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Essays in Public Policy and Mental Health

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Dedication

This work is dedicated to those who have experienced mental illness, their loved ones, and those that advocate on their behalf.

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Abstract

In this work I examine how various public policies can influence mental health outcomes. As mental health issues can be created or exacerbated by numerous different factors, many types of public policy can have mental health implications, perhaps not always intended by policy makers. In many cases, these implications are ambiguous from a theoretical level, making empirical research in the area important. Specifically, I model how labor market policies with minimum wages and Earned Income Tax Credit, health policy with the Dependent Coverage Mandate aspect of the Affordable Care Act, and social policy with recreational cannabis laws impact mental health outcomes in different populations.

In my first chapter, joint work with Dr. Karen Smith Conway, we estimate how minimum wages and the Earned Income Tax Credit impact mental health around the time of pregnancy. Using data from the Pregnancy Risk Assessment Monitoring System and a generalized difference-in-differences approach, we find both policies can be effective in reducing mental distress around pregnancy. Effects are largest for groups with lower levels of education and robust to event study analysis.

My second chapter examines how the Dependent Coverage Mandate, which required private insurers to allow dependents to remain on their guardian's health insurance plan until age 26, influenced mental health outcomes and mental health care utilization with special attention to differences across race and gender. Using data from the Medical Expenditure Panel Survey, I find previously overlooked improvements in mental health outcomes for Black non-Hispanic young adults, driven by Black non-Hispanic women, the same group that experienced the largest gain in health insurance. When looking at mental health care, I do not find increases for this

population. Together, these results suggest a pathway for insurance to improve mental health outside of care utilization.

The third chapter models the impact of recreational cannabis laws on mental health. With data from the Behavioral Risk Factor Surveillance System and a staggered difference-in-differences model, I find improvements in mental health status driven by various subgroups. I also find a reduction in activity limitation, possibly driven by mental health. These results are robust to new difference-in-differences methods such as a Goodman-Bacon decomposition and Callaway-Sant'Anna estimators.

INTRODUCTION

Many have claimed that the U.S. is in the middle of a mental health crisis. Rates of mental illness are rising, particularly for young adults and adolescents, as are deaths by suicide. While treatment can be effective, obstacles such as high cost for the uninsured and shortages of providers prevent many from receiving the care they may need. Media members and government bodies have recognized this as a growing issue, yet much remains unknown with regards to possible solutions. Some argue that while awareness around mental health has grown, access and quality of care remain problematic. This dissertation aims to improve our understanding of the role public policy plays in influencing mental health outcomes.

The nature of mental health allows for a wide set of policies to have potential impacts, including those not intended to address health issues. This opens the door for many public policies to help ease the mental health crisis, with or without intention. In this work, I examine effects stemming from three different sectors of public policy: labor, health, and social policy.

First, with coauthor Dr. Karen Smith Conway, I examine the role of labor market policies and perinatal mental health. Specifically, we model state minimum wages and state Earned Income Tax Credit (EITC) levels and measures of mental health before, during, and after pregnancy. We find sizable reductions in depression before pregnancy for both policies and after pregnancy for minimum wages. Additionally, we find improvements in mental health during pregnancy resulting from EITC levels. Effects are concentrated in groups most likely impacted by these policies and are robust to numerous checks and tests. These estimates show benefits for both policies previously unknown for a particularly important population. We also find some suggestive evidence of financial stress relief as a primary mechanism, although other pathways such as mental health care utilization cannot be modeled for this population.

My second chapter is motivated by two observations others have noted as a point of concern. First, young adults (age 18-25) have the highest rates of mental illness out of any age group in the U.S. At the same time, young adults also receive the least amount of mental health care, conditional upon illness. I investigate how young adult mental health and mental health care utilization was impacted by the Dependent Coverage Mandate (DCM) aspect of the Affordable Care Act, which offered young adults an additional pathway for insurance coverage. Previous research has modeled the DCM and mental health outcomes. However, previous work has not properly investigated differential effects by key demographics such as race, ethnicity, and gender. As both mental health outcomes and access to care differ meaningfully by these demographics, it is important to allow for heterogeneous effects in this area.

Using data from the Medical Expenditure Panel Survey and a difference-in-differences framework, I find marginally significant improvements in general mental health for the full sample of young adults, in line with previous research. However, when examining heterogeneous effects, Black non-Hispanic young adults appear to be driving this finding. Additionally, this group is also found to have significant improvements in more serious measures of mental health, a finding overlooked by past work. Furthermore, Black non-Hispanic young adults are found to have the largest gain in health insurance coverage following the DCM. With respect to mental health care utilization, I find that the DCM increased racial differences in mental health care received by increasing care utilization for White non-Hispanic young adults only. This finding also suggests that a pathway exists for insurance to improve mental health without impacting mental health care utilization.

Lastly, my third chapter explores the impact of access to cannabis for recreational purposes on mental health status. Recreational cannabis laws (RCL), which allow for the use of cannabis for recreational reasons, are heavily debated. Cannabis use, as well as a large set of other behaviors, can be influenced by RCL and in turn impact mental health. It is unclear if RCL impact mental health, and if so in what direction. I use data from the Behavioral Risk Factor Surveillance System to estimate the impact of RCL on a measure of mental health as well as a measure of activity limitation due to health reasons.

Results suggest that RCL may improve mental health for the general population, though estimates are only marginally significant. However, various subgroups are estimated to have larger and more significant impacts, including women, older adults, those with lower levels of education, and possibly Black non-Hispanic respondents. With respect to activity limitation, results suggest a reduction in limitations and are generally stronger than mental health measures alone, with similar subgroups showing the largest improvements. For both measures, these effects appear to be present across the spectrum of mental health and activity limitations, with improvements present even for the more severe cases. New robustness checks for difference-in-differences models lend support to these findings and showcase a benefit of RCL with respect to mental health.

Together, these three chapters attempt to advance knowledge around public policy and mental health. The estimates reported highlight benefits of four policies that were either previously unknown or under-estimated. Moving forward, additional research may focus on subsequent mechanisms or other details with respect to these policies. As governments continue to address concerns around the mental health crisis and debate divisive policies, this work may allow for a better understanding of actions that can be taken.

Chapter 1.

Minimum Wages, the Social Safety Net, and Mental Health Around Pregnancy

By

Bryce J. Stanley and Karen Smith Conway

1.1 Introduction

Depression is a major health condition that causes severe mental suffering and is a leading cause of disability worldwide (World Health Organization, 2021; Friedrich, 2017). For pregnant people and those that have recently given birth in particular, depression can pose destabilizing threats during a susceptible period of time. Both young adults and women are shown to have considerably higher rates of depression than other subgroups (National Institute for Mental Health, 2023). Likewise, depending on the measure, postpartum depression (PPD) has a prevalence rate of around 13 - 19 percent and is even higher for low-income people, making it a common but serious mental illness (O’Hara and McCabe, 2013; Goyal et. al., 2010; Dearing et. al., 2004). Yet, historically, perinatal depression has often gone untreated and lacked a sense of urgency from the medical field and policy makers alike.

Policy advocates have highlighted the severity of perinatal depression and called on further government intervention to address the issue (Vericker et. al., 2010; Chester, 2016). Several states have applied to extend public health insurance coverage postpartum, with the *Boston Globe* citing postpartum depression as a major reason to do so (Boston Globe Editorial Board, 2021). As pre-pregnancy depression is correlated with postpartum depression it is of additional concern both for its own harms but possible continuing effects.

Financial stress may create or exacerbate existing feelings of depression, carving a pathway for policies like the minimum wage and Earned Income Tax Credit (EITC) to be potential tools in fighting perinatal mental illnesses. State minimum wages and EITC levels have increased in recent years, and a growing body of research suggests that they have beneficial mental health impacts, especially for women (Horn, Maclean, and Strain, 2017; Kuroki, 2021; Dow et. al., 2020; Gangopadhyaya et al., 2021).

We build on this research by investigating if the beneficial effects of state minimum wages and EITC levels extend to the vulnerable period around pregnancy. While pregnancy and childbirth may reduce labor market participation, the potential effects of these labor market policies remain strong. Not only could they be affected through their partners' work experience, but we provide evidence showing that the majority of people work during pregnancy and the year of giving birth. We also investigate the effects of two programs that likely target this group specifically, the Temporary Assistance for Needy Families (TANF) and the Medicaid expansions that occurred as part of the Affordable Care Act (ACA). We further explore possible mechanisms such as financial related stress, health insurance status, care utilization, and birth outcomes. We find robust evidence that state minimum wages improve mental health before and after pregnancy and that EITC levels improve mental health before and during pregnancy. The effects of both policies seem likely driven by reductions in financial stress.

The paper proceeds as follows: Section 1.2 outlines the policy background and relevant previous research. Section 1.3 details our conceptual framework, while section 1.4 explains the data and methods used. Results are discussed in section 1.5 with concluding remarks given in section 1.6.

1.2 Background and Previous Work

In order to fully conceptualize the ways in which anti-poverty policies can impact perinatal mental health we must first understand the nature of perinatal mental health and the institutional structure of these policies. The following section reviews background on perinatal depression, relevant work done on Medicaid, TANF, EITC levels, and minimum wages followed by brief evidence that this population has connections to the labor market.

1.2.1 Perinatal Depression

Depression rates are considered higher for reproductive age women than the general population (National Institute for Mental Health, 2023). While the symptoms of postpartum depression are similar to that of major depressive disorder – a persistent lack of hope, interest, energy, and joy for example – it can stem from different origins associated with birth (postpartumdepression.org). People who have recently given birth are especially vulnerable as they recover from childbirth; they are at risk of developing PPD and also may have not dealt with mental health issues prior to becoming pregnant. Hospitalizations from PPD increased 34 percent from 2005 to 2012, reaching 36.7 hospital visits per 10,000 births in 2012, highlighting the growing danger of the illness (Franca and McManus, 2018). Depression before and during pregnancy can be equally threatening, posing similar symptoms and leading to a higher risk of developing PPD. Evidence suggest prenatal depression may be associated with preterm birth or low birth weight (Accortt et at., 2015; Conway and Kennedy, 2004).

The onset of postpartum mental illness can have substantial spillover effects, including lower quality childcare, a potentially large social cost (Field, 2009; O'Hara and McCabe, 2013). Efforts to combat perinatal mental illnesses include psychotherapies and medication, both of which have shown promising results (O'Hara, 2009; American Psychological Association, 2012).

1.2.2 Medicaid

As Medicaid results in a lower cost of health care, it has a direct pathway to improved mental health before and potentially during and after pregnancy. As the means tested public health insurance program, Medicaid eligibility levels have changed over time, with notable increases in the 1990's for pregnant people. These income eligibility levels not only differ by state but by parental status in additional to pregnancy status. Following the ACA, states had the option to increase income eligibility levels regardless of parental or pregnancy status, a policy that would primary effect people in our sample before pregnancy although woodwork effects are likely present.

Two papers consider the effects of Medicaid on perinatal mental health. Guldi and Hamersma (2021) find that the Medicaid expansion for pregnant women occurring in the 1990's improved maternal health outcomes including mental health. Likewise, recent research by Margerison et al. (2021) suggests that the ACA's Medicaid expansions led to a decrease in depression in the months leading up to pregnancy, as well as an increase in mental well-being postpartum. Our study extends this research by incorporating labor market policies that other studies have suggested impact mental health.

1.2.3 Temporary Assistance for Needy Families

Other federal and state level safety-net policies have also been examined by researchers. TANF has a direct route to impacting perinatal mental health as it is largely targeted at low-income people with children. While no research to date that we are aware of has focused on causal estimates of TANF specifically on mental health, studies have connected exogenous sources of income to improved mental health (Lindahl, 2005). Recent work by Schmidt et al. (2023) finds that higher safety-net levels, measured by a combination of EITC, TANF, and SNAP benefits, can reduce severe psychological distress for single mothers by 5.5 percent per $1,000 in combined benefits. Research has also suggested SNAP benefits alone can improve mental health outcomes for women (Munger, 2016). Additionally, many studies control for TANF when isolating the impact of minimum wages or EITC. These studies also typically control for changes in Medicaid policy.

1.2.4 Earned Income Tax Credit

The Earned Income Tax Credit is a federal program that provides income subsidies to low-income households. The tax credit is equal to a specific percentage of earnings, which increases with the number of children in the household up to three children, until a maximum amount is met and then phases out. The credit amounts for those without children is substantially smaller than those with children, making the program targeted at families. With this in mind, it is likely first-time parents and existing parents are impacted different from changes in EITC levels. Additionally, some states have an EITC "multiplier" that simply adds an additional percent on top of the federal credit. With this, there is variation in possible EITC levels by states, time, and family size.

Many economic studies on the EITC focus primarily on employment related outcomes, with a growing literature examining health and education effects. Single parents have been consistently shown to increase their labor supply in response to increases in EITC levels, resulting in a reduction in poverty rates, providing a potential pathway for mental health implications (See Hoynes, 2019 for full review).

Two previous studies show that the federal expansion of the EITC in the early 1990s resulted in improvements in mental health measures and subjective well-being for women (Evans and Garthwaite, 2014; Boyd-Swan et al., 2016). More recent work on state EITC payments confirms these effects and shows the effect is largely through income and employment effects, rather than changes in insurance status (Gangopadhyaya et al., 2021). State EITC levels are also found to reduce deaths by suicide (Dow et. al., 2020; Lenhart 2019).

1.2.5 Minimum Wage

The bulk of economic studies on minimum wages focus primarily on labor market outcomes, finding mixed results for the general population but adverse effects for teenagers and those with low education levels (see Neumark and Shirley, 2022 for full review). More recent work has started to examine the effects on health (see Leigh, 2021 for full review).

Focusing solely on those that consider mental health impacts, previous work suggests a beneficial relationship between minimum wages and mental well-being. Horn, Maclean, and Strain (2017) find state minimum wages are associated with decreases in "not good" mental health days among women, but not men, using the Behavioral Risk Factor Surveillance System. Kuroki (2021) extends on Horn et al. (2017), and suggests state minimum wages reduce extreme mental distress, as measured by reporting all of the past 30 days were spent with "not good" mental health. Deaths by suicide have also been shown to decrease with increases in state minimum wages (Dow et al., 2020). However, work looking specifically at young adults, 18-25 years old, finds no significant effect in mental health outcomes (Allegretto and Nadler, 2020).

Outside of the U.S., studies examining minimum wages in the U.K. also find a reduction in mental illness, citing stress relief and changes in health behaviors (Lehart, 2017; Reeves et al., 2017). Research has also found a reduction in financial stress, such as trouble paying bills, following increases in sub-minimum wages for people who have recently given birth, a potential pathway for mental health improvements (Andrea et al., 2020).

1.2.6 Potential Effects of Labor Market Policies During Pregnancy

To our knowledge, our study is the first to investigate the effects of labor market policies -- the minimum wage and EITC in particular -- on mental health around the time of pregnancy. However, for these policies to have a direct effect, either the person giving birth or a member of their household must be connected to the labor market in some way. Health impacts and additional time demands of pregnancy and childbirth may reduce labor supply and thus limit the possible impact on this group. Because our primary data source, the Pregnancy Risk Assessment Monitoring System, contains little information on labor market behavior, we turn to other data sources for evidence to explore the likely reach of these policies.

Using the Behavioral Risk Surveillance System (BRFSS) we find that during our sample period of 2012-2018, roughly 65 percent of pregnant people report being employed. Similarly, using data from the American Community Survey (ACS) we can identify the past year's employment for those who have given birth in the past year, giving us a measure of employment around the time of pregnancy and birth. Using this measure, we find 68 percent of those who gave birth in the past year were employed at some point in the past year, and 57 percent were employed at the time of interview. Additionally, 94 percent of spouses or partners with a child under one-year-old were employed at some point in the past year, and 90 percent were employed at the time of the interview[1].

These statistics therefore suggest that the majority of people, including those giving birth, are participating in the labor market in the time before, during or after pregnancy and childbirth. Moreover, to the extent that these policies affect labor market participation their potential impacts reach beyond those currently working. For example, if an increased minimum wage causes the person to become unemployed and thus be observed not working, the policy may still have impact on their mental health -- via their exit from the labor market. Additionally, households with more than one person, such as married couples, seem more likely to be impacted by these policies as they have two potential earners to be impacted by the minimum wage or EITC.

1.3 Conceptual Framework

To consider the possible pathways for these policies to impact mental health, we adopt a simplified framework in the spirit of the Grossman health production model (Grossman, 1972) where people maximize utility subject to their budget constraints with mental health as one input into their utility. We assume that today's mental health is a function of yesterday's mental health, investments in both mental health care and non-mental health care goods, and environmental

[1] Both the BRFSS and ACS include observations from all states and contain sampling weights that we use to construct these statistics. The BRFSS asks 1) if the person is pregnant, and 2) their employment status. The BRFSS does not provide information on how far along the pregnancy is. The ACS contains information on employment in the 12 months prior to the interview and if the person has given birth in the past 12 months.

factors that can impact mental well-being (such as work environment, home life, etc.). All four policies can impact this production function differently but primarily through the price of care, labor supply, and income effects.

1.3.1 Price of Care

Perhaps the most straightforward pathway policies to impact mental health is via Medicaid's ability to lower health care cost. The gaining of health insurance may increase health care utilization, including mental health care. This gives Medicaid expansion a direct mechanism to improve mental health around pregnancy. While employment and income implications are theoretically possible for Medicaid expansion, they are more likely for our other policies.

1.3.2 Time Usage and Labor Supply

The employment effects of these policies on perinatal depression are ambiguous both because the policy effects on employment are ambiguous and because the effects of employment on depression are ambiguous. In other words, we do not know the signage of $\frac{\partial \text{Mental Health}}{\partial \text{Policy}}$ as we do not know the signage of either $\frac{\partial \text{Mental Health}}{\partial \text{Employment}}$ or $\frac{\partial \text{Employment}}{\partial \text{Policy}}$.

The EITC provides both incentives and disincentives to increase labor supply depending upon whether the household is in the phase-in, plateau or phase-out range of the credit. Similarly increasing the minimum wage could lead to a decrease in employment if the individual is the marginal worker who gets laid off or has their hours reduced as a result. Conversely, to the extent that these individuals are now more relatively desirable workers (say, as compared to teenagers), their labor supply could remain constant or even increase. Lastly, TANF's tax structure may be seen as a disincentive for employment.

However, even if we could predict the effects on employment ($\frac{\partial \text{Employment}}{\partial \text{Policy}}$), the subsequent effects of that change in employment on perinatal depression is ambiguous ($\frac{\partial \text{Mental Health}}{\partial \text{Employment}}$). Because one's home and work environment may affect mental health, employment may either improve or degrade mental health depending on those environments. If a newly employed person enjoys the structure and social aspect of working, then we might expect improvements on measures of depression. However, stress from a workplace can also cause or exacerbate mental health issues. The movement to unemployment may also yield a similar story: with additional free time, people may engage in mental health boosting activities like hobbies or time with loved ones. Conversely, the stress, stigma, and other aspects associated with unemployment may increase depression (Zuelke et al., 2018). Thus, given the chain rule shown above, the impact of these policies on mental health through employment effects are ambiguous.

1.3.3 Income Effects

In our conceptual framework, income and measures of mental health also have an ambiguous relationship, much like the ambiguity in time usage. While the EITC increases income, minimum wages and TANF likely increase income for some and decrease for others. However, even with the EITC, we cannot be sure how this income will be used with respect to mental health implications. Additional money can be spent on goods that improve mental health (mental health care, food, housing), or potentially goods that harm mental health (drugs, alcohol). Increases in income may also reduce financial stress, which in turn may lead to improvements in mental health. With this in mind, minimum wage, TANF, and EITC may theoretically improve or harm measures of mental health through changes in income, depending on how the marginal dollar is spent.

1.4 Data and Methodology

We use data from the restricted access Pregnancy Risk Assessment Monitoring System (PRAMS) from 2012 to 2018 births, or phases 7 and 8. The PRAMS is a survey conducted by the Center for Disease Control typically sent out to people who have given birth within 2 to 4 months, with the goal of collecting data on conception, pregnancy, and birth related health outcomes. The PRAMS is the ideal dataset for investigating the relationship between anti-poverty policies and mental health before and after pregnancy as it contains measures of perinatal depression not available in other datasets and has ample sample size[2]. We limit our sample to those ages 18 to 45 and drop those with extreme values[3]. In our primary analyses, we focus on respondents with less than a 4-year college education because they seem the most likely to be affected by these policies. In robustness checks, we estimate the effects for other groups, including a college-educated group and a less-than-high school group that we expect to be less/more affected.

As the PRAMS is conducted at the state level, some states do not have data in some years. The PRAMS requires a response rate threshold to be met, while others are missing because the state did not conduct the survey which together limits some state year combinations. We use the

[2] Other options include datasets capturing the general public, not people who have recently given birth in particular, such as the National Institute on Health Survey.
[3] Specifically, those reporting more than ten dependents, or gestation periods less than six months or more than a year.

largest possible sample, which includes 33 states and 182 state-year combinations. We re-estimate our main models using a fully balanced sample with 16 states that include all 7 years of data and find qualitatively similar results[4]. The years and states of our PRAMS analyses are reported in the first two columns of Table 1.1.

1.4.1 Changes in the Safety Net

Identifying and isolating the empirical effects of these different policies requires substantial independent variation. Table 1.1 summarizes the years and states covered by the PRAMS data alongside the changes to these four policies and reveals considerable independent variation. Of the 33 states represented in the PRAMS, the minimum wage increased in 18 (often multiple times in a state), the state EITC multiplier increased in 9, and 25 expanded Medicaid eligibility at some point between 2012 and 2018. Figure 1.1 further depicts these changes and shows the geographic variation. Summary statistics, definitions and sources for the full set of state-level variables and individual characteristics are reported in Appendix Table 1.1.

1.4.2 Outcome Variables

The PRAMS provides two measures of depression that differ in timing and construction. The first is the respondent's self-reported depression in the three months prior to pregnancy. As reported in Table 1.2, pre-pregnancy depression has a prevalence rate of 14.1 percent for those without a 4-year college education. Depression prior to the pregnancy is an important outcome

[4] These results and all other results discussed but not reported are available upon request.

on its own as it measures mental health entering pregnancy. Additionally, it allows us to use a potentially key control when modeling postpartum depression.

The second is a measure of postpartum depression constructed from depressive-related symptoms reported at the time of the interview. Specifically, our PPD variable is captured with a binary measure based on a person's response to two depression-related screening questions, called a PHQ-2. In phase 7 and 8 of the PRAMS these two questions are “Since your new baby was born, how often have you felt down, depressed, or hopeless?” and “Since your new baby was born, how often have you had little interest or little pleasure in doing things you usually enjoyed?”. Each is presented with a five-point scale of answer choices containing: always, often, sometimes, rarely, and never. The typical threshold to qualify for depression is answering often or always to at least one of these questions. This process is common for PHQ-2 questions aimed at measuring depression in general, aside from postpartum. A binary flag for PPD is provided by the PRAMS, using this same method. Again, prevalence rates for those without a 4-year college degree is around 14 percent. We can also generate a measure of mental well-being from the PHQ-2, a flag for those least likely to have depression, following Margerson et al. (2021). A measure of mental well-being allows us to model those at both ends of the mental health spectrum.

The wording of the PHQ-2 questions is different in phase 6 of the PRAMS[5], which requires us to limit our sample to phases 7 and 8, or 2012-2018 births. Our main two outcome measures are thus 1) self-reported depression in pre-pregnancy, and 2) a PHQ-2 constructed measure for PPD. We caution that these two measures are not directly comparable. This

[5] Births prior to 2012.

difference likely explains why the prevalence of depression does not appear to increase in the postpartum period and instead remains around 14 percent in both periods.

There is a strong cross prevalence rates showing a high correlation between the two measures of depression. The bottom of Figure 1.2 shows how strong this correlation is, as the rate of PPD is almost 20 percentage points higher for those reporting depression before the pregnancy. Figure 1.2 shows prevalence rates for both PRAMS measures of depression based off of different subgroups. Differences across race show potentially important trends. White non-Hispanic respondents are more likely to report pre-pregnancy depression than Black non-Hispanic or Hispanic respondents. However, when looking at PPD Black non-Hispanic respondents have a higher prevalence rate than other groups. This suggest that race may play a role for the onset of PPD. To account for this, we report later models stratified by race and ethnicity.

To fill in the gap between the three months prior to pregnancy and the postpartum period, we conduct a supplementary analysis using the BRFSS. While the BRFSS does not identify those who have recently given birth, or ask questions about mental health before the pregnancy, it does identify people who are currently pregnant. Using the BRFSS therefore allows us to fill in the gap between the before conception and time of interview (postpartum) measures available in the PRAMS. The BRFSS measure of mental health is substantially different from the PRAMS, which limits the comparability between the two. However, its common use in resent research on mental health more broadly (e.g. Horn et al., 2017) facilitates comparison with existing work. In a supplemental analysis, we consider impacts to mental health during pregnancy using the BRFSS. The BRFSS asks how many out of the past 30 days has the person had "not good" mental health. We use this question to generate three measures of mental health. Following Horn et al. (2017), we first use the linear number of days reported. Next, to consider those most likely to have a mental illness or serious mental health issue, we create two binary measures with cut offs at those reporting at least 10 days and those reporting all 30 days, similar to Kuroki (2021). Table 1.2 shoes that these two cut-offs produce estimates that roughly match the percentage of Americans living with any mental illness and a serious mental illness respectively, which is estimated to be around 20 and 5 percent (National Alliance on Mental Health, 2023).

After investigating the effects of these policies on mental health around pregnancy, we explore possible mechanisms such as health insurance status, health outcomes and economic well-being. The bottom of Table 1.2 provides summary statistics and definitions for these possible mechanisms.

The PRAMS also contains several characteristics that could be related to depression and the other outcomes, such as respondent's age, marriage status, race and ethnicity, number of people dependent on household income, months between birth and interview, and education attainment. The BRFSS contains these same, or very similar, measures. Finally, we also control for the overall state environment by including the annual average state level unemployment rates, political party of the state governor, and per capita mental health care supply from County Business Patterns data. More information on control variables is reported in appendix table 1.1.

1.4.3 Identification Strategy

We estimate, via OLS[6], a generalized difference-in-differences model, shown in equation (1).

[6] Main results are robust to using probit rather than OLS.

$$(1)\ Y_{ist} = \beta_1 MinWage_{st} + \beta_2 EITCMax_{ist} + \beta_3 TANF_{st} + \beta_4 MedExp_{st} + \psi X_{ist} + \theta A_{st} + \delta_s + \tau_t + \varepsilon_{ist}$$

Where Y_{ist} is the outcome considered in each model for individual i in state s in year t. $\beta_1 MinWage_{st}$ is the minimum wage in dollars in the state[7], while $EITCMax_{ist}$ is the EITC amount receivable in their state-year combination for their family size, in thousands of dollars. As reported shortly, we also explore alternative forms of these variables, such as using logged or lagged values, the EITC multiplier (instead of dollar value) and the minimum wage relative to the state-year median wage[8]. $TANF_{st}$ is defined similarly to the EITC and is the maximum amount a family of 3 is eligible for in state s and year t. $MedExp_{st}$ is a dummy variable for if state s expanded Medicaid under the ACA at least 9 months before the respondent gave birth.

State and year fixed effects are included with δ_s and τ_t, while state and individual level controls are shown with A_{st} and X_{ist}. State controls include the political party of the state's governor, the state's unemployment rate in year t, and a measure of mental health care supply from the County Business Patterns. Individual level controls include month of conception or interview fixed effects (in line with the outcome), race, ethnicity, age, age squared, marital status, education, and time between birth and interview. Standard errors are clustered at the state level (Bertrand et al., 2004).

We limit our main analysis to those without a 4-year college education, defined as less than 16 years of schooling, to focus on the group of people most likely to be impacted by these anti-poverty policies. We conduct a number of event study analyses to verify that these models satisfy

[7] Unless the state's minimum wage is below the federal level, in which case we use the federal amount.
[8] We further explore if the effect of EITC differs when it is refundable and find no evidence. These results, and all others discussed and not reported, are available upon request.

the pre-trend assumption and also to investigate the dynamic effects -- i.e., whether the effects grow, diminish or are roughly constant as time passes. In addition to the complication of having multiple policies to investigate, these analyses must also be adapted because minimum wages and EITC 1) are continuous, and 2) change multiple times in some states during our sample period. To address these complications, we follow the empirical approach of Schmidheiny and Siegloch (2019) and conduct analyses both for each policy separately (controlling for the others) and then jointly[9].

1.5 Results

Both pre-pregnancy and postpartum depression results are summarized in the following sections. Next, we explore various robustness checks and conduct a supplemental analysis for mental health during pregnancy. We conclude the results section with a discussion of possible mechanisms.

1.5.1 Pre-pregnancy Depression

Table 1.3 reports pre-pregnancy depression results, using equation (1) and first including each policy separately and then including all at once. Our results suggest minimum wages and EITC levels are each associated with a reduction in depression before the pregnancy whether or not the other policies are controlled for. In contrast, neither TANF nor Medicaid is found to have

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[9] The Schmidheiny and Siegloch (2019) event study distributes lags and leads values for changes in independent variables. For example, a \$0.50 increase in the minimum wage in year t would be considered a value of 0.5 for time zero during year t, for t-1 in year t-1, t+1 during t+1 and so on. It also allowed for multiple changes within the sample period. See Schmidheiny and Siegloch 2019 for more details on the event study make up.

an effect. Medicaid expansions approach statistical significance when no other policies are considered, echoing the findings of Margerinson et al. (2021), but those effects are completely eliminated in the full model[10].

These results suggest a one dollar increase in the minimum wage – about the average increase during our sample -- is associated with a 1.55 percentage point decrease in the likelihood of reporting being depressed in the months before the pregnancy. With an average prevalence of 14 percent in our sample, this reduction represents a more than 10 percent decrease in pre-pregnancy depression. Similarly, a thousand dollar increase in the annual maximum EITC amount receivable is associated with a roughly 1.84 percentage point decrease in the likelihood of reporting being depressed in the months before the pregnancy. Back of the envelope comparisons suggest these two effects are similar in size for a part time minimum wage employee[11].

To verify the validity of these estimates, we perform event study analyses following Schmidheiny and Siegloch (2019) that are summarized for minimum wages and EITC levels in Figure 1.3a and 1.3b, respectively. For both policies, the pre-treatment estimates are centered around and not statistically different from zero, suggesting that the parallel pre-trends assumption is not violated. In unreported analyses, we find similar results from an event study that analyzes the pre-trends and dynamic effects of both policies at the same time. Besides satisfying the pre-trends assumption, these figures also reveal that the effects of each policy are

[10] Margerinson et al. (2021) does not control for EITC, minimum wage, or TANF levels. Our results therefore suggest that including labor market policies may prove important to estimating the impact of Medicaid expansion on mental health outcomes.
[11] Specifically, the estimated effects of the increased income from an additional $1000 in EITC is roughly equivalent to that of a one dollar increase in the hourly wage for those working (1.86/1.55)*1000 = 1200 hours. Put differently, the 1.55 refers to the effect of a $1 increase in the hourly wage, suggesting that the resulting $1000 increase in wage income from someone working 1000 hours is a little less than the 1.86 effect of the $1000 increase in maximum EITC received.

fairly stable in the years following the change. That is, the estimated coefficients for policies occurring 1, 2 or 3+ years in the past are of similar magnitudes and not statistically different from one another. These findings lend support to our primary specification and suggests that concerns raised by Goodman-Bacon (2019) and others are less of an issue here.

We also conduct a traditional event study for the Medicaid expansion policy to explore if our (lack of) findings are due to a violation of the pre-trends assumptions or dynamic effects. That analysis suggests a downward trend in the effect even before the expansion and so violates the parallel pre-trends assumption. However, such a downward trend should bias our results towards finding a negative (beneficial) impact of Medicaid on pre-pregnancy depression and so therefore does not seem likely responsible for the lack of effect we find.

Next, we subject our findings to several robustness checks and additional exercises, summarized in Table 1.4. We report the results for only the minimum wage and EITC coefficients because the Medicaid and TANF coefficients continue to be statistically insignificant. The first column in the top panel repeats the results for our main model (column 4 in table 1.3) for comparison. Our first exercise addresses the concern that by including the unemployment rate we are potentially shutting down a possible avenue for these policies to have an effect. For example, an increase in the minimum wage may lead to increased unemployment, but this effect would instead be captured by the state unemployment variable. However, the second column in the first panel shows that dropping the unemployment rate from the model has a negligible impact on the estimated effects.

The rest of this panel reports the estimated effects for different education groups, including those more and less likely to be affected. Those with less than a high school education have substantially larger effects, while those with a college education show no effect. These findings therefore lend support to a causal interpretation. Additionally, married households have at least two potential earners and thus may be expected to be more strongly affected by labor market policies than unmarried households. While the point estimates are fairly similar, the lower incidence of pre-pregnancy depression among this group suggests a substantially larger proportional impact of the EITC while the minimum wage's effect is similar[12].

The second panel explores stratifying the sample by parity and race/ethnicity. EITC payments are much larger for households with a child, which our specification takes account of by including the maximum amount for that specific household. However, it is important to confirm that first time parents are not driving our results, as their pre-birth EITC payments are substantially lower than those with previous children. While our estimated impact of EITC levels on pre-pregnancy depression for those with a previous birth is not precise, it is a similar magnitude to the impact of the main sample. Additionally, estimates for those without previous children are close to zero[13].

