

ESSAYS IN HEALTH ECONOMICS

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Eric Scott Wilkinson
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Examining Committee Members:

Johanna Catherine Maclean, Advisory Chair, Department of Economics
Douglas Webber, Department of Economics
Michael Leeds, Department of Economics
Hope Corman, External Member, Rider University

ABSTRACT

In this dissertation, I study three issues in the field of health economics.

Chapter 1, “The Effect of Internet Gambling Laws on Suicide: Evidence From New Jersey,” examines the effect of legalizing Internet gambling on suicide rates following the introduction of legal Internet gambling in New Jersey. The emergence and subsequent rapid growth of Internet gambling has raised significant public health questions and concerns. The relationship between Internet gambling and pathological gambling has been studied extensively. However, the link between them is not well understood. This study exploits a change in the legal status of Internet gambling to estimate the effects of Internet gambling on state level suicide rates using both a differences-in-differences model and a synthetic control model. I find no statistically significant effect of the law on suicides. Secondary analyses using Internet search data find evidence of an effect on mental health and addiction. These results are important because they show that, once endogenous correlation in Internet gambling participation is controlled for, the effects of its legalization on public health may diminish. This is in sharp contrast to the heft of existing literature and may help to better understand the link between Internet gambling and pathological gambling.

Chapter 2, “The Effect of Increased Cost-Sharing on Low-value Service Use,” examines the effect of a value-based insurance design (VBID) program implemented at a large public employer in the state of Oregon. The program substantially increased

cost-sharing for several healthcare services likely to be of low-value for most patients: diagnostic services (e.g., imaging services) and surgeries (e.g., spinal surgeries for pain). Using a differences-in-differences design coupled with granular, administrative health insurance claims data over the period 2008 to 2013, we estimate the change in low-value service use among beneficiaries before and after program implementation relative to a comparison group not exposed to the VBID. Our findings suggest that the VBID significantly reduced the use of targeted services, with implied elasticities of demand somewhat larger than estimates for general healthcare services. We find no evidence that increasing cost-sharing for these low-value services led to substitution to non-targeted services or increased overall healthcare costs. These findings have implications for both public and private healthcare policies as VBID principles are proliferating in United States healthcare markets.

Chapter 3, “The Effect of Mandatory Managed Care on Preventable Hospitalizations for the Aged, Blind, and Disabled Population of Medicaid in New Jersey,” examines the effect of a mandatory transition to Medicaid managed care for the aged, blind, and disabled population in New Jersey Medicaid. Medicaid has grown over the last few decades to a program which now covers one in five Americans and costs over half of one trillion dollars to administer. Medicaid represents the largest item on a state’s budget; the largest share of that money is spent on a small group of high-cost individuals: the disabled. Seeking to expand upon the successes, no matter how limited, and the ability to smooth costs over time, states began to shift these high-cost, complex patients into managed care plans. The evidence on how well these plans can handle the demanding needs of this population is still debated. In this paper, I utilize the variation induced from a shift to mandatory managed care in preventable hospitalizations for the physically and developmentally disabled in New Jersey’s Medicaid program to assess the impact on access to care for this extremely vulnerable population.

Using a difference-in-differences model I find the introduction of managed care reduced the monthly preventable hospitalization rate 6.4%[-11.5,-1.3]. To my knowledge, this would be one of the first causal estimates for this population, and the first for New Jersey.

DEDICATION

To my family, whose support and encouragement made all of this possible, and to my partner, Christopher, whose love and patience motivated me to finish.

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CHAPTER 1

THE EFFECT OF INTERNET GAMBLING LAWS ON SUICIDE: EVIDENCE FROM NEW JERSEY

1.1 Introduction

The gaming industry in the United States has expanded over the last several decades, from a handful of jurisdictions in the 1980's to over 40 states in 2017 (American Gaming Association 2017), with a value of \$ 83 billion dollars annually (Alvarez 2018). When accounting for lotteries, all but two states allow for legal gambling in some form (American Gaming Association 2017). In addition to brick-and-mortar casinos, the expansion of legal Internet gambling opportunities has grown as well, with four states allowing the practice by 2018(American Gaming Association 2018). Although many states have been quick to exploit legal land-based gaming to fill revenue shortfalls, the uptake in Internet gambling has not been as swift.

Public health concerns over the effects of Internet gambling have given law makers reason to pause in enacting legislation to legalize the practice (e.g. Wood and Williams 2007; Gainsbury et al. 2015). However, given that almost every American household has access to high speed Internet (Ryan and Lewis 2017) and that current state and federal laws prohibiting Internet gambling are seemingly unenforceable (Wood

and Williams 2007; Wood and Williams 2011), the demand for Internet gambling is growing, regardless of its legal status. Additionally, without regulation, the industry has engaged in unscrupulous practices (e.g. McCormack and Griffiths 2013; Gainsbury et al. 2012).

There is a substantial literature on the effects of problem gambling and its relation to Internet gambling. Internet gambling has many unique features not found in other forms of gambling. These structural and situational features include 24-hour access, total anonymity, rapid pace of play, and the ability to provide an immersive environment for the player (Gainsbury et al. 2012; Gainsbury 2015; Petry and Gonzalez-Ibanez 2015; McCormack and Griffiths 2013). Rates of problem gambling and pathological gambling are almost universally found to be substantially higher for Internet gamblers than for non-Internet gamblers (e.g. Petry and Gonzalez-Ibanez 2015; Nower, Caler, and Peters 2017). Many researchers thus regard Internet gambling as one of the most lethal forms of gambling and give the practice particular scrutiny. Although a strong relationship can be seen between Internet gambling and pathological gambling, there is little evidence on how this relationship works and in which direction. To date, only one study has provided causal evidence on the direction of this relationship. Thus, there is a need for additional work (Philander and MacKay 2014).

Problem gambling in the United States is a serious public health issue. Studies on the prevalence of problem and disordered gambling in the general population have found rates as high as 8.1% (Williams 2012). The availability of gambling opportunities has grown significantly, with commercial and tribal gambling now present in all but 10 states (American Gaming Association 2017). Internet gambling gained legal status in 4 states as of 2018 and is expected to grow further, particularly as a conduit to implementing sports betting given the recent Supreme Court decision striking down federal prohibition of gambling on sporting events (*Murphy v. National*

Collegiate Athletic Association 2018; American Gaming Association 2017). Following the legalization of Internet gambling in New Jersey, the uptake has been swift and the growth, strong (Nower, Caler, and Guan 2016; Nower, Caler, and Peters 2017; American Gaming Association 2017). Though small in number, problem gamblers generate up to one-third of all government revenues collected from gambling. Furthermore, only a small portion of those revenues are used in the prevention and treatment of disordered gambling, with one study finding this portion to be only 0.045% of total state-level, direct gambling-related revenues (Wood and Williams 2007; Lesieur 1998).

In a recent study of the prevalence of gambling in NJ by Nower and Volberg (2017), nearly 70% of respondents reported gambling in the last 12 months, with 24.5% of gamblers, or 17.1% of total respondents, having reported betting over the Internet¹. The rise of Internet gambling is not without its costs. In New Jersey, 63.1% of respondents reporting having bet online were considered at least low-risk problem gamblers, with 28.3% probable disordered gamblers. These figures were substantially higher than land-only gamblers, reporting 19.7% and 2.7%, respectively. Of all gamblers in the study, those gambling online displayed the highest levels of suicidality in the sample. Of those having reported any Internet gambling, 8.7% reported having suicidal ideation, with 5.5% actually having attempted. When compared to non-gamblers, reporting 2.3% and 0.3% respectively, and land-only gamblers, reporting 1.7% and 0.4% respectively, Internet gambling appears to be, at the very least anecdotally, more dangerous than other forms.² An interesting finding from the study was that nearly 35% of respondents having gambled online did so solely because Internet gambling became legal. If those who gambled online as a result of the law developed

¹This includes both online-only gamblers and mixed-venue gamblers. Each subsequent reference to “online gamblers” in the context of Nower and Volberg (2017) will reflect this distinction. For the purposes here, it is not necessary to distinguish between the two categories. However, the literature indicates such a distinction must be made in more detailed analyses.

²For a more in-depth analysis and complete study findings, please see Nower and Volberg (2017).

new or worsened existing gambling problems, a measurable effect on suicidality may potentially be found.

The most recent literature has disputed the idea that individual gambling activities (e.g. Internet gambling) pose a unique risk. These studies argue, instead, that the cumulative involvement in gambling activities poses the actual risk of problem gambling (e.g. LaPlante et al. 2009; Welte et al. 2009). Only one of these studies presents causal findings, of which the results are suspect. The new focus on involvement assumes that one's level of involvement is exogenous in nature, though this likely not the case.

In this study, I exploit a change in the legal status of Internet gambling resulting from a novel law in New Jersey to estimate the causal effect of Internet gambling on mental health, namely the suicide rate, using a difference-in-difference framework. I find no statistically significant effect of Internet gambling on the suicide rate. Outside of very small effects, I find no evidence of an effect of this law. This is especially evidenced in the primary model's Poisson estimation, which places the effect of the law in the interval $[-0.09\%, 0.14\%]$ over pre-treatment means. There are several explanations for this result. The first, consistent with Philander and MacKay (2014), is that Internet gambling may pose a lesser risk than other gambling activities, especially given the levels of consumer protections afforded by the law in question (The State of New Jersey 2013; Nower, Caler, and Peters 2017), especially if existing Internet gamblers substitute their illegal Internet gambling with legal gambling that offer these protections. Another possible explanation is that the time frame to observe an effect on suicidality is outside that of this study. This study provides an important contribution to the literature by estimating causal effects using recent administrative and the only study to date that circumvents the potential endogeneity of gambling involvement.

1.2 Previous Literature and Background

1.2.1 Previous Literature

The previous literature³ on the link between Internet gambling and suicide is scant. In this section, I frame the current research question in the context of existing literature and focus on the following topic areas: the effects of gambling opportunities, the effects of disordered gambling, the effects of Internet gambling, differences between Internet gambling and other forms of gambling, and the role of gambling involvement.

The question of how increasing gambling opportunities affects public health has been explored extensively through the lens of multiple disciplines. Some of the earliest of these studies focused on the experience in jurisdictions where casino gambling is legal. In a 1989 study examining mortality counts at Atlantic City casinos, 1989 find that the proportion of suicides to all deaths was twice that of the national level. The finding that casino presence was associated with elevated levels of suicide is not unique. Studies have found suicide rates in areas where casino gambling is legal to be elevated compared to controls and in some cases to be significantly elevated (Phillips, Welty, and Smith 1997; McCleary et al. 2002). In a related study examining the relationship between video poker machines and the number of Gamblers Anonymous groups, Campbell and Lester (1999) find a modest positive correlation in Louisiana parishes, suggesting at the very least a small, positive relationship between gambling opportunity and disordered gambling.

One of the largest topic areas in the gambling research examines the effect of pathological gambling⁴ on individual and family health, with the majority focusing

³The figures in this section refer to correlation unless noted otherwise.

⁴In this study, unless noted otherwise, following the definitions given by Welte et al. (2015) the term pathological or disordered gambler is given to those individuals displaying 5 or more symptoms given by either the South Oaks Gambling Screen (SOGS) or outlined in the DSM-IV. The term problem gambler is given to those with 3 or more symptoms on either scale. Thus, pathological gamblers are a subset of problem gamblers.

on suicidality⁵. There is, however, considerable variation in the findings of these studies. A significant challenge facing researchers is data availability and reliability. Almost every study on this topic relies on self-reported survey data, many relying on samples of in-treatment pathological gamblers, introducing the possibility for significant selection bias. The variation in findings may reflect these differences in samples. The one common finding is that a link between pathological gambling and suicide exists, and that this link is not well understood (Hodgins, Mansley, and Thygesen 2006).

Measuring the effects of pathological gambling on suicidality is difficult given the significant variability in the severity of the condition. To fully capture the breadth of the issue, it is necessary to distinguish the severity of intent (e.g. thoughts of hopelessness and wanting to die versus actually making plans to, and further following through on self-harm by making an actual attempt). This has been studied extensively, and the results vary considerably. Pathological gamblers are significantly more likely to have thoughts of hopelessness and feelings of “wanting to die.” Between 48% and 80% of pathological gamblers report having these thoughts (Lesieur 1998).

Suicidal ideation is the condition in which an individual has thoughts of self-harm and suicide. This includes fantasies, playing out scenarios in one’s head, etc. Pathological gamblers report ideation at rates of 7.8% to as high as 80%, which is as high as 68 percentage points greater than in the general population whose rates fall between 5% and 18% (Lesieur 1998; Lloyd et al. 2016; Maccallum and Blaszczyński 2003). An individual having made plans on how they would self-harm is a considerably more severe condition than the thoughts themselves. Pathological gamblers report having made self-harm plans at rates between 45-49%, 15 times that of the general popula-

⁵The definition of the term “suicidality” used throughout this paper is given by Slade et al. (2009): “The term suicidality covers suicidal ideation (serious thoughts about taking one’s own life), suicide plans and suicide attempts. People who experience suicidal ideation and make suicide plans are at increased risk of suicide attempts, and people who experience all forms of suicidal thoughts and behaviours are at greater risk of completing suicide.”

tion, with a prevalence of 3% (Lesieur 1998; Maccallum and Blaszczynski 2003).

Suicide attempts were rarer than plans, with between 4% and 36% of pathological gamblers reporting having made an attempt on their life. This is again substantially higher than the 1-5% experienced in the general population (Lesieur 1998; Maccallum and Blaszczynski 2003; Thon et al. 2014; Black et al. 2015). Of these attempters, between 28 and 41.2% did so as a direct result of their pathological gambling (Black et al. 2015; Lloyd et al. 2016). Given the difficulties and constraints in measuring the effect on completed suicides, there are few estimates of the extent to which problem gambling contributes to completed suicides. Maccallum and Blaszczynski (2003) estimate that 1.7% of completed suicides in Australia were the result of gambling related problems. This rate is significantly higher than the recorded 0.01% of the general population that commits suicide in Western industrialized nations.

The effects of pathological gambling go well beyond the individual and affect those around the pathological gambler. Studies have estimated that each Pathological gambler affects between 10 and 17 people, including spouses, children, parents, extended family, and co-workers with several studies indicating a relationship between pathological gambling and family violence (Kalischuk et al. 2006). The financial burden of significant levels of debt is often borne by the Pathological gambler's family. Debt accumulation can lead to more resources being devoted to servicing the debt and less left for essential family care and maintenance (Lesieur 1998; Kalischuk et al. 2006). The families of pathological gamblers report being less cohesive and independent, and having problems with communication, affective involvement, and over all functioning (Lesieur 1998).

The effects of this familial stress are apparent in the reported adverse outcomes. Spouses of pathological gamblers report being harassed by bill collectors as well as problems with insomnia, anxiety, depression and physical manifestations of stress (Lesieur 1998; Kalischuk et al. 2006). Additionally, partners of pathological gamblers

are up to 3 times as likely to attempt suicide as the partners of non-pathological gamblers (Lesieur 1998). In addition to spouses, the children of pathological gamblers are substantially more likely to experience adverse outcomes than non-pathological gamblers. Children in pathological gambling households report feelings of abandonment and isolation, adjustment difficulties, anxiety, and depression (Black et al. 2015; Kalischuk et al. 2006). One study found that the children of pathological gamblers were over 14 times as likely to commit suicide than controls, even after controlling for demographics and within family correlation (Black et al. 2015).

Internet gambling is typically associated with high rates of pathological gambling, regardless of the population studied (Petry and Gonzalez-Ibanez 2015). Additionally, the prevalence of pathological gambling and problem gambling can be as much as 10 times that of the general population. Studies have found that between 29.8 and 42.7% of Internet gamblers, and almost two-thirds (65.9%) of whom do so regularly, meet the criteria for pathological gambling. This is significantly higher than the general population (4%) and non-Internet gamblers (7.6%) (Wood and Williams 2007; Petry 2006; Nower, Caler, and Peters 2017).

A second prevalent finding is that Internet gamblers are noticeably younger (Petry 2006; Petry and Weinstock 2007). Younger gamblers are more likely to gamble on the Internet and are also more likely to experience problems as result (Gainsbury et al. 2013). Thus, these young Internet gamblers are at higher risk of pathological gambling due to immaturity, poor decision making skills, misconceptions regarding probability, and the belief that one can beat the system (Hubert and Griffiths 2018).

In a study of Spanish gamblers, the proportion of those in treatment under the age of 26 rose 500% after the introduction of legal Internet gambling. Additionally, nearly a quarter (24.2%) of Pathological gamblers' attributed the primary cause of morbidity to Internet gambling, almost half of whom were under 26. This is substantial considering that the typical pathological gambler does not enter treatment until years

after the onset of disordered gambling symptoms. In Internet gambling patients, this occurred within months, not years, with prevalence rates markedly increasing shortly after the introduction of legal Internet gambling (Chóliz 2016).

College educated, computer literate young adults, who have grown up with computers and Internet access, gamble more on the Internet than adults drawn from the general population (Petry and Weinstock 2007). In a multi-campus study of college students, Petry and Weinstock (2007) found that nearly one-third of students reporting having ever gambled online met the criteria for lifetime probable pathological gambling status. Furthermore, nearly two-thirds of those reporting regular Internet gambling were found to be lifetime probable pathological gamblers . The study also found that Internet gamblers bet with higher frequency and larger amounts when compared to non-Internet gamblers. As a result, Internet gamblers were more likely to experience family and school related problems and had higher rates of anxiety.

Compared to traditional gambling activities (*e.g.* casinos, sports betting, lottery,) Internet gambling has several distinct features that research suggests poses an increased risk for harm and abuse. Internet gambling has near-universal accessibility, available anywhere with an Internet connection, at any time (Gainsbury et al. 2012; Gainsbury et al. 2013; Gainsbury et al. 2015; Gainsbury 2015; McCormack and Griffiths 2013; Petry and Gonzalez-Ibanez 2015). While gambling online, one’s identity may be completely shielded (Petry and Gonzalez-Ibanez 2015). Like non-gambling gaming, this anonymity in Internet gambling is able to create and foster a totally immerse experience. This immersion can facilitate disassociation with both time and one’s surroundings, and even put one in a trance-like state (McCormack and Griffiths 2013; Wood and Williams 2007). Online gambling venues cater to a wide array of groups and demographics by tailoring the experience to suit different wants and interests. Operators are able to vary site appearance and theme, known as “*skins*”, without changing the underlying mechanics, games, or odds with little additional

costs (Gainsbury et al. 2012).

Internet gambling venues offer higher bonuses and more preferential odds than terrestrial venues given their relatively low overhead and labor costs. This also allows for a more affordable product⁶, thus capturing a gambling segment otherwise not reached by traditional venues (Gainsbury et al. 2012). The digital nature of Internet gambling also allows for a faster rate of play with many sites offering an auto-play feature. When engaged, this allows a player to bet continuously without any additional actions and continues until either disengaged, or the preset duration or number of bets has completed (McCormack and Griffiths 2013).

The majority of gambling promotion and advertising relies heavily on creating false illusions and misperceptions about the odds of winning. This is especially apparent in the promotion of state lotteries, which use get-rich-quick slogans that sometimes denigrate the value of hard work, initiative, responsibility, perseverance, optimism, investing for the future, and even education (Griffiths 2005). Marketing and promotion of Internet gambling is distinct from that of other venues in that these activities take place in the same space as the product itself. Unlike a terrestrial casino, a virtual casino's advertisement can bring the target directly to its product instantly. The use of registration strategies like free-play periods, sign-up bonuses, and give-aways are extremely effective in cultivating new players (Gainsbury et al. 2012; McCormack and Griffiths 2013; Hing et al. 2014).

Marketing of Internet gambling has also been quick to integrate social components to their products. When coupled with the wide availability of information and player experiences, integration of social media allows for sharing of tips, statistics, experiences. This information, in addition to marketing messages, fosters unwarranted confidence and the illusion of reduced risks (Gainsbury et al. 2012; Hing et al. 2014;

⁶Whereas traditional casinos would be prohibited by cost, especially labor, from offering blackjack below a minimum of \$10 per hand, Internet gambling venues are not, with some offering blackjack starting as low \$0.01 per hand.

McCormack and Griffiths 2013). Far more concerning are unscrupulous practices that target gamblers trying to reduce their play, especially those who have recognized gambling related problems. Consumer tracking, “circlejerks⁷,” and embedding are strategies to discourage discontinuation of play (McCormack and Griffiths 2013; Hing et al. 2014). By embedding particular keywords into a website’s code, gambling sites become featured and promoted in the results when searching to help quitting (McCormack and Griffiths 2013). In an exploration into the marketing experiences of online gamblers, Hing et al. (2014) found that players receive messages encouraging loss-chasing, discouraging closing accounts, and bonus offers even after barring themselves from the site.

More recent studies have addressed gambling involvement as a predictor of problem gambling. These studies posit that individual gambling activities are not a risk for problem gambling rather the cumulative total of activities is the risk. Gambling involvement is typically measured two ways. The first is breadth; commonly measured in terms of the number of unique gambling activities the individual participates in. The second is depth; this is typically measured as the amount of time and money wagered on one or all gambling activities. It is worth noting that no study has acknowledged the strong likelihood that involvement is endogenous.

Welte et al. (2009) analyze the effect of multiple forms of gambling on the number of problem gambling symptoms for American youth aged 14 to 21. Internet gamblers in their sample had the most problem gambling symptoms, greatest depth of involvement, and greatest breadth of involvement. Despite this, Internet gambling did not significantly predict problem gambling symptoms when other forms of gambling are controlled for. These results have been replicated using multiple samples, including LaPlante, Nelson, and Gray (2014). In that study, the authors use the

⁷Circlejerk is the term for invasive and persistent, consecutive pop-up messages when trying to exit a particular website meant to bring the user back to the exited site. (McCormack and Griffiths 2013)

2007 British Gambling Prevalence Survey to evaluate the risks associated with type of gambling and gambling breadth on disorder gambling status. They find that the effect of Internet gambling goes to zero when the breadth of involvement is controlled for.

These findings, however, are not universal. A 2018 study by Volberg et al. using the California Problem Gambling Prevalence Survey data finds a positive and significant effect of Internet gambling on problem gambling status even when controlling for involvement in other games. Nelson et al. (2018) estimate the effect of several forms of gambling on the likelihood of having at least one problem gambling symptom. Like Welte et al. (2009) and LaPlante et al. (2009), they find that Internet gambling has no significant effect when controlling for breadth of all gambling activities. However, when controlling for depth, Internet gambling becomes a strong, significant predictor of problem gambling symptoms.

These studies highlight the disagreement on whether particular forms of gambling increase the risk for problem gambling. However, none of the previous studies generated causal estimates, and all findings are merely correlations. Only one study has produced causal estimates of the effect of Internet gambling on problem gambling, a 2014 study from Philander and MacKay.

Philander and MacKay (2014) acknowledge the endogeneity of participation in Internet gambling, in that unobserved factors contribute to participation in Internet gambling. Using the 2010 British Gambling Prevalence Survey, they use an ordered probit IV model to estimate the causal effect of Internet gambling on problem gambling severity. They find that, when they control for endogeneity and gambling breadth, the causal effect on problem gambling severity is negative and statistically significant. They interpret this result as Internet gambling is less risky than other types of gambling.

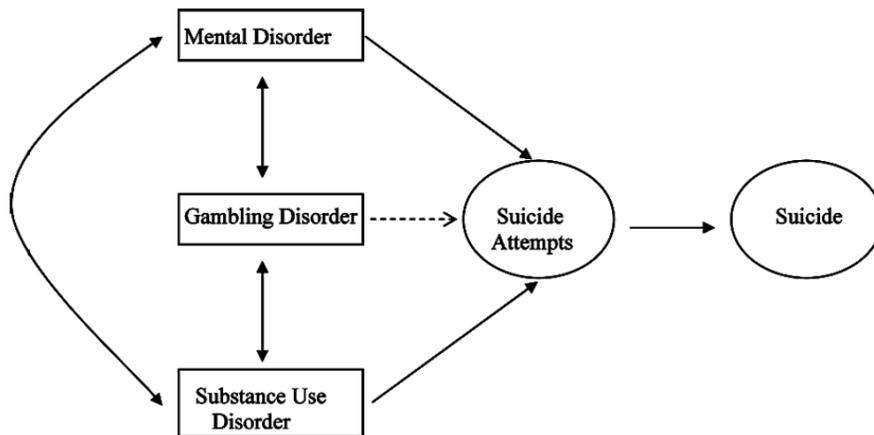
This result challenges most commonly held notions in the literature (e.g. McCor-

mack and Griffiths 2013; Wood and Williams 2007). Additionally, this is the only causal estimate in the literature on the effect of Internet gambling. Because of this, particular scrutiny of the methods of this study is warranted. First, like many recent articles, this study assumes that the number of gambling activities one engages in is exogenously given. I do not believe this to be true. Philander and MacKay (2014) assume that the number of gambling a gambler participates is exogenously given and that problem gambling severity is a function of the breadth of gambling. I believe, however, that it is much more likely that the breadth of gambling is, at least in part, a function of problem gambling severity. If this is the case, then their result is not statistically consistent. This evidenced in their validation sample, as Philander and MacKay (2014) show that, as the frequency of Internet activities increases, the likelihood of gambling online decreases when controlling for the number of gambling activities. The result of this first stage contradicts the rationale for this instrument, as well as the results in their primary analysis. However, when breadth is omitted, the model yields a statistically significant and positive estimate of the effect of Internet gambling on problem gambling with the first stage coefficients in the right direction. Though the data in their validation sample is not representative, the finding raises particular concern for the validity of their results.

The literature on Internet gambling and its effects has yet to provide convincing, causal evidence. Additionally, few studies in the literature use population level administrative data, relying almost exclusively on survey data, much of which is almost a decade old, even for recent studies. My contribution to this literature is to provide causal estimates of the effect of Internet gambling using current population-level administrative data. Examining the effect of the introduction of legal Internet gambling, in the context of the design of this study, circumvents the potential endogeneity issues surrounding gambling participation and involvement.

1.2.2 Research Question

Evidence has shown that there is a link between problem gambling and suicide. The mechanics of this relationship are not well understood. Hodgins, Mansley, and Thygesen (2006) present a common factor model for the link between gambling and suicide based on the frequent observation that mood and substance use disorders are comorbidities present in disordered gamblers with a history of suicidal ideation. Their model posits that gambling disorder affects suicide indirectly through two channels: “mental disorder, which seems to be a broad term covering mood disorders, and substance use disorder. Figure 1.1 illustrates the transmission mechanism through which disordered gambling affects suicide.



Note: Reproduced with permission from Hodgins, Mansley, and Thygesen (2006).

Figure 1.1: **Common Factor Model Illustrating Gambling-Suicide link**

Internet gamblers are frequently found to have higher rates of problem and disordered gambling and greater levels of gambling involvement than non-Internet gamblers (e.g. Welte et al. 2015). Many studies have identified characteristics unique to Internet gambling that are believed to make the activity more dangerous than other forms of gambling (e.g. McCormack and Griffiths 2013; Wood and Williams 2007). However, newer studies in the literature have questioned whether a particular

game, i.e. Internet gambling poses a greater risk to problem gambling or if the level of involvement in gambling activities is the actual risk (e.g. Philander and MacKay 2014).

The main question of this study is to assess whether Internet gambling is a more dangerous form of gambling. If Internet gambling, in itself, presents a greater risk for gambling problems as many studies have suggested, then the introduction of Internet gambling would likely result in the creation and/or exacerbation of gambling related problems and its accompanying effects on mental health status and substance use severity.

1.2.3 The Law

On February 26, 2013, New Jersey enacted a law, L.2013 c. 27 (The State of New Jersey 2013), legalizing Internet gambling, which began in November that year. The law was meant to provide a lifeline to the embattled and failing casino industry in Atlantic City. The industry suffered significant losses, even bankruptcies, due to a marked increase in competition from new gaming venues in neighboring states as well as a long and deep recession. These new casino venues, most located closer to New Jersey's population mass center than Atlantic City, provided increased convenience gambling to both in-state and out-of-state gamblers (American Gaming Association 2017; American Gaming Association 2018; Stirling 2011). These struggles resulted in economic peril for both the city and its people. A secondary goal was to ensure consumer protections that illegal Internet gambling cannot provide.

According to the statute, only current holders of casino license are eligible to offer Internet gaming. The law required each Internet gaming licensee, the provider of Internet gambling, to provide mechanisms for users to control their wagering activities. The first mechanism allows users to set a limit on account deposits. Once the user reaches this limit, they are prevented from making additional deposits for

a chosen period of time. The second mandated mechanism allows users to set cool down periods. This feature lets users suspend their ability to make wagers for any number of hours or days. Once initiated, the user is unable to alter the terms of their restrictions while in place. Only after the restrictions are lifted may the user change the terms. Additionally, if the selection time period exceeds 72 hours the licensee is barred from sending gaming related emails to the user during that time.

Other features of the law provide further user protections. One such protection prohibits licensees from promoting or encouraging more wagering once play has been initiated. This may help users from developing gambling related problems. Each site must also provide information on gambling problems, promotion of responsible and safe play, problem gambling signs and how to find help. In addition to providing information on finding help for gambling problems, each licensee is required to pay \$110,000 annually for problem gambling treatment programs in the state. The responsible gaming features built into the law may help to alleviate the potential negative effects of introducing legal Internet gambling.

