I capture observable heterogeneity in the risk level by interacting high-risk pregnant mothers with a NICU indicator. H_{ht} captures hospital characteristics such as different indicators for total hospital beds, outpatient visits, hospital personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and hospital total expenses. H_{ht} also controls for different types of hospitals such as critical access, sole community, public, and non-profit hospitals.

X_{sct} captures several community features such as hospitals in Medicaid expansion state, obstetric reimbursement rates, Certificate of Needs law (CON), poverty rates, county household income, unemployment rate, county population, and an indicator for county that experienced at least one hospital closures. These impacts are identified by variations in the choice sets that overlap several counties and states[36]. For example, the choice set for mothers living in a state with Medicaid expansion could include hospitals located in another state that did not expand Medicaid. This random utility model has a compensatory or trade-off interpretation between the different explanatory variables. Luft et al., 1990 argues that one major advantage of this type of qualitative choice modeling is that it considers the characteristics of the alternatives rejected and the chosen one in computing

[36]In my sample, 4% of individuals travel across state for delivery purposes. In Table A.1 Appendix, I do another specification where I restrict the choice set to only hospitals within the state. The results are consistent with the main specification.

parameter estimates.

The unobserved random error term, ε_{iht}, captures disturbances from unmeasured attitudes and preference variations that are assumed to be independently and identically distributed according to a type I extreme value distribution. In this framework, a mother will choose an alternative j if it provides the highest utility. The probability of individual i choosing a hospital in county c under these assumptions is given by:

$$P_{ijc} = P(U_{ijc} > U_{ilc}) = \frac{\sum_{j \in C} exp(V_{ijt})}{\sum_{h=1}^{H_{it}} exp(V_{iht})}, \forall \ l \in H_{it}.$$

The summation in the numerator is the sum over all hospitals within the observed county of birth c. The log-likelihood is maximized numerically.

1.5.2 Endogeneity

Distance traveled is an artifact of where the patient resides. If the mother's residential decisions are a function of hospital quality for obstetric services provided in a community, the distance coefficient estimated in the model would be biased. The model's central assumption is that distance and the observed quality measures are uncorrelated with the error term, meaning that they are exogenous. Endogeneity of the distance variable would mean that mothers self-select their place of residence due to the availability of obstetric care providers in a community.

I have found significant evidence of mothers' location decision depending on school quality (Liu, Mroz, and Klaauw, 2010), and medicare patients' location decision depending on hospitals' quality in treating heart diseases (Kessler, 2005). However, to my knowledge, no study suggests that mothers sort into communities of residence due to obstetric care [37]. As a result, I argue that distance from a county of residence to a potential county of birth is exogenous.

Regarding the possible endogeneity of hospital quality in the choice set, the literature takes

[37] One may think that maybe mothers do not choose to live close to obstetric care, but if obstetric care providers are located close to schools and communities with children, that could still be an issue. This could be a concern if I was doing an aggregated model in which the past decision of flows of patients would influence the location decision of obstetric care providers. However, in the context of our individual choice framework, this does not represent an issue.

different positions, including 1) not discussing it (Luft et al. 1990); 2) discussing it and assuming that it is exogenous (Tay 2003) [38], and 3) treating it as endogenous and using hospital fixed-effects among other approaches (Gutacker et al., 2016). The possible endogeneity of the hospital quality measures would mean that there are unobserved individual factors that are correlated with at least one of the quality metrics and that affect hospital choice. As a robustness check, I present a lagged quality [39] measures model to examine whether recent changes in quality measures might be related to the unobserved components (Gutacker et al., 2016).

It may also be that there is a systematic selection pattern where sicker patients select better quality hospitals. Tay, 2002 argues that this systematic patient selection bias is more of a problem for studies that use hospital outcomes (mortality rates, readmission rates, etc.) as quality indicators. If lower-quality rural hospitals only attract and admit less ill and low-risk patients, the quality of care they actually produce is likely to be lower than that implied by their patients' average mortality or readmission rates. As noted above, the quality metrics used in this study are related to the process of care or the settings and instrumentalities associated with quality care provision. Also, provider quality metrics are adjusted by a large set of demographic and community characteristics. Consequently, patient selection issues should not bias our results substantially.

A hospital fixed-effect approach could remove possible unobserved time-invariant hospital characteristics such as hospital culture and attitudes toward quality care. It would capture each hospital's time-invariant features as the between-hospital variations would be eliminated. As such, the model would be identified of within-hospital variations over time. One key problem with this approach is that there are very low within-provider variations over time and much of these variations could reflect simple measurement issues. To investigate the possibility of low within-provider variations, I compute the Intraclass Correlation Coefficient (ICC) for the quality metrics.

[38] Tay (2003) even argues that to her knowledge no other study has addressed the fact that the choice may be endogenous.

[39] Here, demand reacts to past hospital quality values, but past indicators of quality cannot be affected by demand today. Additionally, demand may influence the provision of obstetric beds, bassinets beds, NICU beds, etc. Due to hospital short-run capacity constraints, an increase in demand may even cause some beds' re-allocation into obstetric beds. This would also lead to potential simultaneity bias, where choice affects quality and quality influences choice. Although the short-run capacity constraint is also less relevant for my individual choice model, I believe that the use of a lagged-quality model may solve this potential problem.

Intraclass Correlation Coefficients (ICC) account for the total amount of variance attributable to between-unit rather than within-unit differences over time (Hausknecht, Hiller, and Vance, 2008). The between-provider variations range between 94% and 98% for the quality metrics; all of these variations will be discarded by the inclusion of hospital fixed effects, which would induce very low within variations in the hospital quality metrics. These low within-providers variations are consistent with Tay, 2003's idea, according to which a health facility may take several years to adjust quality. Furthermore, Gutacker et al., 2016 argues that minimal within-provider variations such as these are likely to obstruct the identification in hospital fixed-effect model and that this model could yield very large standard errors for the estimates of the marginal utility of quality.

1.5.3 Results' Interpretation

The model estimates marginal utility for travel distance and different hospital quality measures. The marginal utility represents the net utility from the change in a given factor. For example, the point estimates for teaching hospitals [40] represent the net utility from comparing teaching to non-teaching hospitals. A negative marginal utility [41] for teaching hospitals means that patients value the characteristics of teaching hospitals less than non-teaching hospitals. It may be that the patients like the quality of the services provided by teaching and non-teaching facilities alike but dislike the cost of those facilities relatively more than non-teaching institutions. As such, they could be less likely to go to those facilities. On the other hand, if the marginal utility for teaching hospitals is positive, patients would be more likely to go to those facilities.

This paper uses different observable heterogeneity models. Considering that coefficients in separate Multinomial Logit Model may be scaled differently, I use an interpretation consistent for all the models. I compute the willingness to travel (WTT) for a unit change in a given quality measure m by the following formula: $WTT_m = (-\frac{MUquality_{ht}}{MUdistance_{iht}})$ as in Moscelli et al., 2016

[40]It is worth noting that patients do not need to know the peculiarities of different types of hospitals (e.g., ownership status) to value their characteristics. Valuation may be a function of past experiences, doctor's preferences and affiliations, patients' lay network (family, friends), etc.

[41]The utility function (1) is unique up to a linear transformation. Any monotonic transformation will affect the marginal utility. A more consistent measure is the ratio of marginal utilities.

and Gutacker et al., 2016. WTT_m is the change in distance that a rural mother requires to offset a one-unit increase in a given quality measure m. When WTT is positive, it represents the additional miles mothers would be willing to travel to a hospital of higher quality. In that sense, patients are willing to travel to a given hospital because they like the facility's characteristics. When WTT is negative, it represents a disamenity or cost factor that the patients are willing to accept to avoid going to a given hospital. In this case, patients are willing to travel further to find a hospital without those characteristics.

1.6 Empirical Results and Discussions

1.6.1 Main Effects

Table 2 reports the results for the main specification, where the main effects are estimates of marginal utilities for a given factor. The estimates show that rural mothers have negative marginal utilities for travel distance. Mothers also express negative valuations for counties outside of the 50-miles radius from their county of residence. This result makes sense because higher travel distances are synonymous with higher out-of-pocket transportation costs and more discomfort for a pregnant mother. The finding is also consistent with studies using different approaches such as distance decay, gravity model, and other Random Utility Models.

The results also indicate that rural patients value obstetric beds and bassinets (beds for babies). The marginal utility for obstetric beds is 0.00387 and 0.00521 for bassinets. Considering the disutility of distance of 0.126, the willingness to travel (WTT) is 0.03 miles for obstetric beds and 0.04 miles for bassinets, respectively. As such, rural mothers would be willing to travel an additional 0.03 miles for an additional obstetric bed and 0.04 miles for a one-unit increase in bassinets.

Additionally, the effects of Neonate Intensive Care Unit (NICU) on patients' utility are given by the following linear relationship: $\dfrac{\partial U}{\partial NICU} = -0.38 + 0.769 * Risky$. Following the literature, I define High-risk mothers as patients aged 35 years and older and have had at least one previous c-section, or who are aged 44 years and older, or who have a current plural birth (e.g., twins, triplets)

Table 2: Estimates of Marginal Utility

	Coefficient	Standard Errors
Distance Metrics		
Distance	-0.126***	(0.000183)
Ghost County Dummy	-1.437***	(0.0195)
Hospital Quality Metrics		
Obstetric beds	0.00387***	(0.00061)
Bassinets (beds for babies)	0.00521***	(0.00063)
Neonate Intensive Care Unit (NICU)	-0.38***	(0.0134)
High-risk*NICU	0.769***	(0.0211)
Accredited Hospitals	0.0573***	(0.00065)
Major Teaching Hospitals	-0.738***	(0.0529)
Minor Teaching Hospitals	-0.105***	(0.0098)
Public Hospitals	0.0724***	(0.0089)
Non-Profit Hospitals	0.157***	(0.0084)
Hospital Low Quality Metrics		
Critical Access Hospitals	-0.144***	(0.009)
Sole Community Hospitals	0.009	(0.0106)
Birth-Hospital Observations	113,488,826	
Births	6,039,936	

Notes: Authors' analysis using Vital Statistics & American Hospital Annual Surveys Data for 2007-2017. A high-risk individual is a mother that has had a previous c-section and is beyond 35 years old, or that is beyond 44 years old, or that have had a previous c-section, or a mother that had a plural birth. The model controls for distance, different hospital quality metrics, hospital size variables (different indicators for total hospital beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), and community characteristics (unemployment rate, Medicaid expansion, poverty rates, household income, county obstetric reimbursement rates, certificate of needs law, population, and an indicator for any hospital closure in a potential county of birth.). The standard errors are clustered at the individual level.

and have had at least one previous c-section [42]. For low-risk individuals, the marginal effect of NICU is -0.38 compared to +0.389 for high-risk individuals. Low-risk expectant mothers value the characteristics of non-NICU hospitals more than NICUs. They are willing to travel 3.01 miles (9.4% more miles relative to the mean) away from any NICU hospitals [43].

High-risk mothers are willing to travel an additional 3.08 miles for NICU hospitals. This willingness to travel is significant as it represents 9.5% of the average travel distance by those mothers. Therefore, Neonate Intensive Care Unit hospitals represent a contingency plan for high-risk mothers if something is wrong with the baby. Those hospitals are often equipped with advanced technology and healthcare specialists to take care of the newborn.

The results also show that rural mothers value hospitals accredited by the Joint Commission. Rural mothers are likely to go to these hospitals as they value their characteristics. As far as teaching hospitals, the willingness to travel is -6.2 miles for major teaching hospitals and -0.83 miles for minor teaching hospitals. As such, rural patients are willing to travel 6.2 miles (or 19% miles relative to the mean) further to go to a similar non-teaching hospital. These additional mileages represent a disamenity or cost factor that rural patients are willing to accept to avoid going to any teaching hospitals. This result suggests that, ceteris paribus, rural mothers do not like teaching hospital facilities to deliver their babies and are less likely to choose them to deliver even if they have them in their choice set.

In Table A.2 Appendix, I present the descriptive statistics by hospital teaching status and rurality. The raw statistics show that urban hospitals have better quality than rural hospitals and that teaching hospitals have better quality than non-teaching hospitals. Teaching hospitals have bigger sizes, higher probability of having a NICU, higher probability of being accredited by the joint commission, lower probability of being critical access hospitals, and a higher number of maternity care providers with admitting privileges than non-teaching hospitals.

The finding that rural patients are less likely to deliver their babies in teaching facilities is

[42] I use another specification where I widen the definition of high-risk following Bladder, 2000 and Mayo-Clinic, 2020 to add patients below 17 years old and that had one or several pre-pregnancy risk factors and the conclusions did not change.

[43] This result remains an issue that I am investigating further.

puzzling considering that teaching hospitals have better quality and use cutting-edge technology to cure rare and complicated illness conditions. One would think that patients would always want to choose such a technology-friendly environment, holding everything else constant. However, there are at least two reasons why patients may not choose teaching hospitals. First, less than 1% of maternity care providers in rural areas have privileges to admit patients in teaching hospitals, thereby reducing the likelihood that these facilities will be chosen. Second, some patients are afraid that the attending physician may be a resident and not a senior doctor (Mishori, 2003).

It is also worthwhile to note that some rural hospitals have affiliations with Academic Medical[44] Center. Although a patient chooses a rural community facility, depending on the complexity of the care needed, some patients may be overseen by specialists in academic medical centers through Telemedicine[45]. Telehealth is expanding maternity care in rural areas (Barthelemy, 2020) and is beneficial to both low-risk and high-risk obstetric care patients (Lowery, 2018).

Furthermore, the results also show that rural mothers value the characteristics of public hospitals and non-profit hospitals relative to private for-profit hospitals. The estimated marginal utility is 0.0724 for public and 0.157 for non-profit hospital facilities. Considering the disutility of distance, the WTT is 0.57 miles for public and 1.25 miles for non-profit hospitals. As such, rural mothers value non-profit hospitals more than public and private for-profit hospitals as they are willing to travel more miles to reach any non-profit medical facilities relative to public and private for-profit institutions. Forty nine percent of hospitals in the country are non-profit institutions. As tax-exempted institutions, they are required to accept all patients irrespective of their financial situations or health insurance status. They are typically cheaper options for mothers than private for-profit institutions, while for-profit hospitals are generally better equipped with specialized materials.

Finally, the main results show that rural mothers negatively value the characteristics of critical access hospital (CAH) as they are willing to travel 1.14 additional miles to avoid those institutions. CAHs typically have no more than 25 beds and are located up to 35 miles away from another hos-

[44] An Academic Medical Center is generally made of a teaching hospital and a medical school.
[45] Even in my study period.

pital. They also have an average length of stay of 96 hours and provide 24/7 emergency services. The results suggest that rural patients are less likely to go to those hospitals for delivery purposes. Among the possible reasons for this finding are the poor reputation and the low quality of care provided by those institutions (Taylor and Cosenza, 1999; Liu, Bellamy, and McCormick, 2007). Contrary to CAHs, the marginal utility is positive but statistically insignificant for Sole Community Hospitals (SCH).

1.6.2 Mother Heterogeneity Analysis

Location and quality may mean something entirely different for several distinct sub-population groups. In Tables 3 to 6, I present a mother observable heterogeneity analysis by estimating the model for several race groups, education levels, marital statuses, age categories, and types of insurance coverage. This analysis matters for several reasons, including the fact that mothers may be exposed to very different societal factors, and the underlying health and social inequities across groups may differ.

The first two rows of Table 3 show that all the different races (Asians, Blacks, Hispanics, and Whites) have negative marginal utilities for distance. All the race groups have positive valuations for obstetric beds and bassinets. Low risks mothers are less likely to go to any NICU hospitals. They are willing to pay a cost to avoid giving birth in a NICU facility, with low risk black mothers having the lowest willingness to travel additional miles than any other race groups. However, for high-risk mothers, all the race groups express a strong willingness to travel additional miles to reach any NICU hospitals for delivery purposes. For example, high-risk white and black mothers are willing to travel 6.5 and 6.2 miles, respectively, to go to any NICU hospital. The WTT is significant as it accounts for more than 20% of the average travel distance by any of these two races.

The heterogeneity by race also shows that all the different race groups value the characteristics of public and non-profit hospitals. Hispanics have the greatest[46] willingness to travel for

[46] I compare the willingness to travel (WTT) for all the race groups. For more information about how I compute the WTT, please refer to the Result Interpretation Sub-section.

