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The Implications of Public Policies on Health Economics

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Abstract

This dissertation evaluates the effects of public policies on health insurance coverage, mental health, provider quality and patient health outcomes. The first chapter demonstrates evidence of increased enrollments through the Health Insurance Exchange (HIX) as a result of expanded Premium Tax Credits. I use the variation in state Medicaid expansion statuses to identify the change in HIX enrollments. The second chapter evaluates the effects of the U.S. Supreme Court's Dobbs vs. Jackson decision in June 2022 on mental health. I leverage the heterogeneity in state abortion restrictions to identify an increase in moderate to severe anxiety symptoms for individuals living in restricted states. I implement a Difference in Difference analysis using a linear probability model and do not find evidence of any pre-trends. Therefore, without the Dobbs' decision, I would not find an increase in these negative mental health symptoms. The third chapter investigates the impacts of Managed Care on home health provider quality and patient health outcomes. I evaluate Managed Care programs that deliver Long Term Services and Supports (LTSS) for Medicaid beneficiaries. I implement the Callaway Sant'Anna Difference in Difference strategy using panel data at the provider level and find a reduction in overall provider quality. I also find downstream consequences in the form of worsened patient health outcomes. These results may have greater implications in the form of premature admissions to skilled nursing facilities. If patients are unable to receive quality care from home health providers, they may turn to other, more costly, LTSS providers.

This dissertation is dedicated to my wonderful parents and boyfriend, Kyle.

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1 The Effects of the American Rescue Plan Act on Racial Equity in Health Insurance Coverage

With Sankar Mukhopadhyay

1.1 Abstract

Objective: To evaluate the effects on racial disparities in health insurance coverage from the changes in the Premium Tax Credit (PTC) implemented in March 2021 as part of the American Rescue Plan Act (ARPA).

Data Sources and Study Setting: I use nationally representative individual-level data from the Household Pulse Survey (HPS), which provides demographic, economic, and health insurance information for United States residents during the period April 2020 to August 2022.

Study Design: While the PTC changes applied to all states, the 14 states that did not expand Medicaid received substantially more benefits than the expansion states since they had more uninsured individuals eligible for the PTC than the expansion states. In the analysis, the treatment (control) group includes all Medicaid non-expansion (expansion) states. I use a Difference-in-Difference regression analysis to estimate the increase in the probability of insurance coverage after the expansion of the PTC. Furthermore, I conduct sensitivity and heterogeneity analyses.

Data Collection/ Extraction Methods: I focus on survey respondents aged 18 to 64.

Principal Findings: The expanded PTC increased the probability of an individual having coverage through the Health Insurance Exchange (HIX) in a non-expansion state by 0.95 [95% CI: 0.6136, 1.2900], 1.75 [95% CI: 1.1795, 2.3291] and 1.75 [95% CI: 1.1815, 2.3269] percentage points among White, Black, and Hispanic respondents, respectively. It also increased overall health insurance coverage among all groups.

Conclusions: The expanded PTC boosted HIX and overall health insurance coverage and reduced racial disparities.

1.2 Introduction

Between September 2013 and February 2015, 16.9 million individuals gained insurance coverage due to the Affordable Care Act (ACA)[1]. Estimates suggest that about 40% of the increase was due to the Premium Tax Credit (PTC), a subsidy used by eligible individuals to buy health insurance from the Health Insurance Exchange (HIX)[2]. The ACA also mitigated racial disparities in health insurance coverage. After the main ACA provisions went into effect in 2014, the percentage of uninsured adults decreased by 3.0 percentage points among Whites, 5.1 percentage points among Blacks, and 7.1 percentage points among Hispanics[3]. A number of papers[3–5] found that the ACA reduced inequalities in health insurance coverage.

However, despite the ACA, an estimated 30 million Americans were uninsured in 2019 with huge racial disparities in access to health insurance. In 2019, the uninsurance rates among Whites, Blacks, and Hispanics were 7.8%, 11.4%, and 20.0% respectively[6]. The American Rescue Plan Act (ARPA) of 2021 expanded the eligibility and amount of the PTC that was established as part of the ACA. These expansions to the PTCs are the first since the ACA. At the time of the passage of the ARPA, researchers predicted that the ARPA may improve health equity[7]. This paper estimates the effect of the ARPA PTC expansion on racial disparities in insurance coverage. There are three major changes instituted by the ARPA that are relevant for my purposes. First, anyone who received unemployment compensation (UC) in 2021 is eligible for zero-premium health insurance. This provision also applies to individuals in the non-expansion states with incomes below 100% of the Federal Poverty Level (FPL) who were not eligible for PTCs (coverage gap) before the ARPA. Second, households below 150% of the FPL are now eligible for zero-premium health insurance through HIX. Before the ARPA, these households were required to contribute a portion of their income to insurance premiums. Finally, households above 150% of the FPL received higher PTC amounts, including households above 400% of the FPL, who were previously not eligible for the PTC.

Although HIX is available in every state, it may be the only subsidized insurance option for low-income non-elderly adults without employer-sponsored insurance (ESI) and who live in non-expansion states. Although the ACA mandated Medicaid expansion in all states, a Supreme Court ruling made the expansion optional for states. A total of 36 states (and Washington DC) expanded Medicaid before 1/1/2021, leading to wide variability in Medicaid eligibility across states. In 2019, the uninsurance rates in non-expansion states were almost double that of expansion states across all races and ethnicities. For example, in 2019, the uninsurance rate among Whites was 6% in expansion states compared to 11% in non-expansion states. The corresponding numbers for Blacks and Hispanics were 8% and 15% in expansion states and 15% and 28% in non-expansion states[6].

Therefore, it is plausible that non-expansion states will see a larger increase in HIX participation than expansion states because of the ARPA PTC expansions. I compare HIX participation in expansion and non-expansion states before and after the ARPA using nationally representative high-frequency data from the Household Pulse Survey (HPS) and Difference-in-Difference regressions to explore how the expanded PTCs affected racial inequality in insurance coverage.

In addition, I expect the impact of these provisions may be higher among low-income individuals. However, the effect may not be zero among higher-income individuals because Medicaid eligibility is based on current income. Therefore, even individuals with a relatively high income in the previous year may become eligible for Medicaid if they live in an expansion state and their income falls below the cutoff level (for example, because of a job loss) since the ACA removed the asset test for Medicaid eligibility.

1.3 Background

The basic tenets of the ARPA were part of President Biden's campaign. Three important events are most relevant to the implementation of the ARPA. First, all the major TV networks declared President Biden as the winner of the Presidential election on Nov 7th, 2020 (during the 18th round of the HPS). Second, the Georgia senate election, which allowed Democrats to control the Senate was held on January 5th, 2021 (between the 21st and 22nd rounds of the HPS). Third, the ARPA was signed into law on March 11th, 2021 (during the 26th round of the HPS).

The open enrollment period for HIX enrollment for the calendar year 2021 was supposed to be from Nov. 1st, 2020 to Dec. 15th, 2020. Outside of the open enrollment period, individuals can purchase health insurance through HIX with a qualifying life event (such as a loss of a job, marriage or divorce, birth of a child, etc.). However, After President Biden took office, he issued an executive order that created a special enrollment period (SEP) from Feb. 15th, 2021 to August 15th, 2021. During this SEP, individuals could purchase insurance through HIX without any qualifying life events. Individuals who already chose a plan could switch their plans during the SEP.

The PTC can be claimed at the end of the year when one files a tax return, or it can be claimed at the time of purchase through HIX. The latter is known as Advanced Premium Tax Credit (APTC). The APTC reduces the premium an individual has to pay by the amount of the APTC. However, the APTC is based on anticipated income. If the actual income differs from anticipated income (or the amount of PTC changes, which happened in the case of the ARPA), an individual will receive (pay) the difference between what they were supposed to receive and what they actually received as APTC.

1.4 Methods and Data

I use data from the Household Pulse Survey (HPS), a nationally representative individual-level repeated cross-sectional survey, to estimate the effect of increased PTC on coverage. The HPS surveyed respondents on topics including demographics, socioeconomics, and source of health insurance.

I use the first 48 rounds from April 2020 to August 2022. Some individuals may have anticipated that PTCs will be increased/expanded after the presidential election and/or after the GA Senate election, even though the ARPA did not become a law until March 11th, 2021. Therefore, I consider the period from Nov 7th, 2020 to March 11th, 2021 (rounds 18 to 26 of the HPS) as a transition period; since this period is technically neither a true “before” period, nor a true “after” period. In other words, in the baseline empirical implementation, I drop the data from these rounds. I consider rounds 1-17 as “before” and rounds 27 to 48 as “after” periods.

I focus on respondents ages 18 to 64 because adults over 64 qualify for Medicare in all states. This reduces the sample size from 3,720,633 observations to 2,754,608 observations.

From the sample of 2,754,608 respondents, I lose 595,078 observations because either the outcome or one of the control variables is missing. Finally, I eliminated rounds 18 to 26 of the HPS (except when testing for parallel trends) due to this being the policy transition period, leaving us with a final sample size of 1,802,922.

Following the HPS guidelines, I create five mutually exclusive racial-ethnic groups: Non-Hispanic White (NHW), Non-Hispanic Black (NHB), Non-Hispanic Asian (NHAsian), Non-Hispanic Other Races (NHOther) and Hispanic (HISPAN). The sample sizes for each category are: NHW (N = 1,343,001), NHB (N = 139,350), NHAsian (N = 95,541), NHOther (N = 74,020) and Hispanic respondents (N = 151,010).

The HPS asked individuals about their health insurance source(s). A respondent could pick up to eight sources: Employer-Sponsored Insurance (ESI), Marketplace (Health Insurance Exchange (HIX)), Medicare, Medicaid, Tricare, Veteran's Affairs (VA), Indian Health Service (IHS), and Other. In the sample, 21% of respondents reported having multiple sources of health insurance. This problem is not unique to the study or even to the HPS. Previous studies have reported difficulty in classifying the source of health insurance for respondents given the variety of choices and confusion among respondents, especially in the post-ACA period[8,9].

I create mutually exclusive health insurance categories[10]. The ESI classification includes all respondents with ESI coverage. The HIX classification includes respondents with HIX coverage (but no ESI). The Medicaid classification includes respondents with Medicaid coverage (but no ESI, HIX or other insurance). The Other Private Insurance classification includes respondents with any other type of private insurance (but no ESI, Medicaid, Medicare, HIX, Tricare, VA or HIS). The Other Public Insurance classification includes respondents with Medicare, Tricare, VA or HIS (but no ESI, HIX, Medicaid or Other Private Insurance). Finally, respondents without any health insurance are categorized as Uninsured.

There is some evidence that the HPS sample may not be representative due to the relatively low response rate[10]. For example, the estimates from the HPS overestimate the vaccination rate[11]. Other papers found that HPS is representative in some dimensions but not others[10,12]. There is also evidence that health insurance coverage rates in the HPS are different from some of the other surveys[10].

However, the biases in the levels of HPS health insurance coverage variables are similar across states and stable over time[10]. This is critically important for my purpose. Since I am relying on differences in differences, any bias in levels will be differenced out if the bias is similar in expansion and non-expansion states. Furthermore, since the regression-adjusted estimates include state and round fixed effects, the existence of bias in levels is not a problem as long as the bias is similar across states or over time.

The primary outcome variable is whether an individual has health insurance through HIX. I also explore whether the ARPA PTCs changed health insurance coverage through Medicaid, ESI, other private sources, other public sources, and overall health insurance coverage. Please see Appendix Table A1 for a summary of respondent insurance sources across race-ethnicity groups.

I treat the 14 states that did not expand their Medicaid programs before February 2021 as non-expansion states. The non-expansion states include Alabama, Florida, Georgia, Kansas, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Wisconsin, and Wyoming. Two states: Oklahoma and Missouri, expanded Medicaid after the implementation of the ARPA (7/1/2021 and 10/1/2021, respectively). I treat them as non-expansion states, but I show that the results are robust even if I omit these two states in the robustness section.

To address potential confounders, I include controls for gender, marital status, age, education, income, the number of children in the household all taken from the HPS survey data. I also control for the state-level unemployment rate, taken from FRED Economic data. The mean values for all covariates for expansion states are in Appendix Table A2 and for non-expansion states are in Appendix Table A3.

The Difference in Difference (DD) estimates represent the causal effect of the ARPA PTCs under the assumption that the non-expansion and expansion states would have had similar trends in HIX participation in the absence of the ARPA. Testing this assumption is not possible since it requires observing the counterfactuals. Instead, I (and the rest of the literature) test whether the trends in the outcome variable (HIX participation rate in this case) were similar in non-expansion and expansion states before the treatment (the ARPA in this case). To test this, I estimate the following regression equation

$$Y_{it} = \alpha I(NONEXP_{it}) + \sum_{j=2}^{48} \beta_j I(WAVE = j) + \sum_{j=2}^{48} \gamma_j I(WAVE = j) * I(NONEXP_{it}) + \delta X_{it} + S_i + \varepsilon_{it}$$

Where Y_{it} is a binary outcome variable such as whether a respondent has health insurance through HIX or not. X_{it} includes individual-level controls. S_i includes state fixed effects. Since the parameters are identified from intra-state variation, the parameter α is not identified. β_js represent the time effects and γ_js represent the difference between non-expansion and expansion states during wave j. The parallel trend assumption requires that all the pre-ARPA $\gamma_j\, s(j = 2\ to\ 17)$ are jointly insignificant. In addition to testing for the parallel trends assumption, this regression specification and the high-frequency nature of the HPS (weekly from April 2020 to August 2020 and bi-weekly after that) allow us to identify the timing of the response to this law precisely.

After I establish the parallel trend assumption, I estimate the average effect of ARPA PTCs on outcomes. To that effect, I estimate

$$Y_{it} = \alpha I(NONEXP_{it}) + \beta I(POSTARPA) + \gamma I(POSTARPA) * I(NONEXP_{it}) + \delta X_{it} + S_i + \theta_t + \varepsilon_{it}$$

where the coefficient of the interaction term (γ) is the primary coefficient of interest. Since I include state and time fixed effects the parameters α and β are not identified. All standard errors are clustered at the state level.

1.5 Results

1.5.1 Parallel Trend

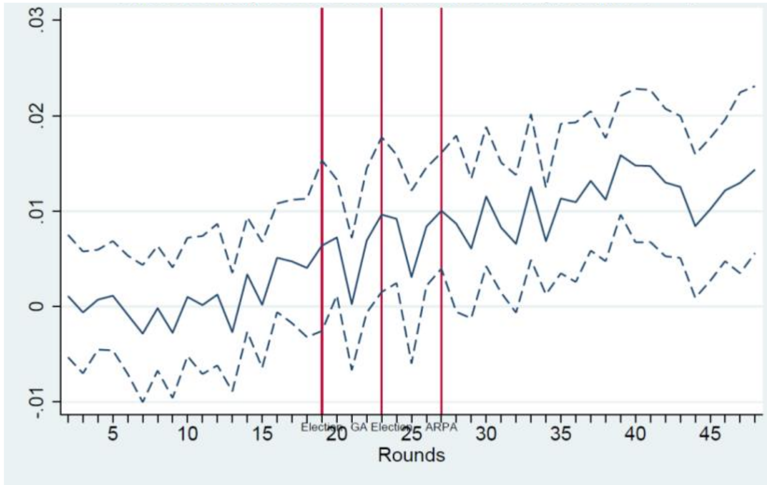
As discussed above, the interpretation of DD estimates depends on the parallel trend assumption. Therefore, before discussing the main results I check the parallel trend assumption. I estimate equation (1) to test whether there are any differences in the pre-ARPA trends in HIX participation across expansion and non-expansion states. The estimated interaction terms ($\gamma_j\, s;\ j = 2\ to\ 48$) for the full sample are presented in Figure 1. A note below Figure 1 lists all the control variables included in the regressions. The bold line shows the estimated coefficients and the dotted lines show the estimated 95% confidence intervals. As shown in Figure 1, the estimated coefficients before the presidential election (rounds 2 to 17) are close to zero and never significantly different from zero. Furthermore, they do not show any trend. Next, to formally test the parallel trends assumption, I test that these 17 interaction term coefficients are jointly zero.

The p-values associated with the hypothesis is 0.44. Therefore, I conclude that the parallel trend assumption holds, and the DD estimates represent the causal effect of the ARPA PTCs.

Figure 1 also allows us to decipher the timing of the response. I can see that the interaction terms show an upward trend during the transition period (between November 2020 and March 2021), especially after the Georgia Senate runoffs. The coefficients after ARPA implementation in March 2021 are always positive and they are almost always statistically significant.

One concern is that the upward trend in the round effects seems to start around the 13th round. I am unaware of any particular event that would differentially increase HIX enrollment in the non-expansion states after the 13th round. Individuals have 60 days after they lose their employer sponsored insurance (ESI) to apply for health insurance through HIX. Therefore, it is possible that during the early part of the COVID-19 emergency, individuals delayed their applications to get insurance through HIX (possibly because of widespread shutdowns and service disruptions). However, there is no way to confirm this in the data. I should note that none of the coefficient estimates between rounds 13 and 18 in Figure 1 are statistically significant. Furthermore, they are jointly insignificant. Please see the Sensitivity Analysis section for more on this issue.

Figure 1. 1: Estimated Changes in The Probability of HIX Enrollment: by the HPS Survey Rounds



Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48] NOTES: Controls include sex, marital status, age, age squared, number of children in the household, educational and income categories, state fixed effects and survey round fixed effects. 95% CI are based on standard errors clustered at the state-round level. HIX is the abbreviation for the Health Insurance Exchange.

From a policy evaluation perspective, it is crucial to understand how the ARPA affected the racial and ethnic inequality in healthcare access, given the pre-existing differences in health insurance coverage across races and ethnicity. Moreover, there are differences in other socioeconomic characteristics across races and ethnicity, and therefore, I should expect different impacts of the ARPA across races and ethnicity. Therefore, I report the results for NHW, NHB, NHAsian, NHOther, and Hispanics separately. I estimated the parallel trend specification for each of the five racial-ethnic groups. The results, presented in Appendix Figure A1, are similar to those in Figure 1.

1.5.2 Mean Difference in Difference

I begin with a mean DD of the primary outcome variable. As discussed in the Backgrounds section, I take waves 2 to 17 as the Pre-ARPA period and waves 27 to 48 as the post-ARPA period in the baseline analysis.

