ESSAYS IN REGIONAL ECONOMICS: NUTRITION ASSISTANCE, PHARMACEUTICAL MARKETING, AND TRIBAL BUSINESS ACTIVITY

By

Thomas Carl Keene

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

Agricultural, Food, and Resource Economics—Doctor of Philosophy
Economics—Dual Major

2024

ABSTRACT

Economic outcomes vary tremendously throughout the United States. This variation is due to an amalgamation of processes, ranging from spatial access barriers to differences in institutions across jurisdictions. This dissertation explores three different aspects of these spatial variations. Specifically, I explore how economic outcomes across the United States are affected by social safety net policies, pharmaceutical marketing, and the expression of American Indian sovereignty through self-determination policies.

The first essay is titled "The Impact of Food Assistance Work Requirements on Labor Market Outcomes." The Supplemental Nutrition Assistance Program (SNAP), formerly named the Food Stamp Program, has long been an integral part of the US social safety net. During US welfare reforms in the mid-1990s, SNAP eligibility became more restrictive with legislation citing a need to improve self-sufficiency of participating households. As a result, legislatures created two of these eligibility requirements: the General Work Requirement (GWR), which forces an adult to work to receive benefits, and the Able-Bodied Adult Without Dependents (ABAWD) work requirement, which requires certain adults to work a certain number of hours to receive benefits. Using restricted-access SNAP microdata from nine states, I exploit age cutoffs of the ABAWD work requirement and General Work Requirement (GWR) to estimate the effect of these policies on labor outcomes. I find that at the ABAWD age cutoff, there is no statistically significant evidence of a discontinuity across static and dynamic employment outcomes. At the GWR age cutoff, unemployed SNAP users and SNAP-eligible adults are on average more likely to leave the labor force than to continue to search for work.

The second essay is titled "Open Payments and Opioid Overdose Mortalities in the United States." The United States registered over 190 opioid overdose deaths per day in 2020. Many factors contribute to the epidemic, but a pronounced feature is overprescribing of opioid analgesics. I explore the legal marketing of medical technologies via provider payments and its potential effect on mortality rates. Using the Open Payments database from the Center for Medicare and Medicaid Services and cause of death data from the CDC, I examine the relationship between population-

weighted opioid-associated payments and overdose deaths at the county level for the contiguous US. The presence and magnitude of payments varies across counties and regions. Results indicate that opioid producers predominantly advertise through food and entertainment related transfers with median values of around $17. Additionally, I found no statistically significant evidence of "detailing" behavior. Regression results suggest that the number of physicians receiving a payment in a county at an earlier period has a stronger association to opioid-mortalities than the aggregate value of the transfers in the county.

The third essay is titled "Business Activity in Tribal Areas During the Self-Determination and Nation-to-Nation Eras." Despite development efforts, tribal nations inside the borders of the US exhibit higher poverty rates and associated social concerns than other areas of the US. Business development can be a key way to sustainably reduce poverty. This article characterizes differences between county-level economies with and without tribal lands using several measures of business activity. Consistent with county-level business size distribution plots, results from regression analyses show that tribal areas on average have more firms and establishments compared to nontribal US counties over the entire analysis period. In terms of industrial diversity, employment is becoming more concentrated in certain industries over time relative to nontribal areas. Estimates from counterfactual kernel densities show that estimating conditional means may not accurately reflect the changing distributions of tribal business activity. The chapter concludes that stronger support systems for existing enterprises could foster future economic growth in tribal counties.

To my parents, David and Erin.
Thank you for always believing in me.

ACKNOWLEDGEMENTS

I am fortunate to be surrounded by a community of researchers, support staff, friends, and family that has supported me throughout my graduate school career. Each person's contribution, whether it was through rigorous peer review or a kind word in passing, helped me survive and thrive these past five years. African wisdom provides us with a proverb: "It takes a village to raise a child." I cannot help but feel this proverb rings true for graduate school: it takes a village to raise an early career researcher.

First, I would like to thank my co-advisors Scott Loveridge and Craig Carpenter. Scott has been a stalwart mentor and advisor throughout my time at Michigan State University. His constant support and interest in my success allowed me to flourish in this program despite the many obstacles and struggles that littered my path. I appreciate all that he has done for me, such as introducing me to Regional Science and delivering Thanksgiving dinner to me during the COVID-19 lockdowns. While Craig accepted the role of co-advisor later on in my program, he has also been quite instrumental in my growth. I thank him for introducing me to the Federal Statistical Research Data Centers and the Census Bureau community in general; including me on multiple projects; and guiding me through panicked calls and texts from the basement of the Institute for Social Research at the University of Michigan.

I am also grateful for the other members of my committee. I am indebted to Liz Mack for showing me the ropes on all aspects of conducting interdisciplinary research under a federally awarded grant. Thank you, Mark Skidmore and Ajin Lee, for your persistent support in my research. I am also grateful to John Mann for his support not only in research, but in teaching. Your calls in the midst of the campus shooting and its aftermath were tremendous — they helped me through one of the toughest times in my life.

I would like to also thank my support system tangential to my research. Thank you especially to my parents, David and Erin, and my siblings, James and Shannon, for their constant support. Thank you also to my lifelong friends back in Chicago for the much needed visits, hangs, weekend trips, and beach days at Rocky Gap Park. I am so fortunate to have such meaningful friendships that span decades so early on in my life.

I would also like to thank the new friends I have met at my time here at Michigan State University. A special thank you to Rania for her unwavering friendship since our first year here. Her presence during the lonelier parts of the COVID-19 lockdowns and the end of the PhD journey kept me motivated and sane. It has also been an honor to get to know her mother, Rajae, who taught me how to make Moroccan tagine and always made sure I was fed when she visited. I'm also grateful for other graduate students, such as Jose and Fernanda, for their camaraderie.

Thank you also to the support staff who helped me along my PhD journey. Thank you to Nicky Mason-Wardell, Jamie Bloom, and Ashleigh Booth for helping me through all the programmatic aspects of graduate school. Thank you to Brent Ross for guiding me through undergraduate instruction in AFRE. A deep appreciation to J. Clint Carter at the Michigan RDC for his indispensable help through the Census Bureau disclosure process. His alertness to my needs ensured that I had a job market paper!

Finally, I'd like to express gratitude to the organizations that funded this dissertation research. The first chapter is supported by funding from the United States Department of Agriculture (USDA) Economic Research Service (ERS) and Robert Wood Johnson Foundation (RJWF). The second chapter is supported through USDA Hatch project 1014691. The third chapter is funded through USDA National Institute of Food and Agriculture AFRI competitive grant 2020-67023-30958 and USDA Hatch project 1014691. Thank you for the generous support.

TABLE OF CONTENTS

LIST OF ABBREVIATIONS

ABAWD Able-Bodied Adult Without Dependents

ACS American Community Survey

AIAN American Indian and Alaska Native

APE Average Partial Effect

BDS Business Dynamics Statistics

BIA Bureau of Indian Affairs

BLS Bureau of Labor Statistics

CBP County Business Patterns

CDC Centers for Disease Control and Prevention

CDFI Community Development Financial Institutions

CMS Centers for Medicare and Medicaid Services

E&T Employment and Training

ERS Economic Research Service

FNS Food and Nutrition Service

FPL Federal Poverty Line

FSP Food Stamps Program

FSRDC Federal Statistical Research Data Center

GWR General Work Requirement

HHI Herfindahl-Hirschman Index

IGRA Indian Gaming Regulation Act of 1988

IRA Indian Reorganization Act of 1934

ITT Intention-to-treat effect

LATE Local Average Treatment Effect

LAU Local Area Unemployment

LSA	Labor Surplus Area
NAICS	North American Industry Classification System
NCAI	National Congress of American Indians
NCES	National Center for Education Statistics
NDC	National Drug Code
NE	Nonemployer
NES	Nonemployer Statistics
NSLP	National School Lunch Program
OLS	Ordinary Least Squares
POLS	Pooled Ordinary Least Squares
PSE	Policy, Systems, and Environmental Change
R&D	Research and Development
RDD	Regression Discontinuity Design
RUCC	Rural-Urban Continuum Codes
RWJF	Robert Wood Johnson Foundation
SNAP	Supplemental Nutrition Assistance Program
SNAP-Ed	Supplemental Nutrition Assistance Program – Education
TWFE	Two-Way Fixed Effects
USDA	United States Department of Agriculture
WIC	Special Supplemental Nutrition Program for Women, Infants, and Children

CHAPTER 1

THE IMPACT OF FOOD ASSISTANCE WORK REQUIREMENTS ON LABOR MARKET OUTCOMES

1.1 Introduction

Work requirements[1] for welfare programs are a controversial policy measure. Some policymakers argue that work requirements restrict government spending and caseload resources to those who "deserve" it by displaying self-sufficiency. Others assert that all that need assistance are deserving of it, and that the strict requirements keep those who do not qualify in poor conditions. Even others argue that the requirements are useless at improving labor market participation, adding a nonbinding constraint to consumers that is costly both to them and to program implementation. As a result of these clashing beliefs, welfare programs are constantly reformed, eliminated, or kept intact through the practice of logrolling.

One of the welfare programs in the United States that has survived an onslaught of said policy changes is the Supplemental Nutrition Assistance Program (SNAP), formerly known as the Food Stamp Program (FSP). SNAP is a federally funded program aimed at relieving food insecurity and promoting healthy diets for low-income individuals. Since the passing of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, eligible individuals are subject to two work requirements: a general work requirement (GWR) for all participants age 16-59, and a more restrictive set of requirements for able-bodied adults without dependents (ABAWD) age 18-49 (Wheaton et al., 2021). Despite limited evidence on work requirement efficacy (Gray et al., 2023), efforts to strengthen these requirements continue to gain traction in the US legislative environment: Congress extended the ABAWD requirements to 52 years old in 2023 (Qiu, 2023). These changes highlight the importance of evaluating the effect of SNAP work requirements.

[1]**Acknowledgments**: Any views expressed are those of the author and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961) All results have been reviewed to ensure that no confidential information is disclosed. I wish to thank the Robert Wood Johnson Foundation and the U.S. Department of Agriculture's Economic Research Service for funding support. All errors that remain are my own.

In this essay, I evaluate whether the ABAWD requirements, as well as SNAP's General Work Requirement (GWR), lead to changes in labor market outcomes for SNAP individuals. While static economic theory suggests that these requirements may reduce caseloads and keep a larger share of SNAP recipients employed, I also test whether SNAP benefits affect more dynamic labor market outcomes, such as income and length of job search while unemployed. I use regression discontinuity designs on microdata from nine US states to capture these effects.

Whether SNAP benefits incite agents to reduce their allocation of labor, as static economic theory suggests, is already an active area of study. Using the staggered introduction of FSP across counties in the 1960s and 1970s, Hoynes and Schanzenbach (2012) found households reduced employment and work hours once they received access. East (2018) finds similar evidence in a more contemporary setting among immigrants who were phased out of FSP due to the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 and were once again reinstated. There is also research activity using the ABAWD work requirement in particular. Some studies find that work requirements do improve probability of employment (Harris, 2021; Cuffey et al., 2022), while others do not find empirical evidence that supports it (Gray et al., 2023; Stacy et al., 2018; Han, 2020). While much of this work has indirectly measured SNAP use and associated outcomes, most recently, Gray et al. (2023) use microdata from Virginia and find no statistical evidence that the requirements affect employment, but find suggestive evidence that it improves income. Much of the variation in the results between studies likely arise from differences in methodological choices, ranging from using different sources of exogenous variation to different strategies of sample construction.

I contribute to this line of research in four separate ways. First, I attempt to replicate and provide external validity of previous work requirement studies by using microdata from nine states. Studies that test outcomes related to the ABAWD work requirement needed to make tradeoffs between the quality of the data used and the external validity of the results. Several studies use public-use cross-sectional data on the entire United States to determine who receives SNAP and their subsequent outcomes, limiting data precision and the internal validity of their results (Harris, 2021; Han, 2020;

Ritter, 2018). Others are able to use administrative or restricted-use data for their analysis, but only for a small subset of states (ranging from one to nine), acknowledging that some of the differences in results may arise from across-state heterogeneity in the impacts of work requirements (Gray et al., 2023; Stacy et al., 2018, Wheaton et al., 2021). To my knowledge, this study is the first of its kind to use administrative panel data for nine US states in this extended analysis period. Additionally, the subset of states included in this analysis is disjoint from the subsets used in other studies using administrative data. In particular, this analysis includes states with large rural populations which often use SNAP benefits differently than their urban counterparts (Harnack et al., 2019). Therefore, this study extends the external validity of previous studies and finds potential heterogeneity in the effect of the work requirements across different US regions.

My second contribution is using multiple cutoffs, which extends the inference to different age groups. The first cutoff I use is the age cutoff at 50, the one most used in the literature. I also test the GWR cutoff at age 60, which has not been tested in prior works. These cutoffs guarantee strict compliance, allowing for a sharp regression discontinuity design (RDD) estimation strategy. However, inferences that can be made on this effect is limited: depending on the bandwidth in which ABAWDs are as-good-as random, the population that many studies have focused on around the ABAWD cutoff could be quite small.

The third contribution of this essay is an ability to test dynamic or long-term labor market effects of the work requirements. Static economic theory predicts that without work requirements, SNAP participants near the cutoff will choose optimal allocations of labor, characterized by working less hours, choosing unemployment, or exiting the labor market. However, this approach fails to acknowledge that unemployment is an active role in the labor market if it is coupled with a job search (Mortensen, 1986). Those who face less liquidity constraints during their search could search longer with a higher reservation wage. This may lead to better labor market outcomes for those who are not subject to work requirements, providing evidence antithetical to static theory.

Finally, my fourth contribution is estimating heterogeneous effects of the work requirements across gender. Men and women in the United States face different constraints, leading to different labor market decisions throughout their lives. This is paired with a gender wage gap, where women earn less than men (Goldin, 2014). In the context of SNAP, East (2018) finds that access to SNAP reduces labor market participation among women. A study about the ABAWD work requirement find that women are more likely to adhere to work requirements than men (Wheaton et al., 2021), and another finds no heterogeneous effects across gender, albeit suffering from data quality issues (Ritter, 2018). This study is the first to test the heterogeneity across both the ABAWD and GWR work requirements. I find that the GWR is especially binding to women, leading to a statistically significant labor market exit once they are no longer subject to the requirement.

This essay proceeds as follows: Section 1.2.1 provides background on SNAP and the current state of the economic literature on its effectiveness in improving the outcomes of its recipients. Section 1.2.2 provides background on the work requirements in SNAP and the literature around the work requirements. Section 1.3 provides the testable hypotheses generated from both static and dynamic optimization models. Section 1.4 presents the sharp regression discontinuity design I use to estimate the causal effect. Section 1.5 describes the longitudinal state and federal administrative data used in my analysis and offers evidence that supports the validity of the empirical strategy. Section 1.6 presents results both on the effect of the requirement on the whole sample and for subsamples divided by gender. Section 1.7 concludes and offers policy implications unique to each work requirement.

1.2 Background

1.2.1 Labor Market and Health Effects of SNAP

In its current form, SNAP has two primary goals: to reduce food insecurity and improve the diets of low-income individuals. Through the program, households that reach the eligibility conditions obtain financial support to purchase food including most staples (USDA, n.d.). In 2021, more than 41 million low-income people received support using the SNAP, two thirds of whom belong to households with children (Policy Basics, 2021). With expenditures of almost $111 billion in 2021 (Policy Basics, 2021), SNAP is the third largest welfare program in the US and is an integral branch of the federal welfare safety net (Nestle, 2019).

Because of the large size and costs of the program, many studies have attempted to estimate the effect of the program on its primary goals. Due to estimation issues such as selection bias (Mabli et al., 2013) and data quality (Caputo and Just, 2022), empirical results vary on whether SNAP contributes to food insecurity alleviation and improvements in diet quality for its participants (Schanzenbach, 2019). However, several quasi-experimental studies have found statistical evidence that supports the claim that SNAP lowers the incidence of food insecurity for low-income families. By leveraging policy changes that excluded immigrants from receiving FSP benefits in 1996, Borjas (2004) found that cutting FSP benefits increases food insecurity. Specifically, results indicate that on average, a 10 percentage point decrease in the number of people that received food stamps results in a 5 percentage point increase in the fraction of food-insecure households. In a more recent study, Mabli et al. (2013) compare new entrants cross sectionally (against SNAP households already six months into the program) and longitudinally (against the new entrants after six months) to find that SNAP reduces instance of food insecurity by around 8% in the short run. In terms of improving diet quality, studies show that the effects are small or not statistically significant (Fan, 2010; Gregory et al., 2013).

While evidence on SNAP's ability to adequately address its stated goals remains contentious, there is evidence that it does affect the economic behavior of its participants. In terms of intertemporal household consumption, access to food stamps is an effective insurance against permanent income shocks (Blundell and Pistaferri, 2003). However, estimates of the effects of SNAP benefits on budget shares for low-income individuals change depending on the timing of policy changes. During the early stages of FSP adoption in the 1960s and 1970s, researchers found that the benefits reduced out-of-pocket food spending, increased food spending, and the marginal propensity to consume out of cash income and food stamp income were similar (Hoynes and Schanzenbach, 2009). Studies using data during and after the Great Recession find that program recipients are much more likely to consume out of their SNAP income relative to their other incomes, challenging classical economic theory (Beatty and Tuttle, 2015; Hastings and Shapiro, 2018). Evidence on the relationship between SNAP benefits and labor market outcomes seem more robust to changes in time and more consistent with classical economic theory. At both the introduction of the FSP (Hoynes and Schanzenbach, 2012) and the policy change in 1996 that disproportionally affected immigrants (East, 2018), access to SNAP benefits reduced hours of work and employment rates, especially for single women.

As food insecurity is associated with poor health (Gundersen and Ziliak, 2015), some studies have attempted to estimate the role of SNAP on health outcomes. Economic stability during childhood is an important determinate in childhood health and adult health (Case et al., 2002) along with educational outcomes and ability (Case and Paxson, 2008). Therefore, safety net participation for low-income families with children may improve future health and labor outcomes. Empirical evidence supports these notions in relation to SNAP. One additional year of FSP eligibility for immigrants who lost access to the federal program in 1996 improved the health of children under five well into their teenage years (East, 2020). Using the exogenous variation at the beginning of the policy implementation in the 1960s, researchers have also found that access to food stamps as a child reduced the chance of suffering from metabolic syndrome as an adult (Hoynes et al., 2016) and increased the accumulation of human capital (Bailey et al., 2023). Additionally, FSP access improved fertility (Currie and Moretti, 2008), and reduced neonatal mortality (Almond et al., 2011), which has been suggested as paramount to human development through the fetal origin hypothesis. The estimated effects of SNAP benefits on health outcomes are overwhelmingly positive, providing evidence that SNAP reduces food insecurity and improved the lives of low-income individuals in a substantial way.

1.2.2 SNAP Work Requirements

Participants in SNAP are subject to three work-related policies: the general work requirement (GWR), the ABAWD work requirement (and time limit), and SNAP Employment and Training Programs (SNAP E&T). All three of these policies are interrelated, and a SNAP participant may be subject to a combination of them at any given time. While the primary goal of this section is to provide a brief overview of these policies and how they interact with one another, I conclude it with a discussion of possible employment actions a SNAP participant may make under this patchwork of policies while still maintaining SNAP benefits. This discussion will inform the economic theory in the next section.

Of the two work requirements, the GWR affects the larger population of SNAP participants. To adhere to the GWR, a SNAP participant must (1) register for work; (2) participate in SNAP E&T if assigned; or (3) accept a job offer. Participants are exempt from this requirement for reasons that may affect their ability to work, including whether they are living with a disability, enrolled in school half-time or more, living with a child under six years of age, or in a drug or alcohol rehabilitation program. Participants are also exempt from the GWR for reasons related to their current employment and income. In particular, participants who would otherwise be subject to the GWR are exempt if they work at least 30 hours a week (or earn the equivalent of thirty hours times the current federal minimum wage); are currently receiving unemployment compensation; or already complying with work requirements for Social Security Insurance. SNAP applicants that do not meet the GWR lose SNAP benefits immediately for at least a month. The GWR applies to unexempt individuals ages 16-59.

A strict subset of those subject to the GWR are also subject to the ABAWD requirement. Specifically, the ABAWD requirement affects SNAP participants aged 18-49 who are not otherwise waived from the GWR, do not live with a child under 18, are not pregnant, and are not otherwise unfit for unemployment due to disability. Those subject to the requirement are called ABAWDs. ABAWDs face more stringent obligations and penalties under the requirement. Specifically, an ABAWD must meet the GWR and work or attend work programs (which include SNAP E&Ts) at least 80 hours a month to receive SNAP benefits. If an ABAWD does not fulfill this requirement, they only receive SNAP benefits for three months in a 36-month period or until they meet the requirement again.

Because ABAWDs are subject to stricter work requirements, there are avenues through which states may waive the work requirement or exempt ABAWDs. States can administer monthly discretionary exemptions to ABAWDs who are not meeting the work requirements, meaning they can receive more than three months of SNAP benefits in a 36-month period. Additionally, states can choose to waive substate areas based on local area unemployment or other conditions that may limit an ABAWD's ability to remain employed. These substates areas must have (1) unemployment levels over 10%; (2) an unemployment rate 20% above the national unemployment rate in a given year; or (3) be deemed to have poor economic conditions through some other measures, such as employment to population ratios.

The work requirements leave little scope for voluntary unemployment decisions for a SNAP participant. While those subject only to the GWR can work up to 30 hours a week, they cannot reduce their hours or work effort without good cause and still receive SNAP benefits. Any ABAWD must work between 20 and 30 hours a week on average, but because they are subject to the GWR, they cannot voluntarily reduce their hours if they become exempt from the time limit due to age, geography, etc.

As mentioned in Section 1.1, the current research on work requirements focuses solely on the ABAWD work requirement. At the time of writing, I am aware of eight total studies on the topic. Studies use a variety of different methodological choices to estimate the effect. Four studies use the staggered reestablishment of the ABAWD work requirement following the Great Recession to identify causal effects (Han, 2020; Harris, 2021; Wheaton et al., 2021; Hall, 2022), while the others rely on the age cutoff at 50 (Ritter, 2018; Stacy et al., 2018; Cuffey et al., 2022; Gray et al., 2023). Four studies use public-use microdata to estimate the effect for the entire United States (Ritter, 2018; Han, 2020; Cuffey et al., 2022; Harris, 2021), while the others use administrative data from a select number of states. Specifically, Gray et al. (2023) uses data from Virginia; Hall (2022) uses data from Maryland; Stacy et al. (2018) uses data from Florida, Illinois, Indiana, Maryland, Michigan, New Jersey, New York, Tennessee, and Virginia; and Wheaton et al. (2021) uses data from Alabama, Colorado, Maryland, Minnesota, Missouri, Oregon, Pennsylvania, Tennessee, and Vermont. As states implement SNAP and the work requirements differently (USDA Program Development Division, 2018), there can exist differences in the effects of the work requirements.

However, these studies yield similar results. With the exception of two (Cuffey et al., 2022; Ritter, 2018), most studies find a reduction in caseloads due to the ABAWD work requirement.

Similarly, most studies find no effect of the requirements on labor market outcomes, again with two as exceptions (Cuffey et al., 2022; Harris, 2021). However, these studies fail to acknowledge the intermingling of the ABAWD work requirement with the GWR, as well as potential heterogeneity due to age. I attempt to fill these gaps.

1.3 Conceptual Model

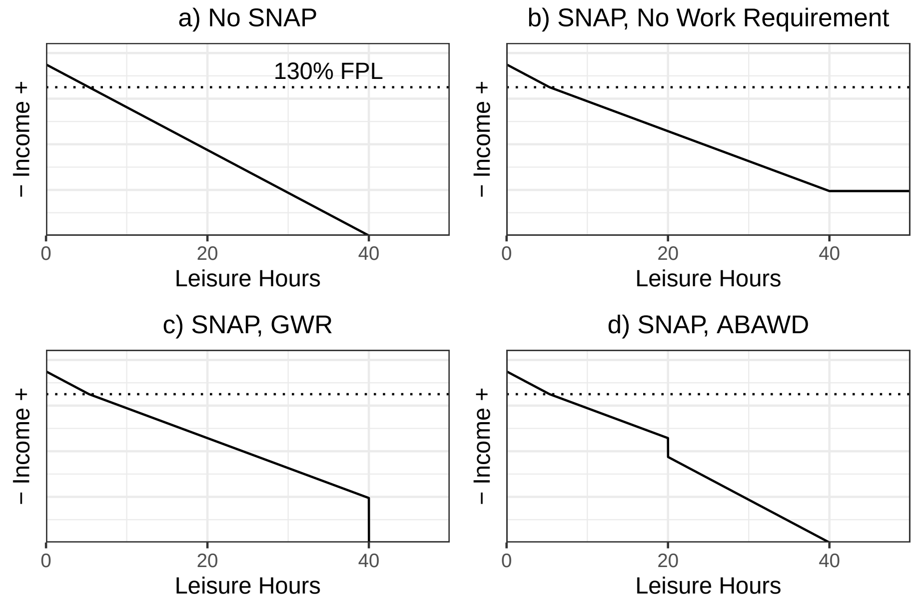
In this section, I present two different economic models that provide testable hypotheses for the behavior of SNAP-eligible adults subject to the work requirements. The first is a static optimization model, which is the more prevalent model in the literature. I then introduce results from dynamic optimization models, which offer more nuanced predictions in terms of job search and unemployment spells. Due to the different structures of the work requirements, SNAP-eligible adults face different constraints in their actions. As a result, these models will yield different predictions for each requirement. The section closes with features not captured in the models that may affect the empirical results.

1.3.1 Static Hypotheses

In much of the preceding literature on the effect of welfare programs on labor market outcomes, an agent's decision-making is modelled using a stylized static budget constraint (Bitler et al., 2006; Gray et al., 2023). Agents maximize their utility by choosing allocations of two goods: income and leisure hours. Agents who do not participate in SNAP face an "unkinked" budget constraint, where the tradeoff between the two goods is the agent's hourly wage. This is displayed in Panel A of Figure 1.1. Agents that can take up SNAP face a kinked budget constraint: they begin receiving SNAP benefits at a gross income equal to 130% of the federal poverty line (FPL). Additionally, SNAP benefits face a benefit-reduction rate (or implicit tax rate) on net income of 0.3. As a result, an agent who receives SNAP has a discounted hourly wage tradeoff of 0.7, and their income flattens to the maximum SNAP benefit as their labor hours reach zero. Panel B of Figure 1.1 shows this budget constraint.

However, agents subject to the work requirements face additional restrictions on their budget. As Section 1.2.2 explains, those subject to the GWR cannot receive SNAP benefits if they are not

Figure 1.1 Stylized Static Budget Constraints for SNAP Participants



Note: Adapted from Gray et al. (2023) and Bitler et al. (2006). Income eligibility for SNAP is 130% the federal poverty line (FPL).

actively working, searching for work, or participating in a work program. As a result, there is a discontinuous jump at an allocation of full leisure hours compared to an agent not subject to the work requirement (see Panel C). Agents whose optimal allocation falls at the discontinuous jump (that is, their optimal income-leisure bundle is collecting the maximum SNAP benefit as income and not participating in the labor market) must choose a second-best allocation. Depending on their marginal rate of substitution between the two goods, an agent bound by the GWR picks from two actions: (1) Move to a lower allocation of leisure to retain benefits; or (2) opt to maintain the leisure allocation and drop out of SNAP.

The ABAWD work requirement has an analogous explanation (see Panel D). As ABAWDs are also subject to the GWR, they face the same constraint when choosing to engage in the labor market. However, an ABAWD must also satisfy the hourly work requirement to receive SNAP benefits. If the work requirement is binding, an ABAWD must choose between a second-best allocation of income and leisure. Compared to agents only subject to the GWR, they may choose an allocation where they are still working to some extent. In summary, the ABAWD must either (1) retain SNAP benefits by choosing to work above the labor hours threshold or (2) drop out of SNAP and choose a leisure allocation under the work requirement.

Empirically, one should see two different effects of the work requirements. If SNAP-eligible adults subject to a requirement are less likely to participate in SNAP, this suggests that the requirement is binding, and those along the cutoff prefer to forgo the benefit than to work more. Conversely, if those subject to a requirement engage in the labor market more intensely, either through working (GWR) or working more hours (ABAWD), there is evidence to suggest that the work requirement incentivizes work among those bound by it.

1.3.2 Dynamic Hypotheses

While the static model is a powerful tool to predict economic behavior, it fails to account for conditions present in actual labor markets. The most glaring absence is that of search frictions: an agent's job search is riddled with joblessness spells. As both the GWR and ABAWD work requirement can limit SNAP benefits for those experiencing spells, they can also affect more dynamic labor market outcomes. Dynamic job search is an active area of study, with a variety of economic models highlighting different aspects of search (Mortensen, 1986; Mortensen and Pissarides, 1999; Maibom et al., 2023).

There are several results derived from these models that are pertinent to SNAP's work requirements. The first is the presence of an aging effect among those nearing retirement. As optimal control results change between finite and infinite horizon abstractions, so should the behaviors of rational agents searching for work. Therefore, there may be a difference in the behavior of SNAP eligible adults based on their age. Specifically, as people near retirement, their return to search falls, encouraging them to quit their search early (Mortensen, 1986).[2] Because the GWR and ABAWD age cutoffs occur at different ages, heterogeneity in results could be driven by age rather than the structures of the requirements.

The other result deals more closely with the work requirements. During a job search, unemployed workers face liquidity constraints. Those with more funds benefit from an increase in the

[2] It is important to note that previous studies suggest that this finite horizon result only exists for people very close to retirement as joblessness spells are often short (Mortensen, 1986)

value of leisure which in turn increases their reservation wage. Additionally, if liquidity is finite and decreases as search tenure increases, those with more liquidity can search for longer at reservation wages well above their value of leisure (Mortensen, 1986).

SNAP benefits can act as liquidity for an unemployed worker not subject to work requirements. As a result, they may accept higher wage offers and endure longer joblessness spells compared to those under the work requirements. ABAWDs that cannot maintain their hourly requirement only retain their benefits for three months, limiting their joblessness spells tremendously. SNAP users subject to the GWR face a similar issue but are not monitored as closely as ABAWDs. If these hypotheses are correct, work requirements may prevent inframarginal workers from accepting jobs with higher wages.

1.3.3 Other Considerations

Individuals who are designated as ABAWDs may face other more binding constraints from their employers in regards to leisure time. Employers may offer work hours discretely. For example, an employer may be looking for a worker that can provide exactly 20 hours of work. As a result, the agents described in model presented in Figure 1.1 may already be at their second-best allocations and the work requirement is not a binding constraint. This could lead to null results.

1.4 Empirical Strategy and Identification

To estimate the effect of the work requirements on static and dynamic labor market outcomes, I use sharp RDDs at different age cutoffs created by SNAP policy. As mentioned in Section 1.2, SNAP users are subject to the GWR if they are between the ages of 16 and 59. Analogously, SNAP users subject to the GWR are also subject to the ABAWD work requirement if they are between the ages of 18 and 49. After removing potentially waived individuals from the sample, I estimate the effect of the work requirements, τ_{GWR} and τ_{ABAWD}, using the upper bound cutoffs. Expressed in terms of potential outcomes, I estimate

$$\tau_{GWR} = \mathbb{E}[Y_i(1) - Y_i(0) | Age_i = 60] \tag{1.1}$$

for the GWR and

$$\tau_{ABAWD} = \mathbb{E}[Y_i(1) - Y_i(0)|Age_i = 50] \tag{1.2}$$

for the ABAWD work requirement. Y_i is a labor outcome for individual i, and the number in parenthesis denotes whether (=1) or not (=0) the person is subject to the work requirement. I estimate these local average treatment effects (LATE) using the following:

$$\tau = lim_{x \downarrow c}\, \mathbb{E}[Y_i|X_i = x] - lim_{x \uparrow c}\, \mathbb{E}[Y_i|X_i = x] \tag{1.3}$$

Additionally, I use local linear point estimation using Mean Squared Error (MSE) optimal bandwidths to estimate this effect. I also use heteroskedasticity-robust nearest neighbor standard errors for valid inference (Calonico et al., 2014; Cattaneo and Titiunik, 2022).

There are benefits to the sharp RDD approach using age cutoffs. The first is the inability for SNAP-eligible adults to sort across the age cutoff: one cannot control their age, no matter how hard they try. Other forms of noncompliance around the cutoffs can only occur through the use of discretionary exemptions or geographic waivers from the ABAWD work requirement. I control these forms using data from the USDA Food and Nutrition Services. As a result, these estimates (especially for the GWR) can be considered credible for a treatment effect rather than an intention-to-treat (ITT) effect.

While the properties of the age cutoffs are advantageous for estimation, there is a drawback: estimates of treatment effects using RDDs are "local" in nature. Using the continuity framework for sharp RDDs, this inference is valid for relatively small subpopulations of those subject to the requirements. Specifically, I can only draw inference on people around the age cutoffs. As Section 1.3.2 suggests, liquidity constraints and aging effects may affect the labor market decisions of people at different ages. A different approach would need to be used to make the estimates generalizable across a more age diverse population.

Despite the drawbacks, sharp RDD is still a powerful causal inference tool. With the microdata used in this study, I am able to estimate the effect of the work requirements across nine states: the largest cross-section with this level of granularity compared to previous work requirement research.

Further, by including an RDD at age 60 for the GWR, I reduce the concern of previous work only examining age 50 for the ABAWD.

1.5 Data

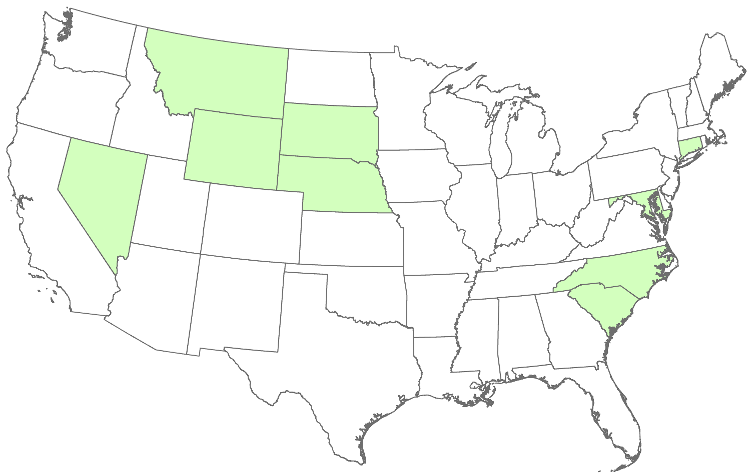
In this section, I describe the data I use to estimate the effects of the work requirements and then provide evidence of the validity of the estimation strategy. I use monthly client-level SNAP administrative data from nine states to identify SNAP users. Then, I merge this data longitudinally to annual American Community Survey (ACS) microdata. Using responses from the ACS and data from ABAWD work requirement waiver requests, I create subsamples of SNAP-eligible and SNAP-participating ACS respondents that are likely subject to the GWR and the ABAWD work requirement. I close this section with placebo and predetermined outcome tests using the samples. Estimates from these exercises show a lack of statistically significant discontinuities at the age cutoffs, validating the empirical design.

1.5.1 Construction of the Samples of Interest

At the time of writing, the Census Bureau has active SNAP administrative data sharing agreements with 10 states: Connecticut (2004-2017), Illinois (2017-2020), Maryland (2017-2019), Montana (2012-2022), Nebraska (2018-2020), Nevada (2017-2021), North Carolina (2014-2020), South Carolina (2004-2021), South Dakota (2015-2021), and Wyoming (2004-2017). For this study, I use longitudinal data from all states apart from Illinois, which (1) suffers from data quality issues, and (2) waived ABAWD requirements for most of the years available. Figure 1.2 presents a map of the states in this analysis. According to the 2010 Decennial Census, five of these states have at least 30% of their population living in rural areas. Two of them (Montana and South Dakota) are among the top 10 states with the highest share of their population living in rural areas. Unlike previous studies that focus on more urban states, such as Virginia and Maryland, this analysis contains many urban and rural respondents.

Because SNAP is operated through state governments with limited federal restrictions, there is state-level variation in what is recorded administratively. However, all states contain monthly client-level data on SNAP recipients. This includes data on benefit amount for the case, income and

Figure 1.2 States in Analysis



Note: States highlighted in green have active SNAP data sharing agreements with the US Census Bureau with the exception of Illinois. Illinois has an active data sharing agreement, but suffers from data quality issues that would jeopardize the analysis if included.

resources of the household, and demographic information for at least the head of household. For the purposes of merging the SNAP administrative data to the American Community Survey microdata, I summarize the monthly datasets into annual measures. Specifically, a client is considered a SNAP user if their case receives a benefit amount greater than zero in any month of the corresponding year. I also record the number of months each client receives a positive SNAP benefit in the year.

The annual client-level SNAP administrative data for the nine states are then merged longitudinally to survey respondents from the American Community Survey (ACS) for years 2004-2021. The ACS has a wealth of data for each respondent, including information about employment, education, and other demographic characteristics. To minimize the prevalence of mass points in the running variable for the RDD, I construct a granular measure of age (age in days) for each respondent by taking the difference of the ACS interview date and birthdate of each respondent. I then use several variables to construct the samples of interest, which are based on the national eligibility criteria for SNAP (having a household income at most 130% above the poverty line) and exemption rules outside of the age limits for the GWR and ABAWD mentioned in Section 1.2.2.[3]

As the ABAWD work requirement can be waived through geographic waivers, I compiled data from state geographic waiver requests (USDA FNS, 2024a). These data contain the geographic areas in the nine states that were waived along with their cited unemployment rates and requested waiver duration. Throughout the analysis period, the most waived geographies were at the state level (during the Great Recession, its recovery, and the COVID-19 pandemic) and at the county or county-group level. Other waived geographies include cities, which Connecticut exclusively waived, and American Indian Reservations. Counties were assigned their corresponding FIPS code and were merged to the ACS at the month-county level. Other geographies were overlaid onto census tract shapefiles and then merged to the ACS on the month-tract level. ABAWDs that reside in areas that were waived at the time of their interview were removed from the samples.

Table 1.1 presents summary statistics of the four main samples used in this analysis. All samples are restricted to individuals aged 18-80. Columns 1 and 2 display statistics for potential ABAWDs (outside of the age limits) that are either SNAP-eligible based on the national eligibility cutoff (Column 1) or receiving SNAP based on the administrative data (Column 2). Analogously, Columns 3 and 4 display those that are SNAP-eligible or receiving SNAP that are potentially subject to the GWR outside of the age limits. Approximately 20% of eligible ABAWDs in the sample take up SNAP, and around 26% of those likely subject to the GWR receive SNAP.

As the statistics displayed in Table 1.1 are of individuals both above and below the age limits, it is difficult to see an effect of the work requirements. However, there are several interesting aspects to note in these samples. The first are the average ages of the samples. For the ABAWD samples, the average age is between 53 (Column 1) and 56 (Column 2), well above the age cutoff at 50. In contrast, the average age of those subject to the GWR hover around 47-48 years old, which is

[3]It is important to acknowledge that assignment into these subsamples is not perfect due to data constraints. For example, the ACS does not ask whether a respondent is pregnant. Therefore, I cannot remove pregnant individuals from the samples that attempt to estimate the effects of the ABAWD work requirement. However, the ACS does contain information on the majority of characteristics listed in Section 1.2.2 that would exempt worker from the requirements, such as work hours, school enrollment, age of children in the household, and disability status. This makes up for the often insufficient demographic data found in the SNAP administration data.