Table 3: Estimates of Marginal Utilities for Different Races

	(1)	(2)	(3)	(4)	(5)
	All	Black	White	Hispanic	Other-race
Distance Metrics					
Distance	-0.126***	-0.145***	-0.131***	-0.108***	-0.110***
	(0.000183)	(0.000701)	(0.000229)	(0.000515)	(0.000453)
Ghost County Dummy	-1.437***	-1.899***	-1.404***	-1.313***	-1.208***
	(0.0195)	(0.0721)	(0.0248)	(0.0587)	(0.0493)
Hospital Quality					
Obstetric beds	0.00387***	0.00567***	0.00371***	0.00258	0.00820***
	(0.00061)	(0.00192)	(0.000784)	(0.00186)	(0.00158)
Bassinets	0.00521***	0.0102***	0.00421***	0.0110***	0.00206
	(0.00063)	(0.00199)	(0.000796)	(0.00196)	(0.00160)
NICU	-0.38***	-0.252***	-0.422***	-0.400***	-0.282***
	(0.0134)	(0.0477)	(0.0173)	(0.0401)	(0.0308)
High-risk*NICU	0.769***	0.783***	0.827***	0.534***	0.715***
	(0.0211)	(0.0780)	(0.0260)	(0.0651)	(0.0542)
Accredited Hospitals	0.0573***	0.0258	0.0549***	0.121***	0.0932***
	(0.00065)	(0.0250)	(0.00808)	(0.0207)	(0.0162)
Major Teaching Hospital	-0.738***	-0.821***	-0.681***	-0.987***	-0.783***
	(0.0529)	(0.233)	(0.0633)	(0.217)	(0.124)
Minor Teaching Hospitals	-0.105***	-0.0923**	-0.119***	-0.0260	-0.127***
	(0.0098)	(0.0465)	(0.0123)	(0.0306)	(0.0216)
Public Hospitals	0.0724***	0.0225	0.0917***	0.144***	0.0319
	(0.0089)	(0.0282)	(0.0116)	(0.0257)	(0.0231)
Non-Profit Hospitals	0.157***	-0.0666**	0.208***	0.132***	0.0931***
	(0.0.0084)	(0.0279)	(0.0106)	(0.0244)	(0.0220)
Low Quality					
Critical Access Hospitals	-0.144***	-0.249***	-0.134***	-0.167***	-0.176***
	(0.009)	(0.0360)	(0.0116)	(0.0284)	(0.0244)
Sole Community Hospitals	0.009	-0.00984	0.000234	0.0961***	0.0433*
	(0.0106)	(0.0425)	(0.0137)	(0.0290)	(0.0244)
Birth-Hospital Observations	113,488,826	10,369,975	79,653,506	9,508,859	13,956,486
Births	6,039,936	552,181	4,241,400	506,329	740,026

Notes: Author's analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017. Each column represents a different model where the outcomes are All, Black, White, Hispanic, and Other-race, respectively. A high-risk individual is a mother that has had a previous c-section and is beyond 35 years old, or that is beyond 44 years old, or that has had a previous c-section and a plural birth. Each model controls for distance traveled, hospital characteristics (beds, outpatient visits, registered nurses and other personnels, number of registered adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), and community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates,of needs law, poverty rates, household income, county population, and an indicator for any hospital closure in a potential county of birth). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. For a consistent interpretation, I compute the Willingness To Travel (WTT). See empirical section for more details about the WTT.

public institutions and Whites prefer non-profit medical facilities more than any other race groups. Blacks have the greatest disamenity in critical access hospitals while Hispanics and other-race (e.g., Asians) have positive marginal satisfaction for sole community hospitals. Only Hispanics have results that are somewhat different than the main findings. All race groups except Hispanics have statistically significant results for obstetric beds. Results are also imprecisely estimated for minor teaching hospitals for Hispanic mothers. Contrary to Blacks and Whites, Hispanics have positive and statistically significant marginal utilities for Sole Community Hospitals. They are willing to travel about 3% more miles (relative to the corresponding mean) to SCH. Thomas, Holmes, and Pink, 2017 argues that SCH served about 27 individuals per square miles and about 3% of these individuals are Hispanics. Table 4 presents the estimates of marginal utilities for different education and marital status groups. The results show that high educated rural mothers have relatively lower disutility for travel distance than low educated mothers. It is also the case that married women have relatively lower disutility for distance than non-married patients. All education groups and marital statuses have positive valuations for obstetric beds and bassinets. Low-risk rural mothers of all education and marital status would be willing to travel several miles to avoid going to a NICU hospital. High-risk college-educated mothers (high-education) are willing to travel 5.55 additional miles to go to any NICU hospitals compared to 6.3 miles for high-risk non-college educated mothers (low education).

Table 5 presents the estimates of marginal utilities for three different age categories (14-19;20-39;40-49). All the age groups show negative marginal utilities for travel distance, with teenage mothers (below 20 years old) having the greatest disutility for distance relative to obstetric beds and older mothers having the lowest. All age groups have positive marginal utilities for obstetric beds and bassinets. All age groups like the characteristics of hospitals accredited by the Joint Commission, with the effects being the greatest for young mothers (between 20 and 39 years). High-risk [47] teenage mothers are willing to travel 12.8 miles (or 40% relative to the mean) to go to any NICU hospital. Given that high-risk mothers are old patients with a previous c-section

[47]The risk definition is a function variables such as age, previous c-section, and plural birth. I use another risk definition that also includes pre-pregnancy risk factors such as diabetes, hypertension, and eclampsia.

Table 4: Estimates of Marginal Utilities for Different Education And Marital Status Groups

	(1)	(2)	(3)	(4)	(5)
	All	High Education	Low Education	Married	Non-Married
Distance Metrics					
Distance	-0.126***	-0.119***	-0.133***	-0.122***	-0.133***
	(0.000183)	(0.000257)	(0.000291)	(0.000236)	(0.000292)
Ghost County Dummy	-1.437***	-1.257***	-1.569***	-1.238***	-1.679***
	(0.0195)	(0.0280)	(0.0307)	(0.0258)	(0.0304)
Hospital Quality					
Obstetric Beds	0.00387***	0.00292***	0.00167*	0.00495***	0.00273***
	(0.00061)	(0.000905)	(0.000975)	(0.000808)	(0.000948)
Bassinets	0.00521***	0.00504***	0.00610***	0.00430***	0.00641***
	(0.00063)	(0.000914)	(0.000991)	(0.000831)	(0.000961)
NICU	-0.38***	-0.379***	-0.388***	-0.400***	-0.350***
	(0.0134)	(0.0191)	(0.0212)	(0.0178)	(0.0206)
High-risk*NICU	0.769***	0.661***	0.836***	0.685***	0.860***
	(0.0211)	(0.0278)	(0.0370)	(0.0253)	(0.0389)
Accredited Hospitals	0.0573***	0.0566***	0.0700***	0.0609***	0.0541***
	(0.00065)	(0.00945)	(0.0102)	(0.00864)	(0.0101)
Major Teaching Hospital	-0.738***	-0.665***	-0.807***	-0.677***	-0.820***
	(0.0529)	(0.0685)	(0.0897)	(0.0676)	(0.0854)
Minor Teaching Hospitals	-0.105***	-0.118***	-0.0970***	-0.113***	-0.0932***
	(0.0098)	(0.0134)	(0.0154)	(0.0128)	(0.0153)
Public Hospitals	0.0724***	0.0706***	0.0659***	0.0916***	0.0562***
	(0.0089)	(0.0134)	(0.0138)	(0.0119)	(0.0137)
Non-Profit Hospitals	0.157***	0.191***	0.147***	0.180***	0.131***
	(0.0.0084)	(0.0124)	(0.0129)	(0.0111)	(0.0129)
Low Quality					
Critical Access Hospitals	-0.144***	-0.138***	-0.145***	-0.136***	-0.153***
	(0.009)	(0.0136)	(0.0147)	(0.0123)	(0.0146)
Sole Community Hospitals	0.009	-0.000185	-0.00376	0.0117	0.00766
	(0.0106)	(0.0153)	(0.0168)	(0.0140)	(0.0164)
Birth-Hospital Observations	113,488,826	50,144,748	63,344,078	63,946,744	49,542,282
Births	6,039,936	2,670,114	3,369,822	3,405,045	2,634,891

Notes: Author's analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017. Each column represents a different model where the outcomes are All, High Education, Low Education, Married, and Non-Married, respectively. A high-risk individual is a mother that has had a previous c-section and is beyond 35 years old, or that is beyond 44 years old, or that has had a previous c-section and a plural birth. Each model controls for distance traveled, hospital characteristics (beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), and community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates, certificate of needs law, poverty rates, household income, county population, and an indicator for any hospital closure in a potential county of birth). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. For a consistent interpretation, I compute the Willingness To Travel (WTT). See empirical section for more details about the WTT.

or patients with plural births and a current c-section[48], the teenage mothers who fit the high-risk definition are likely to be mothers with a plural birth and have a current c-section. They are more likely to give birth to a baby that necessitates the care of trained neonatologists and be in a facility with advanced equipments. As such, choosing a NICU hospital to give birth may represent an emergency plan for high-risk teenage mothers.

Table 6 shows the estimates of marginal utilities for different insurance (Medicaid, Private, and Uninsurance) and Women Infants Children (WIC) groups. The patient heterogeneity analysis by insurance status shows that individuals of all insurance types dislike traveling further to deliver their babies. Private insurance holders show a greater willingness to go beyond the 50 miles than other insurance groups. High-risk private insurance holders have strong preferences for NICU hospitals. The WTT is 6.2 miles (or 19% more miles) for high-risk private insurance holders, 5.6 miles (or 17% more miles) for high-risk Medicaid patients, and 4.7 miles (or 14% more miles) for high-risk uninsured patients. Uninsured patients are more willing to travel additional miles for hospitals accredited by the Joint Commission than any other type of insurance coverage groups. However, they have lower preferences for teaching hospitals than Medicaid and private insurance holders. They are willing to travel 9 miles (or 27% more miles) away from any major teaching hospitals compared to only 5.2 miles (or 16% more miles relative) for Medicaid patients and 5.4 miles (16.6% more miles) for private insurance holders. All types of insurance holders have a disamenity to going to major teaching hospitals for delivery, with the travel cost factor being greater for uninsured patients. These latter show a greater desire to go to non-teaching hospitals, probably because these institutions are too expensive for them to pay out-of-pocket.

The effects are somewhat different for uninsured patients compared to the main results. Contrary to the main findings or other insurance holders, uninsured patients are unwilling to travel additional miles for obstetric beds - more for bassinets. The effects are not statistically significant for low-risk uninsured mothers. They show stronger preferences for non-major teaching hospitals than any other group. They have relatively stronger preferences for public and non-profit insti-

[48] Not necessarily planned.

Table 5: Estimates of Marginal Utilities for Different Age Categories

	(1)	(2)	(3)	(4)
	All	Teen	Young	Old
	Aged 14-49	Aged 14-19	Aged 20-39	Aged 40-49
Distance Metrics				
Distance	-0.126***	-0.140***	-0.126***	-0.113***
	(0.000183)	(0.000506)	(0.000212)	(0.000541)
Ghost County Dummy	-1.437***	-1.798***	-1.412***	-1.001***
	(0.0195)	(0.0525)	(0.0226)	(0.0592)
Hospital Quality				
Obstetric Beds	0.00387***	0.00354**	0.00404***	0.00328*
	(0.00061)	(0.00159)	(0.000712)	(0.00189)
Bassinets	0.00521***	0.00632***	0.00499***	0.00500***
	(0.00063)	(0.00165)	(0.000725)	(0.00193)
NICU	-0.38***	-0.339***	-0.361***	-0.526***
	(0.0134)	(0.0356)	(0.0154)	(0.0436)
High-risk*NICU	0.769***	1.793***	1.206***	0.357***
	(0.0211)	(0.107)	(0.0362)	(0.0348)
Accreditation	0.0573***	0.0554***	0.0629***	0.0242
	(0.00065)	(0.0173)	(0.00755)	(0.0202)
Major Teaching	-0.738***	-0.803***	-0.765***	-0.546***
	(0.0529)	(0.165)	(0.0611)	(0.138)
Minor Teaching	-0.105***	-0.0710**	-0.108***	-0.126***
	(0.0098)	(0.0280)	(0.0112)	(0.0291)
Public	0.0724***	0.0284	0.0814***	0.0809***
	(0.0089)	(0.0229)	(0.0104)	(0.0286)
Non-Profit	0.157***	0.117***	0.165***	0.173***
	(0.0084)	(0.0217)	(0.0218)	(0.0265)
Low Quality				
Critical Access (CAH)	-0.144***	-0.132***	-0.149***	-0.120***
	(0.009)	(0.0249)	(0.0109)	(0.0292)
Sole Community (SCH)	0.009	-0.00456	0.00457	0.0751**
	(0.0106)	(0.0289)	(0.0122)	(0.0320)
Birth-Hospital Observations	113,488,826	18,419,249	84,437,831	10,631,746
Births	6,039,936	980,791	4,496,157	562,988

Notes: Author's analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017. Each column represents a different model where the outcomes are All, Teen (below 19 years), Young (20 and 39), and Old (40 or above) respectively. A high-risk individual is a mother that has had a previous c-section and is beyond 35 years old, or that is beyond 44 years old, or that has had a previous c-section and a plural birth. Each model controls for travel distance, hospital characteristics (beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), and community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates, certificate of needs law, poverty rates, household income, county population, and an indicator for any hospital closure in a potential county of birth). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. For a consistent interpretation, I compute the Willingness To Travel (WTT). See empirical section for more details about the WTT.

tutions as opposed to private hospitals. Contrary to other insurance holders who dislike critical access hospitals and have no statistical significant effect for sole community hospitals, uninsured individuals do not value the characteristics of both critical access and sole community hospitals. Overall, they are less likely to go to those small rural community hospitals.

WIC is a federal program[49] that provides financial support to states to safeguard the health of low-income women, infants, and children up to the age of five. Besides nutritional supports, the program also educates parents about breastfeeding and a healthy diet. In all states, applicants do not need to live in the WIC service areas to satisfy the residency requirement. When applying, mothers cannot get WIC without naming the father of the beneficiary. The eligibility criteria vary slightly from state to state. The requirement for parents to participate in the WIC program in Georgia, for example, are infants and children age 1 to 5 years (including foster children), pregnant women, breastfeeding mothers (up to 1 year), and postpartum women (up to 6 months), while the state of California also includes 185% Federal Poverty Level (FPL) as another criterion.

The results in columns 5 and 6 of Table 6 show that WIC subscribers have greater disutility for travel distance relative to obstetric beds than non-WIC mothers. This result is consistent with our expectations considering that WIC mothers are generally more impoverished and more sensitive to out-of-pocket transportation costs. Both WIC and non-WIC mothers value obstetric beds, bassinets, hospitals accredited by the Joint Commission, public and non-profit hospitals. Both groups have negative marginal utilities for major and minor teaching hospitals and critical access hospitals. While WIC mothers have negative marginal utilities for Sole Community Hospitals, non-WIC mothers are willing to travel additional miles to reach any Sole Community Hospitals[50]. I believe that it may be because of the works done by Sole Community Hospitals in recent years to localize care, minimize the need for referrals, and the establishment of satellite sites and outreach clinics for the provision of primary and emergency care. Non-WIC mothers are more likely to be wealthier and more educated, they are also more likely to look for information online about hospitals and possible diagnoses (Fox and Duggan, 2013). Overall, the patient heterogene-

[49]Parents or guardians need to sign up to the program.
[50]This result will require future analyses.

Table 6: Estimates of Marginal Utilities for Different Insurance and WIC Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Medicaid	Private	Uninsured	WIC	Non-WIC
Distance Metrics						
Distance	-0.126***	-0.131***	-0.117***	-0.118***	-0.133***	-0.115***
	(0.000183)	(0.000351)	(0.000349)	(0.00126)	(0.00036)	(0.00031)
Ghost County Dummy	-1.437***	-1.597***	-1.039***	-1.251***	-1.666***	-1.0132***
	(0.0195)	(0.0383)	(0.0408)	(0.131)	(0.039)	(0.0358)
Hospital Quality						
Obstetric Beds	0.00387***	0.00135	0.00144	-0.000614	0.0032***	0.0046***
	(0.00061)	(0.00120)	(0.00130)	(0.00469)	(0.0031)	(0.0011)
Bassinets	0.00521***	0.00643***	0.00522***	0.0120***	0.0043***	0.0036***
	(0.00063)	(0.00119)	(0.00129)	(0.00448)	(0.0012)	(0.0011)
NICU	-0.38***	-0.346***	-0.397***	-0.0351	-0.26***	-0.35***
	(0.0134)	(0.0252)	(0.0273)	(0.0918)	(0.025)	(0.023)
High-risk*NICU	0.769***	0.738***	0.723***	0.549***	0.733***	0.68***
	(0.0211)	(0.0442)	(0.0372)	(0.141)	(0.068)	(0.05)
Accreditation	0.0573***	0.0634***	0.0712***	0.185***	0.056***	0.0575***
	(0.00065)	(0.0122)	(0.0129)	(0.0466)	(0.012)	(0.01)
Major Teaching	-0.738***	-0.678***	-0.635***	-1.064**	-0.78***	-0.65***
	(0.0529)	(0.0904)	(0.0796)	(0.435)	(0.094)	(0.074)
Minor Teaching	-0.105***	-0.103***	-0.157***	-0.237***	-0.097***	-0.134***
	(0.0098)	(0.0160)	(0.0162)	(0.0589)	(0.016)	(0.014)
Public	0.0724***	0.0791***	0.143***	0.269***	0.06***	0.106***
	(0.0089)	(0.0167)	(0.0192)	(0.0650)	(0.017)	(0.016)
Non-Profit	0.157***	0.160***	0.262***	0.243***	0.15***	0.22***
	(0.0.0084)	(0.0157)	(0.0178)	(0.0611)	(0.016)	(0.015)
Low Quality						
Critical Access (CAH)	-0.144***	-0.166***	-0.182***	-0.255***	-0.16***	-0.16***
	(0.009)	(0.0182)	(0.0189)	(0.0690)	(0.018)	(0.016)
Sole Community (SCH)	0.009	-0.0229	-0.00688	-0.182***	-0.038***	0.005***
	(0.0106)	(0.0178)	(0.0189)	(0.0663)	(0.018)	(0.01)
Birth-Hospital Observations	113,488,826	33,238,899	26,710,979	3,458,884	32,880,740	32,136,160
Births	6,039,936	1,769,909	1,422,310	184,179	1,826,708	1,785,342

Notes: Authors' analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017. Each column represents a different model where the outcomes are All, Medicaid, Private, Uninsured, WIC, and Non-WIC, respectively. A high-risk individual is a mother that has had a previous c-section and is beyond 35 years old, or that is beyond 44 years old, or that has had a previous c-section and a plural birth. Each model controls for distance traveled, hospital characteristics (beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), and community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates, certificate of needs law, poverty rates, household income, and county population). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. For a consistent interpretation, I compute the Willingness To Travel (WTT). See empirical section for more details about the WTT.

ity analysis shows that rural mothers dislike traveling far to give birth and like many aspects of high-quality hospitals. The several distinct sub-populations have preferences for hospital quality as measured by obstetric beds, bassinets, hospitals accredited by the Joint Commission, public and non-profit hospitals. They are willing to pay a cost to avoid going to teaching hospitals, critical access, and sole community hospitals for some groups. Low-risk patients exhibit the same characteristics. They prefer non-NICU hospitals over NICU facilities, while high-risk patients are willing to travel to hospitals with NICU. Overall, the heterogeneity analysis across the different groups is robust and appears consistent with the main study result.