1.3 Data and Methods

The purpose of this study is to estimate the causal effect of the legalizing Internet gambling on suicide. I identify this effect using the variation in suicide rates induced by the introduction of legal Internet gaming in New Jersey that took effect in November of 2013 (The State of New Jersey 2013). To study the impact of this law, I use suicide data from the Center for Disease Control's Compressed Mortality files for the years 1999-2016.

I use the number of deaths for individuals aged 16 and older where the cause of death is listed as intentional self-harm⁸ at the state-month level and calculate the monthly suicide rate per 100,000 residents using annual state population projections.

1.3.1 Comparison Group

In order to capture causal estimates for this DD study, a valid comparison group is necessary. An ideal group of comparison states would seem to be those states whose trends in suicide rates most closely resembling the trends in New Jersey and did not have a law legalizing Internet gambling during the study period. The comparison group consists of all states that did not permit Internet gambling and had at least 10 suicides in each period, as the data is suppressed for confidentiality below this threshold. This procedure yielded a comparison group of 34 states. The non-suppression constraint potentially biases the comparison group towards 1) high population states and 2) states with high suicidality. As a robustness check I impute missing values for states with less than 10% suppression, assigning a randomly generated integer between 0 and 9. I find no change in the models results⁹.

1.3.2 Controls

To control for potential omitted variable bias, I use a robust set of state-level control variables. I use demographic data on gender, race, and ethnicity extracted from the CDC Wonder data tool in order to capture time-variation within a given state as well as interstate differences. In addition, to control for differences in the state-level age distribution, I include a set of age count variables in 10-year buckets.

I include data from the University of Kentucky Center for Poverty Research to control for state-level differences in income, employment, political, and public assis-

⁸The ICD-10 codes for intentional self-harm begin with a three digit root between X60 through X84.

⁹These results are not reported here and available upon request.

tance variables, as these factors may affect both the decision to engage in Internet gambling, as well as suicide. I include data on Gross Product per-capita, personal income per-capita, and the proportion of the population that is employed to control for differences in state-level macroeconomic conditions. Additionally, I include an indicator for whether the period was during a recession using Federal Reserve Bank data (Federal Reserve Bank of St. Louis 2018).

I include data on public assistance and safety net to capture differences in poverty rates but also how generous a given state is in regards to these programs. This may reveal how accessible healthcare is to those at the lower end of the income distribution, especially when it comes to substance-use disorder and mental health benefits; two areas strongly tied to suicide. These variables include the proportion of the population receiving AFDC/TANF benefits, whether the state has an earned income tax credit (EITC), etc.

Lastly, to control for state-level differences in time-variant variables affected by political affiliation, I include a dummy that equals 1 if the governor is a Democrat, the proportion of the state upper house seats held by Democrats, and the proportion of the state lower house seats held by Democrats.

1.3.3 Model

This study uses a differences-in-differences model to estimate the effect of Internet gambling on suicide. This model estimates the change in the suicide rate for those induced into *legal* Internet gambling by the change in legal status. Nower (2017) estimates nearly 35% of all New Jersey Internet gamblers to be in this group. This group includes new entrants to gambling, those complementing existing gambling, and those substituting existing gambling with Internet gambling.

I estimate the effect of Internet gambling legalization on suicide rates with the

DD model given by:

$$y_{smt} = \alpha + DD_{smt} + X'_{smt}\beta + \gamma_s + \delta_m + \lambda_t + \epsilon_{smt} \quad (1.1)$$

where y_{smt} is the suicide rate per 100,000 residents for state s , in month m , in year t . Additionally, I include X_{smt} , a vector of state-year level controls to control for time-variant factors that may affect suicidality. I include three fixed effects: γ_s is a state fixed effect to control for time-invariant differences between states, δ_m is a month fixed effect to control for the seasonality in suicide rates, *e.g.* Seasonal Affective Disorder (SAD)¹⁰, λ_t is a year fixed effect to capture year specific national-level factors that affect suicidality. Finally DD_{smt} is the coefficient of interest and the indicator function that takes the value of 1 for NJ in the treatment period and 0 otherwise.

I estimate all rate models using ordinary least squares regression (OLS) with robust standard errors. Cluster-robust standard errors are likely the correct specification however, the small number of clusters presents potential inference issues.

1.3.4 Cluster-Robust Inference

Attention to the validity of standard errors in difference-in-differences models has grown with the popularity of the model, especially since Bertrand, Duflo, and Mulainathan (2004). In that paper, the authors show that not only do researchers often fail to control for within-cluster correlation, if they do they often cluster at the wrong level. Failure to control for this correlation may lead to downward biased standard errors and thus small, and incorrect, confidence intervals resulting in over-rejection of $H_0: \beta = 0$ (Cameron and Miller 2015). The methods to overcome this bias, including the wild-cluster bootstrap, often produce poor results (Hagemann 2019; Bertrand,

¹⁰SAD is described by the American Psychiatric Association as seasonal depression that typically occurs in the winter months when there is less sunlight and typically improves as Spring arrives. (Parekh 2017)

Duflo, and Mullainathan 2004; MacKinnon and Webb 2017a; MacKinnon and Webb 2018; Cameron and Miller 2015).

When the number of clusters is small, as often the case in the DD setting (Conley and Taber 2011), OLS leads to over-fitting and thus the cluster-robust variance matrix estimate (CRVE) will be biased downward and suffer the same maleffects that motivated the CVRE in the first place (Cameron and Miller 2015; MacKinnon and Webb 2018; Bertrand, Duflo, and Mullainathan 2004). Due to the small number of clusters in this study, the use of traditional CRVE techniques are not suitable. Instead, this paper will use tests, nearly identical to those used in Abadie, Diamond, and Hainmueller (2010) discussed later in this paper in the context of synthetic control methods, based on randomization and placebo inference described by Bertrand, Duflo, and Mullainathan (2004), Hagemann (2019), and MacKinnon and Webb (2018).

The idea behind the test is that if the null hypothesis, $H_0: \beta = 0$, is true then the treatment effect for the treated unit (cluster) should be drawn from the same distribution as the treatment effect estimated for a unit placebo treated at random (Hagemann 2019; MacKinnon and Webb 2018). The test makes no assumptions about error structure and does not rely on large sample approximations, thus the test is robust to irregular error structures and any sample/cluster size. The only restriction is that it is assumed that treatment assignment is random conditional on the fixed effects and observable covariates (Bertrand, Duflo, and Mullainathan 2004).

The test is operationalized by estimating the null distribution of β by using the treatment effect estimates for those units(clusters) placebo treated at random and assessing the position of the treatment effect for the cluster that actually received treatment within that distribution. Given the assumed symmetry of the distribution, I calculate the p -values of the one tailed test, in the notation of Maclean and Saloner (2018), $P(\Delta < \Delta_{NJ})$, where $\Delta = |DD|$, which is an exact statistics-type test of H_0 . I conduct this test for every major specification and most extensions in this paper in

addition to including robust standard errors.

1.4 Results

1.4.1 Summary Statistics

Summary statistics for all variables in the pre-treatment for New Jersey and comparison states can be found in Table 1.1. Before the law, the average suicide rate per 100,000 residents, per-month was 0.731 for New Jersey and 1.319 for the comparison states, which is nearly double the rate.

Demographic variables provide evidence that the comparison states are not markedly different from New Jersey. Population means show that New Jersey has about 10% more people than the average, 6.94 million vs. 6.24. Racial and ethnic makeup is also similar between the two groups, though New Jersey is slightly less White, more Black and Asian, and less Latin.

Table 1.1: **Summary Statistics: Pre-Treatment Period**

Sample:	Full Sample	New Jersey	Comparison States
<i>Outcome:</i>	Mean/Proportion	Mean/Proportion	Mean/Proportion
Suicide Rate*	1.302	0.731	1.319
Suicide Count	74.684	50.775	75.388
<i>Demographics:</i>			
Population	7,854,769	8,646,611	7,831,479
White	0.826	0.776	0.827
Black	0.128	0.143	0.128
Asian	0.032	0.077	0.031
Other race	0.014	0.005	0.014
Latin	0.095	0.149	0.093
Male	0.486	0.480	0.486
<i>Macroeconomic:</i>			
Employment Rate	0.470	0.481	0.470
Unemployment Rate	6.087	6.18	6.084
GDP Per Capita	0.042	0.052	0.041
Recession	0.157	0.157	0.157
<i>Income/Poverty:</i>			
Personal Income	35.261	47.059	34.914
% AFDC/TANF	0.014	0.012	0.014
% SNAP	0.103	0.056	0.105
Poverty Rate	13.1463	8.842	13.273
% Low Income	6.231	5.227	6.260
Uninsured Children			
<i>Political:</i>			
Governor Dem	0.506	0.539	0.505
% Lower House Dem	0.520	0.538	0.019
% Upper House Dem	0.499	0.521	0.498
N	6,230	178	6,052

Note: The unit of observation is the State-Month level. * The suicide rate is calculated as the number of suicides per 100,000 residents in a given state, for a given month.

1.4.2 Internal Validity

A critical assumption in recovering causal estimates of this DD model is that New Jersey and its comparison states would have experienced a similar trend had the intervention, i.e. law change, not been applied. Since it is not feasible to show such a counterfactual, suggestive evidence is provided with a comparison of pre-law suicide rate trends in New Jersey and comparison states. As seen in Figure 1.2, both groups follow similar trends in the pre-treatment period. Additionally, I formally test the assumption using OLS. This test provides evidence that the two groups did not trend differently in the years leading up to the change. I estimate the model:

$$y_{smt} = \alpha_0 + \alpha_1 T_{mt} + \alpha_2 (T_{mt} \times NJ_s) + X'_{smt} \alpha_3 + \gamma_s + \delta_m + \lambda_t + \epsilon_{smt} \quad (1.2)$$

where y_{smt} , γ_s , δ_m , λ_t , and X_{smt} are identical to those variables in Equation 1.1. In addition, I include a linear time trend T_{mt} and an interaction of the trend with NJ_s , an indicator variable for New Jersey, the treatment state. The coefficient of interest is α_2 which is the estimate of the difference in pre-treatment trends between the comparison states and New Jersey. If the two groups trended similarly, then α_2 should be zero and the assumption satisfied. The results of Model 1.2 can be found in Table 1.2. We see that the coefficient for α_2 is insignificant. This test provides evidence that the parallel trends assumption is satisfied.

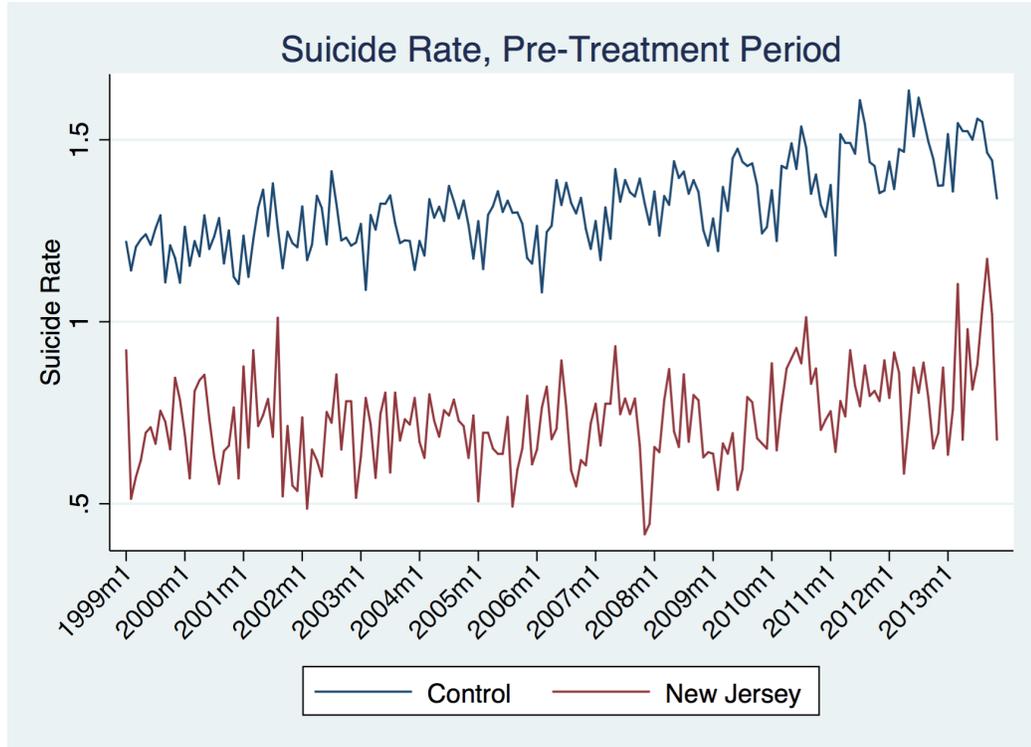


Figure 1.2: Unadjusted Trends in Suicide Rates in the Pre-Treatment Period

Table 1.2: Parallel Trends Testing

Outcome:	Suicide Rate	Suicide Counts
Time	0.00261*** (0.00018)	0.00136*** (0.00013)
(Time×NJ)	-0.00004 (0.00023)	-0.00009 (0.00027)
N	6,230	6,230

Note: Rate model estimated using OLS and count model estimated with Poisson, with population offsets. Both models control for state-level characteristics and state, year, and month fixed effects. Standard errors are robust. The study period is between January/1999 until October/2013. Those states with any suppressed observations during the study period are excluded. *, **, ***=statistically different from zero at the 10%, 5%, and 1% level

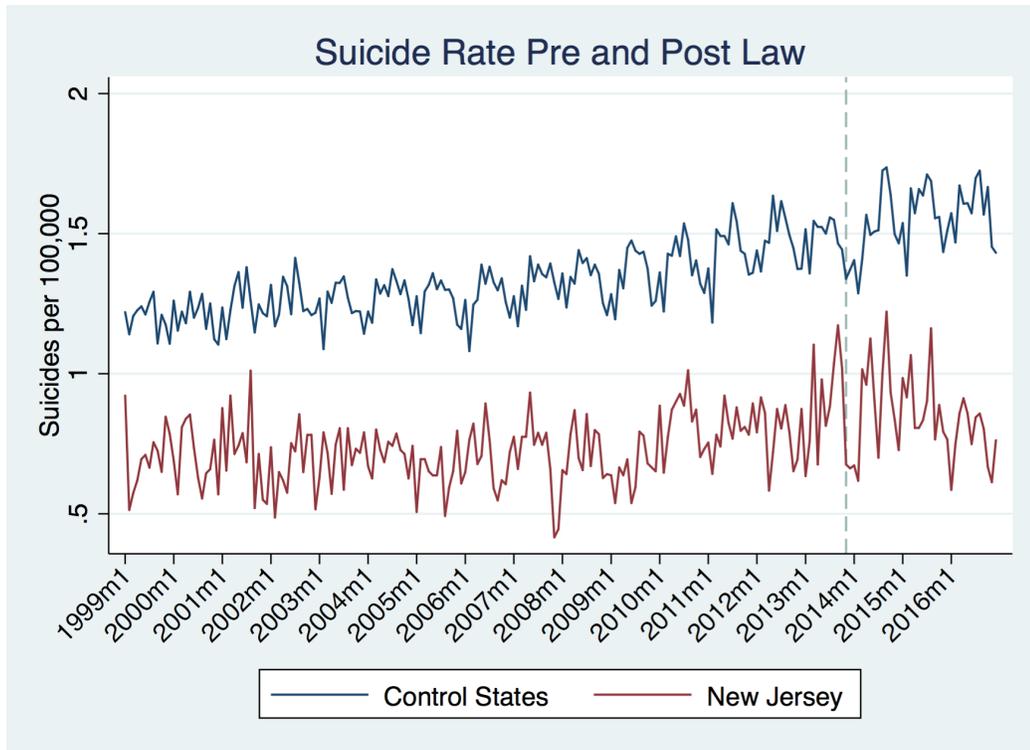


Figure 1.3: Unadjusted Suicide Rates for New Jersey and Comparison States: Pre and Post Treatment

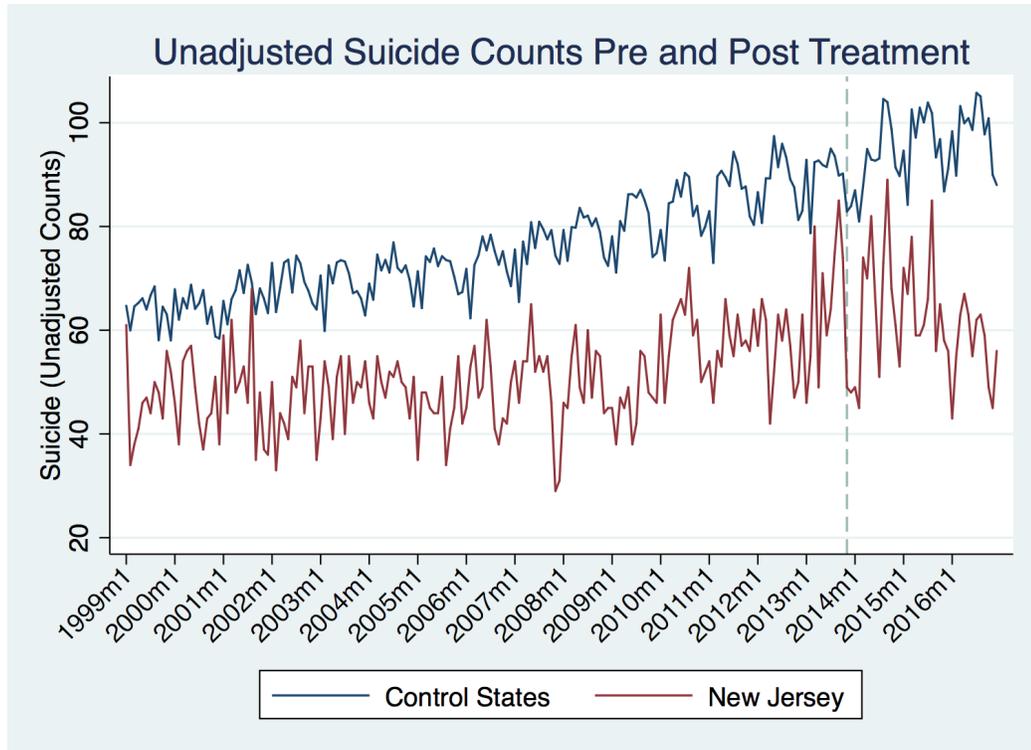


Figure 1.4: Unadjusted Suicide Counts for New Jersey and Comparison States: Pre and Post Treatment

1.4.3 DD Estimates

I estimate two main model specifications. The primary model includes demographic controls and state, year, and month fixed effects. I estimate the primary model two ways: the first, using OLS on suicide rates and the second, Poisson regression on suicide counts. I estimate a DD coefficient of -0.0271 with 95% confidence interval $[-0.0789, 0.0247]$ on the rate model. New Jersey experienced a pre-treatment mean suicide rate of 0.731 per 100,000 residents which gives the DD estimate an implied effect of -3.71% $[-10.97\%, 3.37\%]$ on the suicide rate. I estimate a DD coefficient for suicide counts of 0.0106 $[-0.0478, 0.0690]$. New Jersey experienced a pre-treatment mean of 50.775 suicides per-month implying a relative effect of 0.02% $[-0.09\%, 0.14\%]$.

Table 1.3: **The Effect of Legalizing Internet Gambling on Suicide**

Outcome:	NJ Pre-Law mean	DD estimate	Rank of DD estimate magnitude	p -value from one-tailed test $P(\Delta < \Delta_{NJ})$
Suicide Rate	0.731	-0.02712 (0.02641)	9/35	0.25714
Suicide Count	50.775	0.01061 (0.02983)	10/35	0.28571
N				7,560

Note: Rate model estimated using OLS and count model estimated with Poisson, with population offsets. Both models control for state-level characteristics and state, year, and month fixed effects. The study period is between January/1999 until December/2016. Those states with any suppressed observations during the study period are excluded. Standard errors are robust. *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. p -values derived from permutation testing.

The second, is a more robust specification that includes income, employment, political, and public assistance variables. Table 1.3 and Table 1.4 provide the regression coefficients for the primary and secondary DD models. In Table 1.3, I find that the effect of legalizing Internet gambling is statistically insignificant. When additional controls are added to the model, presented in Table 1.4, the magnitude of the effect is larger but still insignificant. In all model specifications I find no evidence of an effect of the law on suicidality barring very small effects, i.e. the effect I estimate is precisely zero.

Table 1.4: **The Effect of Legalizing Internet Gambling on Suicide, Expanded Model**

Outcome:	NJ Pre-Law mean	DD estimate	Rank of DD estimate magnitude	p -value from one-tailed test $P(\Delta < \Delta_{NJ})$
Suicide Rate	0.731	-0.03273 (0.02744)	12/35	0.343
N				7,560

Note: Rate model estimated using OLS with controls for state-level characteristics and state, year, and month fixed effects. . The study period is between January/1999 until December/2016. Those states with any suppressed observations during the study period are excluded. Standard errors are robust. *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. p -values derived from permutation testing.

1.5 Robustness Checks

1.5.1 Alternative Specifications

The primary model specification relies on rates derived from linear population projections. These projections in the denominator of the model can potentially introduce strong trends into the suicide rates. To address this, I estimate all models with the alternative specification using suicide counts. These models are estimated using Poisson regressions with population exposure and the same sets of controls for each model. In our primary specification, presented in Table 1.3, I find no difference in the effect on suicides in that both are statistically indifferent from zero, although the sign of the effect switches.

To account for the differences in population I estimate the model using weighted least squares using population weights. The results from this DD model can be seen in Table 1.5. When population weights are used the effect of legalizing Internet gambling drops to near zero and still insignificant.

A common concern in DD models is that effects assumed to be time-invariant

may not be entirely fixed; that is, there may be time-variant unobservable differences between states. To address this possible omitted variable bias I include state-specific linear time trends. Table 1.5 presents the results from adding these trends to the model. I find no change in the effect which is, again, statistically insignificant.

Table 1.5: The Effect of Legalizing Internet Gambling on Suicide, Alternate Specifications

Modification:	Outcome	NJ Pre-Law mean	DD estimate
State-specific trends	Suicide Rate	0.731	-0.01735 (0.03660)
Population Weighted	Suicide Rate	0.731	-0.00022 (0.02532)
$N=$			7,560

Note: Rate model estimated using OLS. Both models control for state-level characteristics and state, year, and month fixed effects. Robust standard errors are in parentheses. The study period is between January/1999 until December/2016. Those states with any suppressed observations during the study period are excluded. Standard errors are robust. *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. p -values derived from permutation testing.

1.5.2 Alternative Comparison Groups

An appealing comparison group would seem to be those states immediately bordering New Jersey would seem to be the most geographically relevant as well as potentially demographically and culturally similar. Of the three contiguous states to New Jersey, Pennsylvania and New York meet the criteria. Pennsylvania and New York allow for gambling in physical casinos, like New Jersey, though Internet gambling was explicitly illegal in both states during the study period. Given the size of both New York and Pennsylvania, no data suppression issues arose when acquiring monthly mortality. A second appealing comparison group is the set of Northeastern states. This group, however, contains several less populous states and thus encounter data suppression issues. The final comparison group adds only one additional state over the contiguous specification.

I estimate the simple model for both alternative comparison groups using both rates and counts. The results from these regressions can be found in Table 1.6. In none of these alternative specifications do I find a significant effect on suicide which is no different than the primary model.

Table 1.6: The Effect of Legalizing Internet Gambling on Suicide, Alternate Comparison Groups

Comparison Group	Outcome	NJ Pre-Law mean	DD estimate
Contiguous	Suicide Rate	0.731	-0.03988 (0.04039)
	Suicide Count	50.755	-0.02246 (0.04248)
	<i>N</i>	648	
Northeast	Suicide Rate	0.731	-0.03028 (0.03467)
	Suicide Count	50.755	-0.00713 (0.03879)
	<i>N</i>	864	

Note: Rate model estimated using OLS and count model estimated with Poisson, with population offsets. Both models control for state-level characteristics and state, year, and month fixed effects. Robust standard errors are in parentheses. The study period is between January/1999 until December/2016. Those states with any suppressed observations during the study period are excluded. Standard errors are robust. *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. *p*-values derived from permutation testing.

1.5.3 Event Study

The DD model used throughout this study relies heavily on the exogeneity of the policy assignment. One potential threat to this assumption is policy endogeneity. That is, the policy is passed in response trends in pre-treatment outcomes. An example of this would be that New Jersey legalized Internet gambling because there were few problem gamblers as reflected in the suicide rate. Though this seems unlikely, the converse of this example is plausible. In either case, the DD estimates would be biased. To address this potential bias, I conduct an event study (Kline 2011; Wing,

Simon, and Bello-Gomez 2018).

I estimate the simple DD model given by Equation 1.1 and to it I add a series of policy leads and lags. Additionally I impose an endpoint restriction that treatment dynamics end after 6 years (Kline 2011). This assumption is consistent with the model of adaptation (LaPlante and Shaffer 2007) which says that gamblers are able to adapt over time to compensate for added risk. Ideally, the coefficients on treatment leads would all be zero, in that no effect can be seen before the cause.

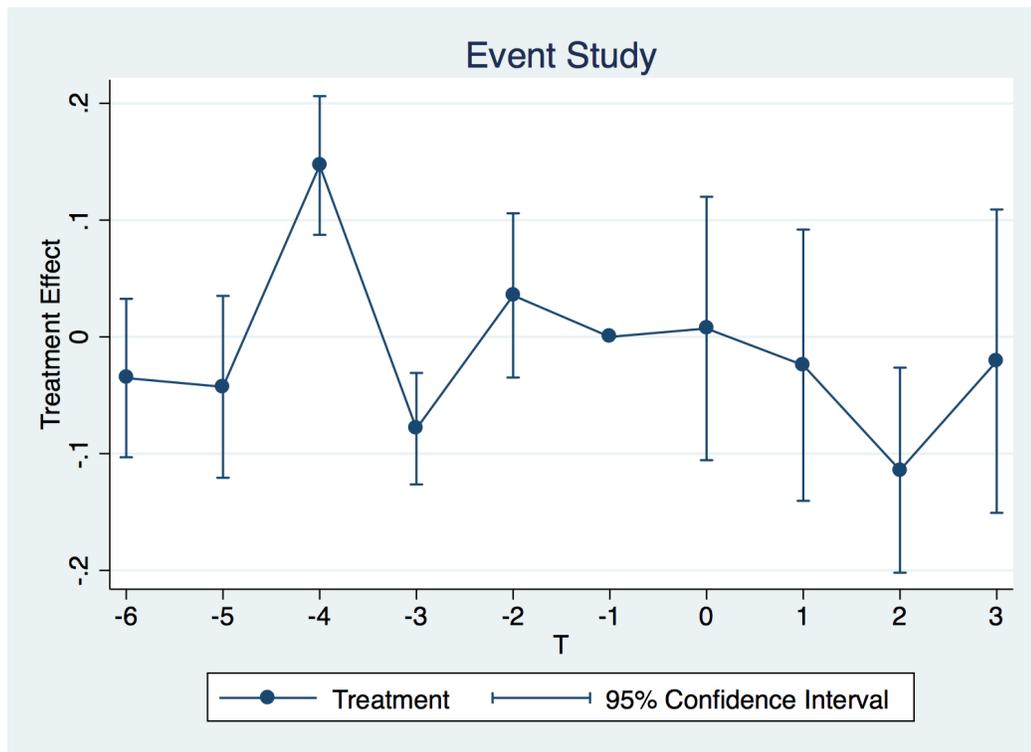


Figure 1.5: **Effect of Legalizing Internet Gambling on Suicides Using an Event Study**

The results of this regression are presented in Table 1.7 and can be seen graphically in Figure 1.5. I find significance on the coefficients on the treatment lead 3 and 4 years prior to the actual policy. Interestingly, I find a treatment effect 2 years post-implementation yet no other treatment effect for $t \geq 0$ is significant.

The event study is used to provide evidence that the policy exogeneity assumption is satisfied. Unfortunately the results here are not able to assuage this fear. However,

I am not entirely troubled by these results. I present findings that suggest the policy has no effect on suicide. The event study shows that even when controlling for these pre-treatment trends, I still find scant evidence of any discernible effect on suicidality.

Table 1.7: **Effect of Legalizing Internet Gambling on Suicides Using an Event Study**

Outcome:	Suicide Rate
6 Years Pre-Law	-0.03527 (0.03660)
5 Years Pre-Law	-0.04279 (0.03972)
4 Years Pre-Law	0.14675*** (0.03029)
3 Years Pre-Law	-0.07862*** (0.02434)
2 Year Pre-Law	0.03548 (0.03587)
1 Year Pre-Law	0 ⁺
Law Year	0.00719 (0.05753)
1 Year Post-Law	-0.02426 (0.05925)
2 Years Post-Law	-0.11415** (0.04479)
3 Years Post-Law	-0.02081 (0.06625)
<i>N</i>	7,560

Note: Rate model estimated using OLS with controls for state-level characteristics and state, year, and month fixed effects. Standard errors are robust. The study period is between January/1999 until December/2016. Those states with any suppressed observations during the study period are excluded. *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. + Restricted to 0 by assumption.