Table 1.1 Summary Statistics

	ABAWD Samples		GWR Samples	
	(1)	(2)	(3)	(4)
	SNAP Eligible	SNAP Users	SNAP Eligible	SNAP Users
Age (Days/365.25)	53.71	56.09	48.81	47.38
	(19.41)	(16.65)	(19.06)	(17.98)
Receive SNAP=1	0.19		0.26	
	(0.39)		(0.44)	
Months Receiving SNAP	1.86	9.42	2.57	9.35
	(4.14)	(3.77)	(4.66)	(3.74)
Employed=1	0.19	0.12	0.19	0.16
	(0.39)	(0.33)	(0.40)	(0.36)
Unemployed=1	0.07	0.10	0.11	0.17
	(0.26)	(0.29)	(0.32)	(0.38)
Not in Labor Force=1	0.74	0.78	0.69	0.67
	(0.44)	(0.41)	(0.46)	(0.47)
ln(Hours Worked)	2.62	2.68	2.65	2.73
	(0.64)	(0.63)	(0.64)	(0.60)
Hours Worked	16.04	16.86	16.48	17.44
	(7.06)	(7.09)	(7.11)	(6.92)
Worked in the	0.26	0.18	0.27	0.23
past 12 months	(0.44)	(0.38)	(0.44)	(0.42)
Last worked 1-5	0.24	0.22	0.25	0.26
years ago	(0.43)	(0.41)	(0.43)	(0.44)
Last worked	0.51	0.60	0.48	0.51
5+ years ago	(0.50)	(0.49)	(0.50)	(0.50)
Annual Wages	989.20	699.60	1034.00	914.30
	(2392)	(2097)	(2449)	(2343)
ln(Annual Wages)	7.97	7.97	7.97	7.98
	(1.10)	(1.16)	(1.13)	(1.14)
Female=1	0.60	0.63	0.63	0.66
	(0.49)	(0.48)	(0.48)	(0.48)
Married=1	0.23	0.15	0.26	0.21
	(0.42)	(0.36)	(0.44)	(0.41)
Poverty Index	59.82	128.50	58.73	118.70
	(43.72)	(129.60)	(43.25)	(124.30)
Earned Associate's	0.23	0.13	0.21	0.12
or Bachelor's	(0.42)	(0.34)	(0.41)	(0.32)
No Child under 18			0.72	0.63
in Household=1			(0.45)	(0.48)
Geographic Waiver			0.29	0.32
			(0.46)	(0.47)
ABAWD waived			0.76	0.83
			(0.43)	(0.38)
N	27500	7700	117000	44500

Note: Standard deviations are presented in parentheses under means. Contains respondents age 18-80. Each column represents a sample created by criteria that determines whether a person is waived of a requirement outside of the age restriction. Columns (1) and (2) include everyone likely subject to the ABAWD work requirement. Columns (3) and (4) include everyone likely subject to the GWR. Columns (1) and (3) contain SNAP-Eligible individuals under the income requirement (130% FPL or lower). Columns (2) and (4) include all individuals participating in SNAP. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961) below the age limits for both requirements. This could be driven by the more lenient exemptions for the ABAWD work requirement. Around 76% of the SNAP-eligible individuals and 83% of the SNAP Users in the GWR samples are exempt from the ABAWD work requirement for conditions outside of age. This seems to be driven in particular by the geographic waivers and the children-under-18 in the household exemption. Around 30% of both GWR samples have respondents living in geographically waived areas. Around 37% of those in the SNAP Users (Column 4) sample have children under 18 in their homes. As many women give birth to children in their late 20s and in their 30s, this could be an exemption that drives the average ABAWD sample age up.

In terms of the labor market outcomes of interest, there are several interesting features. In all samples, the majority of respondents are not in the labor force. Those that are employed work on average about 16-17 hours a week, which is below the ABAWD hourly requirement. People in the sample also earn around $1000 annually on average. Finally, there is a striking difference in poverty levels between the SNAP-eligible samples (Columns 1 and 3) and the SNAP User samples (Columns 2 and 4). For example, the average poverty level for SNAP-eligible ABAWDs (Column 1) is around 60% of the poverty line. ABAWDs that receive SNAP on average have a poverty level of 129%, much closer to the national eligibility cutoff of 130%. However, these means reflect a common empirical result following the restructuring of welfare programs in the mid-1990s. From 1984 to 2004, transfers from means-tested programs to those with incomes under 50% of the poverty declined by 37%, while those with incomes between 100% and 150% of the poverty line experiences a 93% increase in transfers (Moffitt, 2016).

To check the internal validity of these summary statistics and the degree the sample is representative, analogous summary statistics for the entire ACS sample and all SNAP Users in the ACS are presented in Table 1A.1. About 8% of the total sample receives SNAP, which is similar to the percentage that receives SNAP based on state annual participation rates throughout the analysis periods (USDA FNS, n.d.). On average, SNAP participants are younger, less likely to be employed, more likely to be out of the labor force, earning less, and closer to the poverty line compared to the entire sample, which is expected as SNAP is a means-tested program. In terms of SNAP participation for those subject to the work requirements, the USDA notes that only 15-20% of the SNAP population is subject to the GWR, and only 5%-10% are subject to both requirements (USDA FNS, 2024b). Around 23% of the sample is subject to the GWR, and 4% are subject to ABAWD (not considering age). Additionally, around 27% of the total sample and 33% of the sample receiving SNAP resided in waived areas at the times in which they were waived. Overall, the samples seem representative to the populations of interest.

1.5.2 Tests for Validation of the RDD

There are several tests to demonstrate the validity of a RDD. The first is to show that the discontinuity only exists at the cutoff for the outcomes of interest. If it exists for predetermined variables or theoretically independent (placebo) outcomes, identification is threatened because the observations are not as-good-as random or similar around the cutoff (Cattaneo et al., 2019). Table 1.2 show these tests at the ABAWD age cutoff at 50 and the GWR cutoff at 60. Specifically, Columns 1 and 2 test the ABAWD age cutoff for the SNAP-receiving and SNAP-eligible ABAWD samples, and Columns 3 and 4 test the GWR age cutoff for the analogous GWR samples. Across the predetermined and (arguably) placebo outcomes of sex, marriage status, and college education, there are no statistically significant discontinuities. This validates the regression discontinuities using the respondents in the samples of interest.

One can also show the validity of the RDD by estimating placebo and predetermined RDDs on samples that are not directly affected by the policy. Table 1A.2 present these estimates for the full ACS sample, the All SNAP Users sample, and the sample that is nationally eligible for SNAP based on the poverty level requirement. These columns serve to see whether the discontinuities in demographic outcomes presented in Table 1.2 exist for the entire sample (Panel 1). Additionally, these larger samples allow for testing whether covariates that affect whether a person is subject to the work requirements are discontinuous at the cutoffs (Panel 2).

For the larger, more inclusive samples, there are a couple statistically significant discontinuities at the age cutoffs. For the ABAWD cutoff at 50, respondents of the ACS in general (Column 1) and are nationally eligible for SNAP (Column 3) are less likely to have at least an associate's or

Table 1.2 Placebo and Predetermined Outcomes across the Age Cutoffs

	ABAWD Cutoff at 50		GWR cutoff at 60	
	(1)	(2)	(3)	(4)
	Receiving SNAP	SNAP Eligible	Receiving SNAP	SNAP Eligible
Female=1	0.091	0.038	-0.034	0.004
	(0.057)	(0.036)	(0.021)	(0.013)
Married=1	0.025	0.022	0.006	0.016
	(0.043)	(0.028)	(0.017)	(0.011)
Earned Associate's	0.034	-0.023	0.013	-0.004
or Bachelor's	(0.028)	(0.029)	(0.015)	(0.013)
Observations	7700	27500	44500	117000

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the Mean Squared Error (MSE) optimal bandwidth selection method outlined by Cattaneo et al. (2019). Heteroskedasticity-robust nearest neighbor standard errors are displayed in parentheses under the point estimates. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

bachelor's degree at the cutoff. However, this difference is not statistically significant once the sample is cut down to ABAWDs. Panel 2 of Table 1.2 shows whether predetermined variables that affect assignment into ABAWD status are discontinuous at the age cutoff. SNAP applicants (Column 2) and SNAP eligible individuals (Column 3) are more likely to have children under 18 in their households at the cutoff. This means that more people are exempt from the ABAWD requirement at the cutoff, holding all else equal. The GWR age cutoff tests in Columns 4 through 6 have less instances of statistically significant differences of the predetermined variables at the cutoff in general. While these results are important to consider, Table 1.2 shows that once exempt individuals are removed from the samples, there are no statistically significant discontinuities at the age cutoffs in the predetermined variables that could propagate bias in the estimates. Based on this information, the age cutoffs are valid for a RDD.

1.6 Results

Throughout this section, I present estimates of discontinuities on SNAP takeup and labor market outcomes at both the ABAWD work requirement cutoff at 50 and the GWR age cutoff at 60. Each estimate is from its own set of regressions. As a result, the MSE-optimal bandwidths change with each discontinuity estimate. This section follows a similar structure to Section 1.3. As both requirements aim to reduce SNAP caseloads, I present estimates on SNAP uptake first. Then, I present results on static and dynamic labor market outcomes. The section closes with estimates on heterogeneous effects of the work requirements due to gender.

1.6.1 SNAP Uptake

If work requirements prevent those who find it too costly to work at the required intensity, there should exist a positive discontinuity between those subject to the requirements and those that are not. In other words, individuals who are not subject to the requirements should takeup SNAP at a higher rate than those subject to them. Table 1.3 provides estimates on whether take-up of SNAP changes discontinuously at the age cutoffs using the entire SNAP eligible sample (Columns 1 and 3), and then the sample that is both SNAP eligible and likely subject to the work requirements outside of age (Columns 2 and 4). For both the ABAWD work requirement and the GWR, there are no statistically significant discontinuities at the conventional levels. In essence, there is no statistical evidence to support the claim that individuals drop out of SNAP because they find it too costly to work more (in the case of the ABAWD work requirement) or work at all (in the case of the GWR).

Many previous studies that estimate the effect of the ABAWD work requirement find that the requirements do reduce caseloads (Stacy et al., 2018; Harris, 2021; Gray et al., 2023; Cuffey et al., 2022). While there may be many reasons why this analysis yields different results, it could be due to how I define the samples. To be in samples of SNAP-eligible individuals, a respondent must have a poverty index lower than 130. The ACS does not record measures of poverty for people living in group quarters, such as hospitals and homeless shelters, effectively removing representation of

Table 1.3 Take Up of SNAP at the Work Requirement Age Cutoffs

	ABAWD Cutoff at 50		GWR Cutoff at 60	
	(1)	(2)	(3)	(4)
	All SNAP-Eligible	SNAP-Eligible ABAWDs	All SNAP-Eligible	SNAP-Eligible under GWR
Receive SNAP	0.004	-0.011	0.001	0.004
	(0.008)	(0.027)	(0.008)	(0.013)
Observations	358000	27500	358000	117000

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the Mean Squared Error (MSE) optimal bandwidth selection method outlined by Cattaneo et al. (2019). Heteroskedasticity-robust nearest neighbor standard errors are displayed in parentheses under the point estimates. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

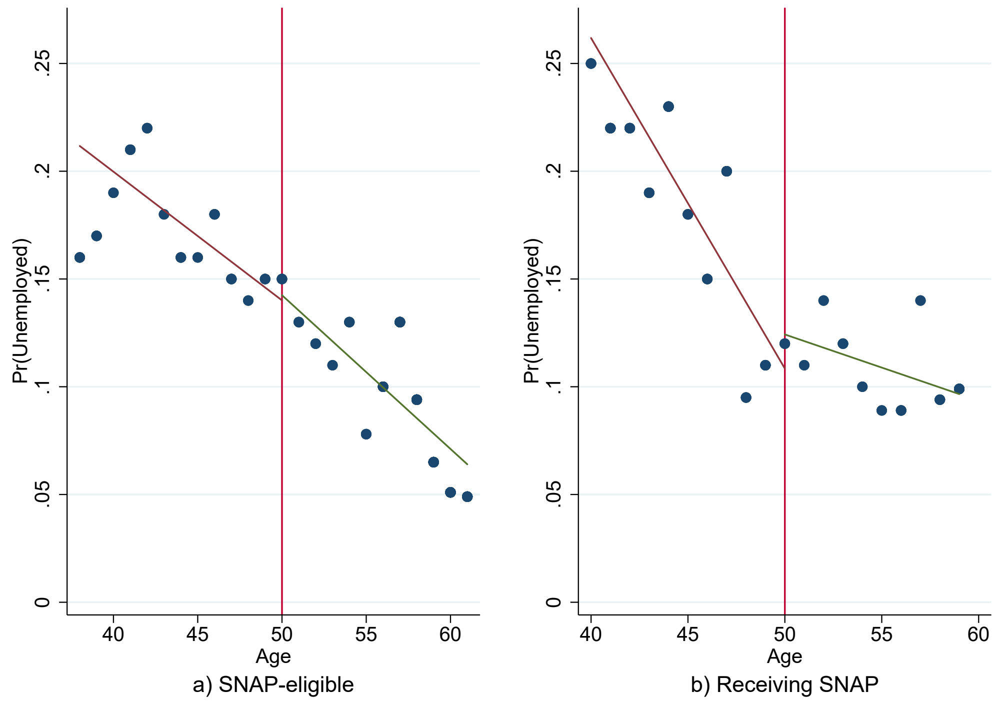
those SNAP Users from the samples. Previous studies suggest these populations are the most adversely affected by the work requirements (Gray et al., 2023).

Table 1A.3 presents the estimates at the age cutoffs using the entire ACS samples. If I do not remove individuals that are not eligible for SNAP under the national income criteria, there is a small in magnitude, but statistically significant positive discontinuity at the ABAWD cutoff. The probability of receiving SNAP is 0.4 percentage points higher for those at 50 years of age. While this is a result that supports past studies, it is difficult to justify that it is driven by people who are in fact ABAWDs outside of the age limit. There is no statistically significant discontinuity at the GWR cutoff using the same sample.

1.6.2 Static Labor Market Outcomes

If the work requirements are binding, analytical results from the static stylized budget curves in Section 1.3.1 suggest that adults subject to them should change their work habits to retain benefits. Specifically, the ABAWD work requirement should change the number of hours ABAWDs work, while the GWR should force those subject to the GWR to work any number of hours. As the previous section found no evidence to support the alternative action of dropping out of SNAP and losing benefits, the results of this section should find some changes in work behavior around the cutoffs if the work requirements are binding.

Figure 1.3 ABAWD Age Cutoff



Note: The figure plots the local polynomial estimation of the regression discontinuity design at age 50. Each point represents an evenly spaced bin between the bandwidth and the cutoff. The score is age in days divided by 365.25. The outcome variable is the binary outcome of whether or not a person is unemployed during the interview. Panel A includes all respondents to the ACS that are nationally eligible for SNAP (report an income level less than or equal to 130% of the federal poverty line (FPL). Panel B includes all respondents that received at least one monthly SNAP benefit during the year they were interviewed. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

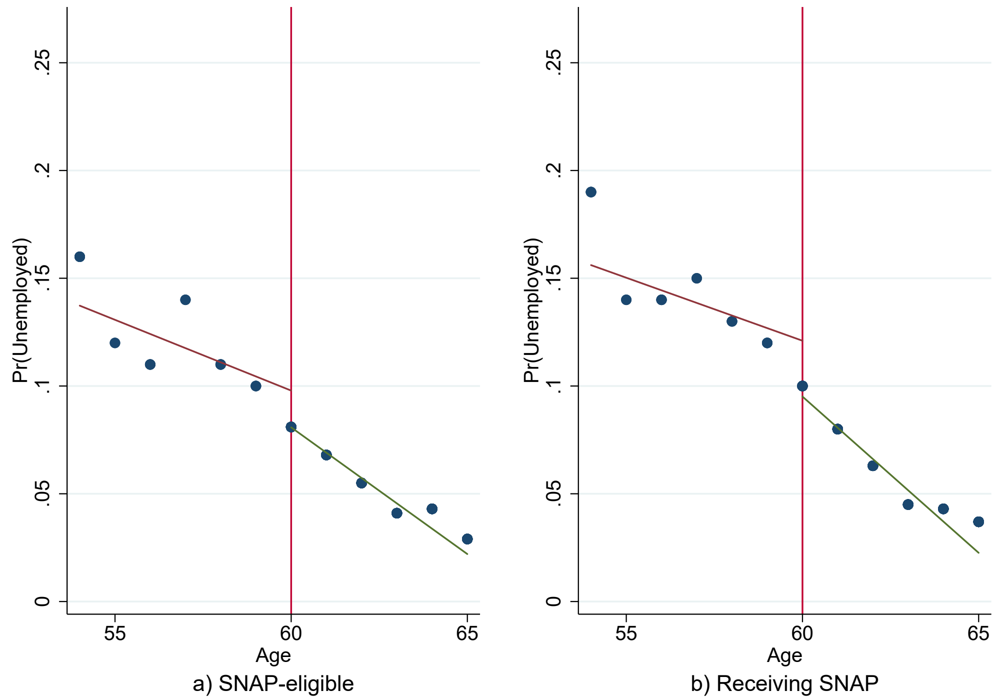
Figure 1.3 presents the graphical representation of the discontinuity for the binary outcome of being unemployed for the SNAP Eligible individuals and SNAP users that are likely ABAWDs. The overall trend of unemployment for these samples around the cutoff is decreasing – the local linear fit suggests that on average, 20%-25% of 40 year old ABAWDs are unemployed, and that by the time they are around 60, the unemployment rate is closer or below 10%. This discontinuity is not statistically significant at the cutoff for both samples, providing no evidence that supports ABAWDs change their labor market status around the cutoff. This is expected based on the features of the work requirement: individuals on both sides of the cutoff are still subject to the GWR, meaning they still must work to retain their benefits.

Table 1.4 presents the associated parameter estimates for Figure 1.3 along with all other labor market outcomes. At the ABAWD cutoff (Columns 1 and 2), there are no statistically significant discontinuities for both a change in employment status and changes in hours worked. There could be several reasons for these results. Across both the SNAP User sample (Column 1) and the SNAP-eligible sample (Column 2), there is a negative discontinuity in hours worked around the cutoff of around 0.5-1 hours. If this was statistically significant, it would show that ABAWDs are working slightly less hours once they are no longer subject to the requirement, supporting the hypothesis that ABAWDs choose the second-best allocation closest to their optimal allocation without the requirement. Because this estimate is not statistically significant at conventional levels however, there is no statistical evidence to support this hypothesis. This is further shown in tests using the larger, more inclusive samples in Table 1A.4.

Around the age cutoff, ABAWDs may face other pressures that either constrain their ability or even their marginal utility to substitute labor for leisure. For example, income at that age might provide more utility at that age for workers compared to those earlier or later in their lives. Additionally, employers may not offer jobs on an hourly gradient that would allow ABAWDs to optimize perfectly around the cutoff, providing a different sort of constraint to workers eligible or using SNAP. This may mean the ABAWD work requirement is not binding, and thus individuals do not respond to it.

In contrast to the ABAWD work requirement, there are statistically significant discontinuities for several labor market outcomes along the cutoff at age 60 for those who would be subject to the GWR. Figure 1.4 presents a graphical representation of the binary outcomes of unemployment for each SNAP-Eligible (Panel A) and SNAP user (Panel B) around the GWR age cutoff. There is a statistically significant discontinuity with a point estimate of around 2 to 2.5 percentage points around the cutoff. This means that once a person is no longer subject to the GWR, they are less likely to be unemployed.

Figure 1.4 GWR Age Cutoff



Note: The figure plots the local polynomial estimation of the regression discontinuity design at age 60. Each point represents an evenly spaced bin between the bandwidth and the cutoff. The score is age in days divided by 365.25. The outcome variable is the binary outcome of whether or not a person is unemployed during the interview. Panel A includes all respondents to the ACS that are nationally eligible for SNAP (report an income level less than or equal to 130% of the poverty threshold. Panel B includes all respondents that received at least one monthly SNAP benefit during the year they were interviewed. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

This discontinuity is driven not by an increase in individuals becoming employed, but by an increase in SNAP-eligible and SNAP users leaving the workforce. Column 3 and Column 4 in Table 1.4 presents estimates of the discontinuity for all labor market outcomes around the GWR cutoff. Employment for SNAP users (Column 3) and SNAP-eligible respondents (Column 4) has a very small discontinuity at the cutoff that is not statistically significant at conventional levels. However, the point estimates on the binary outcome of not being in the labor force are near the same magnitude as the estimates for unemployment, just different signs. I can infer that potential and current SNAP users are dropping out of employment at 60, which supports the notion that the GWR is a binding constraint.

It is important to acknowledge that there is a statistically significant discontinuity for employment and leaving the labor force in the entire ACS sample (Table 1A.4). While this discontinuity could be driven by the GWR, there may be another discontinuity at this age cutoff that affects labor outcomes. for example, encroaching retirement may change the habits of all individuals approaching the minimum age at 62.

1.6.3 Dynamic Labor Market Outcomes

Under the dynamic hypotheses, the work requirements limit the liquidity of SNAP users undergoing a job search. As a result, those subject to the requirements might have shorter search durations, leading to acceptances of lower wages compared to those not subject to them. Panel 2 of Table 1.4 presents the estimates for these dynamic labor outcomes at the ABAWD age cutoff (Columns 1 and 2), and the GWR cutoff (Columns 3 and 4). Across both cutoffs, there are no statistically significant discontinuities for both length of joblessness spells and annual wages. In this study, there is no evidence that workers that use SNAP or are eligible for it modify their search behaviors due to the work requirement.

One reason for the lack of changes in dynamic behavior in search may be due to the inherently transient nature of those subject to the work requirements. A person is waived of the GWR (and in turn, the ABAWD work requirement) if they work more than 30 hours a week or earn an equivalent of 30 hours multiplied by the federal minimum wage. Therefore, those subject to these requirements are not working full time and are earning relatively low wages. The upwardly mobile prospects of job search among these SNAP users may be relatively limited, no matter the length of their search.

1.6.4 Heterogeneity across Genders

Men and women face different societal pressures and roles, which affects their labor market outcomes (Goldin, 2014). Therefore, the effect of the work requirement may affect SNAP-eligible adults and SNAP Users differently depending on gender. Table 1.5 present the estimates of the labor market outcomes at the ABAWD cutoff for men (Colums 1 and 2) and women (Columns 3 and 4). For both genders, SNAP Eligible respondents (Columns 2 and 4) and SNAP users (Column

Table 1.4 Employment Outcomes at the Work Requirement Age Cutoffs

	ABAWD Cutoff at 50		GWR Cutoff at 60	
	(1)	(2)	(3)	(4)
	SNAP Users	SNAP Eligible	SNAP Users	SNAP Eligible
Panel 1: Static Labor Market Outcomes				
Months Receiving SNAP	0.480	-0.453	-0.183	-0.060
	(0.403)	(0.354)	(0.165)	(0.141)
Employed=1	-0.012	-0.023	0.000	-0.003
	(0.040)	(0.025)	(0.015)	(0.009)
Unemployed=1	0.017	0.005	-0.026*	-0.021***
	(0.034)	(0.019)	(0.014)	(0.008)
Not in Force=1	-0.015	0.019	0.023	0.023*
	(0.051)	(0.029)	(0.020)	(0.012)
ln(Hours Worked)	-0.039	-0.069	0.027	0.003
	(0.154)	(0.076)	(0.076)	(0.042)
Hours Worked	-0.495	-1.009	0.553	0.191
	(1.793)	(0.817)	(0.847)	(0.435)
Panel 2: Dynamic Labor Market Outcomes				
Worked in the past 12 months	0.004	-0.025	0.014	-0.001
	(0.044)	(0.027)	(0.017)	(0.010)
Last worked 1-5 years ago	-0.004	0.005	-0.011	0.000
	(0.056)	(0.035)	(0.023)	(0.015)
Last worked 5+ years ago	0.000	-0.001	-0.001	0.006
	(0.054)	(0.035)	(0.025)	(0.017)
Annual Wages	-355.50	-238.20	46.01	26.27
	(271.90)	(174.40)	(92.21)	(54.39)
ln(Annual Wages)	-0.463	-0.165	-0.073	0.001
	(0.290)	(0.160)	(0.156)	(0.074)
Observations	7700	27500	44500	117000

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the Mean Squared Error (MSE) optimal bandwidth selection method (Cattaneo et al., 2019). Heteroskedasticity-robust nearest neighbor standard errors are displayed in parentheses. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

1 and 3) that are likely subject to the ABAWD work requirement do not change their employment status at the cutoff.

However, there are some gender-specific statistically significant results on hours worked and earnings. At the ABAWD cutoff, male SNAP users (Column 1) earn a 70% (-$834.60) lower annual wage. This result is evidence against a dynamic hypotheses, as ABAWDs earn less once they are no longer subject to the requirement. However, it may be suggestive proof of a static hypothesis: an ABAWD no longer subject to the requirement might move to an income-leisure allocation that reduces their income in favor of more leisure hours.

The results for hours worked for men at the ABAWD age cutoff do not provide statistically significant support of this claim. On average, men reduce their work hours by approximately 2 hours, but this estimate is not statistically significant. Women likely subject to the ABAWD work requirement and are eligible for SNAP (Column 4) show a statistically significant reduction in log hours at the age cutoff at the 10% level, but all other estimates (including changes in annual wage) are not statistically significant.

Table 1.6 presents gender-specific estimates at the GWR age cutoff at age 60. Interestingly, the results for the entire sample in Table 1.4 about labor market exit are driven by SNAP-eligible/SNAP-using women. The estimates on probability of being unemployed for men (Column 1 and 2) are quite small in magnitude and not statistically significant at the conventional levels. In contrast, the estimates for women (Columns 3 and 4) are similar in magnitude to those in Table 1.4 for both probability of being unemployed and exiting the labor force.

In terms of dynamic labor market outcomes (Panel 2 in Table 1.6), there is some evidence among SNAP-using men (Column 1) that they engage in relatively short joblessness spells or none at all at the 10% significance level. There is also a positive discontinuity in annual wages at the 5% significance level of roughly $264. Perhaps men and women who use SNAP perceive closeness to retirement differently and no longer being subject to the work requirements inform this result.[4]

[4]Gender-specific estimates for the larger, more inclusive samples can be found in Appendix 1A. Specifically, Table 1A.5 present estimates for the GWR cutoff and Table 1A.6 presents estimates for the ABAWD cutoff.

Table 1.5 Gender Employment Outcomes at the ABAWD Age cutoff

	Men at ABAWD Cutoff		Women at ABAWD Cutoff	
	(1)	(2)	(3)	(4)
	SNAP Users	SNAP Eligible	SNAP Users	SNAP Eligible
Panel 1: Static Labor Market Outcomes				
Months Receiving SNAP	-0.065	-0.170	0.845*	-0.263
	(0.589)	(0.394)	(0.471)	(0.446)
Employed=1	-0.072	-0.028	0.021	-0.032
	(0.046)	(0.034)	(0.057)	(0.034)
Unemployed=1	-0.023	-0.026	0.043	0.026
	(0.060)	(0.030)	(0.037)	(0.025)
Not in Labor Force=1	0.103	0.050	-0.097	0.001
	(0.073)	(0.043)	(0.067)	(0.038)
ln(Hours Worked)	0.215	-0.019	-0.153	-0.169*
	(0.300)	(0.128)	(0.202)	(0.094)
Hours Worked	2.039	-1.117	-1.471	-1.186
	(3.830)	(1.411)	(1.869)	(1.006)
Panel 2: Dynamic Labor Market Outcomes				
Worked in the past 12 months	-0.066	-0.060	0.044	-0.003
	(0.055)	(0.037)	(0.063)	(0.035)
Last worked 1-5 years ago	0.046	-0.020	-0.036	0.038
	(0.078)	(0.052)	(0.075)	(0.040)
Last worked 5+ years ago	0.010	0.052	-0.011	-0.042
	(0.069)	(0.052)	(0.073)	(0.046)
Annual Wages	-834.60**	-300.70	-190.50	-212.80
	(363.30)	(226.20)	(389.40)	(232.90)
ln(Annual Wages)	-1.187**	-0.183	-0.185	-0.176
	(0.523)	(0.275)	(0.266)	(0.171)
Observations	2900	11000	4900	16500

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the Mean Squared Error (MSE) optimal bandwidth selection method (Cattaneo et al., 2019). Heteroskedasticity-robust nearest neighbor standard errors are displayed in parentheses. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

Table 1.6 Gender Employment Outcomes at the GWR Age cutoff

	Men at GWR Cutoff		Women at GWR Cutoff	
	(1)	(2)	(3)	(4)
	SNAP Users	SNAP Eligible	SNAP Users	SNAP Eligible
Panel 1: Static Labor Market Outcomes				
Months Receiving SNAP	-0.076	0.177	-0.253	-0.236
	(0.268)	(0.202)	(0.192)	(0.182)
Employed=1	0.035	-0.010	-0.022	0.003
	(0.022)	(0.013)	(0.019)	(0.012)
Unemployed=1	-0.009	-0.004	-0.028*	-0.027***
	(0.029)	(0.015)	(0.015)	(0.009)
Not in Labor Force=1	-0.031	0.016	0.054**	0.026*
	(0.035)	(0.020)	(0.024)	(0.015)
ln(Hours Worked)	0.016	0.049	0.046	-0.022
	(0.123)	(0.078)	(0.092)	(0.050)
Hours Worked	0.315	0.757	0.943	-0.117
	(1.328)	(0.818)	(1.023)	(0.545)
Panel 2: Dynamic Labor Market Outcomes				
Worked in the past 12 months	0.055*	-0.010	-0.013	0.002
	(0.028)	(0.014)	(0.017)	(0.014)
Last worked 1-5 years ago	-0.030	0.011	-0.009	-0.009
	(0.033)	(0.023)	(0.030)	(0.019)
Last worked 5+ years ago	-0.012	-0.001	0.021	0.010
	(0.034)	(0.024)	(0.036)	(0.022)
Annual Wages	264.20**	54.60	-97.760	-4.33
	(127.20)	(79.89)	(111.40)	(83.14)
ln(Annual Wages)	0.085	0.095	-0.117	-0.033
	(0.256)	(0.150)	(0.179)	(0.087)
Observations	15000	43500	29500	73500

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the Mean Squared Error (MSE) optimal bandwidth selection method (Cattaneo et al., 2019). Heteroskedasticity-robust nearest neighbor standard errors are displayed in parentheses. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

1.7 Conclusion and Policy Implications

The existence and implementation of means-tested programs are often in flux due to different ideological views on their goals and necessity (Barr, 2020). SNAP remains a stalwart program for several reasons, including the protections it has under the Farm Bill and its success at distributing US domestic agricultural surplus (Bosso, 2023). This essay addresses the efficacy of the work requirements, which were created to limit SNAP benefits to those willing to work. If the work requirements are binding to a SNAP-eligible individual, static economic theory suggests an increase in labor or the loss of SNAP benefits depending on the individual's marginal rate of substitution between labor and leisure.

In this chapter, I test the effect of these work requirements using administrative SNAP data from nine states. I longitudinally link this data to microdata from the American Community Survey. I then use the age cutoff for ABAWDs at age 50 and the age cutoff at age 60 for the GWR. For both cutoffs, there is no evidence to suggest SNAP participation changes. For the ABAWD cutoff, I find no statistical evidence to support the hypothesis that the ABAWD requirement does in fact change the labor market behavior of SNAP-eligible adults and SNAP users. For the GWR, I find that SNAP participants and eligible adults likely subject to the requirement are more likely to leave the labor force at the cutoff. When restricting the samples by gender, there is slight evidence that suggests a heterogeneous effect between genders on earnings and hours worked for both work requirements.

The null results for the ABAWD work requirement are similar to results in other works. Other researchers found no changes in labor market participation and earnings. However, my results differ with respect to caseloads: other studies often find a statistically significant reduction, while I find none. There are two likely reasons for this difference. The first is that the ABAWD work requirement is nonbinding for the underlying population of this study. Because of data limitation in the ACS, those living in group quarters (such as hospitals, dorms, and homeless shelters), are not included in the samples created based on SNAP eligibility. Studies have found that these populations are more susceptible to losing benefits under the ABAWD requirement (Wheaton et al., 2021; Gray et al., 2023). Therefore, I cannot capture the effect of the work requirements on those who may have more difficulty meeting the requirements. However, my samples do capture a majority of the population that these requirements are imposed upon.

The second reason may be that the GWR may be binding for most ABAWDs, thus limiting their ability to change their labor allocations. While the general work requirement does not demand a certain number of hours of work like the ABAWD requirement does, it does limit one's ability to reduce hours or willfully become unemployed. As my results show a statistically significant reduction in labor force participation at the GWR age cutoff, this may indicate the GWR may be more binding than the ABAWD for SNAP users at age 50. However, it is difficult to compare the estimates due to the inherent local nature of RDD. Future studies that can capture exogenous variation in both GWR and ABAWD work requirement implementation and adherence across age groups may better disentangle the effects of age and policy structure.

I do find some evidence of heterogeneous effects across gender. Specifically, women seem to be driving the labor force exit at the GWR cutoff at 60. While the mechanisms that motivate this effect should be a focus in future studies, I can make a conjecture based on the prior literature. There is evidence that the gender wage gap worsens as workers get older across all age cohorts (Goldin, 2014). Facing this prolonged gap in wages, women may find that leaving the labor force that close to retirement is less costly than continuing work. However, this wage gap was found for full-time workers. More research on the gender wage gap for part-time work must be investigated before drawing that conclusion. Empirically assessing a potential difference in behavior for SNAP households with grandparents may also help illuminate the effect at this cutoff.

The results of this study offer some policy suggestions. The null results for both SNAP uptake and labor market outcomes for the ABAWD requirement suggest that the requirement is not binding in the underlying population. Depending on the administrative burden and costs of reporting and implementing the requirement, policymakers should consider eliminating it (at least for ABAWDs around the age of 50). This suggestion goes against recent legislation that increased eligibility to age 52 (Qiu, 2023). If the population of ABAWDs around this age is large, simplified and change reporting may become burdensome for caseworkers with no substantial effect at reducing caseloads.

For the GWR, my results suggest that the marginal rate of substitution between income and leisure of the underlying population leans heavily towards a preference for income. As a result, the GWR does not reduce caseloads, but rather almost entirely “improves” self-sufficiency. Policymakers should again consider the administrative costs of this requirement and the benefit it has at preventing SNAP users (nearing retirement) from exiting the labor force.

Future studies that have the luxury of using administrative data should study the heterogenous effects of the work requirements across age. Geographic waivers are issued for the ABAWD work requirement based on an arbitrary local unemployment rate threshold. Studies should use this threshold for a regression discontinuity design to estimate the heterogeneity across age groups. Other studies should exploit potential exogenous variation in GWR across age.

Additionally, future studies should estimate the effect of the requirements on health outcomes. Investment in health is intrinsically tied to labor market decisions as well as participating in means-tested programs. Estimating the effects on health outcomes may offer additional clarity on the mechanisms underlying labor market decisions as well as unintended consequences of the policy.

BIBLIOGRAPHY

Almond, D., Hoynes, H. W., & Schanzenbach, D. W. (2011). Inside the war on poverty: The impact of Food Stamps on birth outcomes. *Review of Economics and Statistics*, *93*(2), 387–403. https://doi.org/10.1162/REST_a_00089

Bailey, M., Hoynes, H., Rossin-Slater, M., & Walker, R. (2023). Is the social safety net a long-term investment? Large-scale evidence from the Food Stamps Program. *The Review of Economic Studies*, rdad063. https://doi.org/10.1093/restud/rdad063

Barr, N. (2020). *Economics of the welfare state*. Oxford University Press, USA.

Beatty, T. K., & Tuttle, C. J. (2015). Expenditure response to increases in in-kind transfers: Evidence from the Supplemental Nutrition Assistance Program. *American Journal of Agricultural Economics*, *97*(2), 390–404. https://doi.org/10.1093/ajae/aau097

Bitler, M. P., Gelbach, J. B., & Hoynes, H. W. (2006). What mean impacts miss: Distributional effects of welfare reform experiments. *The American Economic Review*, *96*(4).

Blundell, R., & Pistaferri, L. (2003). Income volatility and household consumption: The impact of food assistance programs. *The Journal of Human Resources*, *38*, 1032. https://doi.org/10.2307/3558980

Borjas, G. J. (2004). Food insecurity and public assistance. *Journal of Public Economics*, *88*(7), 1421–1443. https://doi.org/10.1016/S0047-2727(02)00188-3

Bosso, C. J. (2023). *Why SNAP works: A political history—and defense—of the Food Stamp Program*. Univ of California Press.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, *82*(6), 2295–2326. https://doi.org/10.3982/ECTA11757

Caputo, V., & Just, D. R. (2022). The economics of food related policies: Considering public health and malnutrition. In *Handbook of agricultural economics* (p. 84). Elsevier.

Case, A., LuBotsky, D., & Paxson, C. (2002). Economic status and health in childhood: The origins of the gradient. *The American Economic Review*, *92*(5), 28.

Case, A., & Paxson, C. (2008). Stature and status: Height, ability, and labor market outcomes. *Journal of Political Economy*, *116*(3), 499–532. https://doi.org/https://doi.org/10.1086/589524

Cattaneo, M. D., Idrobo, N., & Titiunik, R. (2019). *A practical introduction to regression discontinuity designs: Foundations*. Cambridge University Press.

Cattaneo, M. D., & Titiunik, R. (2022). Regression discontinuity designs. *Annual Review of Economics*, *14*, 821–851. https://doi.org/10.1146/annurev-economics-051520-021409

Cuffey, J., Beatty, T. K. M., & Mykerezi, E. (2022). Work effort and work requirements for food assistance among U.S. adults. *American Journal of Agricultural Economics*, *104*(1), 294–317. https://doi.org/10.1111/ajae.12207

Currie, J., & Moretti, E. (2008). Did the introduction of Food Stamps affect birth outcomes in California? *Making Americans Healthier*, 122–42.

East, C. N. (2018). Immigrants' labor supply response to Food Stamp access. *Labour Economics*, *51*, 202–226. https://doi.org/10.1016/j.labeco.2018.01.003

East, C. N. (2020). The effect of Food Stamps on children's health: Evidence from immigrants' changing eligibility. *Journal of Human Resources*, *55*(2), 387–427. https://doi.org/10.3368/jhr.55.3.0916-8197R2

Fan, M. (2010). Do Food Stamps contribute to obesity in low-income women? Evidence from the National Longitudinal Survey of Youth 1979. *American Journal of Agricultural Economics*, *92*(4), 1165–1180. https://doi.org/10.1093/ajae/aaq047

Goldin, C. (2014). A grand gender convergence: Its last chapter. *American Economic Review*, *104*(4), 1091–1119. https://doi.org/10.1257/aer.104.4.1091

Gray, C., Leive, A., Prager, E., Pukelis, K., & Zaki, M. (2023). Employed in a SNAP? The impact of work requirements on program participation and labor supply. *American Economic Journal: Economic Policy*, *15*(1), 306–341. https://doi.org/10.1257/pol.20200561

Gregory, C., Ploeg, M. V., Andrews, M., & Coleman-Jensen, A. (2013, April). *Supplemental Nutrition Assistance Program (SNAP) participation leads to modest changes in diet quality* (ERR-147). U.S. Department of Agriculture, Economic Research Service.

Gundersen, C., & Ziliak, J. P. (2015). Food insecurity and health outcomes. *Health Affairs*, *34*(11), 1830–1839. https://doi.org/10.1377/hlthaff.2015.0645

Hall, L. A. (2022, August). *The ABAWD time limit in Maryland: Impacts on employment and SNAP participation*. University of Maryland School of Social Work.

Han, J. (2020, August 27). The impact of SNAP work requirements on labor supply. https://doi.org/10.2139/ssrn.3296402

Harnack, L., Valluri, S., & French, S. A. (2019). Importance of the Supplemental Nutrition Assistance Program in rural America. *American Journal of Public Health*, *109*(12), 1641–1645. https://doi.org/10.2105/AJPH.2019.305359

Harris, T. F. (2021). Do SNAP work requirements work? *Economic Inquiry*, *59*(1), 72–94. https://doi.org/10.1111/ecin.12948

Hastings, J., & Shapiro, J. M. (2018). How are SNAP benefits spent? Evidence from a retail panel. *American Economic Review*, *108*(12), 3493–3540. https://doi.org/10.1257/aer.20170866

Hoynes, H., Schanzenbach, D. W., & Almond, D. (2016). Long-run impacts of childhood access to the safety net. *American Economic Review*, *106*(4), 903–934. https://doi.org/10.1257/aer.20130375

Hoynes, H. W., & Schanzenbach, D. W. (2009). Consumption responses to in-kind transfers: Evidence from the introduction of the Food Stamp Program. *American Economic Journal: Applied Economics*, *1*(4), 109–139. https://doi.org/10.1257/app.1.4.109

Hoynes, H. W., & Schanzenbach, D. W. (2012). Work incentives and the Food Stamp Program. *Journal of Public Economics*, *96*(1), 151–162. Retrieved July 11, 2022, from https://linkinghub.elsevier.com/retrieve/pii/S0047272711001472

Mabli, J., Ohls, J., Dragoset, L., Castner, L., & Santos, B. (2013). Measuring the effect of Supplemental Nutrition Assistance Program (SNAP) participation on food security, 356.

Maibom, J., Harmon, N., Glenny, A., & Fluchtmann, J. (2023). Unemployed job search across people and over time: Evidence from applied-for jobs. *Journal of Labor Economics*. https://doi.org/10.1086/725165

Moffitt, R. A. (2016). Introduction. In R. A. Moffitt (Ed.), *Economics of means-tested transfer programs in the United States* (pp. 1–20, Vols. 2, Vol. 1). University of Chicago Press. https://doi.org/10.7208/chicago/9780226370507.001.0001

Mortensen, D. T. (1986). Chapter 15 Job search and labor market analysis. In *Handbook of labor economics* (pp. 849–919, Vol. 2). Elsevier. https://doi.org/10.1016/S1573-4463(86)02005-9

Mortensen, D. T., & Pissarides, C. A. (1999). Chapter 39 New developments in models of search in the labor market. In *Handbook of labor economics* (pp. 2567–2627, Vol. 3). Elsevier. https://doi.org/10.1016/S1573-4463(99)30025-0

Nestle, M. (2019). The Supplemental Nutrition Assistance Program (SNAP): History, politics, and public health implications. *American Journal of Public Health*, *109*(12), 1631–1635. https://doi.org/10.2105/AJPH.2019.305361

Policy Basics. (2021). *The Supplemental Nutrition Assistance Program (SNAP)*. Center on Budget and Policy Priorities.