The rest of the second panel shows that while the estimated effects of these policies are negative for all racial/ethnic groups, minimum wages have the strongest effects for White non-Hispanic and Hispanic respondents and EITC has the strongest effects for Black non-Hispanic and Hispanic respondents. The lower incidence of pre-pregnancy depression among Hispanic respondents (as shown in Figure 1.2) suggests that these labor market policies may have an especially powerful effect for this group.

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[12] As shown in Figure 1.2, the incidence of pre-pregnancy depression is 11.4 percent for married people compared to 14.1 percent for the entire main sample. The estimated 2.1 percentage point decline due to the EITC is thus a 2.1/11.4 = 18.4 percent decrease compared to a 13 percent decrease for the entire main sample.

[13] Likewise, in unreported results with the ETIC multiplier the estimated impact is roughly nine times larger for those with a previous birth than those without.

The last panel in Table 1.4 explores alternative measures of both policies that are frequently used in past work. We first limit our sample to respondents ages 18 to 25, as previous work has argued this is the age group most likely to be impacted by the minimum wage. Our findings for both policies remain when examining this age group, which has a slightly larger prevalence rate. We next try using lagged and then logged measures of both minimum wages and EITC levels. Several EITC studies use the state EITC multiplier instead of the dollar amount (which we can tailor to the household's composition), and so we try using it instead. Finally, wage levels vary a great deal across the states and so the potential workers affected by a \$10 minimum wage, for example, may differ. We therefore also try redefining the minimum wage to be relative to the state's median wage in that year to capture these differences. All of these exercises yield negative and statistically significant effects of similar magnitudes[14], suggesting results are robust to policy specifications.

1.5.2 Postpartum Depression

We follow a similar approach and use the same model, with variables adjusted for the different timing of the outcome, in our empirical investigation of postpartum depression. Given the much higher prevalence of PPD for those with pre-pregnancy depression, our main estimates may be capturing the indirect effects on pre-pregnancy depression as well, a possibility we explore shortly. Table 1.5 follows Table 1.3's format of considering each policy separately and then when all are included.

[14] The EITC multiplier and relative minimum wage specifications are proportional values and therefore would be expected to yield much larger values of a similar size to the log specification.

The estimates of minimum wages and EITC are again similar in magnitude but are diminished in size compared to pre-pregnancy; in addition, only the minimum wage has a statistically significant effect. Our main sample results suggest a one dollar increase in the minimum wage is associated with a 0.6 percentage point decline in postpartum depression, about 40 percent of the effect of depression before the pregnancy. This smaller magnitude makes intuitive sense as postpartum depression is associated with more biological changes that may be less influenced by income. Surprisingly, the estimate for TANF is both positive and statistically significant when all policies are included. While this result is puzzling, removing TANF as a control does not change our main results for minimum wages and EITC levels. Additionally, modeling TANF as the maximum amount a family can receive given their number of dependents, as we do for EITC, removes this surprising finding and instead yields a null effect. The effect is also sensitive to the robustness checks performed and reported in Table 6.

As we did with pre-pregnancy depression, we next conduct event studies to explore the parallel pre-trends assumption and possible dynamic effects, reported in Figures 1.4a and 1.4b. We once again find no evidence that the parallel pre-trends assumption is violated; i.e., the effects prior to the policy are close to zero and statistically insignificant. For the minimum wage, we find the point estimates are strikingly similar in the years following the increase despite their imprecision, which again supports our primary specification. In line with the weak effects found for the EITC in Table 1.5, the estimated dynamic effects show much less stability. In the unreported event studies for Medicaid expansion, no coefficients are statistically different than zero, suggesting the pre-trends requirement is satisfied and we do not find evidence for dynamic effects.

Table 1.6 similarly reports results for different samples and model specifications. In panel one, we again drop the state unemployment rate as a control and then consider those with different levels of education[15]. Dropping the state unemployment rate once again has a negligible effect, alleviating any concern that our main model shuts down possible adverse effects of minimum wage increases. Likewise, estimating the effects for different education groups once again shows the strongest impact for the least educated, as expected. The effects of minimum wages now seem appear stronger for married respondents, especially in terms of proportional impact since the incidence of PPD is substantially lower for married people (Figure 1.2). It makes sense that minimum wages exert an even stronger effect on married people in the postpartum period given the decreased labor force participation of those who recently gave birth. That is, the effects via the partner's labor supply grows in importance. In the second panel, impacts by family parity and race/ethnicity are explored. Results suggest those with a previous birth are driving the minimum wage impacts. In contrast to pre-pregnancy results, the stronger effects previously found for Hispanic respondents are completely eliminated here. In unreported results, we also again verify that these results are not sensitive to how minimum wages and EITC are measured.

The bottom panel explores alternative specifications. The first column of the bottom panel reports results for respondents age 18-25, as in table 1.4. While the magnitude of the effect for minimum wage is larger for this subsample, it fails to reach statistical significance. Because the sample is reduced by nearly two thirds with this exercise, there is less statistical power expected.

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[15] While we do not report TANF results in table 1.6, we note that the estimate is positive and statistically significant for the main sample, White non-Hispanic respondents, when controlling for pre-pregnancy depression, and conditioning on not having pre-pregnancy depression.

Next, we explore alternative specifications that control for pre-pregnancy depression. Pre-pregnancy depression is likely endogenous and so these models that include or stratify by it should be viewed with caution. (Recall that PPD is measured differently, which precludes modeling the change in depression status.) Including pre-pregnancy depression as a control diminishes but does not eliminate the statistically significant effect of the minimum wage, suggesting that reduced pre-pregnancy depression is not the primary mechanism. Splitting the sample into those who do versus do not report pre-pregnancy depression suggests that the results are driven by those without prior depression. However, the much smaller sample size for those reporting depression, along with the aforementioned endogeneity, cautions against drawing a conclusion.

Lastly, we consider a different measure of postpartum mental health. Rather than model the probability of an observation showing strong signs of postpartum depression, we consider those without any sign. To examine the other end of the depression spectrum, we follow Margerison et al. (2021) by creating a dummy variable for those that answer "never" or "rarely" to both questions of the PHQ-2. Put differently, the PPD measure flags those most likely to have postpartum depression while the well-being measure flags the least likely. The last column of the third panel of table 1.6 shows results for postpartum well-being. Much like postpartum depression, we find higher minimum wages lead to improved mental well-being in the postpartum period. The postpartum depression and well-being results together imply that the minimum wage appears to help decrease depression for those showing the strongest signs as well as increase the likelihood of someone showing minimal signs. However, preliminary further investigations suggest the findings for postpartum wellness may not hold up to robust examination[16].

1.5.3 Supplemental Analysis of Mental Health During Pregnancy

Our main analysis investigates depression before pregnancy and after birth using the PRAMS. To fill the gap in between the two -- mental health during pregnancy -- we use a measure of mental health during pregnancy from the BRFSS. We investigate the number of days a person reports having "not good" mental health out of the past 30 days, and as discussed in section 1.4.2, the likelihood of reporting 10 or more days or all 30 days. We use 2012-18 data from all available states for respondents reporting they are pregnant at the time of the survey. We attempt to match our PRAMS models as closely as possible, including the same state policy and control variables, the same individual characteristics and year, state and month fixed effects.

Results using these three measures are summarized in Table 1.7 for two sets of models -- 1) including each policy individually, and 2) including all policies -- similar to Tables 1.3 and 1.5. For each of the three measures we model, the EITC maximum amount reduces the frequency of not good mental health days. While the BRFSS mental health measure is different from the PRAMS measures, the marginal impact of an additional 1,000 dollars in maximum EITC payments has a comparable effect relative to the mean compared to pre-pregnancy depression results[17]. However, none of the other policies, including the minimum wage, have a consistently

[16] Event study models for postpartum wellness suggest the parallel pre-trends assumption may be violated. Additional investigation into these findings is needed but beyond the scope of this study.
[17] Pre-pregnancy results suggest a 1,000 dollar increase in the maximum EITC payments is associated with a 13 percent decline in depression as where during pregnancy results suggest a 16 percent decline in reporting not good mental health all of the past 30 days.

statistically significant effect. The diminished effect of minimum wages seems likely due to the different time period (during pregnancy), the different mental health measure used or the vastly smaller sample size. Nonetheless, these supplementary analyses lend additional evidence for the potential effects of labor market policies on perinatal mental health.

1.5.4 Mechanisms

We now turn our attention to possible mechanisms through which the minimum wage and EITC can improve mental health during the time around pregnancy. We focus on four different possible pathways: financial stress, insurance status, health care received, and birth outcomes (recall Table 1.2).

For financial stress we make use of "stressors" questions asked by the PRAMS and utilized by previous research on minimum wages (Andrea et al., 2020). The first is if the respondent "had problems paying the rent, mortgage, or other bills" in the 12 months leading up to birth. The second asks if the respondent or their partner lost their job or had a cut in hours or pay in the same time period. In conjunction with one another, these two measures give us insight into the financial stability and stressors in the past year, a potential key mechanism in reducing depression.

Results for these outcomes, stratified by different education levels, are reported in Table 1.8. We find some evidence that minimum wages reduced problems paying bills in the 12 months before the birth for those with the lowest level of education. As a falsification test, it is reassuring to find no effect on those with a college education. No other policy, including the EITC, has a significant impact although Medicaid is suggestive for the lowest educated.

Moreover, the fact that none of these policies, especially the minimum wage, is associated with job loss or reduced pay/hours reinforces our earlier analyses that suggest the adverse employment effects were small (i.e., dropping the state unemployment rate had no effect). More generally, finding either a null or negative effect is consistent with reduced financial distress, since an increased minimum wage or EITC payment combined with no reduction in work or pay translates into increased household income. These results are suggestive of decreased financial stress or improved financial situations possibly playing a role in the decline in depression we find in previous models.

Ideally, we would have measures for mental health treatment received, as this may be a strong possible pathway. However, no measures exist in the PRAMS that is asked in a wide number of states, which precludes such an analysis. Instead, we investigate available but less direct measures, such as health insurance and other health care utilization. It is also possible that perinatal depression, especially PPD, could be affected by birth outcomes -- which in turn could be affected by safety net policies. We also investigate possible impacts on pregnancy intention because the decision to become or remain pregnant is another possible avenue for these policies to affect perinatal mental health.

Table 1.9 summarizes the results for these other mechanisms. The first column reveals that the EITC is associated with a reduced likelihood of the pregnancy being planned. This finding is consistent with at least two possible scenarios: 1) a generous EITC discourages planned fertility, perhaps in part because of the greater returns to working (especially among those already with children), and/or 2) people are more likely to proceed with an unplanned pregnancy. In either case, a higher EITC suggests a greater proportion of unintended pregnancies -- which seems likely to lead to poorer mental health postpartum. The extent to which the policy changes the composition of pregnancies observed therefore works against finding a beneficial effect of EITC on postpartum mental health and could explain the weaker results we find there.

The estimated effects of EITC on health insurance before, during and after pregnancy also hint at some negative selection as it is associated with a higher likelihood of being uninsured, especially before pregnancy. The effect of the minimum wage on being uninsured is either negative or statistically insignificant, lending weak support, at best, to health insurance as a mechanism. It is reassuring to see the negative and often statistically significant effects of the Medicaid expansions, with the largest effects before the pregnancy -- the time when the income limits increased. This finding is in line with past work that consistently finds that the ACA Medicaid expansions did indeed expand health insurance coverage. The bottom panel revealed that neither Medicaid nor the other policies considered have a beneficial effect on health care access likely to affect PPD. However, we do find EITC levels to have a beneficial effect on birth weight, a result also found by Hoynes et al. 2015.

1.6 Discussion

This research offers new evidence on the effects of a range of labor market and safety-net policies on mental health during a particularly impactful time -- before, during and after pregnancy. While a growing body of research suggests that minimum wages and the EITC have beneficial mental health effects on the broader population, our study is the first to our knowledge to focus on the period around pregnancy. We also consider more commonly studied policies targeting this group, namely the ACA Medicaid expansions and TANF.

Our primary analyses using the PRAMS provide robust evidence that minimum wages substantially reduce depression both before pregnancy and during the postpartum period. Our baseline estimates imply that a one dollar increase in the minimum wage lead to 10 and 4 percent reductions in pre-pregnancy and postpartum depression, respectively, for those without a 4-year college education. These estimates are within the general range suggested by past research for different interventions and samples (Schmidt et al., 2023, Horn et al., 2017). Some segments of the population experience even larger effects, such as less educated groups, married households, and Hispanic respondents. The main mechanism appears to be through the reduction of financial stress.

The evidence for the other policies is less clear, although the EITC appears promising. Its estimated effects on pre-pregnancy depression are similar to the minimum wage in its size, statistical significance and robustness. Supplementary evidence from the BRFSS suggests the EITC also improves mental health during pregnancy. However, while it again mirrors the minimum wage in the postpartum period, it fails to rise to statistical significance. The story is also less clear when it comes to possible mechanisms.

Somewhat surprisingly, neither Medicaid nor TANF – programs that target this population -- appear to affect perinatal mental health. Evidence from the PRAMS echoes the broader findings of the Medicaid expansions literature in finding a strong improvement in access to health insurance. Existing evidence from the PRAMS that the expansions improved pre-pregnancy depression (Margerinson et al., 2021) does not carry over here, in part due to controlling for other policies.

Our study therefore suggests that labor market policies are a promising approach to improving mental health before, during and after pregnancy, but several questions remain. Future research is needed to understand how these policies affect the labor market and overall experience of these vulnerable households. Why these policies appear to affect different racial/ethnic groups and whether and how minimum wages and the EITC might work in tandem is not known. Finally, the Covid-19 pandemic has likely changed the mental health landscape and the labor market environment, which suggest research in this area should be updated and extended as post-Covid data become available.

Figures and Tables

Figure 1.1. Changes in State Minimum Wages and EITC Multiplier, 2012-2018

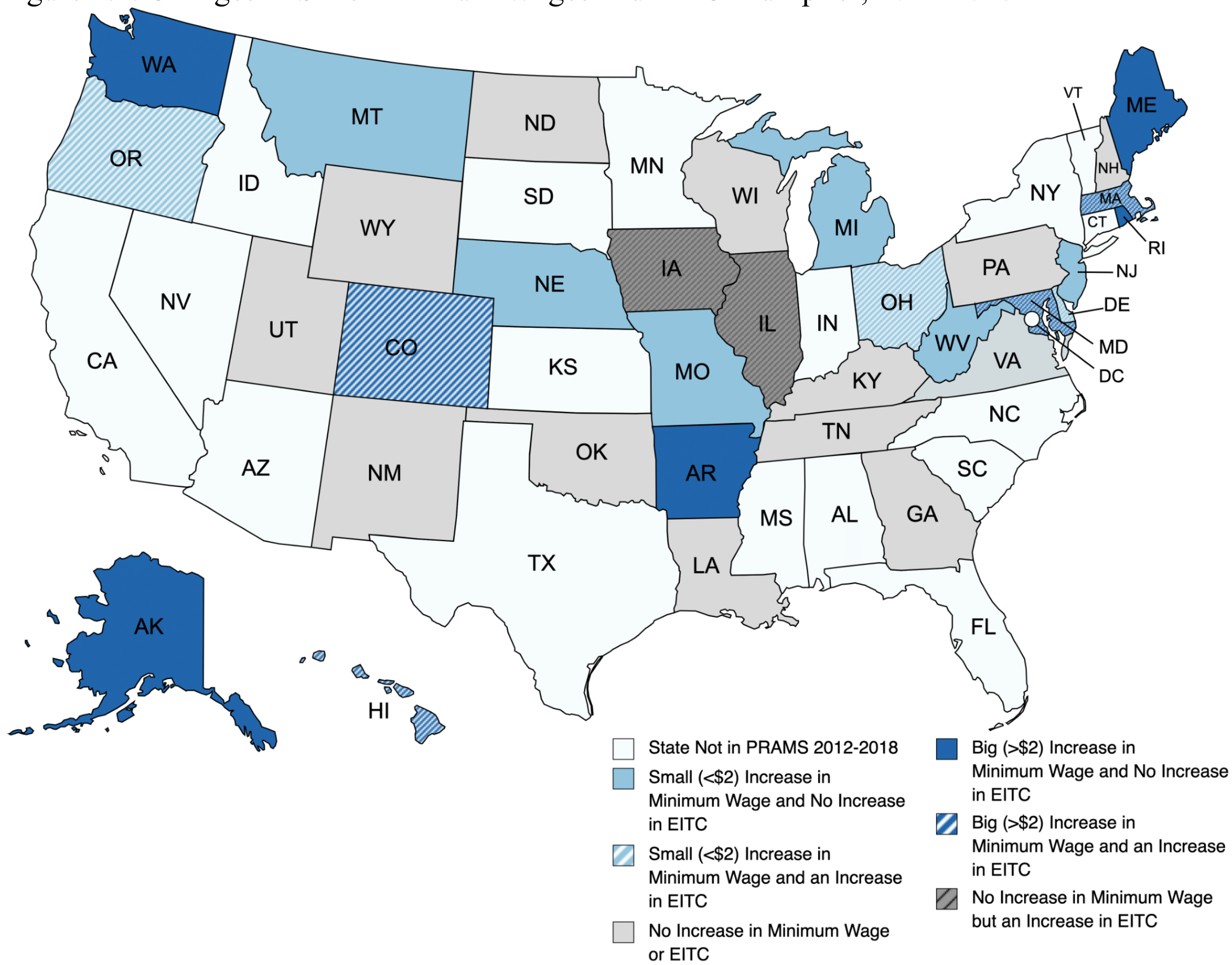
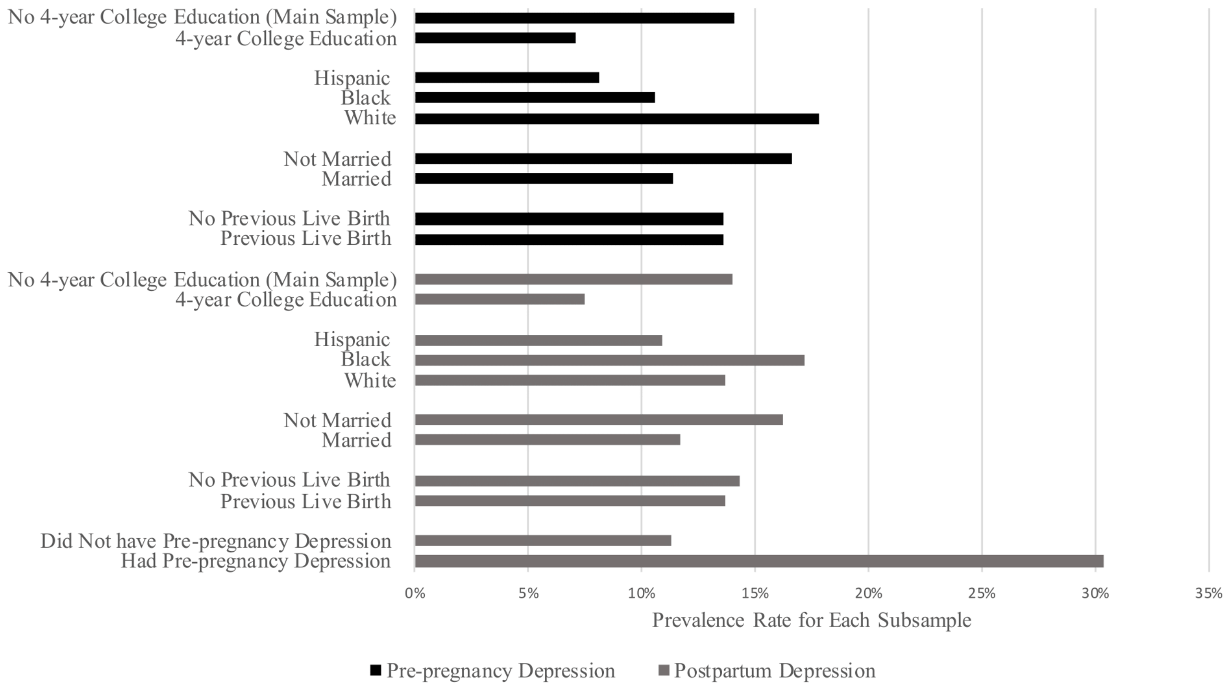
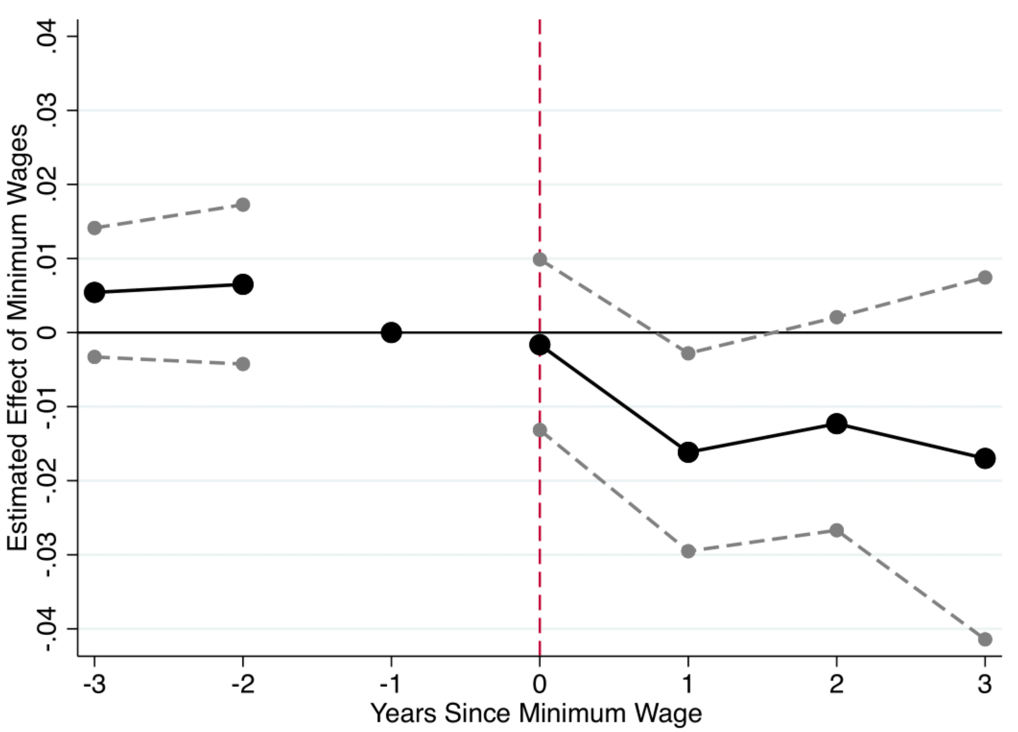


Figure 1.2. Prevalence Rates for Measures of Pre-pregnancy and Postpartum Depression by Subgroup



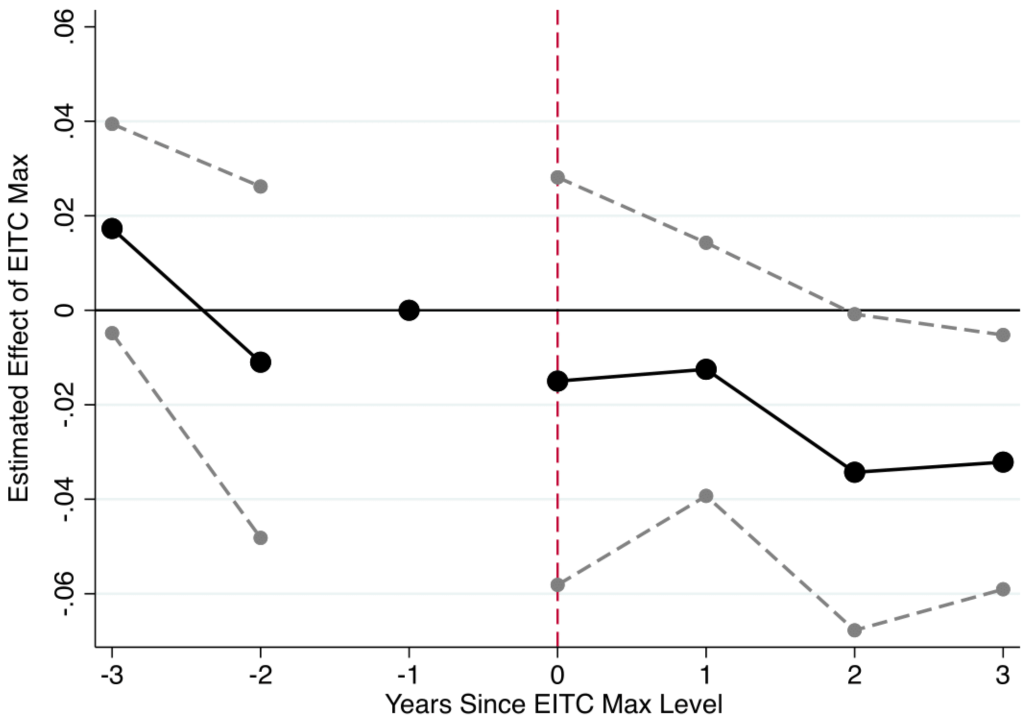
All rates are weighted using sampling weights using the PRAMS with 2012-2018 births. All subgroups are for respondents without a 4-year college education unless otherwise stated. Pre-pregnancy depression is self-reported while postpartum depression is via a PHQ-2. See table 2 for more details on measures.

Figure 1.3a. Event Study for Changes in Minimum Wages on Pre-pregnancy Depression for Main Sample Using PRAMS Data for 2012-2018 Births



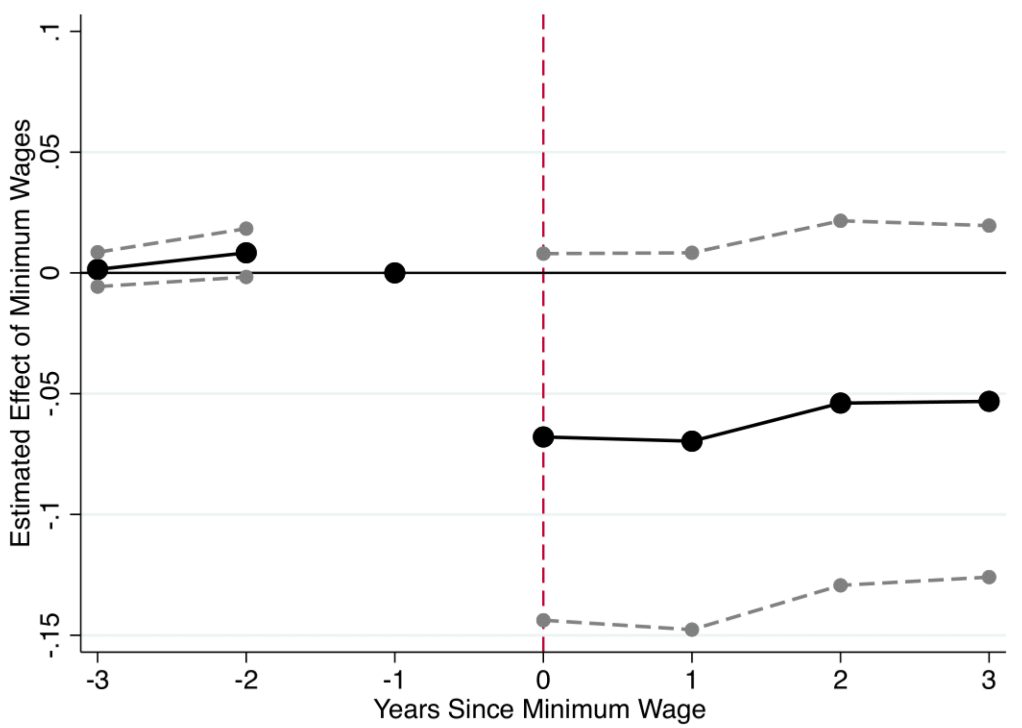
Event study model follows Schmidheiny and Siegloch 2019 for minimum wages while controlling for EITC Max levels, state and year fixed effects, month of conception fixed effects, survey phase, respondent's race, ethnicity, age, education, marital status, time between birth and interview, state mental health care supply, state unemployment rate, and political party of state governor. The gray dashed lines are the 95 percent confidence intervals. All observations are weighted using sample weights and standard errors are clustered at the state level using data from the PRAMS with 2012-2018 births. Policy variables are for year of conception.

Figure 1.3b. Event Study for Changes in EITC Max on Pre-pregnancy Depression for Main Sample Using PRAMS Data for 2012-2018 Births



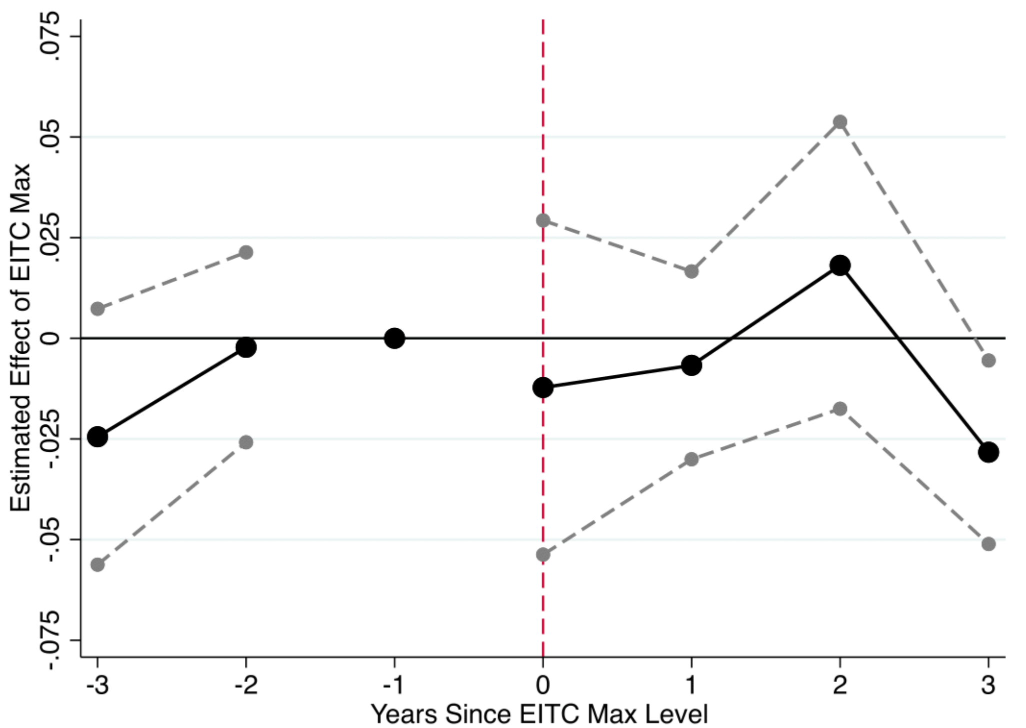
Event study model follows Schmidheiny and Siegloch 2019 for EITC Max levels while controlling for minimum wages, state and year fixed effects, month of conception fixed effects, survey phase, respondent's race, ethnicity, age, education, marital status, time between birth and interview, state mental health care supply, state unemployment rate, and political party of state governor. The gray dashed lines are the 95 percent confidence intervals. All observations are weighted using sample weights and standard errors are clustered at the state level using data from the PRAMS with 2012-2018 births. Policy variables are for year of conception.

Figure 1.4a. Event Study for Changes in Minimum Wages on Postpartum Depression for Main Sample Using PRAMS Data for 2012-2018 Births



Event study model follows Schmidheiny and Siegloch 2019 for minimum wages while controlling for EITC Max levels, state and year fixed effects, month of interview fixed effects, respondent's race, ethnicity, age, education, marital status, time between birth and interview, state mental health care supply, state unemployment rate, and political party of state governor. The gray dashed lines are the 95 percent confidence intervals and follow the corresponding point estimate in black solid lines. All observations are weighted using sample weights and standard errors are clustered at the state level. Policy variables are for year of birth.

Figure 1.4b. Event Study for Changes in EITC Max on Postpartum Depression for Main Sample Using PRAMS Data for 2012-2018 Births



Event study model follows Schmidheiny and Siegloch 2019 for EITC Max levels while controlling for minimum wages, state and year fixed effects, month of interview fixed effects, respondent's race, ethnicity, age, education, marital status, time between birth and interview, state mental health care supply, state unemployment rate, and political party of state governor. The gray dashed lines are the 95 percent confidence intervals and follow the corresponding point estimate in black solid lines. All observations are weighted using sample weights and standard errors are clustered at the state level. Policy variables are for year of birth.