1.5.4 Google Trends

In addition to the health outcome variables, I also examine proxy measures for both the prevalence of Internet gambling and mental health issues using Internet search data from Google Trends. This analysis acts as a pseudo first stage analysis as to substantiate the measured effects, or lack thereof, on the outcomes of interest. The DD model used throughout this paper relies on the assumption that the treatment, in this context the change in legal status of Internet gambling, induces some individuals to engage in gambling online. That is to say, the law induced a behavioral response and it is this variation that the DD model exploits to estimate the effect of the treatment on the outcome of interest. Thus, if the legalization of Internet gambling were to affect the suicide rate, then it is necessary that the law induced a measurable change in online gambling participation. I assess this assumption by examining Google search data for Internet gambling related phrases¹¹.

According to the common factor model given by Hodgins, Mansley, and Thygesen (2006) describing the link between disordered gambling and suicide, see Figure 1.1, Internet gambling should affect suicide through the disordered gambling channel. If the legalization of Internet gambling were to affect suicide rates then disordered gambling prevalence should also be affected. Using search data to answer this question is not maladapted; a study by Khazaal et al. (2008) found that almost 70% of patients with psychiatric disorders sought health-related information online. Using Google search data I estimate the change in prevalence of searches for “gambling addiction” as a change in search volume could translate to meaningful diagnoses. Lastly, I examine later direct effects on mental health examining the phrases “Depression” and “suicide” as these terms may indicate changes in mental health and suicidal ideation, both milder measures than completed suicides.

¹¹I analyze the relative frequency of searches containing any of the following phrases: “online gambling,” “gamble online,” “Internet gambling,” or “online casino.”

To assess the effect of the law I conduct simple DD regressions on annualized¹² data on the popularity of particular search phrases over a 9 year period around the policy (2008-2016). Unfortunately, exact search data are not available. Instead, Google provides measure of relative frequency in terms of total searches which is directly comparable across states, and over time. The measure effectively, in this model setup, compares the popularity of a particular phrase across space and time. This exercise provides both context and evidence of the validity of the research design.

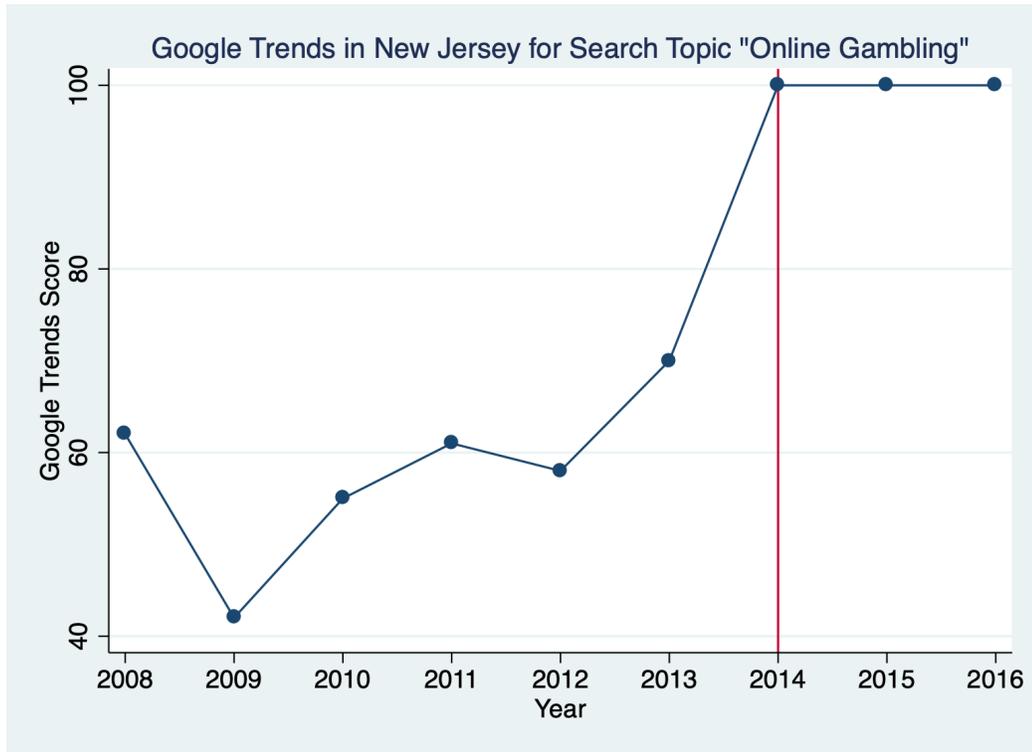


Figure 1.6: Google Trend Score for Google Searches Containing the Phrase "Online Gambling" for New Jersey

I use a DD model to estimate the effect of legalizing Internet gambling on annual state-level search patterns using Equation 1.3:

$$S_{st} = \alpha + DD_{st} + X'_{st}\beta + \gamma_s + \lambda_t + \epsilon_{st} \quad (1.3)$$

¹²Data is annualized around the treatment date where a year is defined as November to October.

where S_{st} is the search score for state s in year t , γ_s , δ_m , λ_t , and X_{smt} are identical to those variables in Equation 1.1 however all controls have been converted to annual means. Additionally, I make the same exclusions from the comparison group as the primary DD model. I estimate these models using standard least squares regression (OLS) and follow the randomization procedures for statistical inference as in the primary data section.



Figure 1.7: Google Trend Score for Google Searches Containing the Phrase "Gambling Addiction" for New Jersey

The results of the Google Trends DD estimates can be found in Table 1.8. I find that there was a large impact on the relative search volume for Internet gambling related terms with a coefficient of 60.92. This translates into a 105.03% [79.96%,130.11%] increase over pre-treatment means. This finding provides evidence that the law induced a behavioral change in peoples' Internet activity and provides further evidence of the validity of this DD model. The second, highly important finding is that after the implementation of the law there was 48.84% [20.20%,65.48%] increase in the

popularity of searches for gambling addiction. This demonstrates a significant increase in individuals seeking information of problem and disordered gambling than before the law went into effect. This finding is especially important because it provides causal evidence of a link between Internet gambling and gambling addiction. This finding is contrary to Philander and MacKay (2014) who find no link between the two. Lastly, similar to the findings in the preceding sections, I find no likely effect of the change in law on searches for “depression” and “suicide”. This finding provides more evidence that legalizing Internet gambling does not affect mental health or that it is too soon to find a measurable change.

Table 1.8: The Effect of Legalizing Internet Gambling on Internet Search Trends

Search Term:	NJ Pre-Law mean	DD estimate	Rank of DD estimate magnitude	p -value from one-tailed test $P(\Delta < \Delta_{NJ})$
Suicide	76.8	1.7602 (2.6208)	14/35	0.40
Depression	74.3	-1.1319 (2.7911)	9/35	0.26
Online Gambling [†]	58.0	60.9204*** (7.3865)	35/35	0.99
Gambling Ad- diction	31.8	12.7492*** (7.9863)	31/35	.89
N				315

Note: [†] Includes any queries that contained any of the following phrases: “online gambling,” “gamble online,” “online casino,” or “internet gambling.” All models are estimated using OLS with controls for state-level characteristics with state, year, and month fixed effects. Study period is 2008-2016. States excluded from the primary analysis also excluded here. Standard errors are robust. *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. p -values derived from permutation testing.

1.5.5 Varying Treatment Intensity

A central piece of the standard DD model is that, when the treatment period is reached, a homogenous treatment is applied to all treated units in perpetuity (or until expired, repealed, etc.). However, this is not likely to be the case in many policy scenarios. Demonstrated in the previous section, I find that there is evidence that the law did induce behavioral change in internet behaviors. To test the extent to which this behavioral change affected suicide rates I use Internet casino “win¹³” data from the New Jersey Division of Gaming Enforcement. Casino win can also be seen as a measure of proliferation or uptake of Internet gambling in New Jersey. When interacted with the DD indicator, the interaction applies the treatment with heterogeneous intensity. Specifically, I estimate the model:

$$y_{smt} = \alpha + \phi(DD_{smt} \times W_{smt}) + X'_{smt}\beta + \gamma_s + \delta_m + \lambda_t + \epsilon_{smt} \quad (1.4)$$

Where all variables are identical to Equation 1.1, with the exception of DD_{smt} being interacted with casino win, W_{smt} . The new coefficient of interest is ϕ . This measures the change in the monthly suicide rate for every \$1,000,000 the Internet casino wins from players.

¹³Casino win is essentially a measure of profit. It can be viewed conversely as player loss.

Table 1.9: **The Effect of Legalizing Internet Gambling on Suicide Rates with Varying Treatment Intensity.**

Outcome:	NJ Pre-Law mean	ϕ Estimate	Rank of ϕ estimate magnitude	p -value from one-tailed test $P(\Delta < \Delta_{NJ})$
Suicide Rate	0.731	-0.00302***	15/35	0.42857
		(0.00188)		
N				7,560

Note: ϕ is the coefficient for the interaction ($DD_{smt} \times W_{smt}$) All models are estimated using OLS with for controls state-level characteristics with state, year, and month fixed effects. Study period is 2008-2016. States excluded from the primary analysis also excluded here. Standard errors are robust. *,**,***=statistically different from zero at the 10%, 5%, and 1% level. p -values derived from permutation testing.

The results for the estimates of equation 1.4 can be found in Table 1.9. I find an estimated effect of casino wins to be -0.003 , suicides per-month, or -0.04% , per-\$1,000,000. While this is significant at the 1% level using robust standard errors, I find that this result does not hold when applying the cluster-robust inference permutation testing. I suspect that this result is driven by the highly linear characteristics of this variable. Figure 1.8 visually illustrates this relationship. Though not reported, I find nearly identical results when estimating Equation 1.4 with casino win replaced by a linear time trend which explains the disparity in significance between the two methods.

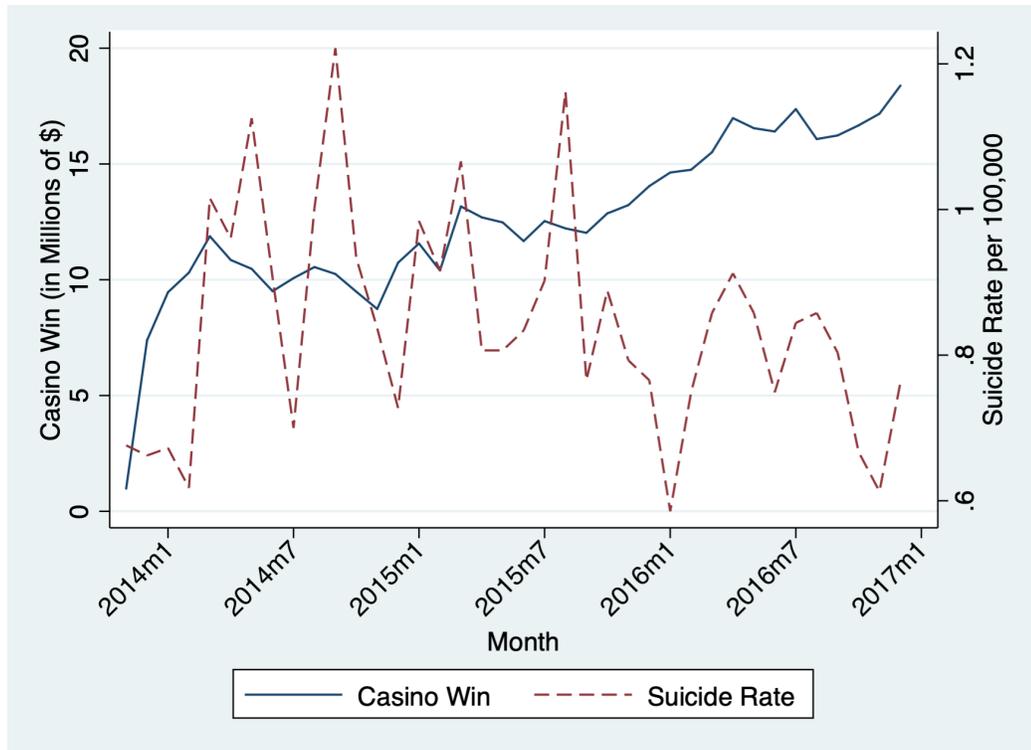


Figure 1.8: Monthly New Jersey Internet Casino Revenues Compared to New Jersey Monthly Suicide Rate

1.6 Alternative Design : Synthetic Control Method

To estimate the causal effect of a treatment, a valid comparison group is required. This group should reflect the counterfactual path of the outcome variable for the treated unit had treatment not been applied. There are multiple approaches for selecting a comparison group. The selection of this comparison group is very important, as an inappropriate comparison group can lead to erroneous conclusions as differences in outcomes may be driven by differences in characteristics. Additionally, the process of choosing this group is often ad hoc, such as geographic proximity. Cross-border analyses often force side-by-side comparisons even if the treated and comparison groups are radically different.

Developed by Abadie, Diamond, and Heinmueller (ADH) 2010, the idea behind

the SCM is to use a data driven process to select the comparison group and requires only pre-treatment data to construct. This allows the researcher to make decisions on study design while being blind to how those decisions affect conclusions. This reduces discretion in choice and forces transparency through quantifiable justification (Abadie, Diamond, and Hainmueller 2010; Abadie, Diamond, and Hainmueller 2015).

Suppose that there is a sample of $J + 1$ units, indexed by j . Let unit $j = 0$ be the unit that receives the treatment and the remaining J units, $j = 1, \dots, J$, be the group of potential comparisons units. In the context of this study, the groups are US-states and $j = 0$ is New Jersey. We observe each these $J + 1$ units over T periods, $t = 1, \dots, T$, implying a strongly balanced panel with dimensions $(J + 1 \times T)$. At some period $t = T_0$ the treated unit, $j = 0$, receives the treatment. Thus we observe the units over T_0 pre-intervention periods, and $T_1 = T - T_0$ post-intervention periods.

The SCM is represented by a vector of weights that form a convex combination of donor units to create a synthetic version of the treated state, had the treatment not been applied. The SCM is given by the $(J \times 1)$ vector $W = (w_1, \dots, w_J)'$ where $0 \leq w_j \leq 1$ for $j = 1, \dots, J$ and $\sum_{j=1}^J w_j = 1$. Let X_1 be a $(k \times 1)$ vector of pre-intervention variables for the treated unit. Let X_0 be a $(k \times J)$ vector of the same pre-intervention variables but instead for the group of potential donor units. For the comparison group, here being the synthetic control, to be valid it should closely resemble the treated unit in the pre-intervention period. The SCM finds a vector of weights on donor states to create a synthetic unit that best approximates the treated unit in the pre-intervention period. The vector W has elements that are the relative contribution of each state in the donor pool.

Let V be a $(k \times k)$ diagonal positive definite matrix with diagonal elements, v_m , be the relative importance of each of the variables in X_0 and X_1 in regards to their power in predicting the outcome variable. Given X_0 , X_1 , and V , the ADH method chooses the optimal vector W^* that minimizes the sum of squared deviations between

the treated unit and the synthetic control unit.

W^* is defined as the solution:

$$W^* = \arg \min_W (X_0 - X_1 W)' V (X_0 - X_1 W) \text{ s.t. } \sum_{j=1}^J w_j = 1, 1 \geq w_j \geq 0 \text{ for } j = 1, \dots, J. \quad (1.5)$$

Once the optimal SCM vector is found it may be used on the post-intervention period to estimate the path of treatment effects. Let $\hat{\alpha}_{1t}$ be the treatment effect for time $T \geq t \geq T_0$ be given by:

$$\hat{\alpha}_{1t} = Y_{1t} - Y_{0t} W^*. \quad (1.6)$$

Following Bohn and Maclean, to assess the impact of the law on suicide rates, a simple (2x2) DD estimate is constructed using the synthetic control group, synthetic New Jersey, and the observed treated group, New Jersey. This estimate is given by:

$$DD_{NJ} = (Y_{NJ}^{Post} - Y_{Synth}^{Post}) - (Y_{NJ}^{Pre} - Y_{Synth}^{Pre}) \quad (1.7)$$

where Y_{NJ}^{Pre} is the average suicide rate for New Jersey in the pre-treatment period, Y_{NJ}^{Post} is the average suicide rate for New Jersey in the post-treatment period, Y_{Synth}^{Pre} is the average suicide rate for synthetic New Jersey in the pre-treatment period, and Y_{Synth}^{Post} is the average suicide rate for synthetic New Jersey in the post-treatment period.

Since large sample inference techniques are not suited for this application, I use the technique of Bohn, Lofstrom, and Raphael (2014) and Maclean and Saloner (2018), adapted from the test developed by Abadie, Diamond, and Hainmueller (2010) for Equation 1.7. This placebo/permutation test examines whether the effect estimated for New Jersey is large relative to the effect estimated for a state chosen at random. To operationalize this test, for each state in the donor pool, the synthetic control group is

identified using Equation 1.5. Once each synthetic control group is found, every state in the donor pool is individually placebo-treated as if they had passed an equivalent law as New Jersey by calculating the DD estimate in Equation 1.7. The empirical distribution of treatment effects for these placebo estimates is the equivalent of a sampling distribution for DD_{NJ} . Using this distribution the statistical significance of the treatment effect can be assessed. This is to say, if legalizing Internet gambling had an effect on the suicide rate then one would expect the size of the DD estimate to be substantially large relative to the effect estimated for a randomly assigned placebo. This type of inference is discussed in more detail in Section 1.3.4.

In order to construct a synthetic New Jersey, a donor pool of appropriate states must be established. Several states are excluded from the potential donor pool. Of the 51 possible donors, 17 are excluded due to missing mortality data. Additionally, I exclude Nevada from the donor pool given that it allowed some sort of Internet gambling during the study period.¹⁴ The last state to be excluded from the donor pool is Massachusetts, as the state experienced a precipitous change in the trend of the outcome variables of interest (Abadie, Diamond, and Hainmueller 2015). These exclusions, identical to those in the primary analysis, result in a potential donor pool of 33 states listed in Table 1.10.

¹⁴Delaware also permitted legal Internet gambling during this period but was already excluded for missing data.

Table 1.10: **Potential Donor Pool For Synthetic New Jersey**

Alabama	Arizona	Arkansas	California
Colorado	Connecticut (0.10)	Florida	Georgia
Illinois	Indiana	Iowa	Kansas
Kentucky	Louisiana	Maryland	Michigan
Minnesota	Mississippi	Missouri	New Mexico
New York (0.90)	North Carolina	Ohio	Oklahoma
Oregon	Pennsylvania	South Carolina	Tennessee
Texas	Utah	Virginia	Washington
West Virginia	Wisconsin		

Note: Weights for the synthetic control group are in parentheses; if none then weight is 0.

In addition to the donor pool, the contents of both X_0 and X_1 must be chosen, outside of the pre-intervention outcome variables. The additional variables included in X_0 and X_1 are state-level demographics, macroeconomic conditions, and certain relevant employment and income figures for the mental healthcare and casino industries. Abadie, Diamond, and Hainmueller (2010) and others include additional lags of outcome and other variables to control for trends over time, however my primary SCM specification does not include these as the results did not change appreciably after including several permutations of pre-intervention mean periods. Table 1.11 displays the mean values for the variables in X_0 and X_1 . Abadie, Diamond, and Hainmueller (2010) say that interpolation bias can be reduced by choosing donors that similar to the treated units in terms of X_1 . I show this by conducting t -tests on the differences in pre-intervention outcome and state-level covariates for New Jersey and the donor pool; this can be found in Table 1.11. The differences in these variables in statistically insignificant.

I estimate the SCM using the `synth` command written by Abadie, Diamond, and Hainmueller (2013) for STATA using the “*nested*” and “*allop*” options which fully nests the optimization using a regression-based selection of V and then re-calculates from several starting points to assure a global minimum. The resulting SCM, W^* ,

can be found in Table 1.10. The optimal synthetic control is built from New York, at 90%, and Connecticut, at 10%. The heavy “*donation*” by New York is not entirely surprising as suicide rates between New York and New Jersey track each other surprisingly closely, which can be seen in Figure 1.9. The SCM achieves fairly good balance for most the covariates, however some deviate substantially. When these variables are removed there is no appreciable difference in conclusion and a minor increase in RMSPE. Predictor balance may be found in Table 1.12. Synthetic New Jersey performs quite well in terms of trending with New Jersey. Since the level of observation is monthly and thus noisy, I present the trends for New Jersey and Synthetic New Jersey averaged annually around the implementation and can be found in Figure 1.10.

Table 1.11: **Pre-Treatment Period SCM Variable Means and Proportions**

Variable	New Jersey	Donor Pool	Difference
<i>Outcome:</i>	Mean/Proportion	Mean/Proportion	(<i>p</i> -value)
Suicide Rate	0.7308	1.3191	<0.0001
<i>Demographics:</i>			
White	0.7755	0.8287	<0.0001
Black	0.1429	0.1260	0.0157
Latin	0.1491	0.0929	<0.0001
Asian	0.0767	0.0315	<0.0001
Male	0.4801	0.4858	<0.0001
<i>Relevant Industries:</i>			
MH Outpatient Emp. PC	0.5535	0.6470	0.0005
MH Drs Emp PC	0.1826	0.1384	<0.0001
Casino Emp PC	0.0083	0.5143	<0.0001
Casino Firms PC	0.0018	0.0082	<0.0001
Casino Pay PC	0.4173	13.6749	<0.0001
<i>Macroeconomic:</i>			
Employment Rate	0.4806	0.4709	0.0001
GDP PC	0.0517	0.0417	<0.0001
Personal Income PC	47.0587	35.2610	<0.0001
<i>Poverty:</i>			
Poverty Rate	8.8416	13.1977	<0.0001
% Low Income Uninsured Chil- dren	5.227	6.1482	<0.0001
SNAP Rate	0.0555	0.1038	<0.0001
AFDC/TANF Rate	0.0116	0.0139	0.0001
N:	178	6,052	—

Note: MH stands for “Mental Health” and PC means the variable is in units per 100,000 residents. *p*-values derived from standard *t*-tests.

Table 1.12: **Synthetic Control Model Predictor Balance for Pre-Treatment Years**

Variable	New Jersey	Synthetic New Jersey
<i>Demographics:</i>	Mean/Proportion	Mean/Proportion
White	0.7755	0.7562
Black	0.1429	0.1658
Asian	0.1491	0.0699
Latin	0.0767	0.1501
Male	0.4801	0.4769
<i>Relevant Industries:</i>		
MH Outpatient Emp. PC	0.5535	0.7107
MH Dr's Emp PC	0.1826	0.1334
Casino Emp PC	0.0083	0.1814
Casino Firms PC	0.0018	0.0032
Casino Pay PC	0.4173	5.4814
<i>Macroeconomic:</i>		
Employment Rate	0.4806	0.4640
GDP PC	0.0517	0.0549
Personal Income PC	47.0587	44.7524
<i>Poverty:</i>		
Poverty Rate	8.8416	14.2137
% Low Income Uninsured Children	5.227	4.4291
SNAP Rate	0.0555	0.1009
AFDC/TANF Rate	0.0116	0.0195
RMSPE	0.13194	—

Note: MH stands for “Mental Health” and PC means the variable is in units per 100,000 residents.

The DD coefficient for the SCM, found in Table 1.7, is -0.008. This represents a -1.1% reduction in the monthly suicide rate. However, I find that this is not statistically significant with a p -value of 0.086. Thus, I find no evidence of an effect on suicide rates. This is evidenced graphically in Figure 1.11 where I plot the trend of treatment effects for New Jersey and every placebo treated state. New Jersey lies in central mass of effects in the post period. If New Jersey were to have had an effect of the law, the trend line for New Jersey in the post period would be a literal outlier, lying on the edges of the plot.

Table 1.13: **Differences-in-differences estimates using New Jersey and synthetic New Jersey.**

Outcome:	NJ pre-treatment mean	DD estimate	Rank of DD estimate magnitude	p-value from one-tailed test $P(\Delta < \Delta_{NJ})$
Suicide Rate	0.731	-0.008	3/35	0.086
RMSPE	0.13194	—	—	—

Note: All models are estimated using OLS with controls state-level characteristics with state, year, and month fixed effects. Study period is January/1999 until December/2016. States excluded from the primary analysis also excluded here. Standard errors are robust. *,**,***=statistically different from zero at the 10%, 5%, and 1% level. p -values derived from permutation testing.

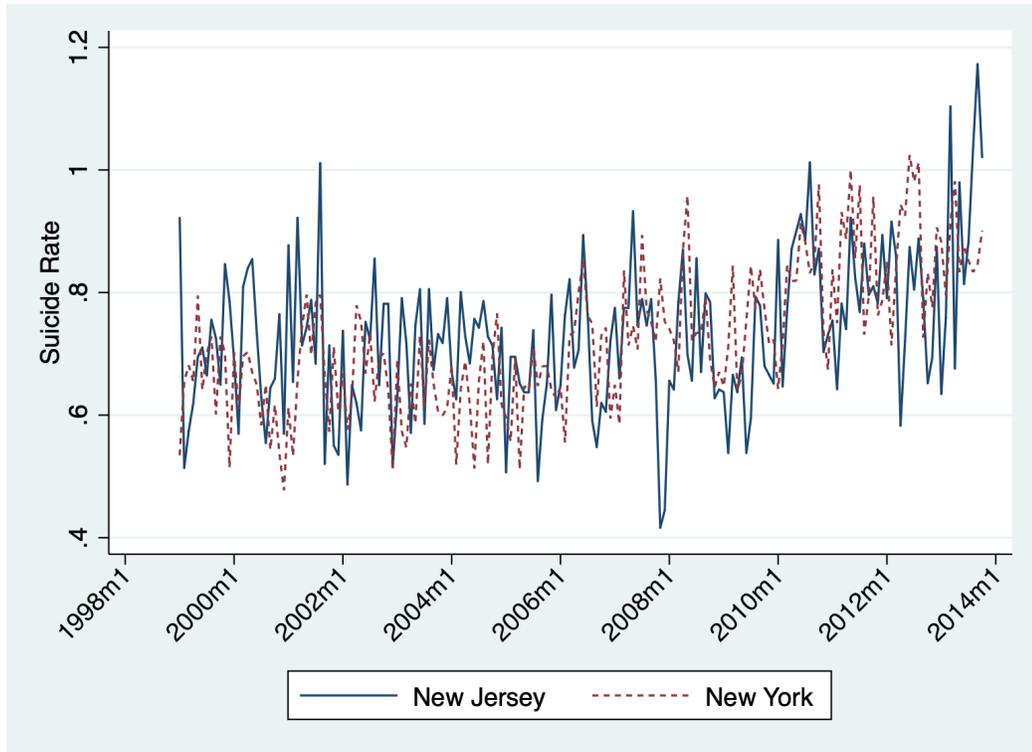


Figure 1.9: Comparison of Monthly Suicide Rates For New Jersey & New York

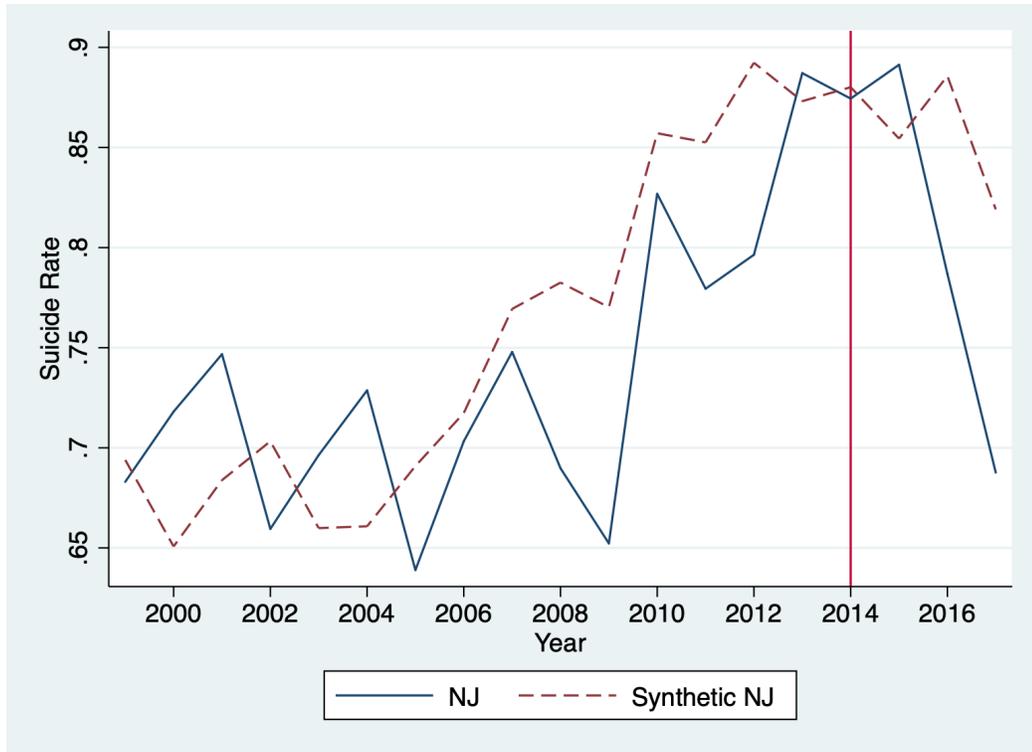
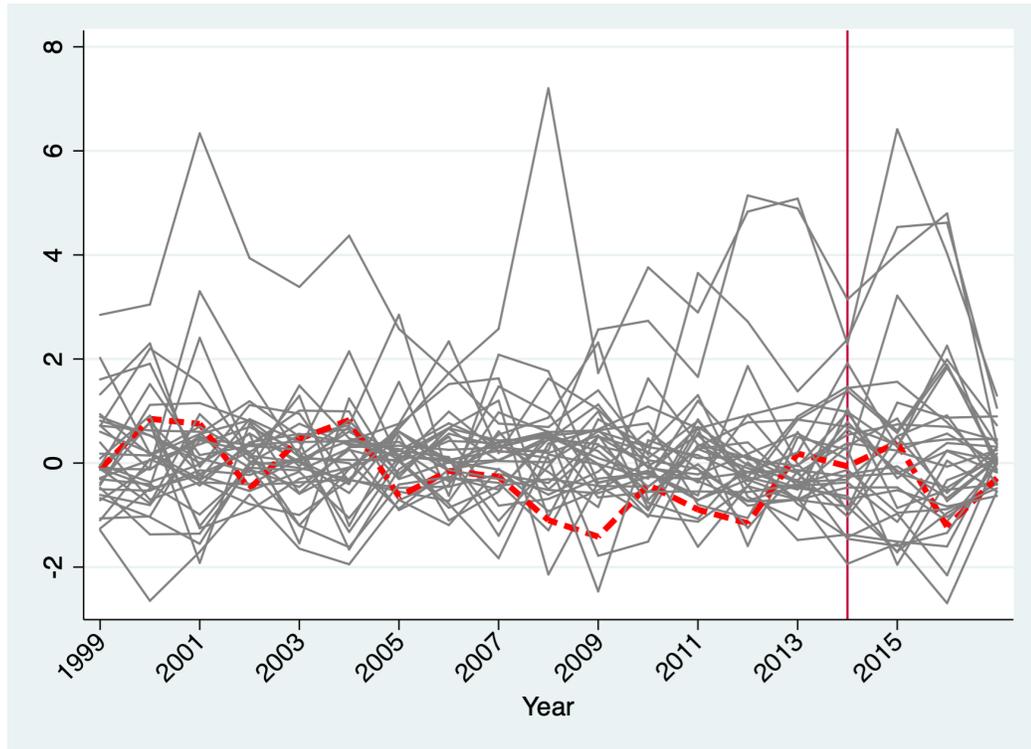


Figure 1.10: Trends in Annual Averaged Suicide Rates for New Jersey and the Synthetic New Jersey

Figure 1.11: **Randomization test of significance of treatment effects.**



1.7 Discussion

In this study, I utilize both a differences-in-differences model and a synthetic control model to estimate the causal effect of law legalizing Internet gambling on suicidality, specifically the suicide rate. I exploit the variation induced by the law to estimate the effect of Internet gambling on the marginal gambler. That is, I estimate the effect of those in the general population who engage in legal Internet gambling purely as result of the practice becoming legal. As of the time of writing, this is the first study to provide causal estimates of the effect of Internet gambling on suicide at the population directly using administrative data at the population level and the first to do so using DD framework and the also the first to use synthetic controls. To the best of my knowledge, the only other study employing causal techniques is Philander and MacKay (2014) which uses survey data from the UK to estimate the causal effect

of Internet gambling on problem gambling severity.