1.7 Robustness Checks

1.7.1 Choice Set Expansion and Contraction

Thus far, I use a market area of 50 miles to evaluate the tradeoff of interest. To further check the robustness of the results to potential unobserved confounders, I use two different specifications: 1) expand the choice set up to 60 miles beyond the county of residence, and 2) shrink the option set to 40 miles. These specifications are equivalent to adding 10 miles (or alternative hospital choices to each individual) and removing 10 miles (or removing alternative hospital choices). Table 7 presents the choice set expansion and contraction models' results. For the 40-miles radius, on average, mothers have 12 hospitals in their choice sets compared to 18 and 27 for the 50-miles radius (main specification) and 60-miles radius, respectively. Only 6.7% of individuals go outside the 50-miles radius (for the main specification), and only 3.5% go beyond the 60-miles. The market area population coverage is very high, and the ghost county is not a preferred option for most people.

Overall, for all the quality metrics, the trade-off decreases with respect to the radius of the choice set. To gauge the extent of the disutility across the three choice set approaches, I divided the marginal utility of distance in each model by the marginal utility of obstetrics beds. The results are larger for 60-miles, followed by the 50-miles radius. The larger the choice set, the larger disutility for distance relative to obstetric beds. Regarding hospital quality, the choice set expansion and

Table 7: Estimates of Marginal Utilities Using Different Choice Sets

	40-miles Radius	50-miles Radius	60-miles Radius
Distance Metrics			
Distance	-0.103***	-0.126***	-0.140***
	(0.000174)	(0.000183)	(0.000198)
Ghost County Dummy	-0.570***	-1.437***	-1.988***
	(0.0177)	(0.0195)	(0.0216)
Hospital Quality Metrics			
Obstetric beds	0.00636***	0.00387***	0.00419***
	(0.000553)	(0.00061)	(0.000665)
Bassinets (beds for babies)	0.00675***	0.00521***	0.00285***
	(0.000567)	(0.00063)	(0.000680)
Neonate Intensive Care Unit (NICU)	-0.443***	-0.38***	-0.383***
	(0.0124)	(0.0134)	(0.0146)
High-risk*NICU	0.839***	0.769***	0.750***
	(0.0192)	(0.0211)	(0.0240)
Accredited Hospitals	0.0620***	0.0573***	0.0331***
	(0.00602)	(0.00065)	(0.00706)
Major Teaching Hospitals	-1.011***	-0.738***	-0.704***
	(0.0547)	(0.0529)	(0.0554)
Minor Teaching Hospitals	-0.100***	-0.105***	-0.0814***
	(0.00892)	(0.0098)	(0.0106)
Public Hospitals	0.0918***	0.0724***	0.0405***
	(0.00825)	(0.0089)	(0.00969)
Non Profit Hospitals	0.187***	0.157***	0.141***
	(0.00772)	(0.0084)	(0.00907)
Hospital Low Quality Metrics			
Critical Access Hospitals	-0.166***	-0.144***	-0.108***
	(0.00870)	(0.009)	(0.0101)
Sole Community Hospitals	0.0142	0.009	-0.0203*
	(0.00950)	(0.0106)	(0.0117)
Birth-Hospital Observations	70,701,421	113,488,826	163,078,272
Births	6,039,936	6,039,936	6,039,936
Average Number of Hospitals	12	18	27
Market Area Population Coverage	91.54%	93.3%	96.5%

Notes: Author's analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017. The three columns are 40, 50, and 60 miles radius between the centroid of the county of residence and county of birth occurrence. Each model controls for travel distance, hospital characteristics (beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates, certificate of needs law, poverty rates, household income, county population, and an indicator for any hospital closure in a potential county of birth). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. For a consistent interpretation, I compute the Willingness To Travel (WTT).

contraction analysis shows that the three models reach the same qualitative conclusion. Rural mothers value obstetric beds, bassinets, accredited hospitals, public hospital facilities, and non-profit institutions. High-risk rural mothers value hospitals with NICU, while low-risk individuals prefer the characteristics of hospitals with NICU at the expense of facilities with NICU. Regardless of how I vary the precise choice set, the three models also conclude that patients dislike traveling far, teaching hospitals, and Critical Access Hospitals to deliver their babies.

Adding or removing 10 miles to the main specification does not change the main specification's conclusion. Considering the Independent and Irrelevant Alternatives (IIA)[51] hypothesis, if the model is well specified, one should not expect a different conclusion when adding or removing hospitals from the mothers' choice sets. Consequently, the fact that the baseline estimates are robust to the various choice set definitions shows that the model is well specified, and the results are unlikely to be driven by unobserved confounders.

1.7.2 Removing Transfer Patients

Often, rural patients necessitate a level of care that cannot be provided at their chosen hospital. The treating physician or the patient may decide to initiate a transfer process. Inter-hospital transfers are a function of the doctor's affiliation, patient's insurance, and, more importantly, the severity of the illness conditions, among others. Regarding the Vital Statistics data, it does not report whether the patients came on their own or were transported by an ambulance. In the latter scenario, the preferences of the ambulance companies may dictate the hospital's choice. Ambulance companies use triage to sort patients based on their immediate needs (Iserson and Moskop, 2007), in which case, the hospital choice is based upon the severity of the patient's illness. Transferred patients are more likely to be high-risk pregnant mothers, and are also more likely to be directed by the sending hospital. In that sense, not controlling for transfer in the analysis could impact the observed heterogeneity in risk estimated in the model.

[51]The most common IIA test are the Hausman and McFadden (1984) and the Small and Hsiao test. These tests estimate a general model that does not make the IIA assumption and estimate a restricted version of the model that lead to the IIA (Cheng and Long, 2007). Cheng and Long, 2007 conclude that these tests are not useful for IIA or that they perform rather poorly (Fry and Harris, 1996, Fry and Harris, 1998).

Table 8: Estimates of Marginal Utilities For Models With Transferred and Non-Transferred Patients

	Main Specification	Without Transferred Patients
Distance Metrics		
Distance	-0.126***	-0.1287***
	(0.000183)	(0.00018)
Ghost County Dummy	-1.437***	-1.487***
	(0.0195)	(0.0197)
Hospital Quality Metrics		
Obstetric beds	0.00387***	0.0047***
	(0.00061)	(0.00061)
Bassinets (beds for babies)	0.00521***	0.0047***
	(0.00063)	(0.00064)
Neonate Intensive Care Unit (NICU)	-0.38***	-0.33***
	(0.0134)	(0.0135)
High-risk*NICU	0.769***	0.65***
	(0.0211)	(0.039)
Accredited Hospitals	0.0573***	0.0518***
	(0.00065)	(0.0065)
Major Teaching Hospitals	-0.738***	-0.732***
	(0.0529)	(0.0537)
Minor Teaching Hospitals	-0.105***	-0.1013***
	(0.0098)	(0.0099)
Public Hospitals	0.0724***	0.072***
	(0.0089)	(0.009)
Non-Profit Hospitals	0.157***	0.168***
	(0.0084)	(0.0085)
Hospital Low Quality Metrics		
Critical Access Hospitals	-0.144***	-0.135***
	(0.009)	(0.009)
Sole Community Hospitals	0.009	0.012
	(0.0106)	(0.01)
Birth-Hospital Observations	113,488,826	111,162,303
Births	6,039,936	5,916,350

Notes: Author's analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017. Each column represents a different model where the first column is the main specification and the second the model without transferred patients. A high-risk individual is a mother that has had a previous c-section and is beyond 35 years old, or that is beyond 44 years old, or that has had a previous c-section and a plural birth. Each model controls for travel distance, hospital characteristics (beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates, certificate of needs law, poverty rates, household income, county population, and an indicator for any hospital closure in a potential county of birth). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. For a consistent interpretation, I compute the Willingness To Travel (WTT). See empirical section for more details about the WTT.

Given that I only have transfer information for 2011 onward, I perform a specification check to remove transferred patients and compare the results with the main specification instead of controlling for it in the regression, which would dramatically reduce my sample size. Table 8 contrasts the estimates of the main result with the model without transferred patients. The model without transferred patients is robust and consistent with the main specification, which includes transferred patients. The interaction term between high-risk and NICU is 0.769 in the main specification and 0.65 in the specification check. When compared with the distance's disutility, the willingness to travel additional miles for any NICU hospital by high-risk mother is 6.1 in the main specification and 5.1 in the model without transferred patients. Overall, the model without transferred patients is consistent with the main specification. Not controlling for transferred patients in the main specification does not significantly affect the estimated heterogeneity in risk level.

1.7.3 Lagged Quality Measures

I check my results' sensitivity to unobserved components by performing a lagged quality measures model. I follow Tay, 2003 and assume that unobserved hospital factors are uncorrelated with our hospital quality metrics. This exogeneity assumption could be violated through potential simultaneity bias, where hospital choice affects quality and quality influences choice. In table 9, I present the estimates of the marginal utilities using lagged values of the quality metrics. This model examines whether recent changes in quality measures might be related to the unobserved hospital components (Gutacker et al., 2016). The results show that the model with lagged values for hospital quality is consistent with the main specification, the main conclusion remains and the estimated willingness to travel are more or less the same for both models. As such, I believe that the model is likely to be robust to unobserved hospital components.

1.7.4 Control Group

To further check our results' sensitivity to potential unobserved hospital heterogeneity, I present a table showing the differences by education and health insurance status. I aim to demonstrate

Table 9: Estimates of Marginal Utilities Using Lagged Values for Quality Metrics

	Main Specification	Model With Lagged Hospital Values
Distance Metrics		
Distance	-0.126***	-0.127***
	(0.000183)	(0.000184)
Ghost County Dummy	-1.437***	-1.563***
	(0.0195)	(0.0168)
Hospital Quality Metrics		
Obstetric beds	0.00387***	0.00377***
	(0.00061)	(0.000633)
Bassinets (beds for babies)	0.00521***	0.00469***
	(0.00063)	(0.000646)
Neonate Intensive Care Unit (NICU)	-0.38***	-0.297***
	(0.0134)	(0.0117)
High-risk*NICU	0.769***	0.770***
	(0.0211)	(0.0222)
Accredited Hospitals	0.0573***	0.0616***
	(0.00065)	(0.00652)
Major Teaching Hospitals	-0.738***	-0.777***
	(0.0529)	(0.0524)
Minor Teaching Hospitals	-0.105***	-0.0833***
	(0.0098)	(0.00921)
Public Hospitals	0.0724***	0.0713***
	(0.0089)	(0.00894)
Non-Profit Hospitals	0.157***	0.155***
	(0.0084)	(0.00838)
Hospital Low Quality Metrics		
Critical Access Hospitals	-0.144***	-0.145***
	(0.009)	(0.00926)
Sole Community Hospitals	0.009	0.0210**
	(0.0106)	(0.0104)
Birth-Hospital Observations	113,488,826	113,488,826
Births	6,039,936	6,039,936

Notes: Author's analysis using Vital Statistics & American Hospital Annual Survey Data for the years 2007-2017. Each column represents a different model. Each model controls for distance and different hospital quality metrics. A high-risk individual is a mother over the age of 44 years, or that is beyond 35 years and have had a previous c-section, or that had a c-section and a plural birth. The model also controls for hospital size variables (different indicator for total hospital beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and hospital total expenses), community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates, certificate of needs law, poverty rates, household income, county population, and an indicator for any hospital closure in a potential county of birth). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. For a consistent interpretation, I compute the Willingness To Travel (WTT). See empirical section for more details about the WTT.

Table 10: Differences by Education and Health Insurance Status

	Non-College-Educated Medicaid	College non-WIC Private Insurance
Distance Metrics		
Distance	-0.135***	-0.113***
	(0.000451)	(0.00056)
Ghost County Dummy	-1.634***	-0.918***
	(0.0493)	(0.064)
Hospital Quality Metrics		
Obstetric beds	0.00141	0.0051**
	(0.00154)	(0.002)
Bassinets (beds for babies)	0.00566**	0.003
	(0.00154)	(0.002)
Neonate Intensive Care Unit	-0.37***	-0.37***
	(0.0326)	(0.043)
High-risk*NICU	0.813***	0.55**
	(0.0591)	(0.08)
Accredited Hospitals	0.0738***	0.02
	(0.0155)	0.02
Major Teaching Hospitals	-0.754***	-0.38***
	(0.119)	(0.11)
Minor Teaching Hospitals	-0.0955***	-0.16***
	(0.0205)	(0.026)
Public Hospitals	0.0877***	0.09***
	(0.0205)	(0.03)
Non-Profit Hospitals	0.166***	0.24***
	(0.0199)	(0.029)
Hospital Quality Metrics		
Critical Access Hospitals	-0.181***	-0.2***
	(0.0232)	(0.03)
Sole Community Hospitals	-0.0229	0.015
	(0.0226)	(0.03)
Birth-Hospital Observations	21,749,109	9,717,560
Births	1,208,284	539,864

Notes: Authors' analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017. Each column represents a different model. The first column is composed of non-college educated Medicaid mothers. The second column is made of college-educated private insurance holders who do not participate in the Women, Infants, and Children (WIC) program. A high-risk individual is a mother that has had a previous c-section and is beyond 35 years old, or that is beyond 44 years old, or that has had a previous c-section and a plural birth. Each model controls for travel distance, hospital characteristics (beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates, certificate of needs law, poverty rates, household income, county population, and an indicator for any hospital closure in a potential county of birth). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. See empirical section for more details about the WTT.

that the effect of quality is lower when it should theoretically be lower. Hence, I use a control group of mothers whose hospital's choice is likely to be less subject to quality. I hypothesize that poor mothers of low education are less likely to respond to hospital quality than non-poor-college-educated mothers who do not participate in the WIC[52] program. Given that I do not observe income measures in the data, I use Medicaid status to proxy for mother's poverty. Medicaid pays for the delivery of low-income patients up to 60 days postpartum. For the control group, I consider mothers whose delivery was paid by Medicaid and whose highest level of education accomplished was a high school degree or less. Theoretically, one can argue that these low-income-low-education groups of rural mothers are less likely to respond to quality[53] than their counterparts in the high education and private insurance group. Their hospital choices are likely to be mainly driven by distance. On the other hand, highly educated private insurance holders who do not participate in the WIC program are more likely to have relatively lower disutility for distance.

Table 10 presents the results of the control group analysis. The results show that poor low-educated mothers and non-poor-college-educated mothers behave the way expected, especially regarding travel distance. The marginal utility of distance for the poor and low education group is -0.135, and the marginal utility for going outside of the 50 miles ("Ghost County") is -1.634. On the other side, the marginal utility of the high education group is -0.113 for distance and -0.918 for the ghost county. Using obstetric beds as a base to gauge the ratio of the marginal utilities for each group shows that the high education group has relatively lower disutility of distance. As far as the quality indicators, there is not much difference, except that high-risk mothers in the non-college educated group show greater willingness to travel to NICU hospitals than high-risk mothers in the more educated group. Mothers in the latter group have a higher desire to travel for non-profit hospitals than mothers in the control group.

Overall, the results suggest that mothers in the two groups are not so different in terms of

[52]The Women, Infants, and Children (WIC) program provides nutritional and educational supports to low-income patients.

[53]This assumption is built upon several considerations such as 1) the fact that low socioeconomic status households not only seek care less often but primarily seek emergency care (Becker and Newsom, 2003), and 2) they are less likely to gather information online about hospitals and diagnoses (Fox and Duggan, 2013).

preferences for hospital quality. Still, the more disadvantaged group is more concerned about distance. In that sense, it plays a bigger role in the non-college-educated medicaid patients group hospital choice process than the college-educated non-WIC privately insured patients.