Qiu, L. (2023). Debt ceiling deal includes new work requirements for Food Stamps. *The New York Times*. Retrieved July 10, 2023, from https://www.nytimes.com/2023/05/29/us/politics/debt-limit-deal-food-stamps.html

Ritter, J. A. (2018). *Incentive effects of SNAP work requirements* (P18-5). University of Minnesota, Department of Applied Economics. Retrieved July 13, 2023, from https://ideas.repec.org//p/ags/umaesp/281156.html

Schanzenbach, D. W. (2019). Exploring options to improve the Supplemental Nutrition Assistance Program (SNAP). *The Annals of the American Academy of Political and Social Science*, *686*(1), 204–228. https://doi.org/10.1177/0002716219882677

Stacy, B., Scherpf, E., & Jo, Y. (2018). The impact of SNAP work requirements. https://www.aeaweb.org/conference/2019/preliminary/paper/Z8ZhzBZt

USDA. (n.d.). *What can SNAP buy? | Food and Nutrition Service*. Retrieved July 10, 2022, from https://www.fns.usda.gov/snap/eligible-food-items

USDA FNS. (n.d.). *SNAP data tables | Food and Nutrition Service*. Retrieved November 5, 2023, from https://fns-prod.azureedge.us/pd/supplemental-nutrition-assistance-program-snap

USDA FNS. (2024a, April). *ABAWD waivers | Food and Nutrition Service*. Retrieved June 20, 2024, from https://www.fns.usda.gov/snap/ABAWD/waivers

USDA FNS. (2024b, May). *SNAP work requirement policy resources | Food and Nutrition Service*. Retrieved June 20, 2024, from https://www.fns.usda.gov/snap/work-requirement-policies

USDA Program Development Division. (2018, May 31). *State options report*. United States Department of Agriculture.

Wheaton, L., Vericker, T., Schwabish, J., Anderson, T., Baier, K., Gasper, J., Sick, N., & Werner, K. (2021, June). The impact of SNAP able bodied adults without dependents (ABAWD) time limit reinstatement in nine states. https://fns-prod.azureedge.us/sites/default/files/resource-files/ABAWDTimeLimit.pdf

APPENDIX 1A

ADDITIONAL TABLES

Table 1A.1 Summary Statistics for Other Samples

	Entire Sample		SNAP	
	(1)	(2)	(3)	(4)
	Mean	SD	Mean	SD
Age (Days/365.25)	49.41	(16.50)	44.84	(16.49)
Receive SNAP=1	0.08	(0.27)		
Months Receiving SNAP	0.68	(2.60)	8.82	(3.94)
Employed=1	0.63	(0.48)	0.38	(0.48)
Unemployed=1	0.04	(0.19)	0.13	(0.33)
Not in Labor Force=1	0.33	(0.47)	0.50	(0.50)
ln(Hours Worked)	3.58	(0.49)	3.43	(0.53)
Hours Worked	38.93	(12.84)	34.09	(12.45)
Worked in the past 12 months	0.70	(0.46)	0.48	(0.50)
Last worked 1-5 years ago	0.09	(0.29)	0.16	(0.36)
Last worked 5+ years ago	0.21	(0.41)	0.36	(0.48)
Annual Wages	33000	(60360)	8661	(22210)
ln(Annual Wages)	10.28	(1.21)	9.28	(1.24)
Disabled=1	0.14	(0.35)	0.32	(0.47)
No Child under 18 in Household = 1	0.67	(0.47)	0.55	(0.50)
Geographic Waiver	0.26	(0.44)	0.32	(0.47)
ABAWD waived	0.94	(0.24)	0.96	(0.19)
GWR waived	0.79	(0.41)	0.77	(0.42)
Living with Disabled = 1	0.27	(0.44)	0.49	(0.50)
Female=1	0.53	(0.50)	0.63	(0.48)
Married=1	0.59	(0.49)	0.25	(0.43)
Poverty Index	374	(234.60)	150.90	(138.10)
Earned Associate's or Bachelor's	0.38	(0.49)	0.12	(0.33)
Child under 6 in Household =1	0.07	(0.26)	0.11	(0.31)
N	2537000		197000	

Note: Means are presented in odd columns, while corresponding standard deviations are presented in parentheses in the following even column. Contains respondents age 18-80. Columns (1) and (2) contain all individuals who responded to the ACS in the states and time periods administrative data are available (See Section 1.5). Columns (3) and (4) contain individuals in the sample that are receiving SNAP. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

Table 1A.2 Placebo Checks on Other Samples

	ABAWD Cutoff at 50			GWR Cutoff at 60		
	(1)	(2)	(3)	(4)	(5)	(6)
	ACS Sample	All Receiving SNAP	All SNAP Eligible	ACS Sample	All Receiving SNAP	All SNAP Eligible
Panel 1: Predetermined Variables						
Female=1	0.004	-0.003	0.003	-0.001	-0.024**	-0.008
	(0.002)	(0.010)	(0.008)	(0.003)	(0.009)	(0.008)
Married=1	-0.003	-0.001	-0.008	-0.004	-0.028**	0.003
	(0.003)	(0.009)	(0.008)	(0.003)	(0.013)	(0.008)
Earned Associate's or Bachelor's	-0.009***	-0.006	-0.014**	0.002	0.007	-0.003
	(0.003)	(0.006)	(0.007)	(0.003)	(0.007)	(0.007)
Panel 2: Predetermined Variables that affect Assignment						
GWR Waived	0.000	-0.003	-0.011	-0.002	-0.006	0.007
	(0.002)	(0.007)	(0.008)	(0.004)	(0.011)	(0.009)
ABAWD Waived	-0.001	0.003	0.004	-0.001	0.002	0.010**
	(0.001)	(0.004)	(0.003)	(0.002)	(0.006)	(0.005)
Geographic Waiver	0.002	0.001	0.004	0.005*	0.012	0.013**
	(0.002)	(0.009)	(0.007)	(0.003)	(0.011)	(0.007)
Disabled=1	0.006***	-0.001	-0.003	0.004*	0.005	-0.004
	(0.002)	(0.010)	(0.009)	(0.002)	(0.013)	(0.009)
Living with Disabled=1	0.007**	-0.010	-0.002	0.005*	0.010	-0.004
	(0.003)	(0.011)	(0.009)	(0.003)	(0.010)	(0.008)
No Child under 18 in Household=1	0.004	-0.042***	-0.018*	-0.001	-0.002	-0.011
	(0.005)	(0.014)	(0.010)	(0.002)	(0.010)	(0.008)
Child under 6 in Household=1	0.000	0.002	0.002	-0.000	-0.006*	-0.002
	(0.001)	(0.005)	(0.004)	(0.001)	(0.003)	(0.002)
Observations	2537000	197000	358000	2537000	197000	358000

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the Mean Squared Error (MSE) optimal bandwidth selection method (Cattaneo et al., 2019). Heteroskedasticity-robust nearest neighbor standard errors are displayed in parentheses. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

Table 1A.3 Take Up of SNAP at the Work Requirement Age Cutoffs, Entire ACS Sample

	ABAWD Cutoff at 50	GWR Cutoff at 60
	(1)	(2)
	ACS Sample	ACS Sample
Receive SNAP	0.004**	0.000
	(0.002)	(0.002)
Observations	2537000	2537000

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the Mean Squared Error (MSE) optimal bandwidth selection method (Cattaneo et al., 2019). Heteroskedasticity-robust nearest neighbor standard errors are displayed in parentheses. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

Table 1A.4 Employment Outcomes on Other Samples

	ABAWD Cutoff at 50			GWR Cutoff at 60		
	(1)	(2)	(3)	(4)	(5)	(6)
	All ACS Sample	All Receiving SNAP	All SNAP eligible	All ACS Sample	All Receiving SNAP	All SNAP eligible
Panel 1: Static Labor Market Outcomes						
Months Receiving SNAP	0.037**	0.051	0.017	0.002	0.022	-0.022
	(0.015)	(0.083)	(0.092)	(0.015)	(0.075)	(0.088)
Employed=1	0.001	0.015	0.009	-0.010**	-0.018**	0.007
	(0.003)	(0.010)	(0.008)	(0.005)	(0.009)	(0.007)
Unemployed=1	0.000	0.003	-0.004	-0.001	0.003	-0.004
	(0.001)	(0.006)	(0.005)	(0.001)	(0.006)	(0.004)
Not in Labor Force=1	-0.001	-0.016	-0.004	0.014***	0.018*	-0.003
	(0.003)	(0.010)	(0.009)	(0.005)	(0.010)	(0.008)
ln(Hours Worked)	-0.002	0.004	0.002	0.001	-0.021	0.016
	(0.003)	(0.018)	(0.017)	(0.005)	(0.032)	(0.024)
Hours Worked	-0.091	0.037	0.209	-0.016	-0.342	0.470
	(0.089)	(0.408)	(0.353)	(0.121)	(0.700)	(0.503)
Panel 2: Dynamic Labor Market Outcomes						
Worked in the past 12 months	-0.001	0.009	-0.005	-0.009*	-0.008	0.003
	(0.003)	(0.009)	(0.008)	(0.005)	(0.010)	(0.009)
Last worked 1-5 years ago	0.003*	0.010	0.014**	0.002	-0.005	-0.004
	(0.002)	(0.008)	(0.007)	(0.003)	(0.010)	(0.008)
Last worked 5+ years ago	-0.001	-0.024**	-0.007	0.006*	0.019	0.004
	(0.002)	(0.010)	(0.008)	(0.003)	(0.013)	(0.010)
Annual Wages	-771.90	517.20	27.25	-1140*	-543.40	223.50**
	(497.20)	(470.60)	(116.60)	(603.40)	(530.30)	(88.70)
ln(Annual Wages)	-0.005	0.105**	0.078**	-0.001	-0.037	0.036
	(0.008)	(0.047)	(0.036)	(0.011)	(0.060)	(0.044)
Observations	2537000	197000	358000	2537000	197000	358000

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the MSE optimal bandwidth selection method (Cattaneo et al., 2019). Heteroskedasticity-robust nearest neighbor standard errors in parentheses. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

Table 1A.5 GWR Gender Employment Outcomes on Other Samples

	Men at GWR Cutoff			Women at GWR Cutoff		
	ACS Sample	All Receiving SNAP	All SNAP eligible	ACS Sample	All Receiving SNAP	All SNAP eligible
Panel 1: Static Labor Market Outcomes						
Months Receiving SNAP	0.037**	0.098	0.218*	-0.025	-0.023	-0.216**
	(0.019)	(0.141)	(0.133)	(0.022)	(0.102)	(0.103)
Employed=1	-0.011*	-0.005	0.006	-0.011*	-0.024**	0.007
	(0.006)	(0.013)	(0.011)	(0.006)	(0.012)	(0.009)
Unemployed=1	-0.000	-0.004	0.000	-0.001	0.003	-0.007
	(0.002)	(0.011)	(0.007)	(0.001)	(0.007)	(0.005)
Not in Labor Force=1	0.013**	0.014	-0.006	0.015**	0.022*	-0.001
	(0.006)	(0.015)	(0.012)	(0.007)	(0.012)	(0.010)
ln(Hours Worked)	-0.003	-0.093*	0.034	0.004	0.035	0.006
	(0.006)	(0.050)	(0.036)	(0.006)	(0.038)	(0.030)
Hours Worked	-0.150	-2.18**	0.565	0.079	1.085	0.368
	(0.171)	(1.108)	(0.785)	(0.150)	(0.782)	(0.593)
Panel 2: Dynamic Labor Market Outcomes						
Worked in the past 12 months	-0.008	0.004	0.001	-0.010*	-0.015	0.009
	(0.006)	(0.015)	(0.013)	(0.006)	(0.013)	(0.010)
Last worked 1-5 years ago	-0.001	-0.011	-0.003	0.004	-0.002	-0.005
	(0.004)	(0.015)	(0.012)	(0.004)	(0.012)	(0.010)
Last worked 5+ years ago	0.008**	0.012	0.004	0.003	0.019	-0.001
	(0.004)	(0.018)	(0.015)	(0.004)	(0.016)	(0.012)
Annual Wages	-2938***	-541.90	279.30**	248.40	-637.10	179.20
	(1113.00)	(782.10)	(131.80)	(457.10)	(681.00)	(115.00)
ln(Annual Wages)	-0.022	-0.093	0.120*	0.014	-0.016	-0.008
	(0.016)	(0.090)	(0.064)	(0.014)	(0.075)	(0.054)
Observations	1206000	72000	145000	1331000	124000	213000

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the MSE optimal bandwidth selection method (Cattaneo et al., 2019). Heteroskedasticity-robust nearest neighbor standard errors in parentheses. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

Table 1A.6 ABAWD Gender Employment Outcomes on Other Samples

	Men at ABAWD Cutoff			Women at ABAWD Ctuoff		
	ACS Sample	All Receiving SNAP	All SNAP eligible	ACS Sample	All Receiving SNAP	All SNAP eligible
Panel 1: Static Labor Market Outcomes						
Months Receiving	0.039*	0.009	0.073	0.033	0.051	-0.002
SNAP	(0.020)	(0.122)	(0.127)	(0.022)	(0.110)	(0.115)
Employed=1	0.002	0.016	-0.003	0.002	0.004	0.012
	(0.004)	(0.015)	(0.012)	(0.004)	(0.011)	(0.009)
Unemployed=1	-0.002	-0.021*	-0.009	0.002	0.013*	0.000
	(0.002)	(0.012)	(0.008)	(0.001)	(0.007)	(0.006)
Not in Labor Force=1	0.000	0.002	0.012	-0.004	-0.023*	-0.015
	(0.003)	(0.015)	(0.012)	(0.004)	(0.012)	(0.011)
ln(Hours Worked)	-0.004	0.060**	0.032	-0.000	-0.033	-0.018
	(0.004)	(0.030)	(0.025)	(0.004)	(0.022)	(0.021)
Hours Worked	-0.113	0.886	0.543	0.028	-0.471	0.070
	(0.121)	(0.680)	(0.549)	(0.114)	(0.493)	(0.419)
Panel 2: Dynamic Labor Market Outcomes						
Worked in the	-0.003	0.004	-0.017	0.001	0.008	0.001
past 12 months	(0.003)	(0.014)	(0.011)	(0.004)	(0.011)	(0.009)
Last worked 1-5	0.003	0.020	0.005	0.002	0.004	0.018**
years ago	(0.002)	(0.013)	(0.011)	(0.002)	(0.010)	(0.008)
Last worked	0.000	-0.021	0.014	-0.004	-0.020*	-0.023**
5+ years ago	(0.003)	(0.014)	(0.012)	(0.003)	(0.012)	(0.011)
Annual Wages	-519.50	-456.60	3.98	-583.00	1097.00*	35.13
	(896.10)	(738.20)	(196.70)	(395.80)	(575.50)	(137.10)
ln(Annual Wages)	-0.003	0.104	0.081	0.002	0.104*	0.072
	(0.010)	(0.072)	(0.051)	(0.011)	(0.056)	(0.045)
Observations	1206000	72000	145000	1331000	124000	213000

Note: Each cell reports a separate regression discontinuity estimate, where the outcome of interest is labelled with the row title. Bandwidths were selected using the MSE optimal bandwidth selection method (Cattaneo et al., 2019). Heteroskedasticity-robust nearest neighbor standard errors in parentheses. Sample sizes are rounded following FSRDC disclosure avoidance requirements. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2875. (CBDRB-FY24-P2875-R10961).

CHAPTER 2

OPEN PAYMENTS AND OPIOID OVERDOSE MORTALITIES IN THE UNITED STATES

2.1 Introduction

The United States is experiencing an opioid crisis. The National Institute on Drug Abuse (NIDA) reports 80,816 drug overdose deaths involving opioids in 2021, a stark increase since 2010 and 2020 (CDC, 2022). While opioid-use issues have continued since ancient times (Halpern and Blistein, 2019), the current crisis is unique in several ways. The U.S. pharmaceutical industry plays a prominent role. Companies focused heavily on creating synthetic pain remedies that maximized relief and minimized threat of addiction to address the growing need to treat pain (Case and Deaton, 2020). Aggressive marketing followed regulatory approval, with campaigns often wrongfully characterizing the true benefits and risks of the technology (Van Zee, 2009). While lawsuits against companies alleged fraud (Balsamo and Richer, 2019; Mulvihill, 2019), opioid marketing is still an important feature of the US healthcare system.

I explore legal marketing of opioids to physicians. I first identify characteristics of these transactions. I explore the avenues through which these transfers occur and their temporal and geographic variation. Leveraging 2014-2022 transaction-level data, I find that while companies spend the most on speaker compensation, the number of gift, food, and entertainment transactions dominate, suggesting that person-to-person advertising is the preferred marketing tool. Using Manski bounds, I do not find definitive statistical evidence suggesting pharmaceutical companies target specific physicians or maintain transactional relationships (e.g., detailing) with physicians they pay. Over this period, I also see decreases in transactions related to agonists and moderate increases in those related to antagonists, indicating changing attitudes in pain management and overdose prevention.

I also explore how payments are associated with overdose deaths. I establish a theoretical model based on information asymmetries and principal-agent relationships to propose the mechanisms through which payments could translate into an increase in opioid prescriptions. I stress in this model that a payment offers a costless alternative to a research-informed prescription. This highlights that these legal payments are not kickbacks. With an increase in opioid prescribing, patients are at a higher risk to become dependent or misuse the substance, resulting in an increase in the opioid-related death rate. My econometric analysis shows evidence of an association between payments and deaths at the county level. The association becomes stronger when the variable of interest measures physicians paid rather than dollars spent, providing evidence suggesting the theoretical framework and the idea of a "reservation payment" accurately reflects reality. However, the sign of this association more often flips or becomes statistically insignificant for the fixed effects estimator compared to the pooled Ordinary Least Squares (OLS) estimates.

I contribute to the literature in several ways. Studies in the economics and medical literature explore physician behavior changes in response to new information (Sacarny et al., 2016) and to receiving gifts (Wazana, 2000; Orlowski and Wateska, 1992). However, I am not aware of an existing theoretical model that describes this relationship, as presented here. In economics, many studies highlight how legislation affected the distribution of opioids and their associated outcomes (Lyapustina et al., 2016; Meara et al., 2016), but none that I am aware of attempt to estimate a quantitative relationship between mortality and pharmaceutical marketing. While the present analysis shows associations, not causal estimates, this descriptive evidence provides motivation for further research.

Section 2.2 reviews health and economics literature pertaining to the supply and demand forces driving the opioid epidemic, as well as physician prescribing behavior in general. Section 2.3 constructs the economic model of how a physician chooses to prescribe when receiving payments, and how it may result in increased fatal overdoses. Section 2.4 describes the open payments data, death data, and other data used in this analysis. Section 2.5 reports the econometric tools I use to analyze the data, which includes physician-level Manski Bound estimation and county level linear regressions. Section 2.6 provides results, showing suggestive evidence that supports the hypothesis that these payments influence physicians and increase incidence of overdose deaths. Section 2.7

concludes by offering potential policy implications, future research suggestions, and a discussion of current policies that attempt to curb the epidemic.

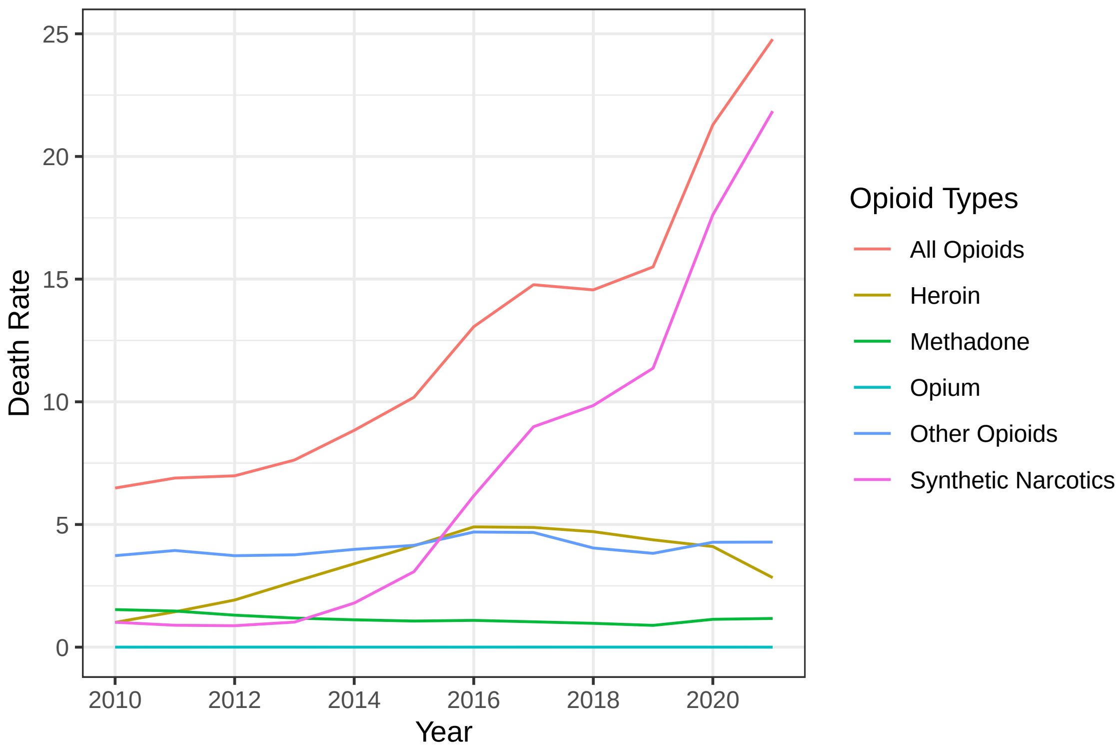
2.2 Literature Review

There is extensive work on the demand side of the opioid epidemic, focusing on why people are susceptible to opioid addiction and subsequent opioid overdose. A consensus among health economists is that the opioid epidemic is part of a wider suicide epidemic, spurred by a recent and rapid increase in morbidity and relatively harsher economic conditions for the working class and working poor (Case and Deaton, 2017; Autor et al., 2013; Ruhm, 2019). The stark change in morbidity and pain led to an increase in demand for pain management, while the overall issue of job loss and decrease in real wages lead to social pressures, increasing susceptibility to suicide (Case and Deaton, 2020). Overdose deaths can be considered an outcome of these two forces, where poor economic and health conditions combine with the availability of highly addictive substances. Figure 2.1 illustrates this trend: as supply and demand forces persist, the number of deaths per 100,000 people in the United States has more than tripled from 6.49 to 24.78 since 2010.

Research on the supply side of the US opioid epidemic focuses primarily on physician characteristics influencing prescribing behaviors. Schnell and Currie (2018) found that physicians educated in more competitive and higher ranked medical schools on average prescribe fewer opioid treatments than physicians educated at lower ranked institutions, a finding that is refuted in articles dealing with physician decisions in other spheres (Epstein and Nicholson, 2009). Doctor et al. (2018) observed through a randomized control treatment that opioid-prescribing physicians reduce their rate if directly notified after a patient opioid overdose death. However, when sent letters comparing their prescribing habits to their peers, overprescribing doctors make no significant change in their habits (Sacarny et al., 2016). Lucas et al. (2010) provide regional variation in physician practices.

Federal and local regulatory climates affect prescribing habits. Rutkow et al. (2015) use data from Florida's Prescription Drug Monitoring Program (PDMP) and pill mill laws to show that regulatory legislation can decrease both the prescribing rates of doctors and opioid use among

Figure 2.1 US Opioid Overdose Mortality Rate (per 100,000 people), 2010-2021



Source: United States Department of Health and Human Services (US DHHS), Centers for Disease Control and Prevention (CDC), National Center for Health Statistics (NCHS), Multiple Cause of Death 1999-2021 on CDC WONDER Online Database, released 2023. Data are compiled from data provided by the 57 vital statistics jurisdictions through the Vital Statistics Cooperative Program. Underlying population estimates are from the NVSS Bridged-Race Population estimates ending in 2020. Census population estimates are used for 2021.

individuals in Florida, albeit modestly.[1] The Texas pill mill law resulted in both practically and statistically significant decreases in not only number of prescriptions dispensed, but the volume and doses prescribed (Lyapustina et al., 2016). In a study involving treatments for acute myocardial infarction, there is evidence that local quality-of-care advisory boards run by opinion leaders and trusted medical professionals result in increased adoption of new medical devices and treatment strategies (Soumerai et al., 1999). Multiple external forces therefore influence physician behavior.

Before the Sunshine Act and CMS open payments records, several studies analyzed physician prescribing behavior after receiving transfers of value from companies. After all-expenses-paid trips to symposiums extolling the benefits of certain drugs, there was a national increase in the volume

[1]The term "pill mill" refers to "rogue pain management clinics where prescription drugs are inappropriately prescribed and dispensed" (Rutkow et al., 2015).

of prescriptions for the drugs, despite the majority of attendees interviewed stating the symposium would have no effect on their prescribing behaviors (Orlowski and Wateska, 1992). Researchers also found that physicians believe gifts are less effective in changing their prescribing behaviors as they receive more gifts (Wazana, 2000). Lack of data on financial relationships between drug companies and physicians has limited a more general understanding of the relationship between opioid abuse and physician payments.

Along with Florida and Texas, several other states passed legislation attempting to restrict opioid prescriptions. These laws often regulate pain management clinics, producing arrests and other consequences for doctors (Kennedy-Hendricks et al., 2016). While these legal restrictions curb opioid dispensing, Meara et al. (2016) found that the creation and implementation of these laws do not affect health outcomes such as nonfatal prescription-opioid overdose associated with disabled adults enrolled in Medicare.

While some physicians are affected by increased scrutiny of their prescribing habits, it is not clear whether this affects all physicians. The state laws seem to target high-risk practices. Moderate upticks in prescribing behavior are likely to go unnoticed by enforcement agencies. Importantly, opioid-related deaths continued at high rates even after the passage of pill mill laws. A small payment may engender a small change in prescriber behavior. A small change in prescriber behavior may generate an addict. Summing these changes may lead to detectable changes in opioid-related deaths.

While Open Payments data are now available, few studies explore payment effects on physician behavior. Several studies provide only descriptive statistics on the number of payments different type of physicians receive, such as neurologists, urologists, and pediatricians (Ahlawat and Narayanaswami, 2018; Bandari et al., 2016; Parikh et al., 2016; Marshall et al., 2016), but none provide econometric analysis.

2.3 Theoretical Framework

The theoretical framework involves three stage games describing mechanisms possibly connecting open payments with adverse health outcomes. The games include three agent types: the pharmaceutical company, the physician, and the patient. I assume that the pharmaceutical company is risk neutral and all other agents are risk averse. I assume the pharmaceutical company is risk neutral mainly because of the current regulatory environment in the United States. To bring medical technology to market, a company incurs large fixed-costs accumulated over years in the form of research and development (R&D) and approvals from agencies such as the Food and Drug Administration (FDA). After approval, the firm usually sustains relatively small marginal costs in producing the drug. Therefore, its main goal will be to maximize profits to break even as soon as possible. With this in mind, I can also assume that firms are myopic when they finally enter the market.

Another modeling consideration is physician income through prescribing. Most US physicians are not directly paid for prescribing a specific type of medication. Physicians only receive payments based on their prescribing behaviors if they offer a service that allows them to directly administer the drug, such as an in-house pharmacy. In the theoretical model and subsequent analysis, I consider all physicians as ones that do not receive any payments based on their prescribing habits outside of open payments.

2.3.1 Pharmaceutical Company-Physician Behavior

The first game involves the pharmaceutical company and physicians. The firm uses an open payment to induce the physicians to prescribe their drug at a higher rate than would occur without payment. As the pharmaceutical company does not know physician receptivity to an open payment, what follows is the canonical competitive market under incomplete information (Akerlof, 1970).

Let θ be physician type such that $\underline{\theta} \leq \theta \leq \bar{\theta}$ where $\underline{\theta}$ is the most easily influenced type of physician and $\bar{\theta}$ is the most difficult to influence. Let $r(\theta)$ be the reservation open payment of a physician with type θ, where $r'(\cdot) \geq 0$. The reservation open payment is a relatively odd concept as most physicians do not believe they change their habits based on receiving transfers from firms (Orlowski and Wateska, 1992; Wazana, 2000). Therefore, a reservation payment can be a subconscious switch: once a physician receives that level of payment, they begin thinking about the medication enough to prescribe it more frequently (Orlowski and Wateska, 1992; Wazana, 2000). Furthermore, the condition $r'(\cdot) \geq 0$ implies less susceptible physicians need a higher payment to flip that switch. Following the classical adverse selection model, the pharmaceutical company will choose open payment price p such that:

$$p = \mathbb{E}[\theta | r(\theta) \leq p] \tag{2.1}$$

By offering open payment p, the pharmaceutical company only successfully changes the behaviors of the most easy to influence doctors, namely $\forall \theta \leq \theta^*$ s.t. $r(\theta^*) = p$.

2.3.2 Physician-Patient Behavior

Once the pharmaceutical company successfully influences some physicians, I can model change in their prescribing strategies. Consider a principal-agent model (Holmström, 1979) where the patient is the principal who is contracting the physician to treat an ailment or illness through a hidden action. With opioid analgesics, I can think of severely painful ailments, such as cancers with breakthrough pain, and chronic pain from injuries. The patient does not know whether the physician was influenced by the open payment, hence the patient's expected utility is

$$\mathbb{E}[u(\theta)] = Prob(\theta \leq \theta^*)u(\theta \leq \theta^*) + Prob(\theta \geq \theta^*)u(\theta \geq \theta^*) \tag{2.2}$$

where the patient's preferences satisfy the necessary conditions for expected utility representation.

Now consider the physicians. Physicians are imperfect agents, where a patient cannot clearly see the processes or effort in reaching a treatment decision (moral hazard). For simplicity, I consider only two choices a physician can make: whether to adequately research medications that best treat a patient with a given ailment or to prescribe based on an open payment.[2] Let the physician's utility be expressed by $v(t, e, \theta)$ where $e \in 0, 1$ denotes the two actions they may make, with not researching medications expressed as $e = 0$. Additionally, t is the prescription drug they use for the patient. Both physician choice types involve selecting a drug to recommend, but the options are so numerous that the probability of choosing an effective drug without researching is zero. The "right" treatment after researching is denoted $\bar{t}$.

[2]Note this need not be a conscious decision by the physician.

If the physician was influenced by the payment, they are afforded the option of prescribing the marketed medication, t^*. This medication has a high chance of adequately treating the patient's ailment, even if it is not the optimal treatment $\bar{t}$. In short, if the physician was influenced,

$$v(t = t^*, e = 0, \theta) > v(t = \bar{t}, e = 1, \theta) > v(t = t, e = 0, \theta), \forall t \neq \bar{t} \tag{2.3}$$

and if they were not influenced,

$$v(t = \bar{t}, e = 1, \theta) > v(t = t, e = 0, \theta), \forall t \neq \bar{t} \tag{2.4}$$

This problem deviates from the conventional principal agent model by the patient's limited ability to implement a certain effort level from the physicians. In many cases, patients have insufficient knowledge to decide whether the prescribed remedy is optimal. There are exceptions to this case, such as patients who are medical professionals or who opt out of treatment after considering potential side effects. For the purposes of this analysis, I assume away this heterogeneity in patient types. I do not observe the patient-physician interactions in the data. Hence, I cannot observe whether and to what extent patients decline a remedy, which could affect dispensing of marketed drugs in equilibrium. As a result, I impose the assumption that patients are uninformed of the treatment's suitability for the ailments, and in turn have no actions that can effectively incentivize doctors of all types to conduct research. Therefore, in a setting unregulated by government intervention that could effectively penalize low-effort physicians, influenced doctors will always prescribe the marketed drug, uninfluenced doctors will always conduct research, and $Prob(\theta \leq \theta^*)$ will always be chosen to maximize the profits of the pharmaceutical company. The patient's expected payoff is effectively decided by the pharmaceutical company.

2.3.3 County-Level Outcomes

Patients seen by an influenced doctor may receive an opioid remedy they may not have received otherwise. A high-effort, uninfluenced doctor may have chosen a weaker, less addictive opioid or even a non-opioid regimen to treat their pain. As a result, a patient of an influenced doctor is likely more susceptible to addiction or misuse of the more addictive analgesic. Addiction and misuse is dangerous and can lead to fatal overdose. Therefore, these interactions could cause deaths.

As data on individual interactions are limited, I must observe their effects at a geographically aggregated level. Let Θ_t be an aggregated measure of open payments at time t. Choices for this measure could be

$$\Theta_t = \sum_{j=1}^{J} 1[p_j > r(\theta_j)] \tag{2.5}$$

$$\Theta_t = \sum_{j=1}^{J} p_j \tag{2.6}$$

$$\Theta_t = \sum_{j=1}^{J} 1[p_j > 0] \tag{2.7}$$

where the first measure is the number of physicians in a given area successfully influenced by the payment, the second measure is the aggregated amount of money transferred, and the third is the number of transfers in a given aggregation level. If an area has a higher number of influenced physicians, prescriptions for the target drug will increase based on the second stage. In the context of opioids agonists used for analgesics, the increase in prescribing can lead to adverse health outcomes, principally fatal overdose. Let fatal overdoses, y, be expressed as the function

$$y_t = f(\Theta_t, \Theta_{t-1}, ...) \tag{2.8}$$

where it is a function of contemporaneous aggregated payments and payments from previous time periods, along with other factors that may contribute to fatal overdose. My hypothesis can then be expressed as

$$\frac{\partial f(\cdot)}{\partial \Theta_t} > 0 \forall t \in 1, 2, ..., t \tag{2.9}$$

The profit and utility maximizing behavior of the pharmaceutical company and physicians in the given market structures lead to the externality of increased overdose deaths.

2.3.4 Other Considerations

In the above formulation, I remained agnostic on complexities that could add to model richness. Most of these details sort into two groups: (1) the determinants and rigidity of physician type and (2) market characteristics with the introduction of transportation costs. As previous work has shown, physicians prescribe differently based on characteristics such as where they attended medical school and access to learning networks. These features, along with others, may also contribute to determining a physician's type. For example, rural doctors located far from research hospitals may have more difficulty receiving information on burgeoning medical therapies outside of the promotions they receive from pharmaceutical companies. Additionally, a physician may also need "detailing," or additional payments to stay influenced over time (Worthington et al., 2017). On the other hand, physicians may also be less inclined to prescribe if they receive too many payments. Leon Festinger's ground-breaking psychology experiments in cognitive dissonance found subjects were less likely to sugarcoat a monotonous task if they were paid more to advertise it to incoming participants (Festinger and Carlsmith, 1959). A similar phenomenon could occur for physicians, even if subconsciously.

The second point refers to a company's decision to market. A pharmaceutical company may choose areas that have larger patient loads, or in the context of opioid analgesics, where chronic pain and injury are more common. Large patient loads could be a function of population, but also the number of jobs requiring repetitive motion or exposure to a dangerous environment, such as coal mining (National Safety Council, n.d.). If companies act in this way, endogeneity may become a larger problem in the estimation. Companies may also choose areas that are easier to reach in geographic terms. State regulatory environments regarding marketing of prescription drugs and other medical technologies may also provide local barriers to the market. However, the simplified model illustrates the main mechanisms I attempt to estimate empirically in the following sections.

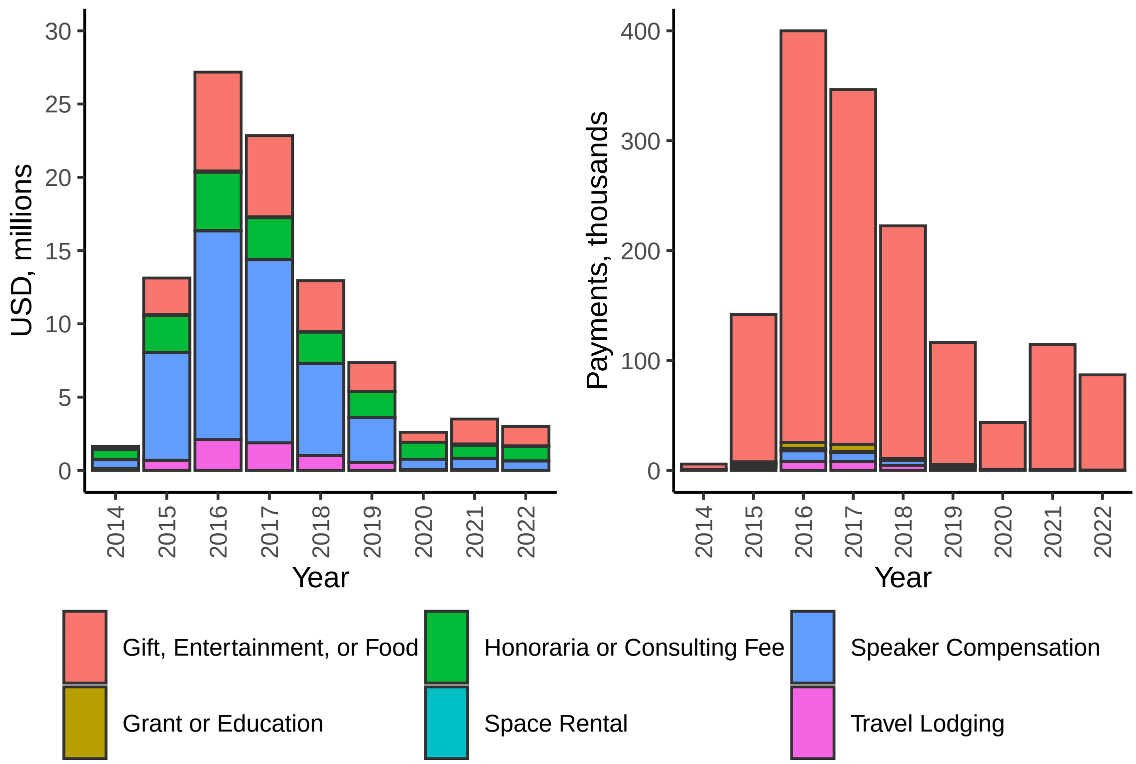
2.4 Data

2.4.1 Open Payments

Since 2013, the CMS provide annual data for open payments to inform the public of financial transactions from the health technology industry received by physicians and other medical professionals. Using the National Drug Code (NDC) database, I identify which general payments are associated with opioids (See Appendix Section 2A.1 for more information). Figure 2.2 provides an overview of the types of general payments for opioids from 2014 to 2022 using two key metrics: the number of payments and the total dollar value of payments. The first panel in the figure presents the payments in dollar terms. These payments primarily consist of transfers of value in the form of food, consulting fees, and compensation for acting as a speaker at an event. Of those three, speaker compensation changes the most dramatically over the period in terms of dollar value. In 2016, these types of payments accounted for over 50% of opioid transfers, but then shrank to only 20% in 2022. While explaining this trend is out of scope for this analysis, it seems to coincide with the increased scrutiny of opioid advertising from companies such as INSYS Therapeutics and Purdue Pharma (Balsamo and Richer, 2019; Mulvihill, 2019). Physicians may be less likely to publicly endorse opioid therapies amidst accusations of kickbacks and overprescribing. The second panel in Figure 2.2 presents the transactions not in dollar terms, but the number of actual transactions. While the "Gift, Entertainment, or Food" section contributed a large share of the dollar amount of payments, it dominates all other categories when it comes to the number of transactions. This shows that while other marketing strategies are used by pharmaceutical companies, direct marketing to physicians in the form of gifts and food is the most widely used.