There exists significant literature on Internet gambling and pathological gambling, yet few studies in this area take place at the population level or using administrative data. The best research relies on survey data, many using samples of in-treatment pathological and problem gamblers, groups that do not necessarily resemble the general population. The literature frequently finds a strong relationship between both Internet gambling and pathological gambling as well as pathological gambling and suicide. However, in this study I find no such link. In no specification do I find an effect other than zero. I am confident these results exclude all but very small effects on suicidality. This is especially evidenced in the primary model Poisson estimation which places the effect of the law in the interval $[-0.09\%, 0.14\%]$ over pre-treatment means. In my analysis of Google search data, I find a significant increase in the relative search volume for searches containing “gambling addiction,” $[20.20\%, 65.48\%]$. This suggests that the implementation of legal gambling induced higher rates of searches for information on disordered gambling, coupled with Khazaaal et al. (2008) finding that shows that upwards of 70% of those with psychiatric conditions seeking health information online, this may reflect a causal increase in individuals with gambling disorders.

The findings of this study are inconsistent with the only other causal study, Philander and MacKay (2014). In that study, the authors find that Internet gambling participation had no effect on problem gambling severity and, when controlling for the breadth of gambling activity, Internet gambling reduced problem gambling severity. The experience of established pathological gamblers may differ significantly than that of the general population, hence extrapolating this onto the entire population distribution may deviate from actual reality. By using DD model and SCM frameworks, I am able to control for the endogeneity of Internet gambling participation that is present in the existing literature that is likely strongly correlated with problem gam-

bling and suicide. Here, I find that evidence that legalizing Internet gambling likely caused in an increase gambling disorder or at the very least, induced a behavioral response strong enough to concern individuals of their gambling behavior yet find no evidence of the mental health that result, namely suicidality and depression.

One possible explanation for these findings is that legal gambling is regulated and may prohibit the unscrupulous behaviors of Internet gambling providers that have been postulated to increase problem gambling severity. In the singular case of New Jersey, the law provides for multiple consumer protections including responsible gaming features. These include self-exclusion lists that bar players from all authorized sites within the state, cool-off periods which prevents a player from betting after a predetermined session length for a period of time, daily bet limits, ect. (Nower and Volberg 2017; The State of New Jersey 2013). These features present in New Jersey may be mitigating the potential maleffects of Internet Gambling. Roughly 14% and 7% of gamblers used responsible gaming features in 2014 and 2015, respectively (Nower, Caler, and Guan 2016; Nower, Caler, and Peters 2017).

Another possible explanation, as postulated by Philander and MacKay (2014), is that Internet gambling can profitably offer games with smaller bets compared to physical casinos. This is evidenced by the fact that average in New Jersey by 2015 was less than \$1.00, and the median bet less than \$0.20 (Nower, Caler, and Guan 2016; Nower, Caler, and Peters 2017). With the possibility of placing smaller bets, Internet gamblers are able to play for similar periods of time with potentially lesser negative financial effects. This may help mitigate one of the factors that lead to suicidality.

This study has some limitations. Completed suicides are relatively rare events. Additionally, suicidality is a symptom often present late in the pathology of pathological gambling. Thus, the window to realize a measurable effect might be outside the frame of the current study. However, as seen in a Spanish sample of gamblers in treatment, the onset of problem gambling happened in months rather than years

after the introduction of Internet gambling (Chóliz 2016). Thus, the current post-treatment window may potentially be sufficient to yield a measurable effect. The combined findings from the Google trends analysis provide evidence that the law induced individuals into Internet gambling, and for some, may have caused an increase in disordered gambling. This highlights that more time may be needed to find evidence of an effect on suicide, or that the change is too small to be measured.

A second limitation is that, absent a significant effect on suicide, I am not able to fully address the relationship between Internet gambling and pathological gambling when disregarding the Google trends analysis. Though my findings are consistent with Philander and MacKay (2014), challenging the hypothesis that Internet gambling is inherently more dangerous than non-Internet gambling, I do not believe this study provides sufficient evidence to do so.

Shrinking tax revenues and increases in interstate gambling competition has spurred further expansion in gambling opportunities, especially since the Great Recession (Rose and Bolin 2012; American Gaming Association 2017). Given this increase in demand, seemingly universal access to the Internet, and a population more connected than ever, states see Internet gambling as a way to boost revenues and close deficits. The existing literature may be reason to pause such efforts. However, when laws implementing Internet gambling are thorough, responsible, and contain ample consumer protections, evidence shows that these negative effects may be mitigated, though more research is needed to fully understand this complex system.

CHAPTER 2

THE EFFECT OF INCREASED COST-SHARING ON LOW-VALUE SERVICE USE

with Jonathan Gruber, Johanna Catherine Maclean, Bill Wright, and Kevin G.
Volpp

2.1 Introduction

Healthcare expenditures in the United States are extremely high: in 2017 the U.S. spent \$3.5 trillion dollars on healthcare or \$10,739 per person; this figure represents 17.9% of the nation's gross domestic product (GDP) (Martin et al. 2019). The U.S. has the highest per capita healthcare expenditures of any Organization for Economic Co-operation and Development (OECD) country, indeed U.S per capita spending is 145% of the OECD median (Anderson, Hussey, and Petrosyan 2019). Healthcare costs outpace inflation, suggesting that these costs will place even greater financial pressure on U.S. patients, governments, and employers in the future. Unfortunately, this high and rising spending on healthcare does not appear to translate into improved

health outcomes for Americans. For example, the U.S. has the highest obesity rate among OECD countries and ranks 28th out of the 34 OECD member nations in terms of life expectancy (Organization for Economic Co-operation and Development 2014).

These statistics suggest that value in terms of health given expenditures on health-care services in the U.S. is not optimal: expenditures are very high while outcomes are very poor. Indeed, in 2012 the Academy of Medicine estimated that 30% of annual healthcare expenditures in the U.S., totaling roughly \$849 billion¹, were wasteful (Smith et al. 2013). Moreover, one half of these wasteful expenditures were attributable to inefficient healthcare service use. Healthcare scholars note that reducing even a small share of inefficient service use could lead to major cost-savings for the U.S. healthcare system (Colla et al. 2017).

In response to the expenditure of large amounts of resources on services that provide little value in terms of improving health, policymakers, payers, healthcare professionals, and employers have displayed great interest in developing strategies to contain healthcare costs by reducing inefficient service use. Many of these strategies involve increasing cost-sharing for patients. The expectation, based on basic demand theory, is that increasing out-of-pocket costs will reduce healthcare service use and, in turn, overall costs. A concern with such a strategy, however, is that increasing across-the-board patient cost-sharing may reduce use of *both* high- and low-value care since consumers often have difficulty differentiating between the two types of care. Indeed, recent economic research on high deductible health insurance plans provides empirical support for this concern (Huckfeldt et al. 2015, Haviland et al. 2016, Beeuwkes et al. 2011, Brot-Goldberg et al. 2017). Importantly, short-run reductions in valuable healthcare services may *increase* downstream costs as diseases remain undiagnosed or undertreated, and conditions worsen without appropriate care (Fendrick et al. 2001, Heisler et al. 2010, Campbell et al. 2011, Hsu, Price, Huang, et al. 2006, Trivedi,

¹Inflated by the authors to 2019 dollars using the Consumer Price Index.

Moloo, and Mor 2010).

In recognition of the need for a nuanced strategy, the concept of value-based insurance designs (VBID) emerged in the late 1990s (Fendrick et al. 2001). VBID designs are based on basic economic principles: use out-of-pocket prices (the prices most visible to patients) to steer patients toward the most clinically indicated healthcare. Further, VBID design is grounded in behavioral hazard theory (Baicker, Mullainathan, and Schwartzstein 2015). In the jargon of VBID, a low-value service is defined as healthcare that is less appropriate than other widely available service alternatives for a given clinical situation. Alternatively, a high-value service is defined as healthcare that is more appropriate than service alternatives. This strategy is differentiated from the larger waste and inefficiency literature, for instance, prior authorization and pre-certification, given that (1) services are directed by clinical relevance, often varying at the diagnosis level, and (2) beneficiaries willing to pay can choose to consume low-value service. In practice, a VBID program will increase patient cost-sharing for low-value care, decrease cost-sharing for high-value care, or some combination of these two approaches, although most programs to date have focused on decreasing cost-sharing for high-value care (Volpp, Loewenstein, and Asch 2012). Programs may be applied to all patients or specific types.

Due to its conceptual appeal, VBID principles have gained traction among policymakers, employers, and healthcare professionals. Indeed, this concept has been incorporated into the framework of the Affordable Care Act (ACA) through, for example, co-payment waivers for all preventive services that receive an ‘A’ or ‘B’ rating by the U.S. Preventive Services Task Force². Medicare Advantage has explicitly adopted VBID into its Health Plan Innovations Initiative³ and VBID principles are being applied in demonstration projects within Tricare, a healthcare program that

²The full list of services can be located here (last accessed October 19th, 2019).

³The program focuses on reduced cost-sharing for high-value services: Link (last accessed October 19th, 2019).

provides health benefits to U.S. Armed Forces military personnel and retirees, and their dependents⁴. U.S. states have also implemented such principles into their employee benefit plans (Hirth et al. 2016, Gibson et al. 2015, Burns, Dyer, and Bailit 2014). A survey of large firms documents that 81% either currently incorporate VBID principles in their benefit packages or plan to introduce these principles in the near future (Gibson et al. 2015).

Beginning in 2012, the American Board of Internal Medicine Foundation launched the ‘Choosing Wisely’ initiative⁵. The objective of this initiative is for each participating physician society to identify five specialty-specific low-value services and to encourage their member physicians to avoid these services. For example, the American Academy of Family Physicians recommends against routine screens for prostate cancer using a prostate-specific antigen (PSA) test. Over 70 professional societies have developed such a list, leading to over 500 services listed as low-value (Gawande et al. 2014). Thus, VBID principles are likely to become increasingly prevalent in both benefit designs and clinical practice in the near future.

Despite its conceptual appeal, the empirical basis for VBID strategies is not strong. A number of studies, reviewed in Section 2.2.2, show that reduced cost-sharing for high-value services can increase use of these services, but beneficiary response has been modest at best and does not appear to reduce overall costs. At the same time, there is little evidence on the degree to which raising the cost of low-value services lowers use of these services and most work to date has focused on medications and programs adopted in hospital settings (Colla et al. 2017).

The contribution of this paper is to provide the first empirical evaluation of higher cost-sharing for a range of low-value services in a range of healthcare settings using gold-standard quasi-experimental methods. In particular, we study the impact of an innovative VBID program implemented by a large public employer in the state

⁴Pilot projects focus on value-based reimbursements: [Link](#) (last accessed October 19th, 2019).

⁵For more details please see: [Link](#) (last accessed October 19th, 2019).

of Oregon on utilization of services determined by plan administrators to be low-value (Kapowich 2010). This program substantially increased patient cost-sharing for sleep studies, upper gastrointestinal endoscopies, advanced imaging services, and potentially over-used surgery services (e.g., spinal surgeries for pain). Copayments for these services increased by \$100 to \$500 and coinsurance rates increased by 10 to 40 percentage points.

We use a differences-in-differences (DD) design to compare changes in outcomes before and after VBID program implementation between beneficiaries at the participating employer and beneficiaries receiving health insurance through a comparison group of employers. The VBID program increased patient cost-sharing beginning in October 2010. We leverage granular, administrative claims data between 2008 and 2013 to examine program effects on out-of-pocket payments, targeted service use, and healthcare costs (overall, and for both targeted and non-targeted services), allowing us to track effects for three full years post-implementation. We have several findings. First, we document a clear first stage: the program substantially increased cost-sharing for patients. Second, our estimates suggest that the VBID program significantly reduced use of targeted low-value services. Demand for these services is relatively inelastic: our overall estimated elasticity is -0.26 for all services. However, we hypothesize that, due to complementary across the targeted services (e.g., patients may receive an imaging service in advance of a surgery, with both services subject to increased cost-sharing), that the proper demand elasticity is roughly double our estimate: -0.52. Therefore, our estimated elasticities are somewhat larger than those documented within the general service use literature (reviewed in Section 2.5.4). The fact that our implied elasticities are larger than those for general healthcare services is perhaps not surprising as we focus on low-value services, which are, by definition, less likely to lead to health improvements than a general set of services, and therefore more likely to be discretionary. Third, we do not observe any evidence that

patients substituted to other (non-targeted) services or that there were increases in overall healthcare costs, two key concerns related to the application of VBID designs. These results illustrate for the first time that significant increases in cost-sharing on low-value services can greatly reduce their use without leading to unintended and downstream consequences.

This manuscript is organized as follows: Section 2.2 briefly describes the literature on VBID effects. The VBID program examined in this study is outlined in Section 2.3. Data, variables, and methods are outlined in Section 2.4. In Section 2.5 we present our main findings, and Section 2.6 reports robustness checks. Section 2.7 offers a discussion.

2.2 A Conceptual Framework and a Brief Review of Previous Research on VBID Programs and Low-value Healthcare Services

2.2.1 A Conceptual Basis for VBID Programs: Behavioral Hazard and Insurance Design

Baicker, Mullainathan, and Schwartzstein (2015) discuss the importance of behavioral hazard in the context of optimal insurance design and, of particular relevance for our study, note that this phenomena, in combination with moral hazard, ‘provides a theoretical foundation for value-based insurance designs’ (page 1661). Traditional insurance models allow for moral hazard, that is, consumers set marginal personal benefit (MPB) to marginal personal cost (MPC) when selecting their optimal quantity of healthcare services ($MPB = MPC$), and MPB may be greater than the marginal social benefit (MSB). Thus, the consumer overuses services from the perspective of the social planner; the solution is to apply cost-sharing ($a > 0$) for patients

such that $MPB = MBC + a = MSC$ and to reign in use to align with the social planner's allocation.

Behavioral hazard is distinct from moral hazard and implies that consumers have erroneous beliefs regarding the marginal benefit of healthcare services, either under- or over-estimating this benefit. This error is termed b , which we assume to be greater than zero ('positive behavioral hazard') given our focus on low-value care which by definition is less valuable for most patients than other treatment options. The consumer believes that a particular service has greater marginal benefit than it actually does. The consumer therefore equates $MPB + b = MBC$ when selecting her optimal treatment. In this context, additional cost-sharing must be applied to account for these decision errors (i.e., cost-sharing that exceeds the level required to counteract moral hazard). The social planner should apply cost-sharing such that the consumer's optimal treatment concurs with $MPB + b = MBC + b$.

Baicker, Mullainathan, and Schwartzstein (2015) contend that features of some health conditions increase the likelihood of behavioral hazard. In particular, the authors note that severity and visibility of symptoms are important. Conditions with painful and/or disruptive symptoms are plausibly the most salient to consumers and therefore likely to lead to positive behavioral hazard. Many of the services we study (e.g., surgeries for pain, sleep studies for diagnoses and treatment of sleeping disorders, knee and shoulder arthroscopies, and knee and hip replacements) are used to treat painful or troublesome symptoms. Thus, patients contemplating undergoing these procedures plausibly display positive behavioral hazard.

While Baicker, Mullainathan, and Schwartzstein (2015), and other economic models and studies, focus on standard price effects, we note that changing the price of a healthcare service may also send a signal to the patient regarding service value. In our context, if services require additional cost-sharing due their perceived lower-value, this change, independent of price effects, may lead patients to change their

belief of service value, potentially reducing cognitive errors (i.e., *b*). This phenomena is observed in other settings in which prices are used to require consumers to internalize social costs such as taxation of sin goods (Gostin 2017). As discussed in Section 2.3.1, OEBC administrators went to great lengths to publicize the VBID, explain program objectives and reasons for elevated cost-sharing to beneficiaries, and encourage discussions between patients and healthcare professionals regarding other treatment options. Thus, listing a service as an ACT service could have a signaling effect and, potentially, a direct information effect regarding service value. While we cannot distinguish between these mechanisms, all suggest that use of targeted low-value services will decline post-VBID.

2.2.2 Previous Research on VBID Programs

In this section we briefly review the VBID literature. For more detailed discussions of this literature, we refer readers to excellent reviews by Tang et al. (2014); Gibson et al. (2015); Look (2015); Colla et al. (2017); and Agarwal, Gupta, and Fendrick (2018).

A large and growing body of research seeks to estimate the impact of VBID programs on healthcare utilization, overall utilization, and patient outcomes. The majority of studies have examined VBID programs that reduce cost-sharing for high-value services, in particular prescription medications. The literature focusing on reduced cost-sharing for high-value medications has shown some evidence that lowered cost-sharing leads to improvement in adherence (Maciejewski et al. 2014, Maciejewski et al. 2010, Farley et al. 2012, Look 2015, Agarwal, Gupta, and Fendrick 2018, Tang et al. 2014, Krack 2019).

However, the estimated effect sizes are often small in magnitude and potentially not clinically meaningful. For example, when Aetna members were randomized to free cardiovascular medication for the year following a heart attack, average adher-

ence increased from 39% to 45%, or 15% (Choudhry et al. 2011). Given the very serious nature of this health shock and the complete removal of copayments faced by patients, this effect size is surprisingly modest. In addition, a recent literature review suggests that VBID programs can improve medication adherence, on average, by three percentage points per year (Lee et al. 2013).

Problematic research designs may be an issue within the VBID literature that examines medication adherence. In a recent review of the literature, Look (2015) documents that many VBID studies suffer from serious methodical issues. For example, numerous studies lack a comparison group and instead rely on before-after designs to estimate VBID effects. Look notes that these study limitations impede our ability to understand VBID effectiveness in this context and encourages the use of more credible research designs within the literature.

Further, as articulated by Volpp, Loewenstein, and Asch (2012), reductions in copayments for high-value care potentially have modest effects due to the asymmetry of losses and gains (i.e., people are more adversely affected by a loss than an equivalent dollar gain) and the fact that copayment reductions may be less visible to non-adherent plan members (the target of programs that reduce cost-sharing) compared to the impact of a copayment increase (which largely affects adherent plan members). In other words, rewarding adherent patients with reduced cost-sharing likely has less effect on service use and associated costs than penalizing non-adherent patients or patients using low-value care with increased cost-sharing. The authors argue, but do not test empirically, that increasing cost-sharing for low-value care may be more effective than previous efforts which focused on lowering cost-sharing for high-value care.

In addition to the documented modest changes in medication use, VBID programs that target high-value care do not appear to meaningfully reduce healthcare costs (Maciejewski et al. 2014, Hirth et al. 2016, Lee et al. 2013, Seplveda et al. 2016,

Agarwal, Gupta, and Fendrick 2018). For instance, in a recent review of the literature, Agarwal, Gupta, and Fendrick (2018) document that VBID programs with lower cost-sharing for high-value medications do not reduce healthcare costs. These studies imply that VBID programs that reduce cost-sharing for high-value services may not generate the desired savings through better enrollee health or lower rates of adverse events triggering emergency room visits or hospitalizations.

A handful of studies has examined the impact of increasing cost-sharing for a particular low-value healthcare service: emergency department (ED) usage (Choudhry et al. 2012, Nair et al. 2010, Siddiqui, Roberts, and Pollack 2015). For example, Siddiqui, Roberts, and Pollack (2015) take advantage of state-level changes in Medicaid cost-sharing for ED usage induced by the 2008 to 2010 recession. Specifically, the Deficit Reduction Act of 2005 gave states the authority to impose cost-sharing strategies on healthcare services. During the 2008 to 2010 recession, eight states adopted cost-sharing for non-urgent ED. Using a differences-in-differences design, the authors find no evidence that increases in cost-sharing impacted ED utilization within the Medicaid population. Hsu, Price, Brand, et al. (2006) and Hirth et al. (2016) document that increases in cost-sharing do reduce ED usage, although the effects on overall healthcare costs are inconclusive. Thus, the ability of VBID programs to reduce low-value healthcare service utilization and costs may be context-specific⁶.

Necessary conditions for VBID programs to affect utilization and costs are (1) employee awareness of the program, and (2) employee ability to differentiate between high- and low-value care (Baicker and Levy 2015). However, the available evidence suggests that, in many cases, employees have incomplete knowledge of program existence and the specific services targeted (Henrikson et al. 2014). Thus, lack of employee knowledge may have hindered previous efforts to effectively utilize VBID

⁶Although beyond the scope of this study, there is a substantial economic literature that investigates the impact of cost-sharing on utilization more broadly than within the context of VBID programs (Baicker and Goldman 2011).

principles in benefit designs.

In summary, the VBID literature suggests that reducing cost-sharing for high-value medications may increase adherence, although the impacts on patient outcomes, overall service utilization, and costs is less clear. Moreover, the literature that studies the impact of increasing cost-sharing for low-value services is nascent and few studies have considered healthcare services beyond ED usage. Much of the existing VBID literature is subject to numerous methodological flaws. In this study, we aim to address some of these research gaps by using a gold standard quasi-experimental research design (DD) to study the effect of increasing patient cost-sharing for a broad set of healthcare services that are believed to be overused by patients.

2.3 The Current VBID Program

2.3.1 Program Details

We examine the impact of a VBID program implemented at a large public employer in the state of Oregon: the Oregon Educators Benefit Board (OEBB)⁷. OEBB provides benefits for approximately 150,000 current or retired employees of school districts, education service districts, and community colleges in the state of Oregon and their dependents. These benefits are provided through two contracted health plans over our study period Kaiser Permanente, which offers HMO plans, and Moda Health, which provides PPO plans. Prior to October 1st, 2012 OEBB also offered PPO plans through Providence Health Plan. OEBB offers different plan designs to the school districts, education service districts, and community colleges/charter schools it serves across the state of Oregon.

⁷Please see here (last accessed October 19th, 2019)

Table 2.1: **Share of OEGB Beneficiaries Covered by Moda**

Coverage Year	Share
2008 Q4 to 2009 Q3	0.682
2009 Q4 to 2010 Q3	0.725
2010 Q4 to 2011 Q3	0.767
2011 Q4 to 2012 Q3	0.790
Full Period	0.741

Note: OEGB beneficiaries were covered by three different insurers: OEGB (for which we have data), Kaiser Permanente, Providence Health. We have access to all Moda Health data. The source of these data are OEGB annual reports. More details available on request from the corresponding author.

For this paper we use data on all Moda Health plans, which cover 75% of the OEGB beneficiary population; we were unable to obtain comparable data for Kaiser Permanente and Providence Health (details available on request). Table 2.1 reports the share of the OEGB population captured in our data in each year of our study period. Although we cannot study Kaiser Permanente and Providence Health plans, the OEGB VBID impacted all plans held by OEGB beneficiaries (thus, impacted plans managed by Moda Health and Providence Health and, to a lesser extent, Kaiser Permanente, but the final plan imposes a range of utilization management techniques on healthcare professionals which likely limit low-value service use)⁸. Hence, we have no reason *ex ante* to suspect that beneficiaries switched to a non-Moda Health insurance plan in response to the VBID (e.g., to avoid exposure to increases in cost-sharing).

OEGB plan years run October 1st to September 30th. Beginning October 1st, 2012, Moda Health re-organized its plan offerings. More specifically, several plans

⁸We note that Kaiser Permanente plans were later to adopt cost-sharing increases and for a smaller set of services. Our conversations with Kaiser Permanente administrators revealed that this system relies on healthcare professional side gatekeeping to contain costs. In particular, administrators do not believe that they require additional demand-side cost-containment strategies. Thus, they did not participate in the VBID apart from some increased cost-sharing for advanced imaging services. We contend that it is very unlikely that a beneficiary with a PPO plan would, in response to increased cost-sharing for a small set of infrequently used services, elect to switch to a Kaiser Permanente HMO plan which employs an arsenal of techniques designed to reduce service use.

were eliminated and new places were developed. Thus, we close the study period in Q3 2012 to avoid confounding from this major change in plans available to beneficiaries. However, in robustness checking (Section 6), we have incorporated additional post-VBID data and results are not appreciably changed, suggesting that the plan changes did not dilute VBID effects. Over our study period, a total of ten different PPO plans were offered to OEGB beneficiaries.

OEGB implemented its VBID program on October 1st, 2010⁹. Administrators at OEGB conducted an extensive effort to develop the VBID program, this effort is outlined in detail in Kapowich (2010). The board hired a healthcare benefit-consulting firm, and communicated with key stakeholders (i.e., payers, healthcare professionals, and beneficiaries) in developing the set of services that would be targeted for increased cost-sharing. Briefly, OEGB administrators compared member utilization to use in nationally representative samples of commercial insurance plans, and consulted the scientific evidence on service use and effectiveness (e.g., Dartmouth Atlas, Cochrane Review, medical journals). In general, the services selected for enhanced patient cost-sharing were assessed to be, across several metrics, over-used and low-value by a range of experts and advocates for members and payers. One of the co-authors, who is a practicing physician in a large healthcare system in the U.S., reviewed the targeted

⁹The VBID also included a Value Based Pharmacy program (VBP). The VBP focused on four medication classes: generic, value, preferred, and non-preferred. Cost-sharing for generic drugs was eliminated and reduced for value and preferred drugs while cost-sharing for non-preferred drugs increased. One concern with our analysis is that we may not be able to isolate the effect of the ACT on outcomes from any VBP that went in at the same time. However, we suspect that because the ACT and VBP targeted very different services (overused and low-value procedures vs. both high- and low-value medications), any cross-program spillover effects are likely small. Our review of the medications targeted by the VBP suggests that they are not directly linked with most of the healthcare outcomes we study here. For example, we study a set of general healthcare procedures (i.e., sleep studies, advanced imaging services, endoscopies, and over-used surgeries) for which most medications targeted by the VBP do not appear to be a direct complement or substitute. At the same time, very few of the targeted medications would be used directly in conjunction with the procedures we study here (one of the authors is a practicing physician in a major healthcare system in the U.S. and has reviewed all affected services and medications). In unreported analyses, we explored the effect of the VBP on use of targeted medications and found no evidence that the program reduced utilization. The null findings of the VBP on targeted services (i.e., the first stage) further supports our hypothesis that this program did not influence the outcomes we study here. VBP results available on request.

services and confirmed that these services are likely low-value for the vast majority of patients.

Table 2.2: **Added Cost Tier (ACT) Services and Cost-Sharing Increases**

Variable:	Services	Copayment Change Date	Copayment Increase	Coinsurance Change Date	Coinsurance Increase
Sleep Studies	Home and Facility	Oct. 1, 2010	\$100	Oct. 1, 2011	10-20 ppts*
Upper GI	Upper GI	Oct. 1, 2010	\$500	Oct. 1, 2010	10-40ppts*
Endoscopy	Endoscopy	Oct. 1, 2011	\$100		
Advanced Imaging	CT, MRI, PET	Oct. 1, 2010	\$100	Oct. 1, 2011	10-20pp pts
Potentially Overused Surgeries	Selected Surgeries†	Oct. 1, 2010	\$500	Oct. 1, 2010	10-40 ppts*

Note: : OEGB plan year October 1st-September 30th. Comparison company plan year January 1st-December 31st. ppts=percentage points.