1.7.5 New Risk Definition

Thus far, I consider high-risk mothers as patients who have had a previous c-section and are beyond 35 years old, or beyond 44 years old, or who have had a previous c-section and have a current plural birth. Only six percent of mothers fall under this category. I widen this definition to consider Bladder, 2000, and Mayo-Clinic, 2020 definition by adding patients below the age of 17 years and patients that have at least one pre-pregnancy risk factor such as diabetes, hypertension, and eclampsia. Twenty-four percent of mothers fall under this new definition. Table 11 presents the estimates of marginal utilities for different risk definition models. The study's main conclusion does not change since in both models patients dislike traveling far and prefer many aspects of the high-quality hospital. However, high-risk patients are less willing to travel to Neonate Intensive Care Unit hospitals in the new risk definition model.[54]

1.8 Discussion & Policy Implications

The results show that patients have disutility for travel distance to a hospital to give birth and strong preferences for better quality maternity care providers as measured by obstetric beds, bassinets, Neonate Intensive Care Unit (especially for high-risk patients), hospitals accredited by the Joint Commission, public and non-profit hospitals. Our findings have value because low accessibility, low availability, and low quality of maternity care can have serious negative consequences on mothers' and infants' outcomes. Travel distance is associated with reductions in health care utilization, higher rates of c-section and neonatal hypoglycemia (Robbins et al., 2019), and higher rates of adverse perinatal outcomes (Grzybowski, Stoll, and Kornelsen, 2011, Ravelli et al., 2011). Besides, Nesbitt et al., 1990 argue that women living in communities with obstetric care shortages

[54] This somewhat surprising finding will require future analyses.

Table 11: Estimates of Marginal Utilities For Different Risk Definition Models

	Main Specification	Model With New Risk Definition
Distance Metrics		
Distance	-0.126***	-.1268***
	(0.000183)	(.0001817)
Ghost County Dummy	-1.437***	-1.433***
	(0.0195)	(0.0193)
Hospital Quality Metrics		
Obstetric beds	0.00387***	0.00487***
	(0.00061)	(0.00060)
Bassinets (beds for babies)	0.00521***	0.0046***
	(0.00063)	(0.0006)
Neonate Intensive Care Unit (NICU)	-0.38***	-0.382***
	(0.0134)	(0.0134)
High-risk*NICU	0.769***	0.41***
	(0.0211)	(0.0151)
Accredited Hospitals	0.0573***	0.0522***
	(0.00065)	(0.0064)
Major Teaching Hospitals	-0.738***	-0.738***
	(0.0529)	(0.0525)
Minor Teaching Hospitals	-0.105***	-0.103***
	(0.0098)	(0.0097)
Public Hospitals	0.0724***	0.075***
	(0.0089)	(0.0089)
Non-Profit Hospitals	0.157***	0.17***
	(0.0084)	(0.009)
Hospital Low Quality Metrics		
Critical Access Hospitals	-0.144***	-0.137***
	(0.009)	(0.0092)
Sole Community Hospitals	0.009	0.0133
	(0.0106)	(0.01)
Birth-Hospital Observations	113,488,826	113,488,826
Births	6,039,936	6,039,936

Notes: Authors' analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017. Each column represents a different model where the first column is the main specification and the second uses a new risk definition. In the main specification, a high-risk individual is a mother that has had a previous c-section and is beyond 35 years old, or that is beyond 44 years old, or that has had a previous c-section and a plural birth. In the new risk definition, I also consider mothers below the age of 17, or a mother that has one of several risk factors such as diabetes, hypertension, and eclampsia. Each model controls for distance traveled, hospital characteristics (beds, outpatient visits, registered nurses and other personnels, number of registered nurses adjusted by the state scope of practice laws, number of maternity care providers with admitting privileges, and total hospital expenses), community characteristics (unemployment rate, Medicaid expansion, obstetric reimbursement rates, certificate of needs law, poverty rates, household income, county population, and an indicator for any hospital). The standard errors are clustered at the individual level. Coefficients from separate Multinomial Logit Models may be scaled differently. See empirical section for more details about the WTT.

have a relatively higher proportion of delivery complications, higher prematurity rates, and greater neonatal care costs.

This paper raises several issues and has important policy implications[55]. The results suggest that reduced travel time is associated with increased patient satisfaction. First, investments in the expansion of health care facilities may improve satisfaction because they are likely to decrease the distance traveled by the patient to deliver their babies. Second, investments in transportation systems may also increase patient satisfaction because they will likely reduce patient travel time due to traffic and congestion.

The results also imply that investment in quality care is also instrumental for patient satisfaction. In that regard, Maddox et al., 2021 shows that neonatal video-assisted resuscitation improves care quality and reduces the number of transfer patients. Donohue, Hoffman, and Marcin, 2019 advocates in favor of telemedicine to enhance care quality[56], especially in rural areas. At the same time, since 2013, several hospitals such as the Randall Children's Hospital and the Mayo Clinic have successfully utilized telehealth video-assisted resuscitation programs to help treat the newborns (Fang et al., 2016). Considering this study's result that patients have strong preferences for better maternity care providers located near them, future research may need to investigate the impact of telemedicine on rural mothers' satisfaction and hospital choice.

The policy recommendations matter because nearly 35% of US counties have no obstetrician-gynecologists (March of Dimes, 2018), and 56% have no nurse-midwives (ACNM, 2018). The American Congress of Obstetrician-Gynecologists (ACOG) estimated a shortage of up to 8,800 Ob-gyn by 2020 and up to 22,000 by 2050. This workforce shortage and the closures of several rural hospitals are likely to exacerbate the already low-accessibility and low-quality care in rural areas. Consequently, there is a necessity to expand and provide seamless maternity care in rural areas.

[55] An indirect policy implication may be the reduction of transportation costs for low-income populations. It enhance patient satisfaction. Medicaid patients are more sensitive to distance than privately insured patients (Phibbs et al., 1993). Public transportation is critical for the health care delivery of low income individuals (Evans and Lien, 2005).

[56] Not specific to deliveries.

1.9 Conclusion

The phenomenon of hospital closures in rural areas has damaging impacts on those economies and dampens the existing shortage of maternity care providers in those communities. Issues such as low availability and low accessibility to maternity care coupled with low quality and poor reputation of some rural hospitals aggravate bypassing behaviors among rural residents. Consequently, patients travel more miles for better quality hospitals. This study uses a hospital choice framework and finds that patients dislike traveling far to give birth. This result is consistent with the empirical literature considering the additional out-of-pocket transportation costs, the opportunity cost of time, and the discomfort associated with traveling further.

The study also finds that patients have strong preferences for several aspects of high-quality hospitals as measured by obstetric beds, bassinets, hospitals accredited by the Joint Commission, Neonate Intensive Care Unit hospitals, public and non-profit hospitals. On the one hand, rural mothers with low risk for birth complications are less likely to go to any Neonate Intensive Care Unit (NICU) facilities. On the other hand, high-risk rural mothers prefer the features of hospitals with NICU more than hospitals without NICU. In that sense, hospitals with NICU may represent an emergency plan for mothers expecting a challenging birth process.

Additionally, the results find that rural patients are willing to bypass teaching hospitals for delivery purposes because they prefer the characteristics of non-teaching hospitals more than teaching hospitals. In that sense, rural patients are willing to accept a disamenity or cost factor of several miles instead of going to a closer teaching hospitals to deliver. Although teaching hospitals are technological-friendly medical facilities that cure complicated illnesses, rural mothers may not go to these institutions for at least one reason: less than one percent of maternity care providers in rural areas have privileges to admit patients in teaching hospitals. Regarding small community hospitals such as Critical Access and Sole Community, rural patients are less likely to go to these institutions for delivery purposes. However, the results suggest that rural mothers prefer Sole Community Hospitals relatively more than Critical Access Hospitals.

The direct implications of this study are two-fold: investments in health care expansion in rural areas are likely to improve patient satisfaction by reducing travel time, and investments in quality care can also enhance patient satisfaction.

Chapter 2

Do Cellphone Bans Save Lives? Evidence From Handheld Laws on Traffic Fatalities

2.1 Introduction

Distracted driving[1] is one of the leading causes of traffic injuries and fatalities in the United States (National Center for Analysis and Statistics., 2021). Schroeder, Wilbur, and Peña, 2018 found that 56% of individuals make phone calls, more than 28% utilize social media applications, and about 12% read text messages or emails while operating a vehicle. In addition, at any given moment during daylight hours, there are more than 620,000 passenger vehicles that are being driven by individuals who are simultaneously communicating on their hand-held devices (Pickrell et al., 2015). Using a mobile device while driving increases the risk of an accident five-fold, and under certain conditions, distracted drivers are just as impaired as drunk drivers (Redelmeier and Tibshirani, 1997; Strayer, Drews, and Crouch, 2006).[2]

This phenomenon also levies a considerable cost on individuals, businesses, and the general public. In 2010 alone, distracted driving imposed a cost of $123 billion on society and accounted for about 15% of the total societal harm caused by motor vehicle crashes (Blincoe et al., 2015). Consequently, inattentive driving has become a serious public health concern and a key priority for policymakers. As of May 2020, legislators in more than 20 states have passed regulations that prohibit the use of handheld wireless communication devices, texting, dialing, or emailing while operating a vehicle (handheld bans).[3] While many states have adopted handheld bans to discourage distracted driving and save lives, there are a limited number of well-identified studies on the effectiveness of this policy in reducing fatal motor vehicle accidents.

In this paper, we evaluate the impact of handheld bans on the number of traffic fatalities in the

[1]Chapter 2 is a co-authored work with Nicholas A. Wright from Florida Gulf Coast University.

[2]Irrespective of the driver's quality, engaging in other activities while driving reduces awareness and places the driver and others in danger (Green, 2000).

[3]Policymakers generally believe that imposing handheld restrictions reduce traffic fatalities. For example, regarding the 2019 handheld bill in Massachusetts, Senator Joseph A. Boncore argued that ”It's really a bill that's going to improve public safety on our roads and hopefully end the act of distracted driving” and Rep. Joseph Wagner asserted that ”this bill will save lives” (Lisinski and Norton, 2019).

United States. This study has value because the number of states with handheld bans have tripled over the last decade and it is likely that the remaining states will adopt similar policies in the future. In addition, given the large cost that distracted driving imposes on society, a well-designed study may offer some insights on the role of this policy in improving traffic safety. To evaluate this policy, we utilize data on the census of all fatal motor vehicle crashes from the Fatality Analysis Reporting System (FARS) and the enactment date of handheld mandates from the National Conference of State Legislatures (NCSL).

We leverage two quasi-experimental designs to estimate the causal impact of these laws. The first approach is the temporal regression discontinuity design (RD) which estimates the short-term impact of handheld bans within a 90-days bandwidth around the effective date of the policy in each state. This is a within-state design which uses time as the forcing variable and compares the trends in traffic fatalities in the period immediately before and after the policy is adopted. The main benefit of this approach is that we are able to difference out any unobserved covariates that do not vary within a local bandwidth around the effective date (demographics, state-level laws[4], economic, and political environment) and examine the immediate impact of the policy when compliance is likely highest (McCartt et al., 2010).[5] Our second estimation strategy, a difference-in-difference design (DID), exploits the variation in the timing of adoption of handheld bans across states. This approach estimates the average treatment effect on treated states over a longer time horizon by comparing states that adopt the policy to those that do not and comparing the states that adopt the policy early to those that adopt the policy later. We also employed the Goodman-Bacon decomposition to assess the robustness of our results to the comparison group utilized (Goodman-Bacon, 2018). Jointly, the two empirical approaches allow us to assess the immediate and longer-term impact of handheld bans and provide meaningful insight on the degree to which the treatment effect fades-out over time.

[4]Some state laws may change at similar dates. We control for a detailed set of time dummy variables.

[5]Hausman and Rapson, 2018 provides an extensive discussion on the features of the temporal discontinuity design and its similarities with an event study framework. Several studies in economics have recently employed this empirical design to examine issues such as traffic congestion (Anderson, 2014; Bento et al., 2014), air quality (Auffhammer and Kellogg, 2011; Chen and Whalley, 2012; Davis, 2008; Gallego, Montero, and Salas, 2013), vehicle sales (Davis and Kahn, 2010), car accidents (De Paola, Scoppa, and Falcone, 2013), and crime (Doleac and Sanders, 2015).

Since handheld laws increase the expected cost of distracted driving, conditional on the likelihood of being caught, economic theory predicts that the introduction of this mandate will likely reduce the level of distracted driving (Becker, 1968). McCartt et al., 2010 found that these mandates have a significant and lasting negative impact on handheld phone use while driving and this impact varies across states between 24-76% up to seven years after the ban. Furthermore, Braitman and McCartt, 2010 found that the proportion of drivers who communicate on hands-free devices was nine percentage points higher in states with handheld bans compared to states without bans. This suggests that there is a high degree of compliance with these mandates over time.[6] Consequently, handheld mandates may ultimately lead to a sustained reduction in the number of traffic fatalities.

While conventional wisdom suggests that this policy should be effective at improving traffic safety, empirical studies have yielded mixed results (McCartt, Kidd, and Teoh, 2014). For example, some studies find that handheld bans are associated with a reduction in fatal accident rates or fatal crashes that involve teenage drivers (Abouk and Adams, 2013; Nikolaev, Robbins, and Jacobson, 2010; Lim and Chi, 2013; Anyanwu, 2012; Sampaio, 2014). In contrast, other studies have found suggestive evidence that the introduction of handheld laws had no impact on collision claims, crash rates, or fatalities under normal weather and road conditions (Trempel, Kyrychenko, and Moore, 2011; Bhargava and Pathania, 2013; Kolko, 2009). Through a systematic review, McCartt, Kidd, and Teoh, 2014 argued that there are several methodological challenges that may have limited these studies from causally identifying the impact of this policy. This review highlighted that the results varied based on the covariates included, the control group, and the sample utilized.[7] These limitations suggest that additional research needs to be conducted to better understand the impact of handheld bans in addressing this public health issue.

[6] McCartt et al., 2010 found up to a 49% reduction in initial compliance level seven years after implementation. In addition, they argued that there were no major enforcement campaigns in the three jurisdictions studied. Similarly, Carpenter and Nguyen, 2015 found that the adoption of a cellphone ban in Canada significantly reduced handheld cellphone use and increased hands-free cellphone use.

[7] For example, they argued that Trempel, Kyrychenko, and Moore, 2011 did not control for changes in other highway safety laws, Anyanwu, 2012 did not adequately account for unobserved factors that may influence driving habits and crash fatalities, and Lim and Chi, 2013 employed a control group that received some related treatment. As such, these results are not robust to alternative model specifications.

We found that handheld bans reduce daily traffic fatalities by 0.63 individuals or 26% in the short term. In addition, several specification checks, such as using a local quadratic polynomial, donut RD design, Poisson and negative binomial model, and varying the bandwidth size lead to the same conclusion. We also found that our main RD specification yielded a Type I error rate that is much lower than 5% when it is used to estimate the treatment effect of 200 placebo implementation dates. Lastly, the results indicate that our RD estimates cannot be plausibly explained by increased law enforcement activities since the policy did not lead police officers to stop or cite drivers more frequently. Using the DID design, we similarly found that handheld laws are effective at curbing traffic fatalities over a longer time horizon, although the results imply that the magnitude of the impact may fade-out over time. In particular, the long-term effects are about one-fifth to one-third of the estimated short-term impact of the policy. These findings suggest that handheld bans may be an effective tool to combat the adverse consequences of distracted driving and that this policy may prevent at least 69 fatalities each year per treated state in the long term.

Our study makes three significant contributions to the literature: (i) this is the first study to examine the short-term impact of handheld bans using the temporal regression discontinuity design. This is important because the literature suggests that compliance may be highest closer to the effective date of the policy. As such, our short-term estimates are likely an upper bound on the effectiveness of handheld bans to reduce accidents-related deaths. (ii) Unlike previous studies, we decompose our DID results to assess the importance of the control group utilized and the covariates included. (iii) Since we estimate both the short- and long-term impact of the policy, our study may assess the degree to which the effect of handheld bans may fade-out over time.

2.2 Mechanisms

The effects of handheld legislation on traffic fatalities are a function of individuals complying with the law. Compliance with the mandate means either not using a wireless communication device or substitute toward hands-free devices. McCartt et al., 2010 showed that reductions in phone use following the introduction of handheld mandates varied between 24% to 76% up to seven

years after the ban. Also, Braitman and McCartt, 2010 found that the proportion of drivers who communicate on hands-free devices was nine percentage points higher in states with handheld bans compared to states without bans. This evidence suggests that there is a high degree of compliance over time.

The reduction in phone use and substitution toward hand-free devices represent the first causal linkages to which handheld legislation may impact traffic fatalities. Compliance with the law may cause a reduction in the frequency or severity of accidents, which will likely cause a reduction in traffic fatalities. Even if the frequency of accidents stays the same, if compliance is correlated with a reduction in the severity of accidents, handheld mandates are also likely to be associated with reductions in traffic fatalities. Several studies found that handheld laws are associated with reductions in fatal crashes (Abouk and Adams, 2013; Nikolaev, Robbins, and Jacobson, 2010; Lim and Chi, 2013; Anyanwu, 2012; Sampaio, 2014).

2.3 Data

To obtain the main outcomes of interest, we utilize the census of motor vehicle crashes from the Fatality Analysis Reporting System (FARS). To be eligible for inclusion in this census, the motor vehicle accident must have occurred on a public trafficway within the US and involve at least one fatality within 30 days. Trained FARS analysts are responsible for collecting and communicating state-level data to a centralized system in a standardized format. These analysts utilize several state documents such as police reports, coroner/medical examiner reports, and death certificates to determine eligible cases (National Center for Analysis and Statistics., 2019). This is the best available source on fatal crashes, and it is widely used in the empirical literature.[8] The data on handheld laws were obtained from the National Conference of State Legislatures (NCSL) and cross-referenced with data from McCartt, Kidd, and Teoh, 2014, the Insurance Institute for Highway Safety (IIHS), and State Highway Safety Offices for accuracy. Finally, law enforcement data was gathered from the Stanford Open Policing Project (Pierson et al., 2020).

[8]Readers may reasonably be concerned about potential measurement errors in the FARS data. However, given that we employed these variables as outcomes in our models, our estimates are unbiased once errors are not systematic.

