ESSAYS ON THE ECONOMICS OF POPULATION HEALTH

BY

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DISSERTATION

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ABSTRACT

This dissertation consists of three essays on the economics of population health. In Chapter 2, I study employment shocks and demand for pain medication. Declining economic opportunity is often portrayed as one of the drivers of the opioid epidemic. Better employment conditions can, however, affect opioid use through two channels: increasing physical pain from working or reducing mental distress that can contribute to substance abuse. I use a large dataset of opioid and over-the-counter (OTC) painkiller sales to measure the effect of employment shocks on demand for pain medication. To separate the channels, I contrast the effect of labor demand shocks on the use of opioids with the effect on the use of OTC painkillers—which address pain but not mental health—allowing for the effects to depend on the injury rate of local industries. I find that a 1 percent increase in the employment-to-population ratio decreases the per-capita demand for opioids by 0.20 percent, while it increases the per-capita demand for OTC painkillers by 0.14 percent. To decompose the effect of employment on opioid use in the two channels, I calculate the substitution between these pain medications, exploring the introduction of a policy that increased requirements to prescribe opioids. My findings show that during local economic expansions, the decline in opioid abuse is 40 percent larger than the total effect on use while, at the same time, the demand for pain relief medication increases and is related to jobs in high injury industries.

In Chapter 3, I study how women learn they are pregnant and pregnancy uncertainty. The earlier a woman learns about her pregnancy status, the sooner she can make decisions about her own and infant's health. This paper examines how women learn about their pregnancy status and measures how access to pregnancy tests affects earlier pregnancy knowledge. Using ten years of individual-level monthly panel data in Nepal, we find that, on average, women learn they are pregnant in their 4.6th month of pregnancy. Living approximately

a mile further from a clinic offering pregnancy tests increases the time a woman knows she is pregnant by one week (5% increase) and decreases the likelihood of knowing in the first trimester by 4.5 percentage points (16.1% decrease). Women with prior pregnancies experience the most substantial effects of distance within the first two trimesters, while, for women experiencing their first pregnancy, distance does not affect knowledge. This difference suggests that access to pregnancy tests is a binding constraint only after women's beliefs, or symptoms, about being pregnant are strong enough.

In Chapter 4, I study how election outcomes affect alcohol drinking. The growing political polarization and the increasing use of social media have been linked to straining social ties worldwide. The 2016 presidential elections in the United States reflected this trend, with reports of fear and anxiety among voters. We examine how election results can be linked to episodes of anxiety and the use of alcohol for self-medication. We analyze a daily dataset of household purchases of alcohol in the weeks following presidential elections. We find that, within 30 days from Election Day, a 10 percentage point increase in support for the losing candidate increases alcohol expenditure by 1.1%. The effect is driven by counties with more immigrants, higher income, higher unemployment, and higher levels of education. Suggestive evidence shows that the number of fatal car crashes also increases in counties with a higher share of losers. These two effects are present in the 2016 elections and absent in previous years.

To all the strong and smart women who inspired me to write this dissertation.

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I am depositing this dissertation in the middle of a worldwide disease outbreak. I feel lucky to be safe right now and, more than ever, grateful for this amazing support network.

TABLE OF CONTENTS

CHAPTER 1

INTRODUCTION

Population health refers to health outcomes and its determinants, including policies to improve health outcomes (Kindig and Stoddart, 2003). Population health is key to economic development; it has been largely connected to income (Bloom and Canning, 2000; Chetty et al., 2016), and it is regarded not only as a measure of welfare on its own but also as a component of human capital (Grossman, 1972).

The positive correlation between income and health is a stylized fact in economics, portrait in what is known as the Preston curve (Preston, 1975). Understanding the details of this relationship is an area of active discussion, including the direction of causality and the mechanisms connecting health to income (Deaton, 2003; Bloom and Canning, 2007).

In Chapter 2, *"Employment Shocks and Demand for Pain Medication,"* I speak directly to this literature, focusing on how fluctuations in income affect health. Temporary changes in income can have multiple effects on health. On the one hand, following a traditional economic model, a higher income shifts out the budget constraint and is expected to improve health (e.g., due to higher consumption of medical care). On the other hand, a decline in health is expected due to other channels, such as changes in risky health behaviors and effects of the production of goods and services into health itself (Ruhm, 1996).

Part of the challenge in identifying the effect of economic fluctuations on health hinges on differentiating physical and mental health. Most of the literature agrees that income is positively correlated with mental health, mentioning the large negative effects of a sudden drop in income on psychological distress (Charles and DeCicca, 2008; Browning and Heinesen, 2012). However, there is still a large debate on the effect of income fluctuations on physical health (Ruhm, 2015; Miller et al., 2009).

In Chapter 2, I explore how the type of jobs suffering economic fluctuations can affect differently physical health, focusing on the differential in the likelihood of getting injured in the job across industries. Despite early developments of the idea of a procyclical effort channel connecting income to physical health (Rosen, 1986), most of the evidence in the area is still correlational (Hummels, Munch and Xiang (2016) is an exception).

In the remaining chapters of the dissertation, I focus on the determinants of population health. Such determinants include factors that can affect health in any stage of life, such as nutrition, medical technology, and public health (Cutler, Deaton and Lleras-Muney, 2006). Especially in developing countries, barriers to access health care and its technologies, such as clinics and medications, are a constraint to optimal care (Kremer, 2002). Policies that affect fetal and child health can have lasting effects on health and human capital (Miguel and Kremer, 2004; Almond and Currie, 2011), and be an important determinant of adult health (Case, Fertig and Paxson, 2005).

In Chapter 3, *"How do Women Learn They Are Pregnant? The Introduction of Clinics and Pregnancy Uncertainty in Nepal,"* we discuss how access to clinics with the technology of pregnancy tests affects the timing women report their pregnancy. In addition to the potential effect of access to this technology on child and adult health outcomes due to an earlier pregnancy detection, Chapter 3 speaks to the literature on demand for tests and information on health status. A person's knowledge of her health status can be a barrier to improved health outcomes through access to care and change in behaviors (Thornton, 2008; Oster, 2017).

Health behaviors are another important health determinant. It includes several choices people make that can affect their health outcomes (Becker, 2007). Specifically, risky health behaviors are choices, such as to drink or smoke, that can have a negative impact on health. In the last decades, with a decline in self-reported physical and mental health and an increase in deaths due to overdose among Americans, understanding risky health behaviors and their connection to mental health has become even more relevant (Case and Deaton, 2015).

In Chapter 4, *"The Effect of Presidential Election Outcomes on Alcohol Drinking,"* we analyze the increase in alcohol consumption in response to election results—a stress-triggering event. Alcohol and other drugs are used as an off-label strategy to deal with mental distress, which can be a rational choice in the short-term (Rigg and Ibañez, 2010; Darden and

Papageorge, 2018). However, in the case of both opioids (Chapter 2) and alcohol (Chapter 4), the long-term effects of the use on health are negative (Murray et al., 2018; Rudd et al., 2016). Chapter 4 sheds light on the determinants of risky health behaviors, which is part of the mechanisms connecting fluctuations in income and population health discussed around Chapter 2.

This dissertation is composed of three independent chapters that contribute to different aspects of the understanding of population health. In the next subsections, I provide an extended summary of each chapter, with its main research question, method, and contribution to the literature.

1.1 Introduction to Chapter 2

In the chapter, I measure the impact of economic fluctuations on demand for pain medication. I focus on understanding the mechanisms of physical pain and mental distress in the use of such medications.

I build a unique dataset of opioid transactions from administrative data and sales of over-the-counter (OTC) painkillers from scanner data. To separate the channels of physical pain and mental distress in the demand for opioids, I contrast the effect of labor demand shocks on the use of opioids with the effect on the use of OTC painkillers—which address pain but not mental health—allowing for the effects to depend on the injury rate of local industries (Hummels, Munch and Xiang, 2016; Browning and Heinesen, 2012). To identify the causal effect of employment, I build a shift-share instrument, also known as a Bartik instrument, for local demand shocks and contrast, within the same county, effects on demand for opioids and OTC painkillers (Bartik, 1991). To decompose the effect of employment on opioid use in the two channels, I implement a second identification strategy, where I calculate the substitution between these pain medications, exploring the introduction of a policy that increased requirements to prescribe opioids.

I find that a 1 percent increase in the employment-to-population ratio decreases the per-capita demand for opioids by 0.20 percent, while it increases the per-capita demand for OTC painkillers by 0.14 percent. My findings show that during local economic expansions, the decline in opioid abuse is 40 percent larger than the total effect on use while, at the same time, the demand for pain relief medication increases and is related to jobs in high injury industries.

My results provide novel evidence that better employment conditions have dual effects on the demand for pain medication. This evidence is relevant for two reasons. First, improving economic conditions to fight opioid abuse has larger effects than what is obtained estimating the total effect on use. Second, a local economic expansion causes an increase in demand for pain relief, which is concentrated among those in more manual jobs and industries with a higher injury rate. Access to pain treatment, in the form of painkillers or others, may allow these workers to join, or remain in, the labor force (Currie, Jin and Schnell, 2018). Because physical pain is currently a growing and important health problem in the United States (Case and Deaton, 2015; Krueger, 2018), my results support the notion that policies that constrain the access to opioids need to be compensated with policies that address physical pain (Kilby, 2015).

1.2 Introduction to Chapter 3

In this chapter, co-authored with Rebecca Thornton and Dirgha Ghimire, we explore the effect of access to clinics with pregnancy tests on the timing women report they are pregnant.

We build a measure of pregnancy uncertainty based on discrepancies between a child's month of birth and the month a mother reports her pregnancy. We use a unique dataset of ten years of monthly individual panel data in Nepal to measure the effect of distance to clinics with pregnancy tests on pregnancy uncertainty.

After controlling for place fixed characteristics, time, and access to family planning, we find a strong negative relationship between distance to clinics with pregnancy tests on earlier knowledge of pregnancy status. Living above the median distance to a clinic offering pregnancy tests (approximately a mile or fifteen minutes walking) increases the time a woman knows she is pregnant by one week. We show that women living farther from clinics are more uncertain and that the distance is binding only for women with previous pregnancies. We interpret this finding as the need of recognizing pregnancy symptoms before acting upon the beliefs and going to a clinic.

The policy implication of this chapter is related to the continuum of care—being aware of pregnancy is the first step in the continuum of care towards a healthy pregnancy or safe abortion (Boerma et al., 2018). Understanding the process of how a woman learns of her pregnancy status can help health providers design policies or provide access to pregnancy tests that could result in healthier infants and mothers. Women can use the knowledge of their pregnancy status to optimize their behavior, such as beginning antenatal care (Simkhada et al., 2008), or having an early abortion (Drey et al., 2006*a*).

1.3 Introduction to Chapter 4

In this chapter, co-authored with Rodrigo Schneider, we measure how election results can be linked to episodes of anxiety and self-medication with alcohol.

We analyze a daily dataset of purchases of alcohol on the weeks before and after Election Day. We implement a difference-in-differences (DID) model with a continuous treatment variable (Acemoglu, Autor and Lyle, 2004) to test if counties with larger support for losing presidential candidates consume relatively more alcohol after the election than counties with smaller support for the losing candidate. We consider presidential elections from 2004 to 2016.

Our results show that the effect of supporting a losing candidate on alcohol expenditure is positive and significant in the 2016 presidential elections. Within 30 days from Election Day, each 10 percentage point increase in support for the losing party increases alcohol expenditure by 1.1%. The effects are significant only for the 2016 elections, which is evidence that the 2016 election was different from others. According to the literature, it was a unique election due to the emotional charge of political campaigns (Nai, Martínez i Coma and Maier, 2019), the unprecedented use of social media, the decline in trust in the mainstream media (Allcott and Gentzkow, 2017), and the overwhelming wrong predictions of who would become the next president (Valentino, King and Hill, 2017).

Our results provide novel evidence that risky behaviors change in response to electoral results depending on who wins, and that such effect depends on characteristics of the electoral process. These findings are relevant for two reasons. First, we show that election outcomes increase risky health behaviors and that the increase depends on the characteristics of the election. Such an increase in alcohol consumption can have long-term effects on health and on human capital which can be a public health concern. Second, some of the components of the 2016 elections are not unique to the United States nor to electoral processes. Our results can be informative about changes in risky behavior in other anxiety-triggering events.

CHAPTER 2

EMPLOYMENT SHOCKS AND DEMAND FOR PAIN MEDICATION

Opioid use is widespread in the United States today, with more than 191 million opioid prescriptions dispensed in 2017 (CDC, 2019). Declining labor market opportunities and worsening employment conditions for less-skilled workers have been explored as one of the reasons for the spike in the use of pain medication (Case and Deaton, 2015; Krueger, 2018). Several studies explore the correlation of economic conditions with opioid use and its health outcomes, finding mostly countercyclical, but some procyclical, effects (Hollingsworth, Ruhm and Simon, 2017; Currie, Jin and Schnell, 2018). One reason for the ambiguous findings is that a higher employment rate can affect opioid use in two opposite ways. It can increase physical pain from workplace injuries, which is correlated with a larger demand for pain medication; at the same time, it can improve mental health, which is correlated with lower demand for pain medication for substance abuse. A simple measure of employment on total opioid use likely confounds labor supply and demand effects and hides important differences between opioid use for physical pain and for substance abuse.

In this paper, I use more than 170 million opioid transactions and more than 1 billion sales of over-the-counter (OTC) painkillers from scanner data in two quasi-experiments to provide new evidence on the mechanisms driving demand for pain medication during local economic fluctuations.[1] Opioids are prescribed for pain but are often used to treat mental health problems, such as insomnia or depression (Rigg and Ibañez, 2010), while OTC painkillers also treat pain but are not used to treat mental health problems (CDC, 2016). This key difference allows me to separate employment effects that operate through a physical pain channel from effects that operate through a substance abuse channel.[2] Because health—

[1] My data measure shipments of opioids to a point of sale or distribution to consumers and sales of OTC painkillers directly to consumers. I interpret both as measures of use or demand, which I use interchangeably.

[2] I refer to the physical pain channel as a channel where opioids are used to treat physical pain, as

including the demand for pain medication—is correlated with a variety of local labor supply shocks, the ordinary least squares (OLS) estimates of the impact of employment on use of pain medication are likely biased. To identify the causal effect of employment, I build a shift-share instrument, also known as a Bartik instrument, for local demand shocks and contrast, within the same county, effects on demand for opioids and OTC painkillers. The instrument is a weighted sum of national industry employment growth (excluding own county employment), where the weight is the county-industry mix in the base year.

My results show that an increase in the employment-to-population ratio of 1 percent decreases the per-capita demand for opioids by 0.20 percent and, importantly, increases the per-capita demand for OTC painkillers by 0.14 percent. The results are very similar when considering morphine milligram equivalents (otherwise known as MME, a morphine index that standardizes opioids into an equianalgesic dose). I interpret the increase in the demand for OTC painkillers as evidence that the need for pain relief increases in response to a positive employment shock. The negative effect on opioids shows that employment affects other channels—unrelated to physical pain—that drive down opioid use. To understand these opposite effects, I set up a simple framework of how changes in employment affect individuals' use of pain medication. In the framework, higher employment increases the need for pain relief due to workplace injuries (Asfaw, Pana-Cryan and Rosa, 2011) and at the same time increases the opportunity cost of using opioids due to missed work opportunities and time spent under its effects (Arkes, 2007).

To test for the physical pain channel, I construct a second shift-share instrument and allow it to vary by industry non-fatal injury incidence rates and by usage of workers' compensation (WC) systems. If physical pain, aggravated by workplace injuries, is the main channel driving the demand for pain medication, positive employment shocks from industries where a worker is more likely to get injured will increase the demand for pain medication even more. Using different measures of occupation's types, I complement the injury shift-share analysis with a heterogeneous analysis by the baseline share of manual jobs in a county.

prescribed by a doctor, and are imperfect substitutes to OTC painkillers. I refer to the substance abuse channel as the use of opioids for any other reason than what they are prescribed for. I define these channels more carefully in the conceptual framework in Section 2.1.

The results show that in high injury industries, the per-capita demand for opioids is slightly positive and significant, while in low injury industries, the point estimate is still negative. This finding is evidence that by focusing on industries with a high incidence rate, the procyclical component of the demand for opioids is larger and even becomes predominant. The change in demand for OTC painkillers is also larger when employment increases in industries that have the highest injury incidence rate. These two results suggest that the increase in demand for pain medication is related to workplace injuries. This pattern is robust to how I define the injury rate. A similar result is found in the heterogeneous analysis: the effect on opioid use is negative and is of larger magnitude in counties that have fewer manual jobs in the baseline, and it is closer to zero in counties with more manual jobs. In contrast, the positive effect on OTC painkillers is larger in counties with more manual jobs.

Given the evidence of the physical pain and substance abuse channels affecting opioid use in opposite directions, I set up a strategy to decompose the net effect of employment on opioid use in these two channels. With this goal, I estimate the substitution between opioids and OTC painkillers. I explore a state-run policy that increased requirements to prescribe opioids: the introduction of "must-access" Prescription Drug Monitoring Programs (PDMPs). PDMPs are state-run databases that contain information about opioid prescriptions to patients. The program informs physicians and pharmacists of a patient's prescription history, with the goal of stopping the prescription or dispense if the use pattern seems suspicious. The must-access version requires that doctors consult the database before writing an opioid prescription, and it has been found to decrease opioid abuse and its related health outcomes (Buchmueller and Carey, 2018). I use a difference-in-differences framework to measure the short-term effect of the introduction of this policy on the use of opioids and OTC painkillers. The effect is measured in the three states that implement it during my period of analysis. I find that the introduction of PDMPs decreases the per-capita demand for opioids by 6.4 percent and increases the per-capita demand for OTC painkillers by 7.6 percent. These estimates imply that a decrease in 1 opioid pill is associated with an increase of 1.76 OTC painkiller pills.

I use these estimates to decompose the effect of labor demand shocks on opioid use for physical pain and for substance abuse. To address physical pain, opioids and OTC painkillers are imperfect substitutes; for substance abuse, they are not substitutes (e.g., individuals do not use OTC painkillers to get high, to go to sleep, or to relieve anxiety or stress). I combine the substitution ratio and the employment elasticity of OTC painkillers to calculate the employment elasticity of opioids due to physical pain. The difference between this value and the total employment elasticity of the demand for opioids, obtained in the first part of the analysis, is the share of opioid use due to substance abuse.

I find that the employment elasticity of opioids due to physical pain equals 0.08, an evidence that opioid use to treat physical pain is in fact procyclical. On the other hand, employment elasticity of opioids due to substance abuse is countercyclical and is equal to -0.27. A back-of-the-envelope calculation suggests that the reduction in substance abuse would result in 0.91 fewer new opioid abusers and 1,795 fewer dollars lost on productivity per county-quarter; the increase in opioid use to treat physical pain suggests that if no pain treatment were available, 5,936 dollars would be lost in productivity due to the increase in pain.

My results provide novel evidence that better employment conditions have dual effects on the demand for pain medication. This evidence is relevant for two reasons. First, improving economic conditions to fight opioid abuse has larger effects than what is obtained estimating the total effect on use. Second, a local economic expansion causes an increase in demand for pain relief, which is concentrated among those in more manual jobs and industries with a higher injury rate. Access to pain treatment, in the form of painkillers or others, may allow these workers to join, or remain in, the labor force. Because physical pain is currently a growing and important health problem in the United States (Case and Deaton, 2015; Krueger, 2018), my results support the notion that policies that constrain the access to opioids need to be compensated with policies that address physical pain (Kilby, 2015).

My paper speaks to three strands of the literature. First, my paper is related to the literature measuring the effect of workplace injuries on health. The effect of workplace activities on health was previously discussed in Rosen (1986), and most of the evidence in this area is correlational (Harkness et al., 2004; Virtanen et al., 2012). One exception is Hummels, Munch and Xiang (2016), who show that trade shocks increase total hours worked and decrease health outcomes in Denmark. In this paper, I also provide a causal estimate by constructing an injury shift-share instrument for labor demand. The effect on demand for pain medication of an exogenous increase in jobs where a worker is more likely to get injured highlights the channel of workplace injuries.