*Coinsurance increase depends on plan.

†Spine Surgery for pain; Knee and Shoulder Arthroscopy; Hip and Knee Replacements

Table 2.2 outlines the affected services, the date of the cost-sharing increase, and the level of the cost-sharing increase. The affected services include: sleep studies (home studies and studies conducted in a healthcare facility); advanced imaging services (computerized tomography [CT] scan, magnetic resonance imaging [MRI], and positron tomography scan [PET], and upper gastrointestinal endoscopies); and potentially overused surgeries including spine surgeries for pain, knee and shoulder arthroscopies, and hip and knee replacements (we refer to the potentially overused surgeries as ‘surgeries’ henceforth).

We note that none of these services or any service of which we are aware are universally of low-value and instead there is heterogeneity in value across patients (Porter 2010). For example, home sleep studies typically are much higher value than

facility-based sleep studies, which offer similar testing often at more than ten times the price. Additionally, upper gastrointestinal endoscopies (which OEBC classifies as low-value) can be life-saving when preformed for gastrointestinal bleeding while advanced imaging can be necessary for diagnosis, and spinal surgery can also be clinically indicated. However, many of these procedures may be over-utilized on average and in any given proportion the ratio of these services that are high- vs. low-value is likely quite low. Since we cannot judge appropriateness of the service use in our data, we simply evaluate here the impact of increased cost-sharing on service utilization.

OEBC administrators, however, recognized the difficulty in classifying low-value services at the plan level and incorporated this challenge into the program. In particular, as a means to promote valuable use of targeted services, healthcare professionals are permitted ample discretion in application of the increased cost-sharing. Indeed, the program was specifically developed to exclude emergency service use and care related to life-threatening health conditions from the increased cost-sharing. Our conversations with OEBC administrators indicate that an objective of the program was to exempt patients for whom an ACT service was likely to be of high-value from increased cost-sharing. As an example, outlined in Section 2.4.2, cancer patients are not exposed to additional cost-sharing.

ACT copayments and coinsurance must be paid in addition to a beneficiary's plan deductible, regular copayment, or coinsurance. Moreover, the increased cost-sharing does not accrue to the plan's out-of-pocket maximum. OEBC administrators intended the increased cost-sharing to act as an incentive for beneficiaries to seek out potential cost-effective alternatives and to discuss these alternatives with their healthcare professional(s).

Before the implementation of the ACT, there were no copayments for these services and there were coinsurance rates ranging from 10% (sleep studies and advanced

imaging) to 30% (endoscopy and surgery). Copayments were increased for all services we study on October 1st, 2010 and are reported in Table 2.2¹⁰. The copayment increases are as follows: \$100 for sleep studies, \$500 for endoscopies (copayments for this service were reduced to \$100 on October 1st 2011), \$100 for advanced imaging services, and \$500 for surgeries considered here. Coinsurance was increased by 10 percentage points on October 1st 2010 for endoscopy and surgery (taking each to 40% coinsurance), and by 10 percentage points on October 1st 2011 for sleep studies and advanced imaging (taking each to 20% coinsurance). Thus, the changes in cost-sharing represent a substantial increase in out-of-pocket payments for these services.

As noted in Section 2.2.2, a concern with analysis of any VBID program is the extent to which beneficiaries are aware of the program. If awareness is poor, the program will not likely lead to substantial changes in utilization. Kapowich (2010) provides a detailed review of the extensive efforts OEBC administrators applied to ensure awareness of the VBID program among beneficiaries. To support effective messaging of the program to beneficiaries, OEBC administrators contracted with a media-consulting firm to create an effective communication plan. Prior to the October 1st, 2010 launch date, administrators provided information through the OEBC website, e-mail, pamphlets, and in-person meetings. Indeed, our conversations with administrators suggest that beneficiaries were well aware of the program. For example, OEBC administrators indicated that their benefits office received a considerable increase in emails and calls in reference to the program, further suggesting that the beneficiaries were aware of the VBID. While the extent to which beneficiaries are aware of any program, OEBC administrators clearly exerted substantial efforts to promote program awareness and the noted anecdotal evidence, in addition to our empirical findings, suggest that these efforts were effective.

¹⁰OEBC changed cost-sharing for several other services (e.g., bariatric surgery). We attempted to explore the impact of the VBID on utilization, but small sample sizes (these services are infrequently used) prevented us from examining these outcomes with any confidence. Details available on request.

2.4 Comparison Group, Data, and Methods

2.4.1 Comparison Group

In this study, our objective is to estimate the causal effect of the OEGB VBID program on healthcare utilization using differences-in-differences (DD) models. Thus, we require a suitable comparison group for OEGB. To this end, we obtained analogous health insurance claims data on three additional public employers in the state of Oregon that did not implement VBID programs before or during our study period (2008 to 2012)¹¹.

While the companies in our comparison group were not willing to have their names included in our study, we are able to provide details on these companies. One company is an electrical utility company based in Portland. The remaining companies are counties within the state of Oregon. All three companies are located in the northwest corner of Oregon and are therefore subject to the same state-level policies and geographic factors as OEGB. Figure 2.1 displays graphically the counties in which the comparison companies are located. Further, all these companies, similar to OEGB, are for public employers. The three employers in our comparison group offer one contracted point of service plan to beneficiaries through Providence Health, and these plans did not change throughout our study period. Plan years at the comparison companies run January 1st to December 31st¹².

¹¹These three companies have requested that we remove their names from the manuscript. We were not able to obtain additional data from Moda Health on different employers to form a comparison group. However, one of the authors is a researcher at Providence Health and was able to access data on additional companies, which we use to form our comparison group. Details available on request.

¹² Although the comparison company plan years run January to December, we use data on comparison companies to match the OEGB plan years (October to September). Overall, the claims data provided by Moda and Providence Health was very similar in format; indeed Oregon has an all-payer claims database which requires a common data format for reporting purposes. Thus, we are very confident in our ability to harmonize variables across sources.

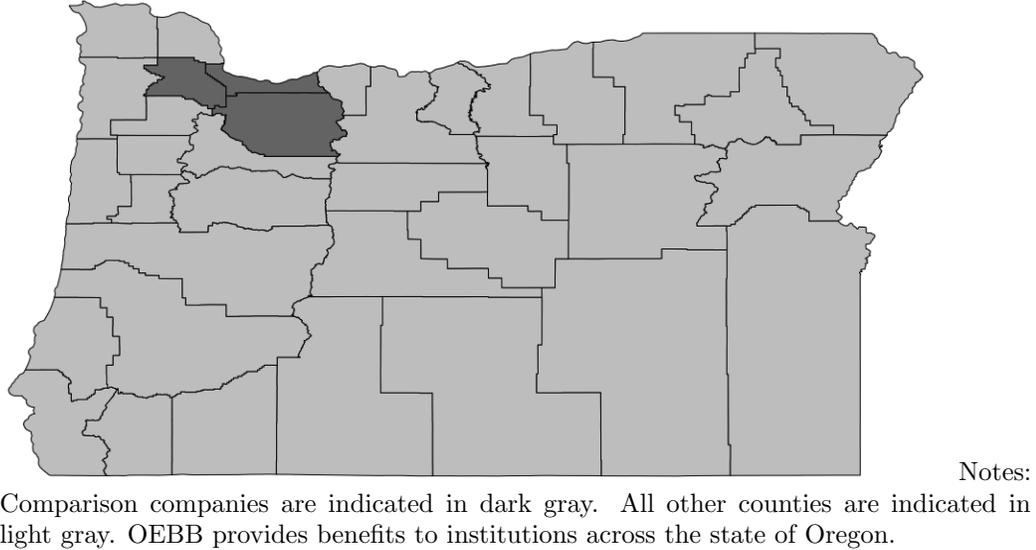


Figure 2.1: Geographic Location of Comparison Companies within the State of Oregon

2.4.2 Data

To study the impact of the OEGB VBID program, we use granular, line-by-line healthcare claims data on all four companies; we obtained these data through data use agreements with Moda Health (OEGB data) and Providence Health (comparison group company data). These data include all healthcare encounters for which Moda Health and Providence Health insurance plans were used for payment of comparison group beneficiaries.

We make several exclusions to the claims data to form our analysis sample. First, we exclude all beneficiaries under age 19 and over age 64; we exclude such beneficiaries as they potentially have access to public health insurance (e.g., Medicaid, Medicare) and therefore could be insulated from the VBID attributable increases in cost-sharing. Second, we exclude 46 beneficiaries who changed jobs across the four employers during the study period.

Unfortunately, from a design perspective, OEGB implemented other changes in cost-sharing during our study period. In particular, OEGB increased deductibles,

out-of-pocket maximums, and office and urgent care visits copayments/coinsurance (more details on the specific changes are available on request). These changes occurred at the same time as some of the increases in cost-sharing for low-value services. A concern with our analysis is that the effects we estimate reflect the impact of the above-noted changes in general cost-sharing rather than the increases in cost-sharing for low-value services. To isolate cost-sharing effects, we leverage differences in the increases in general cost-sharing that occurred across plans.

In particular, three plans (named ‘ODS Plan 3,’ ‘ODS Plan 4,’ and ‘ODS Plan 5’) experienced much larger increases in general cost-sharing than the remaining plans (named ‘ODS Plan 6,’ ‘ODS Plan 7,’ and ‘ODS Plan 8’). In the first set of plans, the copay was substantially increased for specialist office visits, and the deductible was raised between 50% to 200%. In the latter set of plans, there was no change for office visits, and deductibles rose only at most by 33%. We use ODS Plans 6 to 8, i.e., the OEGB plans that experienced the lowest increases in cost-sharing for specialist office visits and deductibles during the study period, to form our treatment group. We return to this issue more formally in robustness checking (Section 2.6).

After exclusions, we are left with 742,670 beneficiary/quarter observations in our analysis sample, 645,220 at OEGB; 24,177 at comparison company 1; 37,713 at comparison company 2; and 35,560 at comparison company 3. We aggregate the data to the beneficiary-quarter-year level. Thus, observations capture per beneficiary per quarter cost-sharing and service utilization. Our study period includes 16 quarters of data, eight quarters pre-VBID and eight quarters post-VBID.

We obtained the specific procedure and diagnosis codes (CPT, HCPCS, and ICD-9) used by OEGB billers to determine if a particular service was subject to increased cost-sharing. Over our study period, administrators at OEGB changed the specific codes used to classify ACT services to some extent (exact coding available on request). Our conversations with administrators suggest that these changes were based on

feedback from healthcare professionals and billers, and continued analysis of services used by beneficiaries and reviews of the available medical literature in efforts to create a more accurate classification of ACT services. In our main analyses we examine services that were ever listed as an ACT. However, as we document in robustness checking (Section 2.6), our results are highly stable if we instead only incorporate codes for ACT that were unchanged across the study period.

Certain health conditions (e.g., cancer, trauma) exempt beneficiaries from the ACT increased cost-sharing; as described in Section 2.3.1 these exemptions were specifically built into the VBID to in recognition that there is heterogeneity across patients in service value and to prevent patients for whom the specific ACT service is likely to be of high value from incurring higher cost-sharing. We incorporate these exemptions into the coding of our outcome variables (described later in the manuscript). More specifically, if diagnoses codes indicate that a patient suffers from a trauma or health condition that would exempt him/her from the ACT cost-sharing, we do not code that service as an ACT service.

2.4.3 Outcome Variables

To test whether the VBID cost-sharing increases were in fact passed through to beneficiaries, we measure the sum of copayments and coinsurance for each service targeted by the program; we examine the unconditional sum to avoid bias from conditioning the sample on an endogenous variable (i.e., ACT service use). We first look at any of the ACT services, and then we combine services into diagnostics (sleep studies, endoscopies, and advanced imaging services) and surgeries. To examine the effect of the VBID on ACT service utilization, we create two measures: (i) any use and (ii) the number of episodes¹³. This classification allows us to explore VBID effects on the extensive and intensive margins of service use.

¹³We define a care episode as a set of services that have the same member identification number, start date, and billing healthcare professional identification number.

2.4.4 Empirical Model

We estimate the impact of our VBIID programs on cost-sharing and utilization with the following differences-in-differences (DD) regression model:

$$Y_{iet} = \alpha_0 + \alpha_1 DD_{et} + \delta_i + \tau_t + \epsilon_{iet} \quad (2.1)$$

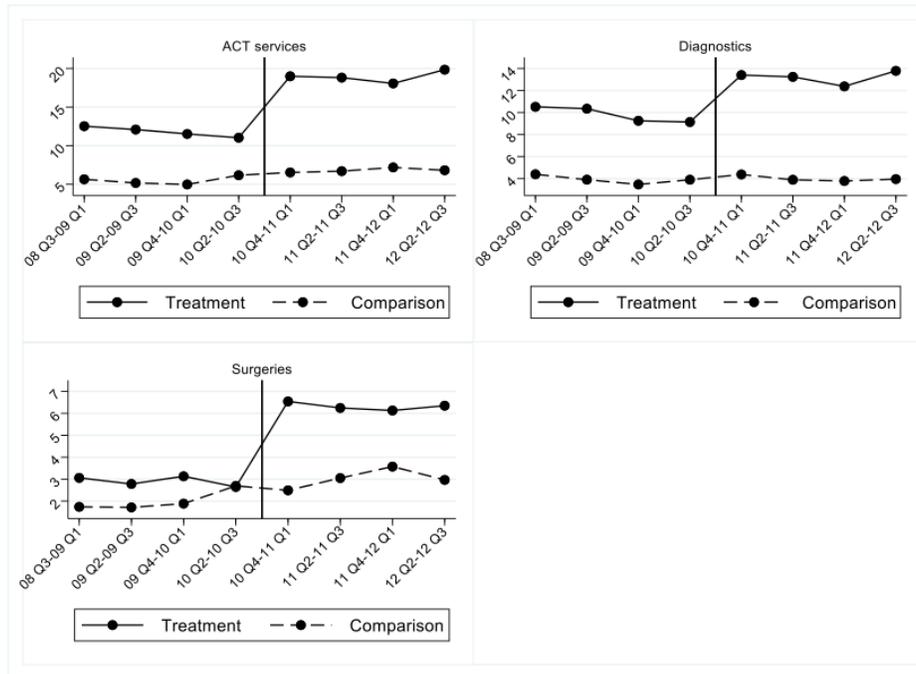
Y_{iet} is an outcome measure for beneficiary i employed by employer e in period (quarter-year) t . DD_{et} is the DD estimate and is the interaction between the treatment group (OEBC) and the post period (October 1st, 2010 and onward). δ_i is a vector of beneficiary fixed effects and τ_t is a vector of period (quarter-year) fixed effects. Lastly, ϵ_{iet} is the error term. We calculate heteroskedasticity robust standard errors in our main analyses. We note that the economics field has not yet reached consensus on how best to conduct inference with two clusters and, to date, suggests that the optimal approach is context-specific. Therefore, in extensions, we use alternative approaches to conducting inference: a permutation based approach and a wild-cluster bootstrap, and results are largely unchanged. We use OLS when the outcome is continuous and linear probability models (LPM) when the outcome variable is binary. We do not estimate non-linear models for binary outcomes given that we include beneficiary fixed effects in regression models, a context in which non-linear models (e.g., logits) perform poorly.

2.5 Results

2.5.1 Trends and Summary Statistics

Unadjusted trends for OOPs, any service use, and the number of service episodes are reported in Figures 2.2, 2.3, and 2.4. In terms of OOPs, the treatment and comparison group followed a similar pattern in the pre-VBIID period, with the two times series

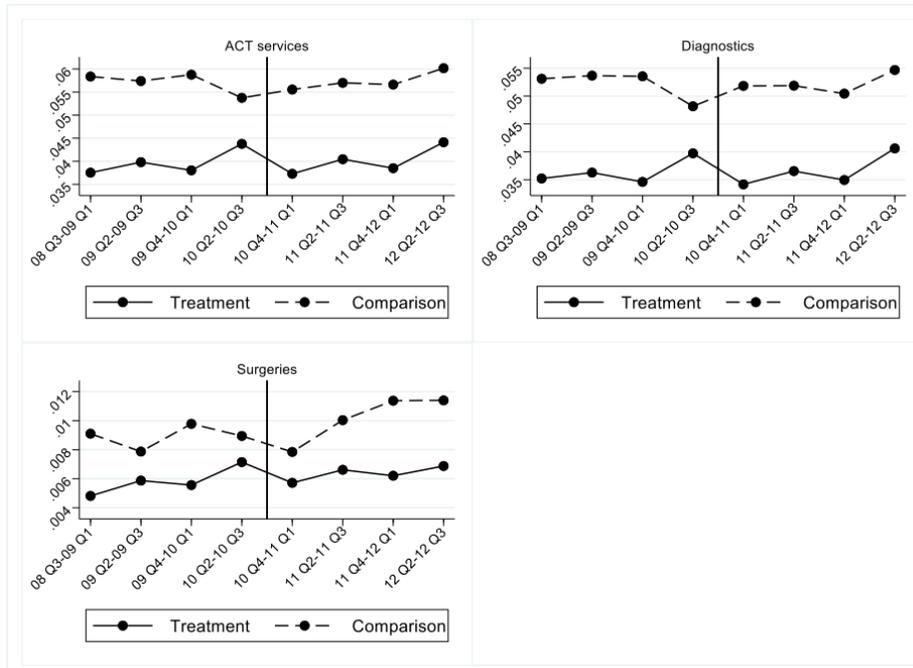
tracking closely, although the comparison group trend was above the treatment group trend in all periods (Figure 2.2). Beginning in Q4 2010, there was a sharp rise in OOPs at OEGB vs. the comparison group for all ACT services, diagnostics, and surgeries, which coincides with the introduction of the VBID. Similarly, the two groups followed comparable but not identical, trends pre-treatment in service use (Figures 2.3 and 2.4). In the post-treatment period, there was a modest decline in service use at OEGB vs. the comparison groups, suggesting that increased cost-sharing reduced service use.



Notes:

Data are aggregated to the treatment-quarter level. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.3 for mean values.

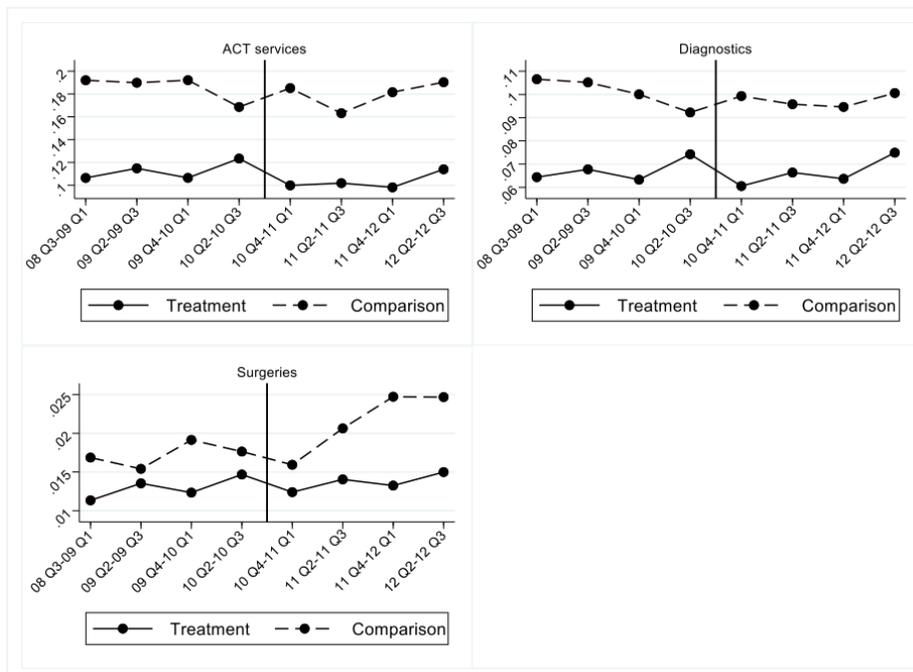
Figure 2.2: Unadjusted Trends in ACT Unconditional Out-of-Pocket Payments (\$)



Notes:

Data are aggregated to the treatment-quarter level. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.3 for mean values.

Figure 2.3: Unadjusted Trends in any ACT Service Use



Notes:

Data are aggregated to the treatment-quarter level. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.3 for mean values.

Figure 2.4: Unadjusted trends in the Number of ACT Episodes

Table 2.3: **Summary Statistics: Pre-VBID Period**

Sample:	OEBB	Comparison Counties†	Difference (p-value)‡
OOP Payments (Unconditional*)			
ACT Services	\$11.65	\$5.496	<0.0000
Diagnostics	\$9.661	\$3.914	<0.0000
Surgeries	\$2.885	\$2.011	0.0007
OOP Payments (conditional*)			
ACT Services	\$348.7	\$278.0	<0.0000
Diagnostics	\$318.1	\$243.0	<0.0000
Surgeries	\$622.2	\$456.1	<0.0000
Any Use			
ACT Services	0.0403	0.0571	<0.0000
Diagnostics	0.0368	0.0521	<0.0000
Surgeries	0.0060	0.0089	<0.0000
Episodes			
ACT Services	0.114	0.186	<0.0000
Diagnostics	0.068	0.101	<0.0000
Surgeries	0.013	0.017	<0.0000
Beneficiary Characteristics			
Age	44.3	44.77	<0.0000
Male	0.455	0.474	<0.0000
Female	0.545	0.526	<0.0000
Employee	0.513	0.557	<0.0000
Spouse	0.340	0.347	0.0069
Child	0.146	0.097	<0.0000
Observations(total)	254,288	48,299	–

Note: : The unit of observation is a beneficiary/quarter/year. Pre-VBID period is October 1st 2008 to September 31st 2010.

*OOP= summation of copayments and coinsurance.

†Comparison companies include three public employers in Oregon.

‡Difference between OEBB and comparison companies calculated with a t-test for continuous variables and a chi-squared test for binary variables.

Table 2.3 reports summary statistics in the pre-treatment period for OEGB and the comparison group. The average out-of-pocket (OOP) payments impacted by the VBID faced by patients at OEGB (comparison companies), i.e., the sum of copayments and coinsurance, were: \$12 (\$5) for any ACT service, \$10 (\$4) for diagnostics, and \$3 (\$2) for surgeries. The corresponding numbers among beneficiaries using the services were \$349 (\$278) for any ACT service, \$318 (\$243) for diagnostics, and \$622 (\$456) for surgeries.

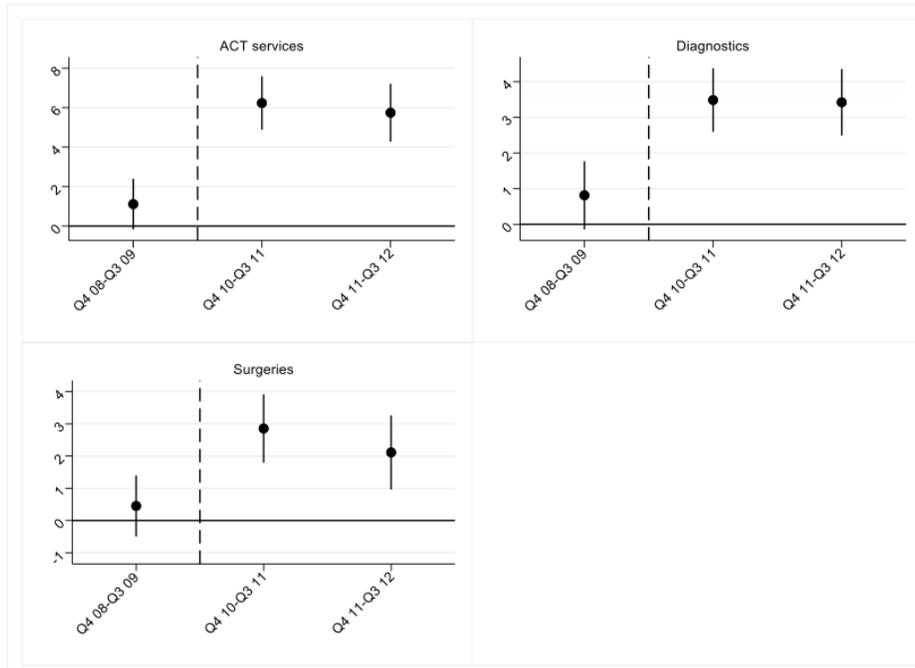
The services we study were not commonly utilized but were not exceedingly rare: 4.0% (5.7%) of the OEGB (comparison companies) sample reported using any of these services per quarter in the pre-treatment period. Not surprisingly, diagnostics were more frequently used than surgeries. In particular, quarterly rates of diagnostic service use were 3.7% at OEGB and 5.2% at comparison companies while the respective use rates for surgeries are 0.6% and 0.9%. The demographics of our sample are broadly comparable to an employed, prime working age population. On average OEGB beneficiaries were slightly older and more likely to be a dependent (spouse or child) than comparison company beneficiaries. We note that differences across groups are statistically significant, which we attribute to our very large sample size.

2.5.2 Validity of the Research Design

Our research design is DD. A critical assumption of this model is that the treatment and comparison groups would have trended similarly in terms of the outcome variables in the post-treatment period, had the treatment group not been treated. Of course, this assumption is untestable. To explore whether our data support this assumption, we conduct an event study following Autor (2003). More specifically, we construct a series of lead and lag indicators that are formed by interacting the treatment group (OEGB) and time periods. To smooth out seasonality in healthcare service use we interact the treatment indicator with four coverage-year indicators (Q4 2008-Q3 2009,

Q4 2009-Q3 2010, Q4 2010-Q3 2011, and Q4 2011-Q3 2012). Our omitted category is the coverage year prior to implementation of the VBID program (i.e., Q4 2009-Q3 2010). Examination of the coefficient estimate on the lead variable can allow us to study, after conditioning on covariates, whether the treatment and comparison groups followed similar trends prior to VBID implementation at OEGB.

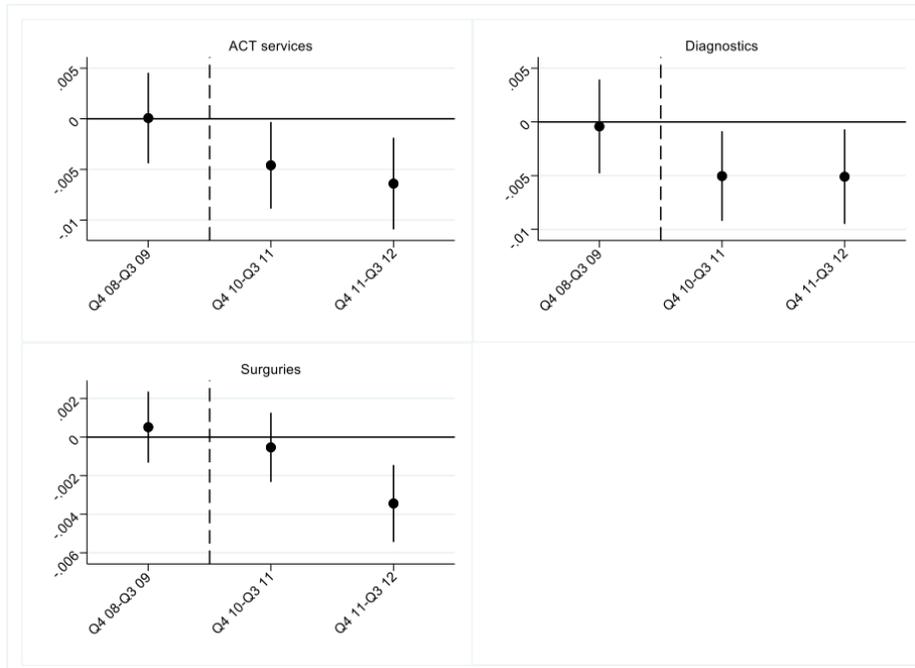
Figures 2.5 to 2.7 report event study results graphically. Our event study results do not reveal evidence that treatment and comparison groups followed different trends prior to the implementation of the VBID at OEGB. However, we observe that these groups followed divergent trends after the VBID implementation. While the researcher can never definitively prove that the data satisfy parallel trends, we view our event study analysis as providing suggestive evidence that our data are able to meet this critical assumption. We thus report DD estimates for the remainder of the manuscript to concisely summarize our findings.



Notes:

The unit of observation is a beneficiary/quarter/year. See Table 2.3 for mean values. All models estimated with OLS and control for beneficiary fixed effects and period fixed effects. 95% confidence intervals that account for heteroscedasticity are reported in square brackets. The study period is October 1st, 2008 to September 30th, 2012. The omitted category is the October 1st 2009 to September 30th 2010, the period prior to VBID adopting at OEBB.