Based on the NDC pharmaceutical classes, payments are assigned as associated with an opioid agonist, an opioid antagonist, or treatment that is an agonist/antagonist hybrid. This distinction is due to the different natures and effects of each type. Opioid agonists activate the opioid receptors in the brain that cause the addictive opioid effect, while antagonists block those receptors and prevent the opioid effect (Preedy, 2016). Drugs classified as both agonists and antagonists provide a full or partial opioid effect to a patient but inhibit the activation of the opioid receptors from

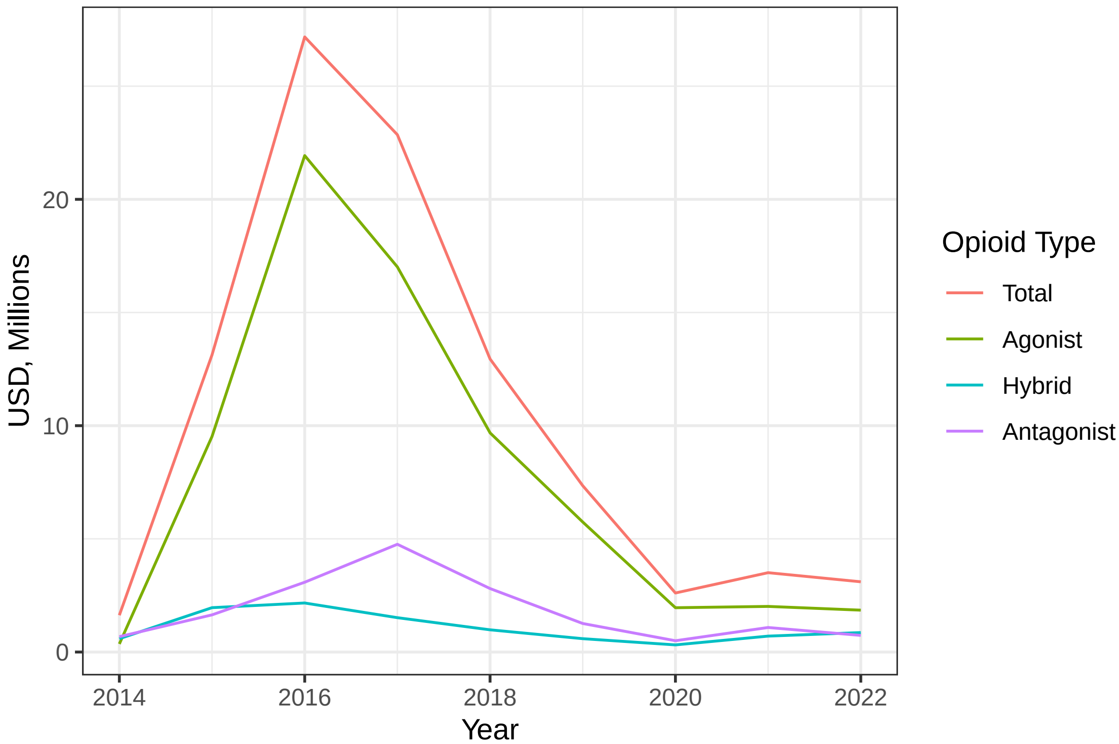
Figure 2.2 Opioid-related Transactions by Nature (Millions USD and Thousands Transfers)



Source: CMS Open Payments, 2014-2022.

other opioids or dosages during its effect time (Preedy, 2016). As illustrated in several studies, the increasing regional presence of opioid antagonists such as Naloxone may result in a decrease of opioid addiction and fatal overdoses (McDonald and Strang, 2016; Clark et al., 2014). Following from their intended physiological effects (Preedy, 2016), the hypothesized effect on the number of fatal overdoses in a county from payments associated with agonists would be the opposite of the hypothesized effect from antagonists. Hybrid drugs have both agonist and antagonist properties, so payments could cause no effect on county-level deaths. While most payments are associated with only one of the three opioid types, some are associated with multiple types. The amounts of those payments are equally divided among their associated opioid types. All payments over the analysis period are aggregated by zip code and then by county, using the centroid of the zip code to determine county. County aggregates are weighted by annual county populations estimates found in the National Vital Statistics System's Bridged-Race Population Estimates to calculate general payments per 100,000 people (National Center for Health Statistics, 2021).[3]

Figure 2.3 Opioid Payments to Physicians by Type of Drug (2014-2020)



Source: CMS Open Payments, 2014-2022. Payments are assigned as belonging to Agonists, Hybrids, or Antagonists based on the National Drug Codes associated with a payment. As a payment can be associated with up to five drug codes, a payment was split among both categories by dividing the payment.

Figure 2.3 displays the total dollar value of opioid-related payments by type. Agonists dominate the other two opioid classifications across all measures. This is to be expected; opioids that can be used as painkillers are more likely to be advertised, especially as treatment for pain has become more popular in the medical profession (Case and Deaton, 2020). Hybrid drugs are marketed the least across the period. Figure 2B.1 displays similar results for the other two measures.

[3]The National Vital Statistics System's Bridged-Race Population Estimates end in 2020. As the analysis includes data from 2021, I use Census population estimates from 2021 to incorporate the extra year. Using datasets from different sources presents its own challenges. Specifically, the Bridged-Race Population Estimates use a number of different sources to validate its estimate from data collected before 2020. In contrast, the Census population estimates for 2021 use only the 2020 decennial as its benchmark. Despite the generated discrepancies and limitations, the CDC recommends this strategy.

The existence of these value transfers and the magnitude of the transfers vary tremendously across the United States. To provide an example, Figure 2.4 displays the time-averaged (from 2014 to 2022) population-adjusted number of payments for agonists. The other types of drugs also display geographic variation (See Figure 2B.2 and Figure 2B.3). Counties with time averages of zero received no payments over the analysis period. When I compare these areas to metropolitan area proximity based on 2013 Rural-Urban Continuum Codes, it seems that while many of the heavily paid counties are areas where a metropolitan area is present, there are areas that are metropolitan-adjacent, micropolitan, or rural that have a high concentration of payments per capita.

These data have some limitations. Probably the most important limitation is due to disputed records. A recipient or manufacturer can delay the publication of their records up to four years if they feel there is a discrepancy with the reported value (Centers for Medicare & Medicaid Services, 2023). Therefore, there may be some noise in the measurement of the open payments (See Appendix 2A.1.2). For the purposes of this analysis, I take the open payments datasets as the true measures of these transfers.

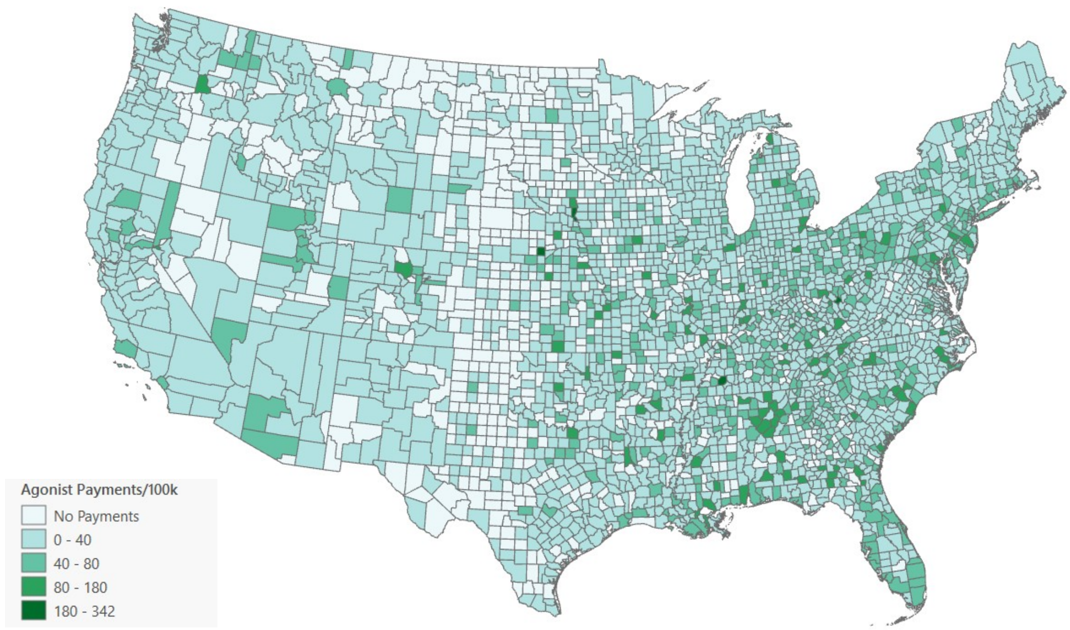
2.4.2 Multiple Cause of Death Files

For county-level analysis, I use the multiple cause of death files from the Centers for Disease Control and Prevention (CDC (National Center for Health Statistics, n.d.). These annual datasets include not only the underlying cause of death, but also up to 20 conditions the decedent had at the time of death. I focus on all deaths associated with poisoning by different types of opioids including heroin, methadone, and other synthetic opioids (codes T400-T404). Figure 2.1 depicts the mortality rate associated with these T-codes for the United States from 2010 to 2021. Most notable is the recent and dramatic increase in deaths associated with synthetic opioids; despite highly publicized legal actions, the opioid crisis is far from over.

2.4.3 Control Variables

Additional covariates follow Ruhm (2018). Covariates for unemployment rates, poverty rates, and median household income are from Local Area Unemployment (LAU) datasets from the Bureau of Labor Statistics (BLS). Poverty and median household income data are from the Census Bureau's

Figure 2.4 Time Averages of Agonist Payments per 100,000 people (Number of Transactions), 2014-2021



Source: CMS Open Payments, 2014-2021. Underlying population estimates are from the NVSS Bridged-Race Population estimates ending in 2020. Census population estimates are used for 2021.

Small Area Income and Poverty Estimates (SAIPE). Information regarding county racial, gender, and age composition are from the National Vital Statistics System's Bridged-Race Population Estimates. Additionally, estimates for county median household value and educational attainment are from the American Community Survey (ACS) Five-Year Estimates, using the end year as the measurement for a given year. The 2013 Rural Urban Continuum Codes (RUCC) were aggregated to urban, micropolitan, metro-adjacent, and rural and are included to control for county metro status. The final county-level dataset is a panel of 3108 counties over the period of 2014 to 2021. Table 2.1 presents summary statistics over the analysis period for the variables of interest.

Table 2.1 County-level Summary Statistics of Opioid Deaths and Opioid Payment Variables, United States 2014-2021

Variable	Mean	Std. Dev.
Total Opioid Deaths	10.40	15.60
Number of Physicians Receiving Payments	7.24	11.86
Total Value of Payments (USD)	2,088.95	65,893.55
Total Amount of Payments (Count)	27.47	61.65
Number of Physicians Receiving Agonist Payments	5.48	9.76
Total Value of Agonist Payments (USD)	1,715.25	60,920.14
Total Amount of Agonist Payments (Count)	19.51	47.29
Number of Physicians Receiving Hybrid Therapy Payments	0.81	2.46
Total Value of Hybrid Therapy Payments (USD)	334.40	28,204.18
Total Amount of Hybrid Therapy Payments (Count)	1.93	7.17
Number of Physicians Receiving Antagonist Payments	2.53	5.70
Total Value of Antagonist Payments (USD)	168.09	1,039.49
Total Amount of Antagonist Payments (Count)	6.50	20.73
Observations	24,864	

Note: All measures are per 100,000 people. Death measures come from US DHHS, CDC, NCHS, Multiple Cause of Death 1999-2021 on CDC WONDER Online Database, released 2023. Data are compiled from data provided by the 57 vital statistics jurisdictions through the Vital Statistics Cooperative Program. Payments measures are from CMS Open Payments, 2014-2022. Underlying population estimates are from the NVSS Bridged-Race Population estimates ending in 2020. Census population estimates are used for 2021.

2.5 Econometric Model

Empirically, I aim to provide evidence related to the stage games outlined in Section 2.3. While I am not aware of nationally representative data that could provide insights into the prescribing habits of physicians who receive payments from opioid companies (Stage 2), I employ econometric techniques to estimate the other two stages in the theoretical framework. For Stage 1, I use open payments data on the physician-level to provide information on payment habits between companies and physicians. For Stage 3, I aggregate to the county level to estimate a potential causal effect these payments may have on opioid overdose deaths.

2.5.1 Physician-level Behavior

I conduct several tests to explore the transactional relationship between pharmaceutical companies and physicians in the context of the theoretical model. Specifically, I try to understand (1) the "prices" or open payment amount offered to physicians; (2) whether there are sustained interactions between physicians and companies over time; and (3) how counties receiving payments differ on a set of observed characteristics from those that do not. Besides calculating features of the sample distribution and difference-in-means tests, I use Manski bound estimation. For a more in-depth explanation, please review Appendix Section 2A.2.

2.5.2 Open Payments on Deaths

The econometric model I use to estimate the effect of opioid-related payments on overdose mortalities is the following unobserved effects model:

$$OD_{it} = P_{it}\gamma + x_{it}\beta + c_i + u_{it} \tag{2.10}$$

where OD_{it} represents the population-adjusted overdose death rate for county i at time t, P_{it} is a vector of the population-adjusted payments associated with different opioid medications, and γ is the vector of associated coefficients. Then, x_{it} is a vector of control variables associated with county factors that contribute to overdose, and β is the vector of coefficients attributed to each control. The random variable c_i is the unobserved heterogeneity associated with each county, and u_{it} is the idiosyncratic error term.

I use two estimators. The first is a pooled OLS estimator with state and year dummies. While it is consistent under very weak exogeneity assumptions, POLS can suffer from inconsistency due to the incidental parameters problem, or more specifically, there is not enough data to accurately estimate the state dummies. Inconsistent estimates for the state dummies can make other estimates inconsistent, including the estimates for the payment variables. Therefore, I also test a fixed effects estimator with year dummies. This estimator is often called the two-way fixed effects (TWFE) estimator. While this estimator can also consistently estimate the parameters under weak assumptions (such as imposing no assumptions on the relationship between the unobserved heterogeneity and other random variables in the regression), it also suffers from some drawbacks. The first is exacerbation of measurement error. As the CMS does not collect all payments between pharmaceutical companies and medical professionals due to disputes, the estimates of the parameters could very well suffer from measurement error. Additionally, estimates from this technique are not the average partial effect, but rather a weighted average of individual partial effect for each county, where counties that have more variation in open payments variables have larger weights (Baum-Snow and Ferreira, 2015). So, this estimation technique will better measure the partial effect of counties receiving shocks of inconsistent marketing rates over the period rather than those with persistent and stable rates.

While studies have shown that 25%-50% of opioid users die twenty years after initial treatment for their addiction (Hser et al., 2015), I am not aware of any statistical information about the lifespan of an individual not subject to intervention. Therefore, it is difficult to say ex-ante the appropriate lag-length for the analysis, but one can argue that lags in this panel setting provide less-noisy evidence of a true statistical association than contemporaneous variables. Because the analysis period consists of only eight years, the primary analysis includes only one-year lags for the payment variables.[4] Because this analysis does not employ more sophisticated causal inference techniques, one should be cautious of these estimates. They should be treated more as observational associations rather than applying a stronger causality claim.

As elaborated in Section 2.3, I analyze several payment variables in linear models for different hypotheses. The first model focuses on the population-adjusted value of the open payments. This tests the hypothesis that more marketing money allocated to doctors in a county will increase prescribing, resulting in more overdoses from increased agonist prescribing or a reduction in overdoses from increased antagonist use. The second model uses the population-adjusted count of payments in a county to test if the number of payments is a more meaningful metric than

[4] Apeendix Section 2A.5 provides tests of other lag lengths.

population adjusted dollar value. A count variable may better show the presence and frequency of pharmaceutical payments across a county compared to a dollar value, as it removes some of the influence of physicians earning large open payments. The third model uses the population-adjusted number of physicians receiving payments in a county. Instead of focusing on the sheer number or dollar amounts of these payments, it may be better to focus on the number of influenced physicians potentially overprescribing opioids.

2.6 Results

2.6.1 Insights into Physician Behavior

Under the assumption that pharmaceutical companies have the means to calculate an optimal payment, p^*, in a market with incomplete information and price discrimination, I calculate the means and medians of the average value of the transfer for all physicians receiving open payments each year. Table 2.2 presents these results. Throughout the entire period, the median average payment is between \$13 and \$20 for all types of opioid medications. The means are much higher in some cases, showing that the distribution is skewed. The minimum and maximums also vary by year and type of medication. While these payments likely change among producers, the type of relationship producers have with a medical professional, and the nature of the transfer, I can say that the behavior of most producers supports the notion that they conclude a payment of around \$15 is enough to change the reservation beliefs of most physicians.

The nonparametric bounds estimation of receiving opioid related payment in the previous period on receiving payment in the current period gives us a set of potential treatment effects without imposing assumptions that could be easily violated if I were to estimate a linear model. Figure 2.5 provides a visual representation of the results from 2015 to 2022. The left pane displays the estimates of the probability model, and the right displays the results of the USD model. The bounds are quite large. In 2022, a doctor who received a payment in 2021 could be either 80 percent points more likely to receive a payment (at “best”) or 20 percentage points less likely (at “worst”) compared to counterparts who did not receive a payment the previous year. Regarding the difference in the total earnings from payments, the estimation set in 2016 is $[-\$101182, \$283411]$.

Table 2.2 Physician-level Features of Distribution of Average Opioid-Related Payments received by Pharmaceutical Companies, 2014-2022

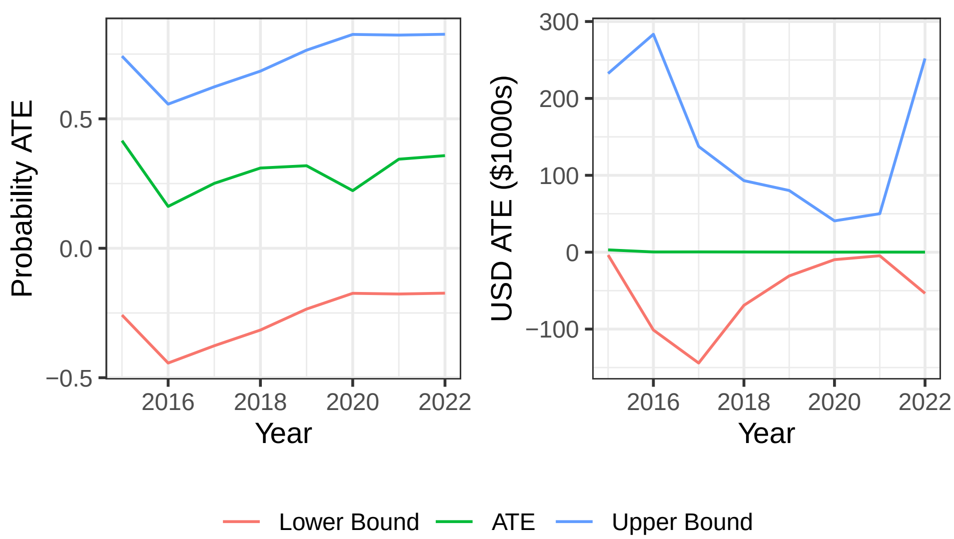
Year	Average Payment (USD)	Median	Mean	Std. Dev.	Min	Max
2014	All	17.45	138.56	424.38	4.66	3,255.15
	Agonist	18.06	73.27	270.68	4.66	3,255.15
	Antagonist	19.16	544.84	860.68	10.29	3,116.67
	Hybrid	16.81	58.96	137.11	10.00	2,317.57
2015	All	14.60	33.65	411.80	0.37	77,539.41
	Agonist	13.89	30.84	460.27	0.37	77,539.41
	Antagonist	17.75	37.88	137.75	0.92	7,250.00
	Hybrid	17.28	42.72	116.22	6.80	5,669.90
2016	All	14.99	30.18	137.23	0.49	17,551.26
	Agonist	14.75	28.96	130.08	0.49	17,551.26
	Antagonist	14.66	23.24	62.05	1.42	4,200.00
	Hybrid	16.93	43.42	223.62	4.25	17,481.53
2017	All	15.05	29.77	129.86	0.54	13,933.33
	Agonist	14.51	28.40	127.03	0.29	13,933.33
	Antagonist	15.59	24.76	73.27	0.65	3,000.00
	Hybrid	15.74	39.02	231.03	1.08	10,796.67
2018	All	14.46	30.13	143.37	0.12	16,986.25
	Agonist	14.56	32.02	113.95	0.12	8,925.00
	Antagonist	14.05	24.73	159.30	1.64	16,986.25
	Hybrid	15.19	23.66	64.48	1.70	1,894.92
2019	All	14.71	34.00	186.02	0.47	18,190.00
	Agonist	15.04	38.24	135.66	0.47	9,717.00
	Antagonist	14.77	30.95	241.98	1.20	18,190.00
	Hybrid	13.60	15.60	41.19	0.48	3,100.00
2020	All	14.84	41.06	270.09	0.24	15,000.00
	Agonist	15.70	53.69	315.92	0.24	15,000.00
	Antagonist	14.37	27.55	208.94	2.83	6,700.00
	Hybrid	14.37	15.10	5.16	0.81	130.91
2021	All	14.93	25.73	140.11	0.16	10,500.00
	Agonist	15.48	25.77	124.91	0.16	6,050.00
	Antagonist	14.49	25.42	149.84	1.17	10,500.00
	Hybrid	15.63	16.88	5.95	4.98	71.40
2022	All	15.66	25.08	506.98	0.55	76,350.00
	Agonist	16.63	28.03	626.92	0.55	76,350.00
	Antagonist	14.82	19.63	74.29	1.39	3,721.10
	Hybrid	16.62	17.12	3.61	8.72	41.23

Note: As physicians can earn multiple payments in a year, average payment is caluclated by the total value of all transfers the physician received divided by the sum of the individual payments they received in a given year. Source: CMS Payments 2014-2022.

This highlights that if I were to estimate the ATE using OLS (visualized by the green line), I would be at risk of providing incorrect estimates of these treatment effects until I have sufficient data on the characteristics of physicians outside of their open payment habits.

While the bounds on the treatment effect on the probability of receiving a payment is persistently large, it is important to note that the bounds converge for some time for the other model. By 2021, the bound is $[-\$4616, \$50084]$. The range of possible treatment effects shrinks to 54,700. There are many potential reasons for this convergence of possible effect magnitudes. One is increasing media exposure of these marketing practices and general discontent toward them. Litigation alone has removed big players in opioid marketing including physicians who received large payments to market to peers. Additionally, the COVID-19 epidemic may have limited the ability for pharmaceutical companies to advertise through their preferred methods requiring in-person interaction. This may be the reason underlying the spike in possible effect magnitudes in 2022.

Figure 2.5 Manski Bounds for Average Treatment Effects (ATE) for Physician "Detailing" Behavior



Note: Manski Bounds are calculated using the procedure outlined in Section 2A.2. The green line represents the estimate using OLS. The blue line and red line outline the upper and lower Manski bounds.

Finally, I explore physician characteristics using place-based features. Table 2B.1 presents the means of county-level aggregate variables between two groups of the dataset: counties that did not receive any opioid-related transfers in the analysis period (n=560) and counties that received at least one. I use robust difference-in-means tests to obtain t-statistics. The difference in means is statistically significant at conventional levels across all death related variables except for methadone deaths. This provides evidence that areas where physicians receive payments are also places where more people die of overdose across many different demographic groups. The only exception is the deaths of members of the American Indian and Alaska Native demographic; more die of overdose in places where payments are not being received. As shown in Table 2B.1, the two groups of counties differ in many other demographic characteristics.

2.6.2 Payments and Overdoses

The first county-level models only use one open payments variable that represents all opioid-related transfers. Table 2.3 presents the results of these models using both the fixed effects estimator and the pooled OLS estimator. Column 1 and Column 2 estimate the effect of the payments in thousands of dollars. The parameter estimates for contemporaneous payments are mixed, with the pooled OLS estimate being negative and statistically significant at conventional levels. A reason for this negative effect could be the nature of payments at higher dollar values. Recalling Figure 2.2, the type of payment that makes up most of the dollar amount is compensation as a speaker at events. If most transfers in a high earning county are associated with that type of payment, the effect on overdoses could be less impactful than counties with lower USD amounts but higher gift payments. However, the parameter estimates on the lag of this variable are positive and I can reject the null hypothesis that they have no effect on opioid overdoses at the 1% level. Moreso, the estimates are quite similar: 0.003 and 0.002 for the FE and POLS estimates, respectively. Holding all else equal, a $1,000 increase in the population-adjusted value of payments results in an average increase of around 0.003 population-adjusted overdose deaths. The whole sample standard deviation of this variable is around $65,000 (see Table 2.1), so a $10,000 increase is within reasonable bounds. This increase would lead to an average increase of 0.03 population-adjusted deaths.

The other two measures of payments also show statistically significant and positive estimates for the lagged variable using pooled OLS. The estimated effect of increasing the number of individual payments regardless of value by one unit is 0.001 and 0.017 for the fixed effects and POLS estimates, respectively. Using the slightly more conservative fixed effects estimate (which is not statistically significant at conventional levels), I see that an increase of 50 payments in a county increases the death rate by more than a quarter of a death. Using the physician parameter estimates in Column 5 and Column 6, marketing to ten more physicians in a county increases the death rate by approximately 0.99 if I believe the pooled OLS estimate is closer to the true parameter. However, the estimate using the fixed effects estimator is close in magnitude to the payments measure and not statistically significant.

Using the lagged pooled OLS estimates, I find evidence that the sheer value of the transfers is less important in deaths than the number of payments. Even more, I can say that the number of payments is less important than the total number of physicians receiving them in a county. This is evidence that the model of a "reservation belief" that changes prescribing habits could be more descriptive of the real world as opposed to a productivity model, where physicians who are paid more by companies are more productive in overprescribing.

However, the null lagged effects using the fixed effects estimator in Column 3 and Column 5 elicit some doubt on these estimates. As the fixed effects estimator removes unobserved heterogeneity at the county level, while the pooled estimates do not, poorly controlled endogeneity may be biasing the estimates up. For example, a county may be more profitable to target due to specific physicians in the area, rendering the estimates inconsistent. Alternatively, opioid policy is often passed at the state or federal levels, making the inclusion of state fixed effects potentially sufficient. If the measure is exogenous in the pooled OLS specification, it may be a sign that consistently high magnitudes of payments are more correlated with deaths rather than higher bouts of variation in counties.

Similarly, the changing sign and magnitude of the contemporaneous parameter estimates raises concerns. For the number of payments and number of physicians regressions, these estimates are negative for the TWFE and positive for pooled OLS. They are so large in magnitude that the combined effect is different in sign and statistically significant at conventional levels (displayed in the last two rows of Table 2.3). It is difficult to ascertain which estimate is closer to the true parameter. On the one hand, incidental parameters in the pooled estimates could bias the estimate of the variables of interest. On the other, removing variation at the county level with TWFE may be removing too much variation and exacerbating measurement error. The reduction of the level of payments throughout the analysis period may also attenuate the estimates.

The notion of a reservation belief and its relationship to the lagged estimates is further justified with a series of regressions that attempt to parse the effects of receiving payments relatively large in dollar value from payments lower in value. An average payment[5] is considered "big" or large in dollar terms if it is higher than the 75% quantile average payment over the analysis period. The 75% quantile payment is $18.50. Using this distinction among payments, Table 2B.2 presents the results of the regressions estimating the partial effects of the two types of payments. In terms of the sign and magnitude of the same-period variables of interest, they closely align with the results in Table 2.3. The contemporaneous estimates are often statistically significant across the regressions, where TWFE estimates are negative and large in magnitude, while pooled OLS estimates are positive and large in magnitude.

The relationship between the parameter estimates of the big and small variables change with each measurement of the value transfers. For the estimates related to the dollar value of the payments, the average partial effects of the small payments are much larger than the estimated effects for the big payments. Using an equality test between these two parameters (shown in the last row of Table 2B.2), I can reject the null hypothesis that these parameters are equal at the 1% level (Column 2). This result indicates that in dollar terms, small payments may influence doctors to overprescribe at higher rates than big payments, leading to a larger effect in dollar terms.

The results from estimating the linear models using number of payments and number of physicians in Column 3 through Column 6 of Table 2B.2 show additional support. In these four estimates, when testing the equality of the big payment coefficient against its small payment counterpart, I cannot reject the null that they are equal for all the lagged terms and contemporaneous terms. This evidence, along with the statistical evidence found in Column 1 and Column 2 supports the hypothesis that targeting physician's reservation beliefs is more descriptive of the real world than a productivity model of prescribing.

[5] As a single observation in the Open Payments dataset can represent multiple payments, an average payment in this case is calculated by the total value of all transfers divided by the number of the individual payments in an observation.

The second set of models attempts to parse out the effects of different type of opioid medications on overdoses. Opioid analgesics are associated more with agonists and hybrid medications, while opioid antagonists such as naloxone are associated with preventing overdose. Table 2B.3 presents these results. The average partial effects of agonists are very similar to the ones presented in Table 2.3. Estimates for antagonists are largely positive and statistically significant, which goes against my initial hypothesis. For example, a $1000 increase in antagonist payments results in an increase in the opioid death rate by approximately 0.3 on average (Column 2). This result may be driven by several different behavioral responses. At the industry level, antagonist marketing may target areas with high overdose deaths. At the patient level, individuals using opioids may choose riskier behaviors with the increased prevalence of life-saving antagonists. The estimate may be capturing a combination of these effects, leading to the positive association. The effect of hybrid medications is mixed across all estimated models. Appendix 2A.3 discusses robustness checks I conducted with analysis of the main results and Appendix 2A.4 presents heterogeneous effects across rurality and race.

2.7 Conclusions and Policy Implications

In 2021, the CDC reported that in the preceding 12 months, 80,816 people died of opioid overdose, up 15% from the year before (CDC, 2022). While there are many reasons for this increase, including an increase in overdose deaths due to recreational drugs such as marijuana being laced with synthetic opioids (National Public Radio, 2022), one cause is overprescribing of opioids for chronic pain. Since the early 1990s, pharmaceutical companies aggressively marketed the benefits of their opioid pain-relief drugs while minimizing the risks. Unfortunately, this strategy has been used by opioid innovators for centuries; inventions such as heroin and morphine had similar marketing treatments with terrible consequences (Halpern and Blistein, 2019).

The Sunshine Act created the Open Payments database to promote transparency of the financial relationships between medical technology companies and medical professionals. Using the data on transfers of value between opioid producers and physicians, I attempt to draw conclusions on

Table 2.3 Association between Opioid Deaths and Different Measures of Opioid-Related Payments

	(1) FE	(2) POLS	(3) FE	(4) POLS	(5) FE	(6) POLS
Value of Payments (Thousands, USD)	0.000	-0.001***				
	(0.001)	(0.000)				
Lag Value of Payments (Thousands, USD)	0.003***	0.002***				
	(0.000)	(0.001)				
Total Amount of Payments (Count)			-0.010***	0.010***		
			(0.002)	(0.003)		
Lag Total Amount of Payments (Count)			0.001	0.017***		
			(0.002)	(0.003)		
Number of Physicians Receiving Payments					-0.054***	0.064***
					(0.013)	(0.014)
Lag Number of Physicians Receiving Payments					0.002	0.099***
					(0.009)	(0.015)
Time Variant Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	No	Yes	No	Yes	No	Yes
Observations	21,756	21,756	21,756	21,756	21,756	21,756
R-squared	0.131	0.359	0.133	0.368	0.133	0.369
Number of Counties	3,108		3,108		3,108	
Cumulative Effect of Payments	0.003*	0.001**	-0.001***	0.027***	-0.052***	0.163***
	(0.001)	(0.001)	(0.003)	(0.006)	(0.017)	(0.024)

Note: Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Payment-related variables, as well as the dependent variable (opioid deaths) are per 100k people.

relationships between the two entities using incomplete information markets and principal agent problems as the framework to interpret the results. Then, I attempt to estimate the effect these transactions have on fatal overdoses in the contiguous United States. I have two key results. The first is that given the assumption that firms are maximizing profits in the framework, they appear to have found that the optimal reservation wage to target is between \$13 and \$20. It seemingly only takes the cost of a lunch to effectively change the habits of enough physicians to maximize profits. The second result has a similar flavor: The number of physicians receiving any type of payment has a larger average marginal effect on overdose deaths than the number of payments received in a county, and in turn a larger effect than the total value of the payments in dollar terms.

There are some limitations to the current analysis. The first is that I do not observe physician characteristics in the Open Payments database outside of their names, their profession, and where their main offices are located. However, this will change: recent legislation has made the National Provider Identifiers (NPI) available for the public-use Open Payments databases. With the NPIs, future work could merge datasets with extensive physician characteristics listed such as the AMA Physician Masterfile. The bounds estimation on the effect of receiving payments in prior periods on what a physician receives in a current period may tighten with the introduction of control variables and enhance inference in how a company may use price discrimination in different market areas and for different physicians based on gender, medical school, citizenship status, and other factors.

The second limitation is that the total effect of these payments across time switches in sign between the TWFE specification and the pooled OLS specification. While each of these estimation strategies have their unique drawbacks, it becomes difficult to ascertain the true effect of these payments with such stark differences. One may try to seek out better identification strategies through more data (that provides estimates in the Stage 2 game, for example) or through exploiting exogenous shocks to better estimate these effects. Models that take into consideration physician characteristics in an area and control better for time invariant demand and supply conditions using pooled OLS may more accurately estimate the effect of these payments.

The third limitation is in the inability to parse out the effect of antagonists on opioid overdose, likely due to endogeneity. A valid instrument would improve the analysis. If the data are available, one may also wish to transform the dependent variable into a fraction of fatal overdoses over an estimated number of total overdoses (both fatal and nonfatal) in each county. I may not properly see the effect of antagonists purely on fatal overdose counts, but I may be able to see a dip in the proportion of overdoses that result in death because of antagonist payments using the proposed strategy. Additionally, one could limit this analysis to include only counties that received agonist payments and use antagonist payments as a treatment with a potential outcomes framework inspired analysis. However, due to the long history of this type of marketing scheme, it is difficult to argue that physicians and counties were not treated before the collection of this data, complicating a justification to use this proposed framework.

In the context of prescribing rates, research may also take advantage of several different datasets released by CMS to explore these financial relationships and their effect on prescribing on a physician level rather than on a county level. Perhaps the most helpful dataset would be the Medicare Provider Utilization and Payment Data, which tracks the prescribing patterns of physicians to patients enrolled in the Medicare Part D program. Finkelstein et al. (2022) used this data to analyze addict migration to less regulated neighboring counties. However, the preliminary results indicate that future analysis may be promising and can help us better understand the effects of these financial relationships on the general population.

Additionally, the open payments database is relatively new. The early years of the database seem as if there may have been some startup problems initially. This study thus bears repetition and expansion as new annual datasets become available. The time series also limits the number of lags that can be included in the model. While a literature review did not reveal the average time from initial opioid prescription to death, it seems very likely that the period of time between the initial opioid prescription and overdose death is likely to be at least a year or two in most cases. While data availability is likely a prominent constraint, this line of research would be quite valuable to not only this analysis, but to the implementation of overdose mitigation strategies.

Finally, a comprehensive analysis on the network effects of marketing could provide more insights on its effect on prescribing rates and in turn overdose mortality. It is well-established both in this study (see Figure 2.2) and in the literature (Orlowski and Wateska, 1992) that speakers, symposia, and conferences are important parts of pharmaceutical marketing. However, it is unclear whether networking occurs through other channels. Do companies target specific physicians within a community to improve spillover effects? Do they ignore potential spillovers? One may be able to estimate geographic spillovers of payments using the office addresses listed in the Open Payments database. This could also improve the present study: I would be able to better estimate the spatial effects of payments without relying on potentially arbitrary county lines.

Overall, I provide a framework and statistical evidence illustrating mechanisms driving opioid marketing to physicians and how they affect health outcomes in the United States. Using the framework, I can categorize where policy interventions fall in the subgames and calculate effects. Recent disciplinary actions from government entities, such as government seizure of Purdue Pharma's business; the Massachusetts ban on physician gifts; and New York's ban on Allergan's opioid-selling are all interventions in the first stage game. While one can argue that removing this market would curb the opioid epidemic, it also removes a viable channel for physicians to learn about new therapies and medical technologies that could improve outcomes for patients. A pro-competition argument would be to augment the provider payments market by subsidizing new, non-opioid pain relief technologies to market to physicians. Physicians who are influenced by these payments could then have adequate or even full information on multiple therapies to administer to patients without conducting additional research.

Interventions in the second subgame are also becoming more popular. The CDC recently announced a revision of their guidelines to physicians on prescribing analgesics, emphasizing the need to prescribe opioids only as a last resort. This could add a constraint to the principal agent problem and induce physicians to research who would otherwise prescribe based on marketing. There is also a growing change in the way physicians treat patients away from a physician decision and towards a more collaborative environment between physicians and patients. If a patient visits a physician who encourages a collective treatment decision, educating the public more on the risks of addiction could let patients impose stronger incentive compatibility constraints that force physicians to conduct more research on other treatments. While these are all potential strategies, as more data become available more rigorous microeconomic analysis of the theoretical model could more completely assess each strategy's hypothesized equilibrium outcome.

BIBLIOGRAPHY

Adams, C. P. (2020). *Learning microeconometrics with R*. Chapman; Hall/CRC. https://doi.org/10.1201/9780429288333

Ahlawat, A., & Narayanaswami, P. (2018). Financial relationships between neurologists and industry: The 2015 open payments database. *Neurology*, *90*(23), 1063–1070. https://doi.org/10.1212/WNL.0000000000005657

Akerlof, G. A. (1970). The market for "lemons": Quality uncertainty and the market mechanism. *The Quarterly Journal of Economics*, *84*(3), 488–500. https://doi.org/10.2307/1879431

Anderson, K. O., Mendoza, T. R., Valero, V., Richman, S. P., Russell, C., Hurley, J., DeLeon, C., Washington, P., Palos, G., Payne, R., & Cleeland, C. S. (2000). Minority cancer patients and their providers. *Cancer*, *88*(8), 1929–1938.

Anderson, K. O., Richman, S. P., Hurley, J., Palos, G., Valero, V., Mendoza, T. R., Gning, I., & Cleeland, C. S. (2002). Cancer pain management among underserved minority outpatients. *Cancer*, *94*(8), 2295–2304. https://doi.org/10.1002/cncr.10414

Autor, D. H., Dorn, D., & Hanson, G. H. (2013). The China Syndrome: Local labor market effects of import competition in the United States. *American Economic Review*, *103*(6), 2121–2168. https://doi.org/10.1257/aer.103.6.2121

Balsa, A. I., & McGuire, T. G. (2001). Statistical discrimination in health care. *Journal of Health Economics*, *20*(6), 881–907. https://doi.org/10.1016/S0167-6296(01)00101-1

Balsamo, M., & Richer, A. D. (2019). Opioid maker agrees to pay $225m to settle federal probes. *Associated Press*. Retrieved March 29, 2024, from Factiva

Bandari, J., Turner, R. M., Jacobs, B. L., & Davies, B. J. (2016). Urology payments from industry in the Sunshine Act. *Urology Practice*, *3*(5), 332–337. https://doi.org/10.1016/j.urpr.2015.12.002

Baum-Snow, N., & Ferreira, F. (2015). Chapter 1 - Causal inference in urban and regional economics. In G. Duranton, J. V. Henderson, & W. C. Strange (Eds.), *Handbook of regional and urban economics* (pp. 3–68, Vol. 5). Elsevier. https://doi.org/10.1016/B978-0-444-59517-1.00001-5

Becker, G. S. (1971, August). *The economics of discrimination* (2. edition, Ed.). University of Chicago Press. Retrieved May 31, 2024, from https://press.uchicago.edu/ucp/books/book/chicago/E/bo22415931.html

Campbell, C. M., & Edwards, R. R. (2012). Ethnic differences in pain and pain management. *Pain Management*, *2*(3), 219–230. https://doi.org/10.2217/pmt.12.7

Case, A., & Deaton, A. (2017). Suicide, age, and well-being: An empirical investigation. In D. A. Wise (Ed.), *Insights in the economics of aging* (pp. 307–334). University of Chicago Press. https://doi.org/10.7208/chicago/9780226426709.001.0001

Case, A., & Deaton, A. (2020). *Deaths of despair and the future of capitalism* (first edition). Princeton University Press.

CDC. (2022, May 11). *U.S. overdose deaths in 2021 increased half as much as in 2020 - but are still up 15%* [National center for health statistics]. Retrieved June 20, 2024, from https://www.cdc.gov/nchs/pressroom/nchs_press_releases/2022/202205.htm

Centers for Medicare & Medicaid Services. (2023). Open payments data (2014-2022). https://openpaymentsdata.cms.gov/

Clark, A. K., Wilder, C. M., & Winstanley, E. L. (2014). A systematic review of community opioid overdose prevention and Naloxone distribution programs. *Journal of Addiction Medicine*, *8*(3), 153. https://doi.org/10.1097/ADM.0000000000000034

Doctor, J. N., Nguyen, A., Lev, R., Lucas, J., Knight, T., Zhao, H., & Menchine, M. (2018). Opioid prescribing decreases after learning of a patient's fatal overdose. *Science*, *361*(6402), 588–590. https://doi.org/10.1126/science.aat4595

Epstein, A. J., & Nicholson, S. (2009). The formation and evolution of physician treatment styles: An application to cesarean sections. *Journal of Health Economics*, *28*(6), 1126–1140. https://doi.org/10.1016/j.jhealeco.2009.08.003

Festinger, L., & Carlsmith, J. M. (1959). Cognitive consequences of forced compliance. *The Journal of Abnormal and Social Psychology*, *58*(2), 203–210. https://doi.org/10.1037/h0041593

Finkelstein, A., Gentzkow, M., Li, D., & Williams, H. L. (2022, September). What drives risky prescription opioid use? Evidence from migration. https://doi.org/10.3386/w30471

Halpern, J. H., & Blistein, D. (2019, August 13). *Opium: How an ancient flower shaped and poisoned our world*. Hachette Books.