Second, the fact that I use pain medication to infer demand for pain relief also connects my paper to the literature that looks at the effect of medical technologies (such as medications) in allowing individuals to join, or remain in, the labor force (Bütikofer and Skira, 2018; Garthwaite, 2012; Daysal and Orsini, 2012). My findings suggest that workers, especially those in more manual occupations, need a medical technology to manage the increase in pain as a consequence of higher employment.

Third, my paper speaks to the literature estimating how economic conditions affect physical and mental health (Ruhm, 2000; Charles and DeCicca, 2008; Bradford and Lastrapes, 2014) and more specifically speaks to the literature on opioid use and opioid-related health outcomes. I add new evidence by measuring the effect on use, while most papers focus on the effect on opioid overdoses and other extreme outcomes. Previous studies have shown that an increase in a county's unemployment rate increases the opioid death rate (Hollingsworth, Ruhm and Simon, 2017; Pierce and Schott, 2018). The studies that measure the effect of economic fluctuations on opioid use have mixed results. Carpenter, McClellan and Rees (2017) finds no effect on use and some countercyclical effects on substance abuse.

The paper most similar to mine is from Currie, Jin and Schnell (2018), who measure the effect by demographic groups and find results that are procyclical for older women and countercyclical for men. However, to the best of my knowledge, previous studies focus on the net effect of employment. My paper is the first to contrast opioids to an imperfect substitute—OTC painkillers—which allows me to show the different channels that connect employment shocks to opioid use. My results suggest that the effect of changes in employment on opioid use due to substance abuse is significantly attenuated if the physical pain channel is not considered.

This paper is organized as follows: Section 2.1 presents a simple conceptual framework of demand for pain relief, which introduces the two different mechanisms of how labor demand shocks can affect use of opioids. Section 2.2 describes the opioid and OTC-painkiller data. Section 2.3 presents the shift-share empirical approach to estimate the effect of labor market conditions on the use of pain medication. Section 2.4 presents the main results, results using the injury shift-share, and robustness checks. Section 2.5 calculates the substitution ratio using the second identification strategy and decomposes the employment elasticity of demand for opioids in the physical pain and substance abuse channels. Section 2.6 concludes.

2.1 Conceptual Framework

In this section, I introduce a simple conceptual framework to predict how changes in employment affect the demand for OTC painkillers and opioids. I first discuss changes in individual behavior that help understand the patterns in the data.[3] Individuals maximize a utility function where physical pain decreases overall utility. Physical pain can be attenuated with pain relief medications. Any combination of opioids and OTC painkillers can be used to obtain pain relief.[4] However, the use of opioids has an opportunity cost because of the time spent under its effects and side effects. Individuals face a cost minimization problem, where they choose how much to purchase of each medication given their pain tolerance level.

First, in an expanding economy, the need for pain relief is expected to increase due to an increase in physical pain. First, the composition of workers is expected to change, with new and less experienced hires and workers with existing health conditions joining the labor force (Autor and Duggan, 2003; Currie and Madrian, 1999). Second, the pace of work can also become more intensive, and third, physical capital can depreciate faster, increasing the likelihood of workplace injuries (Asfaw, Pana-Cryan and Rosa, 2011). Correlational evidence shows that physical demands of work increase reports of pain and aggravation of symptoms (Waddell and Burton, 2001), and pain in the workplace is a common symptom, with more than half of the workforce suffering from it (Stewart et al., 2003). In one the few causal

[3]A recent literature shows that the countercyclical effect of economic conditions on mortality is mostly the result of general equilibrium effects and not the result of a change in employment status per se (Miller et al., 2009; Stevens et al., 2015; Crost and Friedson, 2017). However, there is still agreement that the effects of economic conditions on health outcomes, especially mortality, are mostly procyclical due to mental health reasons and are countercyclical due to physical health reasons (Ruhm, 2000, 2003; Charles and DeCicca, 2008; Bradford and Lastrapes, 2014). In the last year, Ruhm (2015) also shows that mortality from accidental poisoning—which includes deaths from opioid abuse and is related to mental health issues—spiked.

[4]Opioids and OTC painkillers operate in different ways to attenuate pain. This difference is key to the idea that they are imperfect substitutes to treat physical pain but not for other reasons opioids may be used. Appendix Section B provides a more detailed explanation of how these medications work.

estimates in this literature, Hummels, Munch and Xiang (2016) show that a positive trade shock increases the intensity of both on-the-job activities and the rate of injuries and sickness among workers.

Second, while demand for pain relief increases with an expanding economy, the opportunity cost of using opioids for substance abuse motives increases. This increase is due to missed work opportunities and to the reduced need to cope with stressful events, such as not having a job (Browning and Heinesen, 2012; Eliason and Storrie, 2009). The literature shows some evidence of the effect of economic conditions on drug use, but it has focused mainly on smoking and drinking, finding mostly countercyclical responses (Ruhm and Black, 2002; Xu, 2013). Few studies have looked at the effect on other drugs, mostly due to lack of data, and findings vary with type of drug. Specific drug characteristics, such as if it is legal and how long the effect lasts, change how its use is affected by economic conditions (Arkes, 2007; Martin Bassols and Vall Castelló, 2016; Carpenter, McClellan and Rees, 2017). Descriptive studies show that among those who misuse opioids, more than 60 percent mention reasons such as to get high, to sleep, or to relieve anxiety or stress (Rigg and Ibañez, 2010; Evans and Cahill, 2016). In addition, the opportunity cost of opioid is related to the time spent under its effects and side effects that cannot be spent in productive activities. Even if using as prescribed, the patient may feel drowsy and impaired to do certain activities.[5]

As a result, in an expanding economy, the effect of the first channel—physical pain—will increase the demand for opioids, while the effect of the second channel —substance abuse—will decrease the demand for opioids. Which effect predominates when there is an employment shock is an empirical question, which I explore in this paper. In Section 2.5, I calculate the substitution ratio between opioids and OTC painkillers and show evidence of the expected effect on opioid use if I were to isolate each of these channels.

[5]For example, the leaflet of Vicodin, one of the most popular opioid medication brand in my data, warns that the medication may impair mental and physical abilities and compromise the ability to perform potentially hazardous activities, such as driving and operating machinery (Abbvie, 2017).

2.2 Data and Sample

In this section I present the sources of data I use in the main analysis, how variables are constructed, and descriptive statistics.

2.2.1 Data

I use two main sources of data to capture prescription opioids and OTC painkillers sales: retail stores' weekly sales from the Nielsen Retail Dataset and shipments of opioids from the Drug Enforcement Administration's Automated Reports and Consolidated Orders System (ARCOS). Employment is measured using the Quarterly Workforce Indicators (QWI). Both datasets are available at the retailer-weekly level or more frequently, while the employment data are available only at the county-quarter level. To standardize the analysis, I aggregate pain medication sales to the county-quarter level, creating a variable of total sales.

Opioid transactions data To obtain the quantity of opioid prescription pills in each county, I use the ARCOS dataset (Washington Post, 2019). It contains every transaction of pain pills containing scheduled substances in the United States. The *Washington Post* made public a subset of these data, containing nearly 180 million transactions of oxycodone and hydrocodone pills, from 2006 to 2012, where the destination is a point of sale or distribution to consumers (e.g., hospitals, retail pharmacies, practitioners). Oxycodone and hydrocodone are the most commonly prescribed and abused opioids; oxycodone is the active ingredient in medications such as Percocet and OxyContin, and hydrocodone is in medications such as Vicodin and Lortab. In my period of analysis, oxycodone and hydrocodone correspond to approximately 70 percent of the total grams of opioids distributed in the country (ARCOS, 2012).[6] Tablets are also the most common prescribed form of opioids, representing approximately 73 percent of the total transactions of opioids.

On average, a county sells 915,113 opioid pills per quarter, distributed by approximately 559 distinct providers. There are 2,994 counties that receive at least one shipment of opioid

[6] Other less commonly abused opioids include oxymorphone, hydromorphone, tramadol, tapentadol, morphine, and methadone. The literature has shown that these opioids are less responsive to policy changes (Kilby, 2015; Mallatt, 2017).

in the sample period; of these, 97.6 percent receive a shipment every quarter of the sample, composing a balanced panel of 28 quarters. The remaining 70 counties have less frequent opioid sales but are kept in the analysis.

Over-the-counter painkiller sales data The Nielsen Retail Dataset consists of stores' weekly sales from 2006–2016.[7] The initial sample is composed of stores that sell at least one unit of OTC painkillers in the period of analysis. To avoid contaminating the analysis with the effect of stores opening and closing, which itself can be a consequence of economic changes, I use a balanced panel of 23,743 stores, spread over 2,211 counties. This panel corresponds to 55.63 percent of the initial sample of stores.[8]

The retail data are available by product and contain more than one billion weekly registered sales of 14,852 unique OTC painkillers.[9] The sample consists of 23,083 unique stores, nearly equally split among drugstores, mass merchandisers, and food merchandisers. A very small share (2 percent) consists of convenience stores—of these, most are located in gas stations.

Employment data from the Census Bureau The employment data come from the QWI, available through the Census Bureau. The source of the data is the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee microdata, which covers over 95 percent of U.S. private sector jobs. It reports the number of jobs at the beginning of the reference quarter by their two-digit North American Industry Classification System (NAICS) industry.

[7] The researcher's own analyses were calculated (or derived) based in part on data from the Nielsen Company (US), LLC and marketing databases provided through the Nielsen Dataset at the Kilts Center for Marketing Data Center at the University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researcher and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

[8] The results, however, are very similar when using the unbalanced sample.

[9] Nielsen categorizes unique products by unique Unique Product Codes (UPCs). A UPC corresponds to a unique product in terms of brand, quantity, and packaging. The products in the data are not grouped by the active substance but by their recommended use. I identify OTC painkillers by considering products in the following modules: pain remedies, headache; pain remedies, alkalizing effervescents; pain remedies, arthritis; and pain remedies, back and leg.

2.2.2 Variable Construction

I use per-capita measures for the main outcomes and predictors. The per-capita demand for opioids and OTC painkillers are constructed by adding the total number of pills of each medication sold in a county-quarter and dividing by the total population living in a county. The population counts come from the National Cancer Institute's Surveillance Epidemiology and End Results (Cancer-SEER) program. The per-capita demand for MME is constructed in a similar way, after converting opioid pills into MME and then adding the total MME sold in a county-quarter. The MME of each opioid drug is obtained using the Food and Drug Administration National Drug Code Directory, which contains information about each drug. The main predictor — employment-to-population ratio — is constructed by dividing the total employment in a county, given by QWI, by the total population in that county in a year.

In the analysis using the injury shift-share instrument for employment, I split industries by their injury rates using two different measures of this rate. First, I consider the incidence rate of total non-fatal workplace injuries and illnesses, extracted from the Survey of Occupational Injuries and Illnesses (SOII) by the Bureau of Labor Statistics (BLS, 2005). It contains the number of non-fatal injuries and illnesses per 100 full-time workers in 2004.[10] Second, I use WC claims rate by industry. WC provides partial medical care and income to workers who suffer work-related injuries and illnesses. While the SOII is a survey, WC claims come from administrative data. Although the estimates using either measure are expected to be similar, some studies have found that the number of collected WC claims is higher than the estimated number of injuries in the SOII (Boden and Ozonoff, 2008). There is no central source of WC claims in the United States, and details of WC systems vary by state. I take statistics from Ohio to calculate industries with highest rate of WC claims because they were made publicly available by the National Institute for Occupational Safety and Health (NIOSH, 2019). Although, ideally, the data would be from before the period of analysis, the data at the NAICS two-digit industry level is available only as an average from 2001 to 2015 (Wurzelbacher et al., 2016). I use these two measures to rank industries and split

[10] I use these data for all 20 NAICS sectors except for mining. SOII warns that mining does not follow the recordkeeping requirements followed by other sectors and therefore is not comparable.

them in high injury industries — those above the median of the injury rate — and low injury industries — those below the median. Appendix Table A.1 shows the injury rate by industry using each measure.

For the heterogeneous analysis, I split counties in high and low share of manual occupations using the 2000 Census. The share of manual jobs is constructed using the manual task component of occupations, defined in Autor and Dorn (2013).[11] I split the counties in above, or below, the median of the share of manual occupations.

2.2.3 Descriptive Statistics

The use of opioids varies widely across the country, with some counties having more than 100 pills distributed per person in a year. Figure 2.1 shows two maps with the average number of pills per capita sold in a year for opioids and OTC painkillers. The figure shows some known facts—such as that the Appalachian region presents a large per-capita consumption of opioid pills (CDC, 2017)—but also shows that there is significant geographic heterogeneity in the use of both medications.[12] There is no clear relationship between the two medications, as shown in Appendix Figure A.1; the correlation of the per-capita demand for these medications is -0.06.

Table 2.1 shows that, on average, 9.80 opioid pills are demanded per capita per county-quarter. Most of the pills are weak—with no more than ten MME per pill. Another outcome of interest is the MME per capita, which shows the amount of active substances consumed instead of pills consumed. On average, 91.50 MME are consumed per capita; more than two-thirds are contained in weak opioid pills. Panel B shows that, on average, 10.10 OTC painkillers are consumed per capita, an average not much different from the opioid per capita. However, as shown in Figure 2.1, the sample of OTC painkillers does not cover as

[11] To build the share of each measure by county, I follow three steps. First, from the 2000 Census, I obtain the share of jobs in a county by 15 occupation groups. Second, for each group, I calculate the average manual score of the occupations. Third, I calculate the median score among the occupation groups and split them in, above, or below the median score. Finally, each county receives a manual employment share, which is the share of jobs above the median of the manual index.

[12] The figure also shows that the opioid data cover a larger region than the OTC painkiller data. I show in Section 2.4.3 that the results are valid when restricting the analysis to a common sample for both medications. However, when calculating the substitution ratio between these two medications, the estimates speak to counties where data for both are available.

many low-populated places as the sample of opioids does; considering only counties present in both samples, the average of opioid pills consumed per capita is even higher and is equal to 10.15.

Panel C of Table 2.1 shows county characteristics related to employment. The employment-to-population ratio equals 0.33, on average. Restricting jobs to those in industries with a low injury incidence rate, the average employment-to-population ratio equal 0.12; in industries with a high injury incidence rate, it equals 0.18. The numbers are similar when separating industries by the WC claim rate. The last rows show that occupations in a county are, on average, 38 percent manual.

2.3 Empirical Approach

In this section, I present the empirical approach used to identify the effect of changes in local labor employment on the demand for pain medication. Specifically, I want to measure how sales of opioids and OTC painkillers respond to a local labor demand shift. The reduced-form relationship of interest is

$$Y_{ct} = \beta Employment_{ct} + [county\, FE] + [year\text{-}quarter\, FE] + \epsilon_{ct}, \tag{2.1}$$

where Y_{ct} is the log of per-capita sales of opioids or OTC painkillers in county c and time t, defined as a year-quarter and $Employment_{ct}$ is the log of the employment-to-population ratio in county c and time t. The employment-to-population ratio, instead of the unemployment ratio, informs about the availability of jobs in the county where the household resides. This ratio is not as sensitive to individuals leaving the labor force in times of economic crisis, which is especially important in my context since labor force participation is correlated with the opioid epidemic (Krueger, 2018).[13]

I include year-quarter indicators to control for determinants of demand for pain medication

[13]Population can change as a result of migration in response to labor demand shocks (Arthi, Beach and Hanlon, 2019), so I could potentially be introducing an endogenous variable in the denominator. For easier interpretation, I keep the employment-to-population ratio in the main analysis, but I show in a robustness check that my estimation is robust to using total employment instead of the ratio.

that vary uniformly across counties over time, such as regulations and launching of new medications at the national level. County fixed effects controls for sale patterns that vary across counties but are fixed over time, such as taxes and regulations of how medications can be sold or prescribed or lifestyle disparities among counties that influence the use of pain medications. The effect of employment is then identified from within-county variations in the demand for pain medication, relative to changes in other counties, after controlling for time effects.

2.3.1 Shift-Share Instrument for Labor Demand

The challenge to estimate Equation 2.1 is that local labor supply characteristics affect local employment and the use of pain medication. Especially in the case of opioids, reverse causality is an important concern; several papers examine the effect of opioids on labor supply (Aliprantis, Fee and Schweitzer, 2019), the opposite of what I am interest at. In addition, the supply of opioids is an important determinant of use (Ruhm, 2019; Borgschulte, Corredor-Waldron and Marshall, 2018), so the OLS estimates can simply reflect that counties with higher employment also have higher supply of opioids.

To measure the effect of changes in labor demand, I need a shock that only shifts the demand, with no effects on supply. I introduce a shift-share shock in a strategy similar to Currie, Jin and Schnell (2018), which is one of the many variations to study local economic shocks introduced by Bartik (1991). I construct the instrument by interacting the national employment growth rate by industry with the county's initial-year industry composition:

$$LaborDemand_{ct} = \sum_{j \in sectors} \left(\frac{employment_{cj2004}}{employment_{c2004}} \cdot \frac{\sum_{k \in counties \neq c} employment_{jkt}}{\sum_{k \in counties \neq c} employment_{jk2004}} \right), \qquad (2.2)$$

where $employment_{jkt}$ equals the total number of jobs in sector j, county k, and year-quarter t. The first term is the share of employment in each industry in the base period, which equals one when summed over all industries in a county. I use 20 NAICS sectors, listed in Appendix Table A.1. The base period is the average industry composition in 2004.[14] The

[14] I use 2004 to maximize the number of states included in the analysis (47) while still setting it before the period of analysis since the availability of data on pill medications starts in 2006. This choice allows for the

second term is the national employment growth rate by industry in relation to the base year. I use leave-one-out means to construct the growth rate, leaving the county out of the average growth rate when calculating the value of the instrument for that county. This avoids idiosyncratic industry-location components of employment to mechanically affect the first stage, improving its predictive power, which could make the instrument endogenous (Goldsmith-Pinkham, Sorkin and Swift, 2018; Autor and Duggan, 2003).

Looking at the effect at the industry level matters because industries suffer fluctuations at different intensities and times and therefore adjust their labor demand differently. A county with a heavy presence of manufacturing in the base year will be more affected by future manufacturing shocks, while a county with a higher share of agricultural jobs in the base year will be more affected by variations in this sector. Figure 2.2 shows the national growth rate of the industries considered in the analysis.

The identifying assumption of my instrumental variable (IV) approach is that after controlling for county and time fixed effects, changes in the shift-share instrument for labor demand in a county are unrelated to changes in pain medication sales in this county, except through their effect on employment.

The fact that the instrument is composed of two parts—the share and the shift—makes it harder to pin down where the exogenous variation comes from. In the last few years, there has been new research trying to decompose the underlying identification assumption. Since I am interested in the impact of shifters, my approach is closer to the identification assumption developed in Borusyak, Hull and Jaravel (2018): the relationship is causal if the shifter is as-good-as-random conditional on shares and my controls.[15]

The period of analysis covers 2006–2016 for OTC painkillers and 2006–2012 for opioids, which provides enough variation in employment to be captured by the instrument. Having 20 sectors also ensures that the variation in my instrument does not come from one specific

inclusion of all states in the continental United States, except Massachusetts and the District of Columbia, which have QWI data only after 2010 and 2005, respectively. The results are robust to variations in the base period.

[15]Goldsmith-Pinkham, Sorkin and Swift (2018) develop an alternative approach, where the full vector of shares works as an instrument for the endogenous variable. The identification comes from having such shares as random conditional on the shifters. In my case, this assumption may not hold (e.g., the percentage of local jobs allocated in an industry (share) may be correlated with labor supply factors).

sector in a county or region, which is important for the validity of the instrument.

For inference, I use the standard-error correction proposed by Adão, Kolesár and Morales (2019), who show that in a shift-share design, commonly used standard errors tend to over-reject the null hypothesis. This happens because regression residuals are correlated across regions with similar shares; intuitively, two counties with similar shares not only have similar exposure to the shifters (as expected) but will also have similar values of the residuals.