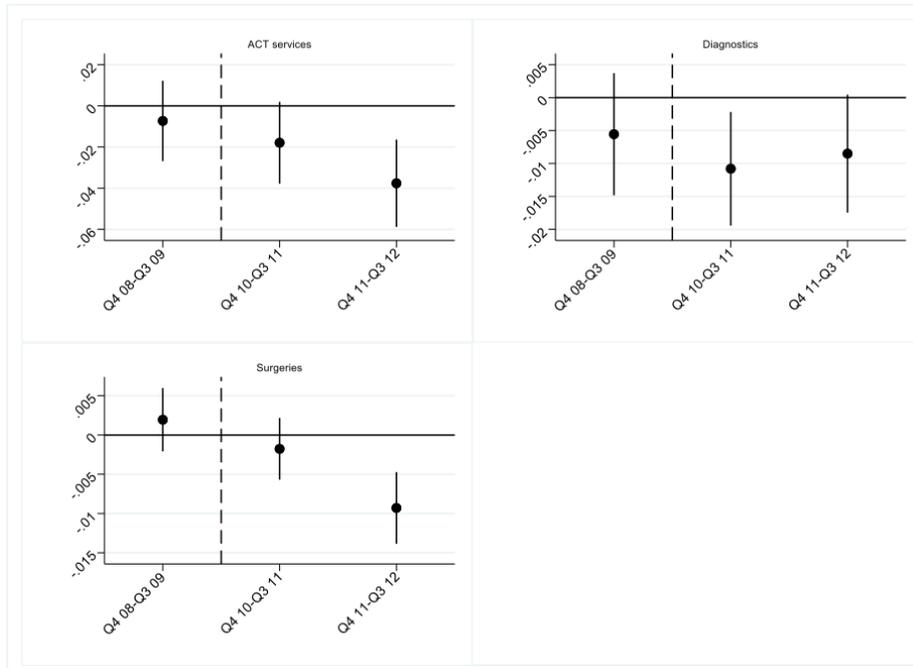
Figure 2.5: Effect of VBID on ACT Unconditional Out-Of-Pocket Payments using an Event-Study Model (\$)



Notes:

The unit of observation is a beneficiary/quarter/year. See Table 2.3 for proportions. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. 95% confidence intervals that account for heteroscedasticity are reported in square brackets. The study period is October 1st, 2008 to September 30th, 2012. The omitted category is the October 1st 2009 to September 30th 2010, the period prior to VBID adopting at OEBC.

Figure 2.6: **Effect of VBID on any ACT Service Use using an Event-Study Model**



Notes:

The unit of observation is a beneficiary/quarter/year. See Table 2.3 for mean values. All models estimated with OLS and control for beneficiary fixed effects and period fixed effects. 95% confidence intervals that account for heteroscedasticity are reported in square brackets. The study period is October 1st, 2008 to September 30th, 2012. The omitted category is the October 1st 2009 to September 30th 2010, the period prior to VBID adopting at OEGB.

Figure 2.7: **Effect of VBID on the Number of ACT Episodes using an Event-Study Model**

2.5.3 Effect of the OEGB VBID on ACT Service Cost-Sharing and Service Utilization

Table 2.5 reports selected regression results from our analysis of the VBID program on OOP payments, any utilization, and number of service episodes. For cost-sharing (panel A), our results are as expected in terms of sign and are precisely estimated: cost-sharing increases post-VBID at OEGB relative to comparison companies. More specifically, the cost-sharing increases in OOP payments are as follows (% increase given in parentheses and are calculated by comparing the estimated beta with the mean value in the pre-treatment period at OEGB, all tables report the same calculation): \$6 (47%) for any ACT services, \$3 (32%) for diagnostics, and \$2 (80%) for

surgeries. These findings imply that the program was effective in increasing cost-sharing for other services, offering empirical evidence of the first-stage.

Table 2.4 reports changes in conditional OOPs post-VBID. While these estimates may be vulnerable to conditional-on-positive bias as we document that the VBID reduced ACT service use, we report the conditional estimates as they provide evidence on effects among those beneficiaries who used the affected services. Following the VBID, conditional OOPs increased by \$192 (55%) for any ACT services, \$97 (30%) for diagnostics, and \$487 (78%) for surgeries.

Table 2.4: Effect of VBID on ACT Service Conditional Out-Of-Pocket Payments

Outcome:	Any ACT	Diagnostics	Surgeries
OEBB mean, Pre-treatment	\$348.7	\$318.1	\$622.2
DD	192.278*** (17.922)	96.943*** (13.628)	486.793*** (118.177)
Percent Change	55.14%	30.48%	78.24%
Observations	31,478	26,396	4,945

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with OLS and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases. *OOP= summation of copayments and coinsurance.

***,**=statistically different from zero at the 1%; 5% level.

Table 2.5: **Effect of VBID on ACT Out-Of-Pocket Payments and Service Use**

Outcome:	Any ACT	Diagnostics	Surgeries
Panel A: OOP Payments			
OEBB mean, Pre-treatment	\$11.65	\$9.661	\$2.885
DD	5.525*** (0.540)	3.100*** (0.363)	2.320*** (0.413)
Percent Change	47.42 %	32.09%	80.42%
Observations	742,670	742,670	742,670
Panel B: Any Utilization			
OEBB Proportion, Pre-treatment	0.0403	0.0368	0.0060
DD	-0.005*** (0.002)	-0.005*** (0.002)	-0.002*** (0.001)
Observations	742,670	742,670	742,670
Percent Change	-12.41%	-13.59%	-33.33%
ϵ^\dagger	-0.26	-0.42	-0.41
Panel C: Number of Service Episodes			
OEBB mean, Pre-treatment	0.114	0.068	0.0132
DD	-0.024*** (0.008)	-0.007*** (0.003)	-0.006*** (0.002)
Observations	742,670	742,670	742,670
Percent Change	-21.05%	-10.29%	-45.45%
ϵ^\dagger	-0.44	-0.32	-0.57

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with OLS when the outcome is continuous and an LPM with the outcome is binary, and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases. OOP = summation of copayments and coinsurance conditional on using the specific service.

$\dagger\epsilon = (\text{percent change quantity})/(\text{percent change price}) = (\beta_Q/\text{mean in service use in pre-treatment period at OEBB})/(\beta_{OOP}/\text{mean OOP payment in pre-treatment period at OEBB})$. β_{OOP} reported in Panel A.

***, ** = statistically different from zero at the 1%; 5% level.

Our estimates of the effect of the OEGB VBID program on use of ACT services are reported in panels B and C of Table 2.5. The results suggest that the VBID reduced utilization of each of the services. Along the extensive margin (any service use), we find that following implementation of the VBID, OEGB patients were 0.5 percentage points (12.4%) less likely to use any ACT service, 0.5 percentage points (13.6%) less likely to use diagnostic services, and 0.2 percentage points (33.3%) less likely to use surgeries relative to beneficiaries in the comparison group. The implied price elasticities across services are -0.26 (all ACT services), -0.42 (diagnostics), and -0.41 (surgeries). We note that the elasticities by type of service are each larger than the overall elasticity. We interpret this finding to imply that some beneficiaries use multiple services (i.e., diagnostics and surgeries), which is perhaps not surprising as the affected services may be complements. For example, a patient may receive a CT scan prior to a spinal surgery for pain, thus increased cost-sharing is applied to both services.

The pattern of results is comparable when we turn to our measures of service episodes that involve ACT services in the third panel of Table 2.5. Following implementation of the VBID program, the number of ACT services overall, diagnostics, and surgeries declined by 21.1%, 10.3%, and 45.5%, respectively, at OEGB relative to the comparison companies. The implied elasticities are -0.44 for any ACT services, -0.32 for diagnostics, and -0.57 for surgeries.

The similar magnitudes for the use of any services and the total number of services used implies that most of the response that we observe is on the extensive margin. Indeed, if we estimate a model of conditional utilization of ACT services, we obtain a zero coefficient for overall service use (see Table 2.6). This evidence is of course not dispositive because models of conditional utilization are subject to potential selection bias, but the finding is suggestive. Overall, results suggest that the VBID was effective in reducing use of low-value services.

Table 2.6: **Effect of VBID on Conditional ACT Service Episodes**

Outcome:	Any ACT	Diagnostics	Surgeries
OEBB mean, Pre-treatment	2.829	1.863	2.189
DD	-0.031 (0.214)	0.016 (0.069)	-0.073 (0.235)
Observations	31,478	26,396	4,945

Note: : The unit of observation is a beneficiary/quarter/year. Results reported in this table are based on the conditional sample of respondents, the conditional sample varies across outcomes. All models estimated with OLS and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases.

***,**=statistically different from zero at the 1%; 5% level.

2.5.4 Comparison of our Estimated VBID Elasticities with Previous Studies

We examine the effect of increased cost-sharing for a set of low-value services and find clear evidence that the quantity demanded declines when the price rises, with a demand-elasticity for any service use of -0.26. Given that patients potentially use multiple services as discussed in Section 2.5.3, we hypothesize that the actual elasticity is roughly double this number: -0.52. We can compare our implied elasticities of demand with various estimates for general healthcare services in the literature. One potential difference between our elasticities and others available literature (apart from our focus on low-value care) is that our estimates potentially incorporate standard price, signaling, and information effects on service value as outlined in Section 2.2.1.

An obvious point of reference is the RAND health insurance experiment (HIE) of the 1970s. Keeler and Rolph (1988) report an overall demand elasticity of -0.20. More recently, Chandra, Gruber, and McKnight (2014) report an overall elasticity of -0.16 in a population of low-income adults facing an increase in copayments using

variation generated by the 2006 Massachusetts healthcare reform. In another study, Chandra, Gruber, and McKnight (2010) examine the effect of increased cost-sharing on older public employees in California and document an implied elasticity of -0.10 for office visits. In a recent paper examining a high-deductible health plan (HDHP) at a private company that instituted a plausibly exogenous change in benefit design, Brot-Goldberg et al. (2017), find semi-arc elasticities between one quarter and one half of those documented in Keeler and Rolph (1988)¹⁴ for white collar workers.

While clearly not a comprehensive review of all available estimates, our implied price elasticity appears to be somewhat larger than findings for general healthcare services generated in previous experimental and quasi-experimental studies. One interpretation of these findings is that the low-value services that we study respond in a similar, although somewhat more elastic, manner to general healthcare services.

However, we note that examination of the tails of our 95% confidence intervals suggest elasticities that are perhaps more similar to findings from recent studies on general services. For example, if we compare the upper tail of the any ACT service use estimate confidence interval (-0.001 or a 2.6% decrease) with the 47% increase in OOPs, our implied price elasticity for the extensive margin of ACT service use is -0.06 and -0.12 after accounting for multiple service use.

2.5.5 Effect of the OEBC VBID on Healthcare Costs

We next estimate the effect of the VBID on healthcare costs. Similar to most studies that use health insurance claims data, we are limited to the list price which may depart from the actual price paid by insurers or self-paying patients (Cooper et al.

¹⁴Given the design of HDHPs and the non-uniformity of prices, the Brot-Goldberg et al. (2017) estimates are not directly comparable to the HIE estimate of -0.20. The authors use statistics provided in Keeler and Rolph (1988) to calculate a comparable estimate. Their constructed estimates are -0.57 and -1.32 and the comparable (Keeler and Rolph 1988) estimates are -2.11 and -2.26, with the first of each estimate unadjusted and the second adjusted for both inflation and age. Thus, the estimates of Brot-Goldberg et al are somewhat smaller in magnitude than the estimates documented by Keeler and Rolph.

2018). We examine total, ACT, and non-ACT expenditures (Table 2.7). We take the logarithm due to skewness (results, available on request, are similar if we do not log expenditures) and coefficient estimates have the interpretation of an approximation to the percent change. We observe no change in total and non-ACT service expenditures post-VBID, but ACT service expenditures declined by 3.9%. To assess whether there are downstream changes in services use, e.g. through delays in care and worsening health, we also lag our DD indicator by one year and re-estimate Equation 2.1. Results are not appreciably different.

Table 2.7: **Effect of VBID on Healthcare Expenditures**

Outcome:	Total	ACT	Non-ACT
OEBB mean, Pre-treatment	\$1,060.27	\$95.72	\$964.54
DD (contemporaneous)	-0.003 (0.021)	-0.039*** (0.012)	-0.000 (0.021)
DD (Lagged One Year)	-0.022 (0.024)	-0.032** (0.013)	-0.016 (0.024)
Observations	742,670	742,670	742,670

Note: : The unit of observation is a beneficiary/quarter/year. Outcomes are the logarithm of expenditures. One dollar is added to each variable to account for zeroes. All models estimated with OLS and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases.

***,**=statistically different from zero at the 1%; 5% level.

We suspect that the ratio of ACT services to total services is too small for changes in the former type of service to lead to overall cost-savings, e.g., only 4.0% of the OEBB sample used an ACT service in a given quarter (Table 2.3). Nonetheless, it is re-assuring that total and non-ACT services expenditures do not appear to have increased and that there was no off-setting increase in non-ACT services, which might suggest substitution across services or that by increasing ACT service cost-sharing,

patients delayed (or avoided) ACT services, leading to worse health and higher costs downstream. Further, the finding that there is no observable effect on non-ACT services provides support for our hypothesis that we are mainly capturing the effects of the VBID and not other co-occurring changes. More specifically, all services (ACT and non-ACT services) were subject to other changes implemented at OEBC (Section 2.4.2).

2.5.6 Effects on Services not Targeted by the VBID

A concern with our analysis is that we may simply be capturing broader trends in low-value service use rather than the specific effect of the VBID on ACT-targeted services. We next explore this hypothesis empirically. To do so, we define an indicator for low-value services based on Schwartz et al. (2014). We modify Schwartz's low-value service set, which is constructed using the Medicare population, to focus on services likely to be used by non-elderly adults with private coverage. In particular, we construct an indicator for any use of the following services: homocysteine testing for cardiovascular disease, hypercoagulability testing for patients with deep vein thrombosis, parathyroid hormone measurement for patients with stage one to three chronic kidney disease, and bone mineral density testing. This classification was determined by one of the authors who is a practicing physician in a large U.S. health-care system. Of note, none of these services were targeted by the VBID program. We document no evidence that use of these low-value services changed at OEBC vs. comparison companies following VBID implementation (Table 2.8).

We also examine alternative falsification outcomes that are not low-value services: emergency appendectomy and tonsillitis. We chose these services because they reflect care required following medical emergencies; that is a medical situation in which there is little to no discretion on the part of the patient or healthcare professional as to whether or not the patient should receive treatment. Trends in the use of the services

are likely to be driven by very different factors than those that drive low-value service use. We find no evidence that the VBID influenced use of these services (Table 2.8).

Table 2.8: **Effect of VBID on and ACT Service Use: Falsification Testing Using Alternative Low-Value Care Services**

Outcome:	Swartz et al †	Emergency Appendectomy	Tonsillitis
OEBB Proportion, Pre-treatment	0.0170	0.0004	0.0016
Swartz et al	0.001 (0.001)	0.000 (0.000)	0.000 (0.000)
Observations	742,670	742,670	742,670

Note: The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases. See text for a description of the Swartz et al low-value care variable.

†Services identified to be of low-value but not targeted by the OEBB VBID. See text for details.

***,**=statistically different from zero at the 1%; 5% level.

2.6 Robustness Checks

We next describe several robustness checks we conduct to assess the sensitivity of our findings to alternative modeling approaches. Overall, our results are quite robust.

We also conduct placebo testing (Appendix A Table A1). We sequentially (falsely) assign each of the comparison companies as the treated employer, assign OEBB to the comparison group, and re-estimate Equation 2.1, i.e., a placebo-in-place analysis. This falsification test also offers an alternative approach to inference in the spirit of Abadie and Gardeazabal (2003). More specifically, we can take our estimates of treatment effects (main coefficient and three placebo-in-place estimates) and assess whether our main estimate is an outlier. For any ACT service use, the main point estimate and the placebo-in-place estimates are reported graphically in Appendix A

Figure 4. Our main point estimate is clearly an outlier, which supports our main findings.

Second, in Appendix a Table A2, we report results from a model in which we decompose the post-period into two sub-periods: ‘early post’ (2010 Q4 to 2011 Q3) and ‘late post’ (2011 Q4 to 2012 Q3). VBID effects for any ACT and diagnostic service use appear to be stable across time, indeed we cannot reject the null hypothesis of coefficient equality (F-statistics <1). However, the VBID effect on surgery use may escalate over time; i.e., the late post coefficient estimate is larger in magnitude and statistically different from the early post coefficient estimate.

Third, we estimate Equation 2.1 on different samples and using different specifications (Appendix A Table A3; Appendix A Tables A4 and A5 report comparisons of demographics for various sub-samples). In particular, we use a balanced sample observed in all 16 quarters; exclude beneficiaries who switch plans during our sample period; control for plan fixed effects; exclude beneficiary fixed effects; include employer-specific linear time trends; use alternative definitions of ACT services (i.e., services always listed as an ACT service and removing the cancer exclusion from the definition); and apply a wild-bootstrap approach to inference (Cameron and Miller 2015). Results are robust, although we note that in some samples and specifications we lose precision for some point estimates.

Third, a concern with our analysis is that, concurrent with the increases in cost-sharing for low-value services that we study, OEGB also rolled out other changes that may have affected service use (Section 2.3.1). Thus far in the manuscript, we have addressed this issue by examining a sub-set of OEGB plans that experienced the smallest increase in (non-VBID) cost-sharing. To dig deeper into this issue, we next estimate Equation 2.1 on different OEGB samples, leaving the comparison group unchanged (Appendix A Table 2.5). We report results based on the main sample to facilitate comparison of regression coefficients, and (i) all OEGB plans and (ii) OEGB

plans that experienced high (non-VBID) cost-sharing increases. The point estimates across these samples are very similar which suggests that we are mainly capturing VBID effects. We note that coefficient estimates generated in the sample in which the treatment group includes OEGB plans that experienced the largest (non-VBID) cost-sharing are larger than those generated in other samples employed in this exercise. However, 95% confidence intervals overlap surrounding the point estimates overlap, preventing us from drawing strong conclusions.

Fourth, we estimate an alternative approach to testing the ability of our data to satisfy the parallel trends assumption. Specifically, we use pre-VBID data only and estimate a variant of Equation 2.1 in which we interact the treatment group indicator with a linear time (quarter-year) trend (Akosa Antwi, Moriya, and Simon 2013); the equations are otherwise identical. If the coefficient on the interaction term is small in magnitude and imprecise, this finding would provide suggestive evidence that our data can satisfy parallel trends. Results are listed in Appendix A Table A7 and generally support this hypothesis. We note that the coefficient in the any ACT service regression is statistically different from zero. However, the coefficient estimate carries a positive sign, suggesting that use of these services was increasing at OEGB relative to comparison companies in the pre-treatment period. If anything, such a trend would work against our ability to detect effects as we expect VBID to reduce (or leave unchanged) ACT service use.

Fifth, also using pre-VBID data only, we falsely assign the program effective date as October 1st, 2009 (rather than October 1st, 2010, the actual effective date). We observe no evidence that our false indicator predicts ACT service use (Appendix A Table A8).

Sixth, we sequentially exclude each of the comparison companies from the sample to ensure that our findings are robust to alternative comparison groups. Results are reported in Appendix A Table A9 and are stable across comparison groups.

Finally, we include data from the 2013 coverage year (Q4 2012 to Q3 2013)¹⁵. As noted in Section 2.3.1, beginning in Q4 2012 Moda Health re-organized its plan offerings, eliminating some plans and creating new plans. We exclude this coverage year from our main analysis to avoid confounding. However, we incorporate this data and report results of VBID effects in Appendix A Table A10. We use two samples: (i) low (non-VBID) cost-sharing plans that we use in our main analysis and (ii) all beneficiaries. Results are very similar which suggests that effects were observable three years after policy adoption.

2.7 Discussion

In this study, we explore the effect of a value-based insurance design (VBID) that substantially increased patient cost-sharing for sleep studies, endoscopies, advanced imaging services, and potentially over-used surgeries. Our work is motivated by theories of behavioral hazard and decision errors on the part of consumers regarding healthcare service value and implications for optimal insurance design (Baicker, Mulainathan, and Schwartzstein 2015), and empirical work on strategies designed to require patients to internalize costs associated with overuse by aligning cost-sharing with service value (Chernew, Rosen, and Fendrick 2007).

To the best of our knowledge, we are the first to study the effect of a VBID design using increased cost-sharing for a broad set of low-value services using a quasi-experimental design. Thus, we offer new insight to the large and growing literature on VBID programs. This information is timely. VBID principles are embedded within the ACA and are being incorporated into major public insurance systems (e.g., Medicare Advantage, Tricare, and state employee benefit plans); most large employers

¹⁵This is the final year of data provided through our data use agreements. Further, we do not wish to incorporate data from the ACA-period given that this law, with key provisions going into effect January 1st 2014, reflects arguably the largest transformation of the U.S. healthcare system in a generation.

currently apply VBID principles in benefit plans or plan to include these principles in the near future (Gibson et al. 2015); and healthcare professionals themselves are attempting to reduce the provision of low-value care (e.g., the ‘Choosing Wisely’ campaign). Even with this policy (public and private) enthusiasm, there is concern that VBID designs may have unintended consequences by reducing both low-value and high-value care, leading to delays in service use, declining health, and increased costs downstream. Moreover, we contribute to the economic literature that examines the impact of cost-sharing and optimal insurance design more broadly.

We exploit a novel VBID implemented by a large public employer in the state of Oregon. We apply a quasi-experimental differences-in-differences design to study program impacts, thus our findings have a causal interpretation. This is an important contribution as a key concern with the VBID literature to date is that many studies lack a suitable comparison group and therefore it is difficult to disentangle VBID effects from contemporaneous trends in healthcare use (Look 2015). Thus, our analysis represents an important step forward in understanding the potential of VBIDs to address the high and rising healthcare costs in the U.S.

Our findings suggest that the low-value services we study (sleep studies, advanced imaging services, endoscopies, and surgeries) decline when their prices increase, with an implied price elasticity of demand for overall services of -0.26. Given the possibility of multiple ACT service use due to complementarity across targeted services (e.g., a CT scan occurring prior to a spinal surgery), we hypothesize that a reasonable estimate is roughly double this elasticity: -0.52. Taking this estimate at face value, our findings suggest that demand for the low services that we study is somewhat more elastic than estimates from the broader literature on general healthcare service use. This difference is reasonable given that we study focus services that are likely of low value and unnecessary for most patients. Further, in addition to standard price effects, the act of labelling a particular service as low value and providing information on the

likely marginal benefit associated with the service (as occurred in the VBID) may send a signal to beneficiaries and/or provided information that corrects the patients decision error and reduces behavioral hazard. Finally, while our elasticity is larger than estimates from studies of general services, it is less than one, which suggests that demand for the low-value services we study is inelastic.

This finding by itself is perhaps not surprising or noteworthy – we provide evidence that the demand curve for these services slopes downward. However, arguably more importantly we show, by comparing our implied elasticities of demand with related studies within the literature, that consumers do not appear to have wildly more elastic demand for such services than they display for a wider set of general healthcare services; indeed, as noted above, all of our implied elasticity estimates are less than one, suggesting that demand is inelastic. This finding is potentially useful both academically and practically as there are often concerns raised by payers that covering discretionary healthcare services (which low-value services likely are) will substantially increase costs as demand for these services is highly elastic. Finally, we do not observe any evidence that overall costs increased or that there was substitution across services following increased cost-sharing for the low-value services we study. The finding that overall costs do not increase, in addition to being interesting in its own right, provides evidence that the services we study are low-value: reducing their use did not affect health (as proxied by costs).

Our study, although novel in many ways, is not without limitations. First, we rely on just one program and four public companies in the state of Oregon. Thus, the generalizability of our findings to other settings is not clear. Second, although the OEBC administrators went to great lengths to select low-value services and allow healthcare professionals substantial latitude to avoid increased cost-sharing for select patients for whom the targeted services were likely to be valuable, some the services targeted by the VBID program may be of high-value for some patients (e.g., upper

gastrointestinal endoscopies can be life-saving when preformed for gastrointestinal bleeding). Thus, we note that the selection by OEBC administrators of services for increases in cost-sharing may not be sufficiently nuanced. The lack of a standardized and validated set of services that are lower and higher value is an important barrier for both the research field, and healthcare professionals and payers. We encourage future studies to rigorously test healthcare service value to facilitate better research and policy.

The question of how to address high healthcare costs, without undermining patient outcomes, is critical for maintaining the financial stability of governments at the Federal, state, and local level; insurers; and patients and the health of the U.S. population. We provide new insight on this question. Future research could explore the effect of VBID programs on utilization of a broader set of healthcare services and in different patient populations.

CHAPTER 3

THE EFFECT OF MANDATORY MANAGED CARE ON PREVENTABLE HOSPITALIZATIONS FOR THE AGED, BLIND, AND DISABLED OF MEDICAID IN NEW JERSEY

3.1 Introduction

Medicaid, the joint state and federal program, has provided healthcare to the nation's poorest and most vulnerable populations since its inception in 1965. Over the last several decades, the program has grown to cover almost one in every five Americans at a cost of over \$500 billion dollars (Centers for Medicare and Medicaid Services 2019; Henry J Kaiser Family Foundation 2019a). As such, Medicaid has become one

of the largest line-items on a given state's budget. The heavy burden of ensuring access to quality care for the nation's poor and disabled while finding ways to pay for it has pushed policy makers to seek innovative cost containment mechanisms that also ensured access and quality in Medicaid care.

In the 1980s and 1990s, managed care (MC) seemed to be a beacon of light in the costly Medicaid problem. As such, states began transitioning their enrollees into MC plans. Touted as being able to save money and provide superior care compared to the aging fee-for-service model, the original financing method of Medicaid in which providers are reimbursed directly for services rendered, by 2010 over 70% of the nation's Medicaid population was enrolled into some form of managed care organization (MCO) plan (Iglehart 2011; Sparer 2012). Managed care is an effective way to smooth budget spending, if not reduce it, by eliminating large fluctuations in unexpected spending, using capitations to transfer that cost risk to the MCOs providing the care alone is a valuable proposition for policy makers (Iglehart 2011).

Managed care, in its most basic form, works by accepting the financial risk of its enrollees in exchange for a fixed payment, a capitation, from Medicaid. Depending on the structure of this risk transfer, there are incentives for the MCO to avoid the most costly of patients, or to simply restrict access to costly services and care. This is most pronounced in plans where there is significant turnover and the likelihood of receiving a capitation payment the proceeding month is not certain (Burns 2009). However, in the presence of perfect risk adjustment and capitation security, the MCO has the incentive to invest in preventive care since the MCO assumes the financial risk for costly interventions in the future (Burns 2009; Bindman et al. 2005).

Despite claims of success, yet having inconsistent research to support such a claim, Medicaid administrators renewed their interest in Medicaid managed care (MMC) to tackle their most costly problem: the Aged, Blind, and Disabled (ABD) population. The ABDs make up the core of the original 1965 Medicaid program, and, though

relatively few in number, the ABDs represent the most significant cost of a state's Medicaid budget (Iglehart 2011). Many state Medicaid programs are reviving the drive to full MMC participation, including the ABDs, to reduce costs (Iglehart 2011; Kellermann and Weinick 2012).

Advocates for those with complex medical needs and disabilities have expressed concern with MMC for the ABD population. Though MMC has been well studied in non-disabled populations, any successes that may be attributed to MMC in those situations should not be generalized to those with complex needs and disabilities, such as those with chronic conditions and intellectual disabilities (Burns 2009). Even when compared to the other beneficiaries in Medicaid, the ABD population is far more vulnerable and relies on medical complex networks, often across multiple specialties (Kellermann and Weinick 2012).

There are several unique impediments to assessing the impact of MMC, not just on the ABDs but in general, given the wide degree of flexibility the federal government gives states in administering the program (Sparer 2012). Research on the topic often relies on survey and interview data with loose ties to actual administrative data using generalizations on Medicaid and MC status, e.g. Davidoff et al. (2007), Long, Coughlin, and King (2005), and Coughlin, Long, and Graves (2008), etc. More importantly, most research is silent about information on key details on the programs they study; features such as “carve-outs,” entire categories of service are excluded from MC contracts, to the way that MC enrollees can be recruited, such as direct marketing (Sparer 2012). These important features are likely to have impacts on measured outcomes, especially in the presence of selection and sorting.

In this study, I examine New Jersey Medicaid, using detailed program documents, including individual MC contracts and state program guidances. I leverage this to generate causal estimates of the effect of implementing a mandatory MMC program for the physically and developmentally disabled in the presence of FFS and voluntary

MMC on hospitalizations for ambulatory care sensitive conditions. Ambulatory care sensitive conditions are conditions that respond well to treatment in an ambulatory care setting. These ACSC hospitalizations are thus regarded as preventable (Kellermann and Weinick 2012). I exploit the variation induced by the as-good-as-randomly assigned mandatory transition at the county level over the course of 5 transition cohorts.

Using a differences-in-differences framework, I use the yet-to-be treated cohorts as comparison groups using monthly counts of hospitalizations for ACSC hospitalizations aggregated from the New Jersey Medicaid claims data warehouse. I contribute to the literature by providing causal estimates of the effect of managed care for the ABD population. There currently exists little evidence on this particular population and none, to my knowledge, that utilizes administrative claims data for the entire population or that does not suffer from selection bias in a voluntary MMC program. I find that the introduction of mandatory MMC for the ABD population reduced preventable hospitalizations by 6.4%[-11.5,-1.30], per county, per month.

The literature on the topic of Medicaid managed care is quite mature. However, much is still unknown given the wide variation in findings, methodological misgivings, and overall lack of clarity on key program details and differences that may be driving findings. My contribution is producing casual estimates of the effect of mandatory MMC on ACSC hospitalizations, a key measure of access to care, for the physically and developmentally disabled in Medicaid, and the first to do so with New Jersey's program.