Table 1.1 Summary of State Availability and Policy Variation

States	Years in PRAMS	Change in Min Wage	Change in EITC Multiplier
AK	2012-2018	$2.09	
AR	2012-13, 2015-16	$2.25	
CO	2012-13, 2015-18	$2.56	0.10
DE	2012-2018	$1.00	a
GA	2012-13, 2017-18		
HI	2012-2016	$2.85	0.2
IL	2012-2018		0.13
IA	2012-2017		0.08
KY	2017-2018		
LA	2015-2018		a
ME	2012-2018	$2.50	a
MD	2012-2017	$2.85	0.03
MA	2012-2018	$3.00	0.08
MI	2012-13, 2015-18	$1.85	a
MO	2012-2018	$0.60	
MT	2017	$0.65	
NE	2012-2018	$1.75	a
NH	2013-2017		
NJ	2012-2018	$1.35	0.17
NM	2012-2018		a
ND	2017-2018		
OH	2012, 2014-15	$0.60	0.1
OK	2012-2017		
OR	2012-13, 2015	$1.95	0.02
PA	2012-2018		
RI	2012-2018	$2.70	a
TN	2012-2015		
UT	2012-2018		
VA	2015-2018		a
WA	2012-2018	$2.46	
WV	2012-2018	$1.50	
WI	2012-2018		a
WY	2012-2018		
Average Change		$1.05	0.03
Average in 2018		$8.50	0.08

Max in 2018	$11.50	0.37

States with a minimum wage below the federal level are considered to have the federal level of $7.25. Superscript a denotes states with an EITC multiplier but did not change the multiplier level during our sample period.

Table 1.2. Summary Statistics of Respondents Without a 4-year College Education

	Mean (std.)	Definition
PRAMS Mental Health Measures		
Pre-pregnancy Depression	0.141	Equal to one if respondent checked "Depression" to “During the 3 months before you got pregnant with your new baby, did you have any of the following health conditions?” for phase 8 or “Before you got pregnant with your new baby, did a doctor, nurse, or any other health care worker tell you that you had any of the following health conditions” for phase 7.
	(0.348)	
Postpartum Depression	0.140	Equal to one if respondent answered "Always" or "Often" to either of “Since your new baby was born, how often have you felt down, depressed, or hopeless?” and “Since your new baby was born, how often have you had little interest or little pleasure in doing things you usually enjoyed?
	(0.347)	
Postpartum Well-being	0.599	Equal to one if respondent answered "Never" or "Rarely" for both of “Since your new baby was born, how often have you felt down, depressed, or hopeless?” and “Since your new baby was born, how often have you had little interest or little pleasure in doing things you usually enjoyed?”
	(0.490)	
BRFSS Mental Health Measure During Pregnacy		
Number of Days with "Not Good" Mental Health	4.976	Persons response to "Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?"
	(11.80)	
10 or More "Not Good" Mental Health Day	0.163	Equal to one if number of days with "Not Good" mental health out of past 30 days is 10 or more.
	(0.370)	
All 30 "Not Good" Mental Health Day	0.059	Equal to one if number of days with "Not Good" mental health out of past 30 days is 30.
	(0.235)	
PRAMS Possible Mechanisms		
Problems Paying Bills	0.236	Equal to one if respondent answered "Yes" to this happening in the past 12 months: "You had problems paying the rent, mortgage, or other bills."
	(0.425)	
Lost Job, Hours, or Pay	0.321	Equal to one if respondent answered "Yes" to any of the following happening in the past 12 months: "My husband or partner lost his job", "I lost my job even though I wanted to go on working", and "My husband, partner, or I had a cut in work hours or pay" as questions.
	(0.467)	
Uninsurance	0.212	Equal to one if respondent answered, "I did not have any health insurance during the month before I got pregnant" to "During the month before you got pregnant with your new baby, what kind of health insurance did you have?". Similar questions exist for during the pregnancy and at the date of interview.
	(0.409)	
Planned Pregnancy	0.436	Equal to one if respondent answered "Yes" to "When you got pregnant with your new baby, were you trying to get pregnant?"
	(0.496)	
Preterm Birth	0.108	Equal to one if respondent reported a gestation period of less than 259 days.
	(0.311)	
Low Birth Weight	0.072	Equal to one if baby was born at less than 2375 grams (necessitated by measure being in 250-gram brackets).

	(0.259)	
Postpartum Checkup	0.879	Equal to one if respondent answered "yes"" to "Since your new baby was born, have you had a postpartum checkup for yourself?"
	(0.326)	
Talked to HCW about PPD	0.760	Equal to one if respondent answered "Yes" to "Postpartum Depression" when asked "Since your new baby was born, did a doctor, nurse, or other health care worker talk with you about any of the things listed below?:"
	(0.427)	

Table 1.3. Marginal Effects of Safety-Net Policies on Pre-pregnancy Depression					
	Min Wage Only	EITC Only	TANF Only	Med Exp Only	All Included
Min Wage	-0.0166***				-0.0155***
	(0.0052)				(0.0049)
EITC Max		-0.0200***			-0.0184**
		(0.0070)			(0.0070)
TANF			-0.0975		-0.0602
			(0.1170)		(0.1044)
Med Exp				-0.0095	0.0002
				(0.0066)	(0.0058)
N	101,045	101,045	101,045	101,045	101,045
Sample Mean	0.141	0.141	0.141	0.141	0.141

All models control for state and year fixed effects, month of conception fixed effects, respondent's race, ethnicity, age, education, marital status, time between birth and interview, survey phase, state mental health care supply, state unemployment rate, and political party of state governor. All observations are weighted using sample weights and standard errors clustered at the state level are in parenthesis. Policy variables are for year of conception. EITC Max and TANF are in thousands of dollars. All models are for observations without a 4-year college education using PRAMS data for 2012-2018 births. *** p<$0.01, ** p<$0.05, * p<$0.1.

Table 1.4. Alternative Samples and Specifications for Pre-pregnancy Depression

	Main Sample	Drop Unemp Rate	College Plus	Less than HS	Married
Min Wage	-0.0155***	-0.0154***	-0.0000	-0.0223**	-0.0111***
	(0.0049)	(0.0049)	(0.0032)	(0.0085)	(0.0050)
EITC Max	-0.0184**	-0.0184***	0.0081	-0.0443*	-0.0210**
	(0.0070)	(0.066)	(0.0064)	(0.0259)	(0.0079)
N	108,260	108,260	55,615	19,273	48,962
Sample Mean	0.141	0.141	0.071	0.153	0.114
	Previous Birth	No Previous Birth	White Non-Hisp	Black Non-Hisp	Hispanic
Min Wage	-0.0270***	-0.0312***	-0.0164*	-0.0020	-0.0166**
	(0.0061)	(0.0068)	(0.0086)	(0.0091)	(0.0067)
EITC Max	-0.0168	-0.0003	-0.0111	-0.0176**	-0.0266**
	(0.0162)	(0.0097)	(0.0113)	(0.0077)	(0.0111)
N	50,910	40,819	50,102	22,158	19,965
Sample Mean	0.136	0.143	0.163	0.105	0.081
	18-25 Year-olds	Lagged Measures	Logged Measures	EITC Multiplier	Relative Min Wage
Min Wage	-0.0194**	-0.0119***	-0.1283***	-0.0167***	-0.2046**
	(0.0083)	(0.0041)	(0.0425)	(0.0044)	(0.0853)
EITC Max	-0.0226*	-0.0184**	-0.1646**	-0.1453***	-0.0192**
	(0.0113)	(0.0091)	(0.0647)	(0.0524)	(0.0071)
N	39,901	108,260	108,260	108,260	108,260
Sample Mean	0.154	0.141	0.141	0.141	0.141

All models control for state and year fixed effects, month of conception fixed effects, respondent's race, ethnicity, age, education, marital status, time between birth and interview, survey phase, state mental health care supply, state unemployment rate, political party of state governor, Medicaid expansion, and TANF levels. All observations are weighted using sample weights and standard errors clustered at the state level are in parenthesis. Policy variables are for year of conception. EITC Max is in thousands of dollars. All models are for main sample, observations without a 4-year college education, unless otherwise stated, and using PRAMS data for 2012-2018 births. *** p<\$0.01, ** p<\$0.05, * p<\$0.1.

Table 1.5. Marginal Effects of Safety-Net Policies on Postpartum Depression					
	Min Wage Only	EITC Only	TANF Only	Med Exp Only	All Included
Min Wage	-0.0059*				-0.0060**
	(0.0031)				(0.0025)
EITC Max		-0.0059			-0.0053
		(0.0080)			(0.0075)
TANF			0.0889		0.1524**
			(0.0675)		(0.0563)
Med Exp				-0.0064	-0.0042
				(0.0058)	(0.0054)
N	109,845	109,845	109,845	109,845	109,845
Sample Mean	0.140	0.140	0.140	0.140	0.140

All models control for state and year fixed effects, month of interview fixed effects, respondent's race, ethnicity, age, education, time between birth and interview, state mental health care supply, state unemployment rate, time between birth and interview and political party of state governor. All observations are weighted using sample weights and standard errors clustered at the state level are in parenthesis. Policy variables are for year of conception. EITC Max and TANF are in thousands of dollars. All models are for observations without a 4-year college education, using PRAMS data for 2012-2018 births. *** p<\$0.01, ** p<\$0.05, * p<\$0.1.

Table 1.6. Alternative Samples and Specifications for Postpartum Depression					
	Main Sample	Drop Unemp Rate	College Plus	Less than HS	Married
Min Wage	-0.0060**	-0.0060**	-0.0030	-0.0169**	-0.0069*
	(0.0025)	(0.0027)	(0.0023)	(0.0073)	(0.0032)
EITC Max	-0.0053	-0.0030	-0.0006	-0.0159	0.0044
	(0.0075)	(0.0072)	(0.0064)	(0.0192)	(0.0090)
N	109,845	109,845	56,150	19,674	52,395
Sample Mean	0.140	0.140	0.075	0.156	0.093
	Previous Birth	No Previous Birth	White Non-Hisp	Black Non-Hisp	Hispanic
Min Wage	-0.0111***	-0.0039	-0.0090***	-0.0015	-0.0000
	(0.0040)	(0.0098)	(0.0029)	(0.0084)	(0.0032)
EITC Max	0.0027	-0.0084	-0.0120	-0.0115	0.0065
	(0.0153)	(0.0107)	(0.0159)	(0.0100)	(0.0074)
N	51,745	40,897	50,648	22,484	20,348
Sample Mean	0.137	0.142	0.137	0.172	0.109
	18-25 Year-olds	Controlling for DB	Had DB	Did Not Have DB	Postpartum Well-being
Min Wage	-0.0096	-0.0042*	0.0032	-0.0050*	0.0960*
	(0.0070)	(0.0024)	(0.0122)	(0.0028)	(0.0051)
EITC Max	0.0004	-0.0031	-0.0346	0.0018	0.0029
	(0.0103)	(0.0073)	(0.0350)	(0.0053)	(0.0111)
N	40,416	108,250	16,236	92,024	109,845
Sample Mean	0.168	0.140	0.304	0.113	0.599

All models control for state and year fixed effects, month of conception fixed effects, respondent's race, ethnicity, age, education, time between birth and interview, state mental health care supply, state unemployment rate, Medicaid expansion, TANF levels, and political party of state governor. All observations are weighted using sample weights and standard errors clustered at the state level are in parenthesis. Policy variables are for year of birth. EITC Max in is thousands of dollars. All models are for main sample, observations without a 4-year college education, unless otherwise stated, using PRAMS data for 2012-2018 births. *** p<\$0.01, ** p<\$0.05, * p<\$0.1.

Table 1.7. Marginal Effects of Safety-Net Policies on BRFSS "Not Good" Mental Health Days During Pregnancy						
	Number of Days		10+ Days		All 30 Days	
	Included Separately	All included	Included Separately	All included	Included Separately	All included
Min Wage	0.0330	0.2146	-0.0181*	-0.0056	-0.0081	-0.0013
	(0.2800)	(0.2808)	(0.0106)	(0.0102)	(0.0059)	(0.0069)
EITC Max	-0.5912***	-0.6360***	-0.0237***	-0.0220***	-0.0103***	-0.0094***
	(0.1543)	(0.1676)	(0.0058)	(0.0061)	(0.0031)	(0.0033)
TANF	-5.9710	-1.986	-0.3217	-0.0172	-0.1766	-0.0387
	(7.5933)	(6.6951)	(0.2287)	(0.1831)	(0.1699)	(0.1571)
Med Exp	0.4229	0.4614	-0.0276	-0.0154	-0.0169	-0.0122
	(0.8305)	(0.8348)	(0.0247)	(0.0268)	(0.0131)	(0.0147)
N	10,293	10,452	10,452	10,452	10,452	10,452
Sample Mean	4.948	4.948	0.163	0.163	0.059	0.059

All models control for state and year fixed effects, month of interview fixed effects, respondent's race, ethnicity, age, education, time between birth and interview, state mental health care supply, state unemployment rate, and political party of state governor. All observations are weighted using sample weights and standard errors clustered at the state level are in parenthesis. Policy variables are for year of interview. EITC Max in is thousands of dollars. All models are for pregnant observations without a 4-year college education, using BRFSS data for 2012-2018 data. *** p<$0.01, ** p<$0.05, * p<$0.1.

Table 1.8. Marginal Effects of Safety-Net Policies on Financial Stress Outcomes

	Problems Paying Bills			Lost Job, Pay, or Hours		
	Main Sample	Less than HS	College Plus	Main Sample	Less than HS	College Plus
Min Wage	-0.0053	-0.0183**	-0.0012	-0.0196**	0.0059	-0.0007
	(0.0051)	(0.0087)	(0.0026)	(0.0074)	(0.0136)	(0.0044)
EITC Max	-0.0045	0.0144	-0.0079	-0.0136	-0.0438	0.0082
	(0.0138)	(0.0240)	(0.0128)	(0.0099)	(0.0338)	(0.0101)
TANF	0.0366	-0.0224	0.0866	-0.1323	0.0734	0.1402
	(0.1032)	(0.2068)	(0.1567)	(0.2805)	(0.4596)	(0.1348)
Med Exp	-0.0155	-0.0277	0.0111	-0.0214	-0.0765**	-0.0018
	(0.0117)	(0.0315)	(0.0089)	(0.0181)	(0.0306)	(0.0173)
N	97,086	17,239	48,544	94,862	16,672	47,962
Sample Mean	0.236	0.221	0.080	0.321	0.321	0.156

All models control for state and year fixed effects, month of conception fixed effects, respondent's race, ethnicity, age, education, time between birth and interview, state mental health care supply, state unemployment rate, Medicaid expansion, TANF levels, and political party of state governor. All observations are weighted using sample weights and standard errors clustered at the state level are in parenthesis. EITC Max is in thousands of dollars. Policy variables are for year of conception. All models use PRAMS data for 2012-2018 births *** p<$0.01, ** p<$0.05, * p<$0.1.

Table 1.9. Marginal Effects of Safety-Net Policies on Uninsurance Status, Pregnancy Intention, Utilization, and Birth Outcomes				
1. Intention and Uninsurance				
	Pregnancy Intention	Uninsurance		
	Planned Pregnancy	Before Pregnancy	During Pregnancy	Date of Interview
Min Wage	0.0065	0.0099	-0.0046**	-0.0018
	(0.0065)	(0.0089)	(0.0018)	(0.0087)
EITC Max	-0.0261**	0.0424***	0.0010	0.0183
	(0.0119)	(0.0129)	(0.0091)	(0.0124)
TANF	0.0000	0.0534	-0.0318	0.1670
	(0.0001)	(0.1179)	(0.0403)	(0.1419)
Med Exp	-0.0014	-0.0496***	-0.0128***	-0.0130
	(0.0055)	(0.0151)	(0.0041)	(0.0204)
N	109,764	109,736	95,584	109,670
Sample Mean	0.436	0.212	0.042	0.159
2. Care and Outcomes	Care Utilization		Birth Outcomes	
	Postpartum Checkup	Talked to a HCW about PPD	Low Birth Weight	Preterm Birth
Min Wage	0.0023	-0.0079	-0.0024	0.0021
	(0.0029)	(0.0130)	(0.0020)	(0.0025)
EITC Max	0.0056	0.0101	-0.0108**	-0.0020
	(0.0061)	(0.0115)	0.0050	(0.0048)
TANF	0.0896	-0.1307	0.0648	-0.0925
	(0.0649)	(0.4036)	(0.0438)	(0.0552)
Med Exp	-0.0050	-0.0165*	0.0038	-0.0005
	(0.0071)	(0.0091)	(0.0044)	(0.0068)
N	109,459	71,781	102,573	109,845
Sample Mean	0.879	0.760	0.072	0.101

All models control for state and year fixed effects, month of conception fixed effects, demographics, time between birth and interview, mental health care supply, unemployment rate, and political party of state governor. All observations are weighted using sample weights and standard errors clustered at the state level are in parenthesis. EITC Max is in thousands of dollars. Policy variables are for year of birth. All observations are birth givers without a 4-year college education. *** p<$0.01, ** p<$0.05, * p<$0.1.

Chapter 2

The Dependent Coverage Mandate and Mental Health by Race, Ethnicity, and Gender

By

Bryce J. Stanley

2.1 Introduction

Unlike many health conditions, young adults have higher rates of mental illness than older adults. Americans age 18-25 have the highest prevalence rate of mental illness out of any age group in the United States, as of 2021 (National Alliance on Mental Health, 2023). Conversely, and alarmingly, they also seek the lowest amount of mental health treatment, conditional upon having a mental illness (National Institute of Mental Health, 2023a). In 2021, over 30 percent of 18-25-year old's had any mental illness, with 11.4 percent having a serious mental illness. Yet, only roughly 44 and 57 percent of them received any mental health treatment, respectively (National Institute of Mental Health, 2023a). Lack of care is especially high for the uninsured (Garfield et al., 2011).

Barriers to care are particularly concerning as treatment of several forms has been found to be effective for various mental health issues (Cipriani et. al. 2018; National Institute of Mental Health, 2023b; Tihonen 2016). Treatment for a wide variety of mental illnesses offers potentially life changing effects, yet many remain without care. High cost is often cited as a key barrier in receiving treatment, implying insurance coverage may have a unique ability to increase care received (Alang, 2015). Expanding insurance coverage to young adults may allow for greater utilization of mental health care that can improve mental health outcomes.

Recent health insurance legislation has specifically targeted young adults. The Affordable Care Act (ACA) was passed in 2010 representing a major intervention into the U.S. health insurance market with one aspect impacting young adults directly. With a goal of reducing uninsurance rates, the ACA had several prongs changing the structure of health insurance markets. Subsidies for non-group markets and expansions to Medicaid eligibility were key tools used to insure more people. The Dependent Coverage Mandate (DCM) was included to increase the insurance rate among young adults. The DCM requires private insurance plans to allow dependents on plans up to age 26, rather than 18 as was common prior to the ACA. For those 18-25 years old, this mandate offers a pathway to insurance previously not present. Using variation over time and across age groups, I model the impact of the DCM on various measures of mental health and explore potential mechanisms in two forms of mental health treatment with particular attention paid to differential impacts by race, ethnicity, and gender. Results reported here suggest previous research overlooked mental health benefits for Black non-Hispanic young adults and that this improvement is likely not driven by an increase in care.

The paper proceeds as follows: Section 2.2 reviews the background and relevant literature on both mental health care treatment and the DCM. A conceptual framework for mental health and insurance is presented in section 2.3. I overview data in section 2.4. Sections 2.5 and 2.6 discuss the empirical strategy used and the results found. Closing remarks and discussion are made in section 2.7.

2.2. Background and Previous Work

2.2.1 Prior Policy Landscape

Prior to the passing of the ACA, roughly one-third of young adults were uninsured (Antwi et al., 2013). The typical private insurance plan had dependents age out at 18 years old. While some states had laws extending coverage to a higher age, these laws often had additional criteria that limited the bite of the policies. State dependent coverage laws have been shown to increase coverage by 1-2 percentage points (Levine et al., 2011; Depew, 2015; Gamino, 2018) though some research suggests much of the gains were offset by crowding out of own name coverage (Monheit et al., 2011).

The DCM was enacted nationwide in September of 2010 and mandated that all private insurance plans extend coverage for dependents up to age 26. The DCM has been well-studied by health economists and public health researchers alike, most often comparing those just below age 26 to those just over, before and after the enactment of the DCM. Research suggests the DCM lead to an increase in insurance coverage by around 3-7 percentage points (Antwi et al., 2013; Barbaresco et al., 2015; Chua and Sommers, 2014: O'hara and Broult, 2013; Sommers and Kronick, 2012; Shane and Ayyagari, 2014).

2.2.2 Previous Work on DCM

In addition to health insurance coverage, several other outcomes and behaviors have been studied in relation to the DCM with many of these outcomes offering possible pathways to impact mental health. Out-of-pocket healthcare spending has been found to decrease following the DCM driven by a decreased likelihood in very high expenditures (Chua and Sommers, 2014; Ali et al., 2016; Busch et al., 2014; Fone et al., 2020). Some research suggests that the DCM increased the likelihood of having a usual source of care (Kotagal et al., 2014, Wallace and Sommers, 2015), as well as increased the number of visits to a physician per year (Jhamb et al., 2015).

Most relevant for this work are studies on the DCM's impact on mental health care utilization and mental health outcomes. One study suggests the likelihood of receiving any mental health care treatment increased following the DCM for those most likely to be in need of treatment (Saloner and Lé Cook, 2014). Two previous studies use data from the Medical Expenditure Panel Survey (MEPS) and find evidence of improvements in the mental health composite score, a general measure of mental health (Burns and Wolfe, 2016; Shane and Wehby, 2018). However, these studies do not find any change in other measures of more serious mental illness available in the MEPS. In a similar direction, Chua and Sommers (2014) find the DCM increased the likelihood of young adults reporting having "excellent" mental health, though no other measures of mental health are shown to be impacted. Together, these previous findings provide evidence of some mental health improvements, but for only measures of general or excellent mental health rather than severe conditions. Additionally, previous work on the DCM and mental health does not explore differential impacts by important demographics that could influence the DCM's ability to impact mental health.

2.2.3 Demographics and Mental Health

Racial and gender differences exist with respect to both mental health and mental health care utilization in ways that could be meaningful for the DCM. For example, evidence generally suggests Black Americans report higher rates of symptoms of various mental illnesses than White Americans. Estimates suggest Black Americans have higher rates of depression than White Americans (Dunlop et al., 2003; Ettman et al., 2020). Additionally, evidence of higher rates of post-traumatic stress disorder exist (Asnaani et al., 2010)[18]. Likewise, women also report higher rates of several mental illnesses.

[18] Other estimates using different data and different criteria often suggest White Americans exhibit higher rates of anxiety (Terlizzi and Villarroel, 2020) or lifetime risk of mental illnesses (Alvarez et al., 2018.) Results from data

In conjunction with these demographic differences, access to mental health care varies across racial and ethnic groups in the U.S.. Racial and ethnic minorities utilize much less mental health care than White Americans (Substance Abuse and Mental Health Services Administration, 2015; McGuire and Miranda, 2008) and are more likely to receive poor quality care when they do access care (U.S. Department of Health and Human Services, 2001). Lack of access to mental health care providers that share racial or ethnic identity with the individual seeking care is often cited as a possible reason for this gap in care, as well as other factors such as insurance status (Shao et al., 2016).

These differences, in both prevalence rates and utilization of care, may influence the ability of the DCM to impact mental health outcomes. Due to the higher rates of mental illnesses for both women and Black non-Hispanic people, there may be a higher potential for insurance to improve outcomes in these populations. Likewise, provider shortages, particularly providers of color, may limit the ability of the DCM to increase mental health care usage among racial and ethnic minority groups.

However, previous research on the DCM and mental health outcomes has largely ignored these differences and how they may influence the impact of the DCM. Given the differences in access to care, rates of mental illness, and social factors that could impact mental health, it is possible the DCM impacted young adults differently by race and ethnicity as well as gender. Likewise, the DCM may also have differential impacts on insurance coverage which could alone lead to different impacts on mental health. This study differs from previous work by allowing for differential effects by race, ethnicity, and gender and is the first to my knowledge to do so when studying the DCM and mental health outcomes.

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used in this study suggest a higher rate of depression (via a PHQ-2) for Black non-Hispanic young adults, consistent with Dunlop et al. (2003) and Ettman et al. (2020).

2.3 Possible Pathways

There are several possible pathways for the DCM to influence mental health outcomes for young adults. The most straightforward pathway is the price of mental health care. Access and cost are often cited as major barriers to care (Alang, 2015), allowing for insurance to potentially increase care and in turn improve mental health outcomes. Other insurance interventions have been shown to increase mental health treatment or treatment availability in different populations (Ayyagari and Shane, 2015; Blunt et al., 2020).

Additionally, health insurance also lowers the price of physical health care. If physical health and mental health are connected, then increases in other forms of health care utilization may improve physical health and mental health.

Time allocation can similarly be influenced by the DCM. As most people in the U.S. obtain health insurance through their employer, having the additional option of dependent coverage can reduce the total benefit of working a job that offers health insurance. Young adults have been shown to move away from full time employment and towards part-time jobs in reaction to the DCM (Coleman and Dave, 2018). If young adults find full time employment more stressful than part time employment, then reductions in labor supply may lead to improved mental health outcomes. Conversely, the reduction in income associated with the DCM from this decline in labor supply could theoretically lead to worse mental health outcomes.

Lastly, it is possible that the act of possessing health insurance may reduce financial or other stress or anxiety and thus improve mental health measures. Expectations about the future may be improved by gaining health insurance coverage, resulting in reduced anxiety about future financial or health outcomes. This pathway, though difficult to quantify or model, may allow for insurance to improve mental health without the use of other pathways discussed, including care utilization.

2.4 Data

I use data from the Medical Expenditure Panel Survey (MEPS), from 2006 to 2013[19].The MEPS is a nationally representative 2-year panel survey with data pertaining to health care utilization and health status. It is the ideal data source for two key reasons. First, it contains several rich measures of mental health that are preferred over other commonly used data sources. Secondly, it includes variables on potential mechanisms to improve mental health, such as psychotherapy and psychotherapeutic medication received. Other important measures, such as health insurance status, employment status, and demographics are also included in the MEPS. However, one key limitation is the lack of state identifiers in the MEPS. Without information on the state of each observation, I am unable to control for state level variables that may have a relationship with mental health, such as minimum wages, Earned Income Tax Credit levels, or economics conditions. Additionally, models used in this study do not utilize the panel aspect because of the short duration, much like previous DCM work using the MEPS.

2.4.1 Outcome Variables

[19] As many states expanded their Medicaid eligibility thresholds in 2014, I end my sample period in 2013 to avoid any complications introduced by Medicaid policy.

The MEPS contains measures on health insurance status and source. I use binary measure for both uninsurance and holding any private health insurance. These measures have been commonly used in the DCM literature to show an increase in insurance coverage spawned by increases in holding private insurance.

For measures of mental health, the MEPS offers four detailed variables: PHQ-2, K6, mental health composite score, and self-rated mental health. Each variable captures a different, but detailed, dimension of mental health. First, the PHQ-2 is a commonly used method that generates a binary of measure depression. The PHQ-2 consists of two questions regarding the respondent's frequency of feeling down, depressed, hopeless, and other similar emotions in the past two weeks. Those that are considered at high risk of depression are flagged by the PHQ-2.

The K6 is a binary measure of serious psychological distress. Like the PHQ-2, the K6 asks questions about emotions in the preceding weeks but covers a broader range of potential signs of mental illness. Respondents that meet a certain threshold are flagged. The MEPS also reports a standardized overall measure of mental health called a mental health composite score (MHCS) from Short-Form 12. The MHCS is a continuous measure with a population average of 50 and standard deviation of 10 made up by the respondent's answers to 12 separate questions. Unlike the PHQ-2 and K6, the MHCS is a sign of positive mental health, meaning the higher the score, the healthier the respondent's mental health is considered to be.

Lastly, the MEPS contains a self-rated mental health measure, ranging from “poor” to “excellent”, which is asked during all three waves of the survey. I create two binary measures using this variable: those reporting "poor" or "fair" mental health in any of the periods and those reporting "excellent" in all periods. These two measures allow me to examine mental health on both sides of the severity spectrum.

Together, these five measures cover a wide range of mental health issues. The PHQ-2, K6, and fair or poor self-reported mental health represent more serious mental health measures. The MHCS shows mental health on the average and with a general measure. Lastly, excellent self-reported mental health captures the most positive end of the mental health spectrum. Table 2.1 shows sample questions for each of these variables to offer more insight into the differences across measures.

In addition to rich measures of mental health status, the MEPS also contains detailed variables regarding mental health care and treatment. I use binary measures for receiving any psychotherapy or mental health counseling in the past year as well as being prescribed any psychotherapeutic drugs such as anti-depressants or anti-psychotics. These two measures allow for an investigation into how the DCM impacts mental health care utilization, and how that impact may differ for different types of care.

2.4.2 Control Variables

The MEPS contains demographic and other individual level variables that are important controls. Race, ethnicity, age, marital status, and education attainment are all used as controls in this study. As mentioned, one limitation of the publicly available MEPS is the lack of state identifiers. However, region identifiers are included in the MEPS and used in this study.

2.4.3 Summary Statistics

Table 2.2 reports summary statistics for the key outcome variables discussed for the full sample and then separated by race and ethnicity followed by gender. With respect to mental health measures, the patterns here generally support observations made with other data sources. For example, Black non-Hispanic respondents as well as women are more likely to be flagged by the PHQ-2 than the full sample. Women also have a higher likelihood of being flagged by the K6 measure, a higher likelihood of reporting fair or poor mental health, and a lower MHCS than men.

With mental health care utilization, White non-Hispanic young adults appear to utilize care at roughly three times the rate of Black non-Hispanic and Hispanic young adults. This is true for both mental health care counseling as well as medication. These descriptive statistics support the argument that different racial and gender groups may interact with the mental health care system differently and in ways that are important for the DCM.

2.5 Empirical Methods

To isolate the impact of the DCM, I use a difference-in-differences model utilizing the change in insurance options over time for those 18-25 years old. Put differently, I compared those just under the age of 26 to those just over before and after the DCM. I use the following OLS equation:

$$Y_{it} = \beta_1 DCM_{it} + \gamma YoungAdult_i + \tau_t + \theta X_{it} + \varepsilon_{it}$$

Here, Y_{it} is a vector of outcomes discussed in section 4.1 for person i in year t. The variable DCM_{it} is a binary dummy set equal to one if the observation is under the age 26 and after September 2010 when the DCM was implemented, making β_1 the marginal impact of the policy. $YoungAdult_i$ represents a binary dummy[20] if observation i is less than 26 years old in year t. A year fixed effect is included with τ_t and individual level controls, such as race, ethnicity, census region, education, and marital status[21] are included with X_{it}.

All models are conducted using OLS and contain standard errors adjusted for the complex design of the MEPS. Additionally, main models limit the sample to those 19-30 years old. Following other DCM studies, I drop all 26-year-olds from my sample. Likewise, as the publicly available MEPS does not allow for month of year identifiers, I drop all observations from 2010. Robustness checks conduct models with a more narrow age range as well as region-specific linear time trends.

2.6 Results

Results are reported first for insurance status, followed by mental health outcomes, mental health care utilization, and additional possible mechanisms. In general, results for each set of outcomes are reported in two tables. First, I report results for the full sample followed by models for each race and ethnic group as well as gender. Second, I report models separated by both race and ethnicity and gender combinations.

[20] Results are nearly identical when age is included as a set of dummy variables rather than only one.
[21] Results are robust to the exclusion of marital status as a control.

2.6.1 Insurance Status

To first confirm the DCM led to increases in insurance coverage, and that those increases came from private insurance, I examine insurance status. Table 2.3 reports results for models of health insurance coverage. Results suggest the DCM is associated with a 6.15 percentage point decrease in the likelihood of not holding any insurance for the full sample, about a 25 percent change. Estimates for private insurance coverage suggest the decline in uninsurance is largely coming from private insurance. These estimates are in the range suggested by previous research.

When stratified by race and ethnicity, results suggest insurance gains are largest for Black non-Hispanic young adults with gains of around 12 percentage points, or roughly 50 percent, much larger than estimates for the full sample. Likewise, models separated by gender suggest men saw larger insurance gains than women.

Table 2.4 shows estimates on uninsurance status by race and ethnicity and gender to further investigate where gains in insurance are strongest. Although other groups show increases in insurance rates, Black non-Hispanic women are estimated to have the largest gains of nearly 15 percentage points. In general, these results show gains in insurance that correspond with previous work and further suggest these gains are largest for Black non-Hispanic respondents, particularly Black non-Hispanic women.

2.6.2 Mental Health Outcomes

Next, table 2.5 reports results when examining the mental health measures following the structure of table 2.3. For the full sample, the DCM is associated with a marginally significant increase in the mental health composite score - a measure of general mental health - equal to about one twentieth of a standard deviation. However, results do not show a statistically significant relationship with any other measure including measures of serious mental illness such as the PHQ-2 and K6. These findings are similar to Burns and Wolfe (2016) using the same data.

When separating samples by race and ethnicity, results for White non-Hispanic and Hispanic young adults do not suggest a change in mental health outcomes. However, Black non-Hispanic young adults, the group with the largest estimated change in health insurance status, see a statistically significant increase in their MHCS of roughly twice the size of the full sample. Additionally, results also suggest a 3.44 percentage point decrease in PHQ-2 and a 3.76 percentage point decrease in the likelihood of reporting fair or poor mental health for Black non-Hispanic young adults, or roughly 40 percent. These results suggest improvement in both the average and more serious measures of mental health for Black non-Hispanic young adults, a major benefit of the DCM that has been previously overlooked by studies not focusing on differential effects by demographic groups.