Using the instrument for employment in Equation 2.2, *LaborDemand*, I obtain the following first stage:

$$Employment_{ct} = \gamma LaborDemand_{ct} + [county\, FE] + [year\text{-}quarter\; FE] + \eta_{ct}. \tag{2.3}$$

The second stage is simply Equation 2.1 replacing $Employment_{ct}$ with the predicted employment using the exogenous labor demand shock in the first stage, $\hat{Employment}_{ct}$.

2.3.2 Injury Shift-Share Instrument for Labor Demand

To emphasize the role of workplace injuries as a channel affecting demand for pain medication, I modify the construction of the instrument and the first-stage regression. First, I rank industries by one of the two measures of injury rates discussed in Section 2.2, the incidence rate of non-fatal injuries and illnesses or the WC claims rate. Second, I split industries in high and low injury rates, defined as above and below the median of the injury rate. Third, I define the first stage to consider only employment from a group of these industries. For example, for industries in the high injury group, the first stage is given by

$$EmploymentHI_{ct} = \lambda LaborDemandHI_{ct} + [county\, FE] + [year\text{-}quarter\; FE] + \varepsilon_{ct}, \tag{2.4}$$

where $EmploymentHI_{ct}$ is the employment-to-population ratio considering only jobs from high injury industries in the numerator and $LaborDemandHI_{ct}$ is the labor demand shift-share instrument for labor demand considering also only the national growth rate of high injury industries. The instrument is defined, in this example, as

$$LaborDemandHI_{ct} = \sum_{\substack{j \in sectors \\ j \in highinjury}} \left(\frac{employment_{cj2004}}{employment_{c2004}} \cdot \frac{\sum_{k \in counties \neq c} employment_{jkt}}{\sum_{k \in counties \neq c} employment_{jk2004}}\right). \tag{2.5}$$

The second stage is unaffected and follows the main specification. For the low injury industries, the first stage and construction of the shift-share instrument follow the same logic.

2.4 Effect of Employment on Demand for Pain Medication

In this section, I estimate the effect of labor demand shocks on the demand for pain medication. I start with the naive estimator—OLS—and show that the results are similar to what is found in the raw data. This estimator shows the use of pain medication for a certain employment change that combines the effect of labor supply and demand. Next, I show the results using the shift-share estimator of labor demand, which provides a causal estimate of the effect of the shift on demand for pain medication. After, I adjust the shifts to account for the injury incidence rate in each industry. Last, I check the robustness of my results.

2.4.1 Instrumental Variable Results

My IV strategy identifies local employment changes that are due to changes in the national employment growth rate. Figure 2.3 shows the predictive power of the shift-share instrument for labor demand. An increase of 1 percent in labor demand corresponds to an increase of approximately 0.86 percent in the employment-to-population ratio. The F-statistic is large in both samples, above 600. Both first stages are strong and ensure that I have a valid instrument. Appendix Table A.2 reports the first-stage results.

Table 2.2 shows the main results of the paper, using the two-stage least squares (2SLS) strategy to obtain the effect of a change in employment on demand for pain medication. Columns 1 and 3 show OLS estimations, following Equation 2.1. In this naive estimation, an increase of 1 percent in employment increases the total quantity of opioids sold by 0.11 percent and increases the quantity of OTC painkillers sold by 0.32 percent. The OLS reports the demand for pain medication in equilibrium, considering the supply from doctors and the demand from patients. The concern is that by regressing demand for pain medication on local employment growth, the estimates will be biased due to several local labor supply shocks. Even after controlling for my set of fixed effects, omitted variables and reverse causality can still bias the results. The bias moves the point estimate in the same direction as found in Currie, Jin and Schnell (2018).

The IV results are reported in columns 2 and 4 of Table 2.2. An increase of 1 percent in the employment-to-population ratio decreases the per-capita demand for opioids by 0.20 percent. From an average sale of 9.80 pills per capita, per county and quarter, this decrease means that 0.02 fewer pills per capita are sold with an increase in employment.The effect on OTC painkillers, however, is positive; a 1 percent increase in employment increases the demand for OTC painkiller pills by 0.14 percent. From an average sale of 10.10 pills per capita, this increase in demand reflects an increase of 0.01 pills per capita, per county, and quarter.

Table 2.3 extends the analysis to different outcomes. Although the number of pills has been shown to matter in decisions of how much to prescribe (Chiu et al., 2018), it does not say much about the quantity of active substance consumed. In Table 2.3, I show the effect of employment on type of opioid consumed in two different ways. First, I split the total number of pills per capita in two categories, weak and strong (those with more than ten MME per tablet).[16] Second, I measure the effect on MME, overall and in strong and weak pills. The effect on MME is very similar to the effect on opioid pills shown in Table 2.2. Because I focus on oxycodone and hydrocone pills, there is not much variation in the MME of pills in the data—more than two-thirds have between 7.5 and 10 MME. The small variation in MME in opioid pills in my data explains why the results on pills and on MME are similar. The point estimate of the effect of employment on the demand for stronger medications is more negative both in terms of pills and MME. However, the average consumption of stronger

[16] I do not perform the weak-strong analysis using OTC painkillers because they are identified by UPCs in the data. Contrary to opioids, which are identified by NDC, no simple crosswalk exists between UPCs and NDCs. It is therefore harder to obtain details of each medication, such as active substances and concentration levels. Even after matching UPCs to a third-party dataset, I could obtain detailed information for less than a third of products, resulting in noisy estimates.

opioids is much lower, so the effect of employment shocks on opioid use is still concentrated in weaker opioids.

2.4.2 The Role of Workplace Injuries

In this section, I introduce two estimation strategies to help understand the mechanisms driving the demand for pain medication. The main results show that although opioids and OTC painkillers are both pain medications, they respond differently to changes in employment. As discussed in the conceptual framework in Section 2.1, one potential reason is that while use of OTC painkillers is motivated only by physical pain, the use of opioids is also motivated by substance abuse. To test for this, first I estimate the effects using the injury shift-share instrument defined in Equation 2.5. This provides causal estimates of how changes in employment from industries with different injury rates affects the demand for pain medication. Second, I implement a heterogeneous analysis using the main instrument and splitting counties by the share of manual occupations in the baseline.

Table 2.4 shows the results of the injury shift-share instrument. First, across both panels, the main results presented in Table 2.2 remain consistent: a higher employment-to-population ratio causes a decrease in demand for opioids and an increase in demand for OTC painkillers. The decline in opioid use remains negative for low injury industries, as shown in column 1; however, for high injury industries, the effect becomes closer to zero and even positive, while still significant, as shown in column 2. These estimates show that when the increase in employment comes from industries with a higher likelihood of a worker getting injured, the demand for opioids due to the physical pain channel becomes larger, driving the point estimate up.

Columns 3 and 4 reinforce this perspective for OTC painkillers. A 1 percent increase in the employment-to-population ratio in industries with a high injury rate increases the demand for OTC painkillers by 0.21 percent, a point estimate larger than what is found for overall employment in Table 2.2. When the increase in employment comes from an industry with a low injury incidence rate, the point estimate is close to zero and insignificant. The results in panel B, where industries are classified by the usage of WC systems, follow the same pattern.

To supplement this evidence, I return to the main instrument, which considers labor demand shifts from any industry, and run a heterogeneous analysis of characteristics of occupations in a county. Table 2.5 shows the results. Overall, the demand for opioids remains countercyclical in all subsamples, and the demand for OTC painkillers remains procyclical. However, two main differences appear: the effect of a 1 percent increase in the employment-to-population ratio on the demand for OTC painkillers in counties with a large share of manual jobs is a 0.23 percent increase, even larger than the estimates for the whole sample, while the estimates for counties with a low share of manual jobs is closer to zero. On the other hand, the effect of a 1 percent increase in the employment-to-population ratio on the demand for opioids in counties with a small share of manual jobs is even more negative than the whole sample estimates and is equal to a 0.48 percent decrease. The fact that the coefficient becomes closer to zero in counties with more manual jobs is again evidence that the physical pain motive to use opioids is larger in these cases.

2.4.3 Robustness Checks

In this section, I show that my results are robust to changes in my main specification. First, I show that results hold when I use a balanced sample, keeping the same estimation strategy or using a two-period analysis. Second, I show that results are robust to using total employment and total use of pain medication instead of the ratio divided by the population. Third, I control for the share of individuals insured in a county to understand the effect of access to health insurance in regard to the demand for pain medication. These results are consistent with those presented in Section 2.4.

The first test is to change the sample and reproduce the analysis with the subset of counties whose opioid and OTC painkiller sales are observed in every quarter. Doing this reduces the sample size of opioids from 82,692 to 60,586, mainly because some counties used in the main sample do not have OTC information. It also reduces the sample size of OTC from 95,462 to 60,586 because in the main sample, OTC data are available until 2016, but opioid data are available only until 2012. Panel A of Appendix Table A.3 shows the results for this subsample. Although the coefficients change slightly, they still point in the same direction as the main results in Table 2.2. In panel B of Appendix Table A.3, I keep the balanced sample but restrict the period of analysis to 2006 and 2012 only, at the annual level, in a framework similar to the original shift-share setting in Bartik (1991). The point estimates point in the same direction as the main specification, but the reduced sample size results in slightly larger standard errors.

The second test is to use total employment and total use of pain medication instead of the ratio divided by the population. This test addresses concerns that migration is itself a response to employment shocks (Arthi, Beach and Hanlon, 2019), which could introduce an endogenous denominator in my variable of interest. Appendix Table A.4 shows that the point estimates are very similar in this specification, so my results do not seem to be affected by changes a county's population size.

The third test is to include the percentage of individuals insured by county as a control. Because health insurance is the most common way to obtain prescription medication, and because approximately half of the population in the United States obtains insurance through an employer (KFF, 2019), a mechanical relationship between the increase in employment and the increase in individuals insured could explain a higher opioid use. Although my point estimate is negative—an expanding economy decreases opioid use—my estimate could be a lower bound if access to health insurance increases with an economic expansion and it also increases opioid use. I test for this following Currie, Jin and Schnell (2018) and the strategy in Heutel and Ruhm (2016), adding the percentage of individuals insured in a county as a control. The data are obtained from the estimated share of the county population with insurance, provided by the Census Bureau's Small Area Health Insurance Estimates (SAHIE). Appendix Table A.5 shows that the point estimate of employment is very similar to the main estimation in Table 2.2. The similarity in the point estimate is evidence that health insurance is not a relevant channel to explain my results.[17]

[17]One limitation of this analysis is that the ratio of the insured population can be an endogenous variable, so this test could introduce a "bad control" bias in the estimation (Angrist and Pischke, 2008). This introduces a selection bias if, after controlling for county and year-quarter fixed effects, an exogenous change in employment changes the composition of the pool of insured individuals. Since the point estimate is very similar to the estimation without controlling for ratio of insured, this problem is likely insubstantial.

2.5 Decomposing the Employment Elasticity of Opioids

To provide a measure of substitution between opioids and OTC painkillers, in this section, I explore a quasi-experiment that increases the cost of obtaining prescription opioids and compare its effects on the demand for opioids and OTC painkillers. Using the ratio of substitution between the two medications, I calculate the share of the employment elasticity of opioids estimated in Table 2.2 that would be expected if opioids were only used for physical pain.

2.5.1 Empirical Approach

To calculate the quantity of opioids that are substituted with OTC painkillers, I explore the introduction of a policy that decreased access to opioids while keeping physical pain constant. The policy is the implementation of PDMPs, which varies at the state level. One important variation among PDMPs is the requirement that prescribers consult the program before prescribing controlled substances, which the literature calls must-access PDMPs. Buchmueller and Carey (2018) show that this variation of the policy is most likely to reduce the misuse of opioids. My identification strategy explores the variation at the timing of adoption of must-access PDMPs in a difference-in-difference framework.[18]

The estimated equation is as follows:

$$Y_{st} = \delta PDMP_{st} + [state\, FE] + [year\text{-}quarter\, FE] + \xi_{st}, \tag{2.6}$$

where Y_{ct} is the log of per capita sales of opioids or OTC painkillers in state s and time t, defined as a year-quarter, and $PDMP_{st}$ is a binary variable equal to one if state s enacted PDMP on or after quarter t, and zero otherwise. Data on the date of implementation of must-access PDMPs come from the Prescription Drug Abuse Policy System (PDAPS, 2019). The first must-access PDMPs were implemented in 2012. Since the ARCOS data are only available until the end of 2012, my estimation has two important limitations: the effect

[18] The data are aggregated to the state level for this analysis because that is the level of variation. Because I have only three states treated, I show in Appendix Table A.6 that the estimates are not driven by any one of these states.

is based on the implementation of the policy in three states only, which were the first to implement and therefore the effect may differ from future implementations, and the effect is measured right after the implementation, corresponding to the immediate short-term effect.[19] After 2012, other states are treated; since the impact on those states could differ, and in this section I am interested in the ratio of the effect on opioids and OTC painkillers, I estimate the effect on both medications only until the end of 2012.

2.5.2 Effect of PDMP on Demand for Pain Medication

Table 2.6 shows the results of the estimation of Equation 2.6. After the PDMP, the per-capita demand for opioid pills decreases by 6.4 percent, while the per-capita demand for OTC painkillers increases by 7.6 percent. My finding for opioids is of the same magnitude of what has been found in the literature (Buchmueller and Carey, 2018; Buchmueller, Carey and Meille, 2019; Kilby, 2015); the effect on OTC painkillers is a novel estimate. Since the introduction of PDMPs decrease the demand for opioids while increasing the demand for OTC painkillers, I interpret this as an indication that the two pain medications are imperfect substitutes.

To obtain the substitution ratio, I interpret the PDMP as an increase in the cost of obtaining opioids, which does not affect physical pain. My estimation shows that the introduction of PDMPs reduces the use of opioids by 0.62 pills per capita, while it increases the use of OTC painkillers by 1.09 pills per capita. This suggests that to keep the level of pain relief constant, a reduction in one opioid pill needs to be compensated with an increase of 1.76 OTC painkiller pills.

2.5.3 Recovering the Components of the Effect of Employment on Opioid Use

Using the ratio of substitution between opioids and OTC painkillers, and the employment elasticities of demand estimated in Section 2.4, I provide evidence of the share of the elasticity of opioids attributable to the physical pain channel, and the residual is attributed to

[19] The states are Kentucky (July 2012), New Mexico (September 2012), and West Virginia (June 2012).

substance abuse.

First, considering only the effect of a labor demand shift on pain relief, the demand for opioids is expected to respond similarly to the demand for OTC pills after adjusting for the substitution ratio. The main results, presented in Table 2.2, show that a 1 percent increase in the employment-to-population ratio increases the per-capita demand of OTC painkillers by 0.014 pills. Since opioids and OTC painkillers are substituted at a ratio of 1.76, the expected effect on opioid use is an increase of 0.008 pills per capita, or an increase of 0.08 percent.

Second, I suppose that a positive labor demand shock only increases the opportunity cost of opioid use, with no effect on the demand for pain relief. This effect is obtained from the difference between the employment elasticity of opioids, estimated in in Table 2.2, and what cannot be explained by the simulation in the previous paragraph. A 1 percent increase in the employment-to-population ratio decreases opioid use by 0.20 percent, which corresponds to a decrease in 0.019 pills per capita. The effect due to increased demand for pain relief only was calculated as an increase of 0.008 pills per capita; the difference—0.027 pills per capita—is attributed to the decrease in use only due to substance abuse. This decrease in opioid use for substance abuse corresponds to 0.27 percent.

This calculation is a strategy to recover each channel of the effect of labor demand shocks on the demand for opioids. If the ratio of substitution calculated in the previous step changes, these values also change slightly, but the evidence of the two channels —physical pain and substance abuse— that pull opioid use up or down during local economic fluctuations remain. These estimates suggest that the per-capita demand for opioids for pain relief in fact increases by 0.08 percent with a 1 percent increase in the employment-to-population ratio, while the per-capita demand for opioids for substance abuse decreases by 0.27 percent.

2.5.4 Cost Implications

In this section, I discuss the policy relevance of the effect of changes in employment on the demand for pain medication using a back-of-the-envelope calculation. I first calculate what the estimated effect on painkillers means in terms of opioid prescriptions. Second, I extrapolate the effect on use to effects on health outcomes, separating the analysis of a decrease in substance abuse from an increase in use for physical pain.

The employment elasticity of the demand for opioids is equal to -0.20, as shown in Table 2.2, or 0.02 fewer pills per capita per county-quarter. After scaling this up by the average population in a county in the balanced sample, the increase in employment results in 1,969.66 fewer pills per county-quarter. The number of prescribed opioid pills depends on several factors, such as the characteristics of the patient and the medical reason for the prescription. I consider the estimate that, on average, 30 pills of hydrocodone/acetaminophen (5 mg/325 mg) are prescribed after surgery procedures (Howard et al., 2019)—this is the substance concentration found in the most popular brand of opioid in my data. The effect can then be translated into 65.65 fewer opioid prescriptions per county-quarter. Considering the decomposition presented in the previous subsection, the same calculation means that an increase of 1 percent in the employment-to-population ratio decreases the number of opioid prescriptions for substance abuse by 88.63 and increases the number of opioid prescriptions for physical pain by 26.26.

How the share of prescriptions for substance abuse affect the risk of misuse and abuse depends on several factors, such as the duration of the prescription, medical comorbidities, and the individual's history of drug abuse. I assume that the 88.63 opioid prescriptions are given to different opioid-naive individuals, of whom approximately 1.03 percent are expected to become dependent on, abuse, or overdose (Brat et al., 2018). This mean that 0.91 fewer individuals will abuse opioids by county-quarter. To obtain a dollar value of this reduction, I consider the cost of substance abuse treatments and of loss in productivity. The reduction of 0.91 individuals abusing opioids represents a decrease in expenditures of 10,875 2010 dollars (Florence et al., 2016) in annual treatment and 16.83 fewer days missed at work per year (Goplerud, Hodge and Benham, 2017). Considering the average weekly wage in 2010, this means a decrease in lost productivity in the amount of $1,795 (Bureau of Labor Statistics, 2019).

The consequences on health outcomes of an increase in opioid use for pain relief depend on how well pain is managed with this medication. The more interesting exercise in this case is what are the expected consequences if opioid prescriptions were not available and individuals remained in pain. This is an upper bound because opioids and OTC painkillers are imperfect substitutes and opioids may have other substitutes, such as medical marijuana (Powell, Pacula and Jacobson, 2018). However, this calculation is still of interest because, with the increase in restrictions to prescribe opioids in the last years (Frieden and Houry, 2016), doctors and patients have manifested concerns about being left without alternatives to address pain (Kertesz et al., 2019). I assume the demand for the extra 26.26 prescriptions for physical pain are given to different individuals with moderate pain. If opioids were removed from the market and these individuals were left in pain, then health costs would increase by 118,590 2010 dollars, and lost productivity would sum up to 5,936 2010 dollars (Gaskin and Richard, 2012).

2.6 Discussion and Conclusion

In this paper, I explore the more immediate effect that economic conditions have on the use of pain medication and compare two opposite channels that can affect the use of pain medication. At the same time that a higher employment rate is correlated with improved mental health and lower opioid abuse, it is correlated with increased workplace injuries and physical pain, which is expected to increase the demand for pain medication.

My results show that the demand for opioids is countercyclical—the employment elasticity of the demand for opioids is equal to -0.20. At the same time, the demand for OTC painkillers is procyclical—the employment elasticity of the demand for opioids is equal to 0.14. The effect on opioids is large and negative with employment shocks from industries with a low injury rate, but the effect is close to zero and is even positive for shocks from high injury industries; the positive effect of OTC painkillers is driven mostly by high injury industries. A similar pattern is found in the heterogeneous analysis, where counties are split by the share of manual jobs. These findings shows that employment shocks increase the demand for pain relief, which suggests a higher prevalence of physical pain in the population.