The summary mean DD results for HIX participation (the primary outcome variable) are shown on the left panel of Figure 2. The underlying data is in Appendix Table A4. The percentage of people who purchased insurance through HIX in the sample is similar to previous studies (7.3% in the sample, which covers the period 2020-2022. If I restrict the sample to just 2020, then the corresponding number is 7.1%. For comparison, Bundorf et al. 2021[10] report the percentage of people who purchased through HIX at 7.0% in the spring and summer and 7.1% in the fall and winter.)

For the full sample (all races combined; Appendix Table A4), coverage through HIX among increased from 7.7% before the ARPA to 9.2% after the ARPA in the non-expansion states, an increase of 1.5 percentage points (statistically significant at 1%). On the other hand, coverage through HIX in expansion states increased by only 0.3 percentage points (not significant at conventional levels). The mean DD estimate indicates that the ARPA increased HIX coverage by 0.9 percentage points (statistically significant at 1%) among NHW respondents in the non-expansion states compared to the expansion states. This mean DD estimate is shown in the left panel of Figure 2. Similar analysis suggests that the mean DD estimates for NHW, NHB, NHAsian, NHOther and Hispanic subsamples are 0.9, 1.7, 0.8, 0.7 and 1.7 percentage points respectively (see left panel of Figure 2).

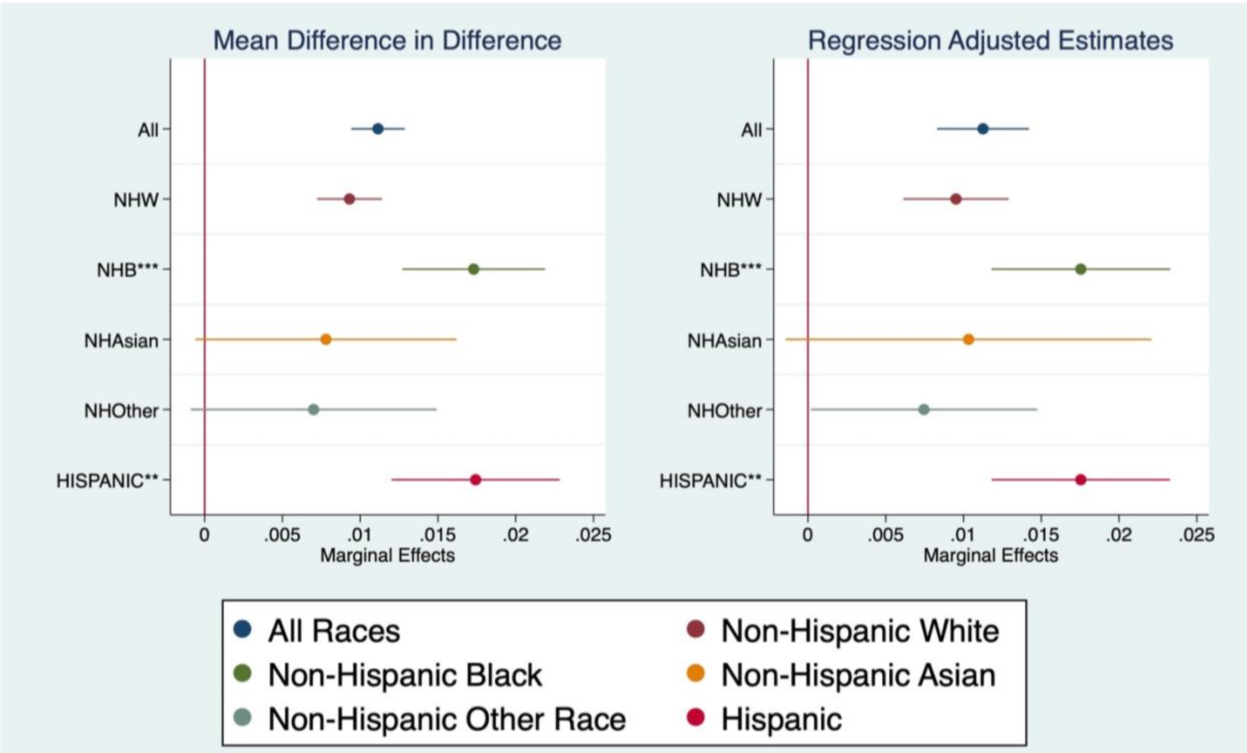
Appendix Tables A5 and A6 show the sample means for health insurance coverage through ESI and Medicaid respectively. Appendix Table A7 shows the effects of the ARPA on uninsurance rates, which indicates that in the full sample, uninsurance rates in the non-expansion states declined by 1.1 percentage points more than the expansion states. The corresponding estimates are 0.8, 1.8, 0.9, 1.3, and 1.4 percentage points among NHW, NHB, NHAsian, NHOther, and Hispanic respondents, respectively.

1.5.3. Regression Adjusted Estimates

Next, I estimate equation (2) to estimate the average marginal effect. The estimated coefficients of the interaction terms for the full sample and for each racial-ethnic group are presented in the right panel of Figure 2. The underlying estimates are in Appendix Table A8. Estimates show that the additional PTCs increased the probability of an individual having HIX coverage by 1.13 percentage points ($p<0.001$) in the full sample. I also estimated the effect separately for each of the racial-ethnic groups. The effect among NHW respondents is 0.95 percentage points ($p<0.001$). The effect among NHB, NHAsian, NHOther and Hispanic respondents are 1.75 ($p<0.001$), 1.03(p-value 0.084), 0.75(p-value = 0.044), and 1.75($p<0.001$), respectively.

Next, to check if the differences across the racial-ethnic groups are statistically significant, I estimate a regression where I include a triple interaction term (Post ARPA*Non-expansion*Racial-ethnic groups). The stars next to a racial-ethnic group in Figure 2 signify that the group is significantly different than NHW respondents. The estimates for the triple interaction terms along with p-values are in Appendix Table A9. The effect among NHB respondents is 0.8 percentage points more than the NHW respondents (p-value = 0.003), and the effect among Hispanic respondents is also 0.8 percentage points more than the NHW respondents (p-value = 0.045). This result is unsurprising given that more NHB and Hispanic respondents were uninsured to begin with. The estimated effects among the NHAsian and NHOther groups are not statistically different from that of NHW respondents.

Figure 1. 2: Mean DD and Regression Adjusted DD Estimates of the ARPA on Health Insurance Exchange Enrollment



Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 to 26.] NOTES: Controls include sex, marital status, age, age squared, number of children in the household, educational and income categories, state unemployment rate, state fixed effects and survey round fixed effects. 95% CI are based on standard errors clustered at the state level. Stars indicate coefficient is significantly different from NHW group. "ARPA" is defined as the American Rescue Plan Act.

I also estimated the regression-adjusted effects of the ARPA on coverage through Employer Sponsored Insurance (ESI), Medicaid, other public sources, and other private sources (please see Appendix Table A8). Estimates show that the additional PTCs reduced the probability of being uninsured by 0.96 percentage points (p<0.001) in the full sample. The effect among NHW respondents is 0.75 percentage points (p<0.001). The effect among NHB, NHAsian, NHOther, and Hispanic respondents are 1.41 (p-value 0.004), 0.94(p-value 0.007), 0.96(p-value 0.11), and 1.26($p<0.001$), respectively.

1.5.3 Sensitivity Analyses

Next, I check the robustness of my results. The top row of Table 1.1 presents the baseline estimates for comparison purposes. When I use the original response to the source of the health insurance question (instead of creating mutually exclusive categories, which means some respondents are in multiple categories), the qualitative results remain the same (shown in Table 1.1).

Second, in the baseline specification, I dropped data from rounds 18 to 26 (the transition period). When I add the data from these rounds and classify them as the before period (since they are from before the formal passage of the ARPA), the qualitative results remain the same (shown in Table 1.1).

Third, I noted that two states (Missouri and Oklahoma) expanded their Medicaid in 2021. I exclude these two states from the sample and re-estimate the treatment effects as a robustness check. The results (shown in Table 1.1) are similar to the baseline results.

Finally, I also evaluate results in which rounds 1-12 are the "before" and rounds 27 to 48 are the "after" periods. I find qualitatively similar results, as demonstrated in Appendix Figure A2.

Table 1. 1: Regression Adjusted DD Estimates of the ARPA on Health Insurance Exchange Enrollment Sensitivity Analyses

Sensitivity Analysis	Non-Hispanic White	Non-Hispanic Black	Non-Hispanic Asian	Non-Hispanic Other	Hispanic	All
Baseline HIX	0.00952***	0.0175***	0.0103*	0.00746**	0.0175***	0.0113***
P-Value	(0.000)	(0.000)	(0.084)	(0.044)	(0.000)	(0.000)
Original HIX Category	0.00633**	0.0250***	0.00632	0.00654	0.0171***	0.00909***
P-Value	(0.015)	(0.000)	(0.356)	(0.421)	(0.002)	(0.001)
Include rounds 18-26	0.00834***	0.0149***	0.00628	0.00665*	0.0150***	0.00969***
P-Value	(0.000)	(0.000)	(0.230)	(0.059)	(0.000)	(0.000)
Exclude OK and MO	0.00978***	0.0179***	0.00944	0.00503	0.0186***	0.0117***
P-Value	(0.000)	(0.000)	(0.114)	(0.237)	(0.000)	(0.000)

Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 to 26.] NOTES: Controls include sex, marital status, age, age squared, number of children in the household, educational and income categories, state unemployment rate, state fixed effects and survey round fixed effects. Standard errors clustered at the state level. P-values in parentheses. "HIX" is defined as the Health Insurance Exchange. "OK" is defined as Oklahoma. "MO" is defined as Missouri. Stars indicate statistical significance at the 1%, 5% and 10% levels.

1.5.4 Heterogeneity Analyses

In the Introduction section, I discussed that I expect the effect of the ARPA to be higher among low-income people. I also expect a heterogeneous response by parental status. Since parents with children are more likely to be eligible for Medicaid than non-parents (even in non-expansion states), I expect stronger effects among non-parents. I estimate how the effects of the ARPA differed by income, education, and parental status.

Table 1.2 presents the estimates. The HPS does not report the exact income or income-to-poverty ratio. Income is reported only in broad categories (Less than $25,000, $25,000 - $34,999, $35,000 - $49,999, $50,000 - $74,999, $75,000 - $99,999, $100,000 - $149,999, $150,000 - $199,999, and $200,000 and above). Therefore, I combine the bottom four income brackets (i.e., annual household income less than \$75,000, which makes up 45% of the sample) and top four income brackets (i.e., annual household income \$75,000 or more). The HIX participation increased by 1.85 percentage points (p-value < 0.001) in the low-income group and by 0.38 percentage points (p-value = 0.005) in the high-income group. Similarly, the HIX participation increased by 1.59 percentage points (p-value < 0.001) among those without a college degree and by 0.73 percentage points (p-value < 0.001) among those with a college degree (Table 1.2). Finally, HIX participation increased by 1.37 percentage points (p-value < 0.001) among adults without children and by 0.75 percentage points (p-value < 0.001) among adults with children.

Table 1. 2: Regression Adjusted DD Estimates of the ARPA on Health Insurance Exchange Enrollment Heterogeneity Analyses

Heterogeneity Analysis	**Non-Hispanic White**	**Non-Hispanic Black**	**Non-Hispanic Asian**	**Non-Hispanic Other**	**Hispanic**	**All**
Education: Less than Bachelor's	0.0146***	0.0168***	0.0147	0.0129**	0.0228***	0.0159***
P-Value	(0.000)	(0.001)	(0.364)	(0.012)	(0.000)	(0.000)
Education: Bachelor's and Up	0.00529***	0.0194***	0.0112***	0.000560	0.0117**	0.00730***
P-Value	(0.002)	(0.000)	(0.007)	(0.949)	(0.022)	(0.000)
Income: $75k and Under	0.0171***	0.0221***	0.0169	0.00954	0.0270***	0.0185***
P-Value	(0.000)	(0.000)	(0.112)	(0.112)	(0.000)	(0.000)
Income: Over $75k	0.00330**	0.00733**	0.00764	0.00390	0.00387	0.00383***
P-Value	(0.041)	(0.013)	(0.152)	(0.517)	(0.238)	(0.005)
Households without Children	0.0113***	0.0219***	0.0195***	0.0136**	0.0223***	0.0137***
P-Value	(0.000)	(0.000)	(0.008)	(0.019)	(0.000)	(0.000)
Households with children	0.00618***	0.0127**	0.00151	0.000622	0.0127***	0.00751***
P-Value	(0.001)	(0.012)	(0.825)	(0.927)	(0.002)	(0.000)

Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 to 26.]
NOTES: Controls include sex, marital status, age, age squared, number of children in the household, educational and income categories, state unemployment rate, state fixed effects and survey round fixed effects. Standard errors clustered at the state level. P-values in parentheses. Stars indicate statistical significance at the 1%,5% and 10% levels.

1.6 Discussion

Between 2013 and 2014, the uninsurance rate among non-elderly Whites, Blacks, and Hispanics went down by 3.1, 5.1, and 6.7 percentage points, respectively. The declines were 3.3 (2.3), 5.6 (4.0), and 5.4 (7.2) percentage points in the expansion (non-expansion) states among Whites, Blacks, and Hispanics, respectively[3]. Therefore, the Medicaid expansion reduced the uninsurance rate by 1.0, 1.6, and 1.8 percentage points among Whites, Blacks, and Hispanics, respectively, during the first year of the ACA. Estimates also suggest that during the first years after the implementation of the ACA, the uninsurance rate declined by 7.7 and 11.5 percentage points among Whites and non-Whites, respectively, and individually purchased insurance increased by 0.7 and 1.8 percentage points among Whites and non-Whites, respectively[13]. In comparison, the ARPA subsidies increased individual purchase of insurance through HIX by 0.9, 1.75 and 1.75 percentage points among NHW, NHB, and Hispanic respondents, respectively. Therefore, the effect of the ARPA is comparable to previous major policy changes.

The ARPA may change the policy priorities in both expansion and non-expansion states. The Inflation Reduction Act has already extended the ARPA PTCs until 2025. Blavin et al. (2018)[14] found that individuals below 150% of FPL in non-expansion states spend $344 less in out-of-pocket expenditures than those in expansion states. Fiedler (2021)[15] found that the take-up rate of HIX subsidy is about 50% for people below 400% of FPL. One of the concerns since the passage of the ACA has been that even with the ACA PTC and Cost Sharing Reduction (CSR), which is available to families earning up to 250% of FPL, leaves health insurance unaffordable for many families. The ARPA's increased PTC will make health insurance more affordable, and I find that it increased coverage through HIX. The increased PTC should also reduce the need for "wrap-around" state subsidies offered by some non-expansion states. Currently, California, Massachusetts, New Jersey, and Vermont offer such subsidies. States may spend that money to subsidize more targeted populations (such as those affected by "family glitch" or undocumented immigrants)[16]. They may also focus on expanding CSR eligibility, which is not addressed by the ARPA.

On the other hand, the increased PTCs (especially the zero premium subsidies for people below 150% of FPL) and consequently increased health insurance coverage in non-expansion states may reduce their incentive to expand Medicaid. Under the ACA, the federal government pays 90% of the cost of insuring adults who are covered under the Medicaid expansion. Under the ARPA, the federal government offered to pay 95% of the cost of expanded Medicaid coverage for states that chose to expand Medicaid (for the first two years). However, the federal government pays 100% of the PTC. This may reduce the incentive to expand Medicaid. If the non-expansion states do not expand Medicaid, then people below 150% of FPL in those states will continue to pay higher out-of-pocket costs compared to their counterparts in expansion states. On the other hand, if ARPA subsidies are not extended beyond 2025, many people will lose health insurance, and the effects will be more pronounced in the non-expansion states (assuming no additional Medicaid expansion). These results also show that low-income Black and Hispanic Americans will particularly be adversely affected.

Appendix A

Appendix Table A1: Insurance Coverage by Race

Race Categories	ESI	HIX	Medicaid	Other Private	Other Public	Uninsured
Non-Hispanic White	1020300(75.97%)	104906(7.81%)	81678(6.08%)	11034(0.82%)	41344(3.08%)	83739(6.24%)
Non-Hispanic Black	94263(67.64%)	6713(4.82%)	16540(11.87%)	1404(1.01%)	4961(3.56%)	15469(11.1%)
Non-Hispanic Asian	78027(81.67%)	6364(6.66%)	3642(3.81%)	699(0.73%)	1137(1.19%)	5672(5.94%)
Non-Hispanic Other	48190(65.1%)	4425(5.98%)	8874(11.99%)	772(1.04%)	5537(7.48%)	6222(8.41%)
Hispanic	100412(66.49%)	9827(6.51%)	14195(9.4%)	2027(1.34%)	4127(2.73%)	20422(13.52%)
All	1341192(74.39%)	132235(7.33%)	124929(6.93%)	15936(0.88%)	57106(3.17%)	131524(7.3%)

Note. SOURCE: [U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 – 26.] NOTES: Percent of total race-ethnicity group in parentheses. “ESI” is defined as Employer-Sponsored Insurance. “HIX” is defined as Health Insurance Exchange.

Appendix Table A2: Covariate Mean Values for Expansion States

Covariate	Non-Hispanic White	Non-Hispanic Black	Non-Hispanic Asian	Non-Hispanic Other	Hispanic	All
Female	0.61	0.69	0.51	0.63	0.62	0.61
Marital Status	0.60	0.38	0.64	0.47	0.52	0.58
Age	45.91	45.56	43.31	42.98	42.80	45.36
Total Number of Kids in Household	0.73	0.85	0.78	0.87	0.95	0.77
Education: Less than High School	0.00	0.01	0.00	0.01	0.02	0.00
Education: Some High School	0.01	0.02	0.01	0.02	0.04	0.01
Education: High School Graduate or Equivalent	0.10	0.15	0.05	0.14	0.15	0.10
Education: Some College	0.19	0.26	0.11	0.27	0.25	0.20
Education: Associate's Degree	0.10	0.11	0.06	0.12	0.11	0.10
Income: Less Than $25,000	0.09	0.20	0.07	0.17	0.16	0.10
Income: $25,000 to $34,999	0.07	0.13	0.06	0.10	0.12	0.07
Income: $35,000 to $49,999	0.09	0.14	0.07	0.12	0.13	0.09
Income: $50,000 to $74,999	0.16	0.18	0.13	0.17	0.17	0.16
Income: $75,000 - $99,999	0.15	0.12	0.13	0.13	0.13	0.14
Income: $100,000 - $149,999	0.21	0.13	0.20	0.15	0.15	0.20
Income: $150,000 - $199,999	0.11	0.06	0.13	0.07	0.07	0.10
Income: $200,000 and above	0.14	0.05	0.22	0.08	0.07	0.13
Unemployment Rate	7.49	8.16	8.83	8.23	8.40	7.71
N	994054	81645	79165	55261	104580	1314705

Note. SOURCE: [U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 – 26.]