Table 2.6 shows estimates for different race and ethnicity and gender combinations. Black non-Hispanic women, the combination with the largest estimated gain in health insurance, see a consistent improvement in mental health measures. Black non-Hispanic women are estimated to have a decrease in the PHQ-2, K6, and the likelihood of reporting fair or poor mental health of roughly 6, 3, and 5 percentage points respectively. Black non-Hispanic women also see an increase in the MHCS equal to roughly one-fourth of a standard deviation. No other group is found to have a statistically significant change in any outcome, including Black non-Hispanic men, suggesting Black non-Hispanic women are driving previous results.

2.6.3 Mental Health Outcomes Robustness Checks

An assumption used by the difference-in-differences model I employ is that the pre-treatment trends for both groups above and below age 26 were parallel. To test this assumption, I use event study models that attribute the enactment of the DCM to each year in my sample with the year before for the policy as the reference year. Figures 2.1-2.11 report event study findings for the PHQ-2, K6, MHCS, self-rated fair or poor mental health, and self-rated excellent mental health for Black non-Hispanic respondents as well as Black non-Hispanic women. For each outcome and sample, no pre-treatment years are found to be statistically different than zero, suggesting the parallel pre-trends assumption is not violated and instilling confidence in my findings. These results also suggest the mental health improvements associated with the DCM are stable in general, though some outcomes may be growing modestly over time.

As an additional robustness check, appendix tables 2.1 and 2.2 replicate tables 2.5 and 2.6 for models with young adults age 22-29. The more narrow age group reduces the sample size but does focus on those more closely around the age of the policy change. While estimates are considerably less precise than with the larger sample, the general patterns of tables 5 and 6 remain. Additionally, appendix tables 2.3 and 2.4 report results for mental health outcomes with the inclusion of region-specific linear time trends. Results are nearly unchanged with this inclusion.

2.6.4 Mental Health Care Utilization

Table 2.7 reports results for two types of mental health care utilization: psychotherapy and prescription medication. While no statistically significant effect is found for the full sample, White non-Hispanic young adults are found to increase their usage of psychotherapy following the DCM by 2.29 percentage points, or about 50 percent. This finding is robust to event study analysis reported in figure 2.8. It is worth noting that White non-Hispanic young adults do not see an increase in the likelihood of receiving any treatment, perhaps suggesting a shift from medication towards psychotherapy following the DCM.

No other group is found to change their utilization of mental health care. Looking at results by race and ethnicity and gender combinations in table 2.8 suggest very similar results for White non-Hispanic men and women but notably does not indicate a change in care received for Black non-Hispanic men or women.

These results suggest two different conclusions. First, the DCM may not have decreased the gap in mental health care usage among White and non-White Americans, but rather may have increased the gap in regard to psychotherapy utilization. Second, the group found to increase mental health care utilization, White non-Hispanic young adults, is not the same group found to have mental health improvements in previous models.

Black non-Hispanic young adults, particularly women saw improvements in measures of serious mental illnesses but did not increase their utilization of mental health care. Together, this suggests there may be a pathway for private health insurance to improve mental health that is separate from mental health care usage. Identifying this pathway may be of particular importance for policy makers looking to combat mental health issues. However, as Black non-Hispanic young adults also saw the largest increase in health care coverage following the DCM, it is unclear if the larger gains in coverage lead to the improvements, or rather if something idiosyncratic to the Black non-Hispanic population led to the improvements. The following section explores possible mechanisms where data is available. However, as previously mentioned, the stress reduction or mental health benefit associated with simply having health insurance cannot be measured or thus modeled in this study. It is possible the improvements shown in this paper are due to this sense of "relief" rather than other mechanisms.

2.6.5 Other Possible Mechanisms

As argued by previous research, the introduction of the DCM may remove part of the total compensation of working a job that offers health insurance. With an additional pathway for insurance, full time employment may not be as appealing as before the DCM for young adults. Previous studies have documented a shift from full to part time work following the DCM. In table 2.9 I test this relationship within the MEPS data. While the MEPS may not be the ideal dataset for measuring labor supply, largely due to smaller sample size compared to the Current Population Survey or American Community Survey, being internally consistent with the estimates on mental health is important.

Results reported in table 2.9 confirm the relationship suggested by past research. Young adults within this sample moved away from full time work and for some groups towards part time employment following the DCM. White non-Hispanic young adults are estimated to decrease the likelihood of working any hours in the reference week by roughly 11 percentage points. Likewise, full time employment for this group declined by an estimated 17 percentage points with an increase in part time work of 6 percentage points. However, for Black non-Hispanic young adults the movement away from working any hours is larger with 17 percentage points, while the change is made entirely by a shift from full time employment to no employment at all. If full time employment exacerbates or creates stress and anxiety that can lead to mental health issues, this marks a possible mechanism outside of mental health care for the DCM to improve mental health.

As physical health and mental health may be closely related and impact each other, effects on physical health offer a potential pathway for the DCM to impact mental health. Both the MEPS and Behavioral Risk Factor Surveillance System (BRFSS) contain measures of physical health that differ from one another. Much like the MHCS estimated in previous models, the MEPS also collects data on a physical health composite score. This score is also normalized to a mean of 50 with a standard deviation of 10 and is made up of questions around physical health impacting daily life. Additionally, the BRFSS, a nationally representative survey on health-related outcomes, asks respondents how many of the past 30 days have been spent in "not good" physical health. Appendix table 2.5 reports estimated effects for the physical health composite score and number of "not good" physical health days. In general, neither measure of physical health is found to have a change, with the exception of women and the number of "not good" physical health days. These results do not suggest physical health as a likely pathway for the DCM's improvement in the mental health of Black non-Hispanic young adults.

This exploration on mechanisms leaves a less-than-clear picture of the pathways allowing the DCM to impact mental health. As the group with improvements in mental health also see the largest insurance gains, it is possible the "relief" of gaining insurance discussed previously may be a main mechanism. However, future work may examine these pathways in more detail.

2.7 Discussion

Understanding the mental health implications of the DCM aspect of the Affordable Care Act are important when evaluating the program and health insurance policy more broadly. This study models the DCM on measures of mental health, mental health care utilization, and other possible pathways insurance policy can influence mental health.

Results presented suggest previous estimates of the DCM's impact on mental health status overlooked the mental health benefits for Black non-Hispanic young adults. This study presents robust evidence of reductions in measures of serious mental illness and increases in a general measure of mental health for this population, driven by Black non-Hispanic women. Conversely, I do not find any changes in mental health care utilization for Black non-Hispanic young adults. Exploration of other possible mechanisms suggests perhaps changes in labor supply have mental health benefits though stress relief of simply gaining health insurance may play a major role and cannot be measured or modeled in this study. Future work may explore this pathway more directly. While Black non-Hispanic young adults, particularly Black non-Hispanic women, are estimated to have mental health benefits from the DCM, what is not known is if this improvement is due to the larger increase in insurance coverage or if insurance coverage may have differential mental health implications based on demographics. Additionally, as the national recovery from the Great Recession takes place over my sample period, it is possible differential impact of the recession by age could be influencing the results found. However, it appears likely these differential impacts would lead to worsen employment outcomes for young adults which could biases my estimates towards worsen mental health. Given the models reported here find an improvement in mental health, this pathway of possible bias may be minimal.

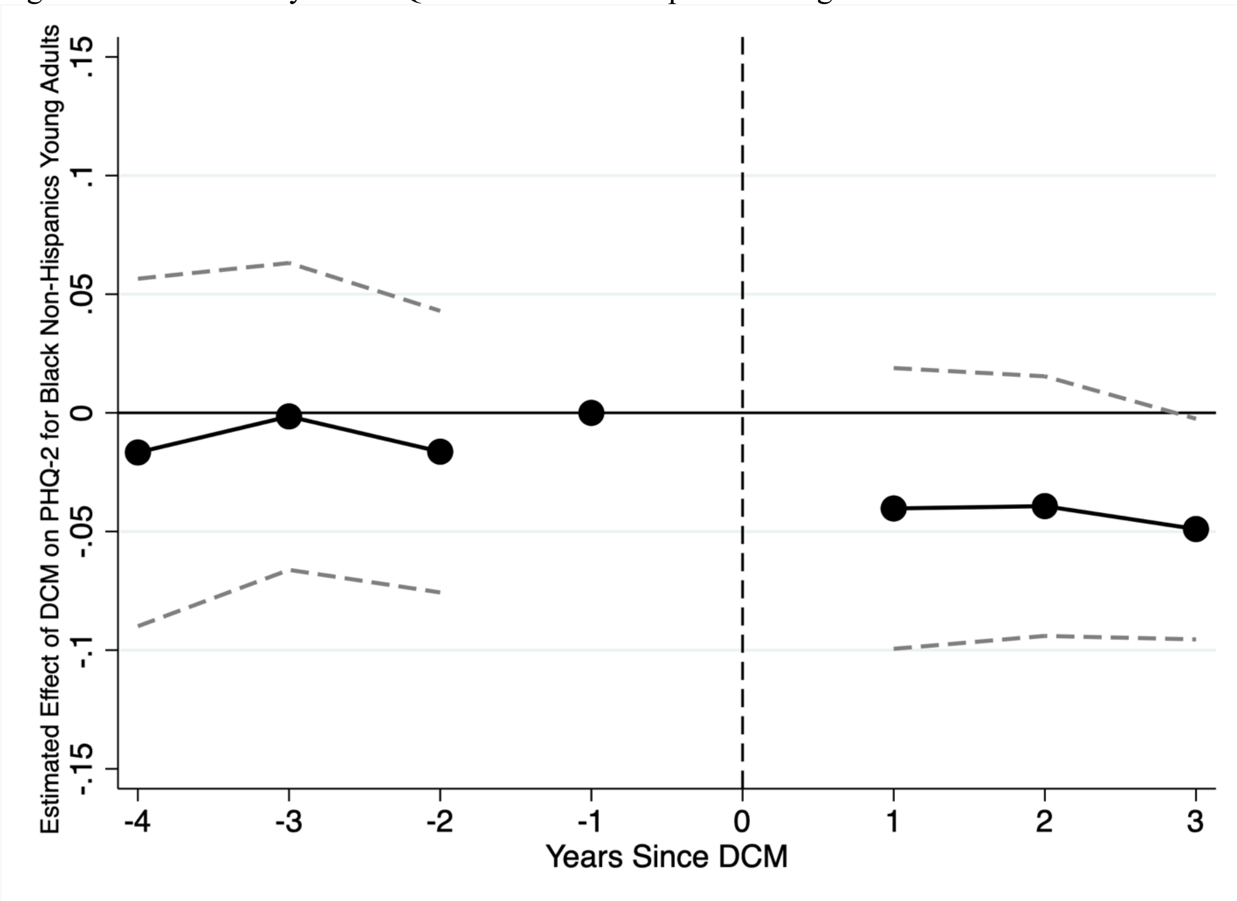
It is worth noting this is not the first study to suggest health insurance can reduce signs of mental illness without increasing treatment for such conditions. The Oregon Health Experiment (Baicker et al., 2013) models the impact of receiving public health insurance through a lottery system and finds a large reduction in screening positive for depressive via a PHQ-8 as well as no change in medication for depression at the 5 percent significance level[22]. These results, while looking at a different population and different type of health insurance, are similar to the results presented here and further suggest that a connection between insurance and mental health exists outside of mental health care.

The results reported here suggest the DCM is associated with mental health improvements for Black non-Hispanic young adults, though more work may be needed to fully understand the mechanisms at play. Additionally, future work regarding mental health outcomes and different policies may benefit from playing close attention to different impacts by demographic groups.

[22] Online appendix tables do, however, show an increase in medication for depression statistically significant at the 10 percent level.

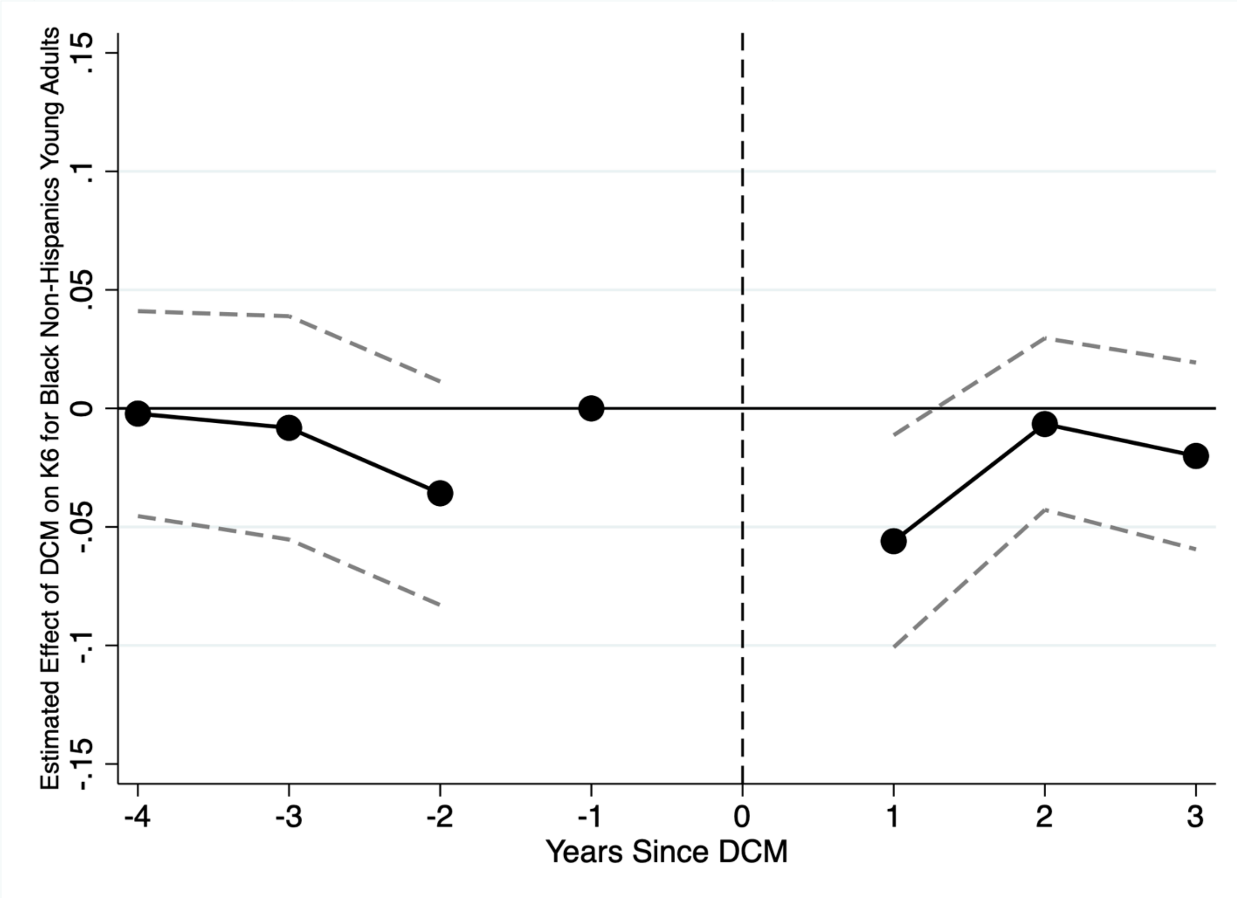
Figures and Tables

Figure 2.1. Event Study for PHQ-2 - Black Non-Hispanic Young Adults



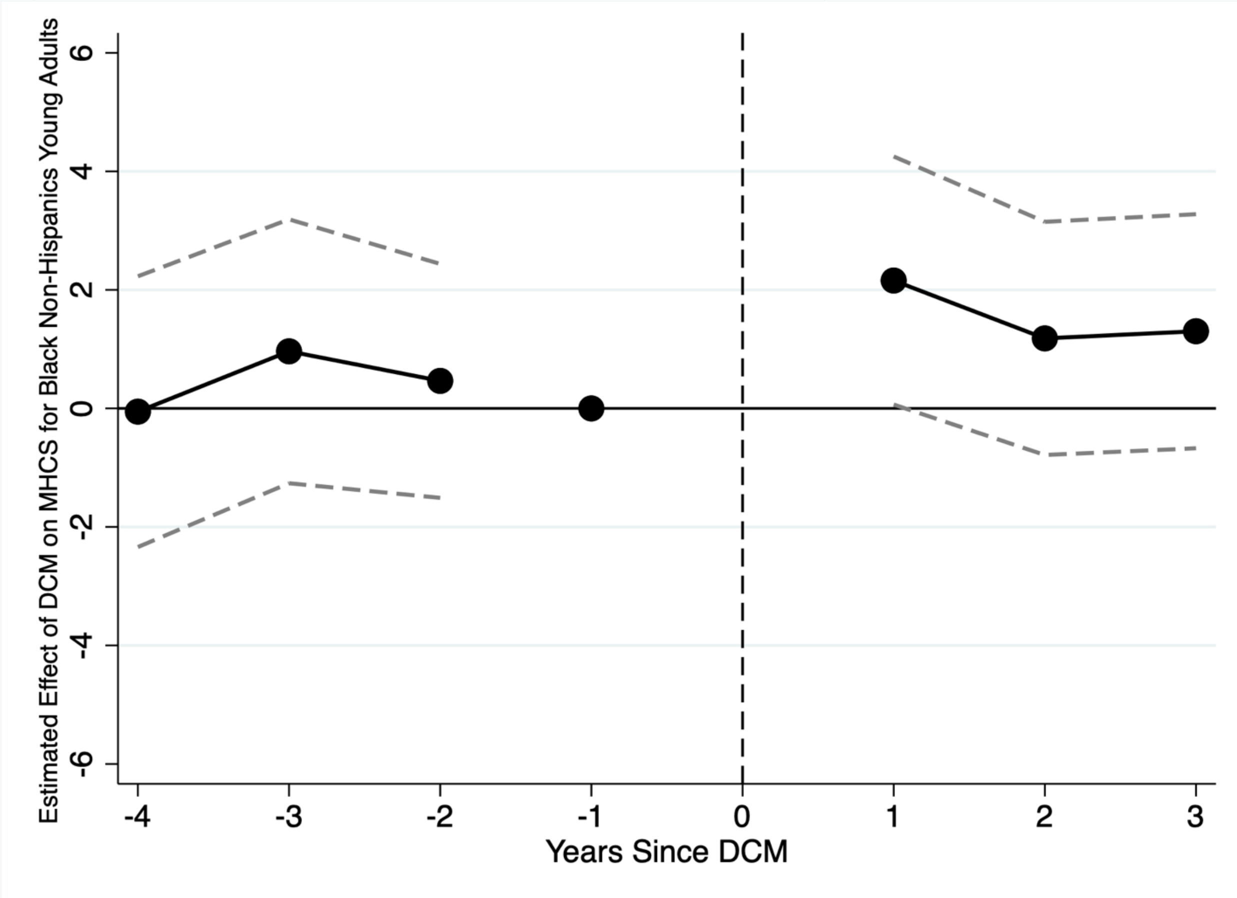
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.2. Event Study for K6 - Black Non-Hispanic Young Adults



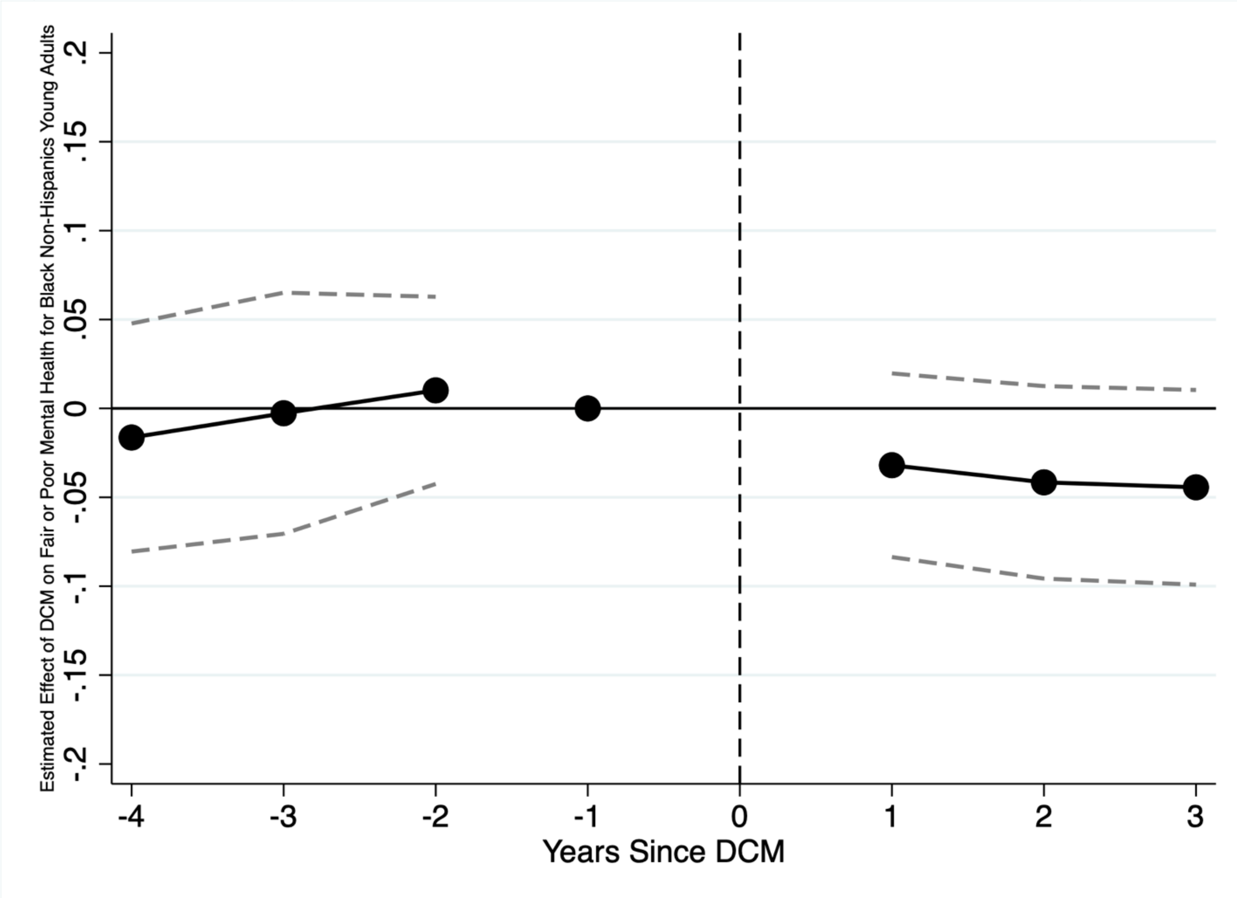
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.3. Event Study for MHCS - Black Non-Hispanic Young Adults



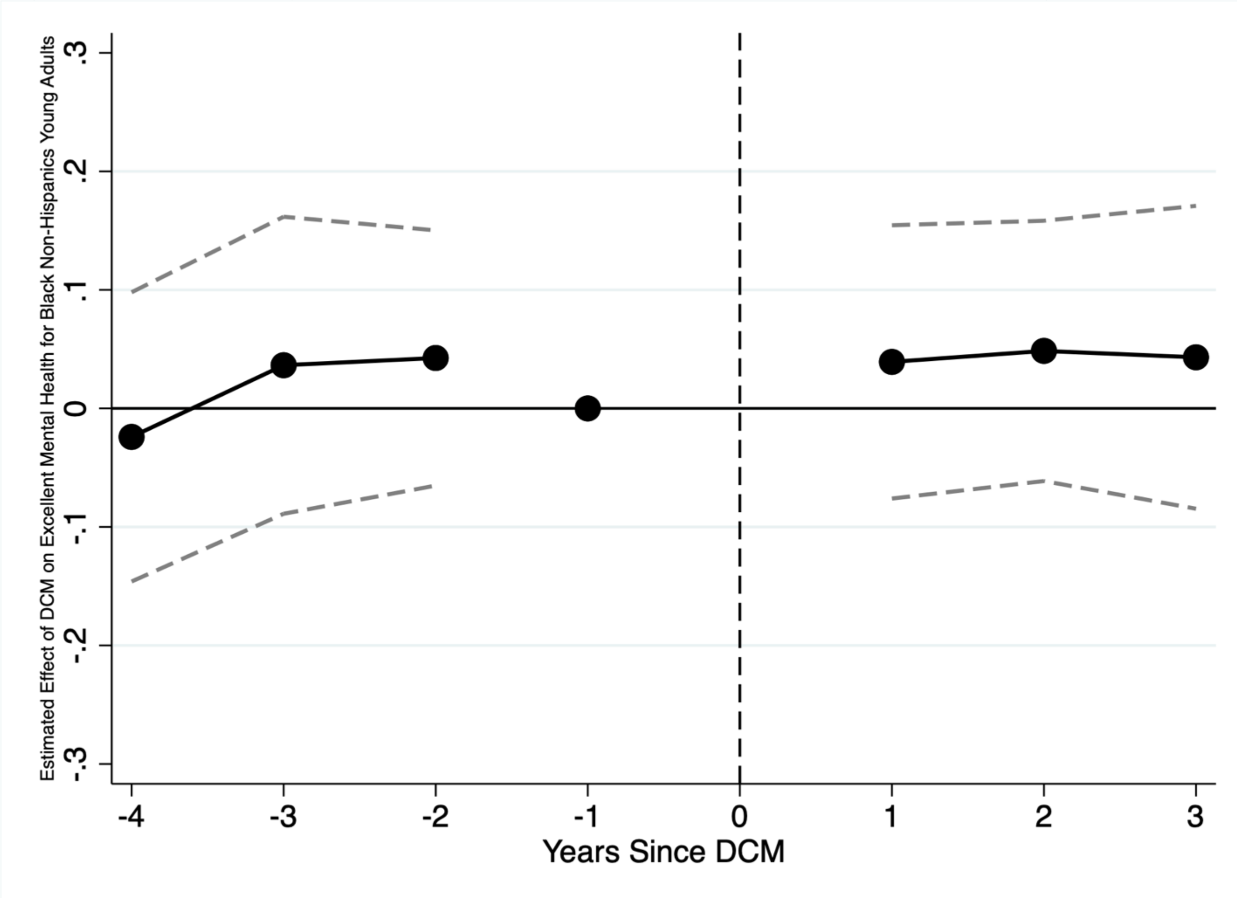
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.4. Event Study for Fair or Poor Mental Health - Black Non-Hispanic Young Adults



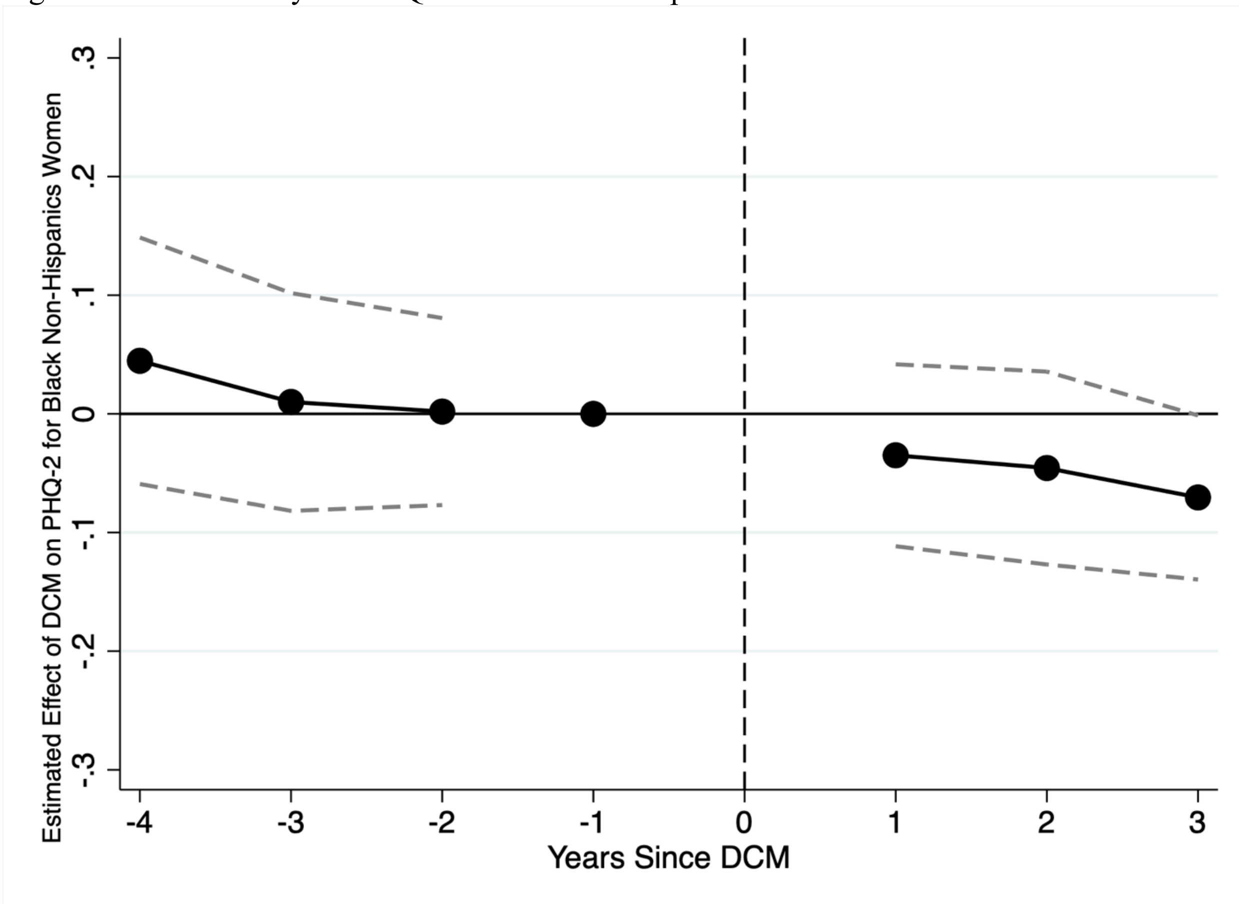
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.5. Event Study for Excellent Mental Health - Black Non-Hispanic Young Adults



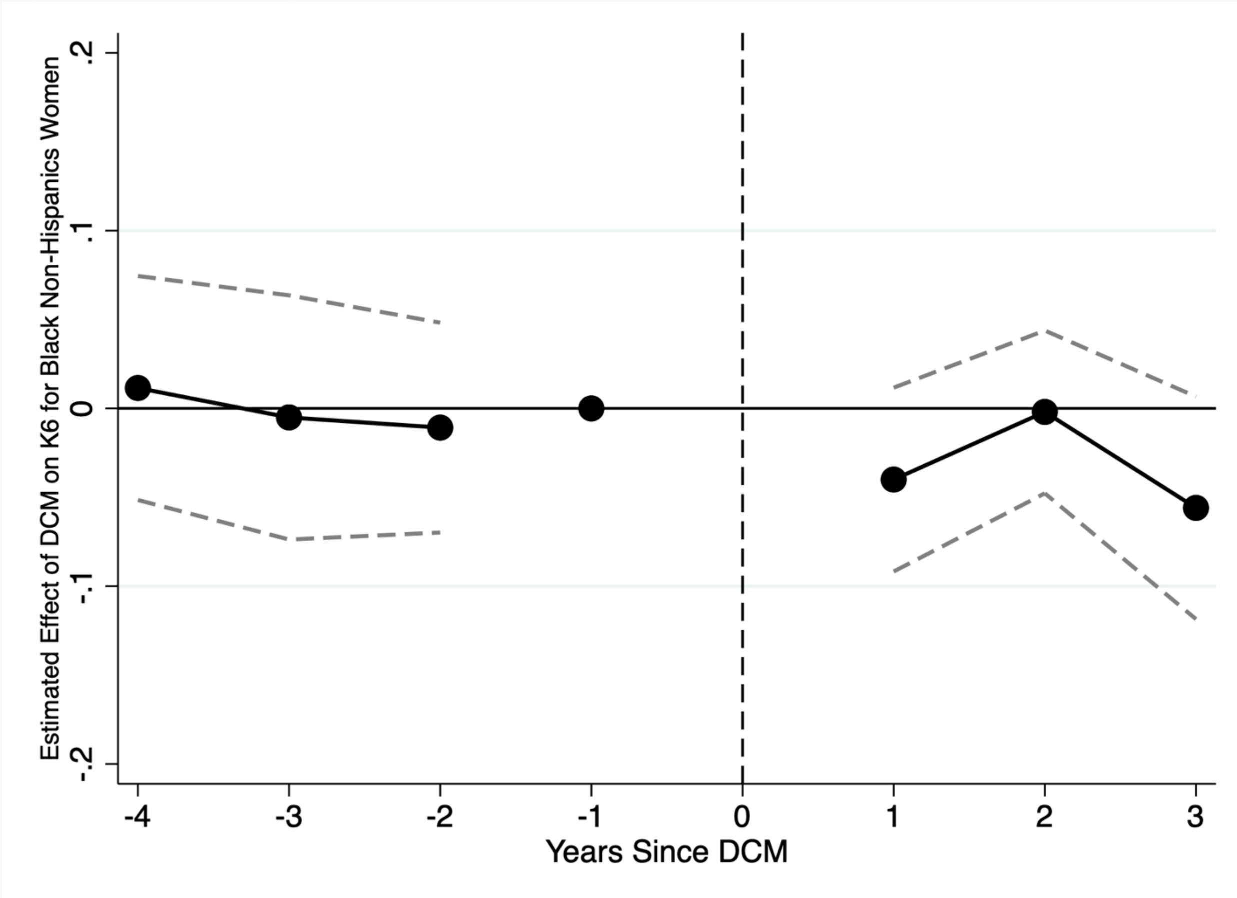
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.6. Event Study for PHQ-2 - Black Non-Hispanic Women