To estimate the impact of handheld legislation on traffic fatalities in the RD design, we use a balanced panel that is comprised of 14 states. Under this approach, observations are measured at the day level, and we consider up to 90 days before and after the implementation of the law in each state. As such, the maximum sample size is 2,534 observations.[9] For the DID design, observations are measured at the month by state level over the period 2000 to 2015. This results in a balanced panel that contains 9600 observations.[10]

Table 12: Handheld Legislation Implementation

State	Applicable to	Effective Date
New York	All drivers	Nov 1, 2001
District of Columbia	All drivers	Jul 1, 2004
Connecticut	All drivers	Oct 1, 2005
New Jersey	All drivers	Mar 1, 2008
California	All drivers	Jul 1, 2008
Oregon	All drivers	Jan 1, 2010
Delaware	All drivers	Jan 2, 2010
Nevada	All drivers	Oct 1, 2011
West Virginia	All drivers	Jun 1, 2012
Hawaii	All drivers	July 1, 2013
Maryland	All drivers	Oct 1, 2013
Illinois	All drivers	Jan 1, 2014
Vermont	All drivers	Oct 1, 2014
New Hampshire	All drivers	July 1, 2015

The table above shows the states that have enacted a handheld ban during 2001 and 2015. In these states, the law is applicable to all drivers under primary enforcement. Georgia, Maine, Massachusetts, Minnesota, Tennessee, Rhode Island, and Washington had similar bans taking effect between 2016 and 2020.

The sample period ends in 2015 because it is the most recent year of available data in the FARS census when our study concluded. Therefore, we are unable to include states like Georgia, Maine, Massachusetts, Minnesota, Tennessee, Rhode Island, and Washington that implemented a handheld ban between 2016 and 2020. Table 12 provides a summary of the handheld laws' implementation dates for the states that are included in our sample. As can be seen in this table, we focus on the 14 states that prohibit the use of handheld wireless communication devices, texting, dialing, or emailing and apply this policy to all drivers. These states also enforce this policy as a

[9]The sample includes 90 days before and after implementation. As such, including the day of implementation, we have a total of 181 days for 14 states which leads to a maximum sample size of 2,534 state-day observations.

[10]The DID sample includes 16 years of month-level data for 50 states. This leads to a sample size of 9600.

primary offense, giving police officers the legal authority to stop a driver whenever a violation is perpetrated. However, some of the states that are not considered in this paper may have a policy that applies to a learner or intermediate license holder, in school and work zones only, or are restricted to state vehicles.[11]

For the RD sample, between 0 and 24 driving fatalities occur each day across the states that adopt the policy, with an average fatality of 1.74 deaths and a standard deviation of 3 fatalities. Over the sample period, the probability of at least one fatality occurring each day is 0.53. These statistics show the pervasive nature of traffic fatalities across states during the period when these laws are introduced.

2.4 RD Empirical Specification

In this paper, we exploit the temporal regression discontinuity design to estimate the short-term impact of state-level handheld laws on fatal motor vehicle accidents. Our main RD specification can be expressed as follows:

$$F_{sd} = \alpha_0 + \alpha_1 Handheld_{sd} + \alpha_2 Time_d + \alpha_3 Handheld_{sd} \times Time_d + \alpha_h + \alpha_{dw} + \alpha_{dm} + \alpha_m + \alpha_y + \alpha_s + \varepsilon_{sd},$$

where F_{sd} is the outcome variables: total traffic fatalities, the log transformation of total traffic fatalities, and total fatality per 100,000 state population observed in state s on day d of the year. We focus on traffic fatalities because it is the most extreme outcome of distracted driving and a key indicator of the success or failure of handheld mandates. $Handheld_{sd}$ is an indicator variable that takes the value 1 following the introduction of a handheld ban in state s and zero otherwise, $Time_d$ is the running variable centered at zero around the date of implementation in each state, α_h are federal holiday indicators, and α_{dw}, α_{dm}, α_m, α_y, and α_s are fixed effects for day of the week, day of the month, month, year, and state respectively. Finally, ε_{sd} represents the random

[11]While 20 states and Washington D.C. have a cellphone ban applicable to School Bus Drivers, 39 states and Washington D.C. applies this law to novice drivers. Additionally, 48 states together with Washington D.C, Puerto Rico, Guam, and the U.S. Virgin Islands have a text messaging ban.

disturbances in driving fatalities at the state-day level.

In this model, the treatment effect of interest is α_1. This coefficient is causally identified if implementation of the handheld ban is uncorrelated with the unobserved factors affecting driving fatalities. While we do not anticipate any demand-side changes (population composition, driving skills) as a result of the law, there are several potential threats to causal identification. We examine the plausibility of our main estimates by using the model above to estimate the treatment effect on drunk driving fatalities and for several placebo implementation dates. In both of these cases, we test the effectiveness of our model to fail to reject the null hypothesis when it is true.

One may also be concerned that there is a different time trend in the outcome variable in the period before and after implementation. To ensure that our estimates are not being driven by the time trends in fatalities, we include a detailed set of time fixed effects ($\alpha_{dw}, \alpha_{dm}, \alpha_m, \alpha_y$) and several indicator variables controlling for the differential trends levels in fatal accidents on federal holidays (α_h).

Finally, state-level efforts to combat driving fatalities (police presence) may change in response to the policy in the early days around the effective date. We assess how potential increases in law enforcement efforts at the implementation margin affect our estimates in three ways: (a) we directly examine the impact of the mandate on the propensity of law enforcement officers to stop and cite drivers in a subset of treated states[12]; (b) we employ a donut temporal RD and remove up to a three-day interval around the cutoff. This shows the way the estimates change as we remove the period where police enforcement is likely to be heightened in response to the policy change; and (c) we follow Hansen, Miller, and Weber, 2017 and estimate an alternative specification that includes an indicator variable which captures a seven-day interval around the implementation of the policy. Jointly, these three robustness checks provides some intuition about the extent to which the main estimates are affected by changes in law enforcement efforts.

To further check the robustness of our main results, we estimate the following alternative specifications: (i) including higher order polynomials in $Time$, (ii) removing day of the month fixed

[12]Data on police stops and citations is only available for five of the 14 states in our sample.

effects[13], (iii) using Poisson and negative binomial regression models, and (iv) employing various approaches for clustering the standard error. For the main specification, we follow the literature and utilize a bandwidth size of 60 days (60 days before and 60 days after) (Hansen, Miller, and Weber, 2017). However, we also present the estimates for specifications that utilize several alternative bandwidth windows around the implementation date. The lowest and highest bandwidth size we consider are 30 days and 90 days, respectively.

If our model performs well under the various falsification and robustness checks we employ, this is strong supporting evidence that α_1 captures the true causal effect of interest in the short term.

2.5 Empirical Results

In this section, we present the RD estimates and discuss the short-term impact of handheld mandates on traffic fatalities. We find that handheld driving restrictions reduce the total number of traffic fatalities observed in treated states each day. We then show that these results are consistent with the estimates from several falsification, sensitivity, and robustness checks. Table 13 reports the results from our main RD specification using several measures of traffic fatalities. Columns 1, 2, and 3 show the effects of the law on total daily traffic fatalities, the log of total daily traffic fatalities, and total fatality per 100,000 state population respectively. All the models in this table include controls for day of the week, day of the month, month, year, holidays, and state fixed effects. On average, these columns show that enacting a handheld ban reduced daily traffic fatalities by 0.63 individuals or 26%.[14] Column 3 accounts for the heterogeneity in state population and shows that the law reduced total traffic fatality per 100,000 state population by 0.0123 per day. The difference between column 1 and 3 is that column 1 assumes that the effect is the same for large and small states. Overall, linear and non-linear models show that the effects of handheld

[13]Similar to Hansen, Miller, and Weber, 2017, conditional on day of the week and month fixed effects, we do not expect day of the month to have much explanatory power, but it costs several degrees of freedom. As such, we estimate the sensitivity of the results to the exclusion of this variable.

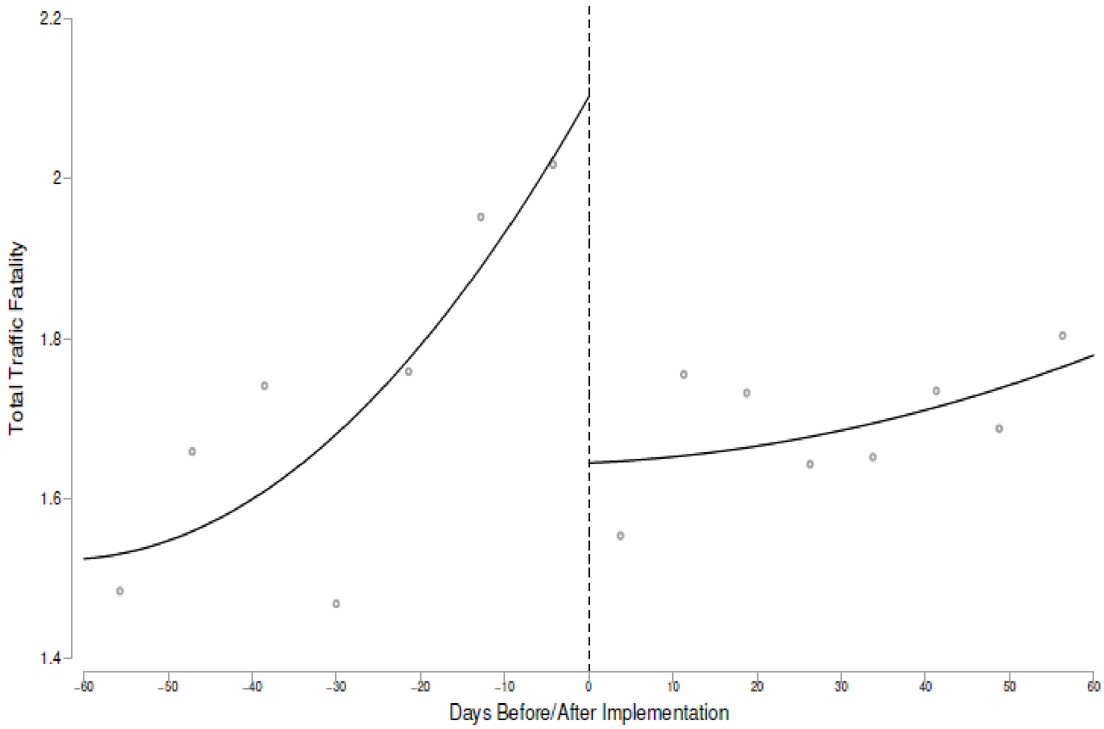
[14]In column 2, since our outcome variable is the log of total fatalities and we are interested in the coefficient on a dummy variable, we need to adjust the point estimate using $[\exp{(\hat{\alpha_1})} - 1] * 100$ to obtain the exact percentage difference.

Table 13: Impact of Handheld Law

	Total Fatalities	Log Total Fatalities	Fatality Per 100k State Population
Handheld Law			
	-0.63***	-0.30***	-0.0123***
	(0.22)	(0.05)	(0.004)
Day of Week FE	Yes	Yes	Yes
Day of Month FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Mean	1.74	0.79	0.026
SD	2.99	0.84	0.042
N	1,694	883	1,694
Bandwidth Interval	[0,60]	[0,60]	[0,60]

Notes: The outcomes in columns 1, 2, and 3 are total daily traffic fatalities, the log of total daily traffic fatalities, and fatality per 100,000 state population. The standard errors are clustered at the state by month level. Each regression utilizes a 60 day bandwidth around the implementation date. Columns 1 and 2 control for: population, holiday FE, and light and atmospheric conditions, while column 3 controls for all those variables except population.

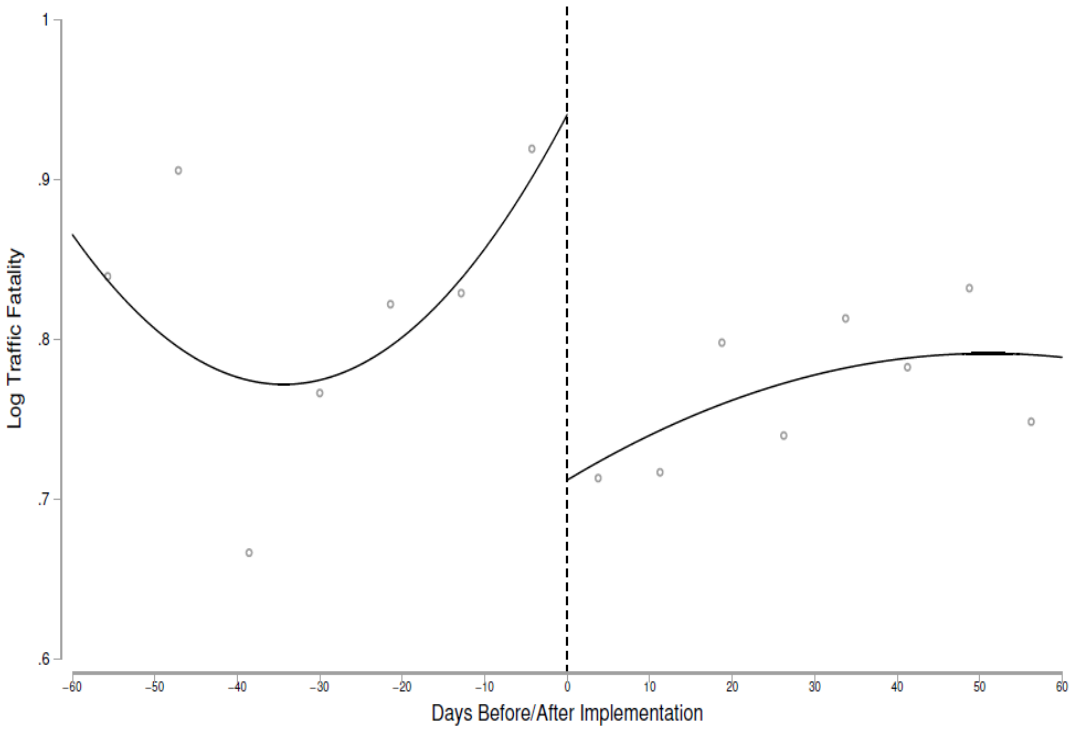
Figure 3: Effect of Handheld Law on Total Traffic Fatalities



law on traffic fatalities are negative and statistically significant. These findings are also supported by the descriptive trends presented in Figures 3 and 4, respectively. Consequently, the RD results suggest that restrictions on phone use while driving is an effective policy to curb traffic fatalities in the short term.

Table E.1 of Chapter 2's online Appendix presents the results of the handheld legislation on fatal accidents. Column 1 and 2 show the results for the total number of accidents and total accidents per hundred state population. On average, the results show that implementing a handheld ban reduced the number of daily accidents by 0.54 individuals or 34% of the mean number of accidents. When adjusting for the heterogeneity in state population, the results show that enacting a handheld law reduced total traffic fatality per 100,000 state population by 0.010 per day or about 42% of the mean of total accidents per hundred thousand state population.

Figure 4: Effect of Handheld Law on Log Traffic Fatalities



2.5.1 Falsification Analysis

We used two placebo tests to assess the plausibility of the RD findings. First, we examined the impact of the policy on drunk driving fatalities. Given that drunk drivers are less likely to change their behavior in response to the law, we expect that when we focus on this outcome, our model should yield point estimates that are statistically insignificant. Table 14 shows the impact of the policy on the main outcomes of interest for a sub-sample of accidents involving drunk drivers. The results confirm that the policy had no significant impact on various measures of alcohol-induced fatalities.