Appendix Table A3: Covariate Mean Values for Non-Expansion States

Covariate	Non-Hispanic White	Non-Hispanic Black	Non-Hispanic Asian	Non-Hispanic Other	Hispanic	All
Female	0.61	0.73	0.46	0.63	0.61	0.62
Marital Status	0.62	0.39	0.69	0.50	0.56	0.59
Age	46.31	45.07	42.64	43.51	43.54	45.67
Total Number of Kids in Household	0.74	0.92	0.88	0.89	0.93	0.79
Education: Less than High School	0.00	0.00	0.01	0.01	0.02	0.01
Education: Some High School	0.01	0.02	0.01	0.02	0.04	0.01
Education: High School Graduate or Equivalent	0.11	0.14	0.05	0.14	0.15	0.12
Education: Some College	0.21	0.25	0.10	0.27	0.23	0.22
Education: Associate’s Degree	0.11	0.13	0.06	0.13	0.12	0.12
Income: Less Than $25,000	0.10	0.23	0.08	0.19	0.17	0.12
Income: $25,000 to $34,999	0.08	0.15	0.06	0.12	0.13	0.09
Income: $35,000 to $49,999	0.10	0.15	0.08	0.13	0.14	0.11
Income: $50,000 to $74,999	0.18	0.19	0.14	0.18	0.18	0.18
Income: $75,000 - $99,999	0.15	0.11	0.14	0.13	0.13	0.14
Income: $100,000 - $149,999	0.20	0.10	0.21	0.14	0.13	0.18
Income: $150,000 - $199,999	0.09	0.04	0.12	0.05	0.06	0.08
Income: $200,000 and above	0.11	0.03	0.17	0.05	0.06	0.09
Unemployment Rate	6.52	6.92	6.82	6.38	7.30	6.64
N	348947	57705	16376	18759	46430	488217

Note. SOURCE: [U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 – 26.]

Appendix Table A4: Mean Difference in Difference Estimates of the Effects of the ARPA on Health Insurance Exchange Coverage

Race	Insurance	Expansion Status	Before	After	Difference	Mean DD
NHW	HIX	EXPAN	0.073(0.260)	0.077(0.267)	0.004[0.001]	
NHW	HIX	NONEXPAN	0.081(0.273)	0.095(0.293)	0.014[0.001]	0.009[0.001]***
NHB	HIX	EXPAN	0.040(0.195)	0.043(0.204)	0.003[0.001]	
NHB	HIX	NONEXPAN	0.049(0.215)	0.070(0.255)	0.021[0.002]	0.017[0.002]***
NHAsian	HIX	EXPAN	0.064(0.245)	0.062(0.241)	-0.002[0.002]	
NHAsian	HIX	NONEXPAN	0.081(0.274)	0.087(0.282)	0.006[0.004]	0.008[0.004]*
NHother	HIX	EXPAN	0.056(0.229)	0.055(0.227)	-0.001[0.002]	
NHother	HIX	NONEXPAN	0.071(0.256)	0.076(0.266)	0.005[0.004]	0.007[0.004]*
HISPAN	HIX	EXPAN	0.052(0.222)	0.054(0.226)	0.002[0.001]	
HISPAN	HIX	NONEXPAN	0.083(0.277)	0.103(0.304)	0.020[0.003]	0.017[0.003]***
ALL	HIX	EXPAN	0.068(0.252)	0.071(0.257)	0.003[0.000]	
ALL	HIX	NONEXPAN	0.077(0.267)	0.092(0.289)	0.015[0.001]	0.011[0.001]***

Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 - 26.]
NOTES: Standard deviations in parentheses, standard errors in brackets. "NHW" is defined as Non-Hispanic White. "NHB" is defined as Non-Hispanic Black. "NHAsian" is defined as Non-Hispanic Asian. "NHother" is defined as Non-Hispanic Other Race. "HISPAN" is defined as Hispanic. "HIX" is defined as Health Insurance Exchange. "MeanDD" is defined as Mean Difference in Difference.

Appendix Table A5: Mean Difference in Difference Estimates of the Effects of the ARPA on Employer Sponsored Insurance Coverage

Race	Insurance	Expansion Status	Before	After	Difference	Mean DD
NHW	ESI	EXPAN	0.768(0.422)	0.760(0.427)	-0.008[0.001]	-
NHW	ESI	NONEXPAN	0.750(0.433)	0.741(0.438)	-0.009[0.001]	-0.001[0.002]
NHB	ESI	EXPAN	0.678(0.467)	0.681(0.466)	0.003[0.003]	-
NHB	ESI	NONEXPAN	0.669(0.471)	0.677(0.468)	0.009[0.004]	0.006[0.005]
NHAsian	ESI	EXPAN	0.813(0.390)	0.828(0.377)	0.015[0.003]	-
NHAsian	ESI	NONEXPAN	0.789(0.408)	0.811(0.392)	0.022[0.006]	0.007[0.007]
NHother	ESI	EXPAN	0.662(0.473)	0.656(0.475)	-0.007[0.004]	-
NHother	ESI	NONEXPAN	0.622(0.485)	0.633(0.482)	0.011[0.007]	0.018[0.008]**
HISPAN	ESI	EXPAN	0.676(0.468)	0.679(0.467)	0.003[0.003]	-
HISPAN	ESI	NONEXPAN	0.635(0.482)	0.638(0.481)	0.003[0.004]	-0.000[0.005]
ALL	ESI	EXPAN	0.753(0.431)	0.749(0.434)	-0.005[0.001]	-
ALL	ESI	NONEXPAN	0.726(0.446)	0.722(0.448)	-0.004[0.001]	0.001[0.001]

Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 – 26.] NOTES: Standard deviations in parentheses, standard errors in brackets. "NHW" is defined as Non-Hispanic White. "NHB" is defined as Non-Hispanic Black. "NHAsian" is defined as Non-Hispanic Asian. "NHother" is defined as Non-Hispanic Other Race. "HISPAN" is defined as Hispanic. "ESI" is defined as Employer Sponsored Insurance. "MeanDD" is defined as Mean Difference in Difference.

Appendix Table A6: Mean Difference in Difference Estimates of the Effects of the ARPA on Medicaid Coverage

Race	Insurance	Expansion Status	Before	After	Difference	Mean DD
NHW	MEDICAID	EXPAN	0.065(0.246)	0.076(0.265)	0.011[0.001]	-
NHW	MEDICAID	NONEXPAN	0.031(0.173)	0.040(0.196)	0.009[0.001]	-0.002[0.001]***
NHB	MEDICAID	EXPAN	0.139(0.346)	0.154(0.361)	0.016[0.002]	-
NHB	MEDICAID	NONEXPAN	0.077(0.267)	0.086(0.280)	0.009[0.002]	-0.007[0.004]*
NHAsian	MEDICAID	EXPAN	0.042(0.200)	0.045(0.206)	0.003[0.001]	-
NHAsian	MEDICAID	NONEXPAN	0.014(0.119)	0.013(0.114)	-0.001[0.002]	-0.004[0.003]
NHother	MEDICAID	EXPAN	0.126(0.332)	0.144(0.352)	0.018[0.003]	-
NHother	MEDICAID	NONEXPAN	0.070(0.255)	0.085(0.279)	0.016[0.004]	-0.003[0.006]
HISPAN	MEDICAID	EXPAN	0.108(0.310)	0.122(0.327)	0.014[0.002]	-
HISPAN	MEDICAID	NONEXPAN	0.047(0.211)	0.050(0.218)	0.003[0.002]	-0.011[0.003]***
ALL	MEDICAID	EXPAN	0.074(0.262)	0.085(0.279)	0.011[0.000]	-
ALL	MEDICAID	NONEXPAN	0.039(0.193)	0.047(0.212)	0.008[0.001]	-0.003[0.001]***

Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 – 26.] NOTES: Standard deviations in parentheses, standard errors in brackets. "NHW" is defined as Non-Hispanic White. "NHB" is defined as Non-Hispanic Black. "NHAsian" is defined as Non-Hispanic Asian. "NHother" is defined as Non-Hispanic Other Race. "HISPAN" is defined as Hispanic. "MeanDD" is defined as Mean Difference in Difference.

Appendix Table A7: Mean Difference in Difference Estimates of the Effects of the ARPA on Uninsurance

Race	Insurance	Expansion Status	Before	After	Difference	Mean DD
NHW	UNINSURED	EXPAN	0.062(0.241)	0.045(0.208)	-0.017[0.000]	-
NHW	UNINSURED	NONEXPAN	0.096(0.295)	0.071(0.257)	-0.025[0.001]	-0.008[0.001]***
NHB	UNINSURED	EXPAN	0.105(0.306)	0.074(0.262)	-0.030[0.002]	-
NHB	UNINSURED	NONEXPAN	0.160(0.367)	0.111(0.314)	-0.049[0.003]	-0.018[0.003]***
NHAsian	UNINSURED	EXPAN	0.063(0.244)	0.046(0.210)	-0.017[0.002]	-
NHAsian	UNINSURED	NONEXPAN	0.093(0.290)	0.066(0.249)	-0.027[0.004]	-0.009[0.004]**
NHother	UNINSURED	EXPAN	0.083(0.275)	0.064(0.244)	-0.019[0.002]	-
NHother	UNINSURED	NONEXPAN	0.127(0.333)	0.096(0.295)	-0.032[0.005]	-0.013[0.005]***
HISPAN	UNINSURED	EXPAN	0.126(0.332)	0.102(0.303)	-0.024[0.002]	-
HISPAN	UNINSURED	NONEXPAN	0.197(0.398)	0.159(0.366)	-0.038[0.004]	-0.014[0.004]***
ALL	UNINSURED	EXPAN	0.071(0.256)	0.052(0.223)	-0.018[0.000]	-
ALL	UNINSURED	NONEXPAN	0.115(0.319)	0.085(0.279)	-0.029[0.001]	-0.011[0.001]***

Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 – 26.] NOTES: Standard deviations in parentheses, standard errors in brackets. "NHW" is defined as Non-Hispanic White. "NHB" is defined as Non-Hispanic Black. "NHAsian" is defined as Non-Hispanic Asian. "NHother" is defined as Non-Hispanic Other Race. "HISPAN" is defined as Hispanic. "MeanDD" is defined as Mean Difference in Difference.

Appendix Table A8: Regression Adjusted Estimates of the Effect of the ARPA on Insurance Coverage

Insurance	Non-Hispanic White	Non-Hispanic Black	Non-Hispanic Asian	Non-Hispanic Other	Hispanic	All
ESI	-0.00150(0.672)	-0.00125(0.811)	0.00247(0.658)	0.00913(0.258)	-0.00465(0.336)	-0.00168(0.622)
HIX	0.00952(0.000)***	0.0175(0.000)***	0.0103(0.084)*	0.00746(0.044)**	0.0175(0.000)***	0.0113(0.000)***
Medicaid	-0.00304(0.121)	-0.00450(0.319)	-0.00210(0.310)	0.00112(0.866)	-0.00804(0.016)**	-0.00239(0.234)
Uninsured	-0.00752(0.002)***	-0.0141(0.004)***	-0.00944(0.007)***	-0.00959(0.110)	-0.0126(0.000)***	-0.00964(0.000)***
Other Private	-0.000116(0.761)	0.000130(0.908)	-0.00145(0.202)	-0.000450(0.761)	0.00221(0.034)**	0.000130(0.731)
Other Public	0.00266(0.010)**	0.00221(0.258)	0.000178(0.933)	-0.00767(0.351)	0.00556(0.061)*	0.00233(0.068)*

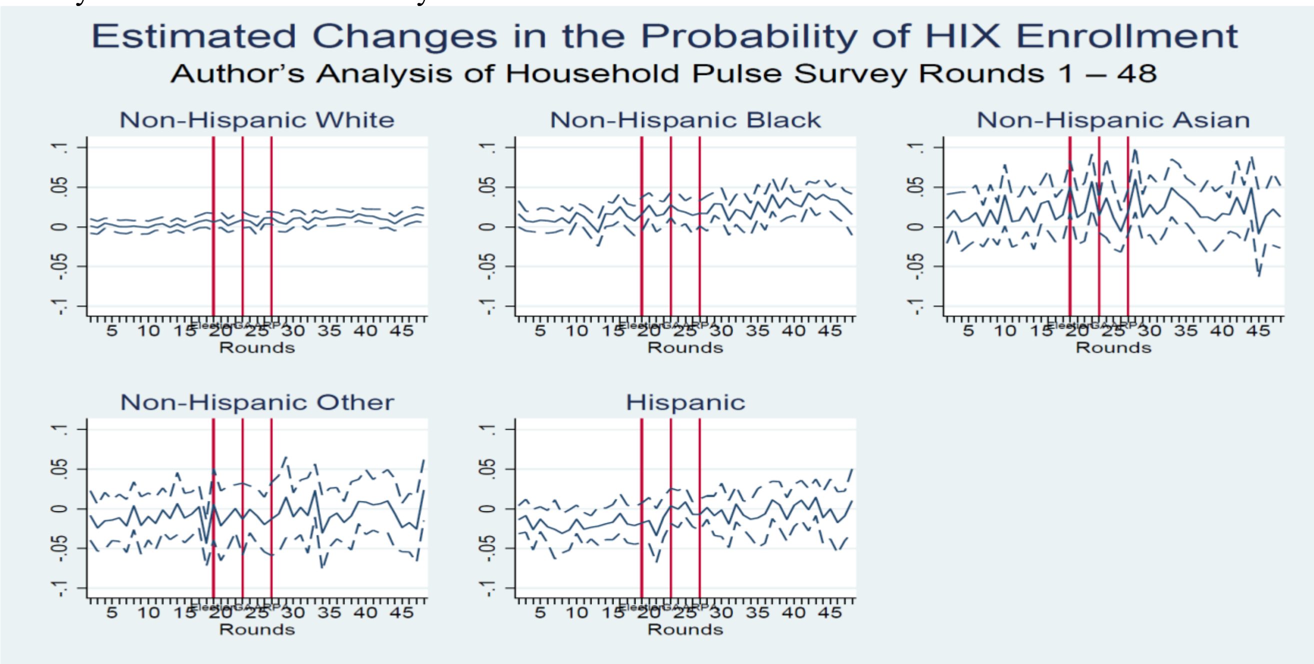
Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 – 26.] NOTES: Controls include sex, marital status, age, age squared, number of children in the household, educational and income categories, state fixed effects and survey round fixed effects. Standard errors clustered at the state level. P values in parentheses. "ESI" is defined as Employer Sponsored Insurance. "HIX" is defined as Health Insurance Exchange.

Appendix Table A9: Regression Adjusted Estimates of the Effect of the ARPA on Insurance Coverage with Race Interaction

Race Category Interaction	ESI	HIX	Other Private	Medicaid	Other Public	Uninsured
NHB	0.00229(0.698)	0.00830(0.003)***	0.000292(0.803)	-0.00161(0.706)	-0.000197(0.929)	-0.00907(0.034)**
NHAsian	0.00439(0.378)	-0.00143(0.811)	-0.00138(0.208)	0.00150(0.520)	-0.00263(0.230)	-0.000443(0.906)
NHOther	0.0114(0.103)	-0.00160(0.638)	-0.000680(0.632)	0.00387(0.493)	-0.0106(0.180)	-0.00236(0.652)
Hispanic	-0.00240(0.630)	0.00800(0.045)**	0.00223(0.042)**	-0.00502(0.104)	0.00315(0.216)	-0.00596(0.071)*

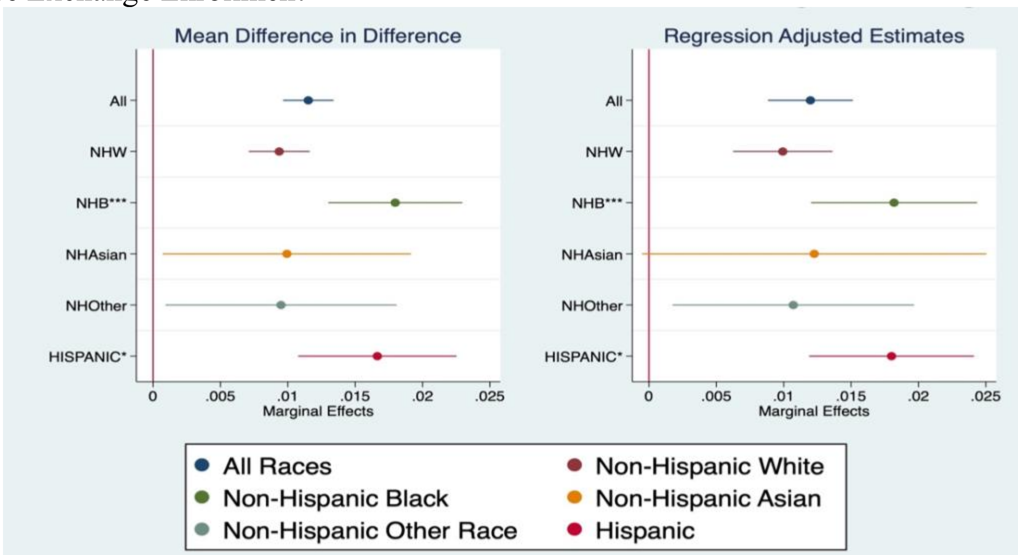
Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 18 – 26.] NOTES: Controls include sex, marital status, age, age squared, number of children in the household, educational and income categories, state fixed effects and survey round fixed effects. Standard errors clustered at the state level. P values in parentheses. "ESI" is defined as Employer Sponsored Insurance. "HIX" is defined as Health Insurance Exchange. "NHB" is defined as Non-Hispanic Black. "NHAsian" is defined as Non-Hispanic Asian. "NHother" is defined as Non-Hispanic Other Race.