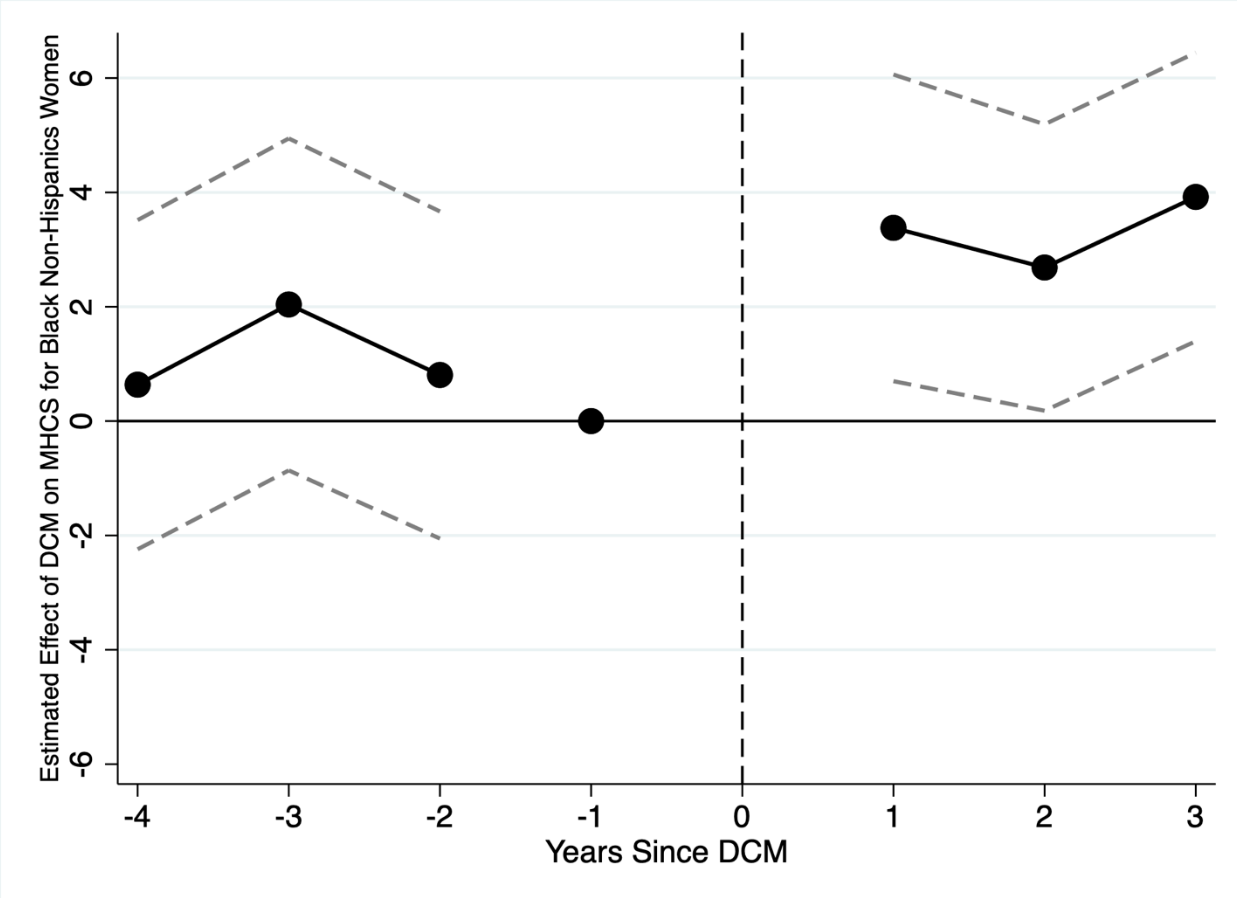
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.7. Event Study for K6 - Black Non-Hispanic Women



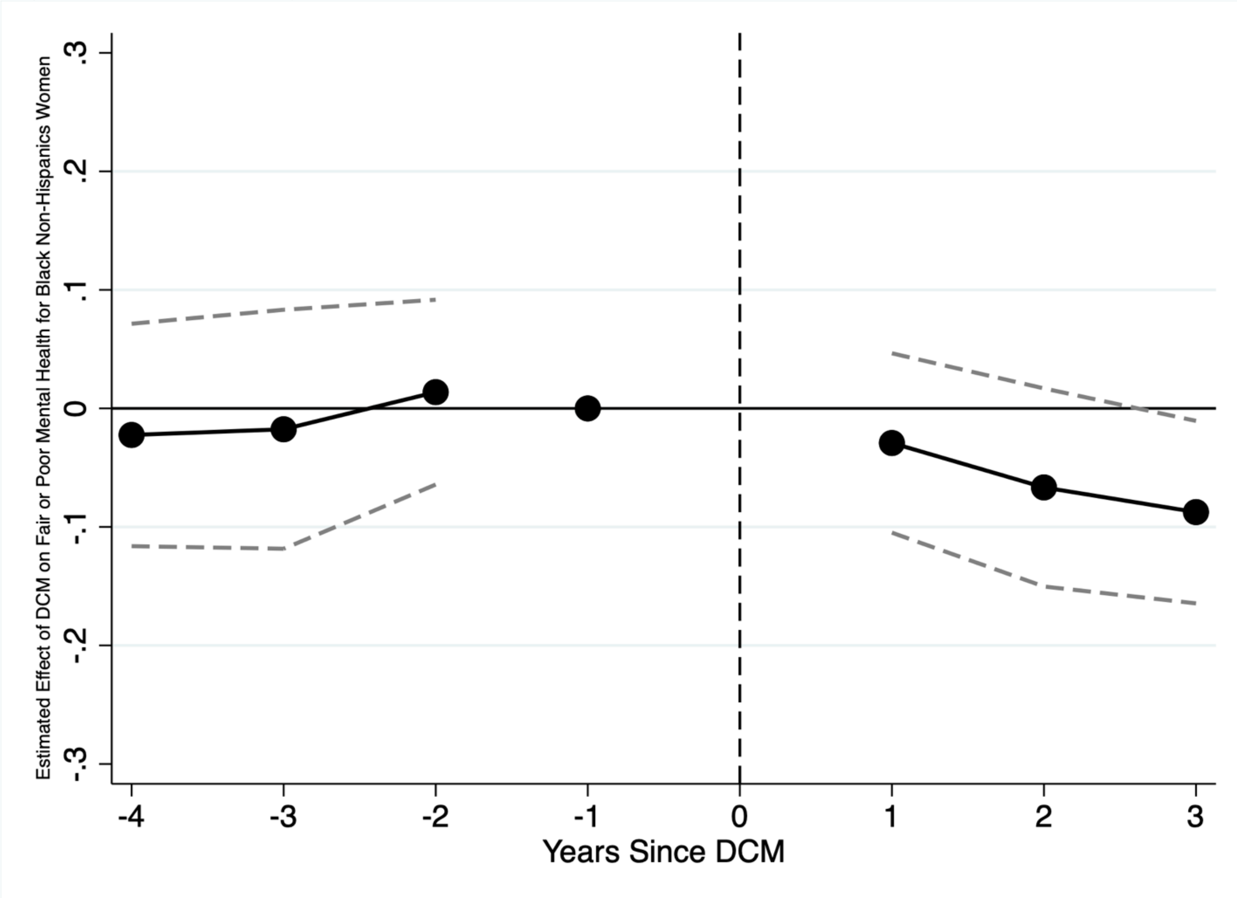
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.8. Event Study for MHCS - Black Non-Hispanic Women



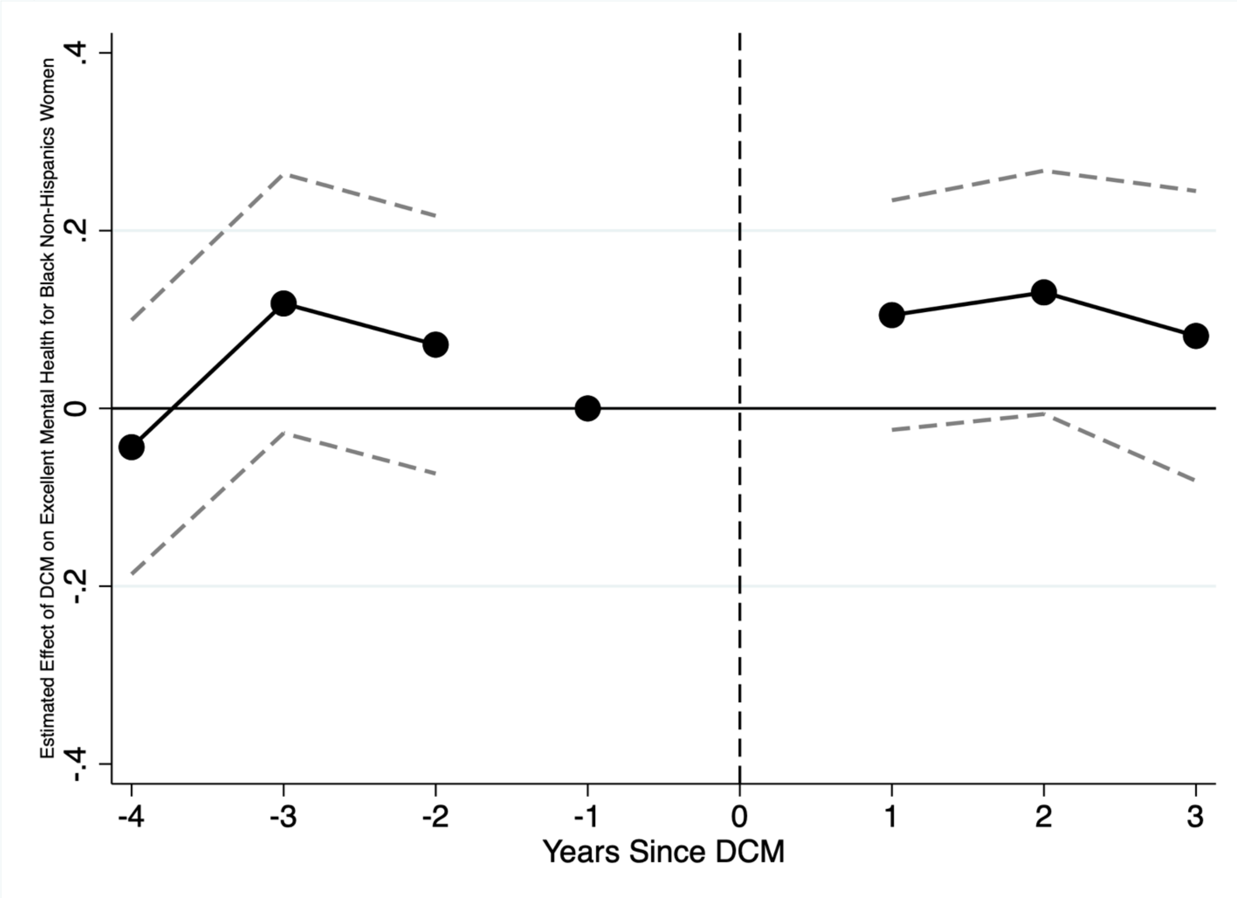
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.9. Event Study for Fair or Poor Mental Health - Black Non-Hispanic Women



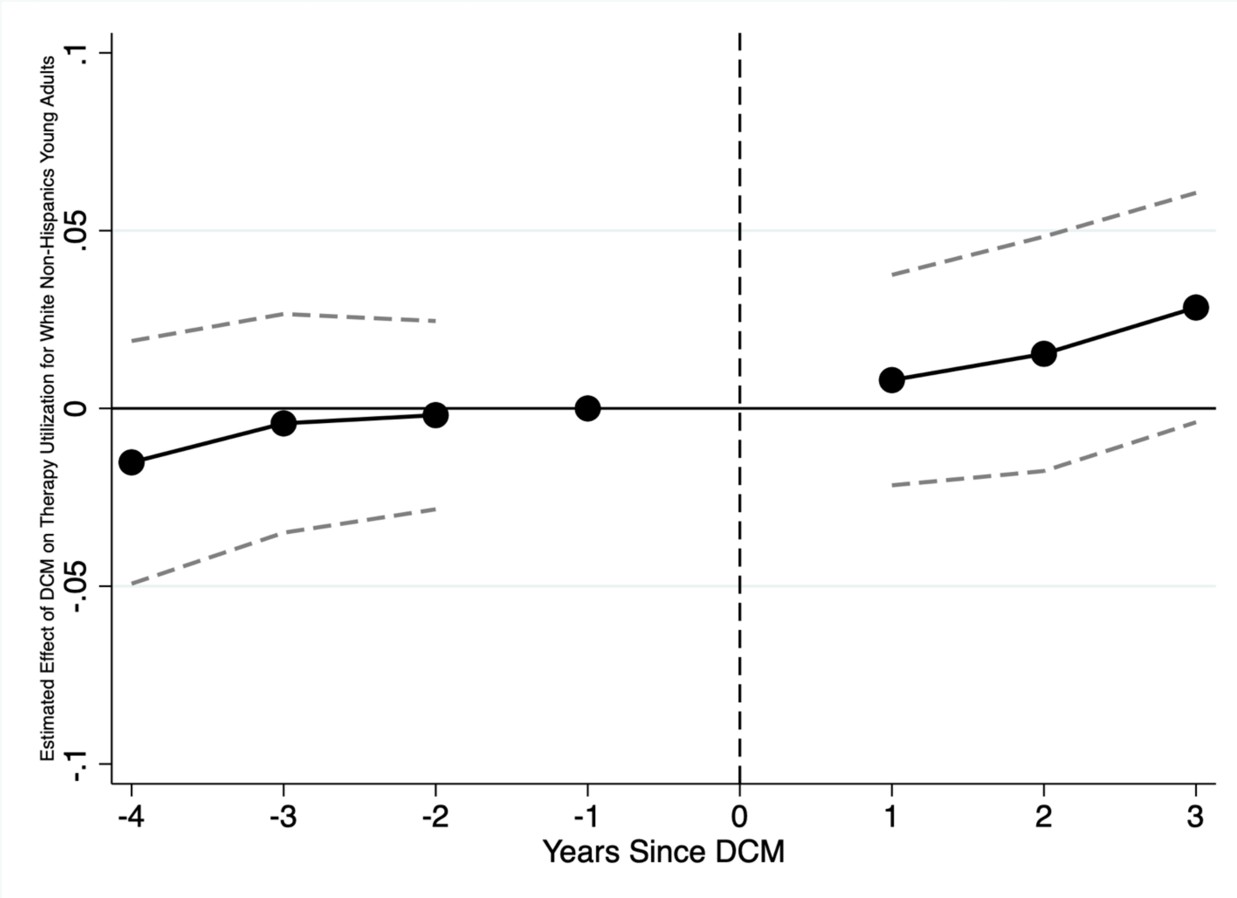
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.10. Event Study for Excellent Mental Health - Black Non-Hispanic Women



Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Figure 2.11. Event Study for Therapy Utilization - White Non-Hispanic Young Adults



Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status using 2006-2013 MEPS for those age 19-30. All estimates are weighted.

Table 2.1. Examples of Mental Health Measures

Measure	Example Questions	Answer Range	Threshold
PHQ-2	Frequency having little interest or pleasure in doing things Frequency feeling down, depressed, or hopeless	Not at all (0) to Nearly every day (3)	Score $>$2
K6	Frequency feeling hopeless, frequency feeling worthless Frequency feeling nervous, frequency feeling restless or fidgety	None of the time (0) to All of the time (4)	Score $>$12
MHCS (SF-12)	Accomplished less than you would like Limited in kind of work or other activities	None of the time (0) to All of the time (4)	NA
Self-reported MH	You would say your mental health is	Poor to Excellent	NA

Table 2.2. Summary Statistics						
	Full Sample	White non-Hispanic	Black non-Hispanic	Hispanic	Women	Men
Insurance Status						
Uninsurance	0.244	0.180	0.259	0.437	0.187	0.301
	(0.417)	(0.295)	(0.518)	(0.623)	(0.386)	(0.432)
Private Insurance	0.557	0.658	0.418	0.333	0.566	0.547
	(0.482)	(0.364)	(0.583)	(0.592)	(0.491)	(0.469)
Mental Health Outcomes						
PHQ-2	0.068	0.066	0.088	0.061	0.080	0.057
	(0.207)	(0.161)	(0.282)	(0.254)	(0.227)	(0.184)
K6	0.039	0.040	0.041	0.036	0.047	0.031
	(0.159)	(0.127)	(0.196)	(0.197)	(0.177)	(0.138)
MHCS	51.325	50.948	52.044	51.857	50.165	52.489
	(7.814)	(6.136)	(9.613)	(10.126)	(8.267)	(7.162)
Fair or Poor MH	0.088	0.091	0.086	0.079	0.093	0.082
	(0.231)	(0.186)	(0.280)	(0.287)	(0.243)	(0.217)
Excellent MH	0.265	0.273	0.272	0.236	0.247	0.283
	(0.361)	(0.289)	(0.443)	(0.451)	(0.361)	(0.357)
Mental Health Care Utilization						
Therapy	0.033	0.043	0.015	0.018	0.040	0.027
	(0.174)	(0.155)	(0.145)	(0.166)	(0.194)	(0.152)
Medication	0.058	0.078	0.026	0.029	0.077	0.040
	(0.227)	(0.206)	(0.190)	(0.210)	(0.265)	(0.184)
N	33,727	12,434	6,758	11,351	17,660	16,067

Table 2.3. Marginal Effects of DCM on Insurance Status		
	Uninsurance	Private Insurance
Full Sample	-0.0615***	0.0868***
	(0.0141)	(0.0167)
By Race/Ethnicity		
White Non-Hisp	-0.0590***	0.1046***
	(0.0206)	(0.0237)
Black Non-Hisp	-0.1229***	0.1069***
	(0.0309)	(0.0327)
Hispanic	-0.0524**	0.0653**
	(0.0270)	(0.0289)
By Gender		
Women	-0.0391**	0.0937***
	(0.0166)	(0.0218)
Men	-0.0802***	0.0793***
	(0.0203)	(0.0241)
Full Sample Mean	0.258	0.554
Full Sample N	33,770	33,544

**Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Table 2.4. Marginal Effects of DCM on Insurance Status by Subgroups

	Uninsurance	Private Insurance
White Non-Hispanic		
Women	-0.0465**	0.1100***
	(0.0225)	(0.0306)
Men	-0.0703**	0.0983***
	(0.0298)	(0.0345)
Black Non-Hispanic		
Women	-0.1479**	0.1943***
	(0.0340)	(0.0419)
Men	-0.0850*	-0.0075
	(0.0487)	(0.0532)
Hispanic		
Women	0.0369	0.0059
	(0.0347)	(0.0413)
Men	-0.1247***	0.1144***
	(0.0341)	(0.0384)
Full Sample Mean	0.258	0.554
Full Sample N	33,770	33,544

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Table 2.5. Marginal Effects of DCM on Mental Health					
	PHQ-2	K6	MHCS	Fair/Poor MH	Excellent MH
Expected Sign	(-)	(-)	(+)	(-)	(+)
Full sample	-0.0096	-0.0035	0.5427*	0.0001	0.0225
	(0.0083)	(0.0067)	(0.3082)	(0.0098)	(0.0181)
By Race/Ethnicity					
White Non-Hispanic	-0.0042	-0.0039	0.2221	0.0060	0.0213
	(0.0120)	(0.0097)	(0.4547)	(0.0146)	(0.0259)
Black Non-Hispanic	-0.0344**	-0.0154	1.1886*	-0.0376**	0.0287
	(0.0170)	(0.0130)	(0.6293)	(0.0185)	(0.0344)
Hispanic	0.0034	0.0167	0.5465	0.0062	0.0168
	(0.0134)	(0.0110)	(0.5313)	(0.0162)	(0.0252)
By Gender					
Women	-0.0105	0.0075	0.8301*	-0.0068	0.0176
	(0.0107)	(0.0087)	(0.4300)	(0.0133)	(0.0225)
Men	-0.0084	-0.0141	0.2044	0.0075	0.0275
	(0.0110)	(0.0089)	(0.4224)	(0.0152)	(0.0240)
Full Sample Mean	0.068	0.039	51.325	0.088	0.265
Full Sample N	30,898	30,727	31,128	31,256	31,369

**Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Table 2.6. Marginal Effects of DCM on Mental Health by Subgroups

	PHQ-2	K6	MHCS	Fair/Poor MH	Excellent MH
Expected Sign	(-)	(-)	(+)	(-)	(+)
White Non-Hispanic					
Women	0.0026	0.0138	0.2257	-0.0037	0.0157
	(0.0152)	(0.0123)	(0.4636)	(0.0192)	(0.0311)
Men	-0.0120	-0.0226*	0.1774	0.0161	0.0278
	(0.0163)	(0.0132)	(0.6225)	(0.0229)	(0.0346)
Black Non-Hispanic					
Women	-0.0635***	-0.0306*	2.4478***	-0.0544**	0.0684
	(0.0234)	(0.0164)	(0.7945)	(0.0253)	(0.0439)
Men	-0.0027	0.0005	-0.1742	-0.0199	-0.0034
	(0.0241)	(0.0188)	(0.9361)	(0.0286)	(0.0524)
Hispanic					
Women	0.0091	0.0286*	0.9027	0.0162	-0.0046
	(0.0178)	(0.0157)	(0.7548)	(0.0220)	(0.030)
Men	-0.0005	0.0064	0.2564	-0.0037	0.0375
	(0.0171)	(0.0123)	(0.7174)	(0.0213)	(0.0371)
Full Sample Mean	0.068	0.039	51.325	0.088	0.265
Full Sample N	30,898	30,727	31,128	31,256	31,369

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Table 2.7. Marginal Effects of DCM on Mental Health Care Utilization			
	Psychotherapy	Medication	Any Treatment
Full sample	0.0111	-0.0128	-0.0076
	(0.0068)	(0.0093)	(0.0104)
By Race/Ethnicity			
White Non-Hispanic	0.0223**	-0.0214	-0.0103
	(0.0103)	(0.0144)	(0.0157)
Black Non-Hispanic	-0.0054	-0.0057	-0.0055
	(0.0104)	(0.0116)	(0.0137)
Hispanic	-0.0039	0.0031	0.0006
	(0.0076)	(0.0100)	(0.0119)
By Gender			
Women	0.0147	-0.0152	-0.0063
	(0.0101)	(0.0144)	(0.0161)
Men	0.0064	-0.0095	-0.0083
	(0.0084)	(0.0110)	(0.0125)
Full Sample Mean	0.033	0.058	0.075
Full Sample N	33,770	33,770	33,770

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Table 2.8. Marginal Effects of DCM on Mental Health Care Utilization by Subgroups			
	Psychotherapy	Medication	Any Treatment
White Non-Hispanic			
Women	0.0212	-0.0296	-0.0160
	(0.0155)	(0.0229)	(0.0248)
Men	0.0220*	-0.0117	-0.0040
	(0.0125)	(0.0183)	(0.0201)
Black Non-Hispanic			
Women	-0.0001	-0.0049	0.0023
	(0.0133)	(0.0165)	(0.01924)
Men	-0.0130	-0.0085	-0.0164
	(0.0155)	(0.0156)	(0.0183)
Hispanic			
Women	0.0076	0.0145	0.0108
	(0.0128)	(0.0174)	(0.0205)
Men	-0.0145	-0.0064	-0.0086
	(0.0100)	(0.0119)	(0.0136)
Full Sample Mean	0.033	0.058	0.075
Full Sample N	33,770	33,770	33,770

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Table 2.9. Marginal Effects of DCM on Employment Status			
	Any Hours	Full Time	Part Time
Full sample	-0.1321***	-0.1670***	0.0349***
	(0.0138)	(0.0151)	(0.0142)
By Race/Ethnicity			
White Non-Hispanic	-0.1107***	-0.1680***	0.0573***
	(0.0205)	(0.0219)	(0.0206)
Black Non-Hispanic	-0.1703***	-0.1772***	0.0069
	(0.0297))	(0.0289)	(0.0305)
Hispanic	-0.1439***	-0.1481***	0.0042
	(0.0217))	(0.0252)	(0.0199)
By Gender			
Women	-0.0934***	-0.1317***	0.0383*
	(0.0179)	(0.0227)	(0.0199)
Men	-0.1762***	-0.2103***	0.0341*
	(0.0184)	(0.0191)	(0.0180)
Full Sample Mean	0.755	0.556	0.199
Full Sample N	33,770	33,770	33,770

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted. Full time worked is defined by reporting at least 40 hours of work in the reference week. Part time work is defined by reporting 1-39 hours in the reference week.

Table 2.10. Marginal Effects of DCM on Employment Status by Subgroup			
	Any Hours	Full Time	Part Time
White Non-Hispanic			
Women"	-0.0744***	-0.1455***	0.0711**
	(0.0249)	(0.0337)	(0.0291)
Men	-0.1564***	-0.2028***	0.0464*
	(0.0267)	(0.0267)	(0.0263)
Black Non-Hispanic			
Women	-0.1470***	-0.1352***	-0.0119
	(0.0356)	(0.0365)	(0.0425)
Men	-0.2000***	-0.2257***	0.0257
	(0.0422)	(0.0450)	(0.0376)
Hispanic			
Women	-0.0864**	-0.0751**	-0.0113
	(0.0352)	(0.0321)	(0.0295)
Men	-0.1892***	-0.2089***	0.0198
	(0.0283)	(0.0320)	(0.0283)
Full Sample Mean	0.755	0.556	0.199
Full Sample N	33,770	33,770	33,770

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted. Full time worked is defined by reporting at least 40 hours of work in the reference week. Part time work is defined by reporting 1-39 hours in the reference week.

Appendix Table 2.1. Marginal Effects of DCM on Mental Health, 22-29-Year-olds					
	PHQ-2	K6	MHCS	Fair/Poor MH	Excellent MH
Expected Sign	(-)	(-)	(+)	(-)	(+)
Full sample	-0.0119	-0.0046	0.7601**	0.0036	0.0349*
	(0.0098)	(0.0082)	(0.3548)	(0.0117)	(0.0209)
By Race/Ethnicity					
White Non-Hispanic	-0.0109	-0.0026	0.5062	0.0086	0.0479
	(0.0141)	(0.0117)	(0.5140)	(0.0170)	(0.0304)
Black Non-Hispanic	-0.0350*	-0.0196	1.1978	-0.0262	-0.0233
	(0.0208)	(0.0152)	(0.7625)	(0.0236)	(0.0364)
Hispanic	0.0012	0.0131	0.9058	0.0125	0.0164
	(0.0154)	(0.0138)	(0.6038)	(0.0207)	(0.0304)
By Gender					
Women	-0.0160	0.0072	0.7850	-0.0005	0.0233
	(0.0136)	(0.0100)	(0.4878)	(0.0155)	(0.0255)
Men	-0.0067	-0.0156	0.6721	0.0082	0.0478*
	(0.0130)	(0.0108)	(0.5148)	(0.0184)	(0.0280)
Full Sample Mean	0.068	0.041	51.107	0.090	0.267
Full Sample N	19,139	19,033	19,286	19,356	19,428

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 22-29 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Appendix Table 2.2. Marginal Effects of DCM on Mental Health by Subgroups, 22-29-Year-olds					
	PHQ-2	K6	MHCS	Fair/Poor MH	Excellent MH
Expected Sign	(-)	(-)	(+)	(-)	(+)
White Non-Hispanic					
Women	-0.0084	0.0168	0.1235	0.0075	0.0258
	(0.1928)	(0.0139)	(0.7226)	(0.0222)	(0.0372)
Men	-0.0129	-0.0223	0.8008	0.1038	0.0717*
	(0.0199)	(0.0158)	(0.7302)	(0.0266)	(0.0404)
Black Non-Hispanic					
Women	-0.0544*	-0.0363*	1.8815*	-0.0518*	0.0116
	(0.0297)	(0.0199)	(1.0128)	(0.0314)	(0.0451)
Men	-0.0189	-0.0025	0.5340	0.0049	-0.0557
	(0.0296)	(0.0229)	(1.0816)	(0.0336)	(0.0607)
Hispanic					
Women	-0.0110	0.0212	1.5102	0.0287	0.0175
	(0.0230)	(0.0203)	(0.8342)	(0.02662)	(0.0368)
Men	0.0132	0.0051	0.4267	-0.0010	0.0175
	(0.0196)	(0.0159)	(0.8672)	(0.0288)	(0.0443)
Full Sample Mean	0.068	0.041	51.107	0.090	0.267
Full Sample N	19,139	19,033	19,286	19,356	19,428

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 22-29 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Appendix Table 2.3. Marginal Effects of DCM on Mental Health with Region Specific Linear Time Trends

	PHQ-2	K6	MHCS	Fair/Poor MH	Excellent MH
Expected Sign	(-)	(-)	(+)	(-)	(+)
Full sample	-0.0096	-0.0035	0.5391*	0.0004	0.0214
	(0.0083)	(0.0066)	(0.3088)	(0.0098)	(0.0180)
By Race/Ethnicity					
White Non-Hispanic	-0.0048	-0.0043	0.2340	0.0071	0.0193
	(0.0120)	(0.0097)	(0.4576)	(0.0147)	(0.0259)
Black Non-Hispanic	-0.0355**	-0.0159	1.192*	-0.0388**	0.0258
	(0.0170)	(0.0131)	(0.6306)	(0.0187)	(0.0345)
Hispanic	0.0029	0.0162	0.5600	0.0056	0.0183
	(0.0135)	(0.0111)	(0.5341)	(0.0161)	(0.0253)
By Gender					
Women	-0.0111	0.0072	0.8556**	-0.0067	0.0172
	(0.0108)	(0.0087)	(0.4323)	(0.0133)	(0.0225)
Men	-0.0080	-0.0141	0.1793	0.0079	0.0258
	(0.0110)	(0.0089)	(0.4212)	(0.0152)	(0.0238)
Full Sample Mean	0.068	0.039	51.325	0.088	0.265
Full Sample N	30,898	30,727	31,128	31,256	31,369
Region Linear Time Trends	Yes	Yes	Yes	Yes	Yes

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Appendix Table 2.4. Marginal Effects of DCM on Mental Health by Subgroups with Region Specific Linear Time Trends

	PHQ-2	K6	MHCS	Fair/Poor MH	Excellent MH
Expected Sign	(-)	(-)	(+)	(-)	(+)
White Non-Hispanic					
Women	0.0007	0.0128	0.2803	-0.0037	0.0155
	(0.0153)	(0.0124)	(0.6535)	(0.0192)	(0.0313)
Men	-0.0113	-0.0224*	0.1501	0.0179	0.0243
	(0.0164)	(0.0132)	(0.6231)	(0.0230)	(0.0342)
Black Non-Hispanic					
Women	-0.0679***	-0.0327**	2.5583***	-0.0566**	0.0660
	(0.0235)	(0.0166)	(0.7831)	(0.0252)	(0.0440)
Men	-0.0008	0.0010	-0.2813	-0.0201	-0.0066
	(0.0240)	(0.0187)	(0.9336)	(0.0286)	(0.0525)
Hispanic					
Women	0.0072	0.0276*	0.9097	0.0156	-0.0042
	(0.0179)	(0.0156)	(0.7528)	(0.0218)	(0.030)
Men	-0.0004	0.0077	0.2586	-0.0061	0.0410
	(0.0175)	(0.0124)	(0.7288)	(0.0210)	(0.0371)
Full Sample Mean	0.068	0.039	51.325	0.088	0.265
Full Sample N	30,898	30,727	31,128	31,256	31,369
Region Linear Time Trends	Yes	Yes	Yes	Yes	Yes

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects, education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Appendix Table 2.5. Marginal Effects of DCM on Physical Health		
Data Source	MEPS	BRFSS
	PHCS	"Not Good" Physical Health Days"
Expected Sign	(+)	(-)
Full sample	0.1832	-0.1547
	(0.2183)	(0.0968)
By Race/Ethnicity		
White Non-Hispanic	0.1949	-0.1197
	(0.3375)	(0.1372)
Black Non-Hispanic	0.3287	0.1205
	(0.4365)	(0.4344)
Hispanic	0.2156	-0.1240
	(0.3451)	(0.2950)
By Gender		
Women	0.2837	-0.3035**
	(0.3354)	(0.1278)
Men	0.0615	-0.0143
	(0.2843)	(0.2087)
Full Sample Mean	54.137	2.197

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Table reports results from difference-in-differences models using 2006-2013 MEPS or BRFSS for those age 19-30 with standard errors in parentheses. All models control for age, year fixed effects, region fixed effects (MEPS) or state fixed effects (BRFSS), education attainment, race, ethnicity, sex, and marital status. All estimates are weighted.

Chapter 3

Recreational Cannabis Laws and Mental Health

By

Bryce J. Stanley

3.1 Introduction

Recent years have seen a considerable increase in prevalence rates for various mental illnesses in the United States. The percentage of U.S. adults with a mental illness, as well as those with a serious mental illness, has increased substantially in the past decade (Substance Abuse and Mental Health Services Administration, 2019). In 2020, roughly 1 in 5 U.S. adults experienced a mental illness and 1 in 20 experienced a serious mental illness (National Alliance on Mental Health, 2023; National Institute on Mental Health, 2023). Likewise, deaths by suicide have increased since 2000 (Marcottee and Hanson, 2023). However, barriers to care, such as high cost and supply shortages, still prevent many from receiving treatment.