The fact that opioids are countercyclical despite the procyclical effect found for OTC painkillers is evidence that opioid substance abuse decreases during local economic expansions. I provide evidence that the effect of a 1 percent increase in the employment-to-population ratio leads to a decrease of 0.27 percent in opioid use for substance abuse and to an increase of 0.08 percent in use for physical pain.

My results have important policy implications. First, improving employment conditions to fight opioid abuse has larger effects than what is obtained considering only the total net effect on use. The opposite is true when a county is hit by a negative employment shock; therefore policies that address mental health problems need to be in place to avoid an increase in opioid abuse. Second, an expanding economy will cause an increase in demand for pain relief, concentrated among those in more manual jobs and in industries with a higher injury rate, and therefore policies that constrain access to opioids need to be compensated with policies that address physical pain. Finally, although I do not incorporate dynamic effects in my analysis, some of the individuals who start to use opioids due to workplace injuries may become dependent; thus, workplace policies informing workers about medication and risks of opioid abuse are important during local economic expansions.

2.7 Figures and Tables

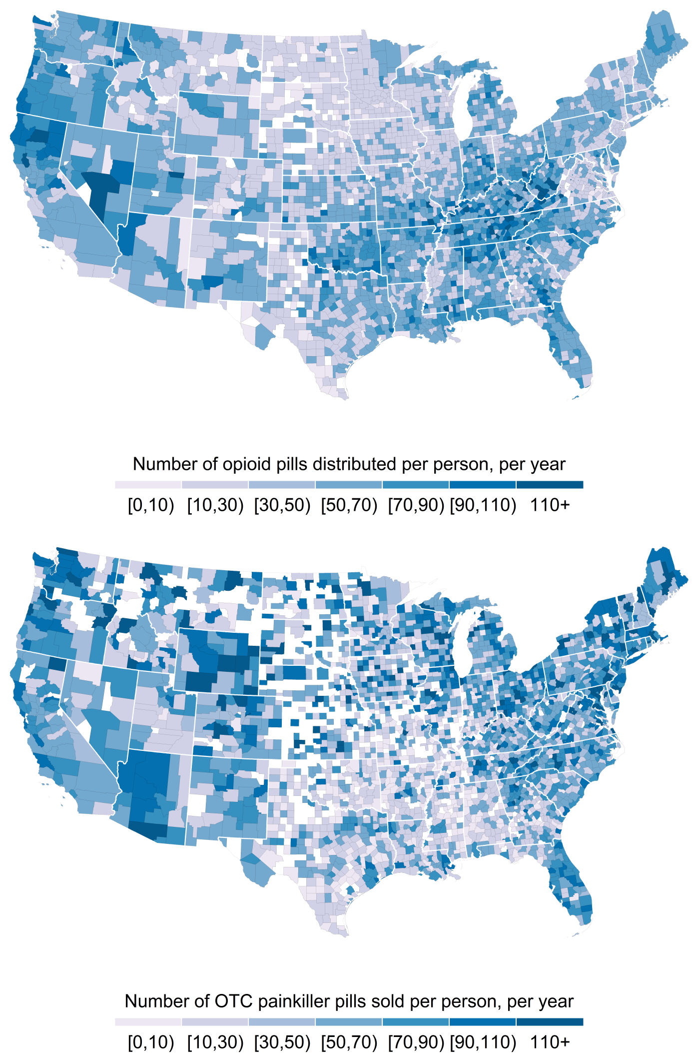

Figure 2.1: Map of Number of Pills per Person in a Year

Notes: This figure shows two maps with the average number of pills per capita sold in a county. The map on top shows the average number of opioid pills distributed in a county in a year divided by the county's population; the map at the bottom shows the average number of OTC painkiller pills sold in a county in a year divided by the county's population. Blank counties are counties for which no data are available.

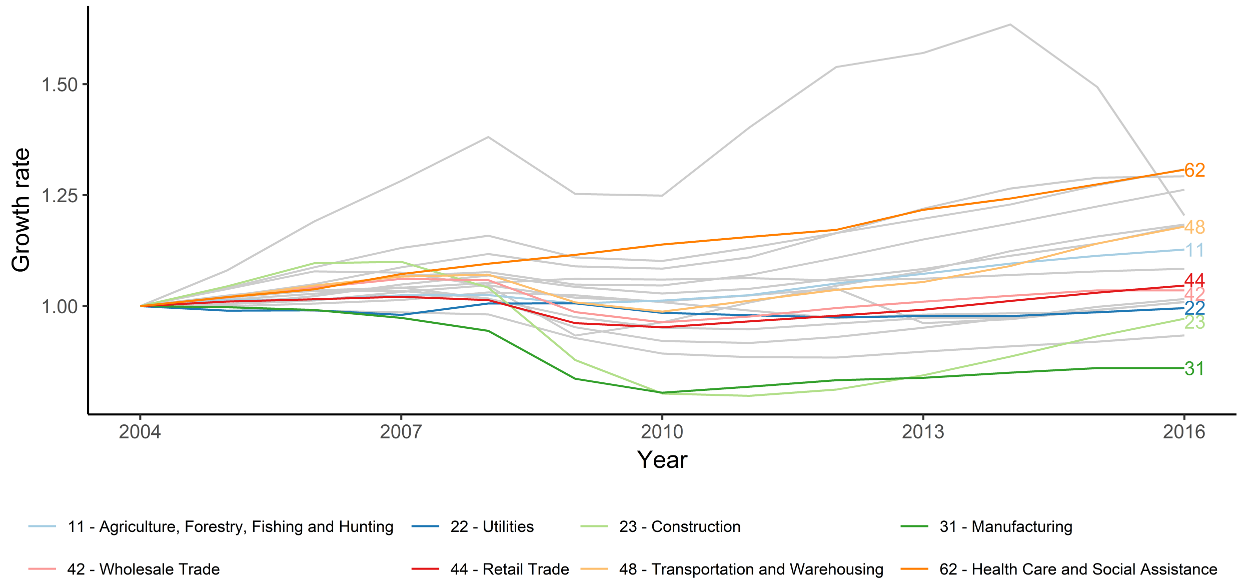

Figure 2.2: Growth Rates of Sectors over Time

Notes: This figure plots the growth rate since 2004 of the 20 NAICS sectors used to build the instrument over the time of analysis. These growth rates are the shift used in the construction of my instrument for labor demand. The highlighted sectors are sectors above the median injury incidence rate in the SOII data.

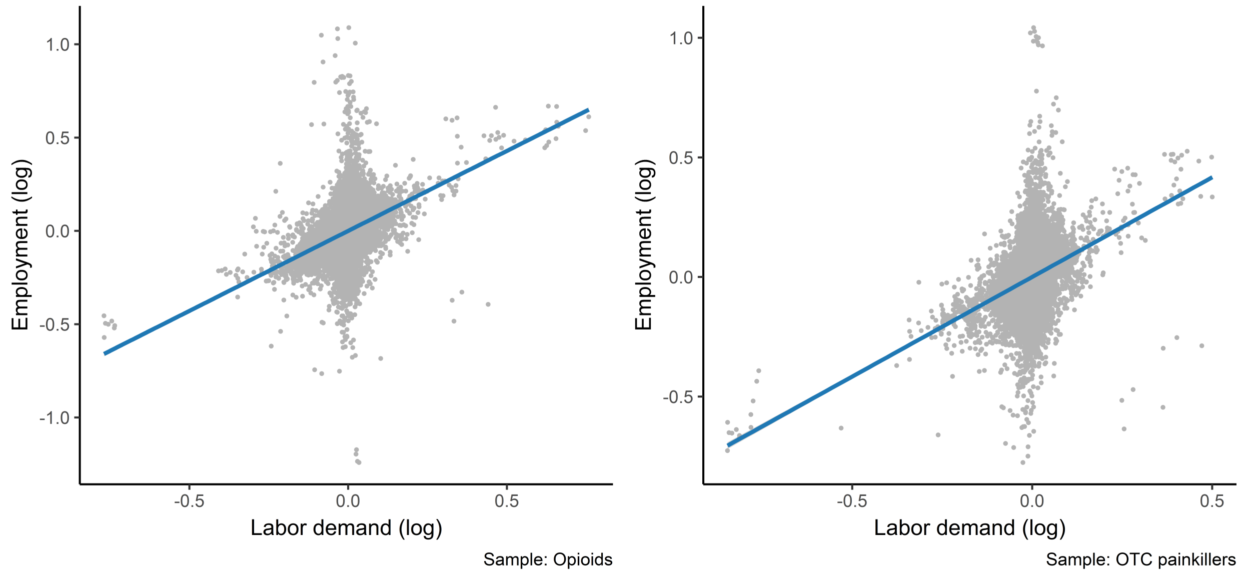

Figure 2.3: First-Stage Results

Notes: This figure plots the partial first-stage regressions of the main specification, which measures the effect of labor demand shocks on local employment, controlling for county and year-quarter fixed effects. The x-axis is the residualized shift-share instrument of labor demand shocks, obtained from the residuals of a regression of the log of the instrument on county and year-quarter fixed effects. The y-axis is the residualized employment-to-population ratio, obtained from a regression of the log of employment-to-population ratio on the same fixed effects. The figure on the left shows the first stage in the sample of opioids, while the figure on the right shows the first stage in the sample of OTC painkillers.

Table 2.1: Summary Statistics

	Mean	SD
Panel A: Opioids painkillers		
Pills per capita	9.80	7.49
Weak pills	9.07	6.85
Strong pills	0.73	1.07
Morphine-miligram-equivalents per capita	91.50	75.60
In weak pills	68.60	54.00
In strong pills	22.90	31.80
Panel B: OTC painkillers		
Pills per capita	10.05	7.65
Panel C: County characteristics		
Employment-to-population ratio (EPR)	0.33	0.13
EPR by industry characteristics:		
Low injury-incidence rate	0.12	0.08
High injury-incidence rate	0.18	0.07
Low WC claims rate	0.13	0.07
High WC claims rate	0.17	0.08
Share of manual occupations	0.38	0.05

Notes: This table shows the summary statistics of the main datasets used in the analyses. The number of observations in each panel is the following: 82,682 in panel A, 95,462 in panel B, and 82,682 in panel C. The unit of observation is a county-quarter. The county characteristics in panel C is based on the opioid sample, which spans over a shorter period (2006–2012) but includes more counties, as shown in Figure 2.1. The corresponding statistics in the OTC-painkiller sample are very similar. Panel A shows statistics of the opioid outcome in pills per capita and MME per capita. Each variable is split into weak and strong, which corresponds to medications with ten or less MME per table and more than ten MME per tablet. Panel B shows statistics of the only OTC-painkiller outcome, pills per capita. Panel C shows county characteristics that are predictors or are used in the heterogeneous analysis. The first five rows correspond to the employment-to-population ratio, overall, split by jobs in industries with low and high injury incidence rates and split by jobs in industries with low and high WC claims rate. Splitting the sample by industry does not add to the total employment-to-population ratio because not all industries are included in the calculation of these rates, as described in Section 2.2. The last row corresponds to the share of jobs considered manual in a county using the 2000 Census.

Table 2.2: Effect of Local Employment on Demand for Pain Medication

	Opioid pills (log)		OTC pills (log)	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Employment (log)	0.114***	-0.196***	0.321***	0.143***
	(0.023)	(0.005)	(0.036)	(0.014)
F-stat (1st stage)		1189.799		609.002
Mean	9.805	9.805	10.057	10.057
Observations	82,692	82,692	95,462	95,462

Notes: This table shows the result of four separate regressions. Columns 1 and 3 are OLS estimations, and columns 2 and 4 are IV estimations. The dependent variable in columns 1–2 is the log of the quantity of opioid pills per capita sold by county-quarter. The dependent variable in columns 3–4 is the log of the quantity of OTC painkiller pills per capita sold by county-quarter. The independent variable is the log of the employment-to-population ratio (predicted using the shift-share instrument for labor demand shocks in the IV regressions in columns 2 and 4). All regressions include county fixed effects and year-quarter fixed effects. Standard errors for all regressions use the AKM adjustment for shift-share designs. The mean of the dependent variables is listed in levels.

Table 2.3: Effect of Local Employment on Demand for Pain Medication, by Strength in Pills and Morphine Milligram Equivalents

	Opioid pills (log)		MME (log)	
	Weak	Strong	Weak	Strong
	(1)	(2)	(3)	(4)
Employment (log)	-0.279***	-0.384***	-0.287***	-0.293***
	(0.008)	(0.011)	(0.012)	(0.008)
Mean	9.205	0.746	69.662	23.433
Observations	80,868	80,868	80,868	80,868

Notes: This table shows the result of four separate regressions. Columns 1 and 2 show the outcomes in terms of the log of opioid pills per capita. Column 1 shows the result for the log of the quantity of weak pills sold per capita, which are pills whose MME per pill is not greater than ten. Column 2 shows the result for the log of quantity of strong pills sold per capita, which are pills whose MME per pill is greater than ten. Columns 3 and 4 show parallel results, but the outcome is in terms of MME instead of pills. Column 3 shows the result for the log of MME sold per capita considering only the subset of weak pills. Column 4 shows the result for the log of MME sold per capita considering only the subset of strong pills. The independent variable in all regressions is the log of the employment-to-population ratio (predicted using the shift-share instrument for labor demand shocks). All regressions include county fixed effects and year-quarter fixed effects. The sample size is slightly smaller than in the main table because counties with zero sales of one of the types of pills are dropped from the analysis because of the logarithmic form. To standardize, I keep only observations with a sale of at least one pill per category in a quarter. Standard errors for all regressions use the AKM adjustment for shift-share designs. The mean of the dependent variables is listed in levels.

Table 2.4: Effect of Local Employment in Industries with Different Injury Rates on Demand for Pain Medication

Panel A: By Injury-Incidence Rate				
	Opioid pills (log)		OTC pills (log)	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Employment (log)	-0.051***	0.049***	-0.007	0.209***
	(0.002)	(0.003)	(0.007)	(0.009)
Mean	9.806	9.806	10.057	10.057
Observations	82,666	82,666	95,462	95,462

Panel B: By Worker's Compensation (WC) Claim Rate				
	Opioid pills (log)		OTC pills (log)	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Employment (log)	-0.099***	-0.007***	0.014	0.248***
	(0.004)	(0.002)	(0.010)	(0.006)
Mean	9.805	9.805	10.057	10.057
Observations	82,691	82,691	95,462	95,462

Notes: This table shows the result of eight separate IV regressions. The dependent variable in columns 1–2 is the log of the quantity of opioid pills per capita sold by county-quarter. The dependent variable in columns 3–4 is the log of the quantity of OTC painkiller pills per capita sold by county-quarter. In Panel A, the predictor is the log of the predicted employment-to-population per capita considering only jobs in industries below (columns 1 and 3) or above (columns 2 and 4) the median injury incidence rate in the SOII data. In Panel B, the predictor is the log of the employment-to-population per capita considering only jobs in industries below (columns 1 and 3) or above (columns 2 and 4) the median of worker's compensation claims rate in the Ohio data. Both estimations are second-stage results of the first stage specified in Equation 2.4. All regressions include county fixed effects and year-quarter fixed effects. Standard errors for all regressions use the AKM adjustment for shift-share designs. The sample size varies in some estimations because a few counties do not have any jobs in the type of industry used to construct the variable of employment. The mean of the dependent variables is listed in levels.

Table 2.5: Effect of County-Level Employment on Pain Medication, by Share of Manual Occupations in a County

	Opioid pills (log)		OTC pills (log)	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Employment (log)	-0.477***	-0.046***	0.001	0.226***
	(0.015)	(0.011)	(0.034)	(0.024)
Mean	9.736	9.878	12.138	7.334
Observations	42,685	40,007	54,112	41,350

Notes: This table shows the result of four separate regressions. It shows heterogeneous effects of the main results, reported in Table 2.2, splitting the sample by county's characteristics. In all regressions, the predictor variable is the log of the employment-to-population ratio. The dependent variable in columns 1–2 is the log of the quantity of opioid pills per capita sold by county-quarter. The dependent variable in columns 3–4 is the log of the quantity of OTC painkiller pills per capita sold by county-quarter. The table splits the sample by low and high shares of manual jobs, based on the measure built in Autor and Dorn (2013). All regressions include county fixed effects and year-quarter fixed effects. Standard errors for all regressions use the AKM adjustment for shift-share designs. The mean of the dependent variables is listed in levels.

Table 2.6: Short-Term Effect of Restrictions on Opioid Prescriptions on Demand for Pain Medication

	Opioid pills (log)	OTC pills (log)
	(1)	(2)
PDMP	-0.064^{***}	0.076^{***}
	(0.017)	(0.029)
Mean	9.64	14.30
Observations	1,316	1,316

Notes: This table shows the result of two separate regressions. The dependent variable in columns 1 is the log of the quantity of opioid pills per capita sold by county-quarter. The dependent variable in column 2 is the log of the quantity of OTC painkiller pills sold by county-quarter. The independent variable is an indicator variable if a state has an enacted must-access PDMP law. The only states with must-access laws enacted in the period of my sample are Kentucky (July 2012), New Mexico (September 2012), and West Virginia (June 2012). I use a balanced panel covering the same counties and periods in both datasets. All regressions include state fixed effects and year-quarter fixed effects. Standard errors are clustered at the state level. The mean of the dependent variables is listed in levels.

CHAPTER 3

HOW DO WOMEN LEARN THEY ARE PREGNANT? THE INTRODUCTION OF CLINICS AND PREGNANCY UNCERTAINTY IN NEPAL*

The knowledge of being pregnant is the first step in the continuum of care towards a healthy pregnancy or safe abortion (Boerma et al., 2018). The timing of learning about a pregnancy matters for some decisions (i.e., pregnancy termination) and behaviors (i.e., taking multi-vitamins, stopping smoking), and may affect health outcomes for both woman and infant. While a pregnant woman will eventually learn her pregnancy status, inferring this information correctly is not trivial and depends on the knowledge of symptoms of pregnancy, prior pregnancy experiences, and access to pregnancy testing. In this paper, we examine the process through which women learn they are pregnant and how prior experience with pregnancy and access to pregnancy tests affect the timing of learning.

To understand pregnancy uncertainty and the effects of access to pregnancy tests on earlier pregnancy status knowledge, we use ten years of data from the Chitwan Valley Family Survey (CVFS)—individual-level monthly panel data measuring reproductive behavior of married women living in Nepal (Axinn et al., 2018). Using each recorded live-birth in the data between 1997 and 2006 (1,593 births), we compare each woman's month of conception with the contemporaneous report of her pregnancy status. We examine the determinants of identifying pregnancy earlier, including experience with prior pregnancies. We then use the openings and closures of health centers in the area over time to evaluate how changes in access to pregnancy test kits at clinics affect the average time women become aware they are pregnant.

We find a strong negative relationship between distance to clinics with pregnancy tests on earlier knowledge of pregnancy status. Living above the median distance to a clinic offering pregnancy tests (approximately a mile or fifteen minutes walking) increases the time a woman

*Jointly with Dirgha Ghimire and Rebecca Thornton knows she is pregnant by one week (5% increase). It decreases the likelihood of knowing in the first trimester by 4.6 percentage points (16.1% increase) and, in the second trimester, by 6.5 percentage points (8.5%). There is no effect of distance on overall pregnancy uncertainty for the whole sample in the third trimester.[1] To prevent confounding access to pregnancy tests with access to family planning, our data allow us to control for distance to family planning services in all of our analysis.

For women with a prior pregnancy, the impact of distance on the probability of reporting pregnancy is negative and significant in all trimesters. Moving above the median distance to a clinic with pregnancy tests, decreases the probability of reporting a pregnancy in the first, second, and third trimesters by 6.2, 12.4, and 2.5 percentage points, respectively. This result suggests that experience with symptoms of pregnancy may be important to utilize clinics with pregnancy tests in earlier months. Among women without prior pregnancy experience, the effect of distance on pregnancy knowledge is not significant during any trimester or overall during pregnancy, while for women with prior pregnancies, the effect is largest in the second trimester. This suggests that access to pregnancy tests is a binding constraint only after women's beliefs, or symptoms, about being pregnant are strong enough.