Second, to further test the veracity of our main results, we estimated the treatment effect for 200 placebo implementation dates where the null hypothesis ($H_o : \alpha_1 = 0$) is likely to be true. We created placebo dates that are outside a 200-day bandwidth around the true effective date to reduce the likelihood of inadvertently capturing the effect of any announcements related to the policy or any staggered implementation of sanctions that are associated with the law. In total, 100 placebo

Table 14: Effect on Alcohol-Induced Fatalities

	Total Drunk	Log Drunk	Drunk Per 100k State Population
Handheld Law	-0.06	-0.03	-0.003
	(0.06)	(0.06)	(0.002)
Day of Week FE	Yes	Yes	Yes
Day of Month FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Bandwidth Interval	[0,60]	[0,60]	[0,60]

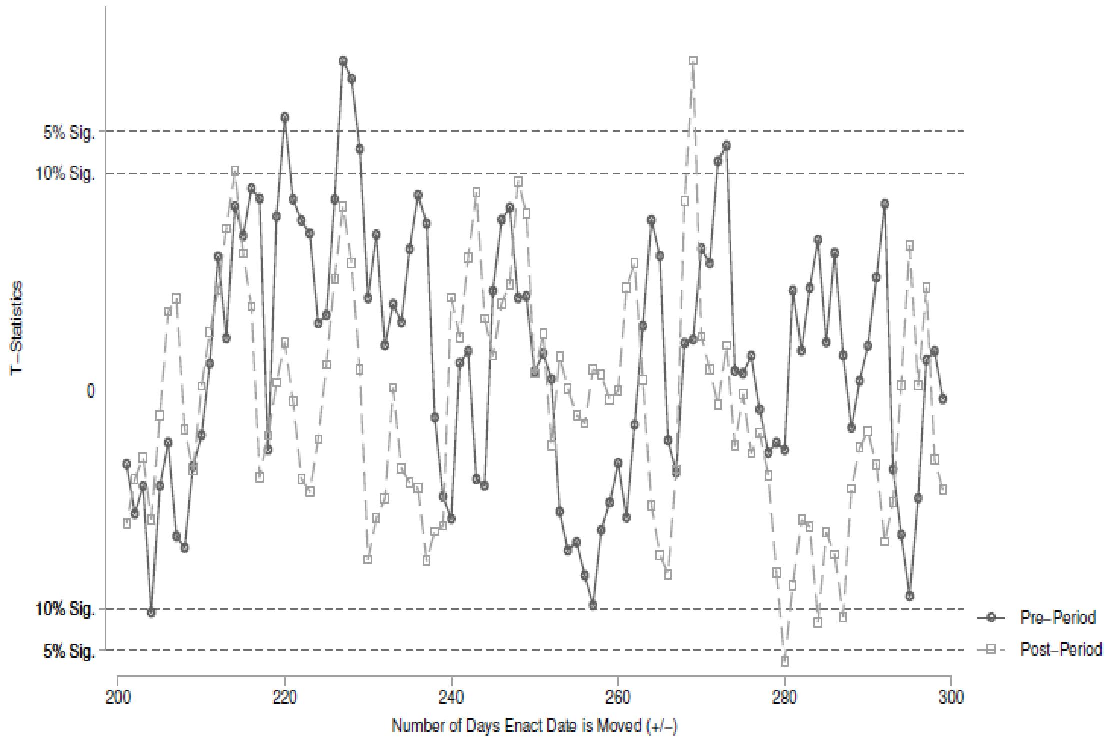
Notes: The outcomes in columns 1, 2, and 3 are total daily drunk traffic fatalities, log of total daily drunk traffic fatalities, and drunk fatality per 100,000 state population. The standard errors are clustered at the state by month level. Each regression utilizes a 60 day bandwidth around the implementation date. Columns 1 and 2 control for: population, holiday FE, and light and atmospheric conditions, while column 3 controls for all those variables except population.

dates are created on each side of the true cutoff and a bandwidth interval of 60 days is utilized around each of these placebo dates.[15] Since we are testing for the presence of a treatment effect across multiple placebo implementation dates, a Type I error rate that is less than 5% is favorable. This would foster a high degree of confidence that our model does a good job of failing to reject the null hypothesis under conditions where we expect it to be true. Figure 5 shows the results from these regressions. The solid line depicts the estimates for the placebo dates created in the pre-intervention period and the dashed line displays the estimates for the corresponding post-intervention period. In this graph, we focus on total daily traffic fatalities, but the same results hold for the other outcome. The graph highlights significant randomness in the estimated coefficients. From the 200 models we estimated, the null hypothesis was rejected five times, leading to a type I

[15]The original running variable is denoted by c_r and is normalized such that on the true effective date of the policy, $c_r = 0$. In the main specification, we utilized the bandwidth interval $c_r \in [-60,\ 60]$. The running variable for the placebo implementation dates is given by $c_{placebo_j} = c_r \pm d_j$, where $d_j \in [201,\ 300]$ and the corresponding bandwidth interval for each model is given by $[c_{placebo_j} - 60,\ \ c_{placebo_j} + 60]$.

For example, West Virginia handheld ban became effective on June 1, 2012. As such, this date would receive a value $c_r = 0$ and the bandwidth interval would range from April 2, 2012 to July 31, 2012. Let $d_j = 250$. This would create two new cutoffs when 250 is added or subtracted from c_r. Therefore, $c_{placebo_{250}} = 0$ on September 25, 2011 and $c_{placebo_{-250}} = 0$ on February 6, 2013. This should create two estimates where the null hypothesis of the treatment effect is true.

Figure 5: Policy Impact at Various Placebo Implementation Dates



error rate of 2% for positive coefficients and 0.5% for negative coefficients.[16]

Jointly, the evidence from the falsification checks suggests that our model is correctly specified and the RD treatment effect is unlikely being driven by unobserved confounders.

2.5.2 Sensitivity and Robustness Analysis

In this section, we present the results from several sensitivity and robustness checks to further bolster the credibility of our RD estimates. First, we show that the policy did not induce any major changes in law enforcement activities during the time period of interest. We then provide evidence that our results are robust to several alternative model specifications and various clustered standard error choices.

[16]The distribution of the T-Statistics is shown in Figure E.6 in the Appendix. The distribution is centered around zero and the proportion of values beyond 1.96 is very small in both tails.

Given that we are testing multiple hypotheses, the p-value can be adjusted using a family-wise error rate correction. Using several of these procedures (Bonferroni and the Holm-Bonferroni) strengthen our conclusion about the Type 1 error rate. This is expected since these corrections reduce the applicable p-value significantly (Romano and Wolf, 2005).

Table 15: Law Enforcement: Stops & Citations

	Stop	Citation
Handheld Law	0.08	0.02
	(0.08)	(0.03)
Day of Week FE	Yes	Yes
Day of Month FE	Yes	Yes
Month FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Mean	1105.53	554.37
SD	1840.40	914.10
N	605	605
Bandwidth Interval	[0,60]	[0,60]

Notes: Columns 1 and 2 show the impact of the policy on the log of daily total stops and citations issued in California, Illinois, Nevada, New Hampshire, and Vermont. The standard errors are clustered at the state by month level.

2.5.2.1 Law Enforcement

Law enforcement data that encapsulates the enactment date of the policy is only available for five of our 14 states of interest. This includes California, Illinois, Nevada, New Hampshire, and Vermont. We focus on the likelihood that handheld law changes induced police officers to stop or cite drivers more frequently. As shown in column 1 of Table 15, the handheld ban did not significantly impact the number of drivers police officers decided to stop. Additionally, column 2 shows that conditional on being stopped, the law did not increase the likelihood that police officers issued a citation. Consequently, this is suggestive evidence that police departments are not induced to adjust law enforcement efforts as a result of the policy change.

2.5.2.2 Bandwidth Size

In our main analysis, we utilized a two-sided bandwidth window of 60 days (two months). In this section, we examine the sensitivity of our results to the choice of the bandwidth size. The

bandwidth windows we consider range from 30 days (one month) to 90 days (three months). We combine the results in Figures E.1 and E.2 in the Appendix for ease of exposition, where each point on the graph represents a separate regression. The estimates in these figures are very consistent with the main results presented in Table 13. For instance, across the various bandwidth choices, the handheld policy significantly reduced traffic fatalities by 16%-39% (Figure E.1) or 0.39 to 0.70 individuals (Figure E.2) each day.[17]

2.5.2.3 Model Specification Checks

In Table 16, we show that the baseline estimates are robust to various alternative model specifications. The outcomes of interest are positioned in rows 1-3. To obtain the results in columns 1-6, we made the following adjustments: (i) removing day of month fixed effects, (ii) including an adjustment indicator variable (seven days), (iii) using a quadratic local polynomial, and performing a Donut RD that excludes (iv) one day, (v) two days, and (vi) three days around the enact date.

Conditional on day of the week and month fixed effects, one may be concerned that day of the month fixed effects do not provide much explanatory power, but it costs several degrees of freedom (Hansen, Miller, and Weber, 2017). In column 1, we explore the sensitivity of our main results to the inclusion of day of the month fixed effects. We find that removing this control leads to a moderate reduction in the point estimates, but had no impact on the standard error. However, the point estimates from our main results still fall within the confidence intervals obtained from this specification.

In column 2, we utilize a local quadratic polynomial regression to capture potential nonlinearities in the relationship between time and the outcome variable.[18] In general, the results indicate that this adjustment produced less precise, albeit sometimes larger estimates. In addition to our checks for changes in law enforcement activities (Section 4.2.1), column 3 includes an ad-

[17] Choosing a bandwidth size close to the enactment date results in a smaller sample size and produces noisier estimates. In contrast, those further away yield estimates that are more efficient but are likely biased upward.

[18] In the RD context, Card et al., 2014 noted that local linear estimators should not always be favored over alternative local polynomial estimators. However, Gelman and Imbens, 2019 cautions against the use of higher order polynomial regressions. As such, following the advice of Gelman and Imbens, 2019, we utilize a local quadratic polynomial regression as a check against the linear specification we presented in our main estimates.

Table 16: Sensitivity and Falsification Checks

	[1]	[2]	[3]	[4]	[5]	[6]
Total Fatalities	-0.50**	-0.61**	-0.62***	-0.62**	-0.69**	-0.42*
	(0.22)	(0.32)	(0.22)	(0.23)	(0.28)	(0.25)
Log Total Fatalities	-0.28***	-0.41***	-0.30***	-0.27***	-0.31***	-0.24***
	(0.05)	(0.09)	(0.05)	(0.06)	(0.07)	(0.07)
Day of Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Day of Month FE	No	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth Interval	[0,60]	[0,60]	[0,60]	[2,60]	[3,60]	[4,60]

Notes: The outcomes in rows 1 and 2 are total daily fatalities and the log of total daily fatalities.
The model specification in each column is adjusted as follows:
(i) removing day of month fixed effect, (ii) quadratic local polynomial, (iii) including adjustment indicator, (iv) Donut RD removing one day around enact date, (v) Donut RD removing two days around enact date, and (vi) Donut RD removing three days around enact date. Standard errors are clustered at the state by month level. Other controls: population, holiday FE, and light and atmospheric conditions.

justment indicator that takes a value of one seven days before and after the enactment date. As in Hansen, Miller, and Weber, 2017, this variable potentially absorbs the various local responses to the policy. Including this additional control has no material impact on the estimates and standard errors.

Another way that we assess the sensitivity of our baseline estimates to possible non-random sorting (e.g. policing) behaviors at the threshold is by using the donut RD design (Hausman and Rapson, 2018; Barreca et al., 2011). The estimates from these regressions are presented in columns 4-6 of Table 16. The results suggest that removing up to three days around the policy cutoff had a small impact on the estimated coefficient. Across the three models, the handheld policy was estimated to reduce average fatalities by 0.42-0.69 individuals or 21%-27%.[19]

2.5.2.4 Poisson and Negative Binomial Models

Our core outcome of interest, the number of fatalities each day, is a count variable that takes non-negative integer values. As such, we examine the robustness of our baseline results to the use of various count data approaches. We focus on the estimates from both the Poisson and negative binomial regression models.[20] The Poisson model is appropriate when the count variable occurs randomly over time with a constant probability. Conditional on the various time fixed effects, traffic fatalities may satisfy this condition over a short-time interval. While the negative binomial model has the same mean structure as the Poisson model, it has an additional parameter that accounts for over-dispersion in the data.

The count data model estimates are shown in columns 1 and 2 of Table E.2 Appendix. These models suggest that the policy reduced the expected number of fatalities by about 19%. This is lower than, but consistent with, the baseline estimate of 26%.[21] Consequently, employing models

[19]We also conducted Donut RD models removing the first three days, seven days, and 14 days of treatment. This allows us to assess the extent to which the magnitude of the baseline estimates are affected by responses in the early days of the treatment period (Hansen, Miller, and Weber, 2018). These results are presented in Table E.3 Appendix. This yields a similar conclusion.

[20]A Hurdle Poisson Model yielded similar estimates.

[21]In columns 1 and 2, we report the Poisson and negative binomial regression coefficients. To obtain the percentage change in traffic fatalities, we used the transformation $[\exp(\beta_1) - 1] * 100$.

that are more suitable for handling count data does not alter the main conclusion.

2.5.2.5 *Standard errors*

Statistical inference in this paper is robust to various assumptions regarding the correlation structure of the error term. In our main estimates, we allowed the day-level disturbances to be correlated within state-month clusters. In columns 3 and 4 of Table E.2, we relax this assumption and allow disturbances to be correlated within day of the week and state clusters, respectively. Finally, in column 5, we allow the disturbances to follow a first-order autocorrelation process. While these adjustments typically increased the standard errors slightly, they had no impact on the significance of the estimated parameter.

2.6 Long-Term Effect of Handheld Laws

In this section, we examine the long-term impact of handheld legislation on traffic fatalities. We first outline and estimate a Difference-in-Differences (DID) model to determine if the short-term results persist over time. Since the states in our sample implemented handheld legislation at different times, we utilize the Goodman-Bacon decomposition to estimate a weighted average of all possible 2x2 DID estimators in the data.

2.6.1 Difference-in-Differences

Up to this point, we have utilized a temporal RD design to evaluate the short-term impact of handheld mandates on fatal motor vehicle accidents. To investigate the long-term impact of handheld laws on the states that adopt this policy, we now employ the two-way fixed effects (TWFE) empirical approach. Specifically, we estimate the following DID[22] model:

$$F_{sym} = \alpha_0 + \alpha_1 Handheld_{sym} + \mathbf{X}_{sym}\beta_1 + \mathbf{X}_{sy}\beta_2 + \alpha_s + \alpha_{ym} + \mu_s.y + \varepsilon_{sym},$$

[22] We estimate another DID model where we interact the law with the time since inception to make it more comparable with the RD model. We present the results did not change. The results show that the law is associated with reductions in traffic fatality over time.

where F_{sym} is the traffic fatality that occurred in state s, year y, and month m. $Handheld_{sym}$ is our variable of interest, which takes the value 1 if state s has a handheld law in month m of year y and 0 otherwise. The vectors $\mathbf{X}_{sym}$ and $\mathbf{X}_{sy}$ contain the control variables for each state's economic condition, political affiliation, racial composition, alcohol and cigarette taxes, and marijuana laws. We also include state (α_s) and month by year (α_{ym}) fixed effects to account for time-invariant state characteristics and fatality trends, and a state-specific-linear time trend $\mu_s.y$ to control for the fatality trend within each state.

The policy effect in this model is a weighted average of all possible 2x2 DID estimates that compare states that enact handheld bans to those that do not and states that adopt the policy early to those that enact it late. The coefficient of interest, α_1, has a causal interpretation under the common trend assumption. This assumption stipulates that conditional on $\mathbf{X}_{sym}$, $\mathbf{X}_{sy}$, and the fixed effects, the changes in traffic fatalities would have evolved in the same way in the treated and untreated units in the absence of the policy.[23] Table 17 reports the DID estimates. Rows 1 and 2 show the policy's effect on total and the log of total traffic fatalities, respectively. Each model includes state, year, and month fixed effects. In column 1, which includes no additional controls, we found that the handheld law reduced traffic fatalities in treated states by 6.22 or 3.3% each month. In column 2, we add a linear state-specific time trends to account for differential trends in fatality across states. This additional adjustment increases the point estimate to 6.97 and 6.57%, respectively. This indicates that state-level trends in fatality is associated with the enactment of the policy, and not accounting for them would likely create a downward bias. Our preferred specification is presented in column 3, which adds the full set of control variables to the second model specification. These additional control variables account for the main differences in the attributes of treated and control states that may be correlated with the adoption of the law. The resulting estimates suggest that the policy reduced motor vehicle fatalities by 5.72 or 5.54% on average each month.

The long-term policy effect is similar to the short estimates from the RD model. On average,

[23]The DID sample is comprised of 50 states over the period 2000-2015, which leads to 9,600 observations at the state by year by month level.

Table 17: Conventional DID Estimates

	Handheld	Handheld	Handheld
Total Fatalities	-6.22***	-6.97***	-5.72***
	(0.96)	(1.08)	(1.05)
Log Fatalities	-0.034***	-0.068***	-0.057***
	(0.01)	(0.02)	(0.02)
Fatalities Per 100,000 State Population	0.05***	-0.0435***	-0.04**
	(0.01)	(0.018)	(0.018)
Observation	9600	9600	9600
Controls	No	No	Yes
State-Time Trend	No	Yes	Yes
State FE	Yes	Yes	Yes
Month x Year FE	Yes	Yes	Yes

Notes: The outcomes in rows 1, 2, and 3 are total monthly fatalities, the log of total monthly fatalities, and total monthly fatalities per 100,000 state population. Each of the regression controls for: minimum wage, state unemployment level, log of income per capita, state population, political party of the state government, share of Black population, share of Hispanic population, wine tax, beer tax, cigarette tax, minimum legal age sale, recreational marijuana law, medical marijuana law, decriminalization of marijuana law, light and atmospheric conditions.

26% fewer fatal daily motor vehicle accidents is equivalent to a 26% monthly reduction in traffic related casualties. Therefore, the 5.54% monthly reduction in fatal accidents which occurs over a longer time horizon represents one-fifth of the short-term policy impact.[24] This result indicates that the handheld legislation is still effective at curbing traffic fatality in the long-term, although the effect is significantly smaller. The reduction in the estimated impact of the handheld ban on fatal motor vehicle accidents in the long-run is likely due to individual behavioral effects, where drivers are less inclined to comply with the policy over time (McCartt et al., 2010).

Jointly the DID estimates for both outcomes show that handheld mandates are effective at curbing traffic-related casualties in the long-term. When comparing the DID estimates with the short-term impacts from the RD design, we find that the long-term effects are about one-fifth to one-third of the policy's short-term impact, suggesting that there is some fade-out of the impact over time (McCartt et al., 2010).

2.6.2 DID Decomposition

The two-way fixed effects model presented above relies on the parallel trend assumption to identify the causal impact of the policy. Goodman-Bacon, 2018 notes that when units receive treatment at different times, the two-way fixed effects DID estimate is a weighted average of all possible 2x2 DID estimates. He further argues that this conventional estimate is biased away from the true parameter when the treatment effect changes monotonically over time.[25] Goodman-Bacon, 2018 proposes a decomposition method that groups the 2x2 DID estimates into four categorizes:

(1) early treated vs late treated; (2) late treated vs early treated; (3) early treated vs untreated; and (4) late treated vs untreated. As such, the two-way fixed effects estimate is a weighted average of (i) the 2x2 DIDs that compare early adopters (treated) to later adopters (control) over the periods when later adopters are not yet treated, (ii) the 2x2 DIDs that compare early adopters (control) to later adopters (treated) over the periods when the early adopters are treated, and (iii) the 2x2 DIDs

[24] Note that an overall reduction of 5.72 each month is equivalent to a daily reduction of about 0.2. As such, the results for total fatalities over the long-term (0.2) is approximately one-third of the estimates we found in the RD design (0.63).