3.2 Literature Review

3.2.1 Medicaid Managed Care

Medicaid, the 1965 joint state and federal healthcare program, was designed with a key feature of physician choice. This feature was ingrained in the fee-for-service (FFS) model it employed, like most healthcare payers of the time. This model allows an individual with Medicaid coverage to see any physician willing to accept Medicaid payments. Physicians bill the state Medicaid program for each of the services rendered to their patients. In return, Medicaid remits payment based on a published schedule of rates for each service.

The rates that Medicaid pays, however, have always been substantially lower than for private payers, which is well documented in the literature. These traditionally low reimbursement rates have resulted in limited provider participation, often skewed towards safety net providers (Hurley 1998; Tenzer 2006; Sloan, Mitchell, and Cromwell 1978). Limited provider networks create barriers to accessing ambulatory and preventative care. These barriers result in over-reliance on emergency departments as a usual source of care, as well as providers, serving Medicaid exclusively, including safety net providers such as clinics (Holahan et al. 1998) Interestingly, Medicaid often provides less physician choice than commercial insurance designed with limited networks.

In contrast to FFS, managed care utilizes a limited network of providers and points-of-service (POS) that have agreed to accept payments for services at a discount in order to access the patients of the managed care organization (MCO.) States employ many types of managed care contracts. The most common are:

Fully Capitated Contract The most common type of contract is fully capitated, also known as a full-risk contract. In this agreement, the MCO is at full financial risk for all healthcare of the beneficiary in return for a single, pre-set payment (capitation.)

The MCO pays for all healthcare, even if the care exceeds the capitation (Levinson 2014).

Partial-Risk Contract A partial-risk contract operates like the full-risk with the exception that the contract covers only predetermined set of services. The MCO agrees to provide these services in exchange for the pre-set capitation payment. The services not covered, or “carved out, by the contract are provided by either Medicaid FFS or another MCO agreement (Levinson 2014).

Primary Care Case-Management In primary care case-management (PCCM) contracts, primary care providers receive a nominal fee to oversee and coordinate care for beneficiaries assigned to them. These contracts sometimes include the provision of all ambulatory non-specialty care via a capitation payment (Levinson 2014).

Medicaid managed care (MMC) was seen as an attractive alternative model to FFS given its success in the private and commercial markets. There, MCOs generated substantial savings by negotiating large discounts, given that most payers at the time paid full price for healthcare services (Bindman et al. 2005). However, compared to privately insured beneficiaries, those in Medicaid are far more vulnerable, have limited resources, and have less access to non-medical service complements to medical care such as childcare, transportation, ability or flexibility to take time off work, translational services, etc. Additionally, their experience and capacity to make complex healthcare decisions may be limited (Basu, Friedman, and Burstin 2004). This facet, as well as the fact that Medicaid's reimbursement rates were already substantially lower than the private sector, were largely overlooked by most Medicaid programs when assessing the viability of MMC (Bindman et al. 2005).

The benefits of MMC may have been oversold, especially in well-developed and mature Medicaid programs since MCOs would not likely be able to lower provider reimbursement rates any lower (Sparer 2012). When coupled with the fact that Medicaid was a key funding source for safety net providers, such as clinics and state-run

hospitals, aggressive negotiating for lower rates could jeopardize the financial viability of these societally important providers. Many states employ strategies used by MCOs in their FFS Medicaid programs. These include prior authorizations, utilization review, and case and disease management, meaning that MMC might not offer meaningful improvements (Sparer 2012). Patient advocates and others worry that resource limitations in MMC can undermine access to care and thus increase hospitalizations, especially when there is substantial uncertainty and variations in beneficiary eligibility continuity (Bindman et al. 2005). Given the extremely low rates of provider participation in FFS Medicaid, limited networks in MMC may represent an increase in provider access. Proponents of MMC argue that, since the MCO is at financial risk for adverse events, there is a strong incentive to prevent costly care in the future through expanded preventative care (Bindman et al. 2005).

Switching from a FFS based model to MMC, states pivoted from their role as payer of bills to the roles of management and oversight, particularly to combat waste, fraud, and abuse. Additionally, they must actively measure and manage contractual compliance and quality (Fossett et al. 2000). Fossett et al. (2000) identified the need for states to become prudent purchasers of healthcare. To achieve this, states must contractually define plan performance requirements. This ensures that benchmarks for plan quality, access to care, and health outcomes are set and known. Additionally, states must ensure that they have access to quality data in order to assess compliance. Lastly, in the event that plans fail to comply with contractual standards, there must be enforceable consequences to bring the plan back into compliance. Studies have found that many states lack access to data of sufficient quality to assess plan compliance and performance, fail to set benchmark standards, and do not consistently collect and analyze such data when available. Even more interesting is that states, when confronted with noncompliance, often choose not to enforce contractual remedies and, instead, bow to market and political forces (Fossett et al. 2000).

3.2.2 The History of MMC

Integrating managed care into Medicaid is an idea almost as old as the program itself. Beginning in the mid 1970s, Medicaid officials saw managed care as a promising alternative to the traditional FFS delivery model. Experiments, however, such as a prominent one in California, did not go well (Sparer 2012). This failure, riddled with fraud, mismanagement, and controversy, ended in strict new federal regulations on MCOs in Medicaid in 1976 (Sparer 2012). The 1980s ushered in a new era of liberal market policies and gave states more autonomy and flexibility in the management of their Medicaid programs (Sparer 2012). This led to large-scale MMC demonstrations in an effort to expand beneficiary access to medical homes (Hurley 1998). The 1990s were fraught with substantial growth in Medicaid costs. These costs were the combined result of large expansions in eligibility and increases in service intensity, all coupled with high inflation of healthcare prices (Hurley 1998).

Facing tight budget constraints and high program costs, MMC emerged as a way to control the growth of costs and smoothing fluctuations in costs by transferring financial risk to MCOs (Kellermann and Weinick 2012; Levinson 2014; Iglehart 2011). This led to a period of high growth for MMC programs, which was partially driven by the success of managed care in the commercial market (Hurley 1998).

Managed care in Medicaid has since grown substantially. By 2012, over 60% of Medicaid beneficiaries were covered under comprehensive (i.e. full-risk) contracts or with few carve-outs. Only 6% of all beneficiaries received all benefits through FFS. This means that about 94% of all Medicaid recipients received some services via managed care (Burns and Layton 2018). When examining MMC participation by eligibility group, the Congressional Budget Office found that 70% of recipients in eligibility groups with low-costs were enrolled in comprehensive managed care plans. Meanwhile, high-cost eligibility groups saw only 39% of recipients in comprehensive MMC. Many states have been pushing towards total reliance on managed care, with

two states currently providing 100% of benefits through their MMC program (Iglehart 2011).

Participation in managed care by Medicaid programs is completely voluntary. States are not required to offer MMC plans to their beneficiaries, with a small number not offering any managed care in their programs. Further, those states that chose to implement a MMC program are not required to follow any prescribed blueprint or plan. This has created a mosaic of MMC programs across the states with substantial inter-program variation (Sparer 2012; Iglehart 2011). The resulting variation across programs has impeded the efficient evaluation of MMC performance. The transition towards MMC was accompanied by high hopes for enhanced access to care, quality improvements, and reductions in program costs. However, the evidence on the success of this approach is mixed (Holahan et al. 1998).

3.2.3 The ABDs: Aged, Blind, and Disabled

Despite the rapid growth of MMC as a whole, some eligibility groups have been left out due to the complexity of their healthcare needs (Hall et al. 2015). The aged, blind, and disabled (ABD) population is a core eligibility group, part of the original Medicaid mandate. The ABDs are one of the smallest eligibility groups in Medicaid, but they represent the largest share of spending within the program, which has made them a prime target for new MMC growth (Iglehart 2011).

People with disabilities are extremely sensitive to small changes in their healthcare, especially in terms of access to care and quality of care (Hall et al. 2015). The needs of the ABDs differ substantially from the other Medicaid beneficiaries and even more so from the commercially insured (Hall et al. 2015). Despite their experience with other eligibility groups in Medicaid, MCOs have relatively little experience with beneficiaries who have complex healthcare needs (Burns 2009). Prior to the introduction of MMC for the disabled, beneficiaries received their care in a fragmented and

uncoordinated FFS setting (Iglehart 2011). States see MMC as a way to control the costs of the ABD population and potentially increase access to care and quality of care for the most vulnerable beneficiaries in their programs.

The growth of MMC was accompanied by a willingness by states to expand eligibility in managed care to include complex recipients, such as the disabled. The expansion of MMC for the disabled was met by swift opposition. Patient advocates, providers, and even some managed care plans raised concerns about the ability to meet the complex needs of this vulnerable population. MCOs had very little experience with beneficiaries with significant and complex needs and some contended that they were ill-equipped to serve them, particularly questioning whether their networks were could deliver such highly specialized and coordinated care (Huffman et al. 2010; Hurley 1998; Hurley and Somers 2003).

Managed care in Medicaid has been well studied in non-disabled and non-elderly populations; those findings should not be generalized to the ABDs (Burns 2009). By design, managed care plans use limited provider networks. This presents added challenges to recipients with complex healthcare needs and disabilities. These beneficiaries have well-established, complex networks of care and, in transitioning to MMC, they are not likely to find a plan in which all existing points of care are in the covered network. Thus, enrolling beneficiaries with elevated service needs into closed networks could disrupt established provider relationships and lead to discontinuities in care, potentially resulting in avoidable complications (Sparer 2012; Davidoff et al. 2007).

Policy makers recognize that the push to expand managed care to include those likely to experience the most profound difficulties are the disabled and those that rely on care provided by several physicians across multiple specialties (Mitchell and Gaskin 2004). Advocates worry that the MCOs may face perverse incentives in relation to high-cost beneficiaries. Fixed capitation payments, coupled with imperfect risk

adjustment, creates the incentive to avoid the most costly recipients or to merely restrict access to necessary but expensive care (Burns 2009; Davidoff et al. 2007). Proponents, however, argue that those same incentives may encourage the MCO to invest in preventative care and increased access to reduce the likelihood of an adverse event for which they are at financial risk (Burns 2009).

3.2.4 The Effects of Medicaid Managed Care

There is substantial difficulty in studying the effect of managed care in Medicaid given the fact that participation at the state-level is voluntary and that states have significant freedom in how they implement such programs. The resulting patchwork of state MMC programs is highly variable (Sparer 2012; Iglehart 2011). This heterogeneity makes it difficult to study MMC and estimate generalizable causal treatment effects. Additionally, MMC is often unevenly applied across eligibility groups, can be voluntary in participation, and have varying levels of benefits.

The growth in MMC has been high and well-documented. Despite this, evidence on the effects of MMC is lacking. The evidence that does exist is largely inconclusive or driven by favorable selection in voluntary programs (Hurley 1998; Bindman et al. 2005). Additionally, since many programs offer different delivery models across counties within a state, even state-level estimation faces difficulty in isolating the causal effect of MMC (Bindman et al. 2005). The empirical evidence on the effect on health is limited. Many studies utilize short time frames and thus may not be able to capture the long-term gains (Lo Sasso and Freund 2000).

One of the biggest issues in the literature is that authors frequently fail to address or even recognize key characteristics on which states and health plans vary. Such issues as how capitation rates are set (by competitive bidding, geographically varying, annual or biannually, actuarially, etc.) can impact conclusions reached. Differences in how states and MCOs enroll or recruit beneficiaries, such as auto-assignment or direct

marketing, influence study outcomes, especially if a plan is able “skim” less costly and healthier recipients (Sparer 2012; Bindman et al. 2005). Lastly, one of the most important features of a managed care plan, a service carve-out, is often overlooked. A carve-out is a service or set of services not covered by a MMC contract and either provided by Medicaid FFS or contracted to another MCO. Service carve-outs are extremely prevalent, with all states except Tennessee employing them in some way (Sparer 2012). Carve-outs are typically uneven across enrollment categories and can be as large as all pharmacy or specialty benefits. Thus, overlooking or failing to acknowledge carve-outs can lead to erroneous conclusions.

A significant cause for concern in the switch to managed care from FFS is the availability of physicians. Plan documentation on participating providers is a key resource that allows for individuals to choose a plan that best fit their unique health-care needs. Inaccurate information in these documents can pose substantial barriers to accessing essential healthcare in a timely manner. In a report from the Office of the Inspector General of the DHHS of 221 full-risk MMC plans across 32 states, 1,800 randomly selected providers listed as participating in plan books were contacted to request an appointment, with the earliest appointment date recorded. The results were alarming. Of those contacted, more than half (51%) of providers could not offer appointments. Of the 49% that were able to offer appointments, 28% had a wait of one or more months. Lastly, only 44% of primary care physicians offered appointments. This study finds that the networks in MMC plans are substantially more limited than their own documentation conveys. This is extremely important given that access to care, especially for the disabled and elderly, is essential to generating positive health outcomes (Levinson 2014).

Evidence on Non-Disabled Populations

There are many different sub-populations of non-disabled Medicaid recipients. Additionally, depending on which population is being studied, the conclusions vary significantly. Medicaid is one of the largest payers of pregnancy-related service in the United States, with 43% of all deliveries in 2016 being financed by Medicaid (Henry J Kaiser Family Foundation 2019a). In a study of Medicaid mothers in the MediCal program, the introduction of managed care was associated with large declines in prenatal care utilization and higher incidence of low birth weight, short gestation, and neonatal death (Aizer, Currie, and Moretti 2007). Another study of California infants found that a switch to managed care from FFS was associated with a substantial increase in spending with little to no improvement in infant health (Duggan 2004).

In two competing national studies, the effect of MMC on women in Medicaid show very different results. A study of adult Medicaid women in managed care found that recipients were 11 percentage points less likely to utilize specialty healthcare and 8 percentage points more likely to report unmet need (Garrett and Zuckerman 2005). In a different national study on adult women in Medicaid with children, managed care was associated with increases in care across all measures. Additionally, there were few differences measured between women in MMC and those in commercial plans (Long, Coughlin, and King 2005). In another two studies, utilizing national health survey data on Medicaid managed care, MMC was associated with increases in the likelihood of a usual source of care (Coughlin, Long, and Graves 2008) and no change in preventable hospitalizations (Basu, Friedman, and Burstin 2004).

One of the largest populations in Medicaid is the non-elderly, non-disabled adult population, including AFDC/TANF eligibles (Burns and Layton 2018). In a study on this population in the MediCal program, Bindman et al. (2005) find that beneficiaries in mandatory managed care experienced hospitalizations for ambulatory care sensitive conditions at a rate 32% less than those in FFS Medicaid. This is in con-

trast to an earlier study on the same MediCal population where MMC was found to increase ambulatory care visits, emergency department use, ambulatory care sensitive condition visits, inpatient admissions, and ancillary service use, but no effect on cost (Lo Sasso and Freund 2000).

Evidence on Disabled Populations

The Medicaid population in the Aged, Blind, and Disabled eligibility group are extremely sensitive to change in their healthcare settings; findings on the effects of MMC on non-disabled populations should not be generalized to them (Burns 2009; Hall et al. 2015). An early target for established MMC programs were children with special health care needs (CSHCN). As increasing numbers of beneficiaries were being shuffled in managed care plans, an important policy debate emerged on how to simultaneously both preserve the existing complex care system already established by the disabled and increase access to primary care for the general population (Iglehart 2011). The result chosen was to carve out all Title V related care for the children (i.e., the MCO was not at financial risk for these services.) This is one of several instances where states would simply cover services on a FFS basis when there was a risk/controversy to providing it through MMC.

In a national study of Medicaid children with special healthcare needs, when compared to FFS, counties with a mandatory integrated capitated managed care program (i.e., a full-risk contract) were associated with decreases in the probability of specialty care and emergency department visits, as well as a reduction in the probability of reporting unmet medical need. Counties with a mandatory managed care program that featured a specialty carve-out (i.e., a service exclusion from the managed care contract) when compared to FFS counties, were associated with decreases in the probability of specialty care visits, including vision and mental healthcare visits, as well as a reduction in the probability of regular pharmacy benefit use. While both programs

were associated with reductions in specialty care, the magnitude of the effect was larger for managed care plans with a carve-out (Davidoff et al. 2007)

In a detailed, systemic literature review, Huffman et al. (2010) evaluate the evidence of the effect of MMC on several metrics for CSHCN. They found that studies examining access to care generally found greater access when than FFS programs, with multiple studies finding decreases in reported unmet medical need. The evidence on utilization, however, was not as conclusive. They find that, while most studies indicated similar utilization between MMC and FFS, some found decreases in specialty care. A study of CSHCN in Michigan found that MMC was associated with a 23% drop in emergency department usage (Pollack et al. 2007).

In a national study of disabled adults in medicaid that compared MMC to FFS, the only statistically significant finding was an increase in having a usual source of care. However, when rural and urban counties were examined separately in that same study, MMC was associated with increases in usual source of care, seeing a general practitioner, seeing a specialist, and receiving a flu shot while seeing not statistically significant difference for rural counties over FFS (Coughlin, Long, and Graves 2008). In another study of disabled adults, researchers found that over 80% lost access to their primary care physician in switching to MMC, and many reported that the in-network specialists were limited or incapable of providing adequate care (Hiranandani 2011). MMC is also associated with increases in wait times, with one study finding that beneficiaries were almost 25% more likely to face in-office waits over 30 minutes – if they were able to find a physician. The same study found disabled beneficiaries were 32% more likely to have difficulty accessing specialty care (Burns 2009), a finding frequently observed in this population (e.g. Hall et al. 2015).

3.2.5 Preventable Hospitalizations

Barriers to primary care are of particular concern in Medicaid managed care, given the incentives managed care contracts impose on MCOs. As discussed in the preceding sections, fixed capitations can incentivize MCOs to restrict access to care in order to reduce costs, especially when the MCO is not at financial risk for all healthcare. In a full-risk arrangement, the MCO is incentivized to encourage inexpensive preventative care in order to reduce the likelihood of expensive hospitalizations that may have been prevented in the less expensive, ambulatory out patient setting. Rates of preventable hospitalizations have thus become an important measure of the success of the primary care delivery system (Billings and Mijanovich 2007; Bindman et al. 2005).

Barriers to accessing timely care are difficult to directly measure in most available data, even in detailed administrative data sets. However, evidence of these barriers may be indirectly measured through changes in rates of preventable hospitalizations (billings). A measure of preventable hospitalization that has been used throughout the literature is the ambulatory care sensitive condition hospitalization (ACSCH) rate (e.g. Laditka, Laditka, and Probst 2005; Bindman et al. 2005). ACSCHs are hospitalizations for conditions, such as asthma and diabetes that have substantial evidence of responding to timely management in the ambulatory care setting. Hospitalizations for these conditions are often indicators for issues in the outpatient setting making this measure an ideal candidate for measuring changes in healthcare access and quality (Bindman et al. 2005). Examples of these conditions include asthma, diabetes, and congestive heart failure (Laditka, Laditka, and Probst 2005).

In a study of physician supply in twenty states, Laditka, Laditka, and Probst (2005) find that increases in the number of primary care physicians for a given county are associated with fewer ACSCHs. This result held across all age groups. This provides evidence that increases in access to primary care may reduce the likelihood of preventable hospitalization. As such, high rates of ACSCH admissions should be

viewed as an issue of access and not an issue of poor judgment on the part of patients (Kellermann and Weinick 2012).

Studies on non-Medicaid patients frequently find a negative relationship between rates of ACSCHs and managed care (Basu, Friedman, and Burstin 2004). In a study of preventable hospitalizations for commercially insured patients, Zhan et al. (2004) found that a 10% increase in HMO penetration rates was associated with an almost 4% decrease in preventable hospitalizations. When examining preventable hospitalizations in a Medicaid population, however, the evidence is mixed. A study on emergency department records found that, though Medicaid patients made up only 14% of the sample, Medicaid patients made up 23% of preventable admissions (Oster and Bindman 2003). Additionally, in a study of Medicaid managed care, rates of ACSCHs were identical for MMC and FFS (Basu, Friedman, and Burstin 2004).

3.3 Data and Methods

3.3.1 Program Details

The purpose of this study is to estimate the causal effect of enrolling the physically and developmentally disabled of Medicaid into a mandatory managed care program on the rate of preventable hospitalizations for ACSC's in the New Jersey Medicaid Program. New Jersey's Medicaid program is one of the largest in the country, ranking 13th in terms of total enrollment(Henry J Kaiser Family Foundation 2019d). Covering 1,696,907 people in its program, Medicaid provides health care to almost one in five in New Jersey(Henry J Kaiser Family Foundation 2019b). This comes at a considerable cost, with total Medicaid spending reaching \$15 billion in 2014, the 10th largest program, in terms of spending, in the nation(Henry J Kaiser Family Foundation 2019c). To put the cost in context, the state proposed \$34.4 billion in total spending in their 2014/2015 budget, meaning that Medicaid represented almost 44% of the

total budget (Christie and Guadagno 2014a)¹.

The ABD population is relatively small, with about 356,000 recipients (Henry J Kaiser Family Foundation 2019b), or approximately 20% of all Medicaid recipients in New Jersey. Though small in number, they are extremely costly, representing almost 65% of all Medicaid expenditures in 2014 (Henry J Kaiser Family Foundation 2019c). Prior to the transition to MMC, New Jersey Medicaid provided benefits to the ABD population through FFS-based model and a voluntary MMC program. In 2000, New Jersey Medicaid announced that the non-dually Medicare eligible ABD beneficiaries would be transitioned into mandatory managed care on a county-by-county basis (N.J. Dept. of Human Services, Div. of Medical Assistance & Health Services 2000). The initiative was listed in Governor Whitman's budget as a priority, citing previous success with managed care in the state (Whitman 2000). The MMC program covered almost all aspects of the recipients care with a few exceptions ("carve-outs"). The services that remained in FFS were long-term institutional care, medical day care, outpatient rehabilitation services, home health, and all pharmacy services. While this seems like a significant number of services not provided by the MCO, the vast majority of healthcare was to be provided and coordinated by the MCO.

To my knowledge, there was not systematic selection criteria used to determine county timing. Any beneficiary not enrolled in a MMC plan by a county-specific cutoff date would be auto-assigned into a plan. Recipients, however, were able to request exemptions from the policy on a case-by-case basis. About 13% of recipients were granted exclusions. This transition began with a trial demonstration in Camden county on May 1, 2002. After a period of monitoring the experience of Camden county, New Jersey Medicaid would begin to enroll the remaining twenty counties . By 2009, all 21 counties in New Jersey had been transitioned into the new MMC

¹Given that Medicaid is a jointly financed program between the states and the federal government, New Jersey did not provide funding for the full \$15 billion in spending. Rather, New Jersey contributed about 30% (Christie and Guadagno 2014a).

regime for the ABD's. The cohort transitions are illustrated in Figure 3.1 and county-level transition dates are found in Table 3.1.

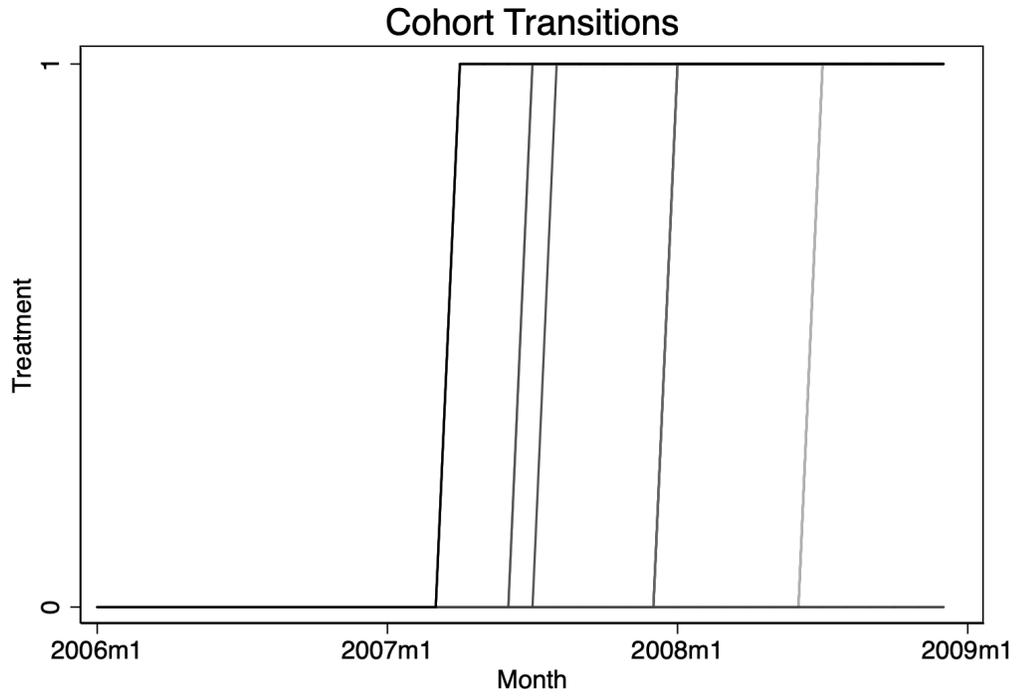


Figure 3.1: Illustration of Cohort Transitions to the Treatment Group

Table 3.1: County Transition Dates and Cohorts

County Code	County Name	Date	Cohort
01	Atlantic	4-2007	2
02	Bergen	7-2007	3
03	Burlington	7-2008	6
04	Camden	5-2002	1
05	Cape May	7-2008	6
06	Cumberland	1-2008	5
07	Essex	7-2008	6
08	Gloucester	4-2007	2
09	Hudson	1-2009	7
10	Hunterdon	1-2009	7
11	Mercer	7-2008	6
12	Middlesex	1-2008	5
13	Monmouth	8-2007	4
14	Morris	1-2008	5
15	Ocean	7-2007	3
16	Passiac	7-2007	3
17	Salem	1-2008	5
18	Somerset	1-2009	7
19	Sussex	4-2007	2
20	Union	1-2009	7
21	Warren	4-2007	2

The primary data for this study is a novel dataset of enrollment and hospital admissions data aggregated directly from the New Jersey Medicaid Management Information System’s confidential databases². To be included in this study, a recipient must have been enrolled for the entire calendar year, not dually eligible for Medicare, non-institutionalized, and may not have spent more than 30 consecutive days in a skilled nursing facility, so that the effect of MMC can be accurately measured. Restricting the inclusion criteria to a full year ensures that individuals are observed for an adequate number of periods and though the ABDs are often the most stable, in terms of maintaining eligibility, there are some recipients that lose and regain eligibility or those new to the program whose experience may not be reflective of the full functionality of the New Jersey Medicaid program. Medicaid-Medicare dual eligibles receive their benefits primarily thorough Medicare and, thus not suitable for this study. The Burns and Layton (2018) notes that dual eligibles and Medicaid-only ABDs share many of the same characteristics and the biggest difference between them is how they receive their benefits. Lastly, those housed in long-term care institutions and those with extended stays in skilled nursing facilities receive their benefits exclusively through the FFS system and were not eligible for MMC.

I use data from the Census Bureau’s County Business Patterns Data (United States Census Bureau 2018) on several NAICS industry codes to capture county-level physician supply in New Jersey . This allows the model to capture differences in access to primary care across counties that may affect preventable hospitalization rates. Further, I add demographic variables from the CDC Wonder data tool (Centers for Disease Control and Prevention, National Center for Health Statistics 2018) to capture the differences in racial, ethnic, and gender composition across counties within the state. To control for county level differences in income and poverty, I include data from the Small Area Income and Poverty Estimates dataset (United States Census

²The data for this study was acquired through a negotiated Open Public Records Act request, NJ OPRA request W100280

Bureau 2019).

The primary outcome used in this study is the preventable hospitalization rate per 1,000 ABD members that meet that inclusion criteria. Additionally, to validate the model, I construct a similar data set using the same construction methods as the primary, except the outcome of interest is a set of diagnoses not likely to be affected by a change in the way the enrollee receives care (e.g., acute appendicitis.) The study period is from January, 2006 until December, 2009. I exclude Camden county from the study as it transitioned prior to the quality data reporting initiatives, and thus not suitable for use³. Since the implementation of the program was staggered and eventually all counties received treatment, the end point of the data was chosen so that the last cohort of counties to be transitioned to MMC are not observed in the treated state. Thus, the comparison group consists of both the never treated⁴ and the not-yet treated. All counties that transition in the study window are observed at least twelve months prior to treatment, and all but four are observed for at least twelve months after treatment.

In this study, I exploit the variation in preventable hospitalizations induced by the mandatory transition into managed care using a differences-in-differences model. Unlike much of the literature, this study avoids the potential selection bias given that treatment is mandatory and timing is as good as randomly assigned. The model I estimate is given by Equation 3.1

$$Y_{cmt} = \alpha + DD_{cmt} + X_{cmt}\beta + \gamma_c + \lambda_m + \delta_t + \epsilon_{cmt} \quad (3.1)$$

where y_{cmt} is rate of ACSC hospitalizations per 1,000 recipients for county, c , in month, m , in year, t . Additionally, I include X_{smt} , a vector of county-year level controls to control for time-variant factors that may affect preventable hospitalizations.

³Starting in 2006, MCOs were contractually required to provide thorough and detailed records on care provided to beneficiaries they serve.

⁴Never treated, in the context of this study, is the cohort of counties that was treated last.