Our paper speaks to the literature about a woman's uncertainty about her pregnancy status. It is common for women to be unaware of their pregnancy status; in the United States, the average gestational age of detection is approximately 5.5 weeks, with 23% of women detecting it only after seven weeks of gestation (Branum and Ahrens, 2017).[2] Moreover, over 60% of adolescents who visit a clinic to take a pregnancy test has negative results (Zabin et al., 1996). In developing countries, uncertainty about one's pregnancy status may be even higher than in high-income countries (Peacock et al., 2001*b*). Malnutrition may cause irregular periods, hiding signs that lead to the suspicion of pregnancy (Rowland et al., 2002).

[1] This result is consistent with the finding that distance to a hospital or clinic is associated with higher gestational age at abortion in clinics in the United States (Cunningham et al., 2017).

[2] Most studies about the timing of pregnancy uncertainty focus on women who had an abortion, and these results may not extrapolate to the whole population (Drey et al., 2006*b*; Baum, DePiñeres and Grossman, 2015; Foster et al., 2008; Saavedra-Avendano et al., 2018). The timing a woman suspects her pregnancy varies widely; for example, Ingham et al. (2008) report that the median time to suspect pregnancy, among women getting an abortion, is 52.5, and the interquartile range is equal to 21–79 days. In our sample, women detect their pregnancy with 4.6 months, on average, and 71.2% of women interviewed in the second month of their pregnancy are not aware of their status.

Women may have less knowledge and education about reproduction (Peacock et al., 2001*a*), and high rates of breastfeeding and lactational amenorrhea may lead to more uncertainty because these methods are effective only under very specific conditions (Kennedy, Rivera and McNeilly, 1989; Shaaban and Glasier, 2008; WHO, 1998).

To the best of our knowledge, only three other papers have examined the relationship between earlier pregnancy detection (or earlier antenatal care utilization) and access to pregnancy tests (Hochman et al., 2012; Comfort et al., 2016; Andersen et al., 2013). As our paper, these studies find that access to pregnancy tests decrease the time to pregnancy detection and increase the take-up of services such as antenatal care and abortion. However, our paper is the first to use monthly report of pregnancy status over ten years to understand how changes in services provided by clinics in a non-controlled environment affect the timing of detection of pregnancy.

The policy implication of our study is that, understanding the process of how a woman learns of her pregnancy status can help health providers design policies or provide access to pregnancy tests that could result in healthier infants and mothers. Women can use the knowledge of their pregnancy status to optimize their behavior, such as beginning antenatal care (Simkhada et al., 2008) or having an abortion (Drey et al., 2006*b*). Pregnancy knowledge is important for women to receive early antenatal care, which is correlated with healthy pregnancy outcomes (Hueston et al., 2003; Ratzon, Sheiner and Shoham-Vardi, 2011). In developing countries, only half (48%) of women receive early antenatal care (Moller et al., 2017); earlier pregnancy detection may not only help women seek earlier care (Morroni and Moodley, 2006; Lamina, 2013) but also help them adopt healthy prenatal behaviors. Earlier pregnancy detection also enables women to begin making plans for delivery and motherhood or to make earlier decisions about abortion—earlier abortions are associated with lower rates of complications and mortality (Grossman, Blanchard and Blumenthal, 2008).

This paper is organized as follows: Section 3.1 presents the research design: the setting of our study, the data, and the sample used in the analysis. Section 3.2 describes the empirical approach to estimate the effect of distance to clinics with pregnancy tests on pregnancy knowledge. Section 3.3 presents the results, including the effect of distance on overall pregnancy uncertainty and uncertainty by trimester. Section 3.4 concludes.

3.1 Research Design

In this section, we describe the setting of the study, the data used in the analysis, and how the analytical sample was selected.

3.1.1 Setting: Chitwan, Nepal, 1997 to 2006

The data used in this study were collected between 1997 and 2006 in the western valley of Chitwan District in south-central Nepal. During this time, Nepal, and particularly Chitwan, went through many changes in regard to its economic and political development, access to reproductive health care, and fertility.

Until 1950, Chitwan was mainly uninhabited and covered by virgin forests. Due to population increase and rapid shortage of farmland in other parts of Nepal, beginning in the mid-1950s, the Nepalese government cleared the forests for farming and eradicated malaria in part of Chitwan, which saw a spike in incoming migration from other regions of the country (Axinn and Yabiku, 2001). At the same time, Chitwan remained mostly isolated from the rest of the country until the construction of all-weather roadways connecting the region with the rest of the country and with India—the first one was built in 1979. Other roads followed, and by 1980s, Narayanghat, the largest town in Chitwan, had become a transportation hub. With these changes in infrastructure, the population grew, the area developed, and more services became available: the first medical provider opened in the area in 1954 (Brauner-Otto, Axinn and Ghimire, 2007), and by 1990 there were more than 80 clinics in the region (Yabiku, 2004).

From 1996 to 2006, Nepal faced an armed conflict between the government and the Communist Party of Nepal (Maoist). The study area was not affected by this conflict until around 2000. From then until 2006, daily activities were affected by bomb blasts, gun battles, and conflict-related fatalities. We describe the effect of the civil conflict on our data and empirical strategy below.

The availability of family planning and maternity care also changed in Nepal during this time. In 2001, the earliest year this information is available, nine percent of women in the country delivered in a health clinic, while 18% did so in 2006; similarly, only 16% of pregnant women received ANC care prior to the fourth month of pregnancy in 2001, compared to 28% in 2006 (ICF, 2019). Another change was the legalization of abortion in 2002. Before that, abortion was strictly illegal in Nepal; it became legal if performed within the first twelve weeks of pregnancy, with some exceptions (Thapa, 2004). However, in 2011, almost a decade after the law, less than 38% of Nepali women believed abortion was legal (ICF, 2019). Recently, abortions have been increasing, especially in more developed areas (ranging from 21 to 59 per 1,000 women aged 15–49) (Puri et al., 2016). The estimates of illegal abortions—by definition, performed after 12 weeks of pregnancy, however, is still high: 58% of the abortions performed in 2014 were estimated to be illegal. The fertility ratio plummeted during this period, falling from 4.6 births per woman in 1996, to 3.1 in 2006 (WHO and the United Nations Population Division, 2015).[3]

3.1.2 Data

We use two main datasets in the analysis: the Chitwan Valley Family Study, used to construct the pregnancy knowledge outcomes, and the Health Providers data, used to build the distance to clinics with pregnancy tests and family planning services.

Chitwan Valley Family Study

The data used in this study comes from the Chitwan Valley Family Study (CVFS) (Axinn et al., 2007). A sample of 1,582 households (4,646 individuals) in 151 neighborhoods was selected to be part of the study in 1997. Each neighborhood consisted of 5 to 15 households surrounded by farmland. The study area includes three sampling strata corresponding to three areas with different degrees of urbanization.

From 1997 to 2006, enumerators regularly visited each household to record any major

[3]The maternal mortality ratio (MMR) also dropped during this period. In 1996, Nepal had one of the highest MMR in the world (631 per 100,000 live births); In 2006, the MMR had been reduced to 425 per 100,000 live births (WHO and the United Nations Population Division, 2015). The ideal number of children for married women fell from 2.9, in 1996, to 2.4 in 2006; the ideal number of children for men also fell, but at a slower pace, reaching 2.4 in 2006.

changes in the household's structure, such as pregnancies, births, marriages, divorces, and living arrangements. All residents of the sampled neighborhoods between the ages of 15 and 59 and their spouses were surveyed. These data also contain a record of the neighborhood where each member of the household was living, and members were followed if they migrated. Households that moved into the CVFS area during the study were also surveyed while living there.

In addition to collecting general household information each month, the study team collected data directly from each woman of reproductive age (18–49) about her pregnancy status and any pregnancy-related events such as miscarriages, abortions, still-births, or live-births.

The data are monthly, but the frequency of enumeration changed over time, as shown in Table 3.1. Between 1997 and 1999, households were interviewed approximately ten times per year. Between 2000 and 2005, survey budget constraints and civil conflict resulted in several changes to the data collection process. Surveys were conducted only during daylight, and the frequency of household visits was reduced, so the total number of visits per year ranged from six visits in 2000 to only one visit in 2003 (Axinn, Ghimire and Williams, 2012).

Health Providers and Access to Pregnancy Tests

We use neighborhood-level information detailing all health service providers in the 151 neighborhoods from 1997 to 2005, collected in the CVFS (Axinn et al., 2018). The data contain the geographical location of each provider, its year of opening and closure, and information on infrastructure, personnel, and services. It includes separate availability of family planning and pregnancy test kits.

The distance from a respondent's home to a health provider is measured by the geodesic distance between the centroid of a household's neighborhood and the exact location of the clinic. For each household, we calculate the minimum distance to a clinic offering pregnancy tests and to a clinic offering family planning services. We then create a binary variable equal to one if the woman lives in a neighborhood where the distance to the closest clinic is above the median distance, and zero otherwise. The median distance is calculated in our sample for each woman-month observation.

We assume the shortest distance to a clinic for the entire pregnancy if, during a woman's pregnancy, the distance changes (because a new clinic opened or because the woman moved to a different neighborhood).[4] For respondents living outside of these areas, we do not know the distance to health providers in their vicinity.

Table 3.2 presents the number and characteristics of health providers offering family planning services or pregnancy tests from 1997 to 2005. Out of a total of 94 health clinics in the area in 1997, 82 provided modern family planning, and 24 offered pregnancy tests. From 1997, the number of providers offering pregnancy tests grew to a total of 103 in 2005. The distance to the closest provider declines significantly over time, from 1.13 miles, on average, in 1997, to 0.48, in 2005. The median distance from a neighborhood to a pregnancy-testing clinic is 0.92 miles. This variable has a mean of 1.20 miles, and the smallest distance is 0.01 miles, while the largest is 4.81 miles.

3.1.3 Analytical Sample: Ever-Pregnant Women

Our sample involves married women who had a live birth during our study period. To determine a woman's pregnancy status, we use the monthly data that asked each woman about her pregnancy statuses. We observe the following possible status: not pregnant, pregnant, uncertain, had a live birth, stillbirth, miscarriage, or abortion. Appendix Table C.1 shows the distribution of pregnancy statuses over the months women were interviewed.

For any month in which a woman reports a live birth, we code each of the prior nine months (including the one when the birth was reported) as that the woman is pregnant. Although some variation of gestation duration exists among women, the nine-month duration is an estimate based on the calculation of average delivery dates (280 days after the beginning of the last menstrual period, or 9.2 months) (Jukic et al., 2013).[5]

[4]This happens in 210, or 2.57%, of woman-months. In most cases, the woman moves out of the region of the study at some point during gestation.

[5]Our definition of the reported month of pregnancy implicitly assumes that the month a woman reports she is pregnant coincides with the month she detects it. This is not necessarily true in all contexts; for example, qualitative studies in developed countries find that women may conceal their pregnancy until birth or until a certain stage of gestation (Peacock et al., 2001*b*; Stokes et al., 2008). Because the survey in our study was administered in privacy by female interviewers, and the interviews occurred frequently over time to build rapport with respondents, women may have felt comfortable to answer truthfully (Axinn et al., 2007). Regardless, even if women were to misreport their pregnancy status, as long as the distance to a clinic is not systematically correlated with time to reveal the pregnancy status, our estimates of the effect

Because we only have information of clinics in the CVFS area, we restrict our sample to women who lived in the CVFS area (i.e., one of the 151 neighborhoods) for at least one month during her pregnancy. Since the clinic data covers only 1997-2005, we include 2006 births only if the pregnancy started in 2005. We only consider months when the woman was interviewed about her pregnancy status directly by enumerators.

Table 3.1 presents the total sample of women, which ranges from 3,033 women in 1997 to 3,528 in 2006. Panel A reports the average number of times a woman was interviewed each year, which varies from almost ten times a year in the first year of the study to approximately two times in 2006. On average, 3356 women were interviewed 5.5 times each year; in 2004, when the civil conflict was at its peak, the average number of interviews reached a low of 1.20 times per year. Appendix Table C.2 shows how the sample composition change over the ten years of the study, indicating how many women leave and join the survey in each year.

Table 3.1, Panel B, shows the average observed pregnancy status over time by women. Across all years, we observe, on average, the reported pregnancy status of 2,496 women (74% of the total sample of women interviewed). It varies from 94% in 1998, the second year of the survey, to 9% in 2004, during the peak of the conflict discussed in Section 3.1. From these women, on average, 254.7 of the women interviewed (8%) report a live birth in a year.

Panel B shows how the sample size changes when we apply the necessary exclusion restrictions for our analysis: considering only women who were directly interviewed by an enumerator in that year, who had a live birth, and who lived in CVFS for at least part of her pregnancy, so we can assign her a distance to a clinic.[6]

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of distance to clinics on pregnancy uncertainty will not be biased.

[6]When the outcome of interest is the month a woman detected her pregnancy we have an additional restriction: we need to observe a month when the woman knew she was pregnant. This is not necessary when the outcome is a binary variable equal to one if the woman knew she was pregnant in that trimester. The change in sample size is shown in the last two rows in Table 3.1, Panel B.

3.2 Empirical Strategy

In this section, we present the two main estimation approaches to understand the effects of access to pregnancy tests on pregnancy status knowledge. First, we measure the effect of distance to pregnancy tests on overall pregnancy uncertainty, where each observation is a pregnancy, and the main outcome is the month a woman learned about her pregnancy.[7] Second, we measure the effect of distance to pregnancy tests on pregnancy uncertainty by trimester; in this case, each observation is a month of pregnancy when a woman was interviewed. We run three separate regressions, one for each trimester, where the main outcome is if a woman is aware of her pregnancy in that trimester.

3.2.1 Overall Pregnancy Uncertainty

Our first approach is to estimate the following using observations at the woman-pregnancy level:

$$ReportPreg_{iph} = \alpha + \beta_1 DistPT_{iph} + \beta_2 DistFP_{iph} + \theta_{iph} + \eta_{iph} + \gamma_{iph} + \epsilon_{iph} \tag{3.1}$$

$ReportPreg_{iph}$ indicates the month of gestation when a woman i during pregnancy p and living at a neighborhood h reports she was pregnant; this number ranges from 1 to 9. $DistPT_{iph}$ equals one if neighborhood h is located above the median distance to a clinic with pregnancy tests, and zero otherwise. In Appendix Table C.3, we show that our results are robust to using a continuous measure of distance.

To isolate the effect of distance to pregnancy tests on pregnancy uncertainty, we consider access to family planning methods as a potential confounder. The literature shows that proximity to women's health clinics affects the take-up of services, such as preventive care, and contributes to a decline in fertility rates (Lu and Slusky, 2016; Rossin-Slater, 2013; Bailey, 2012). Therefore, access to contraception could itself affect women's uncertainty by

[7] After filtering our sample according to the factors described in Section 3.1, most women who remain in our sample have only one live birth (65%). The remaining have among two and five live births.

decreasing the risk of being pregnant, and affect our main estimates due to omitted-variable bias. We specifically control for the distance to family-planning clinics with $DistFP_{iph}$, which equals one if the woman lives in a neighborhood where the distance to the closest clinic offering contraceptive methods is above the median distance, and zero otherwise.

We add age fixed effects, θ_{iph}, since uncertainty about pregnancy may vary with sexual activity and experience, which are correlated with age. We consider age at the first month of pregnancy.

We also control for the number of times during pregnancy a woman was interviewed with fixed effects η_{iph}. This controls for the fact that women interviewed more times are more likely to be interviewed more to the end of the pregnancy, and therefore more likely to be aware of their state.[8]

We include strata-by-year fixed effects, γ_{iph}, to account for possible changes specific to an area over time that could be correlated with pregnancy uncertainty or the access to clinics. These fixed effects control for annual changes in the infrastructure of a strata—such as the improved roads and opening of new schools—and in demographic aspects of a strata—such as changes in the typical family size and age of first birth.

The main identifying assumption to estimate the effect of distance to clinics with pregnancy tests on pregnancy uncertainty is that, after controlling flexibly for a woman's age, the number of times she is interviewed during her pregnancy, unobservable characteristics of the location where she lives that may vary across years, and distance to family-planning services, the distance to a clinic offering pregnancy tests is exogenous. A threat to validity is if, after controlling for the variables mentioned above, other unobservable factors that affect pregnancy uncertainty vary systematically with the distance to clinics with pregnancy tests. For example, the opening of new roads, or new schools, could be positively correlated with the location decision of clinics and also with the location where more educated households decide to live. If a woman living in such households is more likely to be aware of her pregnancy status, and if such changes are not captured by our strata-year fixed effects, it can bias our estimates.

[8]In Appendix Table C.4 we show that our results are robust to not controlling for the number of times during pregnancy a woman was interviewed.

3.2.2 Pregnancy Uncertainty by Trimester

To capture the effect of distance to clinics with pregnancy tests on pregnancy uncertainty by trimester, we estimate a model similar to Equation 3.1, but at the woman-month level. For each trimester, we estimate the following model:

$$ReportTrimester_{imph} = \alpha + \beta_1 DistPT_{imph} + \beta_2 DistFP_{imph} + \theta_{imph} + \rho_{imph} + \gamma_{si} + \epsilon_{imph} \quad (3.2)$$

where $ReportTrimester_{imph}$ is a binary variable equal to one if a woman i during month m of pregnancy p living in neighborhood h reported being pregnant. We estimate Equation 3.2 separately for each trimester, and in each estimation, we restrict our sample to women interviewed during that trimester.

$DistPT_{imph}$ is a measure of distance as defined above, but in this case, allowed to vary monthly. θ_{imph} are age fixed effects. ρ_{imph} are month-of-pregnancy fixed effects, which account for the month of pregnancy when the woman was interviewed. We control for the month of interview because a woman is more likely to know and report her pregnancy as it advances. γ_{iph} are strata-by-year fixed effects, as in Equation 3.1.

We cluster the standard errors of all estimations of Equation 3.1 and 3.2 at the neighborhood level since this is the source of variation in the distance to the nearest health center.

3.3 Effect of Distance to Clinics with Pregnancy Tests

In this section, we present the impact of distance to clinics with pregnancy tests on pregnancy uncertainty. First, we show the effect of distance on the whole pregnancy, measured at the woman-pregnancy level. Second, because the estimate may mask important heterogeneity across pregnancy term (i.e., first, second, or third trimester), we show the effect of distance by trimesters, measured at the woman-month level. For the same reason, we present all results separately by women prior experience with pregnancy.

3.3.1 Effect of Access to Pregnancy Tests on Overall Pregnancy Uncertainty

Table 3.3 presents the estimates from Equation 3.1, showing the effects of distance to a clinic with pregnancy tests on the month a woman learned she was pregnant, controlling for the distance to a clinic with family planning, the woman's age, the number of times she was interviewed during pregnancy, and strata-by-year unobservable and constant characteristics. In Column 1, we see that going from below to above the median distance to access pregnancy tests increases by 0.23 months the time women report their pregnancy, or by 5%.

While the estimate in this first column of the average effect of distance is moderately large and statistically significant, the magnitude of the coefficients masks important heterogeneity across prior experience with pregnancy.

We examine this further in columns 2 and 3 of Table 3.3. We see the effect of distance to pregnancy tests on knowledge about pregnancy is coming from women who have previous experiences with pregnancy. Moving from below to above the median of distance to clinics with pregnancy tests increases the month women with prior pregnancies report being pregnant by 0.52, or 11.2%. The effect is not significant for women in their first pregnancy. In Appendix Table C.3 we show that our results are robust to a different definition of distance to a clinic with pregnancy tests.[9]

3.3.2 Effect of Access to Pregnancy Tests on Pregnancy Uncertainty by Trimester

We now look for differences in the effect of access to pregnancy tests on pregnancy uncertainty by trimester of pregnancy. We care about the effect by trimester for two reasons: we expect women to be more likely to report a pregnancy as gestation advances, and the health benefits of detecting a pregnancy earlier are larger.