There has also been a recent shift toward leniency in policy regarding cannabis. Many states have relaxed restrictions to cannabis access, legalizing both medical and recreational use of cannabis for adults over the age of 21; over 20 states have legislation permitting adults to use cannabis for recreational purposes. The impact of recreational cannabis laws (RCL) on mental health is largely unknown. These laws can theoretically influence a large set of health behaviors and health-related outcomes including but not limited to cannabis use, alcohol use, other drug use, and labor market outcomes. This set of behaviors and outcomes can in turn influence mental health status. As shortcomings in the mental health care sector persist, access to cannabis may allow for self-medication and mental health benefits. Cannabis legalization continues to be a divisive policy in the U.S., making a better understanding of the welfare implications a pressing issue. This study adds to the understanding of RCL by estimating the impact of RCL on mental health outcomes and finds robust evidence of mental health improvements.

This paper proceeds as follows: Section 3.2 discusses the policy landscape for cannabis. Previous work on cannabis laws with a focus on outcomes that have the potential for mental health implications are discussed in section 3.3. Section 3.4 gives an overview of the data used while section 3.5 goes over empirical methods employed. Results are discussed in section 3.6 with closing remarks in section 3.7.

3.2 Policy Background

Since the Controlled Substances Act of 1970, cannabis has remained illegal at the federal level in the U.S.. This Act marked cannabis as a Schedule I drug, resulting in it sharing classification with drugs that are considered to have no accepted medical use and a high risk of misuse, according to the United States Drug Enforcement Administration (Drug Enforcement Administration, 2023). However, many states have recently passed legislation that allows the use of cannabis for both medical and recreational purposes.

As of 2023, 22 states and the District of Columbia (D.C.) have passed RCL. These laws allow for those 21 years old and older to possess and use cannabis without the need for prior medical clearance and typically include quantity limits, such as the number of ounces or plants (ProCon, 2023). Of the 22 states and D.C. that have passed RCL, all had previously passed medical cannabis laws (MCL). These laws, unlike RCL, require prior medical approval before cannabis use is allowed. While RCL allow for the use of cannabis outside of medicinal purposes, they may still impact health outcomes, including mental health. In fact, roughly two thirds of people who use cannabis report doing so for either medical reasons only or recreational and medical reasons[23].

The sample period for this study is 2010-2020. Of the 22 states that have enacted a RCL, only 12 did so before the end of 2020. Table 3.1 shows the effective dates for each of these states and comes from Anderson and Rees (2023). Since 2020, ten more states have passed RCL, highlighting the fast-changing policy landscape for cannabis[24]. While these states are not considered "treated" in this study, results reported here remain important for their policy considerations. As the push for removing federal laws prohibiting cannabis use grows, understanding any possible mental health implications is key.

3.3 Previous Work and Theoretical Pathways

There are several possible pathways for RCL to impact mental health outcomes, primarily through various health behaviors. These behaviors include the use of cannabis, the use of other drugs like opioids, alcohol consumption, cigarette use, dietary decisions, and physical activity. Additionally, fear of arrest or labor supply may also offer pathways other than health behaviors. Many previous studies have examined these outcomes for both MCL and RCL.

Medical cannabis laws have been heavily researched. With respect to mental health, several studies have estimated impacts on both mental health status and deaths by suicide. Two studies find improvements in mental health coming from a reduction in "not good" mental health days reported, driven by younger adults (Sabia et al., 2015) and those most likely to use cannabis for medical reasons (Kalbfuß et al., 2018). Likewise, research suggest MCL also have the ability

[23] Author's calculations from the Behavioral Risk Factor Surveillance System.
[24] These states include CT, DE, MD, MO, MT, NJ, NM, NY, RI, and VA (ProCon, 2023).

to reduce more extreme mental health outcomes with a reduction in deaths by suicide (Anderson et al., 2014; Bartos et al., 2019). However, other work finds no impact of MCL on depressive symptoms in older adults (Nicholas and Maclean, 2019).

While past studies have modeled the impact of MCL on mental health outcomes, the effects of RCL could differ for several reasons. The marginal users of cannabis of MCL and RCL likely differ in several dimensions which could impact how RCL influence mental health. Additionally, RCL typically take effect later than MCL.

Previous studies have estimated the impact of RCL on health behaviors that could in turn have mental health implications. First, while the exact magnitude varies, multiple studies suggest an increase in cannabis use, typically around 2-3 percentage points (Abouk et al., 2021; Cerdá et al., 2020; Dave et al., 2022; Dave et al., 2023; Hollingsworth et al, 2022; Maclean et al., 2021). These studies often use state level data, which limits examination of heterogenous effects.

However, the impact of cannabis use on mental health remains unclear. Because cannabis is considered a Schedule I drug under federal law, medical trials are limited, leading to a gap in the understanding of the impacts of cannabis use. People with mental illnesses tend to use cannabis at a higher rate than those without (Konefal et al., 2019; Lev-Ran et al., 2013; National Institute on Drug Abuse, 2023; Rup et al., 2021), though it is unclear if the relationship is causal. Cannabis could perhaps be used more often by those with mental illnesses for a number of reasons, including as a form of self-medication. If self-medication exists, and is effective, then cannabis use could improve mental health while still being used at a higher rate by those with mental illnesses.

Various other complements and substitutes for cannabis may be impacted by RCL. For opioid use, there is growing evidence of a reduction in demand for prescription opioids following

RCL using claims data. These findings span from those enrolled in Medicare (Abouk et al., 2021), Medicaid (Raman and Bradford, 2022; Wen and Hockenberry, 2018), and employer sponsored insurance (Wen et al., 2021). While tobacco and cannabis could theoretically be either substitutes or complements, empirical evidence from RCL finds no effect, in general (Dave et al., 2023; Hollingsworth et al., 2022). However, Dave et al. (2023) presents some evidence of a reduction in electronic nicotine delivery systems. Some evidence also suggests moderate drinking increased following the implementation of RCL but not binge drinking (Macha et al., 2022), while other studies find no effect (Hollingsworth et al., 2022).

Other papers have studied additional outcomes that may have mental health effects. Multiple studies have found that RCL are associated with a reduction in arrest rates for cannabis possession (Hollingsworth et al., 2022; Gunadi and Shi, 2022). This reduction in arrest rates could reduce fear of arrest for both marginal and existing cannabis users, or their loved ones. March et al. (2022) finds that Washington state's RCL did not lead to an increase in obesity in the state, but rather might have led to a decrease. Additionally, there is little evidence of a change in employment outcomes following RCL (Dave et al., 2022).

The impact of RCL on these outcomes and behaviors can, in turn, impact mental health. One recent working paper has attempted to model the impact of RCL on mental health using "not good" mental health days as an outcome of interest (Borbely et al., 2022). Borbely et al. (2022) models the impact of both MCL and RCL on "not good" mental health days and finds an increase in reported days for younger adults following RCL. This study differs from Borbely et al. (2022) by modeling additional outcome variables that capture mental health status, allowing for effects across the distribution of "not good" days, and examining impacts for subgroups other than age.

3.4 Data

3.4.1 Recreational Cannabis Laws Dates

As mentioned previously, dates for RCL come from Anderson and Rees (2023) and follow effective dates rather than the date when recreational sales are allowed. For the purpose of this study, all RCL are considered the same within empirical models, regardless of quantity allowance or other differences. Each state is considered treated in the month following the RCL effective date, unless the date falls on the first on the month, then said month is treated. For example, Massachusetts enacted a RCL on December 15th, 2016 while Nevada did so on January 1st, 2017. Both states are considered treated for all observations starting on January 1st, 2017.

3.4.2 Behavioral Risk Factor Surveillance System

I use data from the Behavioral Risk Factor Surveillance System (BRFSS) for years 2010-2020 and those age 21-65. The BRFSS is a nationally representative repeated cross-sectional survey that collects data pertaining to various risky behaviors and health outcomes, including mental health.

Included in the BRFSS is a measure of mental health via the question "Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?". Respondents answer with a whole number ranging from zero to 30. While this variable is not as clinical as other measures of mental health - such as a PHQ-2 or formal diagnosis - it has been heavily used within health policy research, largely because of the other advantages the BRFSS presents such as state identifiers.

I use four variations of this measure to examine the spectrum of mental health status. Specifically, I use the number of days with "not good" mental health, a binary measure of reporting any days, a binary measure of reporting at least 14 days\footnote{The Center for Disease Control classifies reporting 14 or more days as having "frequent mental distress" (CDC, 2020)}, and a binary measured of reporting all 30 days. While the number of days reported may represent the mental health as an average, the three secondary measures may capture moderate and more severe mental health issues.

Table 3.2 shows the means and standard deviation of each measure for the full sample as well as subgroups for the "not good" mental health days measures. The average number of "not good" mental health days in the past month is roughly 4.2 for the full sample. Approximately 61 percent of people report zero days, with about 14 and 6 percent reporting at least 14 and all 30 days respectively. These numbers are, in general, larger for women, Asian non-Hispanic respondents, and those with lower levels of education.

The BRFSS also contains a measure of activity limitations with the question "During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?". While this measure encompasses both physical and mental health, it allows for insight into perhaps more severe health conditions. In an attempt to isolate changes in mental health limitations from physical health, I also utilize the BRFSS measure of physical health, which mirrors the "not good" mental health variable\footnote{The physical health question in the BRFSS is "Now thinking about your physical health, which includes physical illness and injury, for how many days during the past 30 days was your physical health not good?"}. I use three different variations of the activity limitation measure including the number of days, a measure of any days, and a measure of all 30 days.

Table 3.3 reports the same statistics as table 3.2 for days with an activity limitation. The full sample reports an average of 2.5 days with 76 percent of the sample reporting zero days. About 4 percent of the sample reports having an activity limitation in all 30 days. Patterns shown in table 3.3 are similar to table 3.2, with the exception of older adults reporting much higher days with activity limitation than younger adults.

Figures 3.1 and 3.2 show the distribution of responses to both the "not good" mental health days variable as well as the activity limitation measure, conditional on at least one day. Both histograms show a bunching of responses in 1-5 days; some spikes on round numbers such as 10, 15, or 20; and a large spike at all 30 days. It is worth noting that the most common value for both measures is zero, but the histograms reported at conditional on at least one day for best visualization of the distribution.

3.5 Empirical Methods

Following the empirical approaches of past work on RCL, I employ a staggered two-way fixed effects difference-in-differences model to isolate the impact of RCL on mental health outcomes using 2010-2020 BRFSS data for those 21-65 years-old[25]. I estimate the marginal effects of RCL using the following OLS model:

$$Y_{ist} = \beta_1 RCL_{st} + \delta_s + \tau_t + t\sigma_s + \varphi X_{ist} + \psi A_{st} + \varepsilon_{ist}$$

In these models, Y_{ist} are outcome variables discussed above. The primary explanatory variable of interest is RCL_{st}, a binary dummy variable equal to one if the observation is in a state and time combination that has enacted a RCL, zero otherwise, making β_1 the parameter of interest. δ_s and τ_t are state and year fixed effects while $t\sigma_s$ is state-specific linear time trends. X_{ist} and A_{st} are individual and state level controls.

State level controls include the state unemployment rate, poverty rate, maximum Temporary Assistance for Needy Families (TANF) levels for a family of three, state Earned Income Tax Credit (EITC) multiplier levels, state minimum wages, party of state governor, an indicator for Medicaid expansion under the Affordable Care Act, and MCL. The majority of state-level controls come from the Center for Poverty Research, while MCL dates come from Anderson and Rees (2023). Individual level controls include many measures available in the BRFSS. I include age, age squared, race and ethnicity, gender, month of survey, and education attainment level.

Given the staggered nature of RCL, the difference-in-differences strategy employed may suffer from bias by comparing early adopters to later adopters (Goodman-Bacon, 2021). To determine the potential degree of this bias, I use a Goodman-Bacon decomposition, which

[25] Results using a count model are qualitatively similar to OLS. Results are not robust to models without state specific linear time trends. All unreported results are available upon request.

reports the portion of comparisons that may be biased. I also utilize the Callaway-Sant'Anna difference-in-differences estimator which is designed to expunge bias presented by staggered treatment (Callaway-Sant'Anna, 2021).

3.6 Results

Results are reported first for "not good" mental health days for both the full sample and subgroups. Next, results for activity limitation days are shown, again for the full sample and subgroups. Robustness checks are then discussed followed by a supplemental analysis of cannabis use as a mechanism.

3.6.1 "Not Good" Mental Health Days Full Sample

Table 3.4 reports β_1 estimates with each of the four variations of the BRFSS mental health measure as the dependent variable. The first row shows estimates for the full sample followed by different subgroups. For the full sample, results suggest the adoption of RCL are associated with a reduction in "not good" mental health days in the past month of 0.1655 days, or about 4 percent. The likelihood of reporting at least 14 days is estimated to reduce by about 0.4 percentage points. Similarly, the likelihood of reporting all 30 days is estimated to reduce by 0.28 percentage points, or about 5 percent. Improvements, while only marginally significant, appear to take place on both the average as well as the more extreme cases of reporting 14 or more or even all 30 days.

3.6.2 "Not Good" Mental Health Days Subgroups

Dividing the sample into different subgroups suggests four particular populations are driving these results. Women are estimated to see a reduction of just over 0.20 of a day with "not good" mental health in the past month. Likewise, women also see a reduction of 1.15 percentage points in the likelihood of reporting any days. The likelihood or reporting 14 or more days as well as all 30 days are also estimated to decrease for women. Men are not found to have a significant change in the number of "not good" mental health days following the adoption of RCL.

Black non-Hispanic respondents show even stronger effects. In fact, the magnitude for "not good" mental health days for Black non-Hispanic respondents are more than four times the estimated effects for women. The enactment of RCL are associated with a reduction of 1.16 "not good" mental health days out of the past 30 days for Black non-Hispanic respondents. Likewise, the likelihood of reporting any days, 14 or more days, or all 30 days is reduced by 7.14, 4.10, and 2.74 percentage points respectively. White non-Hispanic, Asian non-Hispanic, and Hispanic respondents are not estimated to have a change in the number of days reporting "not good" mental health days.

When examining effects by education levels, those with a 4-year college education and above do not see a change in the number of days with "not good" mental health as a result of RCL. However, those without a 4-year college education are estimated to have a reduction of roughly 0.25 "not good" mental health days in the last month. This group also sees a reduction in the likelihood of reporting 14 or more days and a marginally significant effect when looking at any or all 30 days.

The last panel of table 3.4 separates effects by age. Older adults, those 45-65 years old, are estimated to have a reduction in the number of "not good" mental health days by 0.20 as well as a reduction in reporting 14 or more days or all 30 days. Younger adults, age 21-44, do not see a change in the number of "not good" mental health days, according to estimates.

These results suggest two findings: First, there appears to be a previously unknown benefit of RCL in improving mental health with effects present across the severity spectrum. Additionally, these benefits are concentrated in women, Black non-Hispanic respondents, those with lower levels of education, and older adults.

3.6.3 Activity Limitation Days Full Sample

Next, I model the impact of RCL on days with activity limitations with results shown in table 3.5. While this measure may capture changes to both severe physical and mental health conditions, it does give light to another measure of health status that is at least partially made up of mental health.

Table 3.5 reports results in the same format of table 3.4, with the full sample reported first followed by subgroups. For the full sample, estimates suggest RCL are associated with a reduction in days with an activity limitation in the past 30 days by 0.1375 days. This represents a reduction of about 5 percent. Similarly, results suggest the likelihood of reporting having an activity limitation in all 30 days decreased by 0.23 percentage points, or roughly 6 percent for the full sample.

3.6.4 Activity Limitation Days Subgroup

Much like results reported in table 3.4, this decrease in days with activity limitations appears to be driven by women, Black non-Hispanic respondents, those without a college education, and older adults. For women, RCL are associated with a decline of roughly 0.2 days with an activity limitation in the past month. This magnitude is about the same for those without a 4-year college education as well as older adults. For Black non-Hispanic respondents, the estimated reduction is nearly two-thirds of a day in the past month. Additionally, White non-Hispanic respondents also appear to see an improvement in days with an activity limitation with a modest reduction of 0.16 days in the past month.

3.6.5 "Not Good" Physical Health Days

What remains unclear with regards to the activity limitation results is if the improvement is driven by mental or physical health, or perhaps a combination of the two. While the BRFSS does not ask directly about activity limitations coming only from mental health, it does ask about "not good" physical health days. Table 3.6 reports results for models with "not good" physical health days as the dependent variable and follows the format of tables 3.4 and 3.5. In general, these results suggest RCL do not lead to changes in physical health for the full sample as well as most subgroups. However, Black non-Hispanic respondents are found to have a decrease in "not good" physical health days while Hispanic respondents see a perplexing increase. While these two groups are found to have a change in their physical health, the general results of table 3.6 may suggest the improvements in activity limitations likely come primarily from improvements to mental health for groups other than Black non-Hispanic respondents.

While it remains uncertain what avenues are leading to the reduction in activity limitation, the health improvements from this measure represent a positive effect of RCL. Regardless of the degree to which mental health is driving these improvements, a reduction in activity limitation shows a valuable health benefit of RCL that deserves a more thorough investigation.

3.6.6 Robustness Checks

The estimates reported so far come from staggered two-way fixed effects models that may suffer from a degree of biases. Some of the comparisons being made in the two-way fixed effects models are from "treated" to "never treated" units. However, other comparisons are from "early treated" to "late treated" units, due to the staggered roll out of RCL. To gauge the portion of "clean" comparisons used by the staggered difference-in-differences model, I employ a Goodman-Bacon decomposition which measures where of the comparisons made in my models come from. Results from this decomposition show the vast majority, 93.97 percent, of the comparisons made are from "treated" to "never treated" units. With a large percentage of "clean" comparisons, these results suggest the concerns of staggered treatment models may not be as alarming in this case as in others. Results from the Goodman-Bacon decomposition for "not good" mental health days are shown in appendix figure 3.1 and show the vast majority of comparisons are "treated" to "never treated" units as evident by these comparisons receiving the bulk of the weight used in the model.

Additionally, Callaway-Sant'Anna difference-in-differences estimates, which adjust two-way fixed effects models for the issues of staggered treatment, are largely similar to estimates from the two-way fixed effects models. Callaway-Sant'Anna estimates for "not good" mental health days are reported in appendix table 3.1, along with two-way fixed effect results for comparison. Callaway-Sant'Anna difference-in-differences results suggest RCL are associated with a decline of 0.1614 days for the full sample, a magnitude very similar to the two-way fixed effects model. For most groups Callaway-Sant'Anna results are similar in magnitude and often more precisely estimated than two-way fixed effect models. In fact, some results suggest improvements in mental health for select few groups not found to have effects in two-way fixed effects models, such as Asian non-Hispanic respondents.

For activity limitation days, Callaway-Sant'Anna estimates show a similar but smaller effect than the two-way fixed effects model for the full sample. Appendix table 3.2 reports Callaway-Sant'Anna estimates for activity limitation days. Overall, results from both the Goodman-Bacon decomposition and the Callaway-Sant'Anna estimator instill confidence in results reported from two-way fixed effects models for both variables in the BRFSS.

Next, to test if the two-way fixed effects models violate the parallel pre-trends assumption of difference-in-differences models, I use an event study framework for the full sample as well as subgroups that are found to have improvements following RCL. I conduct these event studies for both "not good" mental health days and activity limitation days separately. Results are reported in figures 3.3-3.13.

Figure 3.3 shows event study estimates for the full sample and "not good" mental health days. Pre-treatment estimates are not statistically different than zero and fairly precise, suggesting the parallel pre-trends assumption is not violated for this model. Additionally, the post-treatment estimates appear to be rather stable, though estimates are less precise in later post-treatment periods, possibly due to fewer treated states remaining as the post-treatment period increases[26].

Figures 3.4-3.7 show event studies for the subgroups that see mental health improvements in previous models. In general, event studies for subgroups also suggest the parallel pre-trends assumption is not violated, with the exception of Black non-Hispanic respondents. For the Black non-Hispanic subgroup, multiple pre-treatment period estimates are statistically different than zero and suggest the findings for this group may be less robust than others. Furthermore, the issues with the event study for Black non-Hispanic respondents may help explain the larger magnitude of the estimates for this group. Event studies for activity limitation days show, in general, that these models do not violate the parallel pre-trends assumption. Figure 3.8 shows event study estimates for the full sample, followed by figures 3.9-3.13 for subgroups. Appendix figures 3.2 and 3.3 show event study results using the Callaway-Sant'Anna estimator for the full sample and each of the two outcomes. It does not appear that the parallel pre-trends assumption is violated for Callaway-Sant'Anna models as well, with the exception of one pre-treatment year for activity limitation days. Event studies together suggest results reported, with the exception of Black non-Hispanic respondents and "not good" mental health days, are not biased due to different pre-treatment trends.

3.6.7 Supplemental Analysis of Cannabis Use as a Mechanism

Previous research may allow a window into possible mechanisms at play behind these findings. Of the possible mechanisms outlined previously, many are found to have no change in

[26] For example, only five states have RCL effective dates early enough to have observations in the 5 years post-treatment period.

response to RCL. However, cannabis use itself has been shown to increase (Abouk et al., 2021; Cerdá et al., 2020; Dave et al., 2022; Dave et al., 2023; Hollingsworth et al, 2022; Maclean et al., 2021). Additionally, other work has suggested a decline in both opioid use (Abouk et al., 202; Raman and Bradford, 2022; Raman et al., 2023; Wen and Hockenberry, 2018; Wen et al., 2021) and arrest rates (Hollingsworth et al., 2022; Gunadi and Shi, 2022). These three pathways may play a particularly key role in improving mental health outcomes following RCL. It is not clear, however, in what ways these mechanisms might be impacting mental health or why some groups are impacted more than others.

In an attempt to gauge the role of cannabis use, I build upon past work on cannabis use and RCL by investigating heterogeneous effects. Past work has been limited to the use of state level data which has restricted the ability of researchers to examine who the marginal user of cannabis is following RCL. Using the same data, I test if state level demographics influence the impact of RCL on cannabis use in a way that may help explain my findings. Specifically, I test if the percentage of the state that is female, Black non-Hispanic, without a 4-year college degree, or 45-65 years old changes the impact of RCL on cannabis use. While there is limited variation in these measures at the state level, especially for percentage female, this represents a possible method of estimating the marginal user of cannabis. If states with a higher percentage of these groups also see a larger increase in cannabis use following RCL, then cannabis use may be a primary mechanism.

Like past research, I use state level cannabis use data from the National Survey of Drug Use and Health (NSDUH), and the difference-in-differences approach outlined previously. The public NSDUH files are pooled at two-year averages, making my sample period 2010-2011 to 2018-2019. I consider an observation to be treated if the state had a RCL for at least half of the pooled two year period. The NSDUH data contains measures of the percent of the state's adults that has used cannabis in the past month as well as past year. I first replicate past findings by modeling the impact of RCL on cannabis use. Next, to examine if the groups I find mental health improvements for are associated with a higher likelihood of being a marginal user, I use interaction terms for RCL and percent of the state for each group.

Results are reported in table 3.7, first for the average impact of RCL followed by separate models with interaction terms. The first panel suggests RCL are associated with 1.7 percentage point increase in cannabis use in the past month and a 2 percentage point increase in the past year. However, when examining models with interaction terms, I do not find evidence of stronger effects based on states' percentage of demographics that align with mental health improvements. The lack of evidence of heterogeneous effects may be due to the limited variation and sample size when using state level data. Additionally, it is possible cannabis use is the primary mechanism at play even if marginal use is not more likely for these groups as it is possible they respond differently to cannabis use. Thus, the results reported here should not be interpreted as a ruling out of cannabis use as a mechanism. Future work that has access to individual level data may investigate the marginal user of cannabis in more detail.

3.7 Discussion

This study evaluates the impact of RCL on mental health outcomes using large survey data and a difference-in-differences approach. The estimates presented in this study add to a growing literature examining the impact of RCL on various health behaviors and health outcomes. This is the first study, to my knowledge, to show mental health improvements following RCL.

Estimates for the full sample, while are marginally significant in two-way fixed effects models, represent a roughly 4 percent decline in the number of "not good" mental health days following RCL. To put these estimates in additional context, the magnitude reported for the full sample of 0.1655 days is roughly equal to the estimated impact of a 25 percent increase in the minimum wage[27] (Horn et al., 2017), the magnitude found when examining MCL[28] (Kalbfuß et al., 2018), and one-fifth the effect found following the Affordable Care Act's Medicaid expansion[29] (Lee and Porell, 2020).

Likewise, activity limitation days are also estimated to decline following RCL. Given the lack of evidence of physical health improvements, changes in activity limitation days are possibly driven by improvements to mental health rather than physical health. Results for both outcomes are largely robust to event study analysis and Callaway-Sant'Anna estimators.

The models reported in this study suggest improvements in mental health across the severity spectrum following RCL with strongest results for women, older adults, those without a college education, Black non-Hispanic respondents, and perhaps White non-Hispanic respondents. These groups may be driving results due to a higher likelihood of being a marginal cannabis user, or for other reasons such as higher previous mental health issues or lower access to care. Future work may examine why these groups in particular are driving results.

[27] This result is only found only for women in Horn et al. (2017).

[28] Estimates in Kalbfuß et al. (2018) range from 0.18 to 0.23 days for main models.

[29] Lee and Porell (2020) find Medicaid expansion is associated with a decrease of roughly 0.80 in "not good" mental health days in the past month. Like this study, Lee and Porell (2020) also find estimates are largest for Black respondents.

Supplemental analysis does not find evidence that these groups are more likely to be marginal cannabis user, though the lack of individual level data limits estimates.

Future research may focus on various possible mechanisms, including cannabis use in more detail, opioid use, and fear of arrest. As MCL have been estimated to reduce deaths by suicide, future work may also examine the role of RCL and suicides. Likewise, as many more states have enacted RCL in the years since my sample period, updated work may be able to test for differential impacts by state characteristics such as mental health care shortages or details in RCL. As mental health continues to be a pressing issue, RCL may offer a low-cost pathway for improvement. The public debate around cannabis access continues to unfold and the possible positive mental health implications are critical for policy considerations.

Figures and Tables

Figure 3.1. Histogram of "Not Good" Mental Health Days (Conditional on at least One Day)

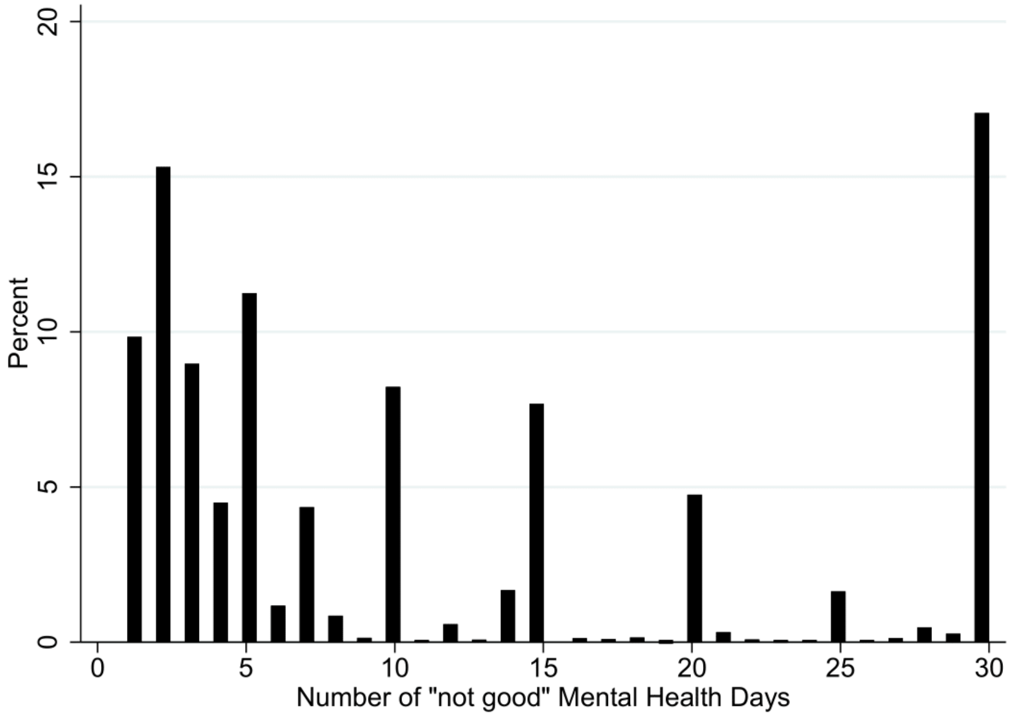


Figure represents the percent of respondents answering each whole number between zero and 30 when asked how many of the past 30 days were spent in "not good" mental health, conditional on at least one day.