Appendix Figure A1: Estimated changes in the probability of HIX Enrollment: by the HPS Survey Rounds and race/ethnicity



Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48] NOTES: Controls include sex, marital status, age, age squared, number of children in the household, educational and income categories, state fixed effects and survey round fixed effects. 95% CI are based on standard errors clustered at the state-round level. HIX is defined as the Health Insurance Exchange.

Appendix Figure A2: Mean DD and Regression Adjusted DD Estimates of the ARPA on Health Insurance Exchange Enrollment



Note. SOURCE: [Author's Analysis of U.S. Census Bureau Household Pulse Survey Rounds 1 – 48, excluding rounds 13 to 26.] NOTES: Controls include sex, marital status, age, age squared, number of children in the household, educational and income categories, state unemployment rate, state fixed effects and survey round fixed effects. 95% CI are based on standard errors clustered at the state level. Stars indicate coefficient is significantly different from NHW group.

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2 The Effect of the Access to Abortion Care on Mental Health: Evidence from the Dobbs Decision

2.1 Abstract

Context: After the US Supreme Court's *Dobbs vs. Jackson Women's Health Organization* (the *Dobbs decision*), abortion regulations are subject to state jurisdiction. Therefore, individuals in different states face very different levels of restrictions.

Methods: I compare self-reported symptoms of moderate to severe anxiety among individuals living in states with limited or no access to abortion care to individuals in states with relatively few or no restrictions before (January to May 2022) and after (June to December 2022) the Dobbs decision using high-frequency (monthly), nationally representative data from the Household Pulse Survey (HPS) and Difference in Difference (DD) regressions.

Findings: Estimates suggest that respondents in the restricted states are 7.69% (p=0.000) more likely to experience moderate to severe anxiety compared to unrestricted states. I show that trends in anxiety were similar in restricted and unrestricted states before the Dobbs decision (i.e. the parallel trend assumption holds), suggesting that the estimates may have a causal interpretation.

Conclusions: My results demonstrate that living in a state with restricted abortion access can increase the anxiety levels of its residents.

2.2 Introduction

According to the CDC abortion surveillance data, almost 620,000 abortions were carried out in the U.S. in 2020 (Kortsmit et al., 2022). Even almost 50 years after the Roe v. Wade decision, abortion remains stigmatized, and only about 30 to 40% of all abortions are reported in national household surveys (Lindberg et al., 2020). The Dobbs v. Jackson U.S. Supreme Court

Decision, which overturned the landmark precedent set by Roe v. Wade in 1973, poses implications for the physical and mental health of Americans. In May 2022, Politico published a leaked draft of the U.S. Supreme Court's opinion on Dobbs v. Jackson. The leaked opinion suggested that the Supreme Court would not only uphold a Mississippi law banning abortions after 15 weeks of gestation (except in the case of medical emergencies or severe fetal abnormality) but would go further to overturn Roe v. Wade (1973). The court's formal decision on June 24, 2022, closely mirrored this leaked draft opinion. As a result, abortion regulations are now subject to state jurisdiction.

I know that women who are denied an abortion report more anxiety symptoms, lower self-esteem, and lower life satisfaction than women who had an abortion (Biggs et al., 2017). A new study found that travel times to receive abortion services almost quadrupled after the Dobbs decision. They further found that almost all the increase was caused by an increase in travel time in states with a complete or six-week ban (Rader et al., 2022). Traveling long distances to access abortion care can add to mental distress (Biggs et al., 2020; McNamara et al., 2022). After the decision, a statement by the American Psychiatric Association noted, "*By dismantling nearly 50 years of legal precedent, the Court has jeopardized the physical and mental health of millions of American women*" (American Psychiatric Association, 2022)

Prior to the Dobbs decision, abortion was a constitutional right. Although states could implement restrictions (such as mandatory counseling, waiting, ultrasound, requiring abortions to be performed by a physician etc., and insurance coverage restrictions), the procedure itself was constitutionally protected. After the Dobbs decision, the procedure lost its federal protection and therefore states could implement a full ban on the service. In the six months following the Dobbs decision, an average of 5,377 fewer abortions have been provided in the U.S. each month

(Society of Family Planning, 2023). Individuals in different states face very different levels of restrictions ranging from a complete ban on abortions to almost full access. I use this inter-state variation in the level of restrictions to estimate Difference in Difference (DD) regressions using data from the Household Pulse Survey (HPS) data, a relatively high-frequency (monthly) nationally representative individual-level survey. Given the nature of the leaked court decision, I consider the publication of the leaked court opinion (May 2022) to be the treatment date. I also evaluate the official court decision (June 2022) in a robustness check and find qualitatively similar results.

The rest of the paper proceeds as follows. Section 2 discusses the identification strategy and the data used to answer the research objective. Section 3 presents the empirical results. Section 4 concludes.

2.3 Methods

I use data from the Household Pulse Survey (HPS), a nationally representative relatively high-frequency (monthly) individual-level survey. The HPS surveyed respondents about their mental health status along with a plethora of demographic and socioeconomic questions. The repeated cross sectional design survey is distributed to a different set of households for each round (every two weeks), beginning in April 2020. I use rounds 41- 52 from January to August 2022.

I use the New York Times database on state laws restricting abortion to classify states that restrict abortion (Times, 2022). According to the NYT database, 12 states have a complete abortion ban, and another five states ban abortion after six, 15, 18, or 20 weeks. I create a dummy variable, which takes the value one if a respondent lives in one of the restricted access states (17 states using the NYT database) and zero otherwise. The residents of these 17 states received the “treatment” of residing in a state with restricted access to abortion care. In contrast, residents of the remaining 33 states did not receive the treatment and therefore are in the comparison group. In the empirical implementation, I compare the anxiety levels of respondents from the “restricted” states to the “unrestricted” states before and after the Dobbs decision.

In each round, respondents were asked a four-question Patient Health Questionnaire-4 (PHQ-4) to assess respondents' mental health (Kroenke et al., 2009). In this paper, I focus on the first two questions, which were about the frequency of respondents’ anxiety and worry symptoms in the two weeks preceding an interview. The responses to each question are recorded on a scale of zero (not at all) to three (nearly every day). A combined score of three or more from these two questions suggests moderate or severe anxiety. This is the primary outcome variable.

In the regression analysis, I control for a number of individual-level demographic and socio-economic variables (such as age, gender, marital status, number of children, race, ethnicity, income, and education level) to account for observable differences. Furthermore, in all regressions, I include state-of-residence fixed effects.

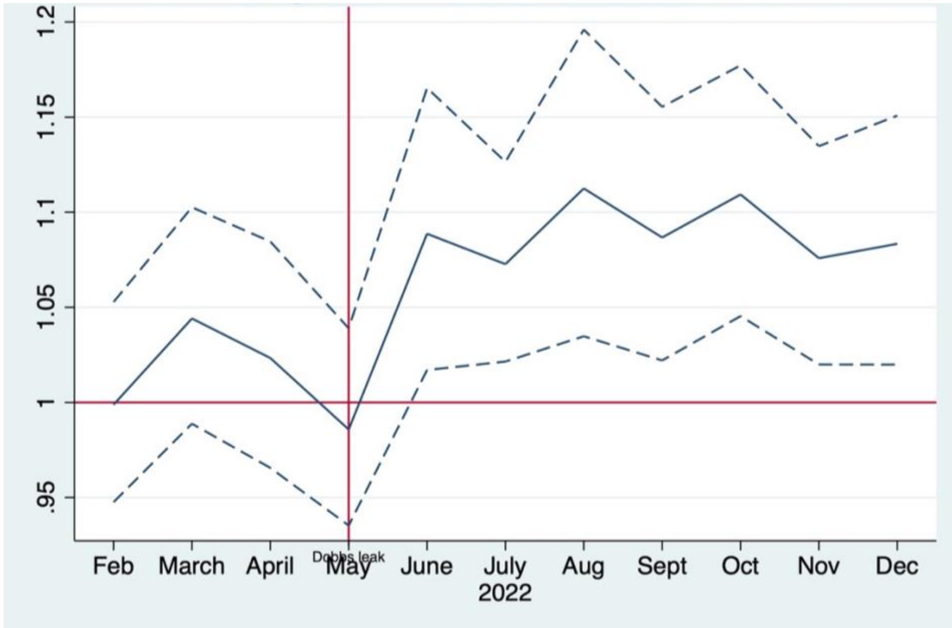
DD estimates may have a causal interpretation if the trends in the treatment and control groups would have been the same in the absence of the Dobbs decision. Testing this assumption is never feasible since it requires us to observe the counterfactual. Instead, following the literature, I test whether the trends in treatment and control states had similar trends before the Dobbs decision. Formally, I test this parallel trend assumption by testing for joint significance of all the pre-Dobbs interaction terms (i.e. from the months of February, March, and April). Another concern is the endogeneity of the treatment status. However, since the composition of the Supreme Court conclusively changed only after the death of Justice Ginsberg in September 2020, it is unlikely that a considerable number of individuals moved across states in anticipation of the Dobbs decision, especially given the COVID-19 pandemic in 2020 and 2021.

2.4 Results

As discussed in the introduction, the study population is nationally representative. I focus on rounds 41 (January 2022) to 52 (December 2022), yielding a sample size of n = 753,217. I omit respondents who failed to answer the questions regarding anxiety symptoms because this is the primary outcome variable. This reduces the sample size from 753,218 to 661,131. I also lose 70,464 observations because one of the control variables is missing, leaving us with a sample size of 590,667. Despite the reduction in sample size, the analytical sample remains representative (see Appendix Table B1). Appendix Table B2 shows the summary statistics of the study population (separated by their treatment status).

The regression-adjusted DD estimates are shown in Figure 2.1. The Dobbs decision was unexpectedly leaked during the 45th round of the HPS (on May 2, 2022). I use logit regressions (since the outcome is binary) and present the DD estimates in terms of odds ratios. In the regressions, the omitted round is the 41st Round (January 2022). None of the odds ratios associated with the interaction term in the pre-Dobbs leak period are statistically significant. Furthermore, the four pre-Dobbs coefficients are not jointly significant (P-value = 0.3250), suggesting that the critical parallel trend assumption holds in this context.

Figure 2. 1: Difference in Difference Regression adjusted Odds Ratios (with 95% CI)



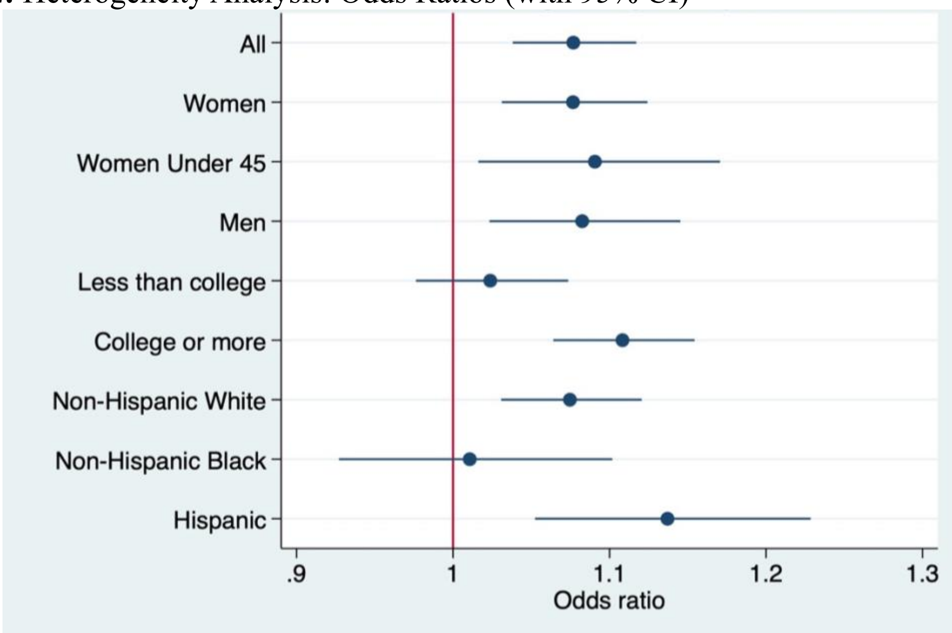
Note. Controls include gender, marital status, age, age squared, race, ethnicity, number of children in households, education, and income. All regressions include state fixed effects.

2.4.1 Heterogeneity Analysis

Next, I present estimates for several subgroups to check for treatment effect heterogeneity across racial and ethnic groups, genders and educational attainment. In these regressions, I interact the treatment dummy with a post-Dobbs dummy, which takes the value of one for June, July, and August surveys and zero otherwise. I consider the May survey round, during which the Dobbs decision was leaked, as the before period, since part of the survey was conducted before the leak, and many people may not have been aware of the decision immediately after the leak. I report the result of a sensitivity analysis where I drop the data from the May survey.

Figure 2.2 results suggest that the estimates are similar among women and men. This is consistent with polling data suggesting that a similar percentage of men and women believe that abortion should be illegal (12% among women vs. 14% among men in 2021) (Gallup Inc, 2017). Estimates are larger among respondents with a college degree compared to those without a college degree. This may be because more individuals with a college degree are aware of the decision. Estimates further suggest that the estimates are largest among Hispanic respondents compared to Non-Hispanic Black or Non-Hispanic White respondents, which is consistent with previous research (Goyal et al., 2020). Furthermore, estimates are higher for women of child bearing ages (ages 15 to 44.) This could be because this segment of the population would be more negatively affected by the policy change compared to others.

Figure 2. 2: Heterogeneity Analysis: Odds Ratios (with 95% CI)

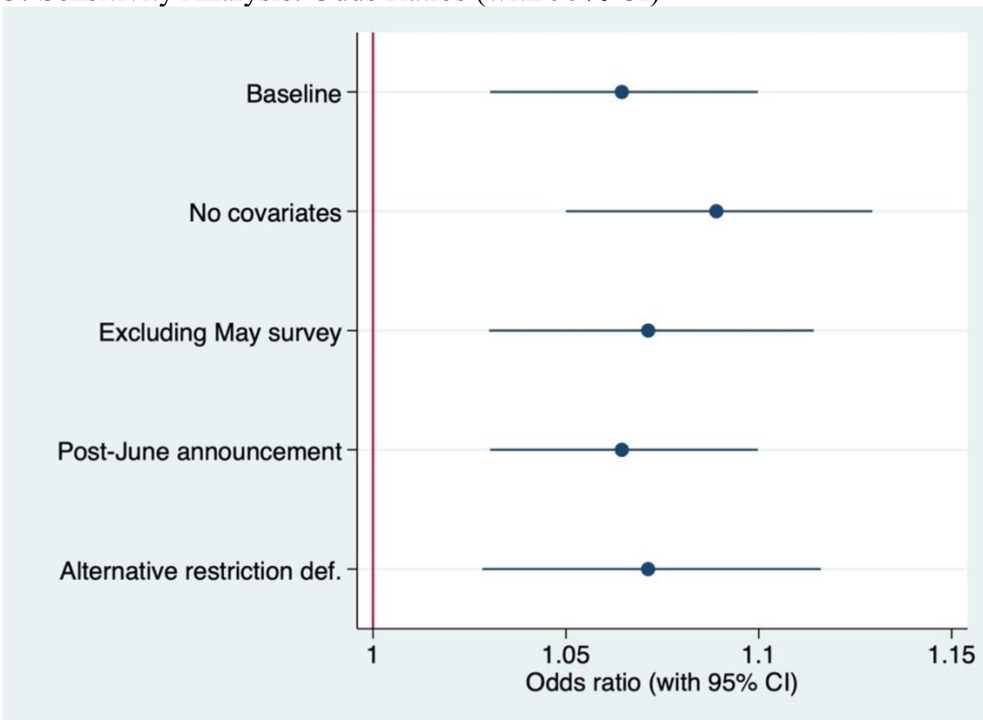


Note. Controls include gender, marital status, age, age squared, race, ethnicity, number of children in households, education, and income. All regressions include state fixed effects.

2.4.2 Sensitivity Analysis

I carried out several sensitivity analyses to check for the robustness of the results to reasonable changes in variable definitions, and specifications. The estimates are presented in Figure 2.3. I include the baseline estimate in Figure 2.3 for comparison purposes. I begin the sensitivity analysis by excluding all control variables from the regression. The estimate remains similar to the baseline. Next, I exclude data from the 45th round, which was conducted from April 27 – May 9, from the analysis because the Dobbs decision was leaked during the survey (on May 2). I get a similar estimate. Next, I re-estimate the model where I define the post-Dobbs period as the period after the Dobbs decision was formally announced on June 24, 2022. This means I classify only the last two rounds (July and August of 2022) as the post-Dobbs period. The estimated effect is qualitatively similar. Next, I use the classification from the Center for Reproductive Rights (CRR) (Center for Reproductive Rights, n.d.) as an alternative measure of restriction on abortion. This classification is based on legislative intents and actions and is somewhat more subjective. Nonetheless, as one would expect, the two classifications overlap. The CRR divides the states into five groups: states where abortion is illegal (the same 12 states with a complete ban), states that are hostile towards abortion (14 states), states where abortion rights are not protected (four states), states where abortion rights are protected (11 states), and states that have expanded access to abortion (nine states). The estimated effect is similar.

Figure 2. 3: Sensitivity Analysis: Odds Ratios (with 95% CI)



Note. Controls include gender, marital status, age, age squared, race, ethnicity, number of children in households, education, and income. All regressions include state fixed effects.

2.5 Conclusion

The estimates demonstrate that residents in states that restricted access to abortion care after the Dobbs decision experienced a higher incidence of self-reported moderate to severe anxiety compared to residents in states that did not restrict abortion. This effect could be due to the fact that the notion of residing in a state with abortion restrictions causes an increase in anxiety. Respondents may have increased fears for women in child-bearing age groups who are now unable to receive the procedure.

These results introduce the hidden cost for public policy. In this case, the hidden cost includes one of a negative impact to constituents' mental health. We should be concerned about who is bearing the burden of these undefined outlays. Furthermore, these costs ought to be included throughout policy conversations. If costs exceed the benefits of policy changes, then legislators need to re-evaluate the economic benefit of these decisions.