Table 3.4 shows the effect of distance to clinics with pregnancy tests on the probability

[9] Appendix Table C.3 shows the results when distance to a clinic with pregnancy tests is defined as the logarithm of the distance in miles, controlling for the logarithm of the distance to a clinic offering family planning services. The effect is still positive but not significant for all women, and positive and significant for women with prior pregnancies.

of reporting a pregnancy in each trimester. If access to pregnancy matters, we expect the coefficient to be negative, which is the opposite of what we expected in Table 3.3—where the outcome was the month the pregnancy was reported.

Column 1 of Table 3.4 shows that moving above the median of distance to clinics with pregnancy tests decreases by 4.5 percentage points the probability a woman knows she is pregnant in the first trimester. This reduction in knowledge corresponds to a 16.1% decrease in the mean of 28% of women who know their status at this point of pregnancy.

In columns 2 and 3, we explore potential heterogeneities in this effect by women's previous experience with pregnancies. The results follow the pattern we found in the overall pregnancy results in Table 3.3: the distance to clinics offering pregnancy tests is a binding constraint only to women who have had previous pregnancies. Column 2 of Table 3.4 shows that living above the median distance to clinics with pregnancy tests decrease the probability women with prior pregnancies know they are pregnant in the first trimester by 6.2 percentage points—a decrease of 25.2% from the mean. Column 3 shows that the effect is not significant for women in their first pregnancy.

Columns 4–6 and 7–9 show the effect of distance to clinics with pregnancy tests on knowledge in the second and third trimester, respectively. The effect of distance on all women and on women with prior pregnancies is larger in the second trimester; it represents a decrease on the probability of knowing about pregnancy in the second trimester of 6.5 percentage points (or 8.5%) for all women, and of 12.4 percentage points (or 16.8%) for women with prior pregnancies.

In the third trimester, the effect of distance to clinics with pregnancy tests is negative but not significant for all women, and negative but smaller than in other trimesters for women with prior pregnancies—equal to a decrease of 2.5 percentage points in the probability of knowing she is pregnant (or 2.5%).

Table 3.4 presents two main results. First, the distance to clinics with pregnancy tests do not affect women in their first pregnancy. These women would have had less experience with knowing the signs and symptoms of pregnancy in the early months and would be less likely to act upon beliefs to go to a clinic with a pregnancy test. Thus, the effects of distance in all trimesters are statistically insignificant (with wide confidence intervals). Second, Table

3.4 also shows that the distance constraint binds the most for women in the earlier months of pregnancy. In the last trimester of pregnancy, the need to access a clinic to confirm a pregnancy is smaller since other signs of pregnancy are likely more evident.

3.4 Conclusion

In this paper, we provide new evidence on when women learn about their pregnancy status, and how constraints on access to pregnancy tests affect the timing of learning. We use a novel high-frequency dataset over ten years in Nepal, where women are asked monthly about their pregnancy status. These unique data allow us to measure uncertainty about pregnancy more accurately than with retrospective reports. We also measure changes in access to pregnancy tests over ten years of data as new clinics open or existing clinics begin stocking pregnancy tests, separately controlling for clinics offering family planning to account for possible confounding factors related to distance to clinics.

We find that women who live farther from a clinic with pregnancy tests report later their pregnancy. By increasing the distance to a health center with pregnancy tests from less to more than 0.92 miles (median distance), women increase by one week the time they report their pregnancy. The likelihood of knowing their pregnant status in the first trimester also decreases by 4.5 percentage points, or by 16.1%. These effects differ by a woman's previous experience with pregnancies and by trimester; the distance constraint is binding the most for women with previous pregnancies, and especially in the first trimesters of pregnancy. These results show that access to clinics with pregnancy tests is a binding constraint when symptoms, or beliefs, are strong enough to motivate a woman to confirm her pregnancy.

A back-of-the-envelope calculation helps to estimate this impact in terms of health outcomes. According to Caughey, Nicholson and Washington (2008), having the first-trimester obstetric ultrasound (OBUS) at 12 weeks of gestation or less decreases the rate of post-term pregnancy (42 weeks or longer) by 27% when compared to having it from week 13–24 of gestation. Assuming that a woman that learns of her pregnancy in the first trimester also gets her OBUS in the first trimester, living within 0.92 miles from a clinic with pregnancy tests decreases the probability of post-term pregnancy by 1.2%. This reduction in post term pregnancy has further effects on the health of the mother and the fetus since the risk of complications increase with gestation beyond 40 weeks—e.g., the risk of cesarean delivery increases by 100% and of neonatal sepsis increases by 50% (Alexander, McIntire and Leveno, 2000).

Our study covers a period of high maternal mortality ratio in Nepal, which is still an important issue in many other countries. Our results show that access to clinics with pregnancy tests is an important constraint for pregnancy awareness. Since pregnancy testing is a relatively inexpensive technology, improving its availability has the potential to affect pregnancy knowledge, and in the end, to improve the woman and the infant's conditions during gestation and beyond.

3.5 Figures and Tables

Table 3.1: Sample of Women by Year

	Year									
	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
Panel A: Married Female Respondents										
Sample	3033	3296	3278	3364	3402	3558	3590	3024	3486	3528
Times Interviewed per Year (Avg)	9.76	8.67	10.05	9.23	5.84	3.11	3.04	1.20	2.41	2.18
Panel B: Observed Pregnancy Status										
Observed Pregnancy Status	2630	3096	2683	2642	2681	2826	2891	267	2597	2646
Had a Live Birth	220	323	345	268	286	288	252	191	191	183
Living in CVFS during Pregnancy	183	227	224	184	215	223	207	91	111	85
Observed Month of Pregnancy Detection	181	216	223	179	202	190	170	54	103	75

Notes: This table shows the total sample of married female respondents in the data, in panel A, and the sample used in our analysis, in panel B. The third row in panel B corresponds to our main sample of women when the outcome is knowledge by trimester. The last row in panel B corresponds to our main sample of 1,593 woman-pregnancies when the outcome is month of detection. The difference between these two last rows corresponds to women who were interviewed at some point during pregnancy when they did not know their status, but were not interviewed again; therefore they cannot be part of the sample when the outcome is month of pregnancy detection.

Table 3.2: Sample of Health Providers by Neighborhood and Year

	Year								
	1997	1998	1999	2000	2001	2002	2003	2004	2005
Total Clinics	94	96	106	108	113	128	133	142	168
Total Clinics with Pregnancy Tests (PT)	24	27	34	34	43	59	66	78	103
Distance to PT Clinics (Miles)	1.128	1.093	1.006	1.016	0.865	0.739	0.711	0.583	0.478
Total Clinics with Family Planning (FP)	82	84	93	95	102	117	119	129	154
Distance to FP Clinics (Miles)	0.599	0.625	0.623	0.624	0.585	0.532	0.505	0.490	0.440
Correlation Distance to FP and PT Clinics	0.496	0.481	0.538	0.532	0.571	0.652	0.659	0.915	0.942

Notes: This table shows the total number of any type of health provider, the total number of health providers offering pregnancy tests, and the total number offering family planning services. The distance is the average distance from a clinic to the centroid of neighborhoods in our sample. The health provider data for 2006 is not available.

Table 3.3: Impact of Distance to Clinic with Pregnancy Tests on Overall Pregnancy Knowledge

	Month Reported Pregancy		
	All women	Prior Pregnancies	No Prior Preg.
	(1)	(2)	(3)
Distance to Pregnancy Tests	0.227**	0.515***	-0.184
	(0.112)	(0.177)	(0.329)
Distance to Family Planning	0.074	-0.149	0.482
	(0.117)	(0.194)	(0.315)
Observations	1,593	636	308
Adjusted R-squared	0.112	0.148	0.106
Mean Dependent Var.	4.591	4.613	4.617

Notes: This table shows the result of three separate regressions. All columns include strata-year fixed effects, number-of-interviews-during-pregnancy fixed effects, and age fixed effects. We have information of prior pregnancies for 944 woman-pregnancy, which affects the sample size in columns 2 and 3. Standard errors are clustered at the neighborhood level.

Table 3.4: Impact of Distance to Clinic with Pregnancy Tests on Pregnancy Knowledge by Trimester

	Knew in First Trimester			Knew in Second Trimester			Knew in Third Trimester		
	All women	Prior Preg.	No Prior Preg.	All women	Prior Preg.	No Prior Preg.	All women	Prior Preg.	No Prior Preg.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Distance to Pregnancy Tests	-0.045**	-0.062**	0.015	-0.065**	-0.124***	-0.002	-0.005	-0.025**	0.033
	(0.020)	(0.030)	(0.059)	(0.028)	(0.042)	(0.058)	(0.009)	(0.012)	(0.026)
Distance to Family Planning	-0.039*	-0.034	-0.100*	0.014	0.044	-0.060	0.013	0.007	0.010
	(0.021)	(0.032)	(0.059)	(0.027)	(0.044)	(0.054)	(0.008)	(0.011)	(0.025)
Observations	2,792	1,324	557	2,732	1,305	537	2,632	1,280	511
Adjusted R-squared	0.146	0.145	0.207	0.089	0.106	0.163	0.027	0.048	0.052
Mean Dependent Var.	0.28	0.246	0.266	0.769	0.736	0.769	0.974	0.976	0.967

Notes: This table shows the result of nine separate regressions. All columns include strata-year fixed effects, month-of-interview fixed effects, and age fixed effects. Standard errors are clustered at the neighborhood level. We have information of prior pregnancies for a smaller share of woman-month pairs, which affects the sample size in the results splitting women by their experience with prior pregnancies. Each column is conditional on the woman being interviewed in that trimester. Standard errors are clustered at the neighborhood level.

CHAPTER 4

THE EFFECT OF PRESIDENTIAL ELECTION OUTCOMES ON ALCOHOL DRINKING

Political polarization has been increasing in the United States in the last years (Iyengar et al., 2019). In 2016, 52% of Americans reported that the upcoming presidential election was a significant source of stress, despite their political affiliation (American Psychological Association, 2017). Election outcomes are largely related to an increase in anxiety and a decline in self-reported well-being (Stanton et al., 2010). However, we do not know if election outcomes also increase risky health behaviors.

In this paper, we measure the impact of supporting a losing presidential candidate on alcohol expenditures—a risky health behavior.[1] We use data on total daily purchases of alcohol at the household level in a difference-in-differences (DID) model with a continuous treatment variable (Acemoglu, Autor and Lyle, 2004) to test if counties with larger support for losing presidential candidates consume relatively more alcohol after the election than counties with smaller support for the losing candidate. We test separately for four presidential elections in the United States, from 2004 to 2016.

Our results show that the effect of supporting a losing candidate on alcohol expenditure is positive and significant only for the 2016 election. Within 30 days from Election Day, each 10 percentage point increase in support for the losing party increases alcohol expenditure by 1.1%. Our results are robust to different bandwidths (20 and 40-day). Our results also hold when using a regression discontinuity (RD) in time, which controls flexibly for unobservable factors that change non-linearly over time and are related to alcohol consumption.

We find that support for the losing candidate increased alcohol expenditure only after the 2016 Election Day, which is evidence that the 2016 election was different from others.

[0] Jointly with Rodrigo Schneider

[1] For a discussion about the correlation between alcohol expenditure and alcohol consumption in the Nielsen dataset, see Cotti, Dunn and Tefft (2014).

According to the literature, it was a unique election due to the emotional charge of political campaigns (Nai, Martínez i Coma and Maier, 2019), the unprecedented use of social media, the decline in trust in the mainstream media (Allcott and Gentzkow, 2017), and the overwhelming wrong predictions of who would become the next president (Valentino, King and Hill, 2017).

To understand what aspects of the population drives our results, we split the 2016 counties by above and below the median of demographic and economic characteristics. We find that counties with a more educated population, higher income, higher share of unemployed population and immigrants are the ones where the share of supporters for the losing party positively impacts alcohol expenditure on and after Election Day. These characteristics agree with findings that places with a more educated population and with a higher share of minority populations (e.g., immigrants) supported the losing candidate in the 2016 election (Tashjian and Galván, 2018).

We run a second analysis changing the outcome from alcohol expenditures to fatal car crashes to discuss policy implications of our results. We use as an outcome the universe of such fatalities in the United States from the Fatality Analysis Reporting System (FARS). We find positive but non-significant effects of supporting a losing candidate in all election years; in 2016, our point estimate is larger, which is stronger evidence that the effect is different from zero. For each 10 percentage point increase in support for the losing party, fatal car accidents increase by approximately 1.56% in the 30 days following Election Day.

Our paper speaks to three strands of the literature. First, it is related to studies on why people care about election outcomes and dislike to vote for the losing candidate. The fact that individuals do vote and participate in elections, despite the marginal probability of casting a pivotal vote, shows that there are psychological motivations to vote (Downs, 1957). Individuals also report a significant drop in their life satisfaction when they support the losing candidate, which shows that they are affected by election outcomes (Pinto et al., 2019).

Second, our paper speaks to the literature about how voting for the losing candidate increases fear and anxiety once election results are revealed (Marcus and Mackuen, 1993). Several studies show that stress and anxiety increase following Election Day, especially among those who supported the losing candidate. The literature reports these spikes in stress and anxiety both from using cortisol as a biomarker (Stanton et al., 2010; Hoyt et al., 2018) and from self-reports (Hagan et al., 2020; Majumder et al., 2017). Third, our paper is related to the literature on the use of alcohol a form of self-medication for anxiety (Darden and Papageorge, 2018). Several studies find a positive correlation between alcohol use and stress in different settings, in what is known as the stress-dampening effect of alcohol (Holahan et al., 2001; Carrigan et al., 2008).

Our results provide novel evidence that risky behaviors change in response to electoral results depending on who wins, and that such effect depends on characteristics of the electoral process. While our analysis cannot pin down why only the 2016 Election Day generated such an increase in the risky behavior of spending more on alcohol, we acknowledge a combination of factors that are unique to 2016: the harsher tone of political campaigns, the use of social media, and the unexpected results.

Our findings are relevant for two reasons. First, we show that election outcomes increase risky health behaviors and that the increase depends on the characteristics of the election. Such an increase in alcohol consumption can have long-term effects on health and on human capital which can be a public health concern. Second, some of the components of the 2016 elections are not unique to the United States, nor to electoral processes overall. Therefore, our results can be informative about changes in risky behaviors in other anxiety-triggering situations.

This paper is organized as follows: Section 4.1 presents a simple conceptual framework of why the demand for alcohol is expected to increase after Election Day for supporters of the losing candidate. Section 4.2 describes the alcohol purchases and election data. Section 4.3 presents the DID and RD in time empirical approaches to estimate the effect of election outcomes on expenditures with alcohol. Section 4.4 presents the main results and robustness checks. Section 4.5 shows the policy implications of our results on fatal car crashes. Section 4.6 concludes.

4.1 Conceptual Framework

In this section, we introduce a simple conceptual framework to predict how alcohol consumption will change after Election Day depending on the share of voters for the losing party in a county. We set up a framework of changes in individual behavior that explains what we observe in the aggregated data.

We have three main assumptions: (i) individuals care about election outcomes and dislike to lose more than they enjoy winning; (ii) voting for the losing candidate increases fear and anxiety once election results are revealed; and (iii) alcohol is self-medication to decrease the feelings of fear and anxiety. In the end, we discuss factors that differentiate the 2016 election from others—including the role of the media and the unexpected results.

First, individuals care about elections. The fact that they turn out to vote, even though the probability of casting a pivotal vote is extremely low, shows that there are psychological motivations behind the decision to participate in the electoral process that are beyond the rational cost-benefit analysis of voting proposed in the seminal work by Downs (1957).[2] As Zech (1975) describes, among the psychological reasons explaining the decision to vote is the feeling of complying with civic duty and the satisfaction of voting for the winning candidate.[3] An analysis of the 2012 and 2016 U.S. presidential elections shows that voters not only care about voting for the winning candidate but feel a significant drop in their life satisfaction when they support the losing candidate (Pinto et al., 2019). The results are especially strong and consistently significant across all measures of happiness among supporters of the losing candidate in the 2016 election. This fact illustrates that losing affects subjective life satisfaction more than winning, especially during an election where the losing party was expected to win. After Election Day, supporters of the losing party use traumatic metaphors to make sense of their loss (Carmack and DeGroot, 2018).

Second, during elections, voters become emotionally involved with the electoral campaign, through fear of the results and enthusiasm with a candidate (Marcus and Mackuen, 1993). To

[2]See Schneider, Athias and Bugarin (2019) for a recent discussion of the consequences of voting costs on electoral outcomes.

[3]For more on the benefits of voting for the winning candidate, see Bartels (1985); Kenney and Rice (1994); Morton and Ou (2015).

measure how elections are correlated with stress, some studies use concentrations of salivary cortisol—a steroid hormone used as a biomarker of the stress response (Hellhammer, Wüst and Kudielka, 2009). Other studies use self-reported level of stress or anxiety as an outcome. Most studies agree that stress increases on and following Election Day, and even more among voters for the losing party.[4] There is still some discussion in the literature about how long the effects last.

In the United States, Stanton et al. (2010) find that, after the 2008 presidential elections, the level of cortisol among those who supported the (losing) Republican candidate rose, while it remained constant among those who supported the Democratic candidate. Hoyt et al. (2018) find similar results—a higher level of cortisol across all adults leading to the election; after the election, they find a smaller recovery in the levels of cortisol for those who had negative attitudes towards the winning candidate. Trawalter et al. (2012) examine the 2012 election and also find an increase in cortisol on Election Day, mainly among Republicans. Within a week after Election Day, however, the authors do not find different effects on cortisol levels by political affiliation. Although these studies arrive at a similar conclusion, an important limitation is that their sample size is usually small and the collection of salivary cortisol is voluntary.

Studies that rely on self-reported stress and anxiety find similar results. Pinto et al. (2019) use Gallup survey data to examine the effect of the 2012 and 2016 presidential elections on subjective well-being, including measures of stress and worry. They find a decline in subjective well-being among voters for the losing party that lasts, on average, two weeks. Hagan et al. (2020) detect an increase in reported symptoms of stress after the 2016 presidential elections among college students; the increase is accentuated among minority groups and those that supported the (losing) Democratic party. Majumder et al. (2017) also find an increase in self-reported stress and anxiety leading to and after the 2016 elections; in the post-period, being affiliated to the Democratic Party was correlated with higher measures of election-related stress. And Smith, Hibbing and Hibbing (2019) show that, in the first

[4]The act of casting a ballot itself is also related to increases in stress. In Israel, Waismel-Manor, Ifergane and Cohen (2011) find an increase in cortisol levels for voters at the ballot box. The authors do not report differential effects among those that expect to win or lose.

trimester of 2017, around 40% of the American population reported feeling stress as a consequence of politics.

Our third hypothesis is that alcohol is used on and after Election Day to alleviate symptoms of anxiety and stress—because alcohol use reduces stress or because people are more inclined to drink it under stress (Sayette, 1999). The use of alcohol has been interpreted as self-medication to mental health problems, especially to explain the comorbidity of anxiety and substance use disorders (Darden and Papageorge, 2018; Bolton, Robinson and Sareen, 2009; Smith and Randall, 2012). Besides, several studies find a positive correlation between alcohol use and stress, in what is known as the stress-dampening effect of alcohol. Such use of alcohol depends on personal characteristics, beliefs about its efficacy, and the type of event triggering stress (Holahan et al., 2001; Carrigan et al., 2008; Dawson, Grant and Ruan, 2005; Keyes et al., 2012). In the lab, alcohol diminishes self-reported anxiety to unexpected threats (Bradford, Shapiro and Curtin, 2013). However, the pharmacological explanation for the effect of alcohol on stress remains unclear (Sayette, 2017).