Holmström, B. (1979). Moral hazard and observability. *The Bell Journal of Economics*, *10*(1), 74–91. https://doi.org/10.2307/3003320

Hser, Y.-I., Evans, E., Grella, C., Ling, W., & Anglin, D. (2015). Long-term course of opioid addiction. *Harvard Review of Psychiatry*, *23*(2), 76–89. https://doi.org/10.1097/HRP.0000000000000052

Kennedy-Hendricks, A., Richey, M., McGinty, E. E., Stuart, E. A., Barry, C. L., & Webster, D. W. (2016). Opioid overdose deaths and Florida's crackdown on pill mills. *American Journal of Public Health*, *106*(2), 291–297. https://doi.org/10.2105/AJPH.2015.302953

Lucas, F. L., Sirovich, B. E., Gallagher, P. M., Siewers, A. E., & Wennberg, D. E. (2010). Variation in cardiologists' propensity to test and treat: Is it associated with regional variation in utilization? *Circulation: Cardiovascular Quality and Outcomes*, *3*(3), 253–260. https://doi.org/10.1161/CIRCOUTCOMES.108.840009

Lyapustina, T., Rutkow, L., Chang, H.-Y., Daubresse, M., Ramji, A. F., Faul, M., Stuart, E. A., & Alexander, G. C. (2016). Effect of a "pill mill" law on opioid prescribing and utilization: The case of Texas. *Drug and Alcohol Dependence*, *159*, 190–197. https://doi.org/10.1016/j.drugalcdep.2015.12.025

Manski, C. F. (1995). *Identification problems in the social sciences*. Harvard University Press.

Marshall, D. C., Jackson, M. E., & Hattangadi-Gluth, J. A. (2016). Disclosure of industry payments to physicians. *Mayo Clinic Proceedings*, *91*(1), 84–96. https://doi.org/10.1016/j.mayocp.2015.10.016

McDonald, R., & Strang, J. (2016). Are take-home Naloxone programmes effective? Systematic review utilizing application of the Bradford Hill criteria. *Addiction*, *111*(7), 1177–1187. https://doi.org/10.1111/add.13326

Meara, E., Horwitz, J. R., Powell, W., McClelland, L., Zhou, W., O'Malley, A. J., & Morden, N. E. (2016). State legal restrictions and prescription-opioid use among disabled adults. *New England Journal of Medicine*, *375*(1), 44–53. https://doi.org/10.1056/NEJMsa1514387

Meghani, S. H., & Keane, A. (2007). Preference for analgesic treatment for cancer pain among African Americans. *Journal of Pain and Symptom Management*, *34*(2), 136–147. https://doi.org/10.1016/j.jpainsymman.2006.10.019

Mulvihill, G. (2019). Purdue Pharma files for bankruptcy as part of settlement. *The Denver Post*. Retrieved March 29, 2024, from https://www.denverpost.com/2019/09/15/purdue-pharma-files-for-bankruptcy/

National Center for Health Statistics. (n.d.). Detailed mortality – all counties (2010 - 2021) as compiled from data provided by the 57 vital statistics jurisdictions through the vital statistics cooperative program.

National Center for Health Statistics. (2021, September 22). Vintage 2020 postcensal estimates of the resident population of the United States (April 1, 2010, July 1, 2010-July 1, 2020), by year, county, single-year of age (0, 1, 2, .., 85 years and over), bridged race, Hispanic origin, and sex. Prepared under a collaborative arrangement with the U.S. Census Bureau. https://www.cdc.gov/nchs/nvss/bridged_race.htm

National Public Radio. (2022, March 9). *Colorado's officials are at odds over how to respond to spike in fentanyl overdoses* [NPR]. Retrieved June 21, 2024, from https://www.npr.org/2022/03/09/1085544576/colorados-officials-are-at-odds-over-how-to-respond-to-spike

National Safety Council. (n.d.). *Top work-related injury causes*. Retrieved May 4, 2023, from https://injuryfacts.nsc.org/work/work-overview/top-work-related-injury-causes/

Orlowski, J. P., & Wateska, L. (1992). The effects of pharmaceutical firm enticements on physician prescribing patterns. *Chest*, *102*(1), 270–273. https://doi.org/10.1378/chest.102.1.270

Parikh, K., Fleischman, W., & Agrawal, S. (2016). Industry relationships with pediatricians: Findings from the Open Payments Sunshine Act. *Pediatrics*, *137*(6), e20154440. https://doi.org/10.1542/peds.2015-4440

Preedy, V. R. (2016, March 25). *Neuropathology of drug addictions and substance misuse volume 2: Stimulants, club and dissociative drugs, hallucinogens, steroids, inhalants and international aspects* [Google-Books-ID: zu5eBwAAQBAJ]. Academic Press.

Ruhm, C. (2018). *Deaths of despair or drug problems?* (w24188). National Bureau of Economic Research. Cambridge, MA. https://doi.org/10.3386/w24188

Ruhm, C. J. (2019). Drivers of the fatal drug epidemic. *Journal of Health Economics*, *64*, 25–42. https://doi.org/10.1016/j.jhealeco.2019.01.001

Rutkow, L., Chang, H.-Y., Daubresse, M., Webster, D. W., Stuart, E. A., & Alexander, G. C. (2015). Effect of florida's prescription drug monitoring program and pill mill laws on opioid prescribing and use. *JAMA Internal Medicine*, *175*(10), 1642. https://doi.org/10.1001/jamainternmed.2015.3931

Sacarny, A., Yokum, D., Finkelstein, A., & Agrawal, S. (2016). Medicare letters to curb over-prescribing of controlled substances had no detectable effect on providers. *Health Affairs*, *35*(3), 471–479. https://doi.org/10.1377/hlthaff.2015.1025

Schnell, M., & Currie, J. (2018). Addressing the opioid epidemic: Is there a role for physician education? *American Journal of Health Economics*, *4*(3), 383–410. https://doi.org/10.1162/ajhe_a_00113

Soumerai, S. B., McLaughlin, T. J., Gurwitz, J. H., Guadagnoli, E., Hauptman, P. J., Borbas, C., Morris, N., McLaughlin, B., Gao, X., Willison, D. J., Asinger, R., & Gobel, F. (1999). Effect of local medical opinion leaders on quality of care for Acute Myocardial Infarction: A randomized controlled trial. *Survey of Anesthesiology*, *43*(5), 256–257. https://doi.org/10.1097/00132586-199910000-00010

Van Zee, A. (2009). The promotion and marketing of OxyContin: Commercial triumph, public health tragedy. *American Journal of Public Health*, *99*(2), 221–227. https://doi.org/10.2105/AJPH.2007.131714

Wazana, A. (2000). Physicians and the pharmaceutical industry: Is a gift ever just a gift?: *Obstetrical & Gynecological Survey*, *55*(8), 483–484. https://doi.org/10.1097/00006254-200008000-00012

Wooldridge, J. M. (2010). *Econometric analysis of cross section and panel data, second edition*. MIT Press.

Worthington, H. C., Cheng, L., Majumdar, S. R., Morgan, S. G., Raymond, C. B., Soumerai, S. B., & Law, M. R. (2017). The impact of a physician detailing and sampling program for generic Atorvastatin: An interrupted time series analysis. *Implementation Science*, *12*(1), 141. https://doi.org/10.1186/s13012-017-0671-z

APPENDIX 2A

ROBUSTNESS CHECKS

2A.1 About Open Payments

2A.1.1 Data Cleaning

Open Payments data are separated into three categories for each year: General Payments, Research Payments, and Physician Ownership data. General Payments are all transfers and payments not associated with a research agreement between a medical professional and a pharmaceutical or therapeutic entity. Each Open Payment is associated with at most five types of drugs, therapies, or medical technologies. For payments associated with drugs, most are assigned a National Drug Code (NDC) or National Drug Package Code. The FDA maintains the NDC Directory, which is a list of all drugs currently distributed commercially in the United States. The current iteration of the National Drug Code public database is incomplete. However, to avoid mischaracterization of payments, this analysis does not include any payments that are not associated with National Drug Codes, or payments that are not associated with drugs explicitly labelled as one of the thirty pharmaceutical classes associated with opioid medications in the current public NDC database.

2A.1.2 Collection and Reporting

Open Payments is a comprehensive database on marketing transactions for medical technologies. Many different entities engage in such transactions. To simplify this discussion on the collection and reporting process, I will group these agents into two overarching groups. The first are firms, which include manufacturers and Group Purchasing Organizations. The second are physicians, which include physicians, non-physician practitioners, and teaching hospitals. Most of this discussion comes from information found in the Open Payments codebook (Centers for Medicare & Medicaid Services, 2023).

The collection process begins with firms. Firms are required by the Sunshine Act to report payment and transfer data for most transactions.[1] Once these data are collected, Open Payments

[1] Transactions meant for patient-use, such as supplying product samples or teaching materials, are not collected from firms.

merges the transaction records to physicians using physician identifiers. Records that do not merge are returned to the firms for correction. Records that are not merged and corrected by the firms are not published in the Open Payments database.

Merged records are then sent to the physicians associated with them for review. Throughout a 45 day review window, physicians and firms work together to resolve any disputed records. If both entities agree on the changes during the review window, the disputed record will be corrected before publication. If they are not able to agree, the disputed record is still published with a dispute flag. Disputed records that are resolved outside the 45-day window are still published as is, but revised in future releases of the dataset. The only type of payment that can be delayed in publication are research payments.

The collection and reporting process seems to eliminate data limitations that would cause measurement error. As disputed general payment records are still released at publication, omitted records would not cause bias in the analysis presented. Small errors in individual records would only affect some of aspects of the analysis. For example, records that misreport the dollar amount associated with a payment would not alter the results using the other two payment measures, such as the number of physicians receiving payments in a county. Endemic misreporting by firms that are not resolved by physicians (either through neglect or collusion), seem to be unlikely. The current collection and reporting process seems to accurately represent the population of transactions.

2A.2 Physician Behavior Analysis

Pharmaceutical companies in the United States have been using payments to market to medical professionals since at least the early 1990s (Orlowski and Wateska, 1992). Therefore, most pharmaceutical companies can choose the correct open payment p such that their profits are maximized. Using features of the annual distribution of physicians, I seek to identify p, the optimal transfer of value that alters the behaviors of enough physicians to keep opioid companies profitable.

Additionally, I test other hypotheses related to changing physicians' beliefs using these value transfers. Namely, do most physicians need "detailing," or recurring payments to keep them educated and prescribing the advertised drug. To see this, I estimate the non-parametric model below:

$$\mathbb{E}[v_t|v_{t-1} = 1] - \mathbb{E}[v_t|v_{t-1} = 0] \tag{2A.1}$$

where $v_t \in 0, 1$ is an indicator variable on whether the physician received an opioid-related open payment at time t. The above equation can be simplified as:

$$Pr[v_t|v_{t-1} = 1] - Pr[v_t|v_{t-1} = 0] \tag{2A.2}$$

In essence, this is the difference in the probability of receiving a payment in time t conditional on whether the person received a payment in the previous year. If detailing is a common practice, I would expect the value of this expression to be positive. If companies are more interested in advertising to medical professionals they do not currently have relationships with, the expression would be negative. If companies diversify between old and new physicians perfectly, the expression would be equal to zero.

One may also be interested in how the dollar value of the transfers compare among those who have received payments in the previous year and those who did not. I estimate a similar non-parametric model as the one above:

$$\mathbb{E}[p_t|v_{t-1} = 1] - \mathbb{E}[p_t|v_{t-1} = 0] \tag{2A.3}$$

where p is the total amount in dollars that a physician received from opioid advertising at time t. If it is costly to keep physicians interested and influenced, this expression will be positive. If it costs more to influence a physician initially, the expression will likely be negative. If there are no startup costs to influencing a physician and the subsequent cost to keeping them interested is not increasing, this expression would be equal to zero.

A limitation to estimating this treatment effect is the sparse data available to use as controls for physician characteristics. The current Open Payments database has limited information on physician characteristics outside of the locations of their main offices, their areas of specialty, and the relationships they have with specific companies in the medical technology industry. Therefore, along with estimating the treatment effects above, I include bounds estimates. The bounds are estimated using the following strategy outlined in Manski (1995) and Adams (2020). The bounds for the effect of receiving a payment the period before on the probability of receiving a payment in the current period are

$$\begin{aligned}
&Pr[v_t = 1|v_{t-1} = 1]Pr[v_{t-1} = 1] + (1 - Pr[v_t = 1|v_{t-1} = 0]Pr[v_{t-1} = 1]) \\
&\geq Pr[v_t|v_{t-1} = 1] - Pr[v_t|v_{t-1} = 0] \geq \\
&(Pr[v_t = 1|v_{t-1} = 1] - 1)Pr[v_{t-1} = 1] - Pr[v_t = 1|v_{t-1} = 0]Pr[v_{t-1} = 0]
\end{aligned} \tag{2A.4}$$

and the bounds for the model regarding the total payment are

$$\begin{aligned}
&(\mathbb{E}[p_t|v_{t-1} = 1] - \tilde{p}_t)Pr[v_{t-1} = 1] + (\tilde{p}_t - \mathbb{E}[p_t = 1|v_{t-1} = 0]Pr[v_{t-1} = 0]) \\
&\geq \mathbb{E}[p_t|v_{t-1} = 1] - \mathbb{E}[p_t|v_{t-1} = 0] \geq \\
&(\mathbb{E}[p_t|v_{t-1} = 1] - \tilde{p}_t)Pr[v_{t-1} = 1] + (\tilde{p}_t - \mathbb{E}[p_t|v_{t-1} = 0]Pr[v_{t-1} = 0]
\end{aligned} \tag{2A.5}$$

These bounds provide worst case scenario estimates of the treatment effect of receiving an open payment in the previous year on the population of physicians receiving payments. Rather than imposing assumptions such as an exogeneity requirement that could be easily violated and lead to inconsistent but statistically significant results, I opt for bounds in this estimation until data on physician characteristics become available to link to the Open Payments datasets (See the conclusion section).

After aggregating the open payments dataset to the county level, I can compare the characteristics of counties that receive open payments against ones that do not throughout the entire analysis period. While this does not give us physician-specific characteristics, it does give us a sense of what type of place-based factors may affect a physician's susceptibility to advertising from companies advertising opioids.

2A.3 Robustness Checks

Before estimating the models using POLS and TWFE, I tested whether I should control for the unobserved heterogeneity in the models. I employ a Mundlak regression-based approach to test for this issue. The Mundlak approach uses only a simple linear modelling of the unobserved heterogeneity to test whether it is correlated with the regressors (Wooldridge, 2010, 328-334). The joint significance test of the time averages in the Mundlak approach provide evidence that the unobserved heterogeneity present in the models is correlated with the regressors. This is justification for controlling the heterogeneity using the fixed effects estimator or using the state-level dummies pooled OLS I employ in the analysis.

To ensure the validity of the main results, I employ two strategies to minimize the outliers in the analysis. The first is to remove outlier counties in terms of dollar values. Specifically, I remove Grand County Colorado, a county that received over $6 million dollars in population adjusted payments in 2016. Removing this county from the sample did not change the results in Table 2.3 regarding the number of payments and number of physicians regressions, but they did increase the estimates in the dollar value results. For example, in Column (2) in Table 2.3, the lagged effect of the dollar value term changed from 0.002 to 0.06. While this is a startling difference, it does not conflict with my main findings: If I were to pay 10 doctors around $10-$20, the predicted effect on deaths in a county would still be higher than a $10,000 increase in the dollar value of payments received in a county.

Another way I test for the robustness of the results is by constricting the sample only to payments related to Food, Entertainment, and Gifts. As Figure 2.2 suggests, all other payment types are high in value, but low in incidence across the analysis period. Using this check, I find similar results: the estimates in the payment and physicians regressions did not change in practical terms, but the results for the dollar value models changed dramatically. The estimates of the lagged term in the pooled OLS estimation are 0.12 and 1.35, respectively. The latter is statistically significant at the 1% level. The implications are striking: a $1000 increase in the dollar value of payments received from food and entertainment results in an increase of around one and a half deaths in the death rate. However, I still see the estimated effect on physicians is stronger. Increasing the number of physicians paid through open payments by thirteen yields a similar effect but uses on average at most \$260.

2A.4 Heterogeneity Across Rurality and Race

Table 2B.4 and Table 2B.5 estimate heterogeneity in the lagged partial effect of payments across rurality and race, respectively. Across rurality categories, the association of lag payment measures on opioid-related death rates is not significantly different from one another. In essence, the relationships between pharmaceutical companies, physicians, and patients do not change across geography in a way that would affect the association between deaths and payments.

Table 2B.5 shows heterogeneous associations across racial groups, specifically, white, black, Asian, and American Indian people. The association between payments and death rates is strongest for white people. The lagged effect for black people is almost six times smaller in magnitude compared to white people, but still statistically significant at conventional levels. The other racial groups have statistically insignificant results.

The underlying mechanisms that create this heterogeneity cannot be identified in this analysis. However, there are several possible explanations found in the literature on racial disparities in health care that may motivate these results. The first is racial discrimination present in the physician-patient relationship. While it is unclear whether this discrimination is taste-based (Becker, 1971), statistical (Balsa and McGuire, 2001), or a combination of the two, there is empirical evidence that suggests it exists in pain management. Racial and ethnic groups in the United States, particularly Black Americans and Hispanics, are less likely to receive opioid analgesics and more likely to receive smaller doses compared to whites after controlling for access issues (Campbell and Edwards, 2012; Anderson et al., 2000). As a result, non-white racial groups may inadvertently avoid increased prescribing due to pharmaceutical marketing, leading to the small and statistically insignificant point estimates.

Another explanation could be differences in pain management preferences and attitudes across patients. While there is evidence that suggests Black and American Indian patients are more sensitive to pain compared to white patients (Campbell and Edwards, 2012), there is parallel evidence that suggests they are more skeptical of pain medications. Some studies have found that Black and Hispanic cancer patients use pain medications less than prescribed, favor stoicism, and express concerns about addiction (Anderson et al., 2000; Anderson et al., 2002). Black patients also feel the severity of their pain is not believed by their physicians, and often face "demeaning litanies" on abuse potential when seeking analgesics (Meghani and Keane, 2007). These patient attitudes may exacerbate statistical discrimination: discouraged patients may not pursue pain remedies, which could change physician prescribing habits toward the ethnic group (Balsa and McGuire, 2001). These preferences and feedback loops could be the reason why the estimated associations are much smaller in magnitude among non-white ethnic groups.

2A.5 Using Multiple Lags

As mentioned in Section 2.5.2, the appropriate number and length of lags may be an obstacle in this analysis. Table 2B.6 presents the results of number of physicians receiving payments estimations using two, three, and six lags. As one adds more lags, the fixed effects estimation results (Columns 1, 3, and 5) trend towards a null cumulative effect of payments. However, pooled OLS estimates (Colums 2, 4 and 6) yield positive and statistically significant estimates over time. The cumulative effect using pooled OLS does increase over the number of lags even compared to the results in Table 2.3. When restricting all regressions to the same sample size (2020 and 2021), the pooled OLS results yield the same conclusions. The cumulative effect using the fixed effects estimator are statistically insignificant across all specifications with minor changes in sign and significance across the lag terms. While these effects are informative, they still suffer from the changing signs in the main results presented in this chapter.

APPENDIX 2B

ADDITIONAL TABLES AND FIGURES

Table 2B.1 Summary Statistics and T-tests, 2014 - 2022

	All	Received Payments	No Payments	T-Statistic
Total Opioid Deaths*	10.40	11.64	4.75	21.88
	(15.60)	(14.15)	(20.00)	
Opioid Deaths, Female*	3.68	4.07	1.88	19.49
	(5.85)	(5.46)	(7.07)	
Opioid Deaths, Male*	6.72	7.57	2.87	16.52
	(12.21)	(10.16)	(18.44)	
Opioid Deaths, White*	7.58	8.48	3.46	16.93
	(13.24)	(11.35)	(19.12)	
Opioid Deaths, Black*	0.53	0.62	0.14	15.56
	(2.40)	(2.53)	(1.66)	
Opioid Deaths, American Indigenous*	0.17	0.14	0.29	-3.30
	(1.60)	(1.12)	(2.91)	
Opioid Deaths, Asian*	0.02	0.03	0.01	8.04
	(0.19)	(0.19)	(0.16)	
Opioid Deaths caused by Opium*	0.00	0.00	-	2.77
	(0.06)	(0.07)	-	
Opioid Deaths caused by Heroin*	2.08	2.38	0.74	23.78
	(4.41)	(4.42)	(4.12)	
Opioid Deaths caused by other opioids*	3.90	4.25	2.34	6.98
	(9.38)	(5.92)	(18.04)	
Opioid Deaths caused by Methadone*	0.79	0.82	0.63	0.74
	(7.48)	(1.94)	(17.12)	
Opioid Deaths caused by Synthetic Therapies*	6.16	7.06	2.06	36.35
	(11.57)	(12.14)	(7.23)	
Median Household Value ($)	145,726.60	152,647.70	114,235.90	34.63
	(89,594.24)	(93,468.89)	(59,926.10)	

Table 2B.1 (cont'd)

	All	Received Payments	No Payments	T-Statistic
Unemployment Rate (%)	5.10	5.22	4.53	20.18
	(2.04)	(2.01)	(2.08)	
Poverty Rate (%)	15.31	15.30	15.36	-0.55
	(6.14)	(6.01)	(6.71)	
Educational attainment, Bachelor's degree or more (%)	21.44	22.00	18.88	25.17
	(9.43)	(9.82)	(6.90)	
Share of Population, Female (%)	49.89	50.09	48.99	25.33
	(2.22)	(2.03)	(2.75)	
Share of Population, Black (%)	9.91	10.65	6.56	17.85
	(14.64)	(14.74)	(13.69)	
Share of Population, Asian (%)	1.64	1.83	0.80	39.55
	(2.65)	(2.84)	(1.12)	
Share of Population, American Indigenous (%)	2.25	1.91	3.79	-11.12
	(7.00)	(5.71)	(10.99)	
Share of Population, Ages 0-14 (%)	18.30	18.28	18.40	-2.00
	(2.97)	(2.78)	(3.70)	
Share of Population, Ages 15-24 (%)	12.58	12.84	11.44	31.53
	(3.26)	(3.36)	(2.51)	
Share of Population, Ages 25-34 (%)	11.87	12.10	10.84	35.72
	(2.12)	(2.04)	(2.14)	
Share of Population, Ages 35-44 (%)	11.58	11.72	10.95	30.61
	(1.41)	(1.33)	(1.55)	
Share of Population, Ages 55-64 (%)	14.10	13.92	14.95	-27.73
	(2.03)	(1.90)	(2.33)	
Share of Population, Ages 65-74 (%)	10.94	10.72	11.95	-25.19
	(2.66)	(2.52)	(3.04)	
Share of Population, Ages 75+ (%)	8.09	7.80	9.44	-38.75
	(2.32)	(2.13)	(2.65)	
RUCC	2.61	2.44	3.37	-61.81
	(1.14)	(1.12)	(0.86)	

Table 2B.1 (cont'd)

	All	Received Payments	No Payments	T-Statistic
Observations	24864	20384	4480	

Note: Standard deviations in parentheses. *per 100,000 people. Payment-related variables, as well as the dependent variable (opioid deaths) are per 100k people.

Table 2B.2 Opioid Deaths and Size of Payments

	(1) FE	(2) POLS	(3) FE	(4) POLS	(5) FE	(6) POLS
Big Payments Value (Thousands, USD)	0.000	-0.001**				
	(0.001)	(0.000)				
Lag Big Payments Value (Thousands, USD)	0.003***	0.002***				
	(0.000)	(0.000)				
Small Payments Value (Thousands, USD)	-1.052***	1.157***				
	(0.233)	(0.311)				
Lag Small Payments Value (Thousands, USD)	0.105	1.764***				
	(0.184)	(0.331)				
Number of Big Payments			-0.018*	0.014		
			(0.011)	(0.013)		
Lag Number of Big Payments			0.012	0.031***		
			(0.008)	(0.010)		
Number of Small Payments			-0.009***	0.009**		
			(0.003)	(0.004)		
Lag Number of Small Payments			-0.001	0.015***		
			(0.002)	(0.004)		
Physicians Receiving Big Payments					-0.041*	0.082***
					(0.024)	(0.028)
Lag Physicians Receiving Big Payments					0.009	0.105***
					(0.020)	(0.028)
Physicians Receiving Small Payments					-0.051***	0.037***
					(0.017)	(0.014)
Lag Physicians Receiving Small Payments					0.000	0.070***
					(0.010)	(0.014)

Table 2B.2 (cont'd)

	(1) FE	(2) POLS	(3) FE	(4) POLS	(5) FE	(6) POLS
Time Variant Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	No	Yes	No	Yes	No	Yes
Observations	21,756	21,756	21,756	21,756	21,756	21,756
R-squared	0.133	0.367	0.133	0.368	0.133	0.369
Number of Counties	3,108		3,108		3,108	
Joint Significance Test, Contemporaneous	0.00	0.00	0.45	0.74	0.78	0.17
Joint Significance Test, Lagged	0.58	0.00	0.17	0.20	0.74	0.26

Note: Robust standard errors clustered at the county level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Payment and death variables are per 100k people.

Table 2B.3 Opioid Deaths and Different Therapy Payments

	(1) FE	(2) POLS	(3) FE	(4) POLS	(5) FE	(6) POLS
Agonist Payments Value (Thousands, USD)	-0.005	-0.003***				
	(0.004)	(0.001)				
Lag Agonist Payments Value (Thousands, USD)	0.004***	0.004***				
	(0.000)	(0.001)				
Antagonist Payments Value (Thousands, USD)	0.007	0.406***				
	(0.060)	(0.124)				
Lag Antagonist Payments Value (Thousands, USD)	0.012	0.327***				
	(0.039)	(0.115)				
Hybrid Payments Value (Thousands, USD)	-0.002	0.000				
	(0.003)	(0.000)				
Lag Hybrid Payments Value (Thousands, USD)	-0.019*	-0.017***				
	(0.010)	(0.002)				
Total Amount of Agonist Payments			-0.010***	0.009***		
			(0.003)	(0.003)		
Lag Total Amount of Agonist Payments			0.001	0.009***		
			(0.002)	(0.003)		
Total Amount of Antagonist Payments			-0.002	0.018**		
			(0.004)	(0.009)		
Lag Total Amount of Antagonist Payments			0.003	0.025***		
			(0.003)	(0.008)		
Total Amount of Hybrid Payments			-0.075***	0.001		
			(0.018)	(0.014)		
Lag Total Amount of Hybrid Payments			-0.011	0.092***		
			(0.018)	(0.025)		
Number of Physicians Receiving Agonist Payments					-0.039**	0.048***
					(0.016)	(0.014)
Lag Number of Physicians Receiving Agonist Payments					0.008	0.026*
					(0.011)	(0.014)

Table 2B.3 (cont'd)

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	POLS	FE	POLS	FE	POLS
Number of Physicians Receiving Antagonist Payments					-0.029	0.072***
					(0.018)	(0.023)
Lag Number of Physicians Receiving Antagonist Payments					0.015	0.119***
					(0.016)	(0.025)
Number of Physicians Receiving Hybrid Payments					-0.187***	0.051
					(0.041)	(0.039)
Lag Number of Physicians Receiving Hybrid Payments					-0.071	0.243***
					(0.043)	(0.062)
Time Variant Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	No	Yes	No	Yes	No	Yes
Observations	21,756	21,756	21,756	21,756	21,756	21,756
R-squared	0.131	0.361	0.135	0.369	0.134	0.370
Number of Counties	3,108		3,108		3,108	

Note: Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Payment and death variables are per 100k people.

Table 2B.4 Heterogenous Association of Payments and Opioid Deaths, Levels of Rurality

	(1) FE	(2) FE	(3) FE
Payment Value (Thousands, USD)	0.000		
	(0.001)		
Lag Payment Value (Thousands, USD)	0.013		
	(0.013)		
Micropolitan*Lag Payment Value (Thousands, USD)	-0.019		
	(0.019)		
Metro Adjacent*Lag Payment Value (Thousands, USD)	-0.069		
	(0.053)		
Rural*Lag Payment Value (Thousands, USD)	-0.010		
	(0.013)		
Number of Payments		-0.011***	
		(0.002)	
Lag Number of Payments		0.006*	
		(0.003)	
Micropolitan*Lag Number of Payments		-0.007*	
		(0.004)	
Metro Adjacent*Lag Number of Payments		-0.008**	
		(0.004)	
Rural*Lag Number of Payments		-0.008*	
		(0.004)	
Physicians receiving Payments			-0.055***
			(0.014)
Lag Physicians receiving Payments			0.028
			(0.017)
Micropolitan*Lag Physicians receiving Payments			-0.043**
			(0.020)
Metro Adjacent*Lag Physicians receiving Payments			-0.014
			(0.020)
Rural*Lag Physicians receiving Payments			-0.044*
			(0.023)
Time Variant Controls	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Observations	21,756	21,756	21,756
R-squared	0.131	0.133	0.133
Number of Counties	3,108	3,108	3,108
Joint Significance Test of Lag Terms	0.515	0.184	0.0901

Note: Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Payment-related variables, as well as the dependent variable (opioid deaths) are per 100k people.

Table 2B.5 Association of Payments and Race-Specific Opioid Deaths

	(1) White	(2) Black	(3) American Indigenous	(4) Asian
Number of Physicians	-0.027***	-0.010***	-0.001	0.000
	(0.010)	(0.002)	(0.001)	(0.000)
Lag Number of Physicians	0.061***	0.010***	0.001	0.000
	(0.009)	(0.002)	(0.002)	(0.000)
Time Variant Controls	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	21,756	21,756	21,756	21,756
R-squared	0.210	0.080	0.018	0.016
Number of Counties	3,108	3,108	3,108	3,108
Cumulative Effect	0.035***	0.000	0.001	0.000
	(0.013)	(0.002)	(0.002)	(0.000)

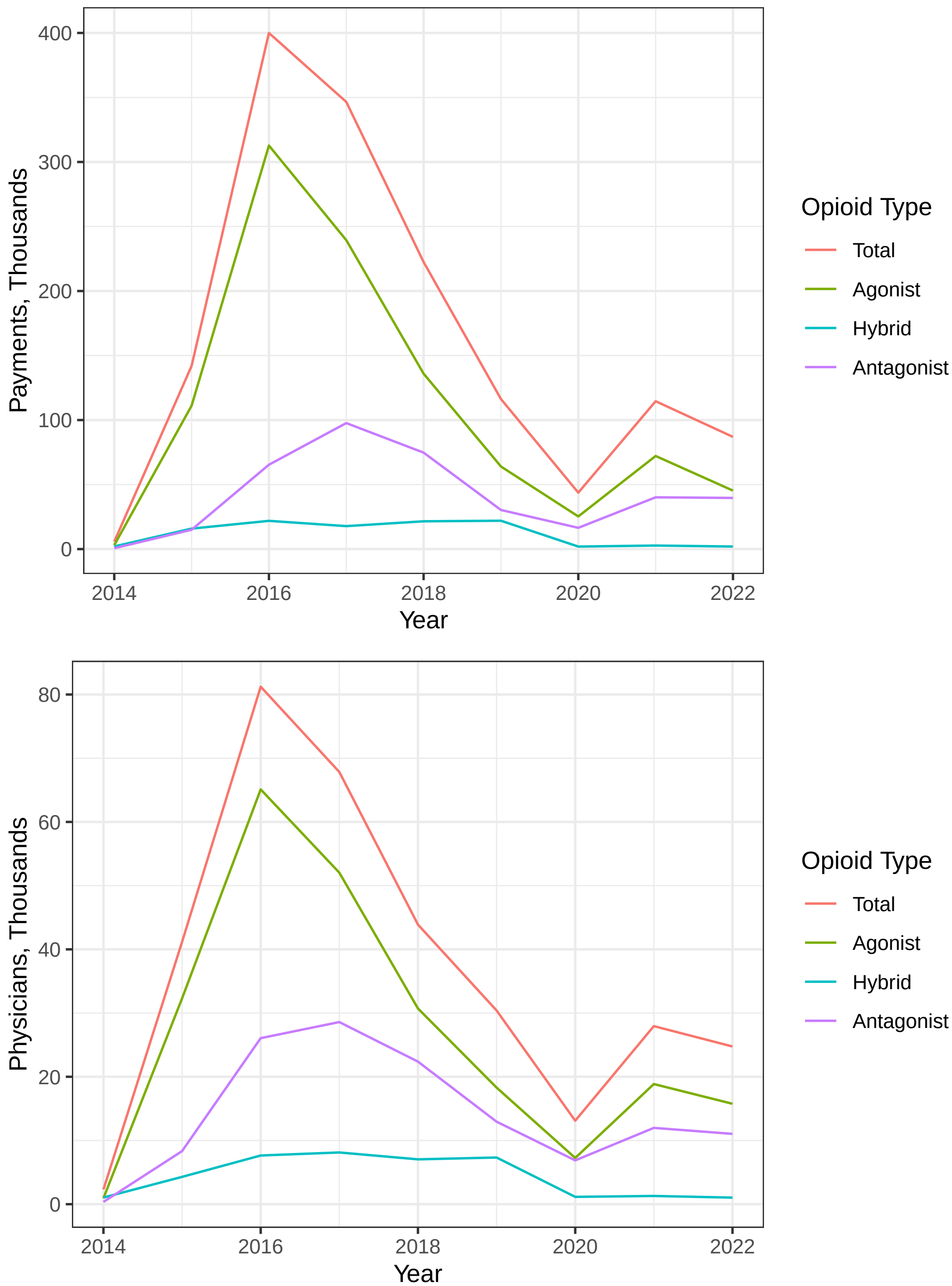
Note: Robust standard errors clustered at the county level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Payment-related variables, as well as the dependent variable (opioid deaths) are per 100k people.

Table 2B.6 Heterogenous Association of Payments and Opioid Deaths, Mutliple Lags

	(1) FE	(2) POLS	(3) FE	(4) POLS	(5) FE	(6) POLS
Number of Physicians	-0.052***	0.071***	-0.014	0.106***	0.091*	0.183***
	(0.015)	(0.014)	(0.015)	(0.019)	(0.055)	(0.051)
1st Lag Number of Physicians	-0.044***	0.017	-0.061***	0.004	0.088	0.154***
	(0.011)	(0.012)	(0.015)	(0.013)	(0.110)	(0.049)
2nd Lag Number of Physicians	-0.011	0.112***	-0.044***	0.021*	0.013	0.017
	(0.010)	(0.016)	(0.012)	(0.012)	(0.109)	(0.030)
3rd Lag Number of Physicians			0.002	0.129***	0.091	0.050**
			(0.012)	(0.016)	(0.113)	(0.024)
4th Lag Number of Physicians					0.069	0.000
					(0.118)	(0.018)
5th Lag Number of Physicians					0.101	0.085***
					(0.093)	(0.031)
6th Lag Number of Physicians					0.089	0.120***
					(0.094)	(0.030)
Time Variant Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,648	18,648	15,540	15,540	6,216	6,216
R-squared	0.138	0.386	0.151	0.400	0.086	0.448
Number of fips	3,108		3,108		3,108	
Cumulative Effect	-0.107***	0.200***	-0.117***	0.259***	0.542	0.609***
	(0.026)	(0.029)	(0.034)	(0.034)	(0.628)	(0.068)

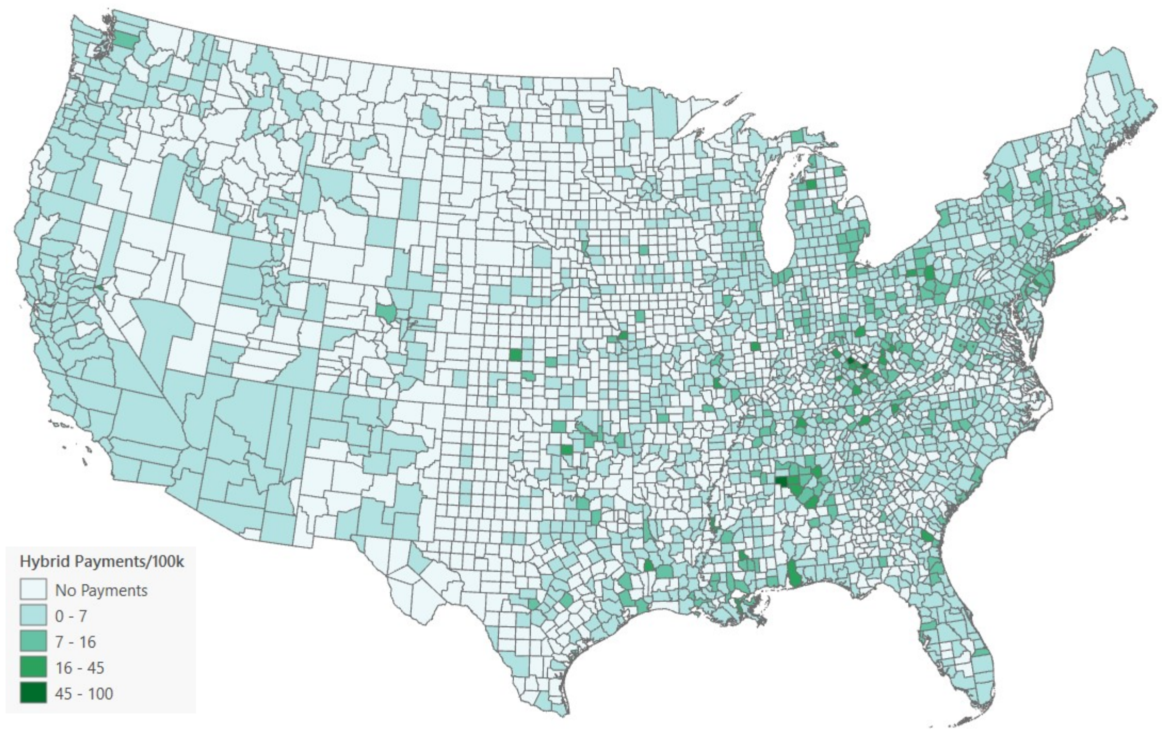
Note: Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Payment-related variables, as well as the dependent variable (opioid deaths) are per 100k people.

Figure 2B.1 Opioid Payments to Physicians by Type of Drug (2014-2020) Across Other Measures



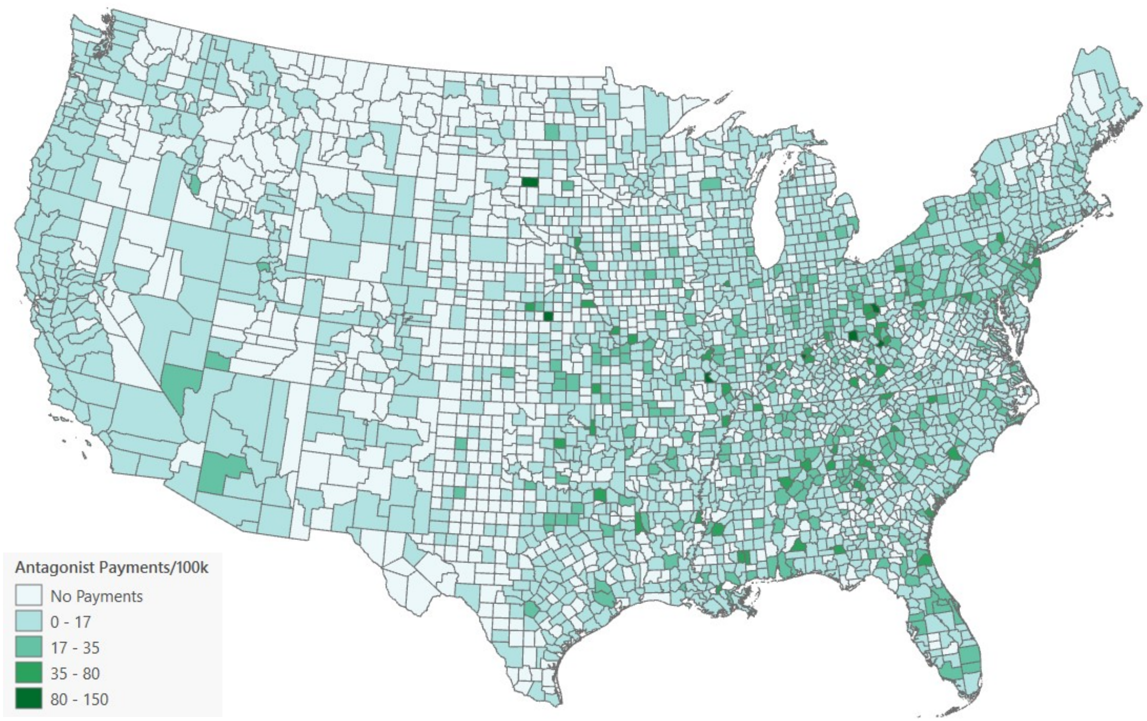
Source: CMS Open Payments, 2014-2022. Payments are assigned as belonging to Agonists, Hybrids, or Antagonists based on the National Drug Codes associated with a payment. As a payment can be associated with up to five drug codes, a payment was split among both categories by dividing the payment.