These results could introduce unintended consequences in the form of increased demand for behavioral health services. Given that constituents living in restriction states experienced an increase in moderate to severe anxiety, they may pursue psychotherapy services to address these symptoms. Policymakers should account for the potential increase in healthcare costs as a result of increased demand for services.

Appendix B

Appendix Table B 1: Checking the Representativeness of the Analytical Sample

	Largest possible sample		**Analytical sample**	
Covariate	**Mean**	**St. Dev.**	**Mean**	**St. Dev.**
Age	50.7170	16.0554	51.2244	15.8204
Female	0.5816	0.4933	0.5786	0.4938
Marital Status	0.5587	0.4965	0.5677	0.4954
Education				
Less Than High School	0.0068	0.0825	0.0048	0.0694
Some High School	0.0143	0.1189	0.0107	0.1029
High School	0.1188	0.3236	0.1058	0.3076
Some College	0.2095	0.4070	0.2016	0.4012
Associate's	0.1032	0.3042	0.1019	0.3025
Bachelor's	0.2866	0.4522	0.2971	0.4570
Graduate	0.2607	0.4390	0.2781	0.4481
N	744,998		590667	

Appendix Table B 2: Covariate Mean Values

	States with abortion restrictions (n=191,350)		States without abortion restrictions (n=399,317)	
Covariate	**Mean**	**Std. dev.**	**Mean**	**Std. dev.**
Marital Status	0.5817	0.4933	0.5610	0.4963
Age	51.4122	15.8705	51.1345	15.7956
Female	0.5840	0.4929	0.5760	0.4942
Non-Hispanic White	0.7672	0.4226	0.7830	0.4122
Non-Hispanic Black	0.0902	0.2865	0.0556	0.2291
Hispanic	0.0786	0.2690	0.0669	0.2498
Number of Children	0.5965	1.0392	0.5356	0.9642
Education				
Less Than High School	0.0057	0.0756	0.0044	0.0662
Some High School	0.0130	0.1133	0.0096	0.0975
High School	0.1189	0.3237	0.0996	0.2995
Some College	0.2239	0.4168	0.1909	0.3930
Associate's	0.1085	0.3111	0.0987	0.2982
Bachelor's	0.2857	0.4517	0.3026	0.4594
Graduate	0.2443	0.4296	0.2943	0.4557
Income				
Less than $25k	0.1255	0.3313	0.1005	0.3007
$25k-$34k	0.1009	0.3012	0.0806	0.2722
$35k-$49k	0.1193	0.3241	0.1008	0.3011
$50k - $74k	0.1815	0.3855	0.1619	0.3683
$75k - $99k	0.1424	0.3495	0.1408	0.3478
$100k - $149k	0.1677	0.3736	0.1889	0.3915
$150k - $199k	0.0764	0.2656	0.0979	0.2972
$200k+	0.0862	0.2807	0.1285	0.3347

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3 The Effect of Medicaid Managed Long Term Services and Supports on Home Health Provider Quality and Patient Health Outcomes

3.1 Abstract

Medicaid programs are increasingly transitioning from Fee for Service (FFS) delivery models to Managed Care (MC) models to control the rising costs of Long Term Services and Supports (LTSS). The rapid growth in Managed LTSS (MLTSS) has been met with opposition. Consumers worry that for-profit managed care organizations (MCOs) receiving capitation payments are incentivized to reduce access to care.

In this paper, I estimate the effects of Managed Long Term Services and Supports (LTSS) on Home Health Provider quality and patient health outcomes. I exploit the plausibly exogenous heterogeneity generated from the presence or lack of Medicaid MLTSS programs across United States (U.S.) counties and the staggered timing of implementation. Results demonstrate a causal reduction in the quality of care and patient health.

In 2019, 23 states had MLTSS programs in place. I evaluate HHPs using the Home Health Quality Reporting Program (HHQRP). This panel data evaluates (n=3,811) HHPs annually from 2013 - 2019. The Callaway Sant'Anna Difference in Difference (CSDiD) strategy is implemented to obtain a casual estimator of the effect of MLTSS on provider quality and patient health outcomes.

I construct two indices to evaluate provider quality and patient health. Each index ranges from 0 to 100, where a greater value indicates better quality or better health outcomes. Each index is composed of eight quality standards from the HHQRP. All 16 quality standards are aggregated at the provider level.

Results demonstrate that the provider quality and patient health indices decrease by 0.277 ($P<0.10$) and 0.392 ($P<0.05$) points, respectively, following the policy change. Furthermore, there is a pronounced effect for providers that are treated in 2014 and 2016. For those treated in 2016, the provider quality and patient health indices decreased by 3.985 ($P<0.01$) and 3.955 ($P<0.01$) points, respectively. Additionally, none of the interaction terms in the pre-treatment period are significant. They are also jointly insignificant, indicating that the parallel trends assumption is valid and therefore the results yield a causal interpretation.

In studying the effects of MLTSS, this work contributes to our understanding of the spillover effects of managed care plans in the form of reduced provider quality. This paper also contributes to the sparse body of literature that evaluates the effect of managed care delivery models on LTSS. This is the first quasi-experimental evidence on the effects of MLTSS on Medicaid HHP quality. I highlight the causal effects of MLTSS beyond the policymakers' analyses of these programs.

Given the increasing prevalence of these policies, the population affected by these negative results is growing. This warrants concern and potential need for monitoring the quality of providers due to the widespread effects of these policies.

3.2 Introduction

Medicaid programs are increasingly transitioning from Fee for Service delivery models to Managed Care models to provide Long Term Services and Supports (LTSS). The growth in Managed Long Term Services and Supports policies (MLTSS) has been significant in recent decades. In 2019, 23 states had MLTSS programs compared to only 8 states in 2004 (Wysocki et al., 2020).

One concern is that for-profit Managed Care Organizations (MCOs) paid on a capitation basis may be incentivized to reduce access to care. Additionally, if states permit MCOs to alter provider reimbursement rates, these two factors could reduce overall financial resources available to providers. Providers may then respond by reducing staffing which could indirectly reduce care quality and worsen patient outcomes. This paper studies the effects of MLTSS implementation on provider quality and patient health outcomes.

Although the policy decision is at the state level, treatment is at the county level because not all states adhere to statewide implementation. I exploit plausibly exogenous variation in the staggered timing of treatment driven by if and when MLTSS delivery models are implemented across United States (US) counties. I apply a Difference in Difference identification strategy to provider-level panel data to evaluate both research objectives. I focus on Medicare-certified Home Health Providers (HHPs) because a majority of Medicaid's LTSS spending funds HHPs and other home and community based providers (Watts et al., 2020).

I find overall negative treatment effects. As a result of MLTSS programs, provider quality is reduced and patient health outcomes are worse off compared to that of FFS-LTSS delivery models. These results can be presented with a causal interpretation because the parallel trends assumption holds to be true. I find a stronger negative effect for providers treated in the years 2014 and 2016.

An estimated 11 million Medicaid beneficiaries utilized Long-Term Services and Supports (LTSS) in 2019 (Kim et al., 2019). Of these 11 million, 46% received these services under managed care. LTSS includes care for older adults and individuals with disabilities in need of support[1]. This care encompasses assistance with activities of daily living (eating, bathing, and dressing) and instrumental activities of daily living (housekeeping and finance management) for extended periods of time.

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[1] The need for support can be due to age, physical, cognitive, developmental or chronic health conditions or other functional limitations.

These services are typically provided in one of two settings: 1) Institutional settings (Skilled Nursing Facilities, Intermediate Care Facilities for Individuals with Intellectual Disabilities) or 2) Home and Community Based settings (Assisted Living Facilities, Home Health Providers (HHPs)) (Reaves & Musumeci, 2015). In this paper, I focus on LTSS provided in the home by HHPs.

MLTSS is the delivery of LTSS via Managed Care Organizations (MCOs). The MCOs receive capitated payments (e.g., a per-member-per-month premium) to provide LTSS for Medicaid beneficiaries. MCOs are effectively incentivized to reduce the amount spent per beneficiary. This can be done via one of two strategies: 1) Reducing service utilization or 2) Reducing the price paid to service providers (Duggan & Hayford, 2013; Geruso et al., 2020).

I use the Home Health Quality Reporting Program (HHQRP) annual panel data to evaluate Medicare-certified HHPs and patient health outcomes from 2013 to 2019. I implement the Callaway Sant'Anna Difference in Difference (CSDiD) strategy. The CSDiD strategy exploits the heterogeneity generated from the staggered implementation across treated counties (Callaway & Sant'Anna, 2021).

The HHQRP evaluates providers on a variety of metrics, including process measures and patient health outcomes. I construct two indices from these ratings to represent overall provider quality and patient health outcomes. These are my two primary outcome variables. I provide details on the index construction process in section 3.1.

The central identification challenge is that FFS-LTSS counties may be an imperfect comparison group. A perfect comparison group is difficult to identify because MLTSS providers may serve clients that are innately different than those served in FFS-LTSS counties. To address this, I implement inverse probability weighting (IPW) to account for confounders that influence the probability for a provider to be subjected to MLTSS.

In studying the effects of MLTSS, this work expands our limited understanding of how these policies affect both providers and beneficiaries. This paper evaluates provider level outcomes relevant to LTSS recipients such as older adults with disabilities. This is the first quasi-experimental evidence on the effects of MLTSS on Medicaid HHP quality. By comparing outcomes between the two groups of providers, I obtain a causal estimator to directly explain the effects of MLTSS.

Despite the substantial number of MLTSS beneficiaries, there is minimal peer-reviewed work evaluating the effects of these care delivery models on provider quality and overall patient health outcomes. I highlight the effects of these programs beyond policymakers' analyses (Dobson et al., 2017). Additionally, I conduct a variety of heterogeneity and sensitivity analyses to assess the robustness of these findings.

The rest of the paper proceeds as follows. Section 2 discusses the policy context of MLTSS programs and reviews relevant literature. Section 3 describes the data and empirical strategies for both research objectives. Section 4 presents the empirical results and robustness checks. Section 5 reviews limitations and conclusions.

3.3 Background

3.3.1 Policy Context

Medicaid LTSS spending has consistently increased every year for the last two decades. From 1995 to 2016, Medicaid LTSS spending grew by 192%. Medicaid is the primary payer of all national LTSS expenditures (Watts et al., 2020).

The upward-bound trajectory of spending prompted concern from policymakers. That concern served as the impetus for the MLTSS programs that I see today. These policies have the potential to provide less expensive home and community based alternatives as well as improve care coordination and reduce the use of unnecessary services (Wysocki et al., 2020).

MLTSS programs have five goals: 1) Rebalancing Medicaid LTSS spending from institutional settings to Home and Community Based Services (HCBS) settings, 2) Improving consumer health and satisfaction, 3) Reducing Medicaid HCBS waiver waiting lists, 4) Increasing budget predictability and 5) Containing costs (Dobson et al., 2017). In this paper, I focus on evaluating the second goal. I investigate whether or not MLTSS programs meet this expectation by evaluating the quality of providers and patient health outcomes.

Managed care plans were first introduced in the early 1970s with the goal of controlling costs as part of the Health Maintenance Organization Act (Gruber et al., 1988). At first, these plans were only implemented to deliver acute and primary care. However, managed care has permeated the LTSS market in recent decades.

Arizona was the first state to implement MLTSS in 1988. The prevalence of MLTSS programs grew significantly from 2004 to 2012 when the number of states with these programs doubled (Saucier et al., 2012). As of 2018, 23 states had 33 MLTSS programs in place (Wysocki et al., 2019). Appendix Table C1 provides the list of states and respective start dates of MLTSS programs as of September 2019.

National enrollment in MLTSS programs totaled 1.8 million beneficiaries by 2017. This was a huge increase from the previous 800,000 beneficiaries in 2012. However, there was (and still is) wide variation in enrollment across states. In 2017, California, Illinois, New York, Ohio and Texas each served more than 100,000 beneficiaries compared to Pennsylvania with fewer than 500 enrolled (Lewis et al., 2017).

MLTSS programs primarily serve older adults and people with physical disabilities. Second to this population are children with disabilities, dually eligible beneficiaries and individuals with behavioral health conditions, traumatic brain injuries and intellectual/developmental disabilities (Dobson et al., 2021; Lewis et al., 2017).

MLTSS policies vary between and within states with regards to 8 general features: 1) Timing of implementation, 2) Federal authority used to implement the program, 3) Enrollment type (mandatory vs. voluntary), 4) Target population, 5) Minimum level of care needed to enroll, 6) Services covered by capitation 7) Dual eligibility and 8) Statewide implementation (Wysocki et al., 2020).

The third feature introduces a threat to the identification strategy. Some states do not require Medicaid LTSS beneficiaries to receive these services via managed care but rather they have the Fee for Service delivery model option. To address the endogeneity concerns that arise from these "voluntary enrollment" cases, I exclude providers in MLTSS counties with voluntary enrollment. I utilize several of the other features to implement heterogeneity analyses and discuss this in section 3.2. Appendix Tables 1, 2 and 3 provide a comprehensive review of each state's MLTSS program.

3.3.2 Literature Review

This paper contributes to three general bodies of literature: Managed Care (MC), Managed Long Term Services and Supports (MLTSS) and Long Term Care (LTC). I review all three in this section.

Literature on patient outcomes in MC is mixed. Aizer et al. finds that California's managed care enrollees had higher rates of delivering low-birthweight babies and having neonatal deaths compared to their fee for service counterparts (Aizer et al., 2007). Other studies find that managed care has no effect on birthweight or infant mortality (Conover et al., 2001; Duggan, 2004; Howell et al., 2004; Kaestner et al., 2005). Controlled trials evaluating primary care management find an improvement in patient satisfaction, functional ability, mortality, bed disability days and overall quality of life (Cohen et al., 2002; Counsell et al., 2007; Schraeder et al., 2001; Stock et al., 2008).

Managed home care literature is both sparse and primarily focusing on healthcare outcomes (hospital admissions, costs). Frick et al. uses a prospective nonrandomized clinical trial evaluating 455 community-dwelling older patients and finds that managed home care programs reduce costs (Frick et al., 2009). Home-based care management also moderately reduces hospital days. However, this effect is highly dependent on the patient's level of acuity (Hughes et al., 1997). Bouman et al.'s systematic review finds that managed home care betters patients' functional status but does not reduce mortality or improve health status (Bouman et al., 2008).

My paper contributes to the MMC body of literature in that I evaluate provider quality and patient health outcomes rather than healthcare outcomes (reduced costs and hospital admissions.) I also contribute to the limited body of managed home care literature by solely focusing on Home Health Providers.

A majority of the MLTSS literature draws on descriptive analyses of state program surveys (Grabowski, 2006). In Florida, a survey for MLTSS beneficiaries found that 60% of respondents improved their overall health since enrollment (Dobson et al., 2017). Mathematica compared self-reported quality of life metrics in 16 MLTSS states to those in FFS programs.

Using a Bayesian hierarchical model, they find that the odds of favorable survey responses regarding experience of care and quality of life were 28% higher for MLTSS enrollees compared to that of FFS enrollees (Wysocki et al., 2020). Texas saw a marginal decrease in potentially preventable hospital admissions between 2013 and 2014 for its STAR+PLUS program (Border et al., 2002). The Minnesota Senior Health Options (MSHO) program found that its members were 48% less likely to have a hospital stay. MSHO beneficiaries were also 6% less likely to have an outpatient emergency department visit (Kane et al., 2004). Although these studies are encouraging, they only compare a few programs. My study differs from other work in that I evaluate programs across all 50 states rather than just a select few. I also focus on home health providers specifically rather than a broad array of healthcare providers.

The only work obtaining a causal estimator is Potter and Bowblis' evaluation of nursing homes. They use a Difference in Difference approach to evaluate nursing home providers in three states that have implemented MLTSS (Massachusetts, Kansas and Ohio). They find little evidence that MLTSS reduces quality of care or occupancy for nursing home providers (Potter & Bowblis, 2021).

A majority of the work evaluating LTSS providers concurs that quality has improved in the last decade (Harrington et al., 2017). Nursing homes have demonstrated significant progress in overall quality in recent decades. From 2011 to 2016, there was an overall reduction in the use of physical constraints, pressure ulcers, use of antipsychotics and urinary tract infections among residents (Abt Associates Inc. & Colorado Foundation for Medical Care, 2013). The increase in nurse staffing levels contributed to these positive changes. States with higher minimum staffing requirements benefit from these improved quality outcomes (Bostick et al., 2006; Bowblis, 2011; Dellefield et al., 2015; Harrington et al., 2017).

The same positive trends were found among HHPs. From 2004 to 2016, there was a significant increase in the percentage of HHP patients that improved in walking, increased overall mobility and had less pain while receiving services (Harrington et al., 2017). However, outcomes vary by ownership type. Nonprofit HHPs perform better in terms of care quality, patient outcomes and overall efficiency compared to that of for-profit providers (Cabin et al., 2014; Grabowski et al., 2009). These analyses utilize the same data from the Home Health Quality Reporting Program.

My paper contributes to this literature in that I develop a framework that evaluates the drivers of provider quality. The primary explanatory variable in my analysis which is lacking from the literature is the presence of MLTSS policies.

3.4 Data and Identification Strategies

3.4.1 Data

This paper utilizes publicly available Home Health Provider (HHP) data to address two research objectives: 1) The effect of MLTSS implementation on HHP quality and 2) The effect of MLTSS implementation on HHP patient health outcomes.

I use panel data from the Home Health Quality Reporting Program (HHQRP) to evaluate providers from 2013 to 2019. This data is available through the Provider Data Catalog on the Centers for Medicare and Medicaid Services (CMS) website, commonly known as the Home Health Care Compare database. The HHQRP evaluates various quality metrics for Medicare certified HHPs using both the Outcome and Assessment Information Set (OASIS) and Medicare claims data. This panel data includes most, if not all, HHPs in the US. The data aggregates evaluations of home health quality episodes at the provider level. A home health quality episode consists of a Start of Care (SOC) or Resumption of Care (ROC) assessment and a matching End of Care (EOC) assessment for each patient, confined within a 12-month period[2]. The data excludes home health quality episodes for four populations: 1) Patients under the age of 18, 2) Patients receiving maternity services, 3) Patients receiving only chore or housekeeping services and 4) Patients for whom the payment source is neither Medicare nor Medicaid.