In addition to these three assumptions, we expect features of the 2016 presidential elections to add another layer of stress and anxiety to the days following Election Day, which would raise even more the consumption of alcohol among supporters of the losing party. Leading to Election Day, the 2016 elections were already different due to the emotional charge of political campaigns (Nai, Martínez i Coma and Maier, 2019). The use of social media reached a high in 2016—for example, the number of Facebook users jumped from 890 million in 2012 to 1,227 million in 2016 (Facebook, 2016). The decline in trust in the mainstream media and the rise of false articles that can mislead voters are also unprecedented to the 2016 elections (Allcott and Gentzkow, 2017). Another unique feature of the 2016 Election Day was the wrong predictions of who would become the next president; the vast majority of polls predicted that Donald Trump would lose the popular and Electoral College vote (Valentino, King and Hill, 2017).

4.2 Data

We use two main sources of data in our analysis. The total expenditure on alcohol comes from the Nielsen Homescan Consumer Panel Dataset. The voter's data come from the MIT Election Data and Science Lab. We aggregate alcohol purchases to the county-daily level, creating a variable of total expenditure.

Alcohol data The alcohol expenditure data are built from alcohol purchases from households in the Nielsen Homescan Consumer Panel Dataset.[5] The data are a longitudinal panel of approximately 40,000 (2004-2006) to 60,000 US households, and it covers the period from 2004 to 2017.[6] We consider all households in the sample and aggregate their purchases to the county-daily level.

Households scan purchases of alcohol that they take home; therefore, our analysis excludes any form of alcohol that is not brought into the house before consumption (e.g., in bars and restaurants). Panelists use in-home scanners to record their purchases, from any outlet, intended for personal, in-home use. Nielsen estimates that, on average, 30% of households' consumption is accounted for in the consumer panel data. Our main outcome is the total dollars spent on alcohol. We consider the following categories of alcoholic beverages: beer, wine, and liquor. The average daily expenditure with alcohol in a county varies from 17.30 dollars in 2004 to 22.20 dollars in 2016 (dollars in the current year).

Voter's data The election data is from the MIT Election Data and Science Lab. We use reports the percentage of the vote at the county level for both Democrats' and Republicans' presidential candidates in the 2004, 2008, 2012 and 2016 U.S. elections. We use the share of the vote for the candidate who lost the presidential election as our main independent

[5] The researcher's own analyses were calculated (or derived) based in part on data from the Nielsen Company (US), LLC and marketing databases provided through the Nielsen Dataset at the Kilts Center for Marketing Data Center at the University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researcher and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

[6] Households join and leave the panel over the years. Around 20 percent of households drop from the sample every year, and new households are included to keep the average number of households at around 60,000 per year. Despite the rotation, more than 15 percent of households in the sample in 2016 have been part of it since 2004.

variable to explain alcohol expenditure. The average share of support for the 2016 losing candidate in our data is 47.31 percent, varying from 16.11 to 95.91 percent.

4.3 Empirical Approach

Our objective is to identify the impact of supporting a losing presidential candidate on alcohol consumption. Although we have information at the individual level on alcohol expenditure, we only observe presidential electoral outcomes at the county level. Thus, we cannot directly test the effect of supporting a losing candidate on alcohol consumption. To circumvent this issue, we aggregate by day and for each county the individual expenditure on alcohol. Then, relying on the assumption that our aggregation of individual expenditure on alcohol represents the consumption of alcohol at the county-daily level, we test our research question using a DID model with a continuous treatment variable, following Acemoglu, Autor and Lyle (2004). More specifically, we test whether counties with larger support for losing presidential candidates consume relatively more alcohol after the election than counties with smaller support for the losing candidate. Formally, we estimate the following model for each presidential election separately:

$$Log(Alcohol_{cde}) = \alpha + \gamma After_{de} + \beta After_{de} * Share_{ce} + \omega_c + \delta_w + \lambda_m + \epsilon_{cde}, \qquad (4.1)$$

where $Alcohol_{cde}$ is the total expenditure on alcohol in county c and day d, within 30 days before and after the Election Day of election e.[7] The difference in alcohol expenditure after the election is capture by γ and our main coefficient of interest is captured by β, which shows the change in alcohol consumption after Election Day across varying degrees of support at the county level for the losing candidate (i.e., $Share_{ce}$). If our theory is correct, this coefficient should be positive showing that alcohol expenditure increases more after Election Day among counties with higher levels of support for the presidential candidate who lost. We include fixed effects at the county level (ω) to control for time-invariant county characteristics. We

[7] We test different bandwidths and obtain similar results.

include days of the week (δ) and month (λ) fixed effects to control for seasonality and differences in alcohol consumption intensity over the weekend. Finally, ϵ_{cde} represents the error term.

We take advantage of the high-frequency nature of our dataset and also propose a regression discontinuity design (RDD) model that gives us more flexibility to test bandwidth selection and manage omitted variables. The RDD approach allows us to rely on an identification strategy that is more flexible than a DID model. While the latter restricts unobserved variables related to alcohol consumption to vary linearly, the former allows unobserved factors to act non-linearly over time. Thus, using RDD allows us to circumvent the potential endogenous relationship between the error term and Election Days by allowing the function of the running variable to vary flexibly across the cutoff. Our RDD is constructed following Davis (2008), Anderson (2014) and Auffhammer and Kellogg (2011), who propose an RD where time is the running variable. Formally, we add the following term to Equation 4.1 and create an RD in time:

$$\sum_{n=1}^{2} \kappa_n (Date_{de} - ElectionDay_{de})^n \tag{4.2}$$

Including the running variable (i.e., $Date_{de} - ElectionDay_{de}$) to our model allow us to capture closeness to Election Day (i.e., cutoff) and, by adding the exponent to our running variable, we allow unobserved variables related to alcohol consumption to vary non-linearly. Finally, we flexibly test for different bandwidths using this specification.[8]

4.4 Results

We start this section estimating Equation 4.1 and then reporting the results of our main specification. In Table 4.1, we estimate Equation 4.1 separately for each U.S. presidential election available in our sample using a 30-day bandwidth. We only find positive and significant results in the 2016 election: there is a 1.1% increase in alcohol expenditure after Election Day

[8]We follow the approach recommended by Gelman and Imbens (2019) and restrict our attention to estimators based on local linear or quadratic polynomials.

for each 10 percentage point increase in support for the losing party (Democratic party in this case). The evidence that alcohol consumption increases among Democratic stronghold counties exclusively after the 2016 U.S. presidential election is consistent with our hypothesis that voters facing adverse and *unexpected* electoral results feel anxious and stressed and, thus, self-medicate with alcohol. In the Appendix, we provide two robustness checks. First, Table D.1 shows that our results are robust to changing the bandwidth to 20 and 40 days. Second, in Table D.2, we relax the assumption that unobserved variables related to alcohol expenditure vary linearly and add equation 4.2 to our main model specification changing it to an RDD model. Our results are not sensitive to this change in the specification.

To provide a better understanding of the populations more affected by what may have been a disappointing electoral outcome in 2016, we split our sample between above- and below-median distribution across several economic and demographic variables.[9] Then, we test whether alcohol expenditure post-election is heterogeneous within these groups. Our goal is to test whether minority groups—who were more likely to report feeling threatened by the winning presidential candidate's remarks (Swank, 2018)—that lived in counties that strongly supported the losing candidate responded more to the results. Table 4.2 shows that counties with a larger share of people with BA degrees; immigrants; income; and unemployment rate spent disproportionately more in alcohol after Donald Trump's election as a consequence of higher levels of support for the losing candidate (Hilary Clinton).

Tashjian and Galván (2018) show that, during the 2016 presidential election campaign, marginalized groups, including immigrants and sexual minorities, reported feeling vulnerable and victimized. Our results show that places with larger support for the losing candidate and above-median share of immigrants in 2016 had a larger expenditure on alcohol. This result is consistent with our hypothesis that citizens use alcohol to mitigate post-election feelings of fear and hopelessness. Places with higher unemployment and larger support for the losing candidate also had a relatively larger increase in alcohol expenditure, which may be related to feelings of vulnerability. Among places with a large share of adults with BA

[9] The county characteristics are taken from the American Community Survey. The share of the population with BA degree is the average between 2013 and 2017; the share of immigrants is from 2016; the average income is from 2017, and the share of the unemployed population is from 2016.

degree, or higher income, post-election alcohol expenditure increased if the county were more likely to support the losing candidate.[10]

Next, we exploit the fact that our DID model allows for variance in treatment and investigate which part of the distribution in the vote share for the losing party drives our results. In Figure 4.1, we split counties into five equal parts, for each of the four elections considered, according to the distribution of vote for the losing candidate. Then, we compare the expenditure on alcohol post-election to pre-Election Day contrasting each quintile to the baseline (i.e., first quintile). As we show, the only significant effect is found in the fifth quintile of the 2016 presidential election after a rise in alcohol consumption in the fourth quintile. In Figure 4.2 we change our model specification to RDD and report an estimation restricting our sample to the 4th and 5th quintiles. We find that within the first two weeks after the 2016 election there was a significant increase in alcohol expenditure among democratic stronghold counties. As the average support for Hilary Clinton in the counties belonging to the fifth quintile was 71 percent, our results suggest that places predominantly voting for the democratic candidate were more likely to consume alcohol after the unexpected 2016 electoral outcomes. This result is in line with Kőszegi and Rabin (2006) model with gain-loss utilities, where reference points are derived from consumers' rational expectations about outcomes and emotional cues are driven by deviations from expectations. As the empirical literature exploring this model shows, people can change their usual behavior when faced with unexpected losses in dramatic ways (e.g, being more likely to be the perpetrator of domestic violence or increasing sentence lengths assigned by judges) (Card and Dahl, 2011; Eren and Mocan, 2018).

[10]Part of the heterogeneous result by the level of education can be understood in light of the positive correlation between the level of education and support for minorities. For example, in the case of sexual minorities, we know that the 2016 election had a large sexuality gap—LGBTs were four times more likely to support Hilary Clinton (Swank, 2018)—and that access to higher education is related to higher support for sexual minorities (Webb and Chonody, 2014). The same reasoning applies to places with a higher income, which have a larger share of the population with BA degree (the correlation among these variables equals 0.74).

4.5 Policy Implications

In this section, we discuss the policy implications of our results. We show in Table D.1 that the increase in expenditures with alcohol is larger in the short term. Therefore we focus on a potential short-term consequence of an increase in alcohol consumption, despite known long-term effects of alcohol consumption on health (Murray et al., 2018).

We estimate the impact of supporting a losing candidate on fatal car crashes. A large literature shows that the consumption of alcohol is positively correlated with car crashes (Levitt and Porter, 2001). Given our finding that alcohol consumption increases among Democratic stronghold counties after the 2016 U.S. presidential elections, as shown in Table 4.1, we expect a sharper increase in fatal car crashes among counties with higher levels of support for the presidential candidate who lost.

We estimate a Poisson regression model because the outcome of interest (fatal car crashes) are count data. Fatal car crashes are a rare event, therefore we aggregate the daily data to two periods, before and after Election Day. The data are obtained from the FARS, built by the National Highway Traffic Safety Administration. It contains the county and date of every vehicle accident with a fatal victim. We estimate the following model:

$$CarCrashes_{cte} = \upsilon + \zeta After_{te} + \tau After_{te} * Share_{ce} + \iota_c + \varepsilon_{cte}, \tag{4.3}$$

where $CarCrashes_{cte}$ is the total number of fatal car crashes in county c and period t, within 30 days before and after Election Day of election e. The difference in fatal car crashes after the election is captured by ζ. Our main coefficient of interest is captured by τ, which shows the change in fatal car crashes after Election Day across varying degrees of support at the county level for the losing candidate (i.e., $Share_{ce}$). Following our hypothesis that higher alcohol consumption would lead to a higher occurrence of fatal car crashes, we expect this coefficient to be positive. We include fixed effects at the county level (ι) to control for time-invariant county characteristics.

In Table 4.3 we estimate Equation 4.3 separately for each U.S. presidential election available in our sample using a 30-day bandwidth, with data aggregated at the county-period level (before or after Election Day). Our results are noisy due to large standard errors.

Comparing the estimate across years, the standard errors are of a similar magnitude, and the coefficients are close to zero except in the 2016 election.

The results in Column 4 suggest that the number of fatal car crashes in a 30-day window after Election Day increases by a factor of 1.16 for each one percentage point increase in support for the losing party (Democratic party in this case). The point estimate of the Poisson model can be interpreted as a semi-elasticity; a 10 percentage point increase in support for the losing party increases fatal car crashes by approximately 1.56% in the 30 days following Election Day.[11] Taking the average number of fatal car crashes in 2016 (1.113), this suggests that, on average, fatal car crashes increase to 1.130 due to an increased in 10 percentage points in the share of supporters for the losing party.

Our main results show that the 2016 presidential election causes an increase in alcohol consumption in counties with a larger share of supporters for the losing party. Despite studies that show a correlation between alcohol consumption and negative health outcomes, the use of alcohol is also correlated to the management of short-term rises in stress (Darden and Papageorge, 2018). Our results in Table 4.3, however, suggest a clear negative effect of the 2016 elections on public health in counties with a larger share of voters for the losing party: an increase in fatal car crashes.

4.6 Conclusion

In this paper, we measure the impact of supporting a losing presidential candidate on alcohol expenditures. Because individuals care about election outcomes and dislike to lose more than they enjoy winning, we expect that voting for the losing candidate increases anxiety, which is often self-medicated with alcohol. We expect that alcohol expenditures would increase after Election Day in a county with a higher share of votes for the losing candidate.

We use data on total daily purchases of alcohol at the household level in a DID model with a continuous treatment variable. Our results show that the effect of supporting a losing candidate on alcohol expenditure is positive and significant only for the 2016 election. Within 30 days after Election Day, each 10 percentage point increase in support for the

[11] The exact marginal effect is equal to 0.168 $(1 - e^{0.156})$.

losing party increase alcohol expenditure by 1.1%. Our results are robust to using different bandwidths, and for controlling for unobserved factors that change non-linearly over time. The fact that our results are unique to 2016 agrees with the literature that discusses the particular features of the 2016 presidential elections (emotional charge of political campaigns, use of social media, and wrong predictions of who would become the next president, among others).

To understand the policy implications of our results, we perform a second analysis of the effect of supporting a losing candidate on fatal car crashes. We find positive but non-significant effects for all years. In 2016, our point estimate is larger and the evidence that the effect is different from zero is stronger: for each 10 percentage point increase in support for the losing party, fatal car accidents increase by 1.56% in the 30 days following Election Day.

Our findings are relevant for two reasons. First, our results show that risky health behaviors increase after a stress-inducing event. The increase in alcohol consumption is larger within four weeks of Election Day, but it can have long-term effects on health and on human capital, which are not captured in our analysis. Second, some of the components of the 2016 elections are not unique to the United States, and not unique to electoral processes. Our results can be informative about changes in risky behavior in the case of other fear and anxiety-triggering episodes.

4.7 Figures and Tables

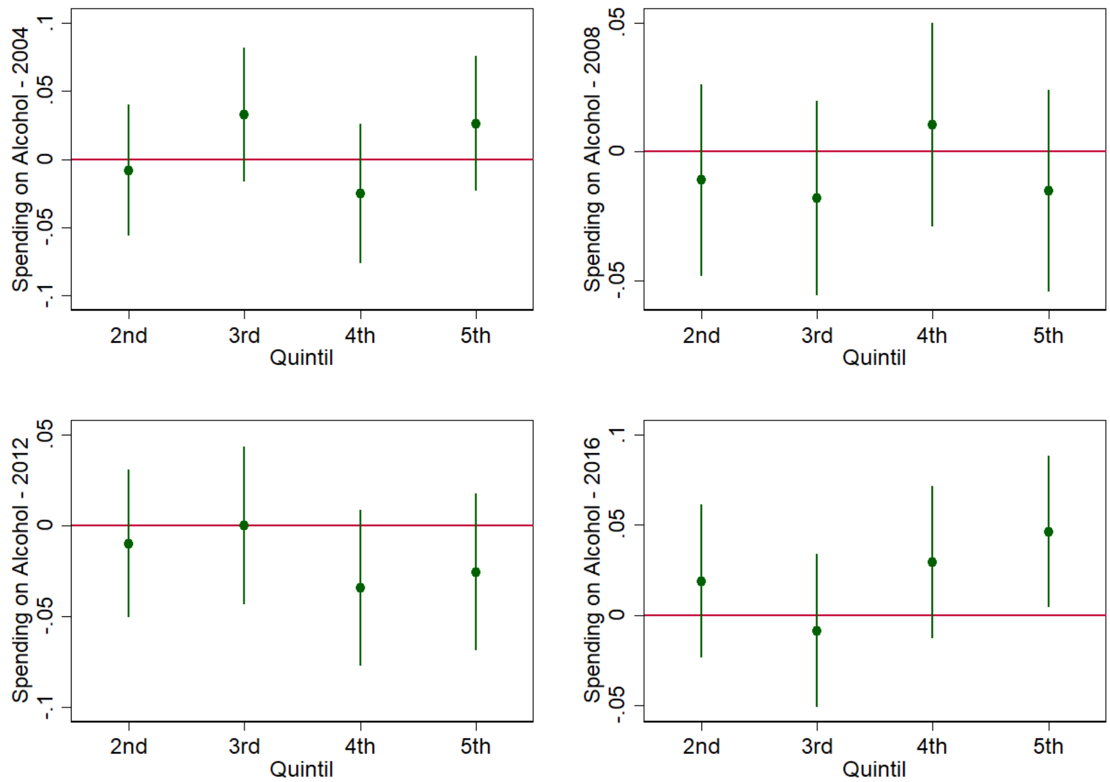

Figure 4.1: Testing how different levels of support for the losing candidate affects alcohol expenditure after Election Day

Note: Each figure shows the result of one regression. Each point shows a coefficient and its 95% confidence interval. The coefficient is the change in the consumption of alcohol post-election in each quintile of support for the losing candidate. The omitted category is the first quintile.

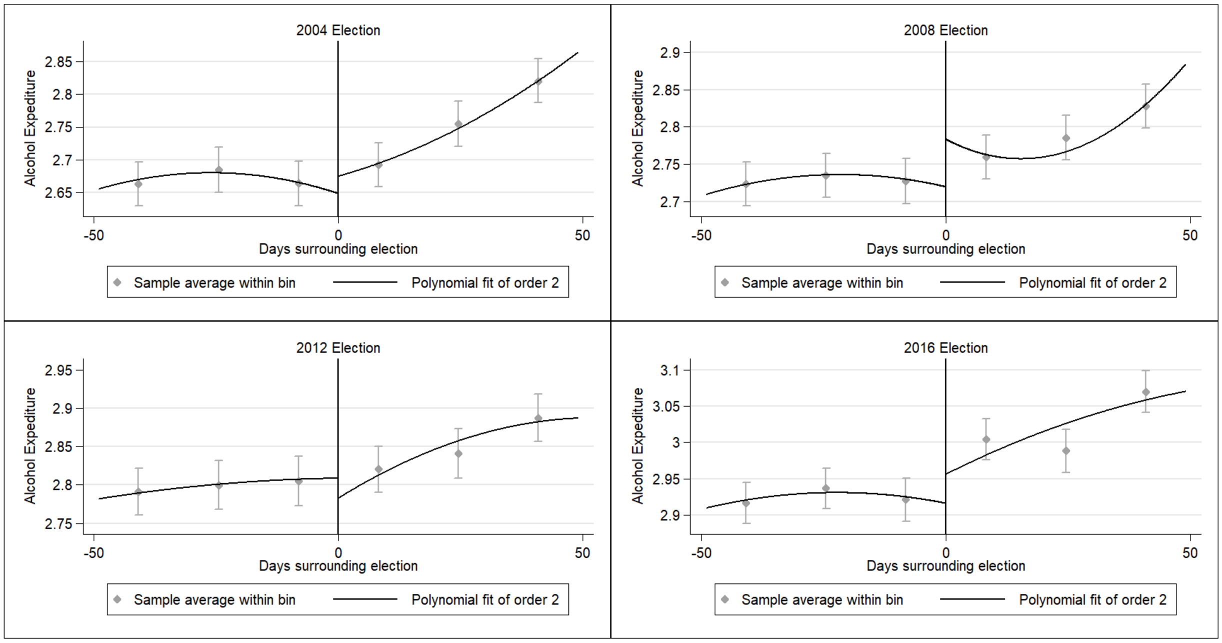

Figure 4.2: Testing discontinuity on alcohol expenditures over time among counties with high levels of support for the losing candidate

Note: This figure shows the regression discontinuity (RD) plot of the average total expenditure on alcohol on the 4th and 5th quintiles of the distribution of the share of votes for the losing party. Positive days are post-election and bins size is approximately 16 days with a 99% confidence interval.