Figure 2B.2 Time Averages of Hybrid Therapy Payments per 100,000 people (Number of Transactions), 2014-2021



Source: CMS Open Payments, 2014-2021. Underlying population estimates are from the NVSS Bridged-Race Population estimates ending in 2020. Census population estimates are used for 2021.

Figure 2B.3 Time Averages of Antagonist Payments per 100,000 people (Number of Transactions), 2014-2021



Source: CMS Open Payments, 2014-2021. Underlying population estimates are from the NVSS Bridged-Race Population estimates ending in 2020. Census population estimates are used for 2021.

CHAPTER 3

BUSINESS ACTIVITY IN TRIBAL AREAS DURING THE SELF-DETERMINATION AND NATION-TO-NATION ERAS

3.1 Introduction

Since the arrival of European settlers in the fifteenth century, American Indians[1] have faced violence, conflict, and oppression. In the past century, the vestiges of this history take many forms, one being persistent socioeconomic disparities. While American Indian tribes and their sovereign lands vastly differ from one another, they suffer from persistent poverty with poor health outcomes on average. American Indians living on reservations (lands held in trust by the federal government on behalf of tribes), have per capita incomes less than half the U.S. average. This is lower than any other minoritized population, including off-reservation American Indians (Akee and Taylor, 2014). Tribal areas also fare worse on average than the rest of the United States on other metrics of well-being, such as COVID-19 hospitalization rates (Indian Health Service, n.d.), Type-2 diabetes diagnoses (Small-Rodriguez and Akee, 2021), and youth suicide rates (Livingston et al., 2019). To rectify these disparities, sovereign American Indian tribes use a plethora of economic development and relief strategies, ranging from U.S. government transfers to the creation of tribal economic development corporations.

Development outcomes vary across tribes, but many of the improvements in the past 50 years stem from a restructuring of United States-American Indian relations in the 1970s and 1980s. This restructuring, which took the form of U.S. Supreme Court decisions and federal legislation, gave tribes more autonomy in their ability to govern and implement policy. In essence, these institutional reforms recognize American Indian tribes as sovereign nations and allies to the United States. This is in contrast to the prior attitude that tribes are enemy states to conquer, and tribal members are foreigners to assimilate. This monumental change is termed the dawn of the Self-Determination Era (1968-2000) for American Indian tribes (National Congress of American Indians, 2020).

[1]Throughout this chapter, I refer to people indigenous to the lands currently occupied by the United States as American Indians. While there is disagreement on whether this label is appropriate (Bird, 1999), it is the terminology that is currently used by the United States federal government and the U.S. Census Bureau. As a result, most of the data in this analysis uses this label. When data and information allows, I will refer to American Indians by their tribe.

The Self-Determination Era brought a boon to tribes. One of the most prominent acts in the legislative patchwork was the Indian Gaming Regulation Act of 1988 (IGRA), which supports the right for tribes to run casino and gaming enterprises. With gaming as an engine for growth, tribes saw increases in per capita income (Conner and Franklin, 2019) as well as other measures of development including literacy rates (Akee et al., 2015). However, much of these profits are concentrated in select tribes: 12% of tribes earned about 65% of all casino profits in 2002 (Treuer, 2019). Tribes leverage other benefits granted through sovereignty to attract business and maintain competitive environments, including favorable tax environments and tax credits (Cowan, 2021; Bureau of Indian Affairs, n.d.). However, tribal areas still face issues. As reservation land is held in trust by the U.S. government for the benefit of the tribe, tribal members can face limited access to capital and land tenure disputes (Akee, 2009). Tribes can also face common hurdles in governance, such as corruption and mismanagement, along with the extra administrative burden of negotiating with entities such as the Bureau of Indian Affairs (BIA) that prevent or slow economic development.

Under these conditions, it is important to look at how businesses fare on tribal lands, as they can help aid communities in their development. This chapter serves to do just that: it is an observational study on business activity and entrepreneurship in tribal areas. Using several measures of business activity and industrial diversity that spans portions of the Self-Determination Era and the Nation-to-Nation Era (2000-Present), I offer a glimpse at the business environment across counties that contain tribal lands. The length of the panel allows for the exploration of dynamic firm and establishment changes. I use an array of econometric methods that provide robust statistical evidence for tribal business activity on average and also the heterogeneity of business activity across the unique tribal areas.

I find that over the analysis period, tribal counties have more firms on average than their non-tribal counterparts, both before and after adjusting for population, relevant covariates, and county and year fixed effects. This difference is stable across the entire period. When looking at the distribution of the size of establishments for a select year in the analysis period, I find that local economies in counties that contain tribal areas have larger shares of businesses between 1 and 19 employees at statistically significant levels, while nontribal counties have more non-employer firms. This suggests that tribal economies may be better suited for growth (Loveridge and Nizalov, 2007). It may also suggest that firms that survive the initial hurdles of starting up in a tribal area are more robust than their non-tribal counterparts (Akee et al., 2021). Using kernel density estimation, tribal counties have a higher probability of having more than 20 firms per 1,000 people than non-tribal counties.

In terms of industrial diversity, tribal counties on average have more employees in the Accommodations sector, which contains Casino Hotels (NAICS 72112), while non-tribal have more in manufacturing. This is likely due not only to the success of tribal casinos and tourism on tribal lands, but the structure of tribal-government run economic development corporations. Using the Herfindahl-Hirschman Index (HHI) as a dependent variable in the estimations, I observe that tribal counties become more concentrated in their employment over time. However, this estimate is small in magnitude, and kernel density estimates suggest that tribal counties are very similar to their non-tribal counterparts in industrial diversity.

This chapter contributes to a growing body of literature on the economic development of tribes in the United States. Much of the current literature in this subject has its roots in political science and anthropology, where researchers compared different tribes and their governments to identify institutions and policies that affect tribal development (Cornell and Kalt, 1995; Cornell et al., 2007; Ferguson et al., 1988). The findings of these qualitative studies helped formulate a newer, data-driven literature in the economics discipline, where researchers have looked at the effects of tribal and U.S. policies on a range of economic development measures, such as income, education, and housing values (Akee, 2020; Dippel, 2014; Frye and Parker, 2021; Feir and Gillezeau, 2023).

However, the quantitative literature on tribal business environment is relatively scant at the time of writing. Evidence from U.S. Census microdata suggests that businesses on American Indian reservations have higher survival rates, with better rates associated with casino operations and tourism during the Great Recession and subsequent recovery (Akee et al., 2021). Other studies find evidence that contradicts the notion that land-tenure institutions such as trust land designation are responsible for low levels of business investment, specifically after a modification of the policy in the 1950s (Akee, 2009; Akee and Jorgensen, 2014). Some studies highlight that distance from credit institutions might be the driving force behind credit access issues (Feir, 2022; LaPlante and Wheeler, 2024). Studies conducted by law scholars investigate and log the histories of State-Tribal Tax compacts, which were enacted to quell differences in tax laws that created price difference across US-tribal lines favorable to tribes (Cowan, 2021). I contribute to this literature by examining business activity over a longer period of time and using methodological tools to better acknowledge the heterogeneity of tribal development outcomes across the contiguous United States.

Additionally, this chapter falls on the tenth anniversary of Growth and Change's special issue on casinos, gambling, and economic development (Wenz, 2014a). This issue looked at many aspects of gambling's effect on local economies, including spatial competition (Walker and Nesbit, 2014; Gallagher, 2014), taxes and income (Hicks, 2014; Leal et al., 2014), and crime (Humphreys and Soebbing, 2014). Additionally, an article estimated the social impact of gaming in several global contexts, finding that many tribal economies have the ideal existing conditions for welfare improvements and economic development (Wenz, 2014b). While this chapter's primary goal is to offer an overview of tribal business activity, it shows that many tribal economies still are concentrated in the accommodations sector, the sector home to casino hotels. The special issue concluded with a look to the future, noting a concern that local (and perhaps tribal) economies might be susceptible to adverse outcomes due to increased competition from online gambling.

The chapter is structured as follows: Section 3.2 provides a review of the literature on tribal development and business activity, paying close attention to difference in entrepreneurial institutions. Section 3.3 offers a conceptual model and testable hypotheses regarding tribal business outcomes based on pertinent stylized facts from the literature. Section 3.4 describes the business activity data and measures to denote tribal counties used in the analysis. Section 3.5 provides the empirical framework and identification problems inherent with estimating tribal outcomes on average, and Section 3.6 presents county-level results. Section 3.7 concludes.

3.2 Literature Review

The issues surrounding economic development for American Indian tribes have roots in early colonial times. A history of war, forced relocation, genocide, and theft all contribute to the current socioeconomic status of indigenous people in the Americas (Dell, 2010; Dippel, 2014; Treuer, 2019; Feir and Gillezeau, 2023). While research continues to be conducted on those long term impacts, this section is devoted to American Indian development following the forced removal of tribes, the creation of reservations, and the government-backed destruction of the buffalo herds in the Great Plains. Specifically, I start this review section with the economic impacts of the Dawes Act of 1887 (also referred to as the General Allotment Act).[2] The second part of this section is devoted to prior literature on tribal business environments during the Self-Determination and Nation-to-Nation Eras.

3.2.1 From the Dawes Act to Self-Determination

Monumental U.S. federal policies shaped the trajectory of American Indian development. One of the most damaging of these policies was the Dawes Act of 1887. Through this act, property rights for reservation lands were transferred from communal holdings to individuals. This move contributes to beneficial development outcomes in contexts where enforceability of these rights are easily implementable. Specifically, it allows landholders to engage in credit markets more easily, improving economic growth (Bardhan and Udry, 1999 pp. 60-75). While strong U.S. land contracts and property rights provide this necessary condition for development, the Dawes Act demonstrates it is far from sufficient.

The underlying goals of the Dawes Act inhibited promising development outcomes. While some of the official intentions of the Dawes Act were to help native farmers and secure reservations as Indian land, it also progressed more sinister parts of the official agenda, such as breaking up tribes, assimilating tribal members into mainstream U.S. culture, and giving land to white settlers (Carlson, 1981). Through the Dawes Act, American Indian families and individuals received a set amount of land. In some cases, American Indians were disenfranchised from their land through the

[2] It is important to note that open discrimination and violence against American Indians persisted throughout this time. The Ghost Dance War and the Massacre at Wounded Knee in 1890 are grim examples of these tensions.

use of blood quantums (Treuer, 2019). The surplus reservation land was sold off to white settlers (Leahy and Wilson, 2008). Overall, the implementation of the Dawes Act ushered in a period of land dispossession.

By the mid-1930s, over 62%, roughly 90 million acres, of land controlled by American Indian tribes prior to the passing of the Dawes Act was transferred out of American Indian hands (Leahy and Wilson, 2008). These transfers were largely made up of foreclosures and intentional undervaluing of land by (American) Indian agents (Akee, 2020). Using a canonical two period difference-in-differences approach with two tribes, Akee (2020) estimates the effect of land loss from the Nelson Act (a Minnesota-specific manifestation of the Dawes Act) on several economic outcomes. For the Minnesota Anishanaabeg living on White Earth reservation, allotment led to increases in household size, decreases in assets, and decreases in tribal members engaged in agriculture (Akee, 2020). As productivity in agriculture is often a driver of structural transformation and development, American Indians lost not only wealth, but avenues towards development because of the Dawes Act.

Members of the U.S. federal government recognized that the Dawes Act was detrimental to American Indians. As a way to mitigate the damage, the U.S. Congress passed the Indian Reorganization Act of 1934 (IRA), which ended land allotments, returned surplus reservation lands to tribes, and aimed strengthen the tribal government institutions (Leahy and Wilson, 2008; The Editors of Encyclopaedia Britannica, n.d.). Tribes that agreed to participate in the governmental reforms were required to implement government structures proposed by the Bureau of Indian Affairs (BIA) in exchange for subsidies and other forms of funding from the U.S. federal government. While this policy attempted to take a more pluralistic approach to American Indian policies, it failed to be flexible in its implementation, forcing tribes to govern similar to the precontact social organization of the New Mexico Pueblos (Anderson, 1995). Tribes that participated in the IRA policies that did not have precontact governments similar to the BIA imposed schemes suffered from creation of weak institutions plagued by mismanagement and incentive issues between the governed tribal members and their leaders (Cornell and Kalt, 1995). Frye and Parker (2021) found that while tribes that prescribed to IRA policies were less volatile in long run economic conditions, they lagged in development compared to their tribal counterparts that did not adhere to it. This supports the notion that tribes that engage in nation-building practices have better economic outcomes, even when their self-governing policies are at odds with the U.S. government (Cornell and Kalt, 1992b).

Almost a decade after the passing of the IRA, the sentiments of U.S. legislators swung back to assimilation. Known as the Termination Period (1945-1968), the federal government terminated "federal recognition and assistance to more than 100 tribal nations" (National Congress of American Indians, 2020), ushered in policies that made tribal areas vulnerable to state and federal jurisdiction, and relocated tens of thousands of American Indians out of reservations to urban areas (University of Alaska Fairbanks, n.d.). These damaging policies provided the impetus for the Self-Determination Era. Through a variety of legal fights which include landmark wins such as Bryan v. Itasca County and the Menominee Restoration Act, American Indian tribes wrestled back sovereignty (Treuer, 2019).

Of the multiple Self-Determination Era wins, the one most often cited as a driver of development is the Indian Gaming Regulation Act of 1988. In the mid-1980s, many state legislatures and tribal governments were using charitable gaming to increase state revenues. However, many states believed tribal governments did not have the authority to conduct these practices. With the approval of the IGRA, tribal governments had complete authority over opening Class I and Class II gaming enterprises but were able to more effectively negotiation the terms of Class III gaming through state-tribal compacts (National Indian Gaming Commission, n.d.).[3]

Overall, the IGRA and other early Self-Determination Era actions have led to increased welfare among Indigenous people living on-reservation. In terms of the IGRA, 240 out of 356 reservations have gaming enterprises, making up around 92% of the population of on-reservation Native Americans (Akee and Taylor, 2014). The reservations that created casinos and other gaming ventures on average have improved incomes and other outcomes such as improved literacy (Akee et al., 2015). Conner and Franklin (2019) note that there is heterogeneity among outcomes related to per capita

[3] Class I refers to traditional forms of gaming for tribes. These forms of gaming are part of, or in connection with, tribal ceremonies or celebrations. Class II gaming involves less risky games, such as slots and bingo. Class III games include riskier games, such as poker and blackjack.

income due both to which gaming classes the tribes participate in and how the tribe decides to distribute the profits from the casinos. The authors found that there were no statistically significant differences in per capita income levels for reservations that did not participate in gaming and ones that participate in only Class I and Class II. They did however find that Class III tribes outperformed these groups in per capita income, with the difference for Class III tribes that directly distributed profits from casinos to the members of their tribe being higher in magnitude than Class III tribes that do not distribute the earnings. Therefore, tribal casinos and gaming have been an integral part of the American Indian sovereign nations development.

The echoes of this string of federal policies reverberate into current American Indian development outcomes. They can manifest in a lack of resources for tribes (Akee, 2020) and structural issues with governance and corruption (Champagne, 1992) despite the progress since the dawn of Self-Determination. This path dependency poses obstacles and assistance for tribal business activity and development.

3.2.2 Background on Business Environment in Tribal Areas

While the preceding section served as an overarching history of tribal economic development in the United States, this section serves to identify specific policies common to tribal communities that affect enterprise and entrepreneurship, especially following the dawn of the Self-Determination Era. It is important to acknowledge that tribes may experience some or all the constraints described here. This section will be organized similarly to a maximization problem: it begins with a discussion about tribal decisionmakers and their objective functions, then moves to input constraints including land, labor, and access to credit. The section ends with tribal, state, and federal government interventions that may alter business activity.

Perhaps the most important decisionmakers on reservations are tribal governments. As reservation land is held in trust by the U.S. federal government on behalf of tribes, tribal governments act as social planners, determining how best to distribute resources and economic activity. According to Cornell and Kalt (1992a), tribal governments make decisions that optimize three goals: economic well-being, political sovereignty (maintaining self-governance), and social sovereignty (control over sociocultural aspects of economic development). The weighting of these goals varies both across tribes and over time. As a result, tribes may decide collectively to implement a policy that favors an outcome that is seemingly at odds with another goal. For example, the Confederated Tribes of Warm Springs prevented nonnative developers from building a ski resort on their land (Cornell and Kalt, 1992a). The tribes restricted successful nonnative entrepreneurs from creating businesses that would yield large tribal revenues to preserve social and political sovereignty.

Assuming away globally common governmental issues such as corruption and mismanagement of funds (Champagne, 1992; Cornell et al., 2007), tribal governments then determine the optimal structure of their enterprise strategy based on the three overarching goals. Specifically, they decide who can own what types of businesses on tribal lands and how they are taxed. Many tribes create economic development corporations that operate businesses. This is mostly due to constraints set in federal laws and funding opportunities, such as the IGRA's stipulation that casinos must be operated by the tribe, not an entrepreneur (Cornell et al., 2007). As a result, a tribe's government acts both as a regulator and as a business entity.

Tribal governments then set up the rules and regulations for tribal-citizen and non-citizen entrepreneurship on their lands. These types of regulations and attitudes vary widely across reservations in terms of amenability to entrepreneurship. Some tribes create bureaucratic hurdles and use their development corporations to directly compete with small businesses run by their citizens (Cornell et al., 2007). Others promote entrepreneurship through "new wave" economic development policies, such as education programs for small business owners (Bartik, 1991; Cornell et al., 2007). Whether or not a tribe supports its entrepreneurial environment depends largely on its attitudes, institutions, and investment decisions.

Land ownership also dictates the ability to conduct business. As reservation land is held in trust by the U.S. federal government on behalf of tribes, tribes have overarching control over the land. However, fee-simple land exists within reservation boundaries as vestiges of the allotment era "checkerboarding" practice (Akee, 2009). This land can be owned by both individual tribe members and nonmembers. Unlike trust land, fee-simple land is also subject to local, state, and federal taxes, and can be used as collateral (Goetting and Ruppel, 2009). Depending on tribal policy, these swaths of land in and around Indian Country can be more attractive business environments for firms.

Outside of bureaucratic and government hurdles, tribal businesses experience barriers and benefits common to lagging regions. Rural areas suffer from thin labor markets (Moretti, 2011), inaccessibility to credit (Burgess and Pande, 2005), brain drain (Artz, 2003), and exclusion from other agglomeration benefits (Stephens et al., 2013). As many reservations are rural, they suffer from the same issues (Cornell et al., 2007). However, there are multiple ways the government attempts to remedy these issues, both for tribes specifically and lagging regions in general. Businesses in tribal and lagging regions can receive tax credits for those that they employ. Examples include the Empowerment Zone Employment Tax Credit, the Indian Employment Credit, and the Work Opportunity Credit, all of which offer tax credits for employing people from certain demographic groups in specific areas (Internal Revenue Service, n.d.).

In terms of credit and loan access, tribes and other disadvantaged areas in the United States may have access to Community Development Financial Institutions (CDFIs). CDFIs were created as part of the Community Reinvestment Act in the 1970s to improve access to financial services in all communities in the United States. In 1994, the federal government improved this effort with the creation of the U.S. Department of Treasury's CDFI Fund. As of 2024, 1,462 CDFIs are certified through the Fund, with 66 of them operated by and for tribal members (CDFI Fund, 2024). This includes Woodland Community Lenders (formerly Niijii Capital) in the Menominee Nation, which provides cash-flow loans to tribal entrepreneurs and a smaller suite of "new wave" services to nontribal members (Woodland Financial Partners, n.d.). Overall, tribal entrepreneurs may have some access to loans despite lack of collateral being a conventional barrier associated with reservation land.

As mentioned earlier, tribal entrepreneurs that have fee-simple ownership can use their land as collateral. They also have a second option: to put the land into trust. While they can put their land into trust on behalf of their entire tribe (reservation land), they can also put it into trust on behalf of themselves. There are benefits to putting land into trust, including tax credits, becoming immune to state and federal taxes, and lower leasing rates (Bureau of Indian Affairs, n.d.). Turning fee-simple land into trust land also ensures the land stays under the control of the tribe and/or its members.

Essentially, the tribal business environment varies tremendously between tribes. There are many opportunities and incentives to improve enterprise. However, tribes and their citizen entrepreneurs can be limited by idiosyncratic bureaucratic attitudes and decisions, along with other chronic issues facing lagging regions in general.

3.3 Conceptual Framework

Because (1) the development of the tribal sovereign nations is largely path-dependent due each nation's roots and interactions with U.S. policy; and (2) tribes have differing attitudes on entrepreneurship outside of the tribal corporation model, I expect to see several phenomena regarding firm dynamics in these areas. The first deals with general trends over time, and the second deals with heterogeneity among the sovereign nations. I attempt to observe these phenomena in three different aspects of firm dynamics: (1) total number of firms, (2) industrial diversity, and (3) establishment entry counts.

As the literature illustrates, American Indians have suffered from many development obstacles. The Self-Determination Era is recent compared to the more than five-hundred-year history tribes in North America have shared with European settlers. While there are still obstacles to development, this era has ushered in improvements in the economic development of sovereign nations (Akee and Taylor, 2014). These improvements will likely be expressed not only on the consumer side with higher per capita incomes and better living conditions, but also on the producer side. I expect that after the introduction of the Self-Determination Era in the 1960s and its first batch of sovereignty-related policies were enacted in the 1970s and 1980s, tribal areas will see an influx in business activity with increases in firm counts and establishment entry counts. In theory, I expect these developing nations to enter a transitional stage in their development. If they are similar enough to the United States, I may see firm counts and establishment entries converge to their non-tribal counterparts.

In terms of industrial diversity, I expect that on average, American Indian economies will be structured towards historically viable engines of economic well-being. As casinos, gaming businesses, and tourism have offered significant gains in business and welfare gains on reservations, I expect higher firm counts and employment in industries that complement the success of casinos. For example, economic sectors such as accommodations (which contains Casino Hotels), entertainment (which contains all other casinos and gambling industries), and services likely will have higher counts compared to nontribal counterparts. Other historically repressed sectors, such as agriculture (due to Dawes Act) and manufacturing (due to barriers to capital, either due to distance from credit institutions or previous trust land policies), will likely have lower counts. As a result, American Indian economies will be less industrially diverse compared to nontribal economies. Monopolistic competition from tribal corporations to tribal entrepreneurs may also suppress firm counts in conventionally viable industries. In terms of dynamics, I expect that with increasing sovereignty initiatives, American Indian economic diversity will increase and perhaps converge to non-tribal levels as time progresses.

Finally, I expect to see heterogeneity across tribal areas. Each tribal band has a unique development story, due in part to their pre-contact tribal government structure and relationship with U.S. foreign and domestic policy. Tribes also have idiosyncratic ideas of socially optimal allocations, manifested partly by differing relationships with their land (e.g. how much they use for hunting, how much land they have put into trust), their relationship with nontribal economies on the periphery of their nations, and to what extent capitalism and entrepreneurship is valued. As a result, I expect the tails of the distribution of firm and establishment counts to be fatter compared to nontribal counties. What some tribes find as economically viable may not be the same as others, leading to stark differences in industrial diversity. As a result, I expect to see more variance in the measures of business activity I use compared to nontribal counties.

In summary, I have three hypotheses related to American Indian businesses:

- H1 (Self-Determination and Entrepreneurship): Improved sovereignty efforts over time will lead to an increase in American Indian entrepreneurship on average. Measures may converge with non-tribal economies as time progresses.

- H2 (Path-dependent Industrial Diversity): American Indian economies will have higher employment in industries that have historically been important to growth. This will lead them to be less economically diverse. As time passes, diversity measures will converge with non-tribal economies.

- H3 (Heterogeneity across tribal lands): As tribes have different attitudes and challenges in economic development, distributions of business activity and industrial diversity will have fatter tails compared to nontribal business distributions.

3.4 Data and Graphical Analysis

This section provides an overview of the data used to test the hypotheses. I begin this section with how I choose to define a tribal county in the analysis. After creating this classification, I then introduce the data used to measure business and firm activity throughout the various analysis periods along with additional covariates used in the statistical analysis. I present figures to illustrate trends in tribal areas located in the contiguous United States throughout.

3.4.1 Tribal Counties

Tribal counties were identified using tract-based data obtained from the IPUMS National Historical GIS database (Manson et al., 2023). I define tribal tracts as those containing current reservation or off-reservation trust land for the contiguous United States because these lands correspond to geographic areas over which American Indian tribes have primary governmental authority (Bureau of Indian Affairs, n.d.). This means I exclude the tribal tracts composed of the following types of tribal subdivisions: Oklahoma Tribal Statistical Areas (OTSAs), State Designated Tribal Statistical Areas (SDTSAs), and Tribal Designated Statistical Areas (TDSAs).

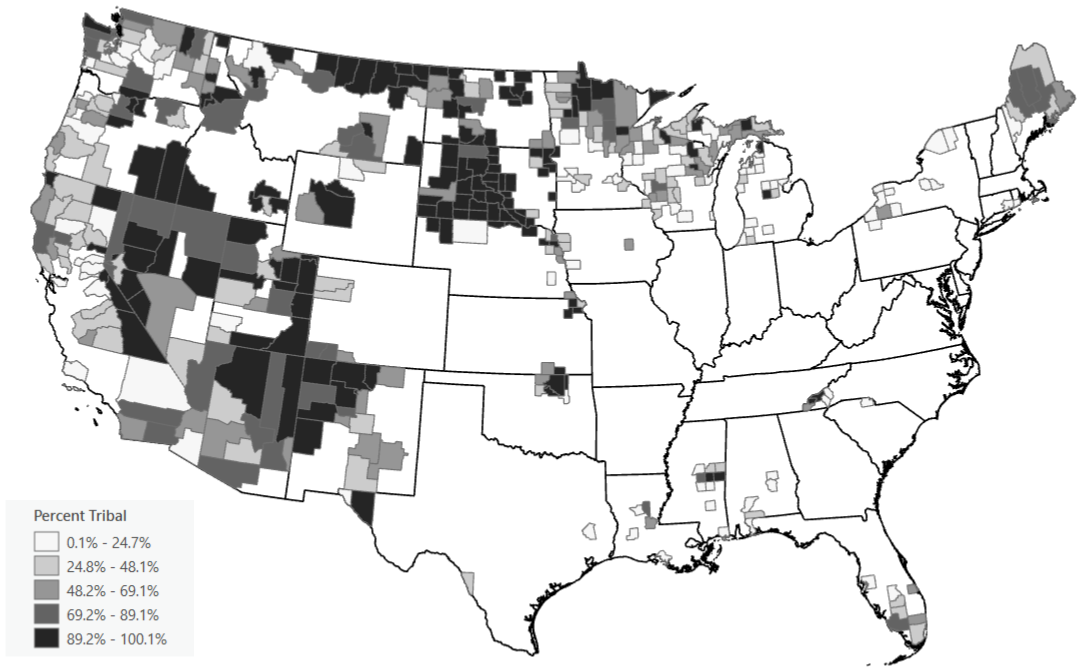
While these areas contain large populations of state and federally recognized tribes, they are not areas governed by tribal governments.

As implied above, a given county may have no tribal land, some tribal land, or be entirely composed of tribal land. For descriptive purposes, I create an indicator measure of tribal influence in each county by estimating the percentage of land in a tribal county covered by reservation and off-reservation trust land. First, I compute the total square miles in each tribal county covered by 1,489 "tribal tracts," or census tracts that contain reservation and trust land. Then, the sum of square miles covered by these census tracts in each county is divided by the total area of the county to make a proportion. Figure 3.1 illustrates the percentage of land classified as tribal within counties across the contiguous United States. Approximately 25% of the 437 counties that contain tribal lands are entirely tribal, and around 33% are at least 89.2% tribal land. These counties, as displayed in Figure 3.1, are primarily clustered in South Dakota, the northern edge of Montana, and in Southwestern states such as Arizona, Nevada, New Mexico, and Utah. States with the highest number of tribal counties with less than 24.7% of their area devoted to tribal lands are found in Michigan (16), Wisconsin (12), New York (9) and California (9). Large clusters of tribal counties are found in the west and north of the United States with only small pockets of tribal counties in the east and southeast, largely due to the forced relocation of eastern American Indian tribes in the 1800s.

3.4.2 Business Dynamics Statistics (1978-2019)

Business Dynamics Statistics (BDS), provided by the U.S. Census Bureau, is a collection of longitudinal datasets that provide information on businesses at a variety of aggregations (Census Bureau, 2021). The datasets contain information on firm, establishment, and job dynamics, such as firm deaths, establishment births and deaths, and job creation rates. This analysis uses three datasets at the county level, the smallest level of geographic aggregation available publicly. All measures are normalized to per 1000 people using population data from the Bureau of Economic Analysis's county population estimates. As with any research using public data to analyze specific

Figure 3.1 Percentage Area of Land Classified as Tribal Land for U.S. Counties

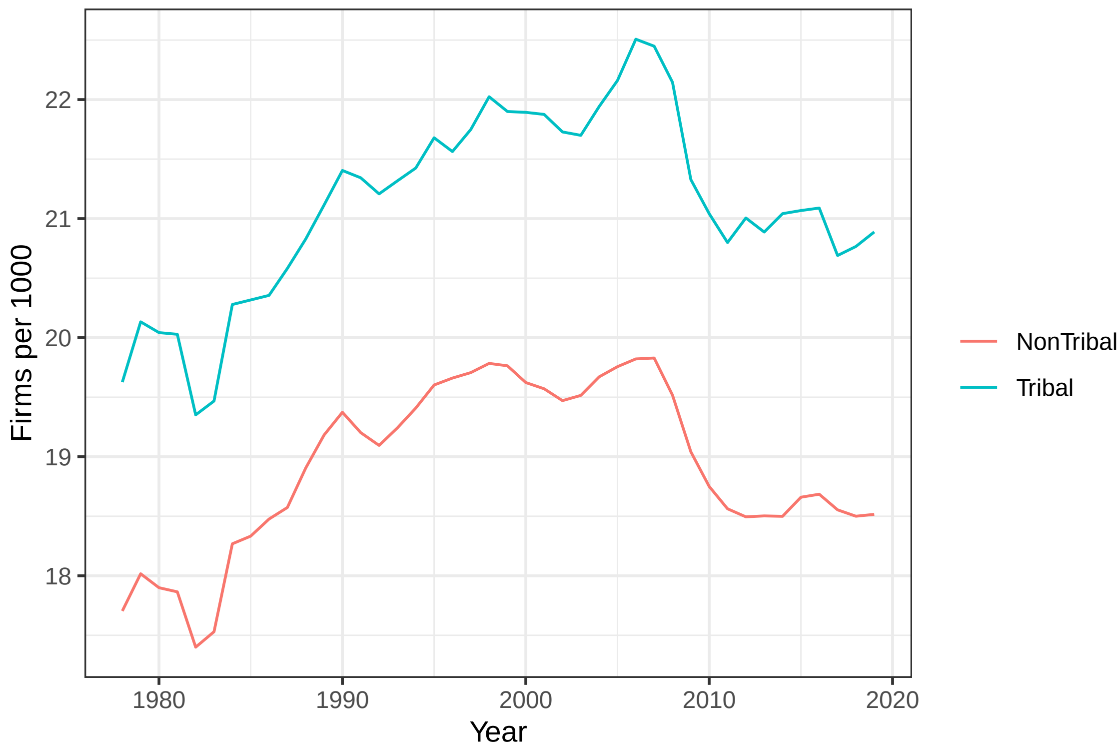


Source: 2020 Census Tribal Tracts. Percent Tribal was calculated using the procedure outline in Section 3.4.1.

populations, data suppression can be an issue (Carpenter et al., 2022). No more than 6% of the counties in each tribal-non-tribal designation are removed from the analysis at any given year.

Figure 3.2 captures the mean firms per 1000 for the two groups of counties determined by tribal assignment from 1978 to 2019. Over the entire period, tribal counties on average have approximately 2.2 more firms per thousand population compared to their nontribal counterparts. Shortly after the start of the analysis period, both tribal and nontribal counties reach the lowest average firm counts in 1982 with 19.5 and 17.4 firms per 1000 people, respectively. Then, both county types experience a steady increase until about 1990 where firm counts become more volatile but continue to increase. In 2006 and 2007, both types reach their highest firm counts with 22.5 for tribal counties and 19.8 for nontribal counties, but then dramatically decline until 2012. After 2012, the average firm counts for both types then become steady. As both county distinctions follow the same trends over time, there is evidence that nationwide economic shocks affect both county types in similar ways.

Figure 3.2 Mean Firms per Thousand Population, Tribal vs. Non-Tribal, 1978-2019



Source: Census Business Dynamic Statistics.

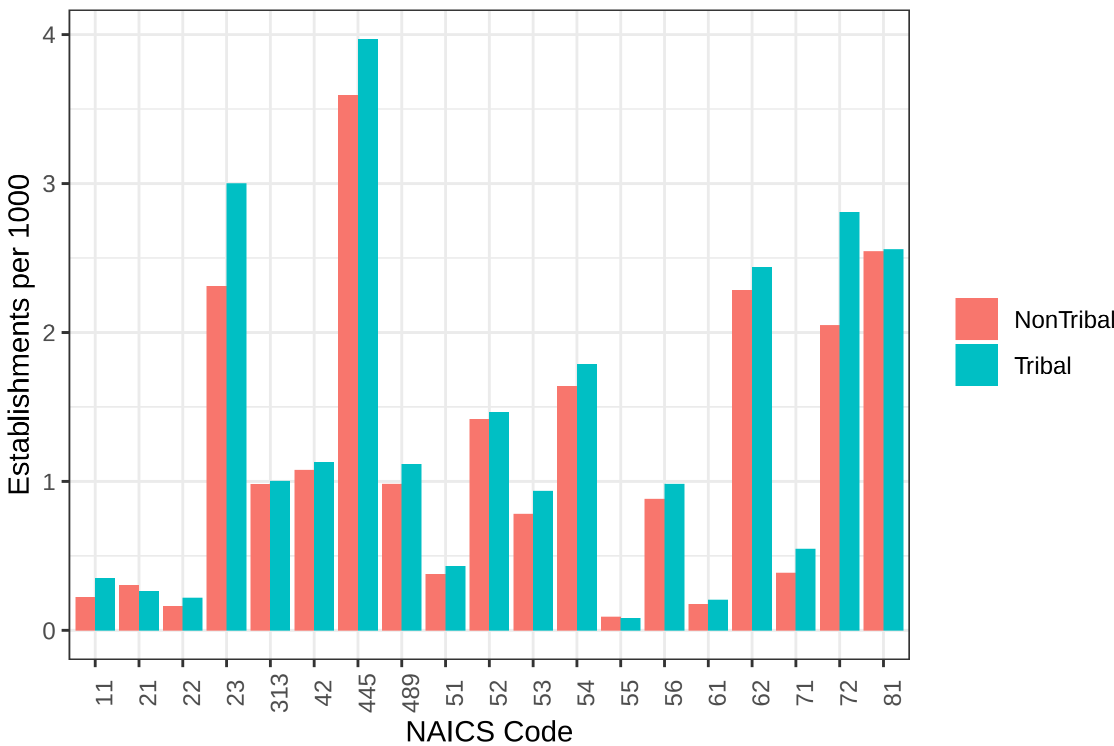
To measure new business activity at the county level, I use annual establishment entry data from the BDS. The finding using this measure mirror Figure 3.2: since the late 1970s, tribal counties on average have higher population-adjusted counts of not only total firms, but also the number of annual establishments entries throughout the entire period. Both county types have similar trends over time in establishment entries.

3.4.3 WholeData (2001-2016)

The county-level sector analysis relies on annual WholeData from 2000 to 2016. WholeData is a collection of annual datasets curated by the Upjohn Institute for Employment Research, which is based on County Business Patterns (CBP) data collected by the Census (Bartik et al., 2018). These data are an improvement over CBP because they provide suppressed cell estimates of employment, establishment counts, and payroll for small counties for which information is not provided for confidentiality reasons. I use the definition of a sector based on the two-digit North American

Industry Classification System (NAICS) level. I use this level of classification because suppression issues are an issue for industry classifications at a finer level of detail below the two-digit level, even with the advantages afforded by the WholeData set. I exclude Public Administration and nonclassifiable establishments from the analysis of business diversity.

Figure 3.3 Mean Establishments per 1000 in Different Industries, 2016



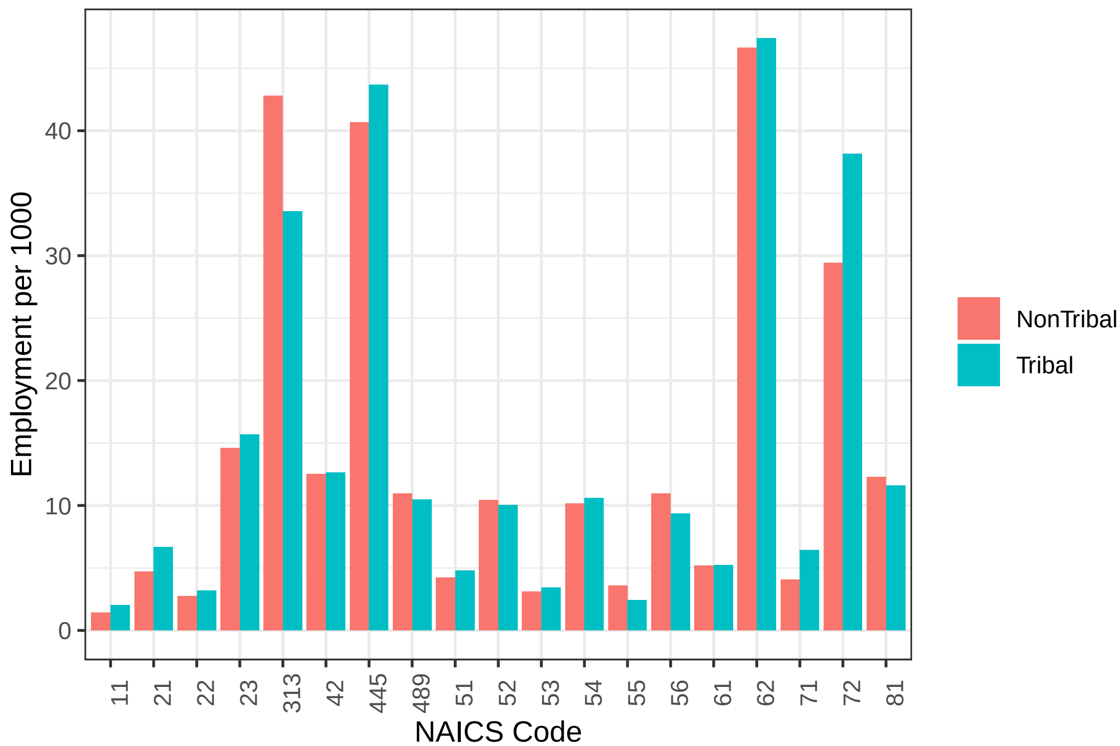
Source: WholeData, Upjohn Institute. NAICS Codes correspond to the industries listed in Table 3B.1.

Figure 3.3 displays the average counts of establishments per 1000 people in tribal and nontribal counties for each sector in 2016. Table 3B.1 provides definitions for each two-digit NAICS level. In 2016, population-adjusted counts of nontribal establishments are higher in all industries except for Management of Companies and Enterprises (55). The difference is largest for three industries: Construction (23), Retail Trade (44-45), and Accommodation and Food Services (72). This is similar to levels earlier in the Nation-to-Nation Era: tribal counties had higher numbers of establishments in every sector in 2000 (Figure 3B.1).

Figure 3.4 displays population adjusted mean employment levels in each two-digit NAICS level in 2016 for tribal and nontribal economies. Tribal economies and non-tribal economies are dominated by the same four industries: Manufacturing (31-33), Retail Trade (44-45), Health Care and Social Assistance (62), and Accommodation and Food Services (72). This is similar to the early 2000s. For the most part, mean employment between these two types of counties are similar in magnitude for most industries over the Nation-to-Nation Era. However, two industries stick out in their differences: Manufacturing and Accommodation. Nontribal counties on average have more of their employees in the manufacturing sector, while tribal counties have more employees in Accommodations. For example, the mean population-adjusted employment levels for tribal counties are 9.26 lower in the manufacturing industry and 8.75 higher in the accommodations industry in 2016. This difference in employment allocation is likely an expression of the importance of gaming enterprises in tribal counties. Casino Hotels (NAICS 72112), a prominent gaming business for tribes, is nested in the Accommodations sector. As Akee and Taylor (2014) note, the IGRA and subsequent events that increased autonomy in tribal governments and tribal entrepreneurs's ability to engage in casinos and other gaming ventures have resulted in improvements in the well-being of those living in tribal areas. It is likely that this economic activity translates into more people being employed in the sector compared to non-tribal economies. Other gambling industries (NAICS 71321 and NAICS 71329) are found in the Arts, Entertainment, and Recreation (71) sector. From Figure 3.4, I can see that county level tribal economies also have higher employment on average in this sector compared to the nontribal counties for 2016 (also for 2000 and 2008), another indicator that gaming may have a significant effect on employment allocation. It is important to note that this measure does not take into consideration the distribution of firm sizes in each sector.