There are 16 available quality standards that evaluate patient outcomes and provider process measures, both of which are aggregated at the provider level. Table 3.1 provides the description and source (OASIS vs. Medicare claims) for each standard.

Each standard is a calculated percentage. The percentage reflects the share of all home health quality episodes in the preceding 12 months for which an individual provider satisfied that quality benchmark. Therefore, each metric ranges from 0 to 100. A higher value indicates a higher percentage of home health quality episodes for which the provided care met the stated quality standard. For example, “Timely Initiation of Care” represents the share of home health quality episodes for which the Start of Care (SOC) or Resumption of Care (ROC) date was on the physician-ordered SOC/ROC date. The denominator is the total number of home health quality episodes in the preceding 12 months.

For most standards, a higher number reflects a higher quality provider. However, this is untrue for two of the 16 standards. Those are: 1) Acute Care Hospitalization During the First 60 Days of Home Health and 2) Emergency Department Use Without Hospitalization During the First days of Home Health. A lower value of these standards would indicate a higher quality provider given that these are perceived as negative occurrences for home health patients.

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[2] The SOC and EOC are determined by agency policy or physician/allowed practitioner order.

Table 3. 1: Home Health Quality Reporting Program: Patient Health Outcomes and Provider Process Measures

Patient Outcome	Description	Source	Abbreviation
Improvement in Bathing	Percentage of home health quality episodes during which the patient got better at bathing.	OASIS	Bathing
Improvement in Bed Transferring	Percentage of home health quality episodes during which the patient improved in ability to get in and out of bed.	OASIS	Bed
Improvement in Dyspnea	Percentage of home health quality episodes during which the patient became less short of breath or dyspneic.	OASIS	Dypsnea
Improvement in Management of Oral Medications	Percentage of home health quality episodes during which the patient improved in ability to take their medicines correctly (by mouth).	OASIS	Medications
Improvement in Pain Interfering with Activity	Percentage of home health quality episodes during which the patient's frequency of pain with activity or movement improved.	OASIS	Pain
Improvement in Ambulation-Locomotion	Percentage of home health quality episodes during which the patient improved in ability to ambulate.	OASIS	Ambulation
Improvement in Status of Surgical Wounds	Percentage of home health quality episodes during which the patient demonstrates an improvement in the condition of surgical wounds.	OASIS	Surgical
Acute Care Hospitalization During the First 60 Days of Home Health	Percentage of home health stays in which patients were admitted to an acute care hospital during the 60	Medicare claims	Hospitalization

	days following the start of the home health stay.		
Provider Process Measures	**Description**	**Source**	**Abbreviation**
Depression Assessment Conducted	Percentage of home health quality episodes in which patients were screened for depression (using a standardized depression screening tool) at start/resumption of care.	OASIS	Depression
Multifactor Fall Risk Assessment Conducted for All Patients who Can Ambulate	Percentage of home health quality episodes in which patients had a multi-factor fall risk assessment at start/resumption of care.	OASIS	FallRisk
Diabetic Foot Care and Patient/Caregiver Education Implemented	Percentage of home health quality episodes in which diabetic foot care and patient/caregiver education were included in the physician-ordered plan of care and implemented (at the time of or at any time since the most recent SOC/ROC assessment).	OASIS	DiabeticCare
Influenza Immunization Received for Current Flu Season	Percentage of home health quality episodes during which patients received influenza immunization for the current flu season.	OASIS	Influenza
Pneumococcal Polysaccharide Vaccine Ever Received	Percentage of home health quality episodes during which patients were determined to have ever received Pneumococcal Polysaccharide Vaccine.	OASIS	Pneumococcal

Timely Initiation of Care	Percentage of home health quality episodes in which the start or resumption of care date was on the physician-ordered SOC/ROC date (if provided), otherwise was within 2 days of the referral date or inpatient discharge date, whichever is later.	OASIS	Timely
Drug Education on All Medications Provided to Patient/Caregiver	Percentage of home health quality episodes during which patient/caregiver was instructed on how to monitor the effectiveness of drug therapy, how to recognize potential adverse effects, and how and when to report problems (at the time of or at any time since the most recent SOC/ROC assessment).	OASIS	DrugEducation
Emergency Department Use Without Hospitalization During the First days of Home Health	Percentage of home health stays in which patients used the emergency department but were not admitted to the hospital during the 60 days following the start of the home health stay.	Medicare claims	ED

Note. SOURCE: [Home Health Quality Reporting Program, commonly known as Home Health Compare Database.] NOTES: [“OASIS” is the abbreviation for Outcome and Assessment Information Set. “SOC” is the abbreviation for Start of Care. “ROC” is the abbreviation for Resumption of Care.]

In order to evaluate providers from a holistic perspective, I construct two indices using the formulas below. One evaluates patient health outcomes, the other evaluates provider quality. Each index is the mean of all 8 quality standards in that category (patient health or provider process measures).

$$PatientOutcomes = [Bathing + Bed + Dyspnea + Medications + Pain + Ambulation + Surgical + (100 - Hospitalization)]/8 .$$

Therefore, $PatientOutcomes$ can range from 0 to 100. A higher value indicates better patient health outcomes.

$$ProviderQuality = [Depression + FallRisk + DiabeticCare + Influenza + Pneumococcal + Timely + DrugEducation + (100 - ED]/8$$

Therefore, $ProviderQuality$ can range from 0 to 100.

It is important to note that the 8 provider process measures are risk-adjusted according to HHQRP protocols to account for the natural progression of disease among home health patients. These adjustments compensate for risk factors so that provider performance is not disproportionately affected by patients with higher acuity needs. The risk adjustment process involves constructing a logistic regression to predict the probability that a quality episode will evidence the outcome based on quality episode-level covariates. The details of this risk adjustment process are in Appendix Section A.

In addition to these quality ratings, the HHQRP data offers provider-specific characteristics including: address, ownership type, services offered, county and certification date.

Data is released quarterly and evaluates quality episodes from the preceding 12-month period. For example, the Q1 2012 release reflects evaluations from Q1 2011 - Q4 2011. Therefore, the Q2 2012 release reflects evaluations from Q2 2011 - Q1 2012. To address this overlap, I eliminate quarterly updates and focus on annual evaluations. By doing so, each observation reflects a single year of evaluation for a provider.

Although provider data is available from 2003 to 2022, there are two limitations that prompt me to focus on data from 2013 to 2019. First, OASIS-C introduced a new set of quality ratings in January 2010. These new ratings provided a more comprehensive evaluation of providers. Prior to 2010, there were a limited number of quality metrics. Appendix Section A includes a detailed summary of the OASIS-C data changes after 2010. Second, insufficient reporting in the years of 2010-2012 and 2020 to 2022 yields numerous missing values. As a result of these two limitations, I focus on data for the years of 2013 to 2019.

From the sample of 94,770 observations, I lose 46,738 observations due to missing outcome variables or covariates. From this, I lose 18,443 observations for providers that have fewer than 20 patients evaluated for that year or less than 6 months of data. I then eliminate 2,435 observations in which the provider moved to a different county in the 2013 to 2019 time frame in order to prevent treatment reversibility (i.e. provider moving from an MLTSS to an FFS county). From this, I eliminate 1,750 observations for which MLTSS was implemented under voluntary enrollment to avoid selection bias. Finally, I eliminate providers (n= 3,347 observations) that do not have enough data available to construct the CSDiD framework. This excludes providers that fall under one of three categories: 1) providers that only have a single observation 2) providers that only have observations after treatment has been implemented 3) providers lacking observations in the year immediately prior to implementation. This provides a final sample size of n = 22,057 observations and n = 3,811 Home Health Providers. This panel data is unbalanced due to the fact that some HHPs opened or closed after 2013. Appendix Table C4 demonstrates the number of providers in the treatment and control groups for each year from 2013 to 2019.

To address potential confounders, I include provider and county characteristics. Provider characteristics include ownership type (for profit vs. non profit) and indicators for services offered (medical social services, nursing care services, occupational therapy, physical therapy, speech therapy, home health aide services). County characteristics are mostly derived from US

Census Data, including the percentage of the county's population below the federal poverty level, percent black and percent over 65. I also control for a county's degree of rurality by including the Urban Influence Code[3]. I also include the Medicaid expansion status of the state in which the county is located. In order to adhere to the time-constant covariate requirement of the Callaway Sant'Anna DiD method, only base period values[4] are used for estimation.

Appendix Table C5 provides an overview of covariate summary statistics by treatment status. The treatment group includes observations for providers in MLTSS counties. The control group includes all observations for providers in FFS-LTSS counties.

3.4.2 Empirical Strategies

The objectives of these analyses are to understand how the implementation of MLTSS affects Home Health Provider quality and patient health outcomes. I leverage plausibly exogenous variation in the staggered timing of treatment driven by if and when MLTSS delivery models are implemented at the county level. I construct Callaway Sant'Anna Difference in Difference (CSDiD) estimators to measure the impacts of MLTSS. I also implement a variety of heterogeneity, sensitivity and robustness analyses.

This strategy adheres to the recent work from Callaway and Sant'Anna on Difference in Difference (DiD) methods with multiple time periods (Callaway & Sant'Anna, 2021). Although there are a variety of DiD techniques, such as the Two-Way Fixed Effects (TWFE) and the dynamic TWFE, the CSDiD method obtains unbiased estimators. This is primarily because of this policy's staggered treatment timing.

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[3] The Urban Influence Code ranges from a value of 1 to 12 and is designated by the U.S. Department of Agriculture Economic Research Service. Appendix Table C6 provides definitions for each code.
[4] The base period is the earliest period that the provider appears in the data.

The Two-Way Fixed Effects (TWFE) estimator is not robust to treatment effect dynamics in the case of multiple treatment periods (Sun & Abraham, 2021). This is because it exploits three different types of variation: Treated vs. Untreated, Treated vs. Not Yet Treated, and Treated vs. Already Treated. Although the first two comparisons yield consistent estimators, the last group can produce unreliable results. As a consequence, the TWFE method is unable to recover a causal estimator (Borusyak et al., 2022; de Chaisemartin & D'Haultfœuille, 2021). Another consequence to this "poor" comparison of Treated vs. Already Treated units is the issue of negative weights, producing negative estimators when in reality, the true effect is positive. Therefore, the TWFE estimators typically produce downward biased estimates (Goodman-Bacon, 2021). In order to obtain a causal estimate, I narrow the focus to exploit only two types of variation: Treated vs. Never Treated and Treated vs. Not yet Treated using the CSDiD method.

A dynamic TWFE model could potentially address treatment effect dynamics. However, the lead and lag estimators of this dynamic model can still be biased due to effects from other periods (Sun & Abraham, 2021).

This empirical strategies section is structured as follows. First, I will explain the Callaway Sant'Anna Difference in Difference (CSDiD) method. Then, I will review identifying assumptions.

The CSDiD method produces one estimator for each group at each time period, yielding numerous group-time Average Treatment on the Treated (ATT) estimators. An individual group (g) refers to a cohort of providers treated in the same year. For example, the "2014 group" includes all providers treated in 2014. The CSDiD framework measures the ATT for each group at each time period (t) in the data, denoted by $ATT(g, t)$. Therefore, $ATT(2014{,}2017)$ is the ATT in 2017 for providers "treated" by MLTSS in 2014.

Each $ATT(g,t)$ measures the difference between the outcome for a unit in treatment group g at time t and the outcome for an untreated unit at time t, denoted by,

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|C = 1], for\ t \geq g$$

Where $Y_t(g)$ is the outcome for a unit in treatment group g at time t, $Y_t(0)$ is the outcome for an untreated unit at time t. The CSDiD framework allows for two potential control groups: Not Yet Treated $(C = 0)$ and Never Treated $(C = 1)$. I present results using the Never Treated control group. This includes all providers that never receive treatment from 2013 to 2019.

I use provider-level data to evaluate provider quality and patient health outcomes for each year from 2013 to 2019. The CSDiD method produces one estimator for each of the 7 groups at each of the 6 time periods, yielding 42 group-time $ATT(g,t)$ estimators. However, due to missing data (i.e. no available observations from 2016 to 2019 for providers treated in 2017), a few $ATT(g,t)$ are not estimated, resulting in 37 $ATT(g,t)$ estimators.

I use Ordinary Least Squares (OLS) to obtain each of the $ATT(g,t)$ estimators using equation (1).

$$Y_{i,t} = \beta I(MLTSS_{it}) + \sum_{j=1}^{6} \lambda_j I(YEAR = j) + \sum_{j=1}^{6} \gamma_j I(YEAR = j) * I(MLTSS_{it}) + \delta X_{i,c} + P_i + \varepsilon_{i,t} (1)$$

Where $Y_{i,t}$ is one of the two outcome variables for each provider i in year t. $X_{i,c}$ is a vector of controls, which includes provider and county characteristics. P_i includes provider fixed effects. The parameters are identified from intra-provider variation. Therefore, the parameter β is not identified. $\lambda_j s$ represents the time effects and $\gamma_j s$ represent the $ATT(g,t)$s for each group-time cohort. Standard errors are clustered at the provider level and wild bootstrapping is implemented to account for heteroskedasticity.

Inverse Probability Weighting (IPW) is implemented to account for covariates that influence the propensity of treatment exposure. This propensity is the result of a logistic regression calculated from potential confounders. Providers are then weighted by the inverse of this probability.

By producing multiple $ATT(g, t)$estimates, I can analyze how the treatment effect varies across groups, time periods and length of treatment exposure. However, it can be challenging to provide a simple interpretation of a high volume of $ATT(g, t)$estimates. Therefore, I produce aggregated parameters that represent a weighted average of all estimators, denoted by,

$$\theta = \sum_{g=2}^{7} \sum_{j=2}^{6} w(g, t) \cdot ATT(g, t) \quad (2)$$

Where θ is the aggregated causal parameter and $w(g, t)$ are chosen weighting functions. In this paper, I present results using three weighting schemes: Simple, Group and Dynamic. The empirical specifications of these three weighting schemes are in Appendix Section B.

Interpreting θ as the causal effect of MLTSS requires two assumptions. First is the standard parallel trends assumption - that absent treatment, FFS and MLTSS groups would have trended similarly. To substantiate this assumption, I check that there are no differential pre-trends between the two groups. I do so by estimating the regression equation (1) above and testing for the joint significance of all pre interaction terms.

The second assumption requires that providers are unaware of treatment prior to being treated and therefore do not “anticipate” MLTSS implementation. To evaluate this, I check that there are no noticeable differences in trends in the periods leading up to the period of treatment.

The CSDiD method also assumes an irreversibility of treatment - where once a unit is treated, it remains treated forever. This is true in here because once a county is subjected to MLTSS, it never reverts back to FFS-LTSS.

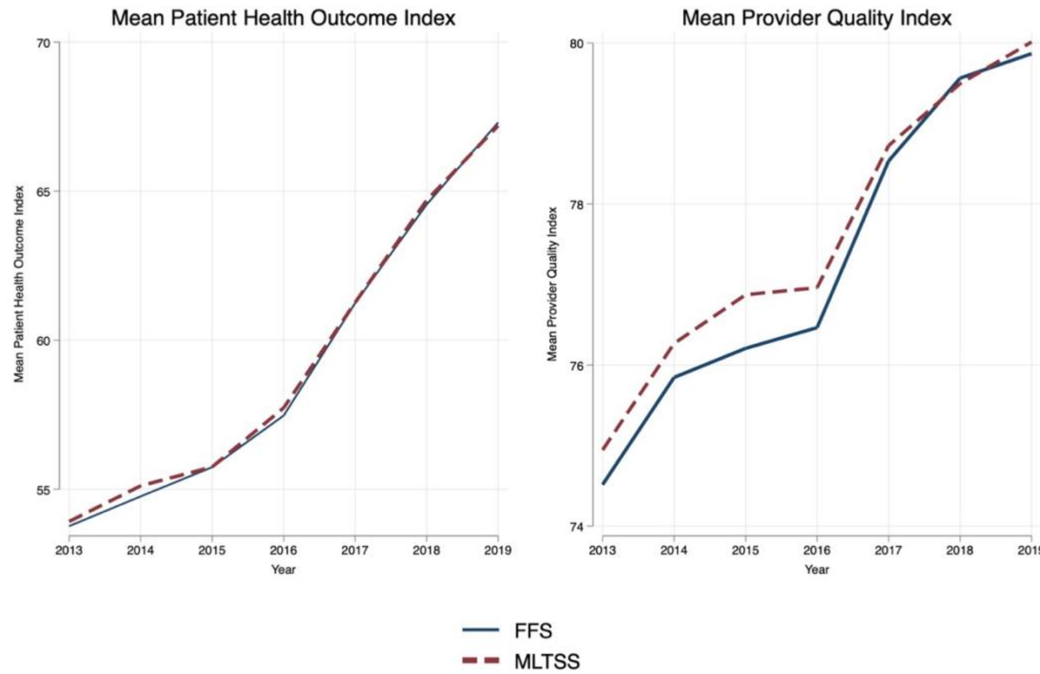
3.5 Results

This paper finds negative results of the effect of MLTSS on provider quality. This results section is structured as follows: First, I present an overview of trends across FFS and MLTSS counties for both outcomes. Then, I test for the parallel trends assumption. Next, I present results for the aggregated CSDiD estimators for both the Mean Difference in Difference and regression adjusted estimates. Regression adjusted estimates include all of the covariates. Finally, I present the heterogeneity, sensitivity and robustness analyses.

3.5.1 Trend Overview

The trend in provider quality and patient health outcomes have been on an upward trajectory throughout the time frame (2013-2019) for both FFS and MLTSS providers (Figure 3.1). The mean provider quality index increased by 5.35 points across FFS providers and 5.07 points across MLTSS providers from 2013 to 2019. I find similar improvements for the patient health outcome index. The mean patient health outcome index increased by 13.54 points across FFS providers and 13.27 points across MLTSS providers from 2013 to 2019. Figure 3.1 demonstrates a consistent upward trajectory for both outcome variables, indicating that regardless of treatment, quality improved. The underlying data for the graphs is in Appendix Table C7.