[25] This is because the units that are treated earlier become apart of the control group for later adopters.

Table 18: Goodman-Bacon DID Decomposition, Total Fatalities

	Total Fatalities	Total Fatalities	Total Fatalities
Overall	-6.22***	-6.97***	-5.72***
Timing	-9.52	-7.56	-7.11
	[0.14]	[0.23]	[0.23]
Never	-5.70	-6.79	-6.35
	[0.86]	[0.77]	[0.75]
Within	88.5	-52.40	23.25
	[0]	[0]	[0.03]
Observation	9600	9600	9600
Controls	No	No	Yes
State Time Trend	No	Yes	Yes
State FE	Yes	Yes	Yes
Month x Year FE	Yes	Yes	Yes

Notes: The outcomes in columns 1-3 are the log of total monthly fatalities. Whenever necessary, each regression controls for: minimum wage, state unemployment level, log of income per capita, state population, political party of the state government, share of Black population, share of Hispanic population, wine tax, beer tax, cigarette tax, minimum legal age sale, recreational marijuana law, medical marijuana law, decriminalization of marijuana law, light and atmospheric conditions. The decomposition weights are included in square brackets. The mean of total fatalities is 63.62.

that compare early or late adopters (treated) to the units that never adopt the policy (control).

We perform a Goodman-Bacon decomposition of the TWFE estimates in Table 18. The decomposition results for total and log total fatalities are reported in Table E.4 Appendix, respectively. In each table, the DID estimates are decomposed into three groups and presented with their associated weights. The Timing group shows the weighted average of the 2x2 DIDs that compares the late to early adopters and the early to late adopters of handheld laws. The Never group summarizes the DIDs that compare treated states to states that did not adopt the policy over the sample period. Lastly, the Within group captures the importance of the control variables that are included in the model. Similar to Table 17, all columns in Table 18 include state and time fixed effects, a state-specific-linear time trend is added in the second column, and we introduce several state-level control variables in the third column.

In Table 18, column 1, the decomposition indicates that 86% of the overall policy impact is determined by the Never category, 14% comes from the Timing category, and the Within category has a zero weight. In addition, there is a large gap in the average DID estimates across the Timing and Never groups in this naive specification. When state-specific-linear time trends are added to the model in column 2, this changes the results in two important ways: (i) it significantly closes the gap between the Timing and Never categories and (ii) it slightly increases the weight for the Timing group relative to the Never group, with no change in the weight for the Within category. Finally, when various state-level covariates are added to the model, the main results are not significantly altered. The results in column 3 indicate that the Never category remains the primary determinant of the overall policy impact (75%), the Timing category contributes about 23%, and the Within category now accounts for about 3%. In addition, while the inclusion of state-level controls slightly lowers the average estimates in the Timing and Never categories, it has no impact on the gap between the two groups.[26]

Consequently, the results from the Goodman-Bacon decomposition suggests that the composition of the control group has a negligible impact on the estimated effect of handheld laws. For

[26]Figures E.3 and E.4 in the Appendix show how the 2x2 DID varies across the three models in the Timing and Never groups.

instance, in our preferred specification, the policy impact is -7.11 or 5.92% when early adopters are compared to late adopters and -6.35 or 6.39% when treated states are compared to states that never enact the policy. The decomposition further illustrates that the inclusion of a very comprehensive set of control variables does not significant change the estimated coefficients.

To assess the parallel pre-trends assumption, we utilize the event study design (Clarke and Schythe, 2020). Under this approach, the leads and lags compare the difference in traffic fatalities between treated and control states to the prevailing difference in the period before the intervention (base month, $t = -1$). The results are presented in Figure E.5 in the Appendix. The figure shows that relative to the base month , there is no difference in traffic fatalities between treated and untreated states in the pre-treatment period. However, in the months immediately after this policy is adopted, there is a notable reduction in traffic fatalities. In addition, there is also some evidence that the effectiveness of the policy may attenuate over time.

2.7 Discussion and Conclusion

Distracted driving is one of the leading causes of traffic fatalities. It accounts for about 15% of the overall societal harm caused by traffic accidents. As such, this is an important public policy issue that has attracted the attention of lawmakers across several states. This concern has led to various mandates restricting the use of cellphones while operating a motor vehicle. In this paper, we evaluate the short- and long-term impact of these mandates (handheld laws) on traffic fatalities.

Using the Temporal RD design, we find robust evidence that the states that implemented handheld laws reduced daily traffic fatalities by approximately 26% or 0.63 deaths in the short-term. Unlike many studies in the literature, we show that these estimates are not sensitive to several robustness and falsification checks. In addition, using the DID design, we found that handheld laws are effective at curbing traffic-related casualties in the long-term, although there is some fade-out of the impact over time. A comparison of the DID estimates and the results from the Temporal Regression Discontinuity design suggests that the long-term effects are about one-fifth to one-third of the short-term impact of the policy.

The estimates in this paper are congruent with the main findings from several studies in the literature. For example, Anyanwu, 2012, Lim and Chi, 2013, and Sampaio, 2014 found that enacting a handheld law reduced overall traffic fatalities by 7%-9%. Similarly, Abouk and Adams, 2013 found that a universally enforced texting ban reduced traffic fatalities by 13% when there is also a handheld mandate in place. Several potential factors explain the quantitative differences between our findings and the literature. First, McCartt, Kidd, and Teoh, 2014 highlighted several empirical issues that may have biased the results of previous studies[27]. Second, Abouk and Adams, 2013 showed that these types of policies may have a heterogeneous impact across states. Our study covers more treated states than any previous work in the literature. Finally, we showed that while the policy has a large impact in the short term, it may experience some fade-out over time due to reduced compliance (McCartt et al., 2010). Abouk and Adams, 2013 also found larger point estimates when they restricted their analysis to a shorter time period and they argued that this is likely due to an announcement effect.[28]

These estimates suggest that handheld laws may be an effective tool to combat the adverse consequences of distracted driving. A simple back-of-the-envelope calculation suggests that by implementing this policy, the average state in our sample was able to prevent about 230 fatal crashes each year in the short term and at least 69 fatalities each year in the long-term.[29] Consequently, if this law is applied universally, it could have a significant impact on the number of fatalities caused by distracted driving.

[27]Most of these issues are related to the control utilized in those studies. For more details, please see footnote 60.

[28]McCartt et al., 2010 showed that handheld bans reduced phone use up to seven years after implementation, but the level of compliance decreased during this period. As such, if less distracted driving is the primary mechanism explaining our results, the estimated impact of the program could persist into the long-term although it may experience some fade-out.

[29]This assumes that the treatment effect of the law remain constant up to a year after implementation.

APPENDIX A Chapter 1 Tables

Table A.1: Choice Set Restriction to Hospitals Within the State of Residence

	Main Specification	Choice Set Restriction
Distance Metrics		
Distance	-0.126***	-0.133***
	(0.000183)	(0.0002)
Ghost County Dummy	-1.437***	-1.638***
	(0.0195)	(0.02)
Hospital Quality Metrics		
Obstetric beds	0.00387***	0.0048***
	(0.00061)	(0.000642)
Bassinets (beds for babies)	0.00521***	0.00485***
	(0.00063)	(0.000658)
Neonate Intensive Care Unit (NICU)	-0.38***	-0.37***
	(0.0134)	(0.014)
High-risk*NICU	0.769***	0.79***
	(0.0211)	(0.0224)
Accredited Hospitals	0.0573***	0.0572***
	(0.00065)	(0.0068)
Major Teaching Hospitals	-0.738***	-0.85***
	(0.0529)	(0.059)
Minor Teaching Hospitals	-0.105***	-0.106***
	(0.0098)	(0.01)
Public Hospitals	0.0724***	0.0753***
	(0.0089)	(0.0094)
Non-Profit Hospitals	0.157***	0.164***
	(0.0084)	(0.0088)
Hospital Low Quality Metrics		
Critical Access Hospitals	-0.144***	-0.133***
	(0.009)	(0.0098)
Sole Community Hospitals	0.009	0.0069
	(0.0106)	(0.011)
Birth-Hospital Observations	113,488,826	108,949,273
Births	6,039,936	5,795,174

Notes: Author's analysis using Vital Statistics & American Hospital Annual Survey Data 2007-2017.

Table A.2: Descriptive Statistics by Hospital Teaching Status and Rurality

	Rural Teaching	Rural Non-Teaching	Urban Teaching	Urban Non-Teaching
Hospital Characteristics				
Obstetric Beds	10.45	4.6	19.7	7.2
	(10.7)	(6.62)	(21.6)	(12.9)
Bassinets (beds for babies)	11	5.2	18.1	7.9
	(10.05)	(6.8)	(19.4)	(12.2)
Neonate Intensive Care Unit	0.13	0.027	0.40	0.24
	(0.33)	(0.16)	(0.49)	(0.43)
Accredited Hospitals	0.71	0.5	0.86	0.73
	(0.45)	(0.49)	(0.34)	(0.41)
Public Hospital	0.19	0.33	0.22	0.15
	(0.39)	(0.47)	(0.41)	(0.35)
Non-Profit Hospital	0.74	0.5	0.63	0.39
	(0.43)	(0.5)	(0.48)	(0.49)
Critical Access Hospital	0.19	0.43	0.02	0.13
	(0.39)	(0.49)	(0.13)	(0.29)
Sole Community Hospital	0.15	0.09	0.01	0.01
	(0.36)	(0.27)	(0.1)	(0.08)
Maternity Providers with Admitting Privileges	31.89	15	110	45.5
	(37.7)	(19.4)	(126)	(74.7)
Community Characteristics				
Unemployment rate	6.91	7.5	6.3	6.7
	(2.8)	(3.1)	(2.1)	(2.23)
Medicaid reimbursement rates for Obstetrics Care ($)	1,282	1,321	1,346	1,344
	(286)	(246)	(313)	(264)
Medicaid Expansion States	0.38	0.16	0.34	0.14
	(0.48)	(0.37)	(0.47)	(0.32)
Certificate of Need Law	0.79	0.72	0.76	0.715
	(0.40)	(0.44)	(0.43)	(0.42)
Percent of the Sample	0.0431	0.4143	0.1295	0.4131

Notes: Author's analysis using Vital Statistics & American Hospital Annual Surveys Data and a 5% sample.

APPENDIX B Chapter 1 Glossary

ACA Affordable Care Act

CAH Critical Access Hospital

CHIPRA Children Health Insurance Reauthorization Act

CON Certificate of Needs Law

UCO Unborn Child Option

SCH Sole Community Hospital

MaTH Major Teaching Hospital

MiTH Minor Teaching Hospital

NICU Neonate Intensive Care Unit

SD Standard Deviation

WTT Willingness To Travel

APPENDIX C Chapter 1 AHA Data-Cleaning Procedures For Missing Values in Responding Hospitals

In the AHA data covering the period 2007-2017, over 20% of the hospital reported missing values for the number of Neonate Intensive Care Unit beds. As it has been argued in Freedman, Lin, and Simon, 2015, a hospital facility will often report missing values for the number of NICU beds for a particular year while reporting the same number of NICU beds before and after the missing. I carry forward the number of beds and fill almost all the missing values. Additionally, some of the remaining missings were filled with the previous year's values, regardless of the same number of beds before and after missing. For the remaining (0.98% of the total observation) missing values, if the hospital facility has zero obstetric beds and zero bassinets for a specific year, I impute zero for the number of NICU beds. This approach is effective for several reasons, including the fact that it takes time for hospitals size to change and the fact that I only consider an indicator variable of any NICU beds for our measure of quality.

For my accreditation indicator, I had the same type of reporting issues. About one-third of the hospitals had missing values for whether or not they were accredited by the Joint Commission for a specific year while reporting the same non-missing value one year before and one or several years after. Given that it unlikely that hospitals' accreditation status will be changing back and forth every other year, I fill the missing values by carrying forward the accreditation status. Over the sample period, 66% of the hospitals were accredited by the Joint Commission, which is similar to the 65% found in Lam et al., 2018. I also use the same methodology to solve some of the missing values of obstetric beds. However, when the number of obstetric beds is different for the year before and after a missing value, I impute the average hospital obstetric beds.

APPENDIX D Chapter 1 The Case of Texas

As reported earlier, one limitation of this study is that the restricted version of the Vital Statistics [30] birth records that I have does not report hospital and patient addresses. To address this limitation, I did the following: first, I use a centroid-based distance measure, and second, I assign probability weight to hospitals using a maximum likelihood estimation method instead of a simple conditional logit (fixed-effect logit model). Although necessary given the data insufficiency, the centroid-based distance approach introduces several measurement issues. Own county births will have an underestimate measure of distance. Travel distance for deliveries in other counties by mothers who live on the border (far side) will be underestimated. Travel distance for mothers living on the near side of the border will likely be overestimated. Though I include several hospital characteristics in the model, these measurement errors may be problematic if correlated with unobserved hospital attributes.

I hope to improve on these limitations and test the robustness of my estimation method using the universe of Texas birth records over 2007-2017, which is available from the Texas Department of State Health Services. This data includes the mother's residential address and birth hospital information. To avoid selection biases caused by non-resident giving birth in Texas because of the quality of Texas' hospitals, I will remove non-resident[31] from the sample. I will also remove Texans giving birth elsewhere from the sample. I will estimate the model using a conditional logit model to the data.

[30] I am in the process of initiating an IRB application to have access to the Texas Vital Statistics data.

[31] There are three groups of people giving birth in Texas. 1) Texans giving birth in Texas, 2) Texans giving birth elsewhere because of hospital quality or not, and 3) non-resident giving birth in Texas because of hospital quality or not. It is not possible from the data to know the specific reasons a patient gives birth in Texas or elsewhere.