Figure 3B.2 presents changes in population-adjusted employment levels in each sector from 2000 to 2016. Over the analysis period, there has been a significant drop in employment per capita devoted to manufacturing for both county types. Non-tribal counties saw a drop of approximately 17.92 employees per 1000 people in manufacturing, while tribal counties saw a decline of 9.64. The industry that experienced the most growth in employees per capita for both types is Health

Figure 3.4 Mean Employment per 1000 in Different Industries, 2016



Source: WholeData, Upjohn Institute. NAICS Codes correspond to the industries listed in Table 3B.1.

Care and Social Assistance. Accommodations saw a rise in employment per capita for both types of counties, but tribal counties in this time period gained about 1.05 more compared to their nontribal counterparts.

As an aggregate measure for business diversity among the two types of counties, I construct Herfindahl-Hirschmann indices in the eighteen two-digit NAICS industries used above. The indices are calculated using the following equation

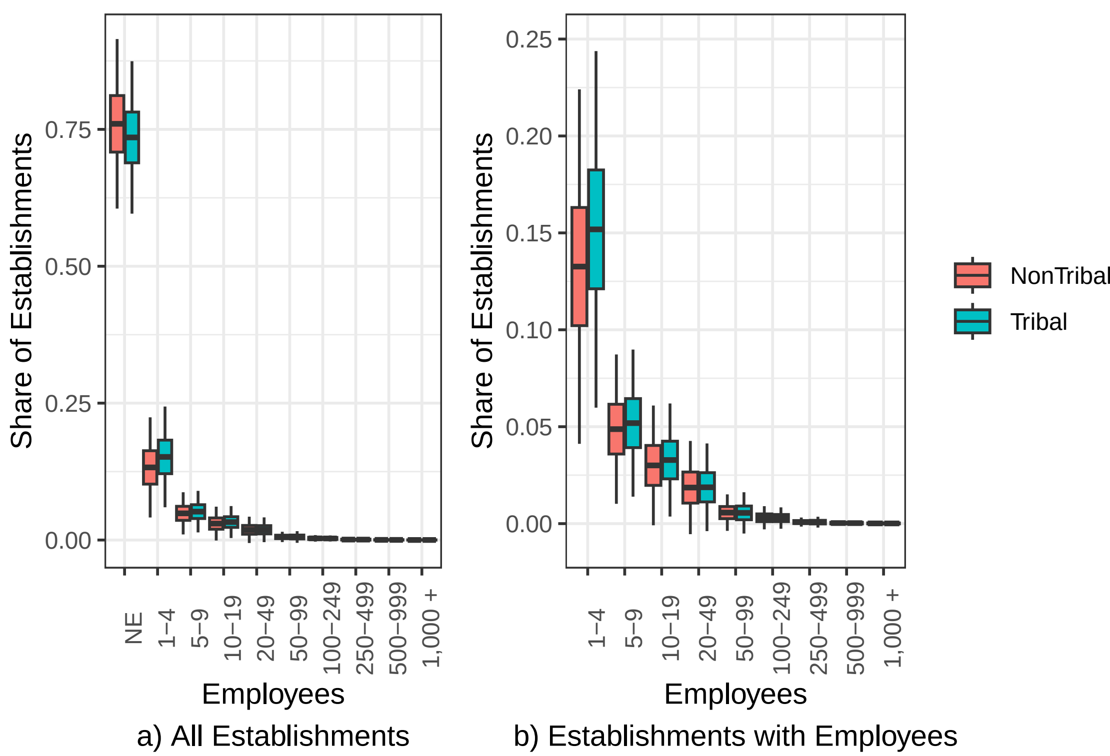
$$HHI_i = \sum_{r=1}^{n} s_{ir}^2 \tag{3.1}$$

for county i and sector r. $s_{ir} \in [0, 100]$ is the percentage of sector r's presence in the county-level industry. For the Employment HHI, this is the percentage of employees in the county employed in the sector. For Establishment HHI, this is the percentage of establishments in the county participating in business in that sector. Smaller HHI values indicate a higher level of diversity in an economy, while larger values indicate specialization. These measures of business diversity will be used in the parametric and nonparametric estimation strategies.

3.4.4 County Business Patterns (CBP) and Nonemployer Statistics (NES)

While WholeData provides almost noiseless data from 2000-2016 on firm counts, it does not provide other important information that County Business Patterns (CBP) covers. In particular, WholeData does not provide information on the number of firms in a county based on employment size. There are links between the distribution of business sizes in a county and its rate of economic growth, suggesting that distributions with higher counts of smaller businesses are nearer to an optimal distribution for growth (Loveridge and Nizalov, 2007).

Figure 3.5 Establishments Sizes of Tribal and Non-Tribal Businesses



Source: Census Nonemployer Statistics and County Business Patterns, 2016. Panel A includes both nonemployer (NE) firms and all firms with employees. Panel B includes only firms with employees, allowing for a better view of firms with lower values. Means are presented as the middle line in the boxplot. The upper and lower sides of the rectangle are one standard deviation away from the mean. The ends of the lines of the boxplots are three standard deviations away from the mean.

Figure 3.5 plots the means and standard deviations of county-level firm employment size shares in 2016 between nontribal and tribal counties using data from CBP and Nonemployer Statistics (NES). For all size categories above and including 5-9 employees, the differences in means are near zero in magnitude. This indicates that for both types of counties, larger firms make up less of the local economy. Nonemployer businesses and businesses with between 1 and 4 employees make up on average almost 90% of the county level economy for both nontribal and tribal areas. However, nontribal counties on average have 2 percentage points more nonemployer firms than tribal counties. Tribal counties make up that difference almost completely in the 1-4 employee category, where it has approximately 2 percentage points more firms on average.

3.4.5 Additional Covariates

In addition to the business data, I use the United States Department of Agriculture Economic Research Service's Rural-Urban Continuum Codes (RUCC) created in 1974, 1983, 1993, 2003, and 2013. Using these codes, four binary variables were created that identified counties as being metropolitan (1, 2), metro-adjacent (3, 6, 8), micropolitan (3, 5), and rural (7, 9). I also use county-level personal income and population data from the Bureau of Economic Analysis from 1990-2019, and unemployment rates from the Bureau of Labor Statistics' (BLS) Local Area Unemployment Statistics from 1990 to 2019 as covariates in the statistical analysis. Table 3B.2, Table 3B.3, and Table 3B.4 provide summary statistics for the samples I use in the statistical analysis.[4]

3.5 Empirical Framework

In this section, I describe the two methods I use for the statistical, noncausal analysis. It is important to note that with the limitations on the availability of county level data for both the variables of interest and control variables, I am unable to run the analysis throughout the entirety of the Self-Determination (1968 – 2000) and Nation-to-Nation (2000 – Present) Eras. For empirical analysis involving data from the BDS, the panel begins in 1990 and extends to 2019. For analysis

[4]A striking aspect of the summary statistics presented in the tables is that tribal counties on average have higher unemployment rates, but also higher income per capita. This could come as surprising, especially as American Indians living on reservations are the poorest ethnic group in the United States on average (Akee and Taylor, 2014). There are two potential reasons for this: (1) measurement error in the tribal definition or (2) the extreme wealth of some tribes pushing the average up (Treuer, 2019).

involving WholeData, I capture a smaller time period from 2000 to 2016. While these limitations are present, I am still able to estimate the differences between tribal and nontribal counties.

3.5.1 Regression Analysis

For the regression analysis, I estimate the following linear model:

$$Biz_{it} = \beta_0 + \beta_1 Tribal_i + X_{it}\beta_2 + c_i + d_t + u_{it} \tag{3.2}$$

where Biz_{it} is a measurement of business activity in county i at time t, $Tribal_i$ is a time invariant binary variable that indicates whether a county contains tribal land, X_{it} is a vector of time variant controls, c_i is the unobserved time-invariant heterogeneity associated with county i, d_t is a year-level control, and u_{it} are idiosyncratic errors.

It is important to think critically about what I am attempting to estimate as β_1 because of the inherent diversity of sovereign nations in the United States. As I remarked in Section 3.2 and Section 3.3, there are many factors that determine the economic development of these nations, despite having a shared history of colonization and discrimination. While some regression analysis and case study analysis involving multiple tribes attempt to control for these specific factors, such as tribal government attitudes (Cornell and Kalt, 1995) and whether they accepted BIA funding in exchange for stronger ties with the U.S. government (Frye and Parker, 2021), it is difficult to make claims using these tools with non-tribal areas as counterparts. Therefore, it is best to interpret estimates of β_1 as the average partial effect of a county containing tribal-designated land on business counts. Estimates should be treated as merely as correlations. One should not use them to purport a causal story.

Another consideration in the estimation strategy is at what level I should control for unobserved heterogeneity. There is likely some correlation between the tribal term and the unobserved heterogeneity ($Cov(Tribal_i, c_i) \neq 0$). However, if one decides to control for the unobserved heterogeneity at the county level using an estimator such as the fixed effects estimator (FE) that uses the within transformation, one should be skeptical as to whether they are removing important variation due to tribal-designated land, even before considering the algebraic concerns (as $Tribal_i$

is time-invariant). However, because the unit of analysis is large enough where only a small fraction of a county could have tribal lands, estimates of β_1 may be biased due to the presence and spillover of non-tribal economies. Therefore, an estimation strategy that uses pooled OLS with state and year dummies may not properly estimate the parameters of interest.

With these concerns, I alter the estimation strategy. First, I modify the linear model to

$$Biz_{it} = \beta_0 + \sum_{t}^{T} \beta_{1,t} Tribal_i * d_t + X_{it}\beta_2 + c_i + d_t + u_{it} \tag{3.3}$$

and I model the unobserved heterogeneity, c_i, as state dummies for the pooled OLS estimation and using the Chamberlain device for correlated random effects (CRE) estimation (Wooldridge, 2010):

$$c_i = \gamma_1 Tribal_i + \bar{X}_i \gamma_2 \tag{3.4}$$

where $\bar{X}_i$ is a vector of time averages of the time-variant controls. This linear model accomplishes several goals. First, it allows us to estimate the heterogeneous effect of being designated a tribal county over time. Using this estimation strategy, I can better answer a story of convergence I propose in H1 and H2. If I cannot reject the null hypothesis that tribal counties on average are different from nontribal counties in more recent years, I have statistical evidence that supports the convergence hypothesis. Second, this linear model provides a practical solution to the unobserved heterogeneity issue: I can use both the CRE estimator (which yields the same estimates as the fixed effects estimator) and the pooled OLS estimator to estimate the average partial effect of being tribal over time and compare results. If the estimates from these two estimators yield similar results, I will have statistical evidence that is robust to misspecification and bias due to the unobserved heterogeneity.

3.5.2 Semiparametric Estimation

Regression analysis estimates the average partial effect, but because of the systemic differences among tribes, it may be better to use estimation methods that capture other features of the distribution of tribal counties against nontribal counties, especially for H3. Therefore, I also estimate counterfactual distributions using weighted kernel density estimation (DiNardo et al., 1996).

Specifically, the kernel density estimate $\hat{f}_K(\cdot)$ at point x in the distribution of the business activity measure can be written as:

$$\hat{f}_K(x) = \frac{1}{nh}\sum_{i=1}^{n} w_i K\left(\frac{x - X_i}{h}\right) \tag{3.5}$$

where K is the kernel density function (in this case the Epanechnikov kernel), h is the bandwidth, w_i is a weight for random sample X_i of size n. Bandwidths are determined using the adaptive estimate of spread, A, in the following equation:

$$h = 0.9An^{-1/5} \tag{3.6}$$

following (Silverman, 2018). This bandwidth method maintains the estimation's mean integrated square error within 10% of the optimal (Silverman, 2018). I use this estimation strategy for estimating both the actual and counterfactual kernel densities.

To create annual counterfactual kernel densities, I reweight the nontribal observations using propensity scores. The propensity scores are the estimated probabilities of being a tribal county using the time-varying variables in the regression specification (unemployment rate, personal income per capita, and rural designation) obtained from logit regressions. In essence, this estimation strategy seeks to answer the question "what would the density of business activity in nontribal counties be if they had the same attributes as tribal counties?" By reweighting the nontribal observations by tribal propensity scores to create a counterfactual tribal density for each year in the analysis period, I can see how the distribution of tribal counties directly compares to a synthesized distribution both cross-sectionally and over time. As a result, I may better see the partial effect of being tribal compared to using the nontribal density as a baseline.

The main issues associated with this estimation strategy (and with the regression analysis) deal with the choice of control variables. Because personal income per capita and unemployment rates might be partially determined by tribal status, I may be biasing the results. Additionally, I may not be controlling for more important unobserved characteristics among the sample, making the propensity scores poor weights. As a result, I present the two unweighted densities along with the reweighted density in all results.

3.6 Results

The results section is divided into three sections with each corresponding to a different hypothesis presented in Section 3.3. I explore results on the three measures of business activity: population-adjusted firm counts, population-adjusted establishment entry, and HHI on the county level. If the regression estimates indicate large in magnitude and statistically significant differences between tribal and nontribal entities, this evidence would support the first two hypotheses. Kernel density estimation results correspond to the third hypothesis. I estimate heterogeneity across time for both estimation methods offering a potential evidence of convergence, divergence, or stasis.

3.6.1 Firms and Establishments

The first hypothesis deals with entrepreneurship: as the analysis period for the data occurs after the dawn of the Self-Determination Era, I expect that there will be an increase in entrepreneurship in tribal counties on average. In Section 3.4, Figure 3.2 suggests that this is the case. The regression analysis will attempt to isolate the effect of tribal lands after controlling for other conditions.

Table 3.1 presents estimates from Equation 3.3 of the average partial effect of the tribal indicator across all business activity measures and estimation strategies. Across all business and entrepreneurship measures (Column 1 through Column 8), correlated random effects estimations are larger in magnitude than pooled OLS estimates. For example, Column 1 and Column 2 indicate that tribal counties on average have between 0.469 and 1.658 more firms per 1000 people compared to nontribal counties. In terms of establishments, tribal counties on average have a higher volume entering and exiting the local economy along with a higher net entry count. Compared to the sample means in Table 3B.2 and Table 3B.3, these statistically significant estimates are rather small, being well within one standard deviation of the sample mean. However, it does show that tribal counties have more businesses and business churn on average. Section 3A.3 explores whether these results hold across different rural designations.

Figure 3.6 and Figure 3B.3 present the estimated coefficients on the tribal-year interactions for population-adjusted firm counts and population-adjusted establishment measures, respectively. These figures visualize whether the effect of being tribal changes over time, compared to the

Table 3.1 Regression Results Across Different Measures of Business Activity

	Firms		Estab. Entry		Estab. Exit		Estab. Net Entry		Estab. HHI		Emp. HHI	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	POLS	CRE	POLS	CRE	POLS	CRE	POLS	CRE	POLS	CRE	POLS	CRE
Tribal APE	0.469*	1.658***	0.088**	0.281***	0.067**	0.216***	0.020	0.065***	-7.153	-25.127***	-32.305	-58.914**
	(0.268)	(0.237)	(0.040)	(0.039)	(0.032)	(0.030)	(0.014)	(0.014)	(8.028)	(7.837)	(33.262)	(28.094)
Unemployment Rate	-0.293***	-0.055***	-0.026***	-0.023***	0.006	0.027***	-0.032***	-0.050***	6.949***	-0.546	4.011	-16.581***
	(0.029)	(0.013)	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)	(1.494)	(0.938)	(4.994)	(2.325)
Income Per Capita	0.375***	0.084***	0.052***	0.004	0.044***	0.001	0.008***	0.003	-1.022**	-1.826**	-7.561***	-0.920
	(0.016)	(0.010)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.472)	(0.909)	(1.319)	(1.244)
Metro-Adjacent	2.786***	2.108***	0.044*	-0.016	0.122***	0.048**	-0.078***	-0.065***	52.296***	19.296***	286.224***	238.717***
	(0.177)	(0.189)	(0.023)	(0.029)	(0.020)	(0.023)	(0.010)	(0.014)	(4.925)	(6.005)	(23.163)	(26.319)
Micro	2.752***	0.426**	0.087***	-0.079**	0.141***	-0.062**	-0.054***	-0.016	20.083***	-31.619***	55.256**	-18.102
	(0.214)	(0.196)	(0.027)	(0.031)	(0.024)	(0.026)	(0.011)	(0.015)	(5.381)	(6.809)	(25.618)	(29.320)
Rural	5.210***	1.797***	0.289***	0.036	0.383***	0.073***	-0.094***	-0.037*	108.801***	38.582***	373.038***	259.469***
	(0.248)	(0.180)	(0.035)	(0.030)	(0.029)	(0.026)	(0.013)	(0.021)	(10.961)	(7.948)	(30.297)	(29.504)
Observations	89382	89382	79666	79666	79666	79666	79666	79666	51938	51938	51938	51938

Note: *** p<0.01, ** p<0.05, * p<0.10. Standard errors clustered by county in parentheses. Headings of the columns dictate the dependent variable in each regression. Columns (1)-(8) use an analysis period from 1990 to 2019, while Columns (9)-(12) use an analysis period from 2000 to 20016. Tribal APE is the average partial effect of the tribal indicator and its year interactions. Firm and Establishment counts are population adjusted (per 1000 people). Pooled Ordinary Least Squares (POLS) are in odd columns, Correlated Random Effects (CRE) are in even. Year dummies, tribal-year interactions, state dummies (POLS only), and time-averages (CRE only) are not shown.

beginning of the analysis period. For firm counts (Figure 3.6), coefficients are not statistically significant over the analysis period, indicating that the effect of being tribal does not change after controlling for other factors. For the brief period estimates are statistically different from zero (2005-2007), the magnitude is small: the difference between tribal and nontribal population adjusted firm counts is at most 0.5 more than in 1990.

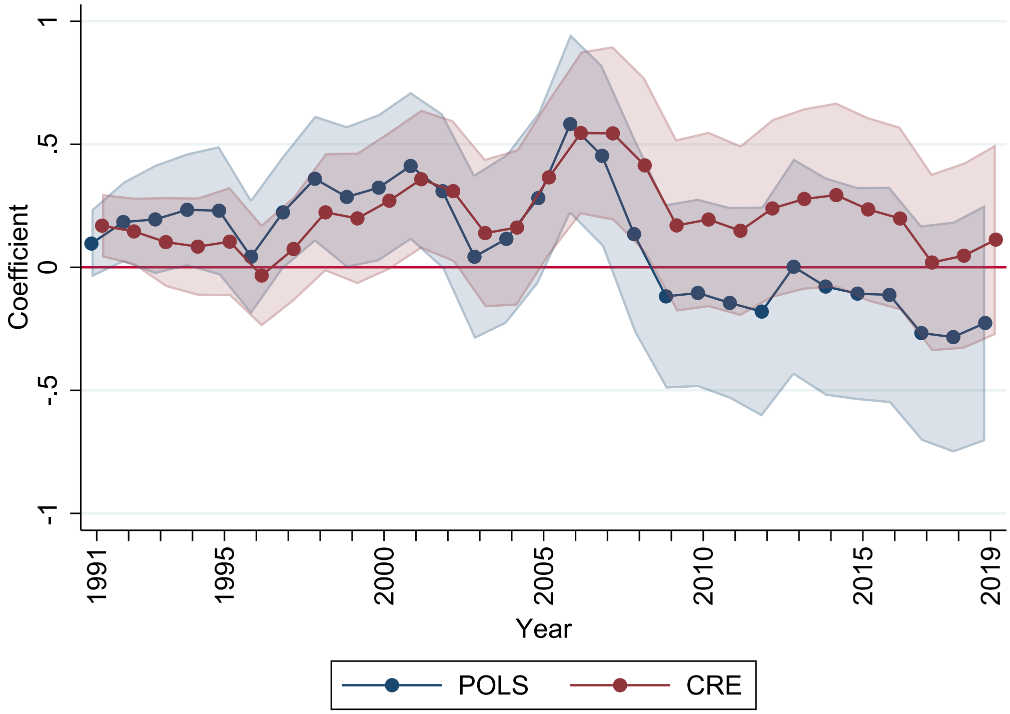
For establishment measures (Figure 3B.3), the trends are a bit more nuanced. Throughout the analysis period, the effect of containing tribal lands in a county does not change over time in a statistically significant manner for entries until about 2005. Similar to firm counts, the effect of being tribal increases entries in a statistically significant but small in magnitude way. However, estimates trend downwards after 2005 across both estimation methods, which becomes statistically significant around 2017. This indicates that recently, the tribal-nontribal gap is shrinking in establishment entries. As for exits, the effect is statistically different from zero compared to 1990 for most of the analysis period until 2015. Estimates for net entry across estimation methods are different in sign but statistically significant across estimation methods.

In the context of the hypothesis, it seems that tribal areas have more business activity and churn compared to their nontribal counterparts following the dawn of the Self-Determination Era. As this analysis does use data before that period, it is difficult to conjecture whether Self-Determination legislation did in fact cause this higher rate of business activity. There is limited evidence of convergence of tribal business activity to nontribal counts except for net establishment entries.

3.6.2 Industrial Diversity

The industrial diversity hypothesis builds on the history of American Indian tribes. Through treaty infringements and U.S. policies such as the Dawes Act, tribes have been excluded from certain industries such as agriculture. On the other hand, Self-Determination policies brought about unique ways for tribes to conduct business compared to nontribal areas. As a result, I expect that tribes would be less economically diverse, but will become less concentrated over time. From the descriptive figures, there are more establishments in each NAICS industry sector (Figure 3.3), but much more employment in Construction, Retail Trade, and Accommodations.

Figure 3.6 Tribal-Year Interaction Coefficients



Source: Study Regression Coefficients. Coefficients are obtained by estimating Equation 3.3 using pooled OLS and Correlated Random Effects. Point estimates are represented by the connected points in the figure. Standard errors are represented by the shaded area around the parameter estimates.

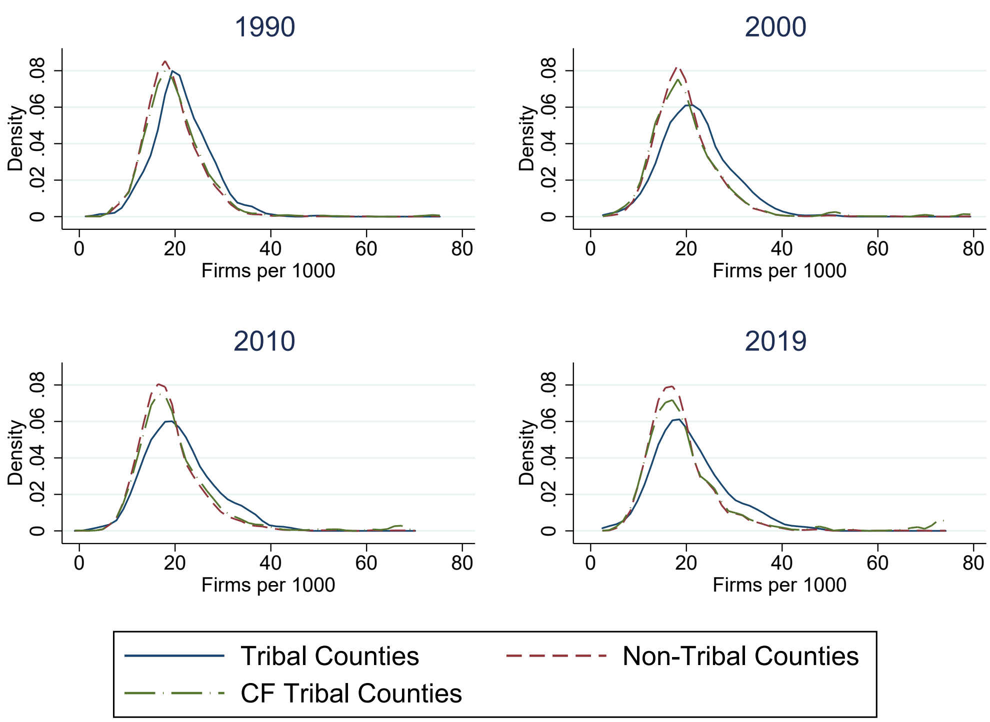
In Table 3.1, Column 9 through Column 12 present estimates of establishment and employee industrial diversity. Similar to the firms and establishment measures used as evidence in the first hypothesis, CRE estimates are bigger than pooled OLS in magnitude. Pooled OLS estimates are also not statistically significant at conventional levels. All estimates are negative, indicating tribal economies are less concentrated across industries compared to nontribal counties. They are more economically diverse, providing evidence against the hypothesis.

Figure 3B.4 displays the changes in the effect of being tribal over time for the HHI measures. Compared to the previous figures discussed (Figure 3.6 and Figure 3B.3), employment HHI has a more obvious trend. Compared to 2000, the effect of containing tribal lands on industrial diversity increases over time, indicating that tribes are becoming more and more concentrated in specific industries. As for establishment, there is no strong trend over time, similar to the other firm and establishment measures. Tribal counties are converging in the employment diversity with nontribal counties over time.

3.6.3 Tribe Heterogeneity

To observe the heterogeneity of business activity across tribes, I implement counterfactual kernel density analysis. Across all figures, tribal distributions are blue, nontribal distributions are red, and the counterfactual tribal distributions (weighted nontribal densities) are green. Comparing the shape of the tribal density against its counterfactual, I can see how features of the distribution other than the mean is different, after controlling for covariates. Figure 3.7 and Figures 3B.5 through 3B.9 present these densities for population-adjusted firm counts, population-adjusted establishment measures, and industrial diversity, respectively.

Figure 3.7 Firm Kernel Density Estimates



Source: Study Source: Study Kernel Density Estimates. Kernel densities are estimated using the procedure outlined in Section 3.5.2. CF Tribal Counties are counterfactual kernel densities estimated using propensity score weighting outlined in in Section 3.5.2.

For population-adjusted firm counts (Figure 3.7), the kernel densities are close in shape in 1990, with tribal skewed towards more firms. Tribal counties, compared both to nontribal and the counterfactual, have a higher probability of having more than 20 firms per capita across the period. The right-side tail of the tribal kernel density gets larger as I approach 2019, indicating increasing heterogeneity in firm counts as time progresses. While some of this phenomenon is captured in covariates as displayed in the fattening of the tails of the counterfactual distribution, the distribution of tribal firms is much more different in shape than the counterfactual.

The establishment story (Figure 3B.5 though Figure 3B.7) is very similar to the firm story: there are higher probabilities of around 2 population-adjusted establishment entries and higher probabilities of around 2 establishment exits. In terms of changes in the distributions over time, they are more static than the firm counts. Net entries are especially similar across years, with noticeable shrinking of the tails from 1990 to 2000.

Industrial diversity across both measures (Figure 3B.8 and Figure 3B.9) reflect similar stories. The distributions of establishment and employment HHI are close to identical across both the tribal/nontribal designations and over time. It seems that industrial diversity across counties are quite similar. In essence, I find evidence to support the third hypothesis among firm an destablishment counts, but no evidence for the industrial diversity measures.

To further highlight the variety of business activity among tribal counties, Table 3.2 displays the top and bottom five tribal counties in 2019.[5] Counties in the top 5 have more than 42.57 firms per 1000 people, while those in the bottom 5 have less than 6.08 firms per 1000. Around 20 nontribal counties (<1%) have at least 42.57, but only 2 nontribal counties (<0.1%) have less than 6.08 in 2019. As five counties constitute approximately 1% of tribal counties in 2019, this indicates that there are a similar number of outliers on the higher end between the county types, but much more tribal counties on the lower end of the distribution.

[5]It is important to note that tribal counties are sometimes suppressed due to disclosure issues in publicly available data. To illustrate this, Menominee County, which contains portions of the Menominee Reservation, is suppressed in 2019 but appears as a member of the bottom five in other years.

Table 3.2 Top 5 and Bottom 5 Tribal Counties by Firms per 1000 People, 2019

County	Tribal Lands	Firms Per 1000	Tribal Percent	Income Per Capita
Top 5 Counties				
1. Dukes County, MA	Wampanoag-Aquinnah Trust Land	47.36	0.64	87120.96
2. Jones County, SD	Rosebud Indian Reservation and Off-Reservation Trust Land	46.10	1.00	56771.68
3. Summit County, UT	Uintah and Ouray Reservation and Off-Reservation Trust Land	45.25	0.49	152309.62
4. Cook County, MN	Grand Portage Reservation and Off-Reservation Trust Land	44.50	0.96	54021.42
5. Grand County, UT	Uintah and Ouray Reservation and Off-Reservation Trust Land	42.57	0.99	58620.18
Bottom 5 Counties				
5. Pinal County, AZ	Gila River Indian Reservation, Maricopa (Ak Chin) Indian Reservation, San Carlos Reservation, Tohono O'odham Nation Reservation and Off-Reservation Trust Land	6.08	0.48	32699.00
4. Apache County, AZ	Fort Apache Reservation, Hopi Reservation and Off-Reservation Trust Land, Navajo Nation Reservation and Off-Reservation Trust Land, San Carlos Reservation, Ute Mountain Reservation and Off-Reservation Trust Land, Zuni Reservation and Off-Reservation Trust Land	4.77	0.95	33843.76
3. Todd County, SD	Pine Ridge Reservation, Rosebud Indian Reservation and Off-Reservation Trust Land	4.56	1.00	25642.82
2. Buffalo County, SD	Crow Creek Reservation, Lower Brule Reservation and Off-Reservation Trust Land	4.54	1.00	20289.61
1. Oglala Lakota County, SD	Pine Ridge Reservation	4.23	1.00	25631.15

Note: The top and bottom five tribal counties are determined by their Firms per 1000 counts in 2019. Tribal lands are the reservation and trust lands found in the tribal counties. Tribal Percent is the percent area covered by Census tracts intersecting tribal lands (See Section 3.4.1). Income per capita is presented in dollars.

There are several other important aspects to this table worth noting. The first are the levels of income per capita across these counties. As the coefficient estimates in Table 3.1 suggest, higher income per capita counties have more firms than lower income counties. All members of the top 5 have more than $50,000 per capita in 2019, while those in the bottom five have less than $35,000. To put these measures into perspective, the poverty threshold for a household of three people in 2019 was $20,335 (Census Bureau, n.d.). Counties in the bottom five have between 99.8% and 166.4% of the threshold, while those in the top five have per capita incomes of at least 265.7% of the threshold. While income per capita does not adequately capture the distribution of wealth in a county, this measure does speak to the fact that higher incomes improve incidence of business, whether that wealth is concentrated on the supply or demand side.

An arguably more important (and tribe-specific) insight found in Table 3.2 are the tribal lands associated with these counties. First, most counties in both lists have more than 95% of the land covered by tribal tracts using the tribal county measure.[6] Second, there is tremendous variation in the number of tribal lands in the bottom five. Four of the bottom five contain multiple reservations, with Apache County containing six distinct tribal lands. In contrast, all counties in the Top 5 contain lands of only one reservation, albeit some of these lands contain multiple historic bands of tribes (such as the Uintah and Ouray Reservation).

Perhaps the reason for this stark difference can be attributed to the aims of reservation formation: U.S. settlers wanted the land best suited for life and enterprise and forced American Indian reservations onto less desirable land. Or, the explanation could be similar to that found in Dippel (2014), where fragmentation and too much local governance in an area could cause tension that hurts governments and in turn, entrepreneurship. On the other hand, there could be agreements between these tribes on how to conduct business leading to less competition. It is difficult to ascertain the mechanisms from these data.

[6] It is important again to acknowledge that this measure has its faults, which contributes to mismeasurement. The top five counties in Table 3.2 do not have any reservation or trust land on them but instead intersect with tribal lands. This remains to capture populations of American Indians living adjacent to reservations. I maintain this measure for this chapter, but use of different definitions of tribal should be used to test the robustness of this measure in future works.

Table 3B.5 and Table 3B.6 present diversity measures for these same counties in 2016. For the most part, tribes in the top five are more industrially diverse than the bottom five in both establishment counts and employment. A larger share of establishments and employment are found in the Accommodations section for the top five counties. As for top industries, Retail Trade and Accommodations seem to appear most across both the top and bottom five. It is worth noting that Jones County, SD, the second county in the top five list intersects land of the Rosebud Indian Reservation and Trust Land. This part of Indian Country also contains Buffalo County, SD, which is the second-to-last county in the bottom five. Perhaps agglomeration inside a tribal land or differences in the use of land across the reservation contributes to this stark difference.

The preceding analysis has its setbacks. Appendix 3A employs robustness checks, including using continuous measures of "tribalness" of a county and different comparison groups. While many of the estimates change, the sign and significance are largely consistent across the checks and the estimates provides in the main body of this chapter.

3.7 Conclusion

This chapter uses several estimation techniques to provide descriptive evidence on business activity in American Indian sovereign nations sharing geographies with the contiguous United States. Using county-level data spanning portions of the Self-Determination Era (1968-2000) and the Nation-to-Nation Era (2000-present), I test three hypotheses: American Indian economies experience a surge of business activity and growth following landmark self-determination efforts (H1); the economies will be concentrated in historically profitable industries (H2); and there is wide variation in business counts due to the variety of American Indian experiences (H3).

I find evidence that supports H1. From 1978 to 2019, tribal counties on average have more firms per capita compared to their nontribal counterparts. This estimate ranges from approximately 0.5 to 1.7 more firms per 1000 residents after controlling for measures of unemployment, income, and rurality. It also stays constant throughout the analysis period. For H2, I find that more tribal firms are in all industries except Management of Companies and Enterprises. Employment is more concentrated in accommodations for tribal areas, and regression results find that American

Indian economies are on average becoming more concentrated in their employment. Kernel density estimates used to provide evidence for H3 show that American Indian economies have fatter right tails in firm counts, suggesting that much of the heterogeneity between tribes are higher firm counts.

There are several limitations to this study. First, the variable of interest is a binary measure of whether tribal areas are in a county, which can cause contamination bias and measurement error. As the robustness checks in Appendix 3A suggest, the binary measure may not adequately capture tribal business activity because of this data constraint. This study would greatly benefit from the use of microdata on the location of the firms as well as the demographics of the owners and employers of the businesses. In addition, this study likely suffers from omitted variable bias. Despite best efforts to control for unobserved heterogeneity and endogeneity through fixed effects and Chamberlain devices, there may be time variant measures, such as changes in tribal government attitudes on entrepreneurship, converting fee-simple land to trust, or the success of local Native CDFIs that may reduce bias in the results. However, this chapter does offer a baseline on quantitative business activity research for tribal areas.

The findings overall seem to raise more questions than answers. Tribal counties may have more firms and more business churn for a variety of reasons. Often, people living in poverty hold multiple occupations and pursue numerous entrepreneurial opportunities (Banerjee and Duflo, 2007). This may be the case for tribal areas: American Indians on reservations might pursue multiple work opportunities and build business ventures to insure against risky employment. Anecdotes support this: members of Tulalip Tribes pursue multiple entrepreneurial activities but do not make much (Treuer, 2019). If this is the case, the higher churn and firm count may signal suboptimal firm distributions (Loveridge and Nizalov, 2007) and tribal governments may want to find ways to support viable businesses that can grow and employ members of the tribe.

High firm counts may also signal more competitive environments. Tribal corporations might compete directly with tribal entrepreneurs, which may hinder development outcomes (Cornell et al., 2007). Tribal governments may also make sovereignty-centered decisions that result in higher county firms counts. Internet provision in Indian Country provides many such anecdotes. Some tribes (such as the Coeur D'Alene) may create their own internet service provider to service their reservations, while others (such as the Navajo Nation) may build out internet infrastructure to attract numerous providers to drive down consumer costs (Duarte, 2017). Both strategies would increase the firm counts of counties that contain both tribal and nontribal lands. Whether or not these strategies improve welfare is an important area of study.

In a similar way, understanding concentration in specific industries may also provide important insights on the differences between tribal and nontribal local economies. The analysis conducted in this paper is limited to two digit NAICS industries, but understanding the size of firms and how many there are in more granular NAICS codes could provide nuanced insights on tribal economic development. This could especially be interesting in the case of the Accommodations sector: casino hotels are established economic development engines, but perhaps tribes diversify in other ways within the accommodations industry. Even more, understanding how nontribal and tribal areas interact with one another in certain sectors may offer further insights. Using uncensored data that underlies the BDS and CBP may offer these research opportunities.

The findings of this study also point to other research agendas. Using microdata on businesses and business owners found in FSRDCs, one may be able to better identify businesses owned and maintained by tribal corporations, tribal members, and nontribal entrepreneurs. Analyses using those data might offer a more accurate view of the benefits and challenges of conducting business in and around tribal areas. Additionally, more research on Native CDFIs and local indigenous credit markets may help subvert or affirm the long-held belief that tribes suffer from a lack of collateral. Several studies already mentioned find evidence in favor of subversion (Akee and Jorgensen, 2014). Finally, exploring the impact of tax credits, especially the Indian Employment Tax Credit, may help better contextualize the results of this chapter. All in all, there is a wealth of research opportunities to explore in Indian Country that can help track and improve development outcomes.

In terms of policy implications, policy should continue to take a more progressive stance in Indian Country. From the heterogeneity analysis, tribal counties with lower income per capita have lower population-adjusted firm counts as well as more concentrated economies. Policy should aim to improve entrepreneurship and industrial diversity in those areas to improve economic resiliency. As for counties with higher firm counts, policy should be created to protect engines of growth, especially the accommodations sector. As online gaming becomes more popular, tribes should adapt to this new market and policy should support them. More long run policies should focus on diversifying the wealthier tribal counties to more sustainable engines of growth.

BIBLIOGRAPHY

Akee, R. (2009). Checkerboards and Coase: The effect of property institutions on efficiency in housing markets. *The Journal of Law and Economics*, *52*(2), 395–410. https://doi.org/10.1086/592718

Akee, R. (2020). Land titles and dispossession: Allotment on American Indian reservations. *Journal of Economics, Race, and Policy*, *3*(2), 123–143. https://doi.org/10.1007/s41996-019-00035-z

Akee, R., & Jorgensen, M. (2014). Property institutions and business investment on American Indian reservations. *Regional Science and Urban Economics*, *46*, 116–125. https://doi.org/10.1016/j.regsciurbeco.2014.04.001

Akee, R., Mykerezi, E., & Todd, R. M. (2021, June 4). *Business dynamics on American Indian reservations* (No. 2021-02). Federal Reserve Bank of Minneapolis Center for Indian Country Development. Minneapolis.

Akee, R. K. Q., Spilde, K. A., & Taylor, J. B. (2015). The Indian Gaming Regulatory Act and its effects on American Indian economic development. *Journal of Economic Perspectives*, *29*(3), 185–208. https://doi.org/10.1257/jep.29.3.185

Akee, R. K. Q., & Taylor, J. B. (2014). *Social and economic change on American Indian reservations: A databook of the US Censuses and the American Community Survey 1990 – 2010.* The Taylor Policy Group, Inc. https://nnigovernance.arizona.edu/social-and-economic-change-american-indian-reservations-databook-us-censuses-and-american-community

Anderson, T. L. (1995). Chapter 8. Myths, legends, and lessons. In *Sovereign nations or reservations? : An economic history of American Indians* (pp. 161–177). Pacific Research Institute for Public Policy ; Distributed to the trade by National Book Network.

Artz, G. (2003). Rural area brain drain: Is it a reality? *Choices*, *18*(4), 11–15.

Banerjee, A. V., & Duflo, E. (2007). The economic lives of the poor. *Journal of Economic Perspectives*, *21*(1), 141–168. https://doi.org/10.1257/jep.21.1.141

Bardhan, P., & Udry, C. (1999, June 24). *Development microeconomics*. Oxford University Press.

Bartik, T. J. (1991). *Who benefits from state and local economic development policies?* W.E. Upjohn Institute for Employment Research.

Bartik, T. J., Biddle, S. C., Hershbein, B. J., & Sotherland, N. D. (2018). WholeData: Unsuppressed county business patterns data: Version 1.0 [dataset]. https://upjohn.org

Bird, M. Y. (1999). What we want to be called: Indigenous peoples' perspectives on racial and ethnic identity labels. *American Indian Quarterly*, *23*(2), 1–21. https://doi.org/10.2307/1185964

Bureau of Indian Affairs. (n.d.). *Converting fee land into trust land and the associated economic benefits*. U.S. Department of the Interior. https://www.bia.gov/sites/default/files/dup/assets/as-ia/ieed/pdf/Fee_to_Trust.pdf

Burgess, R., & Pande, R. (2005). Do rural banks matter? Evidence from the Indian Social Banking Experiment. *American Economic Review*, *95*(3), 780–795. https://doi.org/10.1257/0002828054201242

Carlson, L. A. (1981). *Indians, bureaucrats, and land: The Dawes Act and the decline of Indian farming*. Greenwood Press.