Figure 3.2. Histogram of Activity Limitation Days (Conditional on at least One Day)

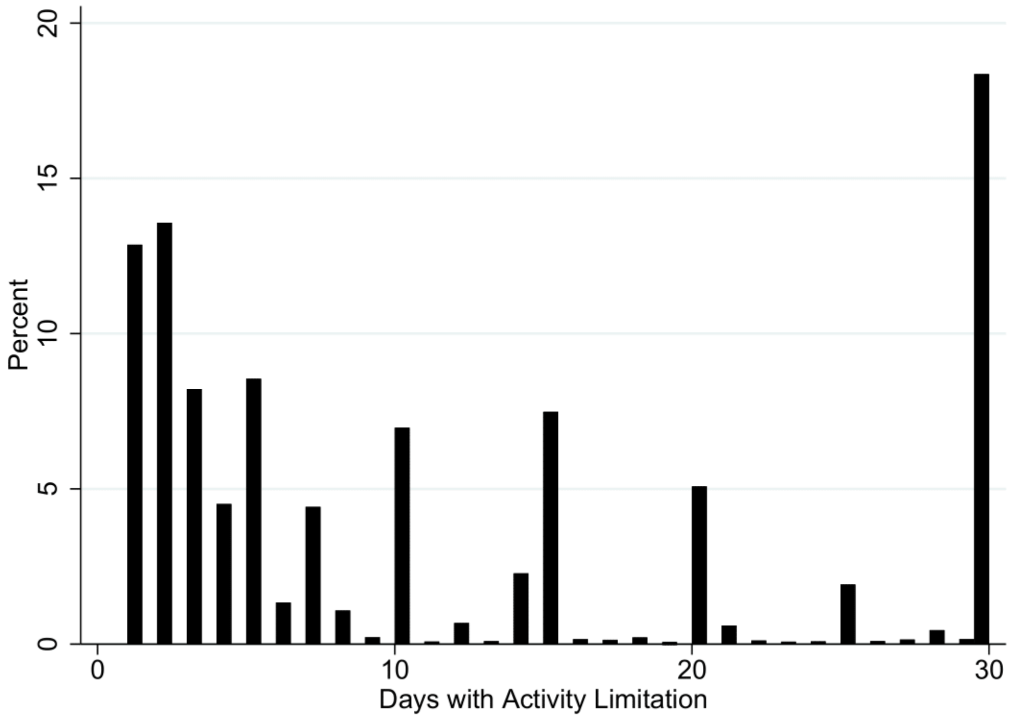
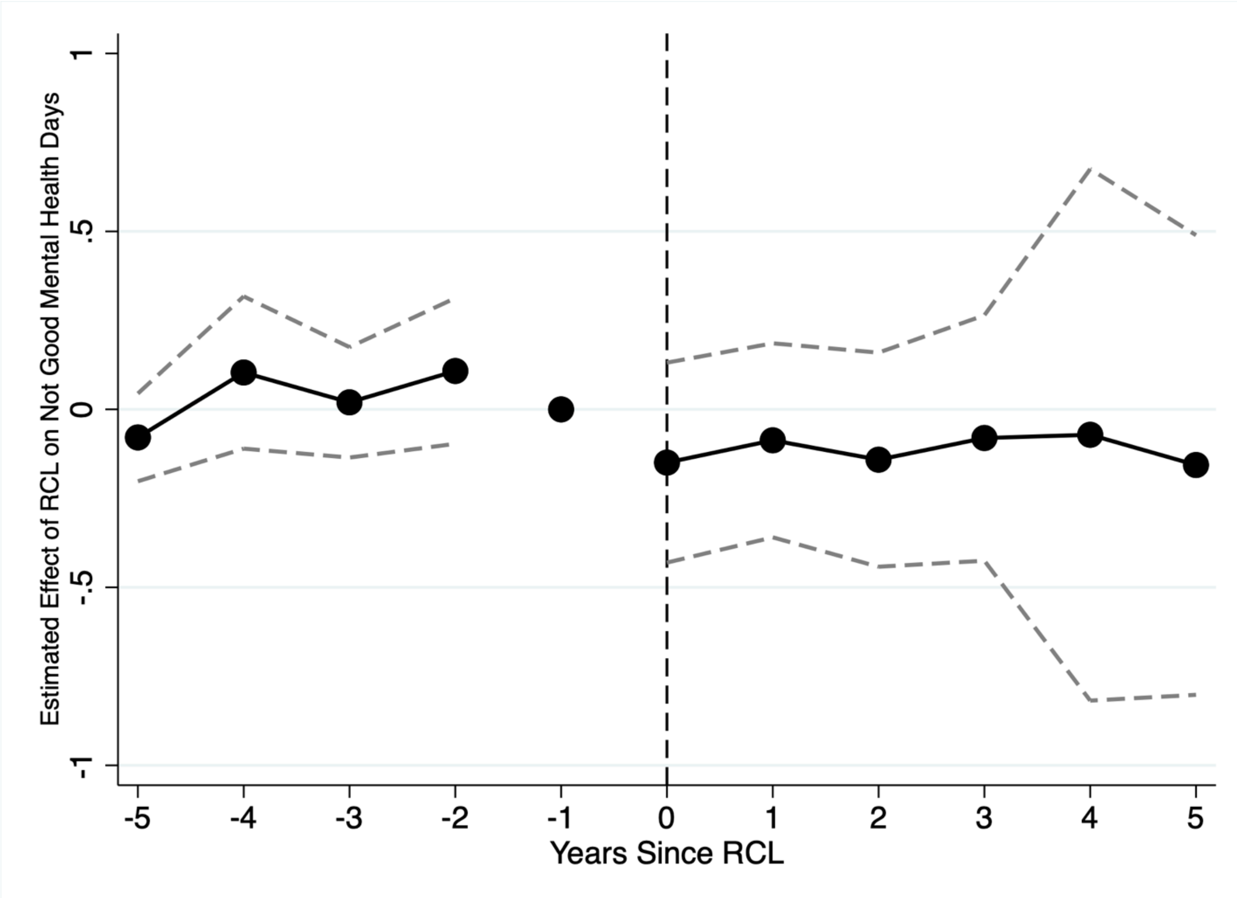


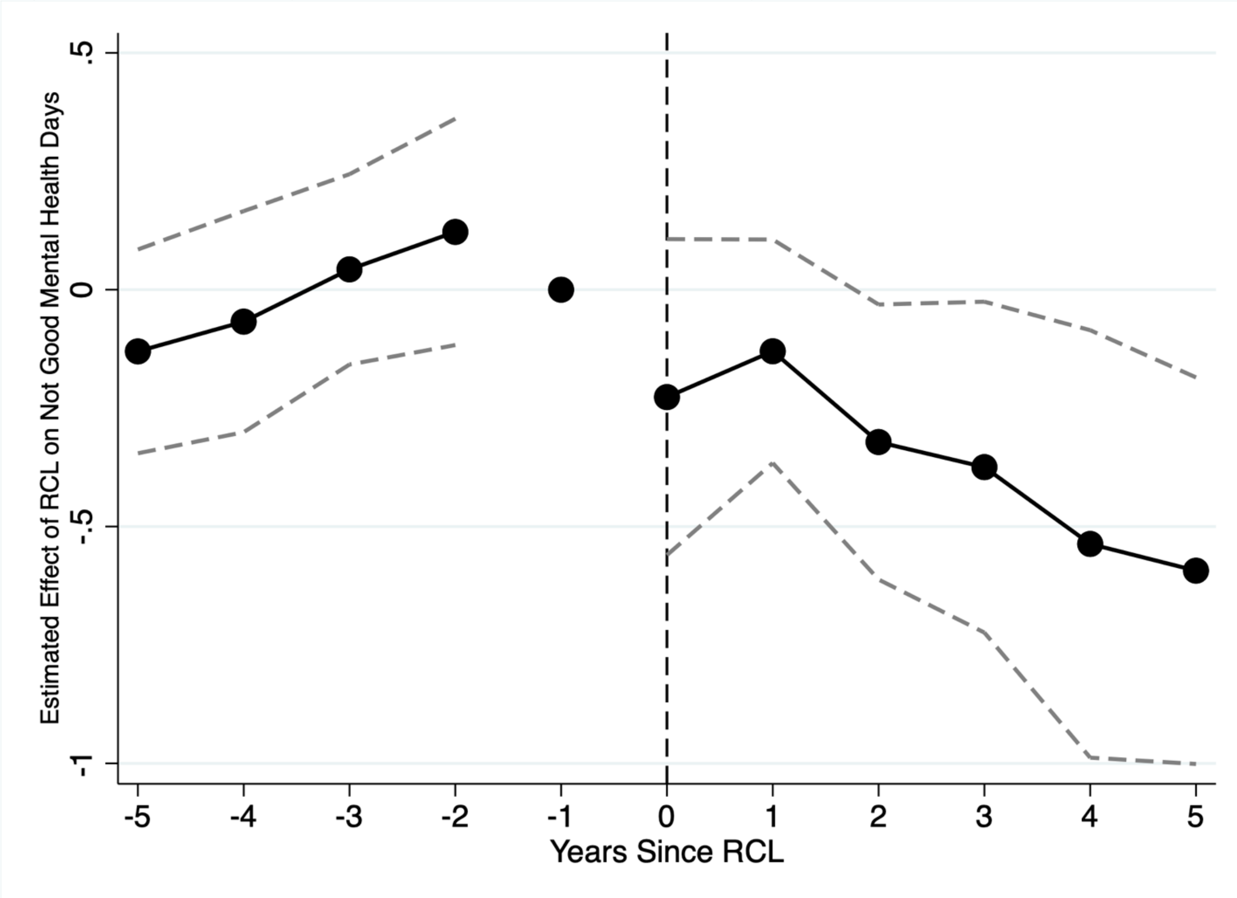
Figure represents the percent of respondents answering each whole number between zero and 30 when asked how many of the past 30 days were spent with activity limitation, conditional on at least one day.

Figure 3.3. Event Study for "Not Good" Mental Health Days - Full Sample



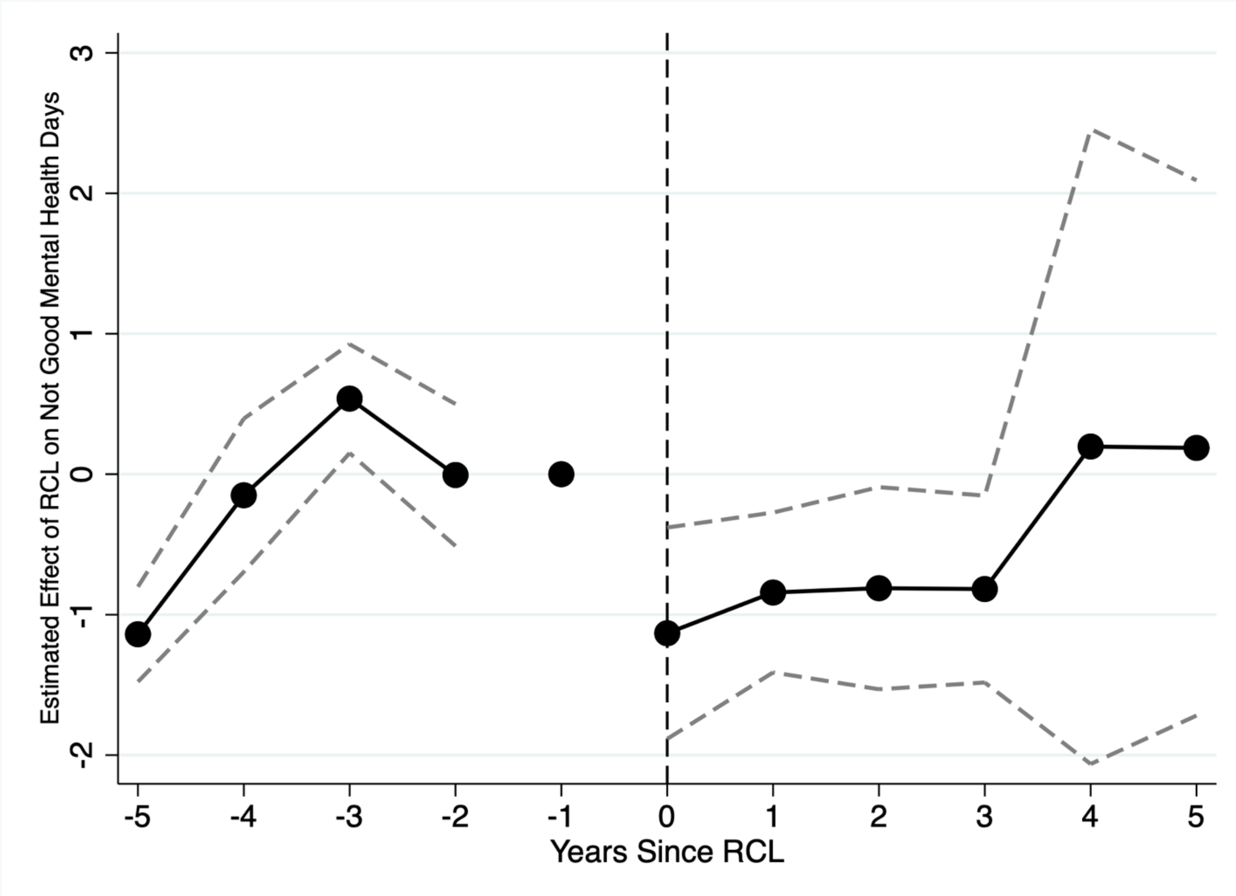
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.4. Event Study for "Not Good" Mental Health Days – Women



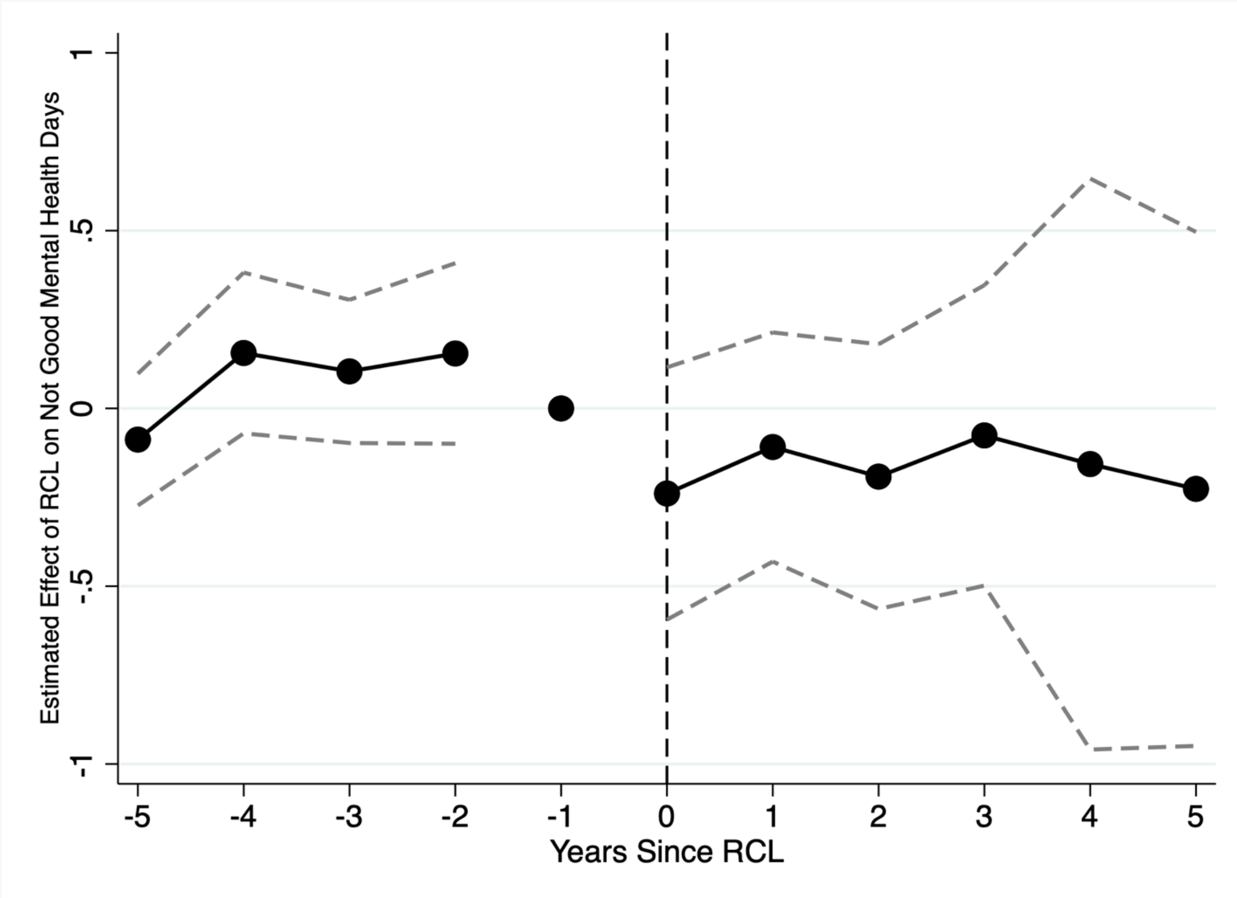
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.5. Event Study for "Not Good" Mental Health Days - Black Non-Hispanic Respondents



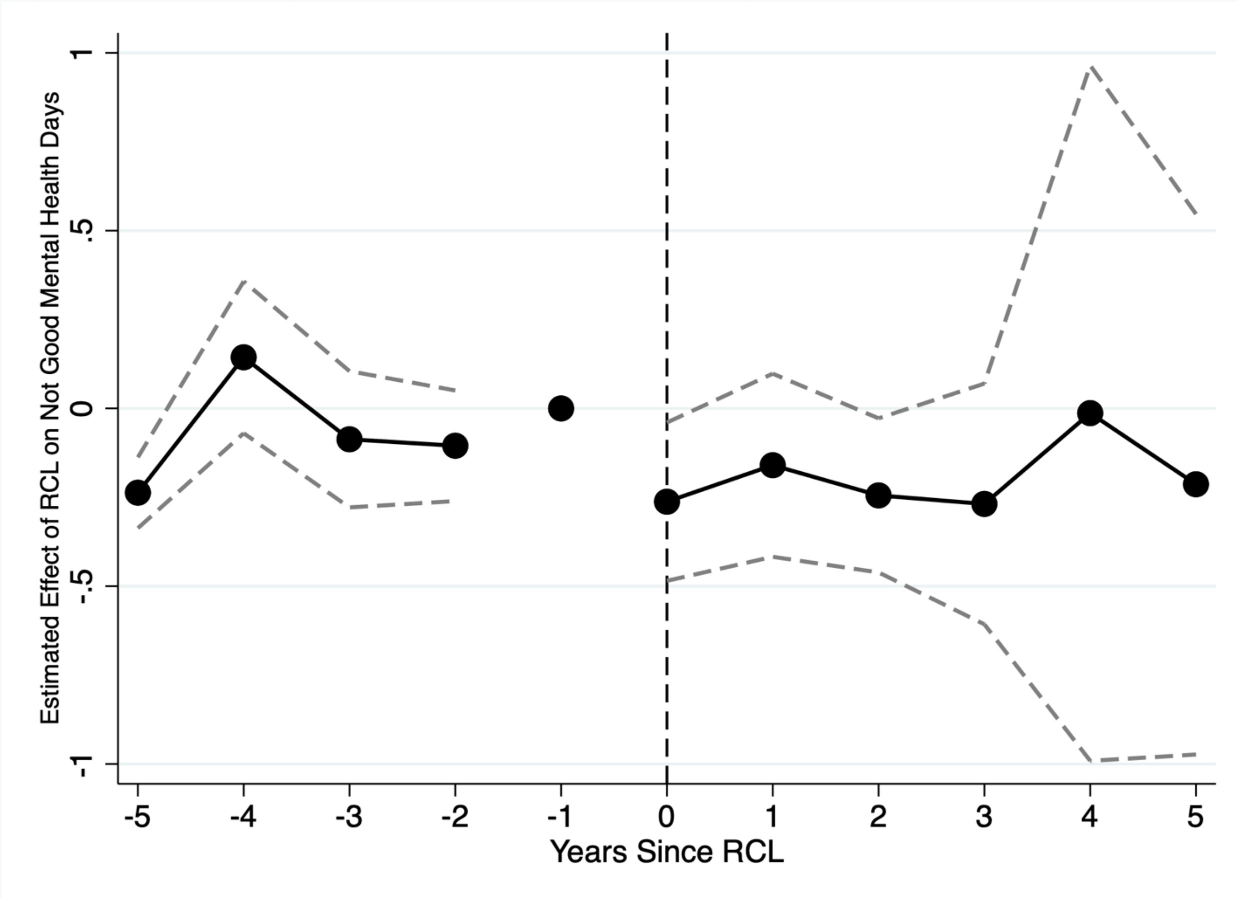
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.6. Event Study for "Not Good" Mental Health Days - No College



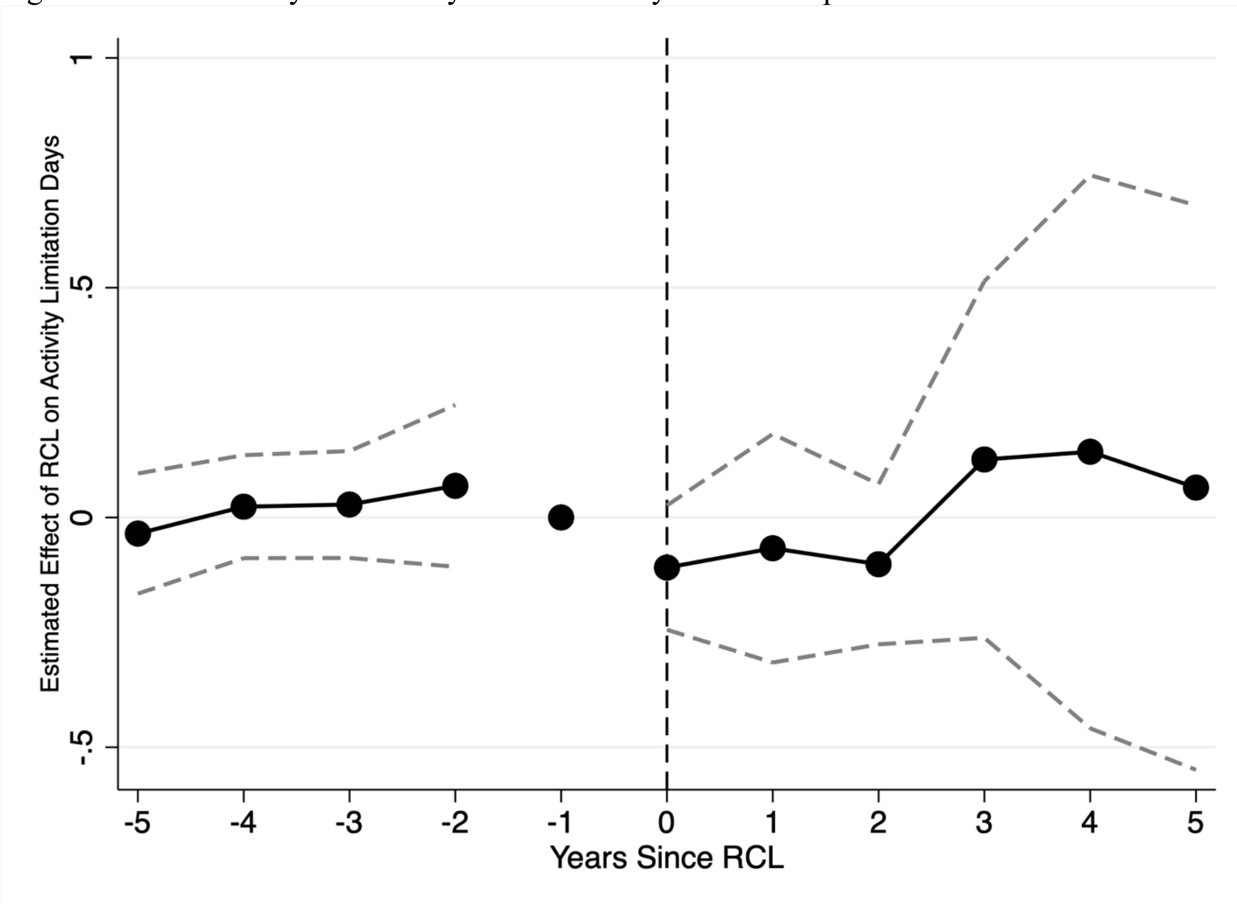
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.7. Event Study for "Not Good" Mental Health Days - 45-65-Year-olds



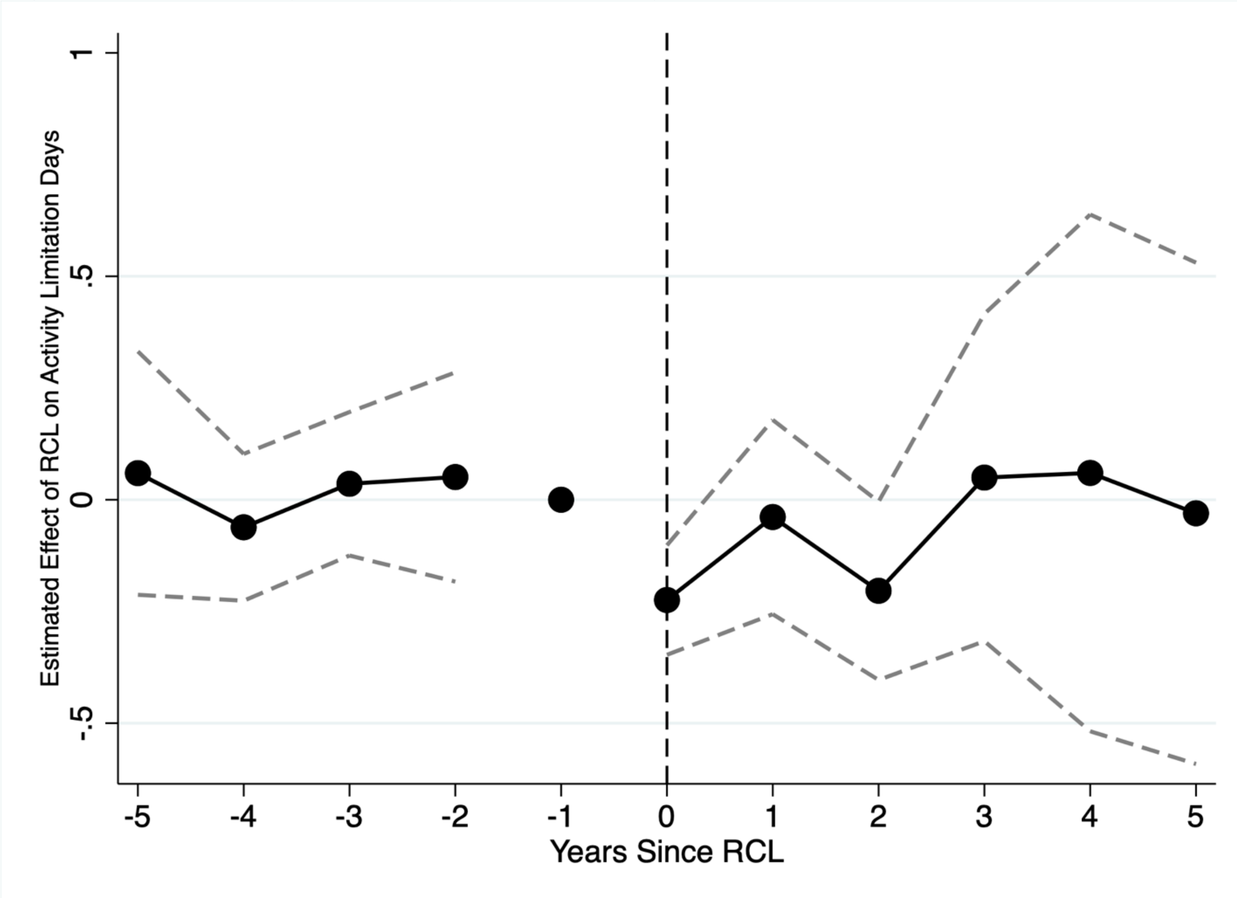
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.8. Event Study for Activity Limitation Days - Full Sample



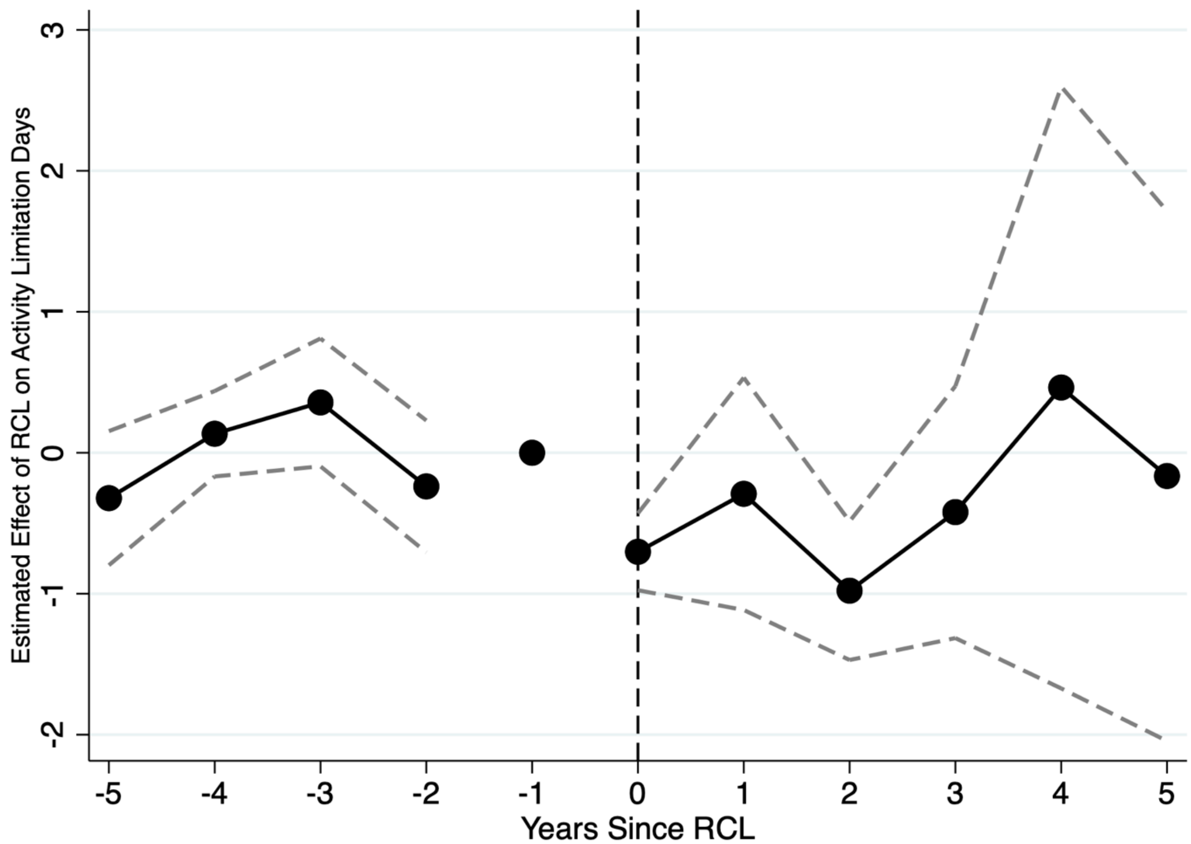
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.9. Event Study for Activity Limitation Days – Women



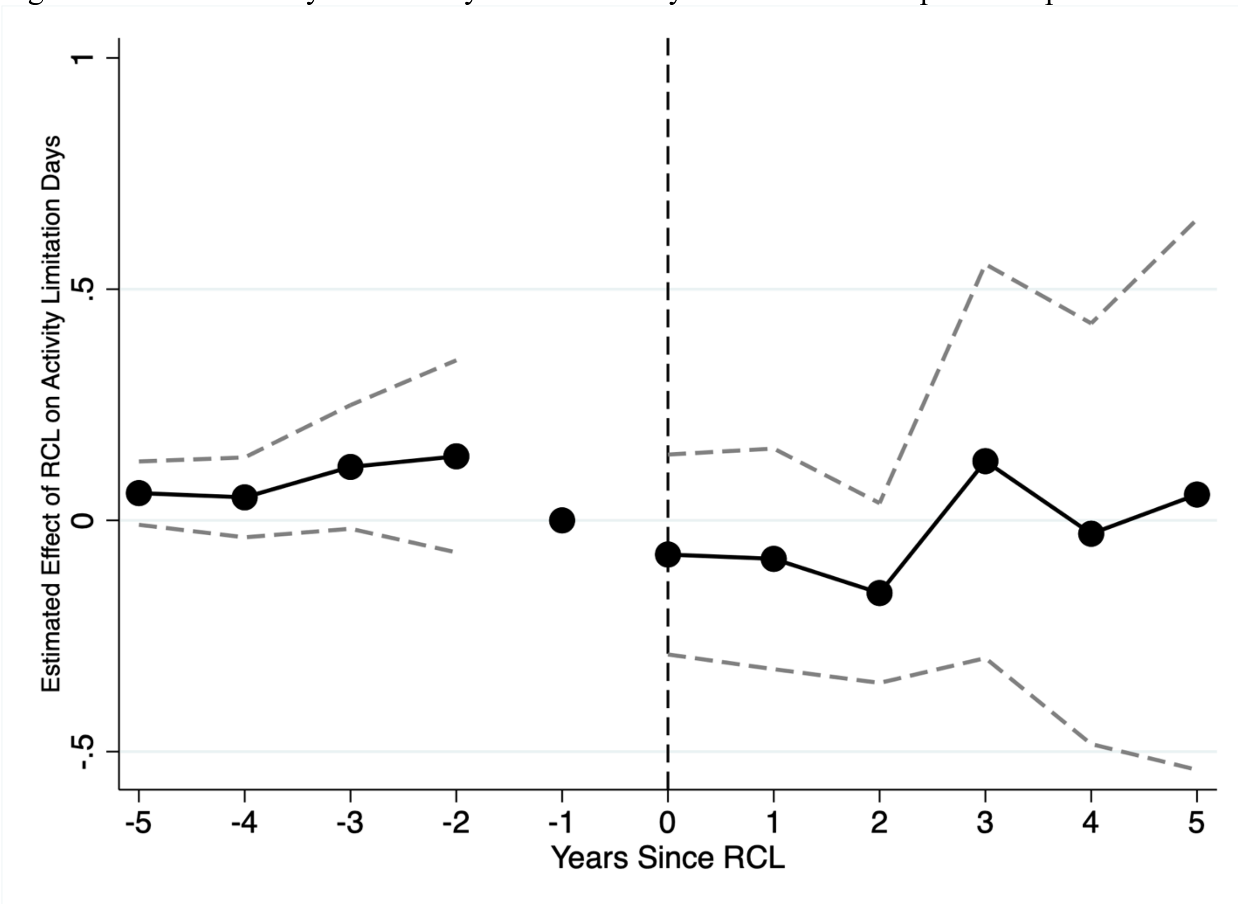
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.10. Event Study for Activity Limitation Days - Black Non-Hispanic Respondents



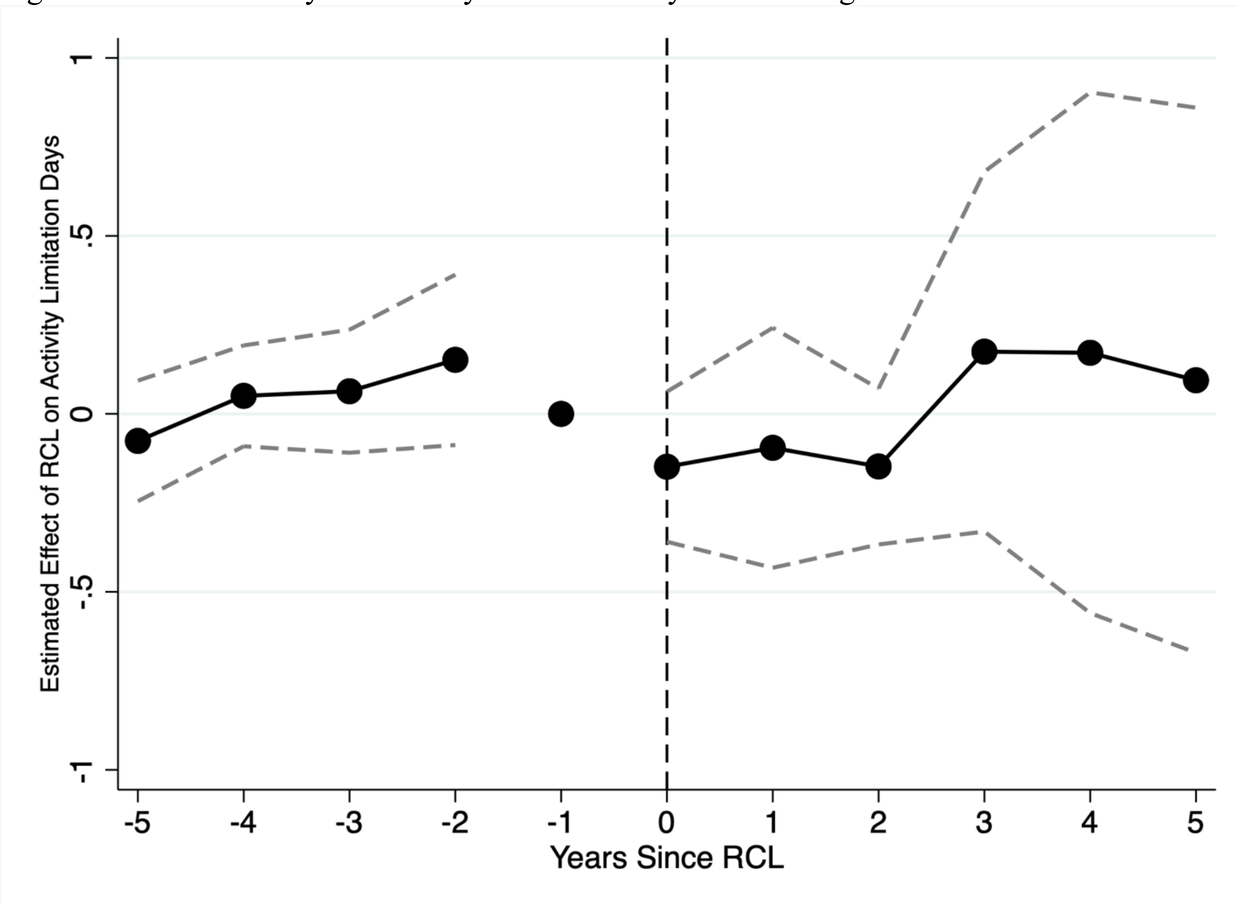
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.11. Event Study for Activity Limitation Days - White Non-Hispanic Respondents