APPENDIX E Chapter 2 Tables and Figures

Figure E.1: Effect of Handheld Law on Total Traffic Fatalities

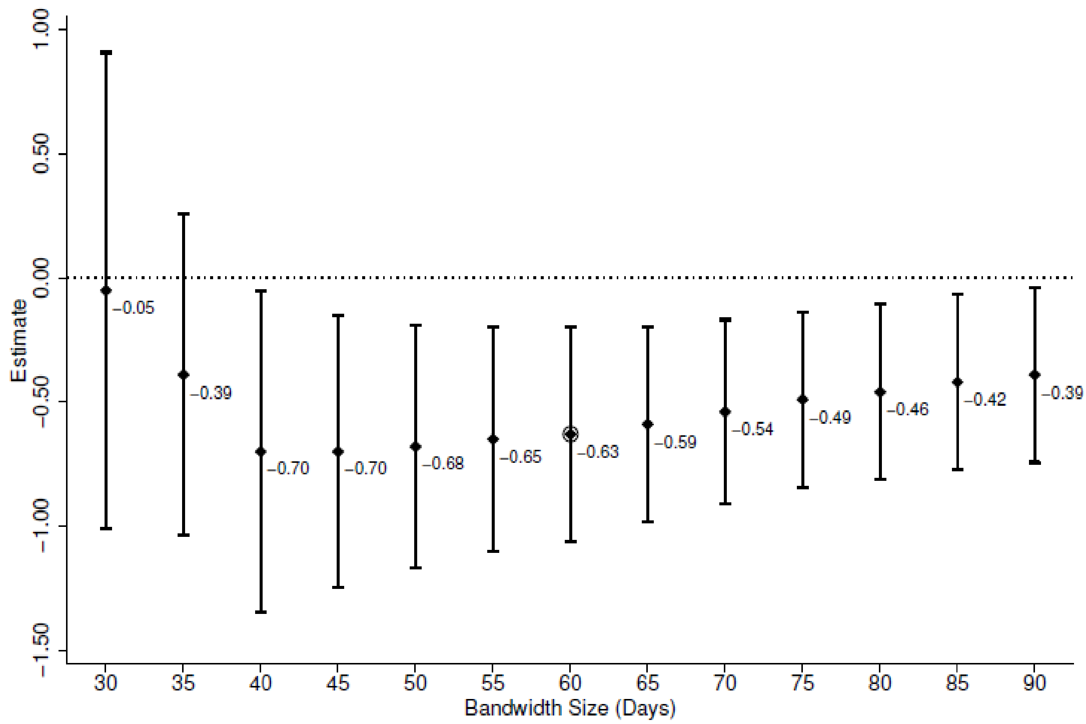


Figure E.2: Effect of Handheld Law on Log Traffic Fatalities

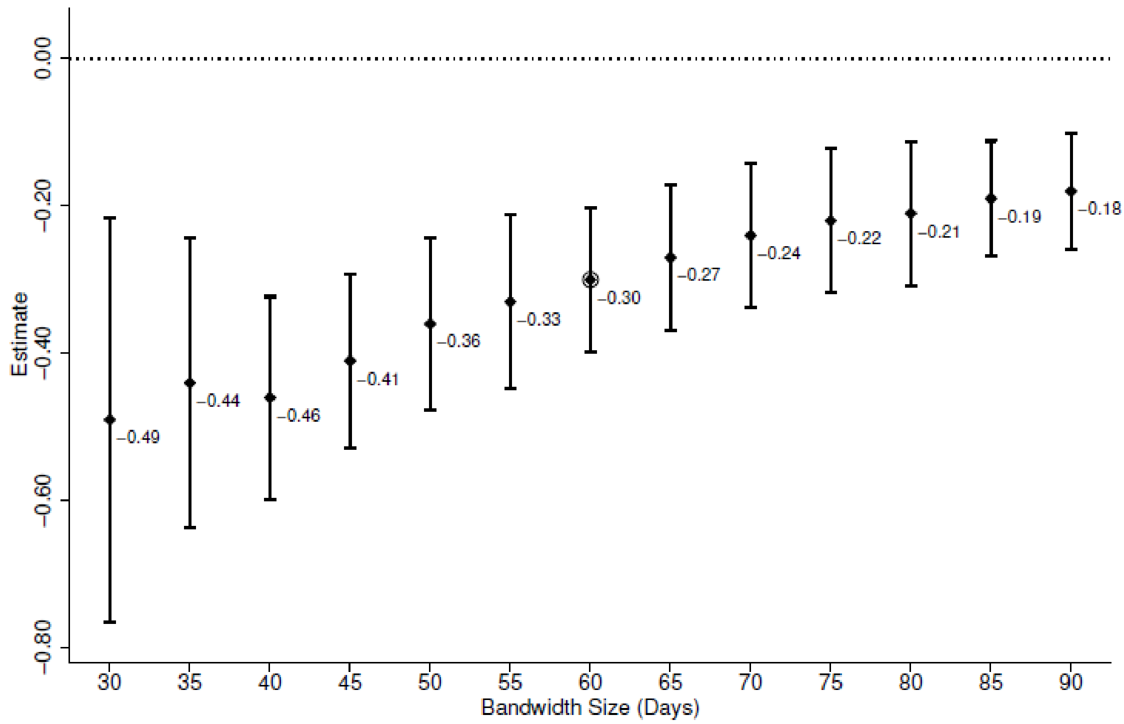


Figure E.3: Goodman-Bacon Decomposition, 2X2 DIDs and Weights

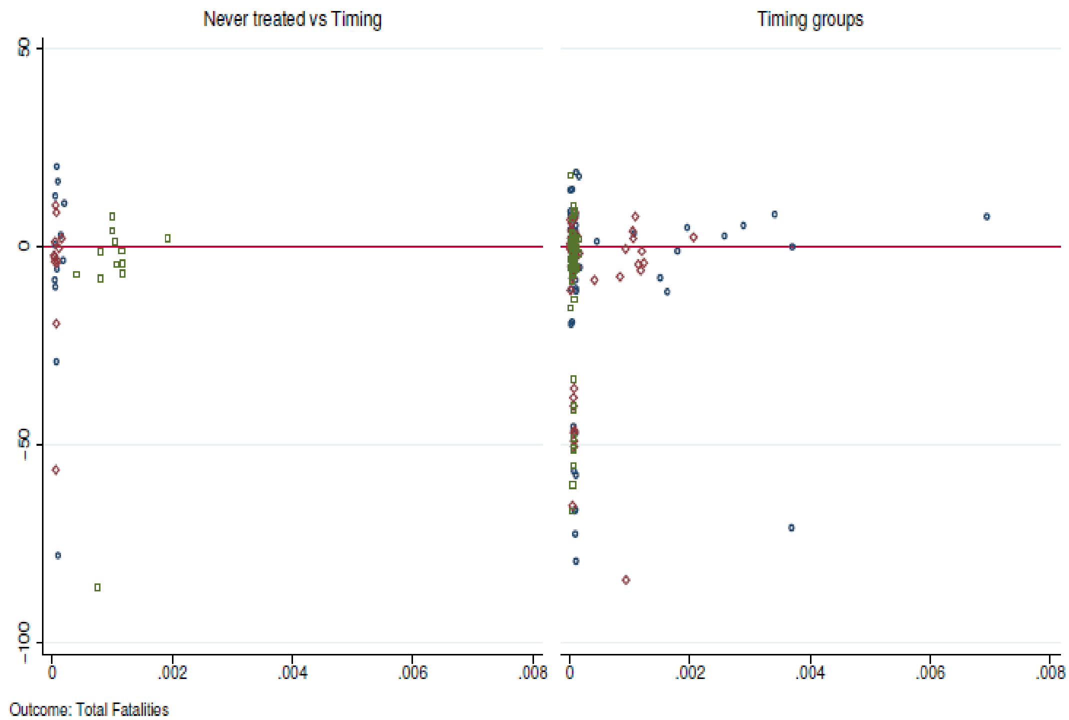


Figure E.4: Goodman-Bacon Decomposition, 2X2 DIDs and Weights

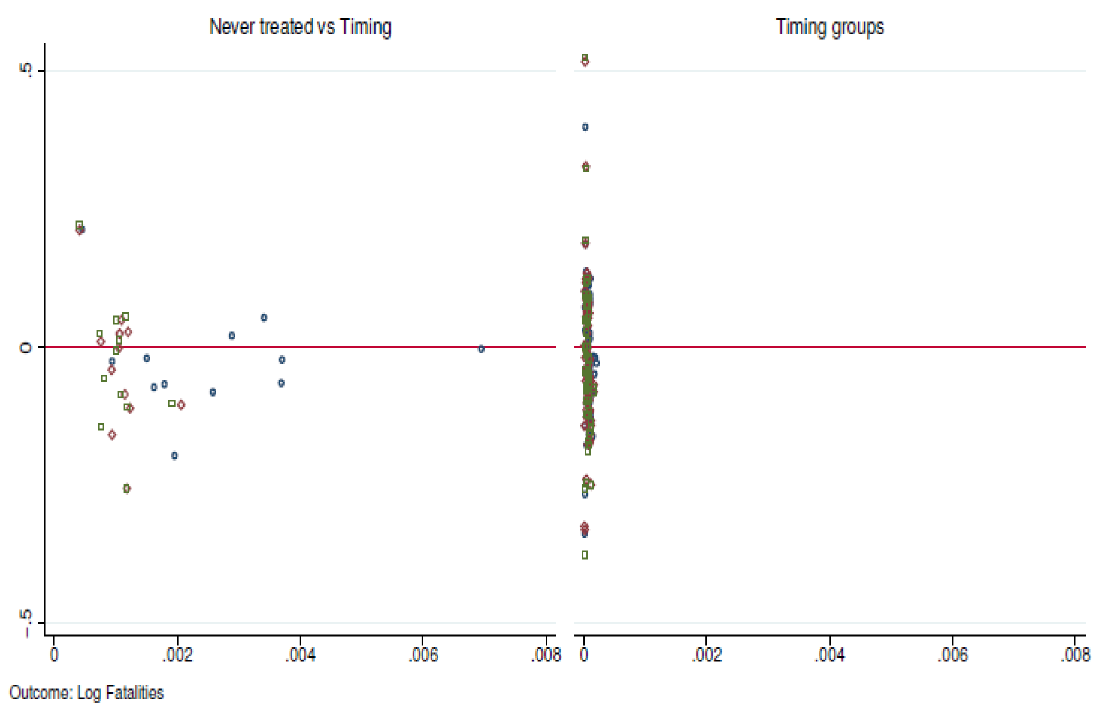
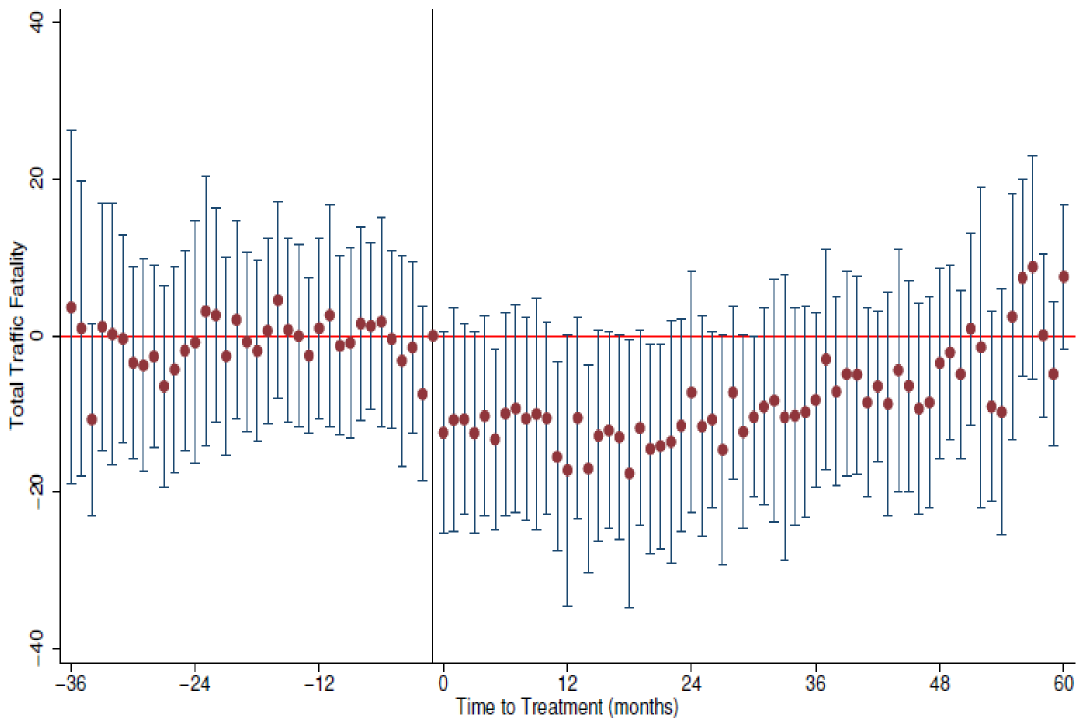


Figure E.5: Event Study, Total Traffic Fatalities



Chapter 2 Online Appendix

Table E.1: Impact of Handheld Law on Fatal Accidents

	Number of Accidents	Total Accidents per 100k Population
Handheld Law		
	-0.54***	-0.010***
	(0.18)	(0.003)
Day of Week FE	Yes	Yes
Day of Month FE	Yes	Yes
Month FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Mean	1.59	0.024
SD	2.63	0.038
N	1,694	1,694
Bandwidth Interval	[-60, 60]	[-60, 60]

Notes: The outcomes in columns 1 and 2 are total daily traffic accidents and the total daily traffic accidents per hundred thousand state population. The standard errors are clustered at the state by month level. Each regression utilizes a 60 day bandwidth around the implementation date. Other controls: population, holiday FE, and light and atmospheric conditions.

Table E.2: Alternative Estimation Approaches and Clustering Choices

	Neg. Binomial	Poisson	Cluster (DOW)	Cluster (St)	PCSE
Handheld Law	-0.21***	-0.20***	-0.63***	-0.63**	-0.63**
	(0.08)	(0.08)	(0.23)	(0.30)	(0.32)
Day of Week FE	Yes	Yes	Yes	Yes	Yes
Day of Month FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Bandwidth Interval	[0,60]	[0,60]	[0,60]	[0,60]	[0,60]

Notes: The table shows the results from various estimation approaches and standard error adjustments. For all columns, the outcome of interest is total daily traffic fatalities. Columns 1-5 include the results for the following model specifications: (i) Negative Binomial Model (ii) Poisson Model (iii) Clustering SE at the day-of-week level (iv) Clustering SE at the state level and (v) Panel-Corrected Standard Errors. Other controls: population, holiday FE, and light and atmospheric conditions.

Table E.3: Additional Donut RD Specifications

	[1]	[2]	[3]
Total Fatalities	-0.74***	-0.68***	-0.77***
	(0.2)	(0.21)	(0.26)
Log Total Fatalities	-0.28***	-0.25***	-0.25**
	(0.05)	(0.07)	(0.1)
Day of Week FE	Yes	Yes	Yes
Day of Month FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

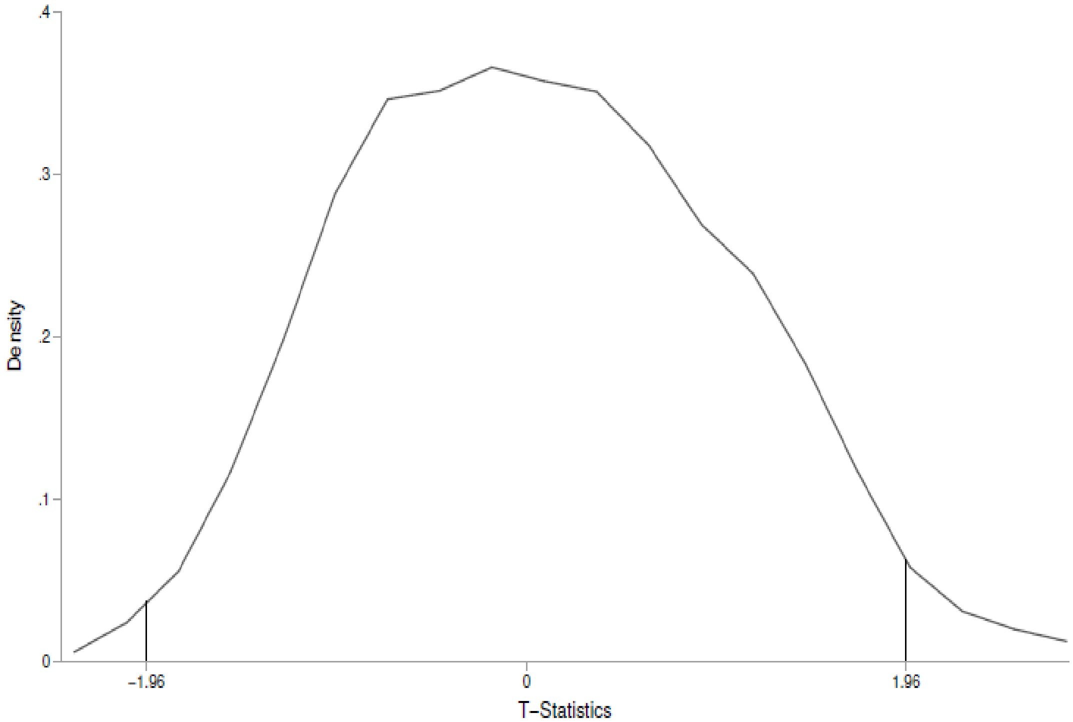
Notes: The outcomes in rows 1 and 2 are total daily traffic fatalities and the log of total daily fatalities.The various columns show the results of a Donut RD model removing:
(i) three days, (ii) seven days, and (iii) 14 days post treatment.

Table E.4: Goodman-Bacon DID Decomposition, Log Fatalities

	Log Fatalities	Log Fatalities	Log Fatalities
Overall	-0.034***	-0.068***	-0.057***
Timing	-0.041	-0.065	-0.061
	[0.14]	[0.23]	[0.23]
Never	-0.033	-0.069	-0.066
	[0.86]	[0.77]	[0.75]
Within	0.71	0.17	0.24
	[0]	[0]	[0.03]
Observation	9600	9600	9600
Controls	No	No	Yes
State Time Trend	No	Yes	Yes
State FE	Yes	Yes	Yes
Month x Year FE	Yes	Yes	Yes

Notes: The outcomes in columns 1-3 are the log of total monthly fatalities. Whenever necessary, each regression controls for: minimum wage, state unemployment level, log of income per capita, state population, political party of the state government, share of Black population, share of Hispanic population, wine tax, beer tax, cigarette tax, minimum legal age sale, recreational marijuana law, medical marijuana law, decriminalization of marijuana law, light and atmospheric conditions. The decomposition weights are included in square brackets.

Figure E.6: Distribution of T-Statistics for Placebo Dates



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VITA

Ernest Dorilas was born and raised in rural Haiti and earned his Bachelor's in Economics from the State University of Haiti (CTPEA) in 2011. He worked at the Clinton Foundation in 2012 and at the Ministry of Economy and Finance from 2012 to 2015. In 2015, he was second in a six-month competition organized by the Central Bank of Haiti that put together 20 candidates (valedictorian) from the country's top universities. He also taught several courses such as Descriptive Statistics, Probability, and Inferential Statistics in two universities (FDSEG and INUQUA) in Haiti.

He joined Georgia State University (GSU) in 2016 and earned a Master's of Arts in Economics in the summer of 2017. In this program, he received the 2017 Mark E. Schaefer Graduate Fellowship for the highest Grade Point Average (4.11 out of 4.3). In the Fall of that same year, he started a Ph.D. in Econometrics and Quantitative Economics in the same school and successfully defended his dissertation in November 2021. While at GSU, he led the Principles of Macroeconomics class in the Fall of 2020 and assisted in several other courses. During the program, he gleaned several honors and awards, including the 2021 Dissertation Fellowship CEAR Scholar, the 2020 Oustanding Graduate Assistant Award, the 2020 American Society of Health Economists Diversity Scholarship, and the 2019 Annual Health Econometrics Workshop Scholarship.

Ernest also participated in several important workshops, including the one at the National Bureau of Economics (NBER) in 2019. His co-authored traffic fatalities paper with Dr. Nicholas A. Wright was cited by several major news outlets, such as the Wall Street Journal and USA Today. He recently published a paper at Health Affairs and has another one currently under review at the Journal of Health Economics.

Ernest uses quasi-experimental econometrics methods to study issues in Health Economics, Labor, and Public Economics. He accepted a job offer from Cone Health in North Carolina as a Health Economics Analyst and is working on creating a research consulting firm to draw accurate and data-driven conclusions to inform decision-making and actions.