Table 4.1: Testing whether higher levels of support for the losing candidate affect alcohol expenditure after Election Day

VARIABLES	(1) 2004 Election	(2) 2008 Election	(3) 2012 Election	(4) 2016 Election
$\hat{\beta}$	0.032 (0.062)	-0.009 (0.047)	-0.081* (0.048)	0.112*** (0.042)
N	22,457	33,622	32,031	32,758
Number of Counties	1,620	2,068	1,996	2,009

Note: Robust standard errors, clustered at the county level, are reported in parenthesis. All regressions use county, day of the week and month fixed effects. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table 4.2: Testing heterogeneous effects for the 2016 election

VARIABLES	(1) BA degree	(2) Immigrants	(3) Income	(4) Unemployment
Panel A - Below Median				
$\hat{\beta}$	0.068	0.008	0.0837	0.105
	(0.074)	(0.0745)	(0.0617)	(0.066)
N	16,111	16,178	16,238	14,950
Number of Counties	1,452	1,401	1,338	818
Panel B - Above Median				
$\hat{\beta}$	0.164**	0.157***	0.128**	0.118**
	(0.065)	(0.060)	(0.060)	(0.055)
N	16,392	16,580	16,482	16,253
Number of Counties	550	608	670	1,120

Note: Robust standard errors, clustered at the county level, are reported in parenthesis. All regressions use county, day of the week and month fixed effects. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table 4.3: Testing whether higher levels of support for losing candidate affect fatal car crashes after Election Day

VARIABLES	(1) 2004 Election	(2) 2008 Election	(3) 2012 Election	(4) 2016 Election
$\hat{\tau}$	-0.031 (0.186)	0.024 (0.177)	0.046 (0.193)	0.156 (0.142)
N	3,864	3,660	3,544	3,622
Number of Counties	1,932	1,830	1,772	1,811

Note: Robust standard errors, clustered at the county level, are reported in parenthesis. All regressions use county fixed effects. The sample size varies over the years because the Poisson model drops counties with all zero crash fatalities. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

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APPENDIX A

CHAPTER 2 - ADDITIONAL TABLES AND FIGURES

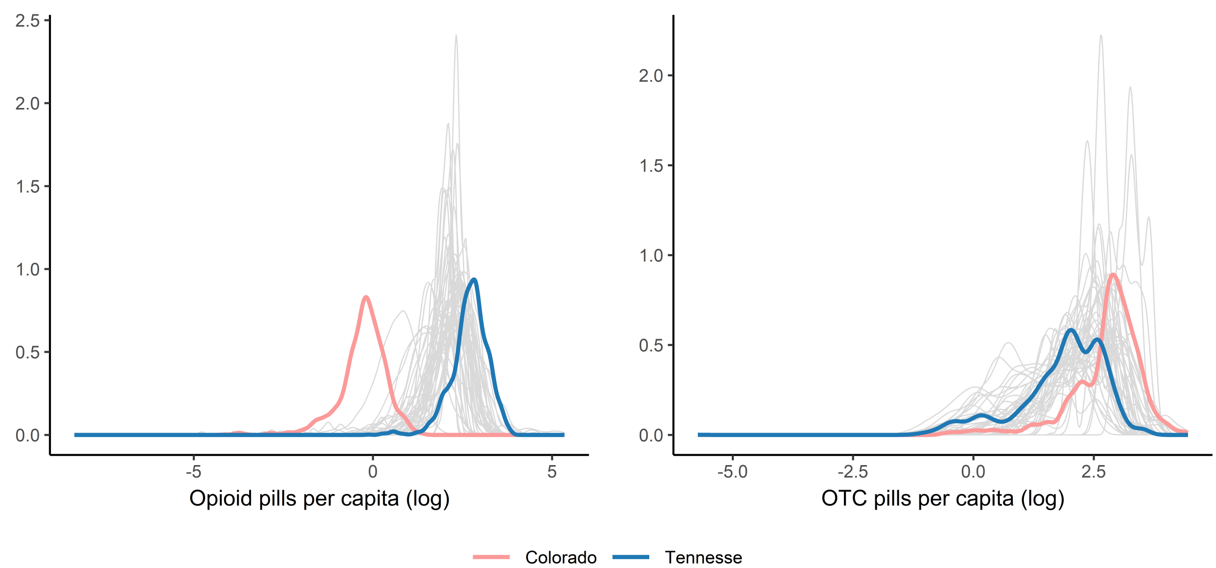

Figure A.1: Density of the Distribution of Pain Medication, by State

Notes: This figure plots the distribution of the logarithm of the per-capita demand for each pain medication by state. The left panel shows the distribution for opiod pills, and the right panel shows the distribution of OTC painkiller pills. For illustrative purposes, the figure highlights two states, with low and high per-capita use of opioids in the left panel. The same states are highlighted in the right panel, showing that in the raw data, there is no clear geographic relationship between the use of opioids and OTC painkillers.

Table A.1: NAICS Sectors and Incidence of Injuries and Worker's Compensation Claims

NAICS	Description	Injury ratio	WC claims ratio
11	Agriculture, Forestry, Fishing and Hunting	**5.69**	4.02
21	Mining		**4.68**
22	Utilities	**5.16**	**4.67**
23	Construction	**5.8**	**5.69**
31-33	Manufacturing	**6.61**	**6.78**
42	Wholesale Trade	**4.28**	**4.7**
44-45	Retail Trade	**4.16**	3.72
48-49	Transportation and Warehousing	**7.13**	4.18
51	Information	1.86	1.98
52	Finance and Insurance	0.84	0.61
53	Real Estate and Rental and Leasing	3.09	**4.63**
54	Professional, Scientific, and Technical Services	1.17	1.63
55	Management of Companies and Enterprises	2.47	2.5
56	Administrative and Support*	2.03	**5.12**
61	Educational Services	1.76	1.68
62	Health Care and Social Assistance	**4.88**	**5.43**
71	Arts, Entertainment, and Recreation	3.68	4.16
72	Accommodation and Food Services	3.03	**4.31**
81	Other Services, Except Public Administration	2.5	3.22
92	Public Administration		

Notes: This table shows the 20 NAICS sectors used in the construction of the shift-share instrument and the industry injury ratios. Column 3 shows the injury incidence ratio per 100 full-time workers based on data from the 2004 SOII; the injury ratio for the mining sector is not reported because it cannot be compared to other sectors. Column 4 shows the WC claims rate per 100 full-time workers using data from 2000–2015 from Ohio. The injuries and WC claims ratio for the public administration sector (92) were not reported in the period of analysis. Values in bold are industries above the median of each ratio, which are called high injury industries in the estimations. *The complete description of sector 56 is "Administrative and Support and Waste Management and Remediation Services."

Table A.2: First-Stage Estimation of the Effect of County-Level Labor Demand Shocks on Employment

	Opioids	OTC painkillers
	Employment (log)	
	(1)	(2)
Labor Demand (log)	0.857***	0.833***
	(0.025)	(0.034)
F-stat (1st stage)	1189.799	609.002
Mean	0.328	0.341
Observations	82,692	95,462

Notes: This table shows the first-stage regression of the main specification, shown in Equation 2.3. Column 1 uses the sample of opioids and column 2 uses the sample of OTC painkillers. The dependent variable in columns 1 and 2 is the log of the employment-to-population ratio by county-quarter. The independent variable is the log of the shift-share instrument for labor demand shocks. All regressions include county fixed effects and year-quarter fixed effects. Standard errors for all regressions use the AKM adjustment for shift-share designs. The mean of the dependent variables is listed in levels.

Table A.3: Effect of County-Level Employment on Demand for Pain Medication: Balanced Sample

Panel A: Balanced sample		
	Opioid pills (log)	OTC pills (log)
	(1)	(2)
Employment (log)	-0.154***	0.253***
	(0.005)	(0.015)
Mean	10.411	9.963
Observations	60,586	60,586

Panel B: Balanced annual sample, two periods		
	Opioid pills (log)	OTC pills (log)
	(1)	(2)
Employment (log)	-0.279***	0.570***
	(0.014)	(0.022)
Mean	10.142	10.526
Observations	4,327	4,327

Notes: This table shows the result of four separate regressions. The dependent variable in column 1 is the log of the quantity of opioid pills per capita sold by county-quarter. The dependent variable in column 2 is the log of the quantity of OTC painkiller pills per capita sold by county-quarter. The independent variable in both panels is the log of the employment-to-population ratio (predicted using the shift-share instrument for labor demand shocks). Panel A reproduces the main result of Table 2.2 but for a balanced sample of counties where data for both opioids and OTC painkillers exist in all periods. The regressions in panel A include county fixed effects and year-quarter fixed effects. Panel B reproduces the analysis using the balanced sample but for data at the annual level and for only the first and last period of analysis where data exist for opioids and OTC painkillers (2006 and 2012). The regressions in panel B include county fixed effects and year fixed effects. Standard errors for all regressions use the AKM adjustment for shift-share designs. The mean of the dependent variables is listed in levels.

Table A.4: Effect of County-Level Employment on Demand for Pain Medication

	Opioid pills (log)	OTC pills (log)
	(1)	(2)
Employment (log)	-0.182***	0.229***
	(0.005)	(0.013)
Mean	915,113.378	1,717,557.717
Observations	82,692	95,462

Notes: This table shows the result of two separate regressions; the difference to the main table is that the variable for employment and the outcome variables are not divided by the population. The dependent variable in column 1 is the log of the quantity of opioid pills sold by county-quarter. The dependent variable in column 2 is the log of the quantity of OTC painkiller pills sold by county-quarter. The independent variable in all regressions is the log of employment (predicted using the shift-share instrument for labor demand shocks). All regressions include county fixed effects and year-quarter fixed effects. Standard errors for all regressions use the AKM adjustment for shift-share designs. The mean of the dependent variables is listed in levels.

Table A.5: Effect of County-Level Employment on Demand for Pain Medication, Controlling for the Ratio of Insured Population

	Opioid pills (log)	OTC pills (log)
	(1)	(2)
Employment (log)	-0.196***	0.137***
	(0.005)	(0.014)
Mean	9.805	10.057
Observations	82,692	95,462

Notes: This table shows the result of two separate regressions. The dependent variable in column 1 is the log of the quantity of opioid pills per capita sold by county-quarter. The dependent variable in column 2 is the log of the quantity of OTC painkiller pills per capita sold by county-quarter. The independent variable in all regressions is the log of the employment-to-population ratio (predicted using the shift-share instrument for labor demand shocks). All regressions control for the ratio of individuals insured by county, extracted from the SAHIE data. The coefficient of this ratio is equal to -0.008 in column 1 and 0.129 in column 2. All regressions include county fixed effects and year-quarter fixed effects. Standard errors for all regressions use the AKM adjustment for shift-share designs. The mean of the dependent variables is listed in levels.

Table A.6: Short-Term Effect of Restrictions on Opioid Prescription on Demand for Pain Medication, Different Treated States

	Opioid pills (log)			OTC pills (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
PDMP	-0.059^{***}	-0.070^{***}	-0.063^{***}	0.046	0.104^{***}	0.072^{*}
	(0.017)	(0.016)	(0.017)	(0.030)	(0.015)	(0.041)
States with PDMP	KY, NM	KY, WV	NM, WV	KY, NM	KY, WV	NM, WV
Mean	9.641	9.641	9.641	14.30	14.30	14.30
Observations	1,288	1,288	1,288	1,288	1,288	1,288

Notes: This table shows three different strategies to measure the the effect of the introduction of PDMPs on the demand for opioids and OTC painkillers, varying the group of treated states. Columns 1 and 4 drop West Virginia (WV) from the analysis, columns 2 and 5, New Mexico (NM), and columns 3 and 6, Kentucky (KY). All regressions control for state and year-quarter fixed effects. Standard errors are clustered at the state level. The mean of the dependent variables is listed in levels.

APPENDIX B

CHAPTER 2 - BACKGROUND ON PAIN MEDICATION

Pain medication—also know as analgesics—exist in three main forms in the Unites States: opioids, non-steroidal anti-inflammatory drugs (NSAIDS), and acetaminophen. Opioids are regulated substances and are sold only with prescription; NSAIDS and acetaminophen are sold over the counter. These drugs reduce the feeling of pain exploring different mechanisms in the body. As a result, they also present different risks from frequent use.

Opioids include opiates—drugs derived from the poppy plant—and synthetic drugs. The first group includes prescription painkillers such as Vicodin, Percocet, and OxyContin, and the second group includes methadone and fentanyl (the latter a synthetic opioid 50 times more potent than heroin). Pain, as touch or taste, is a sensory signal; feeling pain is a signal of harm captured by a nerve receptor and processed in the brain. Our body naturally produces a chemical similar to opioids—endorphins—which block pain by connecting to neuron receptors (called mu opioids that are found in the brain and in other parts of the body) and also block the response to this signal in the brain. Opioids connect to the same receptor, but they are more powerful and capable of blocking more pain.

Opioids can become addictive because mu opioids are also found in an area of the brain responsible for releasing dopamine—a "feel-good" chemical. The connection between endorphin and dopamine is usually connected to physical exercises, such as a "runner's high" from running. Individuals also develop a tolerance for opioids over time, needing a higher dose to achieve the same sensation.

While opioids cause both analgesia and euphoria by affecting the reward system in the human body, NSAIDS and acetaminophen work only in reducing the production of chemicals related to pain. Popular brands of NSAIDS include Advil (ibuprofen), Bayer Extra Strength (aspirin), and Aleve (naproxen sodium). Acetaminophen is the active substance in brands such as Tylenol and Excedrin. NSAIDS work by reducing the production of a chemical that intensifies the feeling of pain (prostaglandins). There is a debate in the medical literature about how acetaminophen works, with some evidence that it works by also blocking the cyclooxygenase (COX) enzyme; although no agreement has been reached, its mechanism of action is closer to NSAIDS than to opioids (Toussaint et al., 2010).

Although opioids became known for its effectiveness in treating chronic pain, there is no agreement in the literature that it is more effective than other methods, especially when weighting the risks of addiction and overdose (Frieden and Houry, 2016). Although heavy use of NSAIDS can damage the stomach, and with acetaminophen, the liver, neither carries the mortality risk of opioids (Solomon et al., 2010).

APPENDIX C

CHAPTER 3 - ADDITIONAL TABLES AND FIGURES

Table C.1: Pregnancy Status Over Time

	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
Not pregnant	0.930	0.922	0.913	0.928	0.947	0.958	0.962	0.936	0.964	0.968
Pregnant	0.046	0.047	0.056	0.044	0.032	0.031	0.027	0.053	0.028	0.024
Live birth	0.008	0.010	0.012	0.008	0.007	0.001	0.002	0.005	0.003	0.001
Uncertain	0.015	0.021	0.019	0.019	0.014	0.009	0.009	0.005	0.005	0.006

Notes: This table shows the distribution of reported pregnancy status by woman-month she was interviewed. The following pregnancy status are omitted from the table because they represented less than 0.10% in a year: stillbirth, abortion, and miscarriage.

Table C.2: Sample Composition

	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
1997	2630	2531	1978	1855	1746	1670	1620	123	1390	1304
1998		565	445	123	70	52	49	12	44	38
1999			260	123	56	40	29	3	19	16
2000				541	209	105	75	7	54	45
2001					600	248	135	13	73	53
2002						711	313	30	121	99
2003							670	34	190	135
2004								45	28	19
2005									678	435
2006										502

Notes: This table shows the composition of the sample over the period of analysis. The total in each column is the total number of women with observed pregnancy status, which corresponds to the first row in Panel B, Table 3.1. Each row corresponds to the year a cohort joins the sample.

Table C.3: Impact of Distance (Log) to Clinic with Pregnancy Tests on Overall Pregnancy Knowledge

	Month Reported Pregancy		
	All women	Prior Pregnancies	No Prior Preg.
	(1)	(2)	(3)
Distance to Pregnancy Tests (log)	0.075	0.208*	0.067
	(0.057)	(0.108)	(0.225)
Distance to Family Planning (log)	-0.082	-0.189	-0.047
	(0.074)	(0.147)	(0.276)
Observations	1,593	636	308
Adjusted R-squared	0.109	0.142	0.098
Mean Dependent Var.	4.591	4.613	4.617

Notes: This table shows the result of three separate regressions. All columns include strata-year fixed effects, number-of-interviews-during-pregnancy fixed effects, and age fixed effects. We have information of prior pregnancies for 944 woman-pregnancy, which affects the sample size in columns 2 and 3. Standard errors are clustered at the neighborhood level.

Table C.4: Impact of Distance to Clinic with Pregnancy Tests on Overall Pregnancy Knowledge not Controlling for Number of Interviews During Pregnancy

	Month Reported Pregancy		
	All women	Prior Pregnancies	No Prior Preg.
	(1)	(2)	(3)
Distance to Pregnancy Tests	0.179	0.315*	-0.175
	(0.122)	(0.188)	(0.295)
Distance to Family Planning	0.072	-0.046	0.549*
	(0.128)	(0.192)	(0.300)
Observations	1,593	636	308
Adjusted R-squared	0.109	0.142	0.098
Mean Dependent Var.	4.591	4.613	4.617

Notes: This table shows the result of three separate regressions. All columns include strata-year fixed effects and age fixed effects. The difference to Table 3.3 is that we do not control for number-of-interviews-during-pregnancy fixed effects. We have information of prior pregnancies for 944 woman-pregnancy, which affects the sample size in columns 2 and 3. Standard errors are clustered at the neighborhood level.

APPENDIX D

CHAPTER 4 - ADDITIONAL TABLES AND FIGURES

Table D.1: Testing different bandwidths

VARIABLES	(1) 2004 Election	(2) 2008 Election	(3) 2012 Election	(4) 2016 Election
Panel A - 20-day bandwidth				
$\hat{\beta}$	0.108	-0.024	-0.112*	0.163***
	(0.078)	(0.059)	(0.062)	(0.057)
N	14,561	21,866	20,787	21,292
Number of Counties	1,512	1,947	1,885	1,922
Panel B - 40-day bandwidth				
$\hat{\beta}$	0.065	-0.055	-0.045	0.098***
	(0.060)	(0.041)	(0.042)	(0.037)
N	29,350	43,977	41,897	42,778
Number of Counties	1,677	2,131	2,075	2,072

Notes: Robust standard errors, clustered at the county level, are reported in parenthesis. All regressions use county, day of the week and month fixed effects. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table D.2: Testing an RDD model to control for non-linearity and closeness to the elections day

VARIABLES	(1) 2004 Election	(2) 2008 Election	(3) 2012 Election	(4) 2016 Election
Panel A - 20 days bandwidth				
$\hat{\beta}$	0.114 (0.077)	-0.026 (0.063)	-0.111* (0.062)	0.163*** (0.056)
N	14,561	21,866	20,787	21,292
Number of Counties	1,512	1,947	1,885	1,922
Panel B - 30 days bandwidth				
$\hat{\beta}$	0.059 (0.063)	-0.028 (0.051)	-0.087* (0.051)	0.118*** (0.045)
N	21,801	32,632	31,038	31,771
Number of Counties	1,614 2,061	1,990	2,003	
Panel C - 40 days bandwidth				
$\hat{\beta}$	0.065 (0.054)	-0.056 (0.044)	-0.046 (0.044)	0.098** (0.039)
N	29,350	43,977	41,897	42,778
Number of Counties	1,677	2,131	2,075	2,072

Notes: Robust standard errors, clustered at the county level, are reported in parenthesis. All regressions use county, day of the week and month fixed effects. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.