I include three fixed effects: γ_c is a county fixed effect to control for time-invariant differences between counties, δ_m is a month fixed effect to control for the seasonality in hospitalization rates, and δ_t is a year fixed effect . All models are estimated using ordinary least squares with heteroskedasticity robust standard errors. I do not to use cluster-robust standard errors because I believe that unobserved shocks that could affect preventable hospitalizations for this particular population are likely systemic in that they would affect all counties in the state and not particular counties given that: 1. the MMC and FFS programs are administered at the state-level with identical benefits and individual MCO's operating statewide, and 2. there are few counties that all receive treatment in a geographically small state. However, I weight the regressions by total county population to address heterogeneity in county size.

3.4 Results

3.4.1 Summary Statistics

Summary statistics for outcomes and control variables are presented in Table 3.2. In the period prior to transitioning into MMC, the average monthly preventable hospitalization rate was 9.6 per 1,000 recipients for the treatment group and 8.8 for the never-treated group. In terms of marker admissions, the average monthly pre-treatment rate for the treated counties was 0.3577 per 1,000 recipients and 0.3911 for the never-treated counties. For the last outcome measure, the recipient mortality rate, the average monthly rate in the pre-treatment period was 0.0667 per 1,000 recipients for the treated group and 0.0918 for the never-treated group.

The demographic control variables were comparable across all measures. Additionally, county recipient counts were also comparable. In terms of MCO market share, the two groups had very similar shares across the treated and never-treated counties. The last control variable of interest, the number of primary care firms per

100,000 county residents was slightly higher for the treated counties, with 91.586 firms per 100,000 residents, compared to 84.0513 for the never treated.

Table 3.2: **Pre-Treatment Period Variable Means and Proportions**

Variable	All Counties	Treated	Comparison
<i>Outcome:</i>	Mean/Proportion	Mean/Proportion	Mean/Proportion
ACSCH Rate	9.3976	9.5940	8.8462
Marker Rate	0.3665	0.3577	0.3911
Mortality Rate	0.0733	0.0667	0.0918
<i>Demographics:</i>			
Population	411,161.1	416,426.8	396,373.4
ABD Members	4,196.595	4,123.641	4,401.475
White	0.7902	0.7943	0.7786
Black	0.1391	0.1425	0.1294
Latin	0.1467	0.1249	0.2080
Asian	0.0654	0.0580	0.0865
Male	0.4889	0.4882	0.4908
<i>Physician Supply</i>			
PCP Firms PC	89.6079	91.5866	84.0513
<i>Income and Poverty</i>			
Log(Median Inc.)	11.0972	11.0497	11.2189
Poverty Rate	7.52	8.7363	7.5583
N:	457	337	120

Note: Outcome variables are in terms of rate of event per 1,000 qualified recipients.

3.4.2 Event Study and Program Dynamics

The DD model employed in this paper critically relies on the exogeneity of the policy assignment. A particular threat to this assumption is policy endogeneity. Policy endogeneity occurs when the policy is implemented in response to pre-treatment outcomes. In the context of this study, an example would be that New Jersey implemented the MMC program for the ABD's in response to high rates of preventable hospitalizations. Since the policy was decided at the state-level, I can only provide anecdotal evidence from the literature to support the endogeneity of the policy. As discussed in the preceding sections, MMC was an attractive option for states to minimize the

large fluctuations in costs associated with their highest cost recipients. Despite the concerns that MCO's would not be well equipped to serve the 2000's saw substantial growth of MMC for the ABD population (Iglehart 2011; Burns and Layton 2018).

To address the exogeneity of the timing of treatment assignment at the county level, I conduct an event study (Kline 2011; Wing, Simon, and Bello-Gomez 2018). I estimate the simple DD model given by Equation 3.1 and to it I add a series of policy leads and lags. In addition, I impose endpoint restrictions that treatment dynamics end after five months. This allows for the estimation of treatment dynamics and provides evidence to support the argument of treatment exogeneity (Kline 2011). Ideally, the coefficients on the treatment leads would be zero meaning that the treatment did not have a statistically significant effect before it was implemented.

Table 3.3 presents the results from the event study regression. They show that most estimates are significantly not different from zero. However, I find that there is some evidence of an effect in the fifth and sixth treatment lead. This can be explained by the fact that, prior to the transition, MCOs started recruiting ABDs into MC plans in the months leading up to the final transition date. Though significant, they are only so at the 10% level with 95% confidence intervals straddling zero. I interpret this finding as the effect of voluntary managed care. Further, when these pre-treatment trends are controlled for, I find a statistically significant treatment effect at the treatment date of similar size and sign as my primary analysis. These results are illustrated in Figure 3.2.

Table 3.3: **The Effect of Mandatory Managed Care on Preventable Hospitalizations Using an Event Study**

Outcome:	ACSC Hospitalization Rate
5 Years Pre-Transition	-0.3666* (0.1963)
4 Years Pre-Transition	-0.3473* (0.1854)
3 Years Pre-Transition	-0.2645 (0.1865)
2 Year Pre-Transition	-0.1322 (0.2053)
1 Year Pre-Transition	0 ⁺
Transition (Treat)	-0.5537** (0.2533)
1 Year Post-Transition	0.0788 (0.1899)
2 Years Post-Transition	0.0444 (0.1861)
3 Years Post-Transition	0.1634 (0.2056)
4 Years Post Transition	-0.1381 (0.1775)
5 Years Post-Transition	0.0028 (0.1971)
Pretreatment Mean	9.5940
<i>N</i>	717

Note: Model estimated using OLS with control for county-level characteristics and county, year, month fixed effects, and are weighted by county population. Standard errors are robust and are in parentheses. The study period is between January/2006 until December/2008. . *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. ACSCH is the preventable hospitalization rate. + Restricted to 0 by assumption.

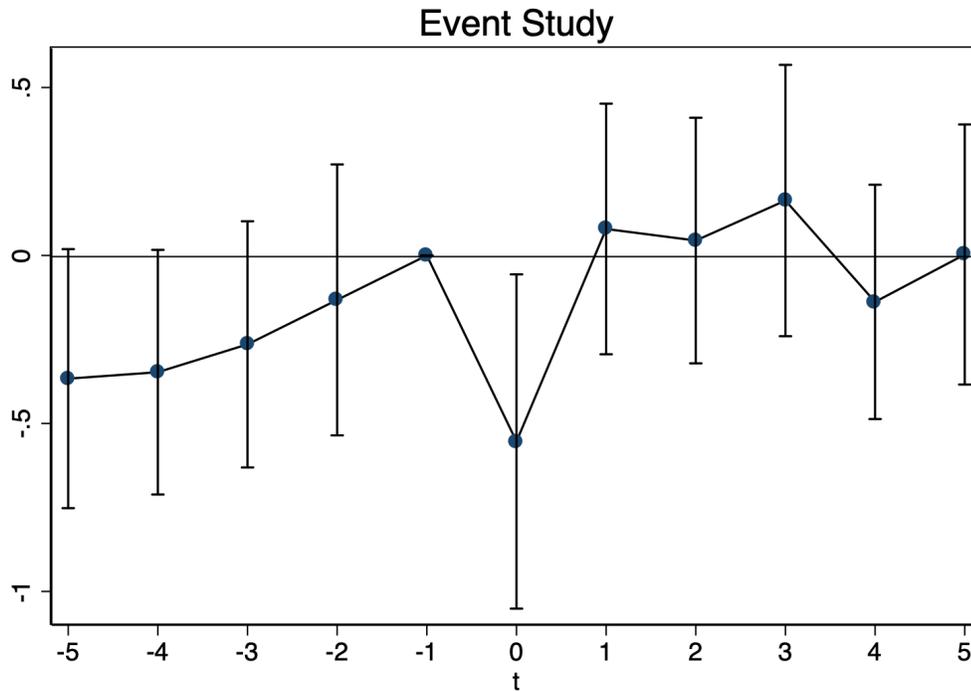


Figure 3.2: **The Effect of Mandatory Managed Care on Preventable Hospitalizations Using an Event Study**

3.4.3 DD Estimates

I estimate two main model specifications. The first model contains county-level demographic controls, fixed effects, and county-specific linear time trends. The results from the first model specification can be found in Table 3.4. I find that the introduction of mandatory managed care reduced the monthly preventable hospitalization rate by -0.6130 per 1,000 ABD recipients. This represents a -6.4%[-11.5,-1.3] decrease over the pre-treatment mean.

The second model I estimate adds to the primary specification controls for the number of primary care firms per 100,000 county residents as a proxy for quantity-related measure of physician access as well as the poverty rate and log of median household income. The results of this model can be found in Table 3.5.

The inclusion of MCO and PCP variables reduced the size of the estimated effect, with a reduction in monthly preventable hospitalizations of -0.6003 per 1,000 recipients. This represents a -6.26% [-11.41,-1.10] decrease in ACSC hospitalizations over the pre-treatment mean.

Table 3.4: The Effect of Mandatory Managed Care on Preventable Hospitalizations

Outcome	Pre-Transition mean	DD estimate
ACSCH Rate	9.5940	-0.6130** (0.2502)
	<i>N</i>	720

Note: Model estimated using OLS with control for county-level characteristics and county, year, month fixed effects, and are weighted by county population. Standard errors are robust and are in parentheses. The study period is between January/2006 until December/2008. . *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. ACSCH is the preventable hospitalization rate.

Table 3.5: The Effect of Mandatory Managed Care on Preventable Hospitalizations, Expanded Model

Model:	Expanded	
Outcome	Pre-Transition mean	DD estimate
ACSCH Rate	9.5940	-0.6003** (0.2519)
	<i>N</i>	720

Note: Model estimated using OLS with control for county-level characteristics and county, year, month fixed effects, and are weighted by county population. Expanded model includes additional controls for income, poverty, and physician supply. Standard errors are robust and are in parentheses. The study period is between January/2006 until December/2008. . *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. ACSCH is the preventable hospitalization rate.

3.5 Robustness Checks

3.5.1 Parallel Trends Test

A critical assumption in recovering causal estimates of this DD model is that the treatment and never-treated counties would have trended similarly had the intervention not been applied. Since it is not feasible to show such a counterfactual, suggestive evidence is provided with a comparison of pre-transition preventable hospitalization rate trends. As seen in Figure 3.3, both groups follow fairly similar trends in the pre-treatment period. Additionally, I formally test the assumption using OLS. This test provides evidence that the two groups did not trend differently in the period leading up to the change. I estimate the model:

$$y_{cmt} = \alpha_0 + \alpha_1 T_{mt} + \alpha_2 (T_{mt} \times Treat_c) + X'_{smt} \alpha_3 + \gamma_c + \delta_m + \epsilon_{cmt} \quad (3.2)$$

where y_{cmt} , γ_c , δ_m , and X_{cmt} are identical to those variables in Equation 3.1. In addition, I include a linear time trend T_{mt} and an interaction of the trend with $Treat_s$, an indicator variable for those counties to receive treatment. The coefficient of interest is α_2 which is the estimate of the difference in pre-treatment trends between the comparison and treatment groups. If the two groups trended similarly, then α_2 should be zero and the assumption satisfied. The results of Equation 3.2 can be found in Table 3.6. We see that the coefficient for α_2 is insignificant. This test provides evidence that the parallel trends assumption is satisfied.

Table 3.6: **Parametric Test for Parallel Trends**

Outcome:	ACSCH Rate
Time	0.1835*
	(0.0965)
(Time×Treated)	-0.0191
	(0.0669)
N	457

Note: Model estimated using OLS with control for county-level characteristics and county, and month fixed effects, with county-specific linear time trends. Standard errors are robust and are in parentheses. The study period is between January/2006 until December/2008. . *, **, ***=statistically different from zero at the 10%, 5%, and 1% level.

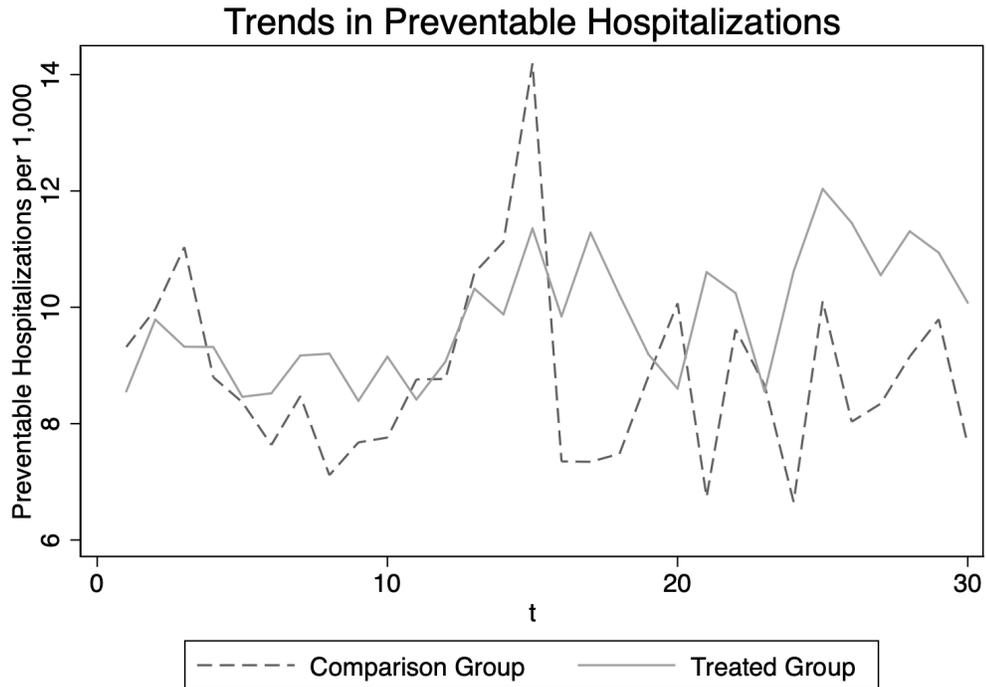


Figure 3.3: **Unadjusted Trends in Preventable Hospitalizations in the Pre-Treatment Period**

3.5.2 County-Specific Linear Trends

A common concern in DD models is that effects assumed to be time-invariant may not be entirely fixed; that is, there may be time-variant unobservable differences

between counties. To address this possible omitted variable bias I include county-specific linear time trends. Table 3.1 presents the results from adding these trends to the model. I find that, in this specification, that the implementation of MMC reduced the preventable hospitalization rate -0.8892 per month, per 1,000 recipients. This reflects a -9.27%[-15.09,-3.44] decrease.

Table 3.7: **The Effect of Mandatory Managed Care on Preventable Hospitalizations, County Trends**

Outcome	Pre-Transition mean	DD estimate
ACSCH Rate	9.5940	-0.8892*** (0.2846)
	<i>N</i>	720

Note: Model estimated using OLS with control for county-level characteristics and county, year, month fixed effects, are weighted by county population, and include county-specific linear time trends. Standard errors are robust and are in parentheses. The study period is between January/2006 until December/2008. . . *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. ACSCH is the preventable hospitalization rate.

3.5.3 Model Validation

Hospitalizations for ACSC’s are considered to be preventable hospitalizations (Bindman et al. 2005). However, not all hospitalizations can be prevented. Conditions such as acute appendicitis cannot be prevented via ambulatory care, and thus is viewed as “unpreventable” (*ACS Codes*). Thus, changes in access to medical care should not have an effect on these types of hospitalizations. To validate the research design of this study, I estimate the primary model given by Equation 3.1 with the rate of marker condition, i.e. unpreventable, hospitalizations per 1,000 recipients as the dependent variable. Given that these hospitalizations cannot be prevented then the transition to MMC should not have a measurable effect. The results for marker conditions can be found in Table 3.8. I find no statistically significant effect on the rate of marker hospitalizations which I believe provides further evidence of the validity of the design of this study.

A second consideration is recipient mortality. ABD recipients are extremely sensitive to small changes in their healthcare (Hall et al. 2015). However, it seems unlikely that a transition to MMC would have a measurable effect on the recipient mortality rate. I test this by estimating the primary model given by Equation 3.1 with the mortality rate per 1,000 recipients as the dependent variable. The results for the

mortality rate are presented in Table 3.8. Again, I find no statistically significant effect. This provides additional evidence for the validity of this study.

Table 3.8: The Effect of Mandatory Managed Care on Preventable Hospitalizations: Robustness Checks and Model Validation

Outcome	Pre-Transition mean	DD estimate
Marker Rate	0.3577	-0.0160 (0.0533)
Mortality Rate	0.0667	-0.0341 (0.0228)
	<i>N</i>	720

Note: Model estimated using OLS with control for county-level characteristics and county, year, month fixed effects, and are weighted by county population. Standard errors are robust and are in parentheses. The study period is between January/2006 until December/2008. . *, **, ***=statistically different from zero at the 10%, 5%, and 1% level. ACSCH is the preventable hospitalization rate.

3.6 Discussion

In this study I utilize a difference-in-differences model to estimate the causal effect of a mandatory transition into managed care for the physically and developmentally disabled Medicaid population in New Jersey on the rate of preventable hospitalizations. I exploit the variation in ACSC hospitalizations induced by the implementation of the MMC program. I contribute to the literature by providing causal estimates of the effect of managed care for the ABD population. There currently exists little evidence on this particular population and none, to my knowledge, that utilizes administrative claims data for the entire population or that does not suffer from selection bias in a voluntary MMC program, and the first to do so with New Jersey’s program.

I find that the introduction of mandatory MMC for the ABD population reduced preventable hospitalizations by 6.4%[-11.5,-1.3], per county, per month. Additionally, I validate the study design by analyzing the effect of mandatory MMC on a set of hospitalizations that are considered to be nonpreventable for which I find no statisti-

cally significant effect. I conduct an event study to address the potential endogeneity of the policy and I, again, find no effect prior to the introduction of the policy. Even when controlling for HMO market share and physician supply, I still find a significant effect of the policy.

A large concern in the literature is that MCOs are not well equipped or have mature enough networks to address the unique needs of the disabled (Hall et al. 2015; Huffman et al. 2010). However, as noted by Iglehart (2011), prior to the introduction of MMC, ABD's received their care through a fragmented and often uncoordinated FFS system with limited provider participation given the historically low reimbursement rates offered by states. The evidence in this paper suggests that managed care, though often inexperienced in dealing with recipients with complex healthcare needs, can deliver quality and access increases over traditional FFS systems. Though additional research is warranted, the results of this study are promising, especially given the recent growth in MMC for this vulnerable population.

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APPENDIX A:

VBID EXTENSIONS AND ROBUSTNESS

CHECKS

Table A1: **Effect of VBID on any Service Use: Falsification Testing using False Treatment Groups**

Outcome:	Any ACT	Diagnostics	Surgeries
OEBB Proportion, Pre-treatment	0.0403	0.0368	0.0060
Comparison Com- pany 1	0.003	0.002	0.005***
	(0.003)	(0.003)	(0.001)
Observations	742,929	742,929	742,929
Comparison Com- pany 2	0.008***	0.007***	0.002
	(0.003)	(0.003)	(0.001)
Observations	742,929	742,929	742,929
Comparison Com- pany 3	0.003	0.003	-0.001
	(0.003)	(0.003)	(0.001)
Observations	742,929	742,929	742,929

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases. See text for a description of the Swartz et al low-value care variable.

***,**=statistically different from zero at the 1%; 5% level.

Table A2: **Effect of VBID on any Service Use: Dynamic Model**

Outcome:	Any ACT	Diagnostics	Surgeries
OEBB Proportion, Pre-treatment	0.0403	0.0368	0.0060
DD Early (2010 Q4-2011 Q3)	-0.005** (0.002)	-0.005** (0.002)	-0.001 (0.001)
DD Late (2011 Q4- 2012 Q3)	-0.006*** (0.002)	-0.005** (0.002)	-0.004*** (0.001)
<i>F</i> -statistic from test of Coefficient Equality (<i>p</i> -value)	0.67 (0.41)	0.00 (0.98)	9.04 (0.003)
Observations	742,929	742,929	742,929

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases. See text for a description of the Swartz et al low-value care variable.

***,**=statistically different from zero at the 1%; 5% level.

Table A3: **Effect of VBID on any ACT Service Use: Alternative Samples and Specifications**

Outcome:	Any ACT	Diagnostics	Surgeries
OEBB Proportion, Pre-treatment†	0.0403	0.0368	0.0060
Panel A: Balance Panel			
DD	-0.004 (0.002)	-0.003 (0.002)	-0.002*** (0.001)
Observations	224,960	224,960	224,960
Panel B: Non-Switching Sample			
DD	-0.004 (0.002)	-0.003 (0.002)	-0.001 (0.001)
Observations	270,211	270,211	270,211
Panel C: Control for Plan Fixed Effects			
DD	-0.006*** (0.002)	-0.005*** (0.002)	-0.002** (0.001)
Observations	742,670	742,670	742,670
Panel D: Exclude Beneficiary Fixed Effects			
DD	-0.017*** (0.001)	-0.016*** (0.001)	-0.004*** (0.000)
Observations	742,670	742,670	742,670
Panel E: Include Employer Specific Time Trends			
DD	-0.009*** (0.003)	-0.010*** (0.003)	-0.001 (0.001)
Observations	742,670	742,670	742,670
Panel F: Always ACT Service			
DD	-0.005*** (0.002)	-0.005*** (0.002)	-0.001 (0.001)
Observations	742,670	742,670	742,670
Panel G: No Cancer Exclusions			
DD	-0.005*** (0.002)	-0.005*** (0.002)	-0.002*** (0.001)
Observations	742,670	742,670	742,670
Panel H: Wild-Cluster Bootstrap Approach			
<i>t</i> -statistic	-3.768	-3.485	-1.608
Observations	742,670	742,670	742,670

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects unless otherwise noted. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases.

†Proportions are based on the full pre-treatment OEBB sample using the primary classification of ACT services.

***,**=statistically different from zero at the 1%; 5% level.

Table A4: **Comparison of Demographics of Members of the Analysis (Unbalanced and Balanced Samples): Pre-VBID Period**

Sample:	Analysis(balanced)	Unbalanced Panel
Age	46.72	43.32
Male	0.454	0.456
Female	0.546	0.544
Employee	0.577	0.477
Spouse	0.349	0.323
Child	0.074	0.200
Observations	224,960	517,710

Note: The unit of observation is a beneficiary/quarter/year. Pre-VBID period is October 1st 2008 to September 31st 2010.

Table A5: **Comparison of Demographics of Members of the Non-Plan Switching and Plan Switching Samples at OEBC: Pre-VBID Period**

Sample:	Non-Plan Switching	Plan Switching
Age	43.48	44.66
Male	0.473	0.447
Female	0.527	0.553
Employee	0.480	0.528
Spouse	0.341	0.340
Child	0.179	0.132
Observations	77,111	177,177

Note: The unit of observation is a beneficiary/quarter/year. We do not include non-OEBC data as each of the three comparison companies offers just one plan during our study period, thus there is no plan switching. Pre-VBID period is October 1st 2008 to September 31st 2010.

Table A6: **Effect of VBID on any ACT Service Use: Alternative OEGB Samples**

Outcome:	Any ACT	Diagnostics	Surgeries
OEGB Proportion, Pre-treatment†	0.0403	0.0368	0.0060
Panel A: Low Cost-Sharing Plans (Main Sample)			
DD	-0.005*** (0.002)	-0.004** (0.002)	-0.002*** (0.001)
Observations	742,670	742,670	742,670
Panel B: All Plans			
DD	-0.007*** (0.002)	-0.006*** (0.002)	-0.002*** (0.001)
Observations	1,249,446	1,249,446	1,249,446
Panel C: High Cost-Sharing Plans			
DD	-0.009*** (0.002)	-0.008*** (0.002)	-0.003*** (0.001)
Observations	604,226	604,226	604,226

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases.

†Proportions are based on the full pre-treatment OEGB sample using the primary classification of ACT services.

***,**=statistically different from zero at the 1%; 5% level.

Table A7: **Parametric Test for Parallel Trends Between OEBC and Comparison Companies in any ACT Use: Pre-Treatment Period**

Outcome:	Any ACT	Diagnostics	Surgeries
OEBC Proportion, Pre-treatment	0.0403	0.0368	0.0060
OEBC*Linear Trend	0.001** (0.001)	0.001 (0.000)	0.000 (0.000)
Observations	302,587	302,587	302,587

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases.

***,**=statistically different from zero at the 1%; 5% level.

Table A8: **Effect of VBID on any ACT Service Use: Falsification Testing Using a False Effective Date**

Outcome:	Any ACT	Diagnostics	Surgeries
OEBC Proportion, Pre-treatment	0.0403	0.0368	0.0060
False Effective Date (Use Pre-VBID Date)	-0.001 (0.002)	-0.000 (0.002)	-0.001 (0.001)
Observations	302,587	302,587	302,587

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases.

***,**=statistically different from zero at the 1%; 5% level.

Table A9: **Effect of VBID on any ACT Service Use: Alternative Comparison Groups**

Outcome:	Any ACT	Diagnostics	Surgeries
OEBB Proportion, Pre-treatment†	0.0403	0.0368	0.0060
Panel A: Exclude Comparison Company 1			
DD	-0.006*** (0.002)	-0.005*** (0.002)	-0.001 (0.001)
Observations	718,493	718,493	718,493
Panel B: Exclude Comparison Company 2			
DD	-0.004 (0.002)	-0.003 (0.002)	-0.002** (0.001)
Observations	704,957	704,957	704,957
Panel C: Exclude Comparison Company 3			
DD	-0.006*** (0.002)	-0.006*** (0.002)	-0.003*** (0.001)
Observations	707,110	707,110	707,110

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases.

†Proportions are based on the full pre-treatment OEBB sample using the primary classification of ACT services.

***,**=statistically different from zero at the 1%; 5% level.

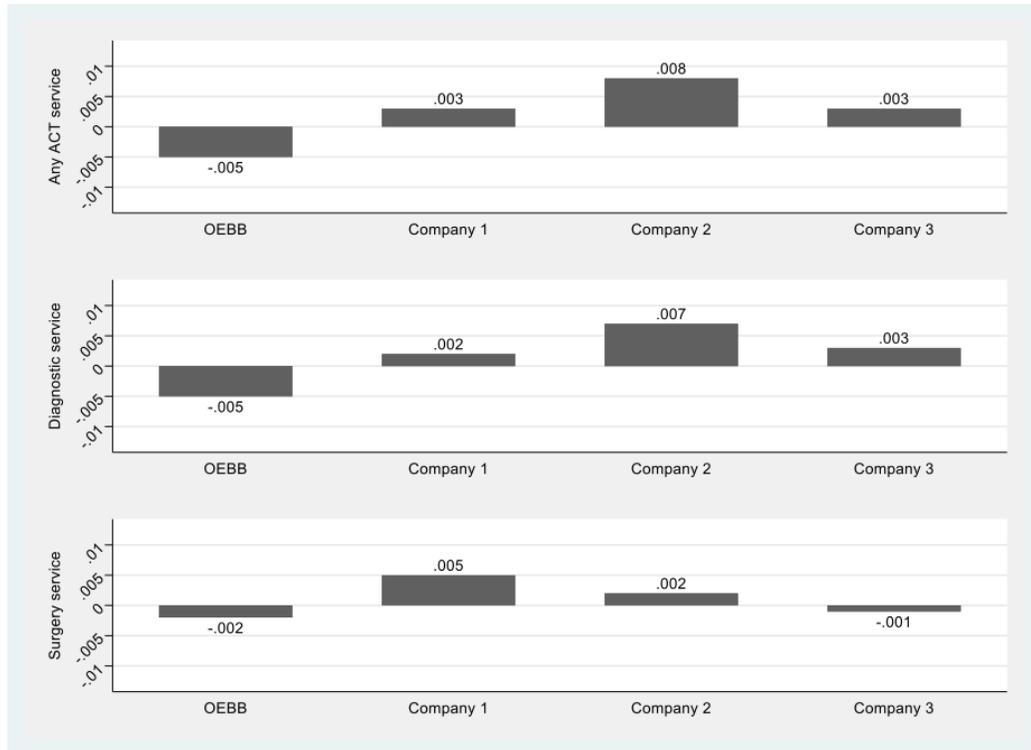
Table A10: **Effect of VBID on any ACT Service Use: Using Post-Q3 2012 Data**

Outcome:	Any ACT	Diagnostics	Surgeries
OEBB Proportion, Pre-treatment†	0.0403	0.0368	0.0060
Panel A: Low Cost-Sharing Plans (Main Sample)			
DD	-0.005*** (0.002)	-0.005*** (0.002)	-0.002*** (0.001)
Observations	742,670	742,670	742,670
Panel B: All Plans			
DD	-0.007*** (0.002)	-0.006*** (0.002)	-0.002*** (0.001)
Observations	1,249,446	1,249,446	1,249,446

Note: : The unit of observation is a beneficiary/quarter/year. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. Robust standard errors reported in parentheses. The study period is October 1st, 2008 to September 30th, 2012. See Table 2.2 for cost-sharing increases.

†Proportions are based on the full pre-treatment OEBB sample using the primary classification of ACT services.

***,**=statistically different from zero at the 1%; 5% level.



Notes: The unit of observation is a beneficiary/quarter/year. See Table 2.3 for proportions values. Each panel refers to a different outcome variable. The bar graph indicates the estimated coefficient estimate from Equation 2.1. The X-axis in each panel reports the employer that is coded as the treatment group in the DD regression model, all other employers are coded as being in the comparison group. All models estimated with an LPM and control for beneficiary fixed effects and period fixed effects. The study period is October 1st, 2008 to September 30th, 2012.

Figure 4: **Comparison of Estimated Treatment Effects: Main DD Estimate and Placebo-in-Place Estimates**

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