Carpenter, C. W., Van Sandt, A., & Loveridge, S. (2022). Measurement error in US regional economic data. *Journal of Regional Science*, *62*(1), 57–80. https://doi.org/10.1111/jors.12551

CDFI Fund. (2024). *CDFI program | Community development financial institutions fund* [U.S. Department of the Treasury Community Development Financial Institutions Fund]. Retrieved June 21, 2024, from https://www.cdfifund.gov/programs-training/programs/cdfi-program

Census Bureau. (n.d.). *Poverty thresholds*. Retrieved May 1, 2024, from https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-thresholds.html

Census Bureau. (2021). Business dynamics statistics. https://www.census.gov/programs-surveys/bds.html

Champagne, D. (1992). Economic culture, institutional order, and sustained market enterprise: Comparisons of historical and contemporary American Indian cases. In T. L. Anderson (Ed.), *Property rights and Indian economies* (pp. 195–213). Rowman & Littlefield.

Conner, T. W., & Franklin, A. L. (2019). 20 years of Indian gaming: Reassessing and still winning. *Social Science Quarterly*, *100*(3), 793–807. https://doi.org/10.1111/ssqu.12610

Cornell, S., Jorgensen, M., Wilson Record, I., & Timeche, J. (2007). Chapters 8. Citizen entrepreneurship an underutilized development resource. In M. Jorgensen (Ed.), *Rebuilding native nations: Strategies for governance and development* (p. 197). University of Arizona Press.

Cornell, S., & Kalt, J. P. (1992a). Culture and institutions as public goods: American Indian economic development as a problem of collective action. In T. L. Anderson (Ed.), *Property rights and Indian economies* (pp. 215–252). Rowman & Littlefield.

Cornell, S., & Kalt, J. P. (1992b). Reloading the dice: Improving the chances for economic development on American Indian reservations. In *What can tribes do? Strategies and* *institutions in American Indian economic development* (p. 336). American Indian Studies Center, University of California, Los Angeles. Retrieved June 1, 2023, from https://www.jstor.org/stable/1185252?origin=crossref

Cornell, S., & Kalt, J. P. (1995). Where does economic development really come from? Constitutional rule among the contemporary Sioux and Apache. *Economic Inquiry*, *33*(3), 402–426. https://doi.org/10.1111/j.1465-7295.1995.tb01871.x

Cowan, M. J. (2021, September). *State-Tribal tax compacts: Stories told and untold* (No. 2021-01). Federal Reserve Bank of Minneapolis Center for Indian Country Development.

Dell, M. (2010). The persistent effects of Peru's mining Mita. *Econometrica*, *78*(6), 1863–1903. https://doi.org/10.3982/ECTA8121

DiNardo, J., Fortin, N. M., & Lemieux, T. (1996). Labor market institutions and the distribution of wages, 1973-1992: A semiparametric approach. *Econometrica*, *64*(5), 1001–1044. https://doi.org/10.2307/2171954

Dippel, C. (2014). Forced coexistence and economic development: Evidence from Native American reservations. *Econometrica*, *82*(6), 2131–2165. https://doi.org/10.3982/ECTA11423

Duarte, M. E. (2017, July 11). *Network sovereignty: Building the Internet across Indian Country*. University of Washington Press.

Feir, D. L. (2022, January 10). *Doing business in Indian Country*. Retrieved March 31, 2024, from https://www.minneapolisfed.org/article/2022/doing-business-in-indian-country

Feir, D. L., & Gillezeau, R. (2023). The slaughter of the bison and reversal of fortunes on the Great Plains. *Review of Economic Studies*.

Ferguson, T. J., Snipp, C. M., & Seciwa, C. (1988). Chapter 5. Twentieth century Zuni political and economic development in relation to federal Indian policy. In C. M. Snipp (Ed.), *Public policy impacts on american indian economic development.* (1. ed., 1. print, pp. 113–144). Univ. of New Mexico.

Frye, D., & Parker, D. P. (2021). Indigenous self-governance and development on American Indian reservations. *AEA Papers and Proceedings*, *111*, 233–237. https://doi.org/10.1257/pandp.20211099

Gallagher, R. M. (2014). An examination of cannibalization effects within the riverboat gaming industry: The case of Illinois-area casinos. *Growth and Change*, *45*(1), 41–59. https://doi.org/10.1111/grow.12029

Goetting, M. A., & Ruppel, K. (2009, March). How reservation land is owned by individuals. Retrieved March 31, 2024, from https://www.montana.edu/estateplanning/factsheets/factsheet3.pdf

Hicks, M. J. (2014). Do good fences make good neighbors? The cross border impact of casino entrance. *Growth and Change*, *45*(1), 5–20. https://doi.org/10.1111/grow.12031

Humphreys, B. R., & Soebbing, B. P. (2014). Access to legal gambling and the incidence of crime: Evidence from Alberta. *Growth and Change*, *45*(1), 98–120. https://doi.org/10.1111/grow.12034

Indian Health Service. (n.d.). *Coronavirus | Indian Health Service (IHS)* [Coronavirus]. Retrieved May 29, 2023, from https://www.ihs.gov/coronavirus/

Internal Revenue Service. (n.d.). *What tax credits are available to businesses that employ Native Americans?* Retrieved March 31, 2024, from https://www.irs.gov/government-entities/indian-tribal-governments/

LaPlante, A., & Wheeler, L. (2024, February 21). *Native entrepreneurs face credit-access challenges*. Retrieved March 31, 2024, from https://www.minneapolisfed.org/article/2024/native-entrepreneurs-face-credit-access-challenges

Leahy, T., & Wilson, R. (2008). *Historical dictionary of Native American movements*. Scarecrow Press. http://ebookcentral.proquest.com/lib/michstate-ebooks/detail.action?docID=467111

Leal, A., López-Laborda, J., & Rodrigo, F. (2014). The inside and outside revenue impact of regional gambling taxes in Spain. *Growth and Change*, *45*(1), 79–97. https://doi.org/10.1111/grow.12033

Livingston, R., Daily, R. S., Guerrero, A. P. S., Walkup, J. T., & Novins, D. K. (2019). No Indians to spare: Depression and suicide in Indigenous American children and youth. *Child and Adolescent Psychiatric Clinics*, *28*(3), 497–507. https://doi.org/10.1016/j.chc.2019.02.015

Loveridge, S., & Nizalov, D. (2007). Operationalizing the entrepreneurial pipeline theory: An empirical assessment of the optimal size distribution of local firms. *Economic Development Quarterly*, *21*(3), 244–262. https://doi.org/10.1177/0891242407301449

Manson, S., Schroeder, J., Van Riper, D., Knowles, K., Kugler, T., Roberts, F., & Ruggles, S. (2023). IPUMS national historical geographic information system: Version 18.0 [dataset]. https://doi.org/http://doi.org/10.18128/D050.V18.0

Moretti, E. (2011). Local labor markets. In *Handbook of labor economics* (pp. 1237–1313, Vol. 4). Elsevier. https://doi.org/10.1016/S0169-7218(11)02412-9

National Congress of American Indians. (2020, February). Tribal Nations and the United States: An introduction. https://archive.ncai.org/about-tribes

National Indian Gaming Commission. (n.d.). *History | National Indian Gaming Commission*. Retrieved June 1, 2023, from https://www.nigc.gov/commission/history

Silverman, B. W. (2018). *Density estimation for statistics and data analysis* (First edition). CRC Press.

Small-Rodriguez, D., & Akee, R. (2021). Identifying disparities in health outcomes and mortality for American Indian and Alaska Native populations using tribally disaggregated vital statistics and health survey data. *American Journal of Public Health*, *111*, S126–S132. https://doi.org/10.2105/AJPH.2021.306427

Stephens, H. M., Partridge, M. D., & Faggian, A. (2013). Innovation, entrepreneurship and economic growth in lagging regions. *Journal of Regional Science*, *53*(5), 778–812.

The Editors of Encyclopaedia Britannica. (n.d.). *Indian Reorganization Act | History & outcome | Britannica*. Retrieved June 1, 2023, from https://www.britannica.com/topic/Indian-Reorganization-Act

Treuer, D. (2019). *The heartbeat of Wounded Knee: Native America from 1890 to the present*. Riverhead Books.

University of Alaska Fairbanks. (n.d.). *Termination era, the 1950s, Public Law 280 | tribal governance*. Retrieved February 27, 2024, from https://www.uaf.edu/tribal/academics/112/unit-2/terminationerathe1950spubliclaw280.php

Walker, D. M., & Nesbit, T. M. (2014). Casino revenue sensitivity to competing casinos: A spatial analysis of Missouri. *Growth and Change*, *45*(1), 21–40. https://doi.org/10.1111/grow.12035

Wenz, M. (2014a). Casinos, gambling, and economic development: An introduction to the special issue. *Growth and Change*, *45*(1), 1–4. https://doi.org/10.1111/grow.12037

Wenz, M. (2014b). Valuing casinos as a local amenity. *Growth and Change*, *45*(1), 136–158. https://doi.org/10.1111/grow.12036

Woodland Financial Partners. (n.d.). *FAQs* [Woodland financial]. Retrieved March 31, 2024, from https://woodlandfinancial.org/about/faqs/

Wooldridge, J. M. (2010). *Econometric analysis of cross section and panel data, second edition*. MIT Press.

APPENDIX 3A

ROBUSTNESS CHECKS

I conducted two different sets of robustness checks to test the sensitivity of the results. For the first set, I use the percent of a county covered by tribal tracts (as defined in Section 3.4.1) as the independent variable of interest, rather than the indicator of whether a county contains or intersects with tribal land. I find that the percent tribal measure has the same sign and significance as the tribal indicator but has a statistically significant and small in magnitude quadratic relationship. For the second set, I use subsets of nontribal counties as comparison groups rather than the whole universe of them. I find that the regression results are similar when restricting the nontribal comparison groups to counties within states that contain tribal counties but are larger in magnitude when restricting the comparison group to nontribal counties outside of those states. Kernel density estimates exhibit similar behaviors across the different measures of business activity.

3A.1 Percent Tribal

During the construction of the tribal indicator, I created an intermediate measure using what are defined as tribal tracts. The intermediate measure, henceforth Percent Tribal, is the sum of square miles covered by tribal tracts in each county divided by the total area of the county and then multiplied by 100. Using this measure has two benefits: I can estimate the marginal association of percent tribal land and I can estimate whether this association has a quadratic functional form. Therefore, I can find statistical evidence that suggests there may exist a maximum or minimum association for a mix of nontribal and tribal land in a county. However, this approach remains agnostic on how the lands are used in a county, which is a limitation to this analysis.

For each measure of business activity, I estimate the relationship using all counties in the analysis and just tribal counties. For the measures of population-adjusted firms, establishment entry, establishment exits, and establishment net entry, the estimates on the linear term are similar in sign and statistical significance as those in Table 3.1. However, I see a small in magnitude but statistically significant negative estimate on the quadratic term, suggesting that the partial association diminishes as the county is covered with more tribal areas. Across these four outcome variables, the point estimates are similar when I restrict the analysis to only tribal counties, apart from population-adjusted firm counts which attenuates towards zero.

The quadratic associations for establishment HHI and employment HHI exhibit similar signs and significance to the estimates in Table 3.1 and show a statistically significant quadratic term. However, the point estimates are very sensitive to the change in sample.

3A.2 Different Comparison Groups

In the analysis presented in the main text, I used statistical techniques to compare tribal counties against the universe of nontribal counties over time. While it is difficult to compare tribal areas to corresponding areas in the United States because of the different governance and regulatory environments, I may be able to better estimate the true difference between tribal and nontribal areas using a subset of counties.

In the following robustness checks, I use two different subsets of nontribal counties: nontribal counties that are in states with tribal lands, and nontribal counties in states without tribal lands. The first subset is used because often states and tribes use compacts to harmonize their public policy such as tax rates (Cowan, 2021). Therefore, nontribal counties that exist in the same states as their tribal counterparts may be a better comparison group. Alternatively, using nontribal counties from states without tribal areas may reduce the risk of spatial spillovers or dependence across observations.

I reconduct all statistical analyses for the two subsets of nontribal counties. In the regression results, the estimates of the marginal effect of being tribal using the in-state nontribal counties are like the ones displayed in Table 3.1, while estimates from the out-of-state nontribal counties are often larger in magnitude. This pattern is also apparent in how the marginal effect of being tribal changes over time. The kernel density estimates have a similar pattern, but the in-state nontribal counties seem to have more similar distributions to the tribal counties compared to the results in the main text. Therefore, one may treat the in-state nontribal estimates as conservative. All in all, the sign and significance of all are estimates are the same across the robustness checks and the analysis presented in the main body of the text.

3A.3 Heterogeneity Across Rurality

As noted in Section 3.2.2, tribes face similar hurdles in economic development as rural and remote areas. However, tribal lands in the United States are not intrinsically rural: 65% of tribal counties are metropolitan, metro-adjacent, or micropolitan using my specification. Therefore, it is important to consider whether higher instances of business activity is uniform across tribal areas. Table 3B.7 presents the APE results across different rural designations. The underlying regressions are like those presented in Table 3.1 except the tribal indicator is interacted with rural designations rather than years. To promote easy interpretation, the parameters presented are not the regression coefficients: they are the average partial effects at the fixed rural designation. For example, Metro Tribal APE is the effect of being a tribal county holding the metropolitan designation fixed. Therefore, it is a direct comparison between counties at the same rural designation.

Similar to Table 3.1, the pooled OLS estimates are more conservative than the CRE estimates. This is especially striking for the estimates on population-adjusted firm counts in Column (1) and Column (2). All CRE estimates are statistically significant at conventional levels, but only the estimate for rural counties is significant using pooled OLS. Both estimates are for the most part positive, indicating that tribal counties on average have more firms than nontribal counties across all rural designations. This is especially interesting for the previously mentioned Rural Tribal APE: given the same constraints due to remoteness, rural tribes have between 0.8 and 1.8 more firms.

For the measures on establishment entry and exit (Table 3B.7, Columns 3-8), the rural designations that are most often statistically significant across both estimators are Metro-Adjacent and Rural. They are again positive, suggesting establishments open and close more frequently in tribal counties. Estimates for Net Entry for pooled OLS (Column 7) are not statistically significant across all rural designations. It seems that despite more business churn in tribal counties, they still have similar net entry statistics across the rural hierarchy.

Finally, Column 9 through Column 12 in Table 3B.7 present the estimates for industrial diversity. It seems that most of the industrial diversity differences present in Table 3.1 are due to metropolitan tribes; most estimates are not statistically significant for metro-adjacent, micropolitan, and rural areas. Perhaps exclusion from agglomeration benefits affect industrial diversity more strongly than nuances in tribal entrepreneurship. Metropolitan tribes are less concentrated in their establishments and less concentrated in their employment. This may indicate that people on tribal lands diversify their entrepreneurial activity across more industries, and employees are scattered across more industries due to agglomeration.

APPENDIX 3B

ADDITIONAL TABLES AND FIGURES

Table 3B.1 North American Industrial Classification Codes

2-Digit Code	**Industry**
11	Agriculture, Forestry, Fishing and Hunting
21	Mining, Quarrying, and Oil and Gas Extraction
22	Utilities
23	Construction
31-33	Manufacturing
42	Wholesale Trade
44-45	Retail Trade
48-49	Transportation and Warehousing
51	Information
52	Finance and Insurance
53	Real Estate and Rental and Leasing
54	Professional, Scientific, and Technical Services
55	Management of Companies and Enterprises
56	Administrative and Support and Waste Management and Remediation Services
61	Educational Services
62	Health Care and Social Assistance
71	Arts, Entertainment, and Recreation
72	Accommodation and Food Services
81	Other Services (except Public Administration)
92	Public Administration
99	Nonclassifiable Establishments

Table 3B.2 Summary Statistics for Firm Sample

	All	NonTribal	Tribal	Diff. in means
Firms per capita	19.54	19.21	21.49	***
	(6.574)	(6.452)	(6.959)	
Unemployment Rate	6.07	5.99	6.54	***
	(2.851)	(2.764)	(3.288)	
Personal Income Per Capita	29.10	29.05	29.34	**
	(12.15)	(12.16)	(12.11)	
Metropolitan	0.23	0.24	0.16	***
	(0.418)	(0.426)	(0.362)	
Metro-Adjacent	0.33	0.34	0.30	***
	(0.471)	(0.472)	(0.460)	
Micropolitan	0.13	0.12	0.14	***
	(0.333)	(0.330)	(0.350)	
Rural	0.32	0.30	0.40	***
	(0.465)	(0.459)	(0.490)	

Note: Means are presented with standard deviations in parenthesis. The "All" column provides means and standard deviations for the entire pooled sample. "Nontribal" and "Tribal" columns present the same summary statistics for the observations in the sample categorized as such. Difference in means displays the statistical significance of the difference in means using Welch's t-tests. *** p<0.01, ** p<0.05, * p<0.10.

Table 3B.3 Summary Statistics for Establishment Sample

	All	NonTribal	Tribal	Diff. in means
Estab. Entries per capita	2.09	2.04	2.37	***
	(0.949)	(0.929)	(1.020)	
Estab. Exits per capita	1.97	1.93	2.21	***
	(0.805)	(0.793)	(0.835)	
Net Entries per capita	0.12	0.11	0.16	***
	(0.637)	(0.625)	(0.702)	
Unemployment Rate	6.12	6.03	6.67	***
	(2.798)	(2.700)	(3.280)	
Personal Income Per Capita	29.28	29.28	29.30	
	(12.00)	(12.07)	(11.54)	
Metropolitan	0.25	0.26	0.17	***
	(0.432)	(0.439)	(0.378)	
Metro-Adjacent	0.34	0.35	0.32	***
	(0.474)	(0.475)	(0.466)	
Micropolitan	0.14	0.14	0.16	***
	(0.346)	(0.343)	(0.366)	
Rural	0.27	0.26	0.35	***
	(0.444)	(0.437)	(0.477)	

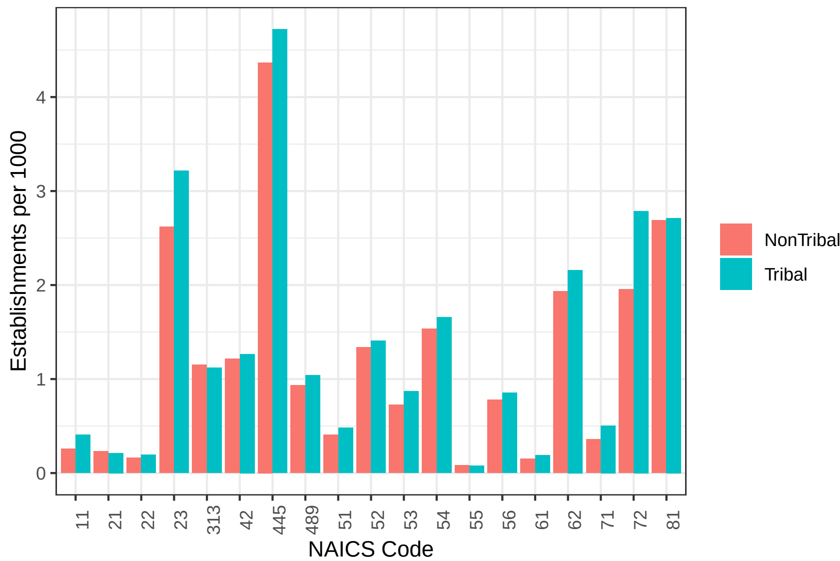
Note: Means are presented with standard deviations in parenthesis. The "All" column provides means and standard deviations for the entire pooled sample. "Nontribal" and "Tribal" columns present the same summary statistics for the observations in the sample categorized as such. Difference in means displays the statistical significance of the difference in means using Welch's t-tests. *** p<0.01, ** p<0.05, * p<0.10.

Table 3B.4 Summary Statistics for HHI Sample

	All	NonTribal	Tribal	Diff. in means
Emp. HHI	1507.48	1511.58	1482.91	***
	(607.5)	(603.1)	(632.9)	
Estab. HHI	1035.54	1037.49	1023.91	***
	(210.8)	(218.3)	(158.2)	
Unemployment Rate	6.31	6.27	6.57	***
	(2.712)	(2.648)	(3.052)	
Personal Income Per Capita	32.63	32.58	32.96	***
	(10.56)	(10.58)	(10.43)	
Metropolitan	0.23	0.25	0.16	***
	(0.423)	(0.431)	(0.366)	
Metro-Adjacent	0.34	0.34	0.32	**
	(0.472)	(0.473)	(0.468)	
Micropolitan	0.13	0.13	0.15	***
	(0.341)	(0.339)	(0.353)	
Rural	0.30	0.28	0.37	***
	(0.457)	(0.451)	(0.483)	

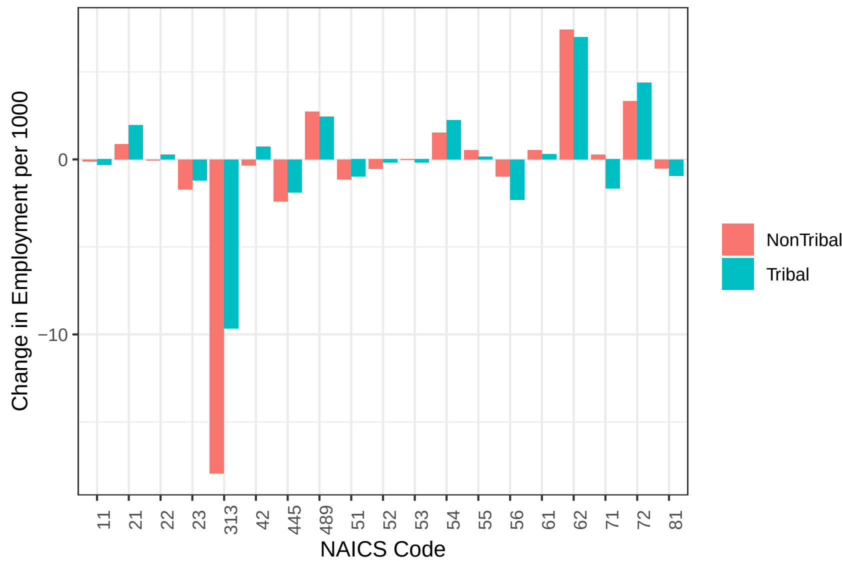
Note: Means are presented with standard deviations in parenthesis. HHI measures are calculated using Equation 3.1. The “All” column provides means and standard deviations for the entire pooled sample. “Nontribal” and “Tribal” columns present the same summary statistics for the observations in the sample categorized as such. Difference in means displays the statistical significance of the difference in means using Welch’s t-tests. *** p<0.01, ** p<0.05, * p<0.10.

Figure 3B.1 Establishments per 1000 in Different Industries, 2000



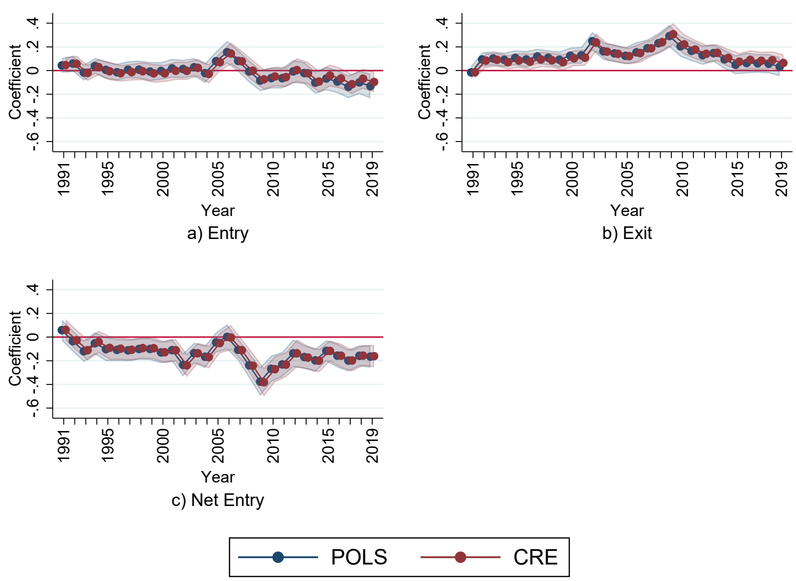
Source: WholeData, Upjohn Institute. NAICS Codes correspond to the industries listed in Table 3B.1.

Figure 3B.2 Average Change in Employment per 1000 in Different Industries, 2000-2016



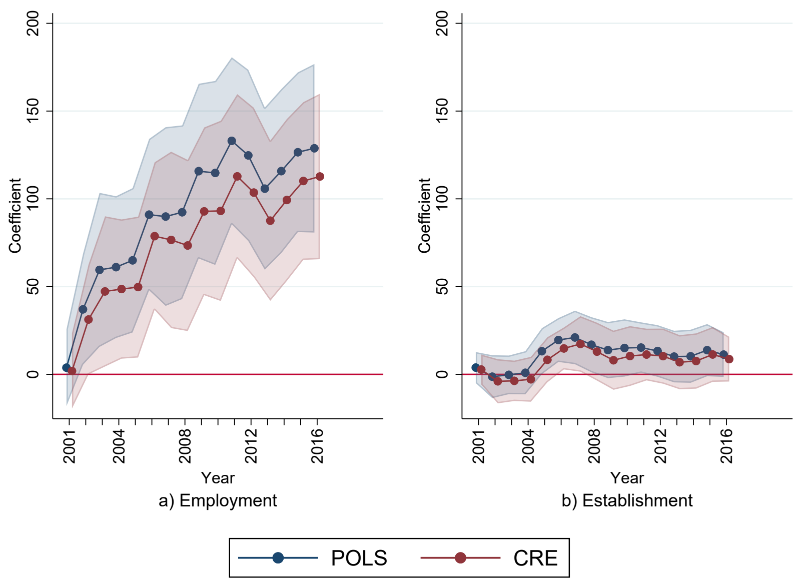
Source: WholeData, Upjohn Institute. NAICS Codes correspond to the industries listed in Table 3B.1. Change in employment is calculated by subtracting population adjusted employment counts in 2000 from population adjusted counts in 2016.

Figure 3B.3 Coefficients on Year-Tribal Interactions, Establishment Entry, Exits, and Net Entry



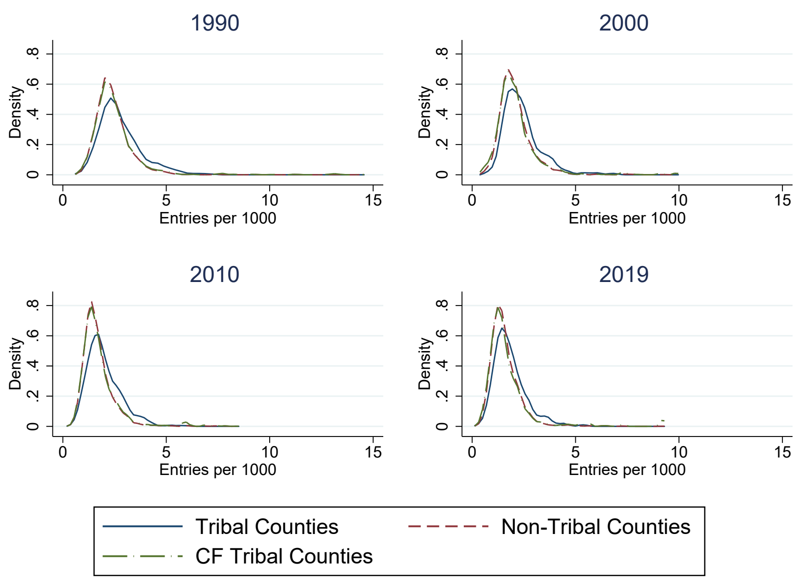
Source: Study Regression Coefficients. Coefficients are obtained by estimating Equation 3.3 using pooled OLS and Correlated Random Effects. Point estimates are represented by the connected points in the figure. Standard errors are represented by the shaded area around the parameter estimates.

Figure 3B.4 Coefficients on Year-Tribal Interactions, Employment HHI and Establishment HHI



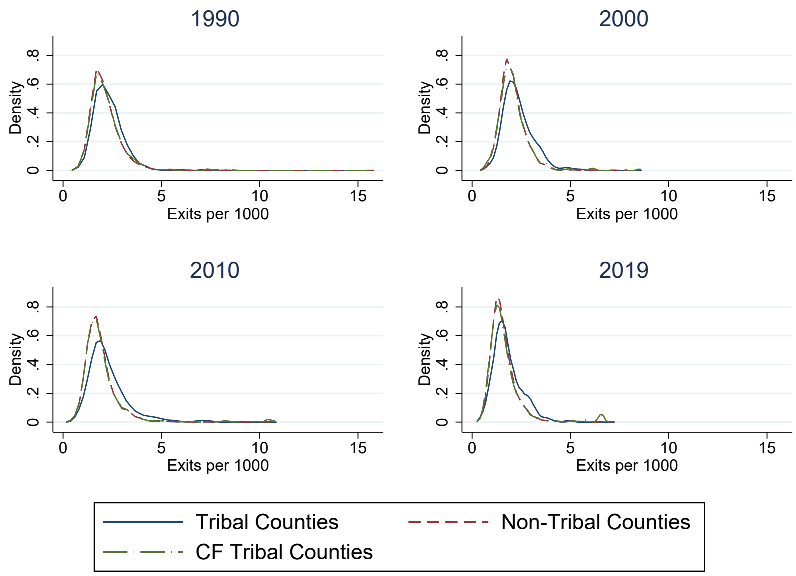
Source: Study Regression Coefficients. Coefficients are obtained by estimating Equation 3.3 using pooled OLS and Correlated Random Effects. Both the employment and establishment HHI measures are calculated using Equation 3.1. Point estimates are represented by the connected points in the figure. Standard errors are represented by the shaded area around the parameter estimates.

Figure 3B.5 Kernel Density Estimates - Establishment Entry



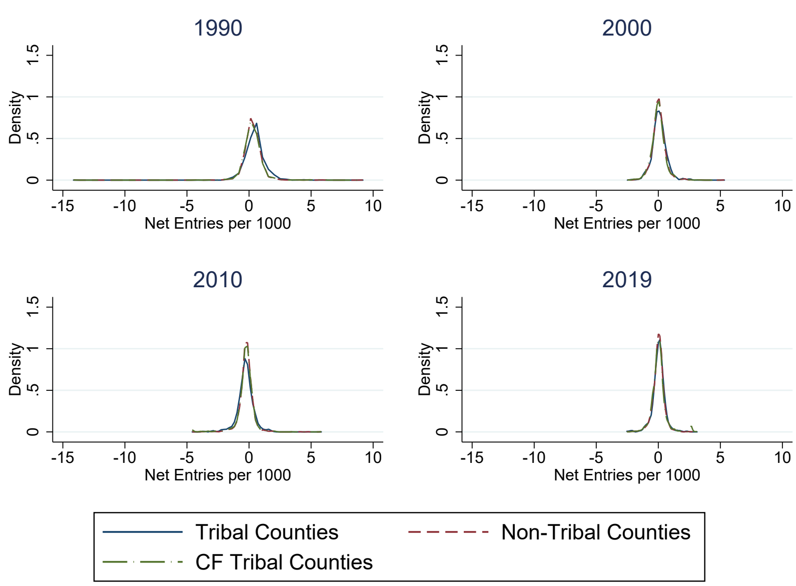
Source: Study Kernel Density Estimates. Kernel densities are estimated using the procedure outlined in Section 3.5.2. CF Tribal Counties are counterfactual kernel densities estimated using propensity score weighting outlined in in Section 3.5.2.

Figure 3B.6 Kernel Density Estimates - Establishment Exit



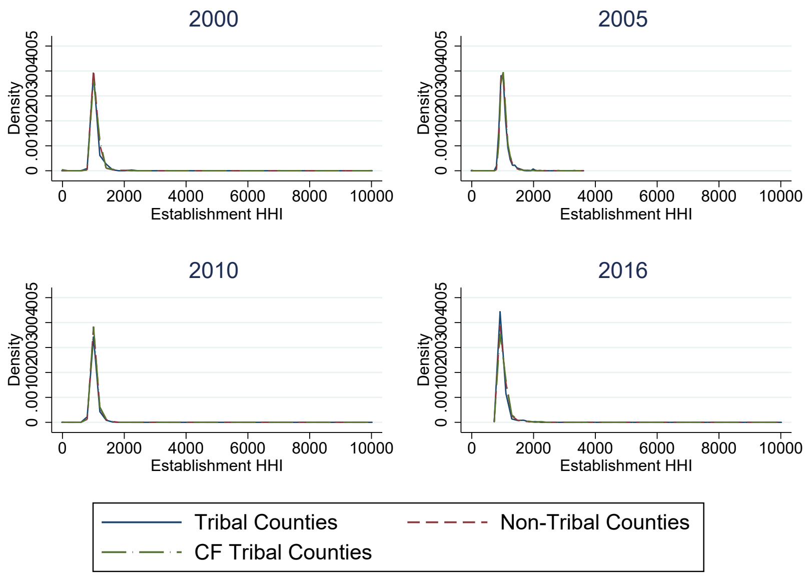
Source: Study Kernel Density Estimates. Kernel densities are estimated using the procedure outlined in Section 3.5.2. CF Tribal Counties are counterfactual kernel densities estimated using propensity score weighting outlined in in Section 3.5.2.

Figure 3B.7 Kernel Density Estimates - Establishment Net Entry



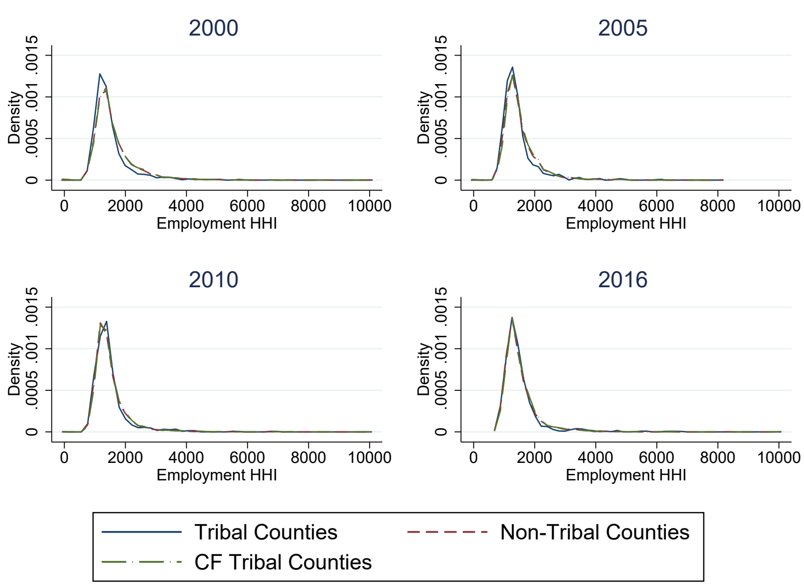
Source: Study Kernel Density Estimates. Kernel densities are estimated using the procedure outlined in Section 3.5.2. CF Tribal Counties are counterfactual kernel densities estimated using propensity score weighting outlined in in Section 3.5.2.

Figure 3B.8 Kernel Density Estimates - Establishment HHI



Source: Study Kernel Density Estimates. Kernel densities are estimated using the procedure outlined in Section 3.5.2. CF Tribal Counties are counterfactual kernel densities estimated using propensity score weighting outlined in in Section 3.5.2.

Figure 3B.9 Kernel Density Estimates - Employment HHI



Source: Study Kernel Density Estimates. Kernel densities are estimated using the procedure outlined in Section 3.5.2. CF Tribal Counties are counterfactual kernel densities estimated using propensity score weighting outlined in in Section 3.5.2.

Table 3B.5 Establishment Diversity Metrics for Top 5 and Bottom 5 Tribal Counties, 2016

County	HHI	Accommodations Total	Accommodations Share (%)	Top Industry
Top 5 Counties				
1. Dukes County, MA	1148.12	144	13.32	Construction
2. Jones County, SD	1831.6	17	35.42	Accommodations
3. Summit County, UT	1010.52	198	8.39	Professional Services
4. Cook County, MN	1258.32	60	21.66	Accommodations
5. Grand County, UT	1091.84	96	21.01	Accommodations
Bottom 5 Counties				
5. Pinal County, AZ	895.86	348	10.01	Retail Trade
4. Apache County, AZ	1128.39	56	12.7	Retail Trade
3. Todd County, SD	1495.2	2	3.7	Retail Trade
2. Buffalo County, SD	2066.12	1	9.09	Health Care
1. Oglala Lakota County, SD	1184.41	10	13.89	Retail Trade

Note: The top and bottom five tribal counties are determined by their population-adjusted firm counts in 2019. Employment HHI is calculated following Equation 3.1. Total lists the total number of establishments in the county belonging to the Accommodations sector (NAICS 72). Share is the percent of establishments belonging to the Accommodations sector in the county. Top Industry is the two-digit NAICS industry with the most establishments in the county.

Table 3B.6 Employment Diversity Metrics for Top 5 and Bottom 5 Tribal Counties, 2016

County	HHI	Accommodations Total	Accommodations Share (%)	Top Industry
Top 5 Counties				
1. Dukes County, MA	1123.33	719	12.68	Retail Trade
2. Jones County, SD	2215.4	79	28.62	Retail Trade
3. Summit County, UT	1301.95	6885	24.96	Accommodations
4. Cook County, MN	1730.2	678	33.56	Accommodations
5. Grand County, UT	2114.37	1803	40.59	Accommodations
Bottom 5 Counties				
5. Pinal County, AZ	1142.96	6930	14.19	Retail Trade
4. Apache County, AZ	2212.24	825	12.27	Health Care
3. Todd County, SD	1964.47	17	1.74	Retail Trade
2. Buffalo County, SD	3633.33	95	52.78	Accommodations
1. Oglala Lakota County, SD	2964.43	235	12.65	Educational Services

Note: The top and bottom five tribal counties are determined by their population-adjusted firm counts in 2019. Employment HHI is calculated following Equation 3.1. Total lists the total number of employees in the county working in the Accommodations sector (NAICS 72). Share is the percent of employees working in Accommodations in the county. Top Industry is the two-digit NAICS industry with the most employees in the county.

Table 3B.7 Tribal Average Partial Effects Across Rurality

	Firms		Estab. Entry		Estab. Exit		Estab. Net Entry		Estab. HHI		Emp. HHI	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	POLS	CRE	POLS	CRE	POLS	CRE	POLS	CRE	POLS	CRE	POLS	CRE
Metro Tribal APE	-0.204	0.698*	0.017	0.232***	0.030	0.197***	-0.013	0.035	-17.183	-40.976***	-129.084***	-171.168***
	(0.388)	(0.378)	(0.054)	(0.064)	(0.049)	(0.053)	(0.019)	(0.022)	(11.054)	(10.351)	(40.292)	(29.436)
Metro-Adjacent	0.268	1.660***	0.087*	0.288***	0.067*	0.224***	0.019	0.064***	10.905	-13.450	-10.157	-52.072
Tribal APE	(0.334)	(0.322)	(0.049)	(0.053)	(0.041)	(0.041)	(0.018)	(0.022)	(12.045)	(11.202)	(50.999)	(48.353)
Micro Tribal APE	0.774	2.340***	0.091	0.315***	0.065	0.251***	0.026	0.064**	-15.391	-42.026***	-1.193	-69.271
	(0.482)	(0.497)	(0.069)	(0.082)	(0.056)	(0.066)	(0.028)	(0.027)	(10.664)	(9.671)	(55.617)	(51.180)
Rural Tribal APE	0.799*	1.826***	0.124*	0.286***	0.089	0.201***	0.035	0.085***	-15.235	-21.509	-19.033	-7.926
	(0.458)	(0.436)	(0.073)	(0.074)	(0.058)	(0.058)	(0.025)	(0.025)	(14.191)	(15.899)	(54.462)	(52.763)
Observations	89382	89382	79666	79666	79666	79666	79666	79666	51938	51938	51938	51938

Note: *** p<0.01, ** p<0.05, * p<0.10. Standard errors clustered by county in parentheses. Headings of the columns dictate the dependent variable in each regression. Columns (1)-(8) use an analysis period from 1990 to 2019, while Columns (9)-(12) use an analysis period from 2000 to 2016. Rurality Tribal APE is the average partial effect of the tribal indicator at different levels of rurality. For example, "Metro Tribal APE" is the average partial effect of the tribal indicator for metro counties. Firm and Establishment counts are population adjusted (per 1000 people). Pooled Ordinary Least Squares (POLS) are in odd columns, Correlated Random Effects (CRE) are in even. Year dummies, tribal-rurality interactions, unemployment rate, income per capita, state dummies (POLS only), and time-averages (CRE only) are not shown.