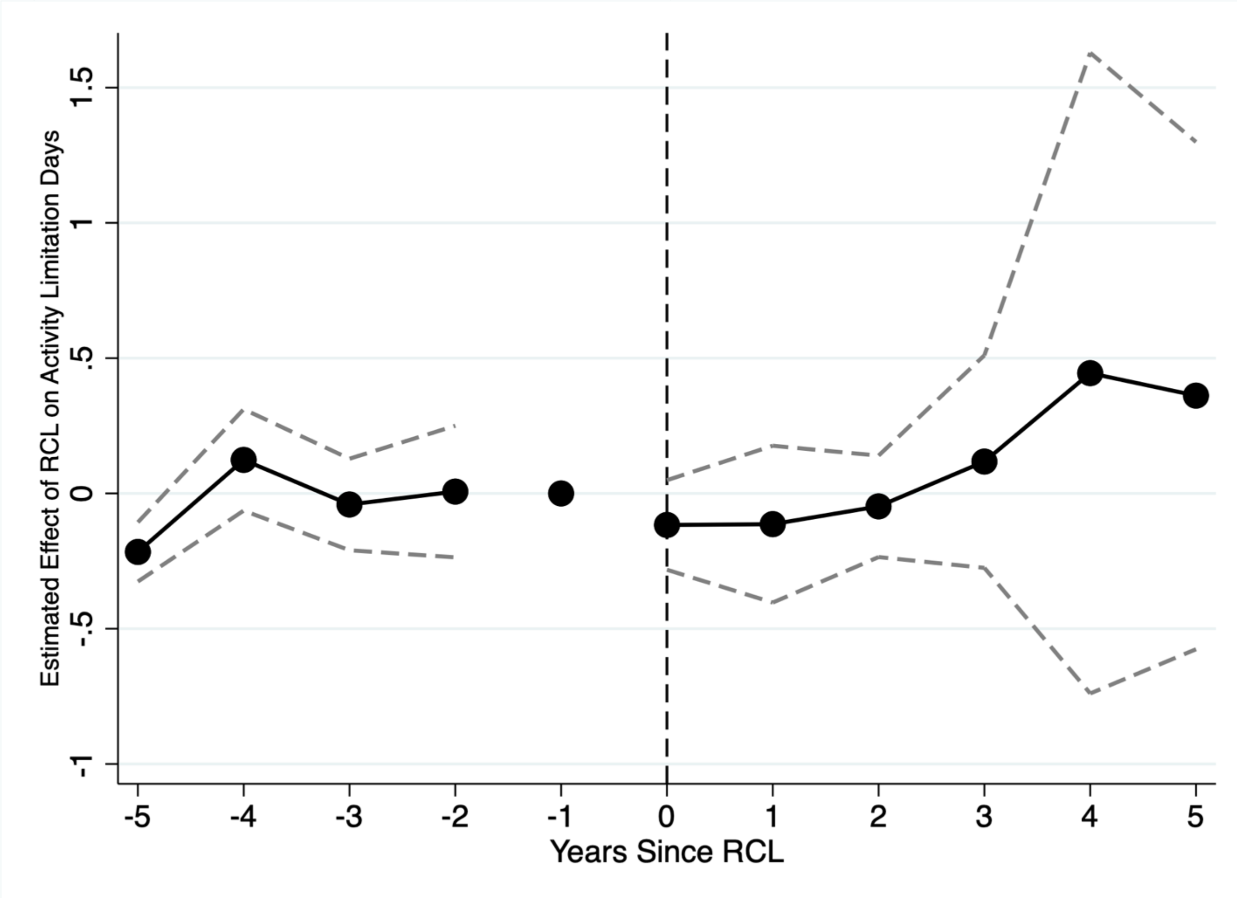
Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.12. Event Study for Activity Limitation Days - No College



Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Figure 3.13. Event Study for Activity Limitation Days - 45-65-Year-olds



Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. All event study models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Table 3.1. Recreational Cannabis Laws Dates

State	RCL Effective Date	Date "Treated" in Models
Alaska	2/24/2015	3/1/2015
Arizona	11/30/2020	12/1/2020
California	11/9/2016	12/1/2016
Colorado	12/10/2012	1/1/2013
Illinois	1/1/2020	1/1/2020
Maine	1/31/2017	2/1/2017
Massachusetts	12/15/2016	1/1/2017
Michigan	12/6/2018	1/1/2019
Nevada	1/1/2017	1/1/2017
Oregon	7/1/2015	7/1/2015
Vermont	7/1/2018	7/1/2018
Washington	12/6/2012	1/1/2013

Dates from Anderson and Rees (2023).

Table 3.2. Means and Standard Deviations for "Not Good" Mental Health Days					
	N	Number of Days	Any Days	14 Plus Days	All 30 Days
Full Sample	3,198,444	4.167	0.386	0.143	0.062
		(8.298)	(0.487)	(0.351)	(0.242)
By Gender					
Men	1,402,982	3.504	0.331	0.122	0.053
		(7.781)	(0.471)	(0.328)	(0.224)
Women	1,795,462	4.819	0.440	0.164	0.071
		(8.729)	(0.496)	(0.370)	(0.257)
By Race/Ethnicity					
White Non-Hispanic	2,363,370	4.247	0.396	0.143	0.063
		(8.347)	(0.489)	(0.350)	(0.243)
Black Non-Hispanic	281,523	4.476	0.387	0.158	0.070
		(8.644)	(0.487)	(0.365)	(0.254)
Asian Non-Hispanic	67,153	6.230	0.490	0.217	0.110
		(9.942)	(0.500)	(0.412)	(0.307)
Hispanic	271,554	3.840	0.361	0.138	0.056
		(7.979)	(0.480)	(0.345)	(0.230)
By Education					
No College	1,933,560	4.745	0.397	0.168	0.076
		(8.931)	(0.489)	(0.280)	(0.374)
College	1,252,314	2.869	0.361	0.086	0.030
		(6.465)	(0.480)	(0.280)	(0.172)
By Age					
21-44 Years Old	1,223,231	4.344	0.421	0.146	0.059
		(8.225)	(0.494)	(0.353)	(0.237)
45-65 Years Old	1,975,213	3.958	0.345	0.141	0.065
		(8.378)	(0.475)	(0.348)	(0.247)

Data from the 2010-2020 BRFSS. Means and standard deviations are gathered using sampling weights.

Table 3.3. Means and Standard Deviations for Activity Limitation Days				
	N	Number of Days	Any Days	All 30 Days
Full Sample	3,218017	2.564	0.240	0.039
		(6.869)	(0.427)	(0.194)
By Gender				
Men	1,412,132	2.312	0.211	0.039
		(6.684)	(0.443)	(0.193)
Women	1,805,885	2.812	0.268	0.040
		(7.037)	(0.443)	(0.195)
By Race/Ethnicity				
White Non-Hispanic	2,377,701	2.560	0.241	0.040
		(6.878)	(0.428)	(0.195)
Black Non-Hispanic	283,312	2.940	0.252	0.045
		(7.337)	(0.434)	(0.207)
Asian Non-Hispanic	67,606	4.130	0.334	0.067
		(8.593)	(0.472)	(0.250)
Hispanic	273,780	2.373	0.228	0.035
		(6.557)	(0.420)	(0.183)
By Education				
No College	1,958,648	3.068	0.257	0.049
		(7.556)	(0.437)	(0.217)
College	1,258,930	1.412	0.200	0.016
		(4.759)	(0.400)	(0.126)
By Age				
21-44 Years Old	1,231,799	2.021	0.231	0.0249
		(5.821)	(0.421)	(0.156)
45-65 Years Old	1,988,060	3.203	0.250	0.056
		(7.878)	(0.433)	(0.230)

Data from the 2010-2020 BRFSS. Means and standard deviations are gathered using sampling weights.

Table 3.4. Marginal Effects of RCL on "Not Good" Mental Health Days				
	Number of Days	Any Days	14 Plus Days	All 30 Days
Full Sample	-0.1655*	-0.0094	-0.0042*	-0.0028*
	(0.0913)	(0.0080)	(0.0025)	(0.0015)
By Gender				
Men	-0.1254	-0.0072	-0.0029	-0.0016
	(0.1052)	(0.0103)	(0.0037)	(0.0022)
Women	-0.2070**	-0.0115*	-0.0053*	-0.0039**
	(0.1038)	(0.0069)	(0.0028)	(0.0020)
By Race/Ethnicity				
White Non-Hispanic	-0.0744	-0.0044	-0.0014	-0.0010
	(0.1365)	(0.0084)	(0.0050)	(0.0020)
Black Non-Hispanic	-1.1627***	-0.0714***	-0.0410***	-0.0274***
	(0.3433)	(0.0166)	(0.0086)	(0.0087)
Asian Non-Hispanic	0.1390	0.0200	-0.0064	0.0059
	(0.4402)	(0.0150)	(0.0151)	(0.0170)
Hispanic	-0.0630	-0.0032	0.0019	0.0014
	(0.1568)	(0.0108)	(0.0069)	(0.0035)
By Education				
No College	-0.2558**	-0.0166*	-0.0079**	-0.0041*
	(0.1232)	(0.0095)	(0.0031)	(0.0024)
College	-0.0034	0.0054	0.0023	-0.0008
	(0.0636)	(0.0078)	(0.0024)	(0.0018)
By Age				
21-44 Years Old	-0.1428	-0.0098	-0.0013	-0.0024
	(0.1347)	(0.0107)	(0.0036)	(0.0023)
45-65 Years Old	-0.2017***	-0.0096	-0.0080***	-0.0033**
	(0.0840)	(0.0065)	(0.0027)	(0.0017)
Full Sample N	3,140,276	3,140,276	3,140,276	3,140,276
Full Sample Mean	4.167	0.386	0.143	0.062

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Models use data from the 2010-2020 BRFSS using sampling weights. All models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Table 3.5. Marginal Effects of RCL on Days with Activity Limitation			
	Number of Days	Any Days	All 30 Days
Full Sample	-0.1375***	-0.0057	-0.0023**
	(0.0433)	(0.0047)	(0.0011)
By Gender			
Men	-0.0773	-0.0012	-0.0016
	(0.0849)	(0.0067)	(0.0020)
Women	-0.1944***	-0.0100***	-0.0029***
	(0.0429)	(0.0034)	(0.0009)
By Race/Ethnicity			
White Non-Hispanic	-0.1631**	-0.0059	-0.0031**
	(0.0750)	(0.0055)	(0.0016)
Black Non-Hispanic	-0.6366***	-0.0446***	-0.0119**
	(0.1679)	(0.0145)	(0.0049)
Asian Non-Hispanic	-0.4900	-0.0253	-0.0004
	(0.3089)	(0.0152)	(0.0059)
Hispanic	0.0600	0.0047	0.0014
	(0.1242)	(0.0073)	(0.0025)
By Education			
No College	-0.2113***	-0.0128***	-0.0041**
	(0.0703)	(0.0048)	(0.0020)
College	0.0056	0.0091	0.0011
	(0.0320)	(0.0078)	(0.0014)
By Age			
21-44 Years Old	-0.1187	-0.0026	-0.0009
	(0.0776)	(0.0086)	(0.0015)
45-65 Years Old	-0.1675***	-0.0099***	-0.0041***
	(0.0514)	(0.0030)	(0.0012)
Full Sample N	3,140,276	3,140,276	3,140,276
Full Sample Mean	2.564	0.240	0.039

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Models use data from the 2010-2020 BRFSS using sampling weights. All models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Table 3.6. Marginal Effects of RCL on "Not Good" Physical Health Days			
	Number of Days	Any Days	All 30 Days
Full Sample	-0.0100	0.0048	-0.0004
	(0.0578)	(0.0064)	(0.0013)
By Gender			
Men	0.0266	0.0104	-0.0023
	(0.1142)	(0.0100)	(0.0023)
Women	-0.0463	-0.0007	0.0015
	(0.0443)	(0.0036)	(0.0015)
By Race/Ethnicity			
White Non-Hispanic	-0.0667	-0.0027	-0.0005
	(0.0761)	(0.0040)	(0.0021)
Black Non-Hispanic	-0.8858***	-0.0190**	-0.0248***
	(0.1528)	(0.0084)	(0.0048)
Asian Non-Hispanic	-0.0755	0.0501***	-0.0075
	(0.3372)	(0.0135)	(0.0106)
Hispanic	0.4829***	0.0324***	0.0084**
	(0.1014)	(0.0083)	(0.0042)
By Education			
No College	-0.0331	0.0013	-0.0009
	(0.0777)	(0.0067)	(0.0016)
College	0.0305	0.0122	0.0001
	(0.0464)	(0.0076)	(0.0013)
By Age			
21-44 Years Old	0.0199	0.0059	0.0001
	(0.0834)	(0.0096)	(0.0014)
45-65 Years Old	-0.0536	0.0028	-0.0012
	(0.0603)	(0.0035)	(0.0019)
Full Sample N	3,140,276	3,140,276	3,140,276
Full Sample Mean	3.605	0.684	0.121

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Models use data from the 2010-2020 BRFSS using sampling weights. All models include state, year, and month fixed effects along with state linear time trend. Models control for demographics such as age, age squared, race, ethnicity, and education level. State level controls include unemployment rate, poverty rate, maximum TANF levels, state EITC levels, minimum wages, party of state governor, Medicaid expansion, and medical cannabis laws. Standard errors are clustered at the state level.

Table 3.7. Marginal Effects of RCL on Cannabis Use for those 18 Years Old and Older		
	Any Use in Past Month	Any Use in Past Year
RML	0.0172**	0.0203**
	(0.0068)	(0.0084)
With Interaction Terms (Separate Models)		
RML * Percent Women	-0.4958	-0.8009
	(0.4296)	(0.6716)
RML * Percent Black	-0.2230	-0.3081
	(0.2823)	(0.3228)
RML * Percent No College	0.0655	0.0350
	(0.0715)	(0.1078)
RML * Percent 45-65-Year-olds	0.0567	0.0054
	(0.2059)	(0.2636)
Sample N	459	459
Sample Mean	0.086	0.137
Data Source	NSDUH	NSDUH

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Models use state level data from the NSDUH public use files for those 18 years old and older. Data in the NSDUH public use files are reported in pooled two-year averages. I use data from 2010-2011 to 2018-2019. All models are estimated with OLS and include state and year fixed effects along with state level controls for MCL, minimum wages, EITC levels, poverty rate, unemployment rate, party of governor, Medicaid expansion, and state level averages for age, percent female, percent Black non-Hispanic, and percent with a 4-year college degree and come from the American Community Survey. Models are weighted using population 18 and older and standard errors are clustered at the state level. Interaction terms are reported for separate models i.e. this table reports results from 5 models. Results are similar when sample is limited to those 26 and older.

Appendix Table 3.1. Marginal Effects of RCL on "Not Good" Mental Health Days for TWFE and CS

	Number of Days		Any Days		14 Plus Days		All 30 Days	
Full Sample	-0.1655*	-0.1614**	-0.0094	-0.0072	-0.0042*	-0.0050**	-0.0028*	-0.0033**
	(0.0913)	(0.0672)	(0.0080)	(0.0053)	(0.0025)	(0.0025)	(0.0015)	(0.0015)
By Gender								
Men	-0.1254	-0.0953	-0.0072	-0.0039	-0.0029	-0.0039	-0.0016	-0.0023
	(0.1052)	(0.0750)	(0.0103)	(0.0058)	(0.0037)	(0.0030)	(0.0022)	(0.0014)
Women	-0.2070**	-0.2074**	-0.0115*	-0.0075	-0.0053*	-0.0061*	-0.0039**	-0.0066***
	(0.1038)	(0.0802)	(0.0069)	(0.0051)	(0.0028)	(0.0032)	(0.0020)	(0.0013)
By Race/Ethnicity								
White Non-Hispanic	-0.0744	-0.1235**	-0.0044	-0.0009	-0.0014	-0.0036	-0.0010	-0.0052***
	(0.1365)	(0.0621)	(0.0084)	(0.0051)	(0.0050)	(0.0022)	(0.0020)	(0.0014)
Black Non-Hispanic	-1.1627***	-0.4165**	-0.0714***	-0.0250	-0.0410***	-0.0325***	-0.0274***	0.0045
	(0.3433)	(0.1885)	(0.0166)	(0.0162)	(0.0086)	(0.0090)	(0.0087)	(0.0058)
Asian Non-Hispanic	0.1390	-1.0395***	0.0200	0.0032	-0.0064	-0.0336***	0.0059	-0.0303***
	(0.4402)	(0.3108)	(0.0150)	(0.0091)	(0.0151)	(0.0113)	(0.0170)	(0.0096)
Hispanic	-0.0630	-0.0870	-0.0032	-0.0063	0.0019	-0.0002	0.0014	-0.0031
	(0.1568)	(0.2014)	(0.0108)	(0.0127)	(0.0069)	(0.0089)	(0.0035)	(0.0042)
By Education								
No College	-0.2558**	-0.1987**	-0.0166*	-0.0135**	-0.0079**	-0.0054	-0.0041*	-0.0045***
	(0.1232)	(0.0946)	(0.0095)	(0.0067)	(0.0031)	(0.0039)	(0.0024)	(0.0014)
College	-0.0034	-0.0880	0.0054	0.0027	0.0023	-0.0040*	-0.0008	-0.0040***
	(0.0636)	(0.0576)	(0.0078)	(0.0060)	(0.0024)	(0.0022)	(0.0018)	(0.0010)
By Age								
21-44 Years Old	-0.1428	-0.0983	-0.0098	-0.0018	-0.0013	-0.0008	-0.0024	-0.0041**
	(0.1347)	(0.0833)	(0.0107)	(0.0047)	(0.0036)	(0.0029)	(0.0023)	(0.0018)
45-65 Years Old	-0.2017***	-0.2152***	-0.0096	-0.0095*	-0.0080***	-0.0094***	-0.0033**	-0.0045***
	(0.0840)	(0.0640)	(0.0065)	(0.0053)	(0.0027)	(0.0033)	(0.0017)	(0.0012)
Model	TWFE	CS	TWFE	CS	TWFE	CS	TWFE	CS

Full Sample N	3,140,276	550	3,140,276	550	3,140,276	550	3,140,276	550
Full Sample Mean	4.167	4.167	0.386	0.386	0.143	0.143	0.062	0.062

***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Models use data from the 2010-2020 BRFSS using sampling weights. Table reports results from two-way fixed effects models (TWFE) and Callaway-Sant'Anna difference-in-differences estimator (CS). Standard errors are clustered at the state level. CS models use population weights, though unweighted results are very similar.

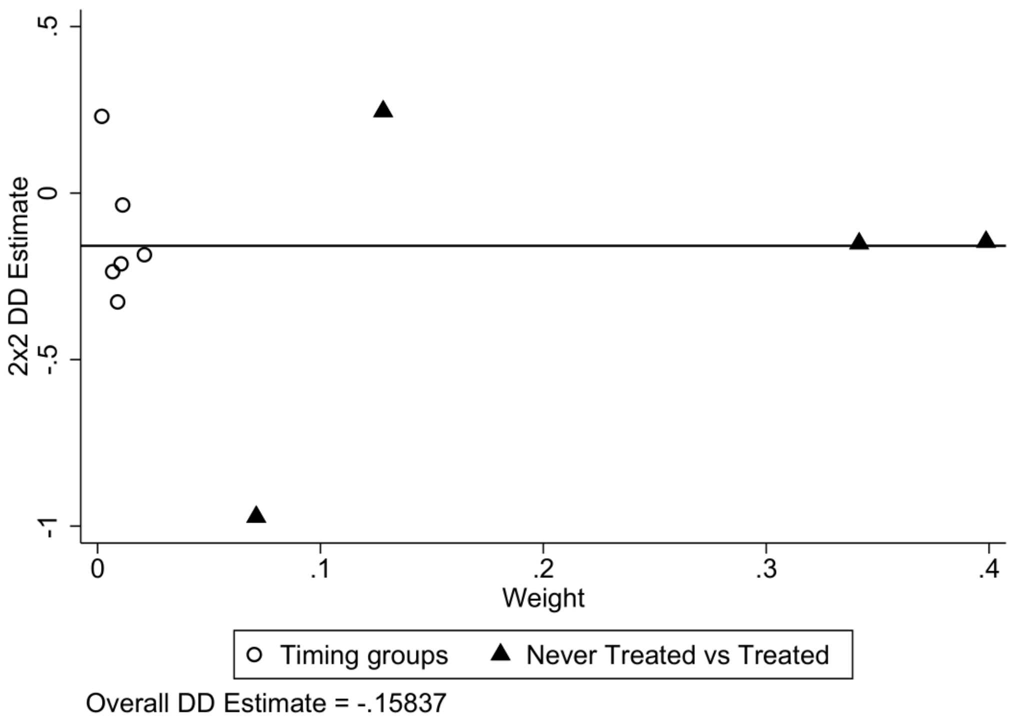
Appendix Table 3.2. Marginal Effects of RCL on Activity Limitation Days for TWFE and CS

	Number of Days		Any Days		All 30 Days	
Full Sample	-0.1375***	-0.0164	-0.0057	0.0005	-0.0023**	-0.0001
	(0.0433)	(0.0616)	(0.0047)	(0.0053)	(0.0011)	(0.0013)
By Gender						
Men	-0.0773	0.0595	-0.0012	0.0007	-0.0016	0.0016
	(0.0849)	(0.0793)	(0.0067)	(0.0050)	(0.0020)	(0.0019)
Women	-0.1944***	-0.1517***	-0.0100***	-0.0034	-0.0029***	-0.0035***
	(0.0429)	(0.0458)	(0.0034)	(0.0047)	(0.0009)	(0.0009)
By Race/Ethnicity						
White non-Hispanic	-0.1631**	-0.0762*	-0.0059	-0.0044	-0.0031**	-0.0017*
	(0.0750)	(0.0414)	(0.0055)	(0.0032)	(0.0016)	(0.0008)
Black non-Hispanic	-0.6366***	-0.0951	-0.0446***	-0.0068	-0.0119**	-0.0012
	(0.1679)	(0.2437)	(0.0145)	(0.0083)	(0.0049)	(0.0084)
Asian non-Hispanic	-0.4900	-1.0995***	-0.0253	-0.0198	-0.0004	-0.0213***
	(0.3089)	(0.2175)	(0.0152)	(0.0138)	(0.0059)	(0.0067)
Hispanic	0.0600	-0.0054	0.0047	-0.0009	0.0014	-0.0014
	(0.1242)	(0.1610)	(0.0073)	(0.0123)	(0.0025)	(0.0038)
By Education						
No College	-0.2113***	-0.0826	-0.0128***	-0.0053	-0.0041**	-0.0019
	(0.0703)	(0.0848)	(0.0048)	(0.0041)	(0.0020)	(0.0020)
College	0.0056	-0.0122	0.0091	0.0029	0.0011	-0.0002
	(0.0320)	(0.0376)	(0.0078)	(0.0054)	(0.0014)	(0.0009)
By Age						
21-44	-0.1187	0.0117	-0.0026	0.0017	-0.0009	0.0021
	(0.0776)	(0.0680)	(0.0086)	(0.0055)	(0.0015)	(0.0016)
45-65	-0.1675***	-0.1548***	-0.0099***	-0.0079**	-0.0041***	-0.0049***
	(0.0514)	(0.0447)	(0.0030)	(0.0034)	(0.0012)	(0.0013)
Model	TWFE	CS	TWFE	CS	TWFE	CS

N	3,140,276	550	3,140,276	550	3,140,276	550
Sample Mean	2.564	2.564	0.240	0.240	0.039	0.039

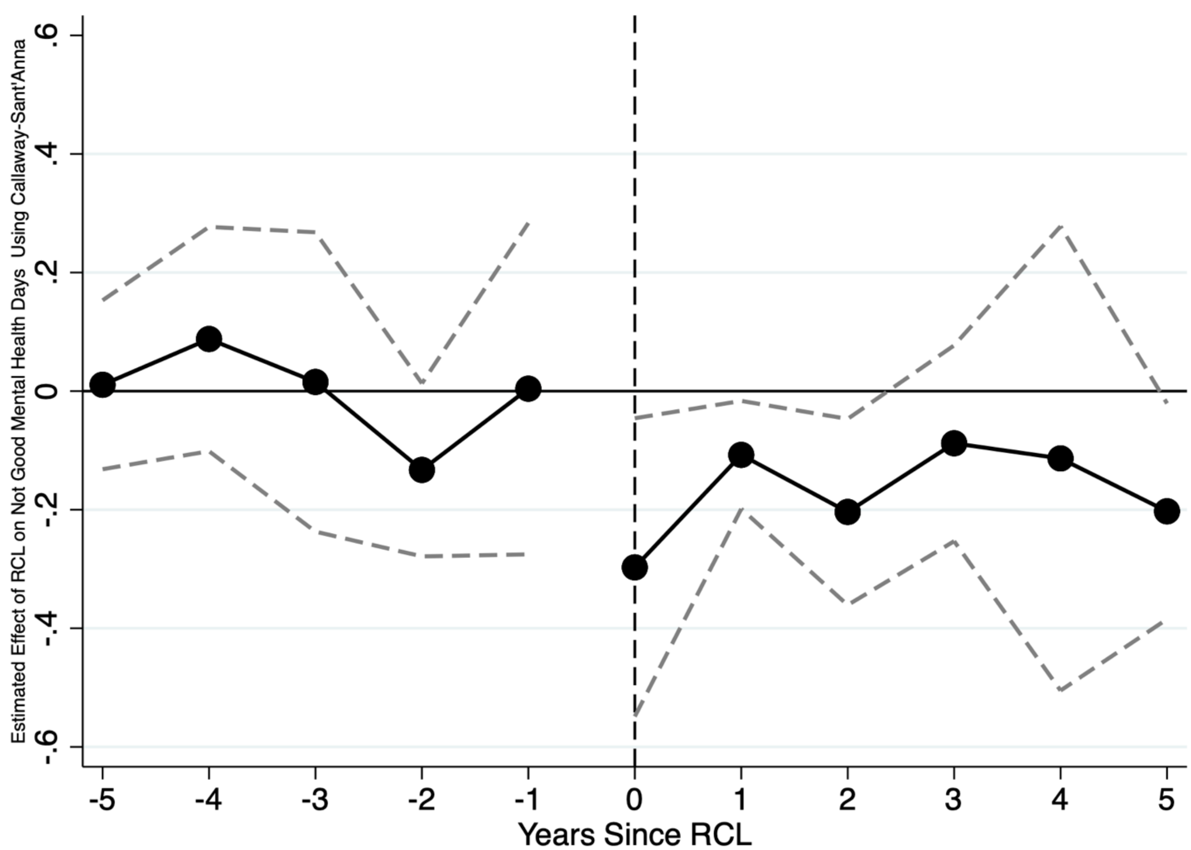
***Significant at 1 percent level, **Significant at 5 percent level, *Significant at 10 percent level. Models use data from the 2010-2020 BRFSS using sampling weights. Table reports results from two-way fixed effects models (TWFE) and Callaway-Sant'Anna difference-in-differences estimator (CS), with and without controlling for MCL. Standard errors are clustered at the state level. CS models use population weights, though unweighted results are very similar.

Appendix Figure 3.1. Goodman-Bacon Decomposition Results for "Not Good" Mental Health Days



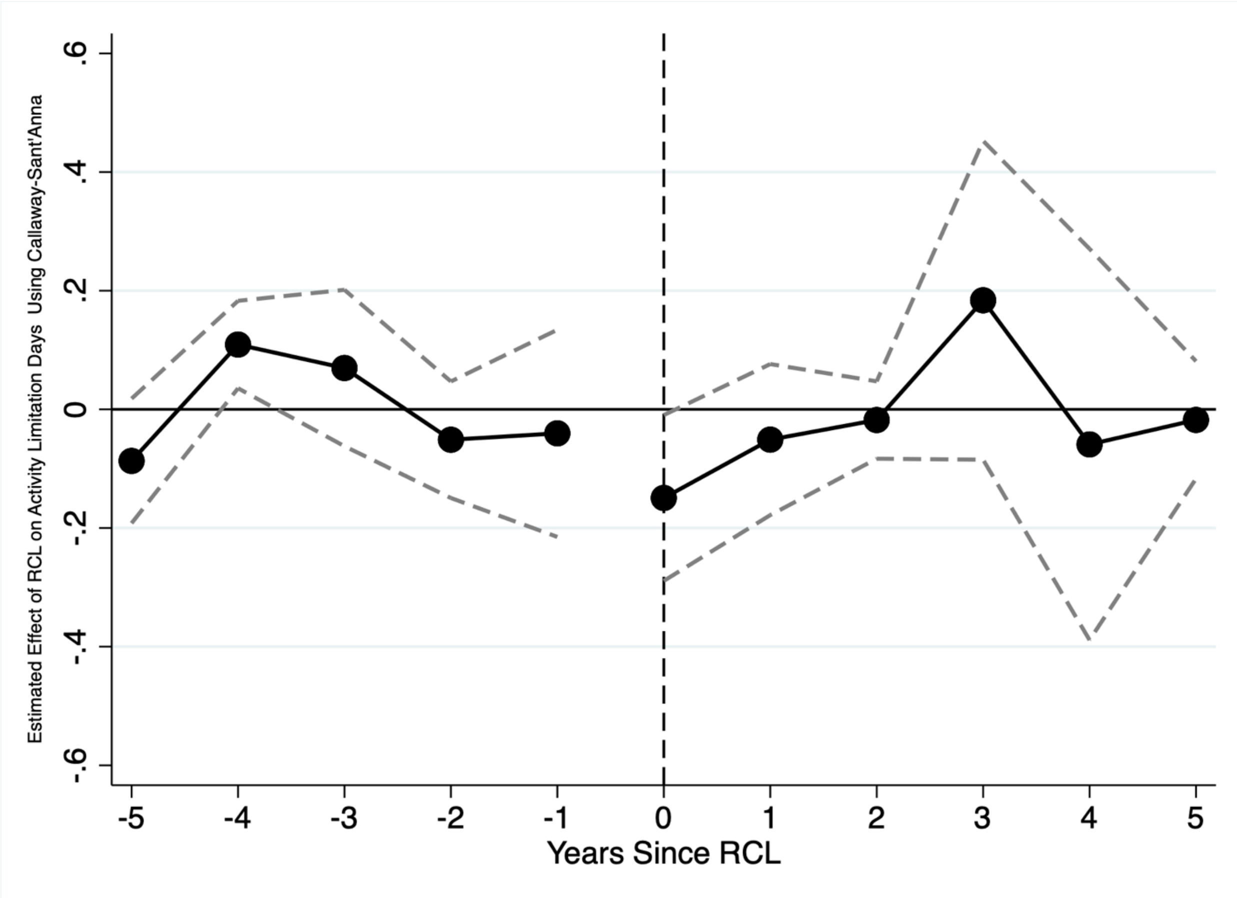
Reported is a Goodman-Bacon decomposition that shows 93.97 percent of the unadjusted difference-in-differences estimate is made up of "clean" comparisons between treated and never treated units.

Appendix Figure 3.2. Event Study for "Not Good" Mental Health Days using Callaway-Sant'Anna - Full Sample



Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. Estimates are gathered using a Callaway-Sant'Anna event study model. Standard errors are clustered at the state level.

Appendix Figure 3.3. Event Study for "Activity Limitation Days using Callaway-Sant'Anna - Full Sample



Event study point estimates are reported in black dots with 95 percent confidence intervals in gray dashes. Estimates are gathered using a Callaway-Sant'Anna event study model. Standard errors are clustered at the state level.

Conclusion

The work presented here provides evidence regarding three different prongs of public policy and their impact on mental health outcomes. In chapter one, I model the impact of minimum wages and the Earned Income Tax Credit on mental health around the time of pregnancy and find both policies can result in improved mental health. Chapter two examines the Dependent Coverage Mandate and mental health and supplies evidence of previously overlooked mental health improvements for some groups. Lastly, chapter 3 estimates the impact of recreational cannabis laws on mental health and finds meaningful improvements in mental health following such laws.

Together, this work suggests various public policies can be effective in improving mental health outcomes. Furthermore, the scope of these polices is not to be overlooked. Outside of the Dependent Coverage Mandate, the polices examined here are unlikely to be implemented with the intention of address mental health outcomes. Thus, this work may also suggest that policies enacted without initial mental health concerns may also have pathways to impact mental health and should be thought of as part of the broader health system.

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