Figure 3. 1: Mean Patient Health Outcome Index and Provider Quality Index for All Home Health Providers by Year

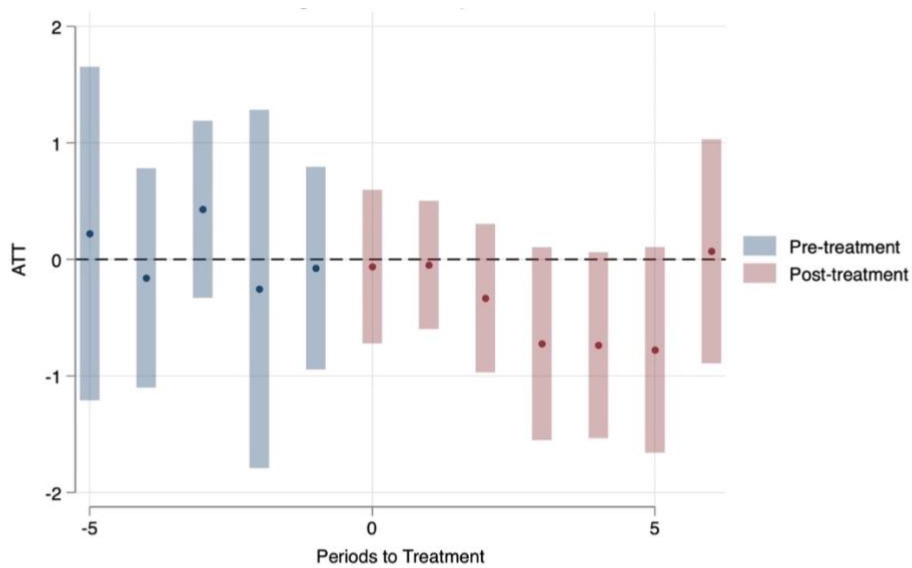


Note. SOURCE: Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.

3.5.2 Parallel Trends

As discussed above, the interpretation of the regression estimates depends on the parallel trend assumption. Therefore, before discussing the main results I verify that this assumption holds. I estimate equation (1) to test whether there are any differences in the pre-MLTSS trends in patient health outcomes and provider quality across FFS and MLTSS counties. The estimated interaction terms ($\gamma_j s, j = 1\ to\ 12$) for the patient health outcome index are presented in Figure 3.2. The dots represent the estimated coefficients and the vertical rectangles indicate 95% confidence intervals. They do not demonstrate any visual trend. To formally test the parallel trends assumption, I test that all of the pre-treatment coefficients are jointly zero. The p-value associated with this hypothesis is 0.6301. The null hypothesis is that all pre-treatment estimates are equal to 0. Therefore, the parallel trends assumption holds, indicating that the main results can be presented with a causal interpretation.

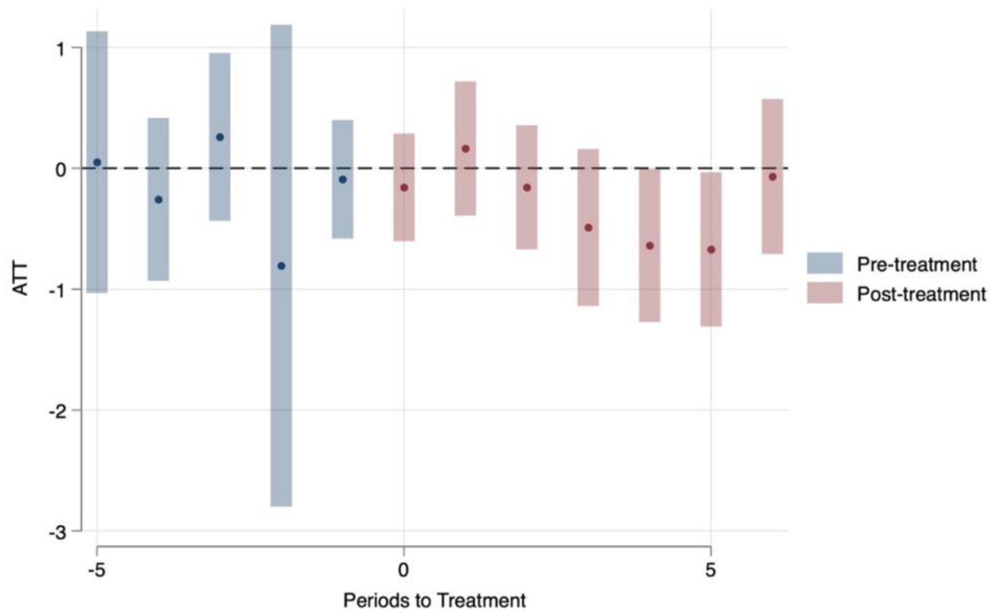
Figure 3. 2: The Regression Adjusted Estimates of the Effect of Managed Long Term Services and Supports on Patient Health Outcomes (Dynamic Aggregation Weights)



Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented for regression adjusted estimates. 95% CI are based on wild bootstrapped standard errors.]

I find similar results for the regression adjusted estimates of MLTSS on provider quality. These estimated interaction terms are presented in Figure 3.3 below. The p-value associated with this null hypothesis is also 0.1786. Therefore, the parallel trends assumption is validated and the results for provider quality can also be presented with a causal interpretation.

Figure 3. 3: The Regression Adjusted Estimates of the Effect of Managed Long Term Services and Supports on Provider Quality (Dynamic Aggregation Weights)



Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented for regression adjusted estimates. 95% CI are based on wild bootstrapped standard errors.]

3.5.3 Simple Weighting Aggregation

The simple aggregation presents negative results. Table 3.2 demonstrates the Mean DD and regression adjusted estimates for both outcomes. These results indicate that MLTSS implementation improves provider quality by decreasing the provider index by 0.277 (P<0.10) points. The results for patient health outcomes demonstrate a similar significantly negative treatment effect, where MLTSS decreases the patient index by 0.392 (P<0.01) points. These results indicate that reduced provider quality has negative downstream effects on patient health.

Table 3. 2: The Mean Difference in Difference and Regression Adjusted Estimates of the Effect of Managed Long Term Services and Supports on Provider Quality (Simple Aggregation Weights)

	Patient Health Outcome		***Provider Quality***	
	Mean DD	**Regression Adjusted**	**Mean DD**	**Regression Adjusted**
Simple Aggregation	-0.145	-0.392**	-0.129	-0.277*

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented for both mean DD and regression adjusted estimates.]

3.5.4 Group Weighting Aggregation

The group aggregation weights present mixed results. Table 3.3 demonstrates that provider quality is negatively affected for providers treated in 2014 and 2016. For those treated in 2014, the provider quality index is reduced by 0.595 ($P<0.05$) points. The 2016 cohort has a treatment effect of -3.985 ($P<0.01$) points. I find similar results for the patient health outcome index. For providers treated in 2014, the patient health index reduces by -1.130 ($P<0.01$) points following the policy change. For those treated in 2016, the treatment effect is -3.955 ($P<0.01$) points upon MLTSS implementation.

Table 3. 3: The Mean Difference in Difference and Regression Adjusted Estimates of the Effect of Managed Long Term Services and Supports on Provider Quality (Group Aggregation Weights)

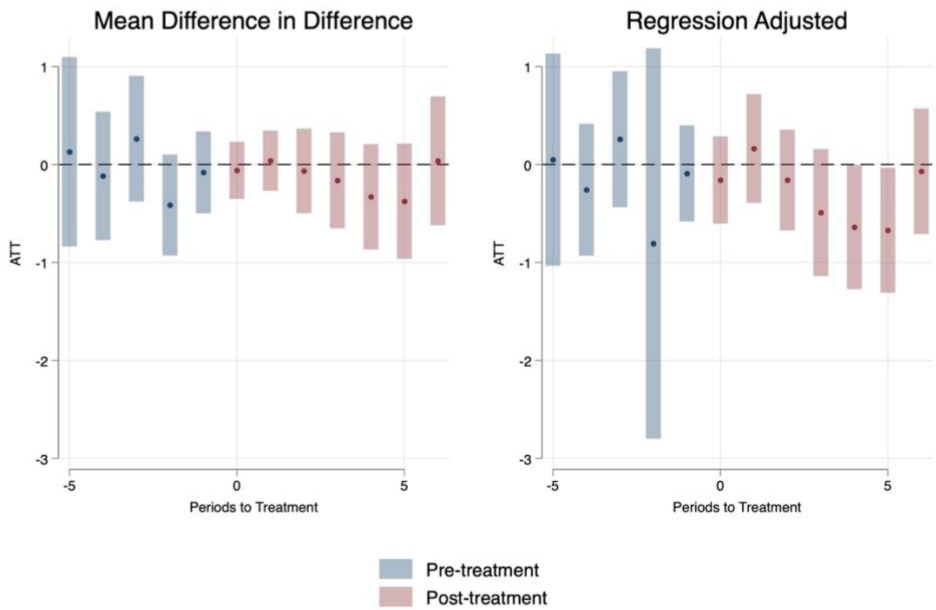
Outcome Variable:	***Patient Health Outcome***		***Provider Quality***	
Treatment Cohort	**Mean DD**	**Regression Adjusted**	**Mean DD**	**Regression Adjusted**
Group: 2013	0.100	-0.011	0.042	-0.008
Group: 2014	-0.627***	-1.130***	-0.318	-0.595**
Group: 2015	0.628	0.481	-0.329	-0.386
Group: 2016	2.780***	-3.955***	-2.165***	-3.985***
Group: 2018	-0.396	-0.490	0.146	0.078
Group: 2019	0.080	0.781	-0.326	-0.231

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented for both mean DD and regression adjusted estimates. Stars indicate significance at 1%, 5% and 10%. There are no available estimators for the 2017 cohort due to insufficient observations for these provider groups.]

3.5.5 Dynamic Weighting Aggregation

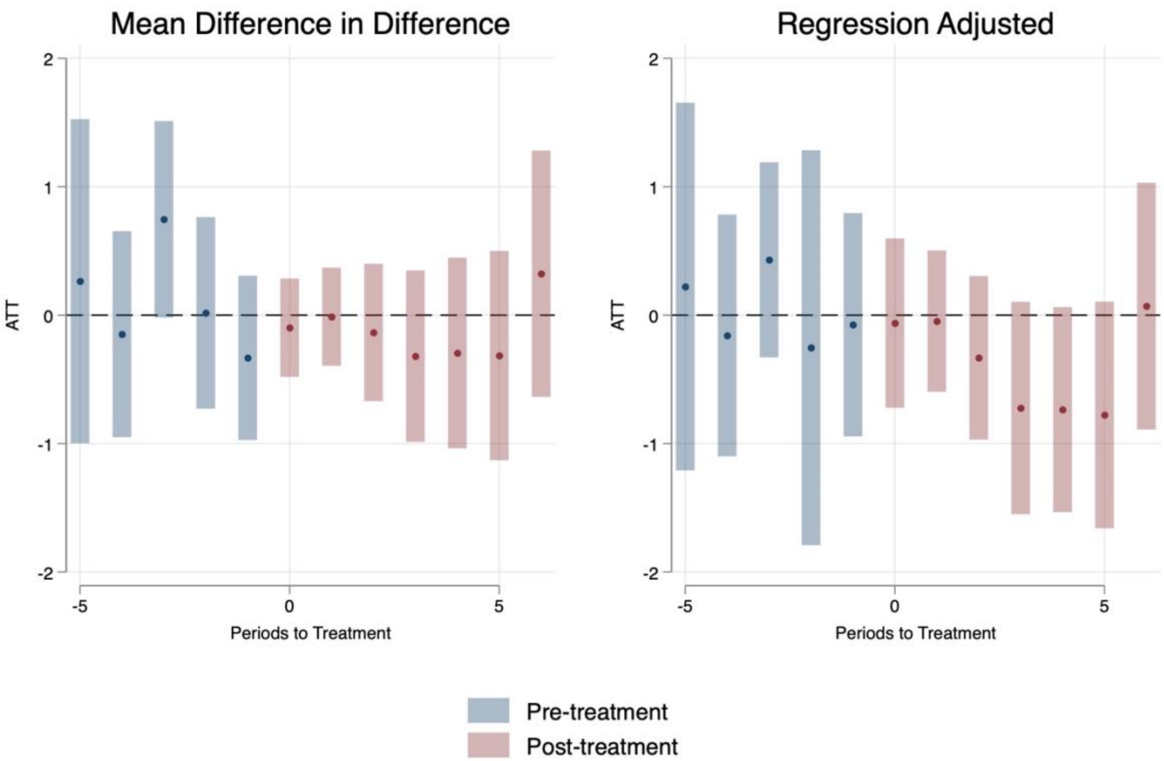
Next I present the Dynamic aggregation schemes for both outcomes for the Mean DD and regression adjusted estimates. Figures 3.4 and 3.5 below illustrate the results for the effect of MLTSS on provider quality and patient health outcomes, respectively. Figure 3.4 demonstrates a negative trend in provider quality, matching the results from the group and simple aggregations. I can interpret this dynamic aggregation as a confirmation of the results from the group and simple aggregations. Figure 3.5 illustrates a negative effect of MLTSS on patient health outcomes, also validating the negative estimates from previous aggregations.

Figure 3. 4: The Mean Difference in Difference and Regression Adjusted Estimates of the Effect of Managed Long Term Services and Supports on Provider Quality (Dynamic Aggregation Weights)



Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented for both mean DD and regression adjusted estimates. Stars indicate significance at 1%, 5% and 10%. Underlying estimates are in Appendix Table C8.]

Figure 3. 5: The Mean Difference in Difference and Regression Adjusted Estimates of the Effect of Managed Long Term Services and Supports on Patient Health Outcomes (Dynamic Aggregation Weights)



Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented for both mean DD and regression adjusted estimates. Stars indicate significance at 1%, 5% and 10%.]

3.5.6 Heterogeneity Analyses

I leverage four of the varying state features to implement heterogeneity analyses. Those are: 1) Federal authority used to implement the program, 2) Target Population, 3) Services covered by capitation and 4) Dual eligibility. Table 3.4 demonstrates the treatment effects based on simple aggregation weights for each of the four analyses.

States implement MLTSS using one of three Medicaid federal waivers (Section 1115, 1915(a), 1915(b), and 1915(c) waivers) (Dobson et al., 2017). Depending on the federal authority in use, state MLTSS programs must adhere to various guidelines. For example, Section 1115 waivers require cost neutrality whereas Section 19159(b) waivers require cost efficiency (MACPAC, 2022). I group waivers into two categories based on their designation in the Social Security Act: Section 1115 and Section 1915a/b/c. I evaluate treatments for these two groups and do not find significant results.

MLTSS programs vary between states depending on the program's intended beneficiary population. Most programs target older adults and people with physical disabilities. However, some states also incorporate individuals with intellectual disabilities. I evaluate the treatment effects for providers and counties under one of four MLTSS target populations: 1) Disabled adults, 2) Adults with intellectual and developmental disabilities, 3) Adults with physical disabilities and 4) Adults over 65. I find significant effects for programs that target adults with intellectual and developmental disabilities. For counties with this targeted population, the provider index is associated with an increase of 2.374 ($P<0.01$) points upon MLTSS implementation. This significantly positive result contrasts with the negative baseline results.

MLTSS programs also differ depending on the types of services covered. Programs generally choose one of two structures: 1) cover only LTSS or 2) cover both primary care and LTSS. I evaluate treatment effects and find significantly positive effects on patient health outcomes for programs that cover both primary care and LTSS.

Some states specify whether dual Medicare-Medicaid beneficiaries are eligible to enroll in MLTSS programs. I evaluate treatment effects for counties in which duals are eligible and not eligible for MLTSS programs. I do not find significant results.

Table 3. 4: Regression Adjusted Estimates for the Heterogeneity Analyses of the Effect of Managed Long Term Services and Supports on Patient Health Outcomes and Provider Quality (Simple Aggregation Weights)

Heterogeneity Analysis	Patient Health Outcome	Provider Quality	Sample Size
Baseline Results	-0.392**	-0.277*	22057
Federal authority			
Section 1115	1.399	0.432	1894
Section 1915a/b/c	1.179	-0.314	8353
Target Population			
Children with disabilities	2.160	1.155	5249
Adults with intellectual and developmental disabilities	1.092	2.374***	7804
Adults with physical disabilities	0.547	-0.174	12288
Adults over 65	0.941	-0.202	11568
Services covered by capitation			
LTSS only	-2.397*	-0.716	1633
Primary care & LTSS	1.607***	0.536	12869
Dual Enrollment			
Duals are eligible	-0.180	-0.658	7486
Duals are not eligible	0.395	-0.193	4675

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented. Stars indicate significance at 1%, 5% and 10%.]

3.5.7 Sensitivity Analyses

I implement two sensitivity analyses - one that changes the sample size and one that changes the control group. Table 3.5 provides results for both outcome variables produced by the simple aggregation weights.

In the main sample, I eliminated providers that moved to a different county within the 2013 to 2019 time frame to maintain clearly identified estimates. I also eliminated providers and counties treated by MLTSS policies with voluntary enrollment structures to avoid selection bias. Although I believe these intentional eliminations were necessary, I conduct a sensitivity analysis where both of these exclusions are included. I find a similar treatment effect of -0.356 ($P<0.05$) points on patient health outcomes. For provider quality, this new sample size yields a treatment effect of -0.311 ($P<0.05$) points on provider quality.

Furthermore, I conduct a sensitivity analysis where I use the Not Yet Treated (compared to the Never Treated) observations as the control group. I find significantly negative effects on patient health outcomes, with a treatment effect of -0.492 ($P<0.01$) points.

Table 3. 5: Regression Adjusted Estimates for the Sensitivity Analyses of the Effect of Managed Long Term Services and Supports on Patient Health Outcomes and Provider Quality (Simple Aggregation Weights)

Sensitivity Analysis	Patient Health Outcome	Provider Quality	Sample Size
Baseline Results	-0.392**	-0.277*	22057
Largest Sample Size	-0.356**	-0.311**	25829
Not Yet Treated as Control Group	-0.492***	-0.223	22057

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented for regression adjusted estimates. Stars indicate significance at 1%, 5% and 10%.]

3.5.8 Robustness Analyses

To substantiate this methodology choice, I compare the results to those of several alternatives. These include: Two Way Fixed Effects (TWFE) and the Chaisemartin D’Haultfoeuille Difference in Difference method (de Chaisemartin & D’Haultfœuille, 2021). Table 3.6 provides these results.

Given the downward bias of the TWFE method, it is unsurprising that these are negative coefficients. TWFE can produce negative weights, and ultimately a negative coefficient, regardless of a real positive treatment effect (Roth et al., 2022). According to the TWFE results, MLTSS reduces the patient health index by 0.436 ($P<0.10$) points and decreases the provider quality index by 0.347 ($P<0.01$) points.

The Chaisemartin D’Haultfoeuille method is the most similar to the Callaway Sant’Anna framework. This method produces an ATT for each time period, then computes a single average for all periods, presented below. The disadvantage to this method is the lack of transparency in the weighting methods, potentially resulting in a downward bias.

Table 3. 6: Regression Adjusted Estimates for the Robustness Analyses of the Effect of Managed Long Term Services and Supports on Patient Health Outcomes and Provider Quality (Simple Aggregation Weights)

Robustness Analysis	Patient Health Outcome	Provider Quality	Sample Size
Baseline Results	-0.392**	-0.277*	22057
Two Way Fixed Effects	-0.436*	-0.347***	22057
Chaisemartin D'Haultofoeuille	-0.317	-0.319	22057

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status.) Provider and time fixed effects have been implemented for regression adjusted estimates. Stars indicate significance at 1%, 5% and 10%.]

3.6 Limitations and Conclusions

Managed Long Term Services and Supports (MLTSS) was initially introduced decades ago as a mechanism to control costs and improve overall consumer health. In this paper, I find negative causal effects on Home Health provider quality and patient health outcomes. Furthermore, these results uphold after several robustness analyses.

These results validate the consumer-oriented concerns for Managed Care Organizations. The evidence presented in this paper demonstrates that a reduction in the financial resources available to providers has significant implications on the quality of care provided. Additionally, I see the downstream consequences of this reduced quality in the form of poorer patient health outcomes.

The effects of MLTSS introduce several policy implications. First, if patients are unable to receive quality care in the home, they may turn to other, more expensive, LTSS providers. For example, Assisted Living and Skilled Nursing Facilities are viable alternatives to home care. Services provided in these settings are far more costly. This would exacerbate the issue driving the goals of managed care, which is to control costs. Second, policymakers should be concerned of the consequences stemming from providers' inability to adjust to policy changes. If providers in Value Based Payment models face reimbursement reductions as a result of diminished quality, this could warrant the concern for providers "dropping out" from Medicaid networks entirely. For rural areas with an already limited pool of providers, this could be incredibly problematic for home health patients in surrounding areas. Finally, there is the possibility that Managed Care delivery models introduce new administrative burdens for providers. If this is the case, these burdens may serve as another deterrent for providers to service Medicaid beneficiaries.

To address the results found in this paper, Medicaid can implement one of two approaches. First, there needs to be more oversight of both MCOs and providers. Although the Home Health Quality Reporting Program is in place, this may be not be sufficient to maintain quality. Second, Medicaid can implement incentive-compatible agreements with MCOs for LTSS beneficiaries. It would be in Medicaid's best interest to provide MCOs with the incentive to adhere to MCO requirements for patient health. Therefore, it would behoove Medicaid to establish quality minimums for patient health in addition to providing incentives for MCOs to meet these benchmarks. The Medicare Access and CHIP Reauthorization Act (MACRA) is an example of these types of policies. Policies similar to MACRA ought to be in place for all healthcare services, including both primary care and LTSS.

Although these results identify negative treatment effects, there may be some hope for MLTSS. These negative effects could be reflective of an adjustment period. Providers may need a few years to transition their Fee for Service operations to those of Managed Care. Therefore, although MLTSS may have negative effects in the short run, there is the potential for positive effects in the long run.

There are two limitations to this study. First, Home Health Providers (HHPs) deliver more than just Long Term Services and Supports. Just like Skilled Nursing Facilities, they also provide post-acute care for individuals recovering from an inpatient stay. These post-acute care episodes differ from LTSS in that they are provided to address a specific condition rather than caring for Activities of Daily Living for extended periods of time. The data does not provide information to differentiate between HHPs that only deliver LTSS versus those that provide LTSS and post-acute care.

The second limitation involves provider misreporting. Although providers are expected to submit authentic quality evaluations, given the nature of the self-reporting system, there is potential for misreporting.

Despite these limitations, I find that the effects of MLTSS are congruent with consumer worries. These results could have greater implications in the form of premature admission to Skilled Nursing Facilities. If patients are unable to receive quality care from a home health provider, they may turn to other providers, such as Skilled Nursing or Assisted Living Facilities. Further investigation is necessary to evaluate these greater implications.

Appendix C

Appendix Table C 1: Managed Long Term Services and Supports Program Details by State

State (N = 23)	Program name	Start Date	Federal Authority	Mandatory (M) or Voluntary (V) Enrollment	Minimum Level of Care Needed to Enroll
AZ	Arizona Long Term Care System (ALTCS)	1/1/89	1115a	M	LTSS less than institutional LOC
AR	Provider-led Arkansas Shared Savings Entity (PASSE)	3/1/19	1915(b)	M	For BH: less than institutional LOC For DD: institutional LOC
CA	Managed MediCal Long-Term Supports and Services	4/1/14	1115a	M	No LTSS need
DE	Diamond State Health Plan-Plus (DSHP-Plus)	4/1/12	1115a	M	No LTSS need
FL	Statewide Medicaid Managed Care	8/1/13	1915(b)/(c)	M	Institutional LOC
HI	QUEST Integration	1/1/15	1115a	M	No LTSS need
IA	Iowa Health Link	4/1/16	1915(b)/(c)	M	No LTSS need
ID	MedicareMedicaid Coordinated Plan (MMCP)	7/1/14	1915(a)/(c)	V	No LTSS need
ID	Medicaid Plus (IMPlus)	11/1/18	1915(b)/(c)	M	No LTSS need
IL	HealthChoice	1/1/18	1915(b)/(c)	M	No LTSS need
KS	KanCare (MLTSS component)	1/1/13	1115(a)/1915(c)	M	Institutional LOC
MA	Senior Care Options	3/1/04	1915(a)/(c)	V	No LTSS need
MI	Managed Specialty Services and Supports Program	1/1/98	1915(b)/(c)	M	Institutional LOC
MN	Minnesota Senior Care Plus (MSC+)	6/1/05	1915(b)/(c)	V	Institutional LOC
NC	NC Innovations (MH/DD/SUD waiver)	4/1/05	1915(b)/(c)	M	Institutional LOC
NJ	NJ FamilyCare (MLTSS component)	7/1/14	1115(a)	M	Institutional LOC
NM	Centennial Care (MLTSS component)l	1/1/14	1115(a)	M	Institutional LOC

NY	MLTC Partial Capitation	1/1/98	1115(a)	M	LTSS less than institutional LOC
OH	MyCare Opt-out	5/1/14	1915(b)/(c)	M	No LTSS need
PA	Community HealthChoices	1/1/18	915(b)/(c)	M	No LTSS need
TN	TennCare CHOICES in Long-Term Care	3/1/10	1115(a)	M	LTSS less than institutional LOC
TX	Texas STAR+PLUS	12/12/11	1115(a)	M	No LTSS need
VA	Commonwealth Coordinated Care Plus	8/1/17	915(b)/(c)	M	Institutional LOC
WI	Family Care	1/1/99	1915(b)/(c)	V	LTSS less than institutional LOC

Note. SOURCE: [(Wysocki et al., 2020)] Notes: ["PD" is the abbreviation for physical disabilities. "I/DD" is the abbreviation for intellectual or developmental disabilities. "LOC" is the abbreviation for level of care. "BH" is the abbreviation for behavioral health.]

Appendix Table C 2: Managed Long Term Services and Supports Program Details by State

State (N = 23)	**Program Name (N = 33)**	**Dual Eligible**	**Nondual eligible**	**Services Covered by Capitation**	**Statewide/Less than statewide**
AZ	Arizona Long Term Care System (ALTCS)	x	x	Medical & LTSS	Statewide
AR	Provider-led Arkansas Shared Savings Entity (PASSE)	x	x	Medical & LTSS	Statewide
CA	Managed MediCal Long-Term Supports and Services	x	x	Medical & LTSS	less than statewide
DE	Diamond State Health Plan-Plus (DSHP-Plus)	x	x	Medical & LTSS	Statewide
FL	Statewide Medicaid Managed Care	x	x	Medical & LTSS	Statewide
HI	QUEST Integration	x	x	Medical & LTSS	Statewide
IA	Iowa Health Link	x	x	Medical & LTSS	Statewide
ID	MedicareMedicaid Coordinated Plan (MMCP)	x		Medical & LTSS	less than statewide
ID	Medicaid Plus (IMPlus)	x		Medical & LTSS	less than statewide
IL	HealthChoice	x	x	Medical & LTSS	Statewide
KS	KanCare (MLTSS component)	x	x	Medical & LTSS	Statewide

MA	Senior Care Options	x	x	Medical & LTSS	less than statewide
MI	Managed Specialty Services and Supports Program	x	x	LTSS only	Statewide
MN	Minnesota Senior Health Options (MSHO)	x		Medical & LTSS	Statewide
NC	NC Innovations (MH/DD/SUD waiver)	x	x	LTSS only	Statewide
NJ	NJ FamilyCare (MLTSS component)	x	x	Medical & LTSS	Statewide
NM	Centennial Care (MLTSS component)l	x	x	Medical & LTSS	Statewide
NY	MLTC Partial Capitation	x	x	LTSS only	Statewide
OH	MyCare Opt-out	x		Medical & LTSS	less than statewide
PA	Community HealthChoices	x	x	Medical & LTSS	Statewide
TN	TennCare CHOICES in Long-Term Care	x	x	Medical & LTSS	Statewide
TX	Texas STAR+PLUS	x	x	Medical & LTSS	Statewide
VA	Commonwealth Coordinated Care Plus	x	x	Medical & LTSS	Statewide
WI	Family Care	x	x	LTSS only	Statewide

Note. SOURCE: [(Wysocki et al., 2020)] Notes: ["PD" is the abbreviation for physical disabilities. "I/DD" is the abbreviation for intellectual or developmental disabilities. "LOC" is the abbreviation for level of care.]

Appendix Table C 3: Managed Long Term Services and Supports Program Target Population by State

State (N = 23)	**Program name (N = 33)**	**Children with disabilities**	**Adults with PD**	**Adults with I/DD**	**Older adults 65+**
AZ	Arizona Long Term Care System (ALTCS)	x	x	x	x
AR	Provider-led Arkansas Shared Savings Entity (PASSE)	x		x	
CA	Managed MediCal Long-Term Supports and Services		x	x	x
DE	Diamond State Health Plan-Plus (DSHP-Plus)	x	x	x	x
FL	Statewide Medicaid Managed Care		x		x
HI	QUEST Integration	x	x	x	x
IA	Iowa Health Link	x	x	x	x

ID	MedicareMedicaid Coordinated Plan (MMCP)		x	x	x
ID	Medicaid Plus (IMPlus)		x	x	x
IL	HealthChoice	x	x	x	x
KS	KanCare (MLTSS component)	x	x	x	x
MA	Senior Care Options				x
MI	Managed Specialty Services and Supports Program	x		x	x
MN	Minnesota Senior Health Options (MSHO)				x
NC	NC Innovations (MH/DD/SUD waiver)			x	x
NJ	NJ FamilyCare (MLTSS component)	x	x	x	x
NM	Centennial Care (MLTSS component)	x	x	x	x
NY	MLTC Partial Capitation		x		x
OH	MyCare Opt-outn		x		x
PA	Community HealthChoices		x	x	x
TN	TennCare CHOICES in Long-Term Care	x	x		x
TX	Texas STAR+PLUS	x	x	x	x
VA	Commonwealth Coordinated Care Plus	x	x	x	x
WI	Family Care		x	x	x

Note. SOURCE: [(Wysocki et al., 2020)] Notes: ["PD" is the abbreviation for physical disabilities. "I/DD" is the abbreviation for intellectual or developmental disabilities. "LOC" is the abbreviation for level of care.]

Appendix Section A: Outcomes and Assessment Information Set

The instrument/data collection tool used to collect and report assessment data by home health agencies is called the Outcome and Assessment Information Set (OASIS). The Outcome and Assessment Information Set (OASIS) is used to calculate the home health quality measures (both outcome and process measures) and determine the service area for each home health agency in the Care Compare search function. OASIS is a group of data elements that represent core items that are included in a comprehensive assessment for each adult home care patient. These core items and the larger comprehensive assessment serve as the basis for the development of the plan of care and ongoing management of the patients. Since 1999, CMS has required Medicare-certified home health agencies to collect and transmit OASIS data for all adult patients whose care is reimbursed by Medicare and Medicaid with the following exceptions

1. Patients under the age of 18.
2. Patients receiving maternity services.
3. Patients receiving only chore or housekeeping services.
4. Home health patients for whom the payment source is neither Medicare nor Medicaid.

Mandatory Reporting

Outcome and Assessment Information Set (OASIS) reporting is mandated in the Medicare regulations at 42 C.F.R.§484.250(a), which requires HHAs to submit OASIS assessments and Home Health Care Consumer Assessment of Healthcare Providers and Systems Survey (HH CAHPS) data to meet the quality reporting requirements of section 1895(b)(3)(B)(v) of the Act.

The reporting of quality data by home health agencies (HHAs) is mandated by Section 1895(b)(3)(B)(v)(II) of the Social Security Act ("the Act"). This statute requires that ''each home health agency shall submit to the Secretary such data that the Secretary determines are appropriate for the measurement of health care quality. Such data shall be submitted in a form and manner, and at a time, specified by the Secretary for purposes of this clause.''

Data Collection Process

The home health skilled care team gather the information by observing the patient and the patient's home situation, by talking with the patient and caregivers, and by recording the care they provide to patients. The home health agency submits the OASIS data to their state repository. The data are converted to rates that measure the quality of the home health agency's care.

As identified in (M0080) Discipline of Person Completing Assessment, the comprehensive assessment and OASIS data collection are the responsibility of a registered nurse (RN) or any of the therapies, including physical therapist (PT), speech language pathologist/speech therapist (SLP/ST), or occupational therapist (OT). A licensed practical nurse or licensed vocational nurse (LPN/LVN), physical therapy assistant (PTA), occupational therapy assistant (OTA), medical social worker (MSW), or Aide may not be responsible for completing OASIS assessments.

This performance system is driven by the principle that each HHA will be expected to submit a minimum set of two "matching" assessments for each patient admitted to their agency. These matching assessments together create what is considered a "quality episode of care," which would ideally consist of a Start of Care (SOC) or Resumption of Care (ROC) assessment and a matching End of Care (EOC) assessment. However, several scenarios could meet this "matching assessment requirement" of the new pay-for-reporting performance requirement. These scenarios have been defined as quality assessments that create a quality episode of care during the reporting period or are consistent with creating a quality episode if the reporting period were expanded to an earlier reporting period or into the next reporting period. Seven types of OASIS assessments submitted by an HHA will fit this definition of a quality assessment. The seven OASIS assessments are:

- A Start of Care (SOC) or Resumption of Care (ROC) assessment that has a matching End of Care (EOC) assessment. EOC assessments are conducted at transfer to an inpatient facility (with or without discharge), death, or discharge from home health care. These two assessments (the SOC or ROC assessment and the EOC assessment) create a regular quality episode of care and both count as quality assessments.
- A SOC/ROC assessment could begin an episode of care but occurs in the last 65 days of the performance period. This is labeled as a "Late SOC/ROC" quality assessment.
- An EOC assessment that could end an episode of care beginning in the previous reporting period, (that is, an EOC that occurs in the first 65 days of the performance period.) This is labeled as an “Early EOC” quality assessment.
- A SOC/ROC assessment is followed by one or more follow-up assessments, the last of which occurs in the last 65 days of the performance period. This is labeled as a "SOC/ROC Pseudo Episode" quality assessment.
- An EOC assessment is preceded by one or more Follow-up assessments, the last of which occurs in the first 65 days of the performance period. This is labeled an "EOC Pseudo Episode” quality assessment.
- A SOC/ROC assessment is part of a known one-visit episode. This is labeled as a "One-visit episode" quality assessment.
- Follow-up assessments (that is, where the M0100 Reason for Assessment = ‘04’ or ‘05’) are considered “Neutral” assessments and do not count toward or against the pay for reporting performance requirement.

- SOC, ROC, and EOC assessments that do not meet any of these definitions are labeled as "Non-Quality" assessments.

Missing or Insufficient Data

The Home Health Quality Reporting Program does not publicly report data for providers in two cases. These two cases are coded as 199 or 201 values:

- Within the data rows, a value of 199 indicates the HHA did not have enough patients (at least 20) for whom the measure could be calculated.
- A value of 201 indicates that no measures were calculated for the HHA because of insufficient data or because the HHA had less than 6 months of data (usually because the HHA had been in operation as a Medicare- certified HHA for less than 6 months at the end of the data collection period). If a measure has a value of 201 for all HHAs, measure values are no longer being displayed and the measure is in the process of being phased out or replaced by another measure.

These two types of observations are dropped from the data.

OASIS-C

The OASIS-C update was the most extensive update since OASIS was initially implemented in 1999. The update reflects the current thinking of domains of quality measurement and evidence-based care practices (Deitz et al., 2010). Prior to OASIS-C, CMS evaluated HHPs solely on the basis of patient health. Following OASIS-C, providers were evaluated on process measures.

Risk-Adjusted Outcomes

All of the patient health outcome measures evaluated in this paper are risk adjusted, including all that are currently reported on Home Health Compare. The risk adjustment methodology uses a predictive model developed specifically for each measure. The risk adjustments compensate for differences in the patient population served by different home health agencies. The risk adjustment process steps are taken from the Abt Associates 2020 report (Abt Associates, 2020).

The following steps are used to calculate each quality measure for a 12-month measure time window after the appropriate exclusions are made:

A. Calculate the agency observed score for each month (steps 1 through 3)
    a. Step 1. Calculate the denominator count: Calculate the total number of quality episodes with a selected target OASIS assessment each month that do not meet the exclusion criteria following each measure's specifications.
    b. Step 2. Calculate the numerator count: Calculate the total number of quality episodes in the denominator whose OASIS assessments indicates meeting numerator criteria for each month, following each measure's specifications.
    c. Step 3. Calculate the agency's monthly observed rate: Divide the agency's numerator count by its denominator count to obtain the agency's observed rate; that is, divide the result of step 2 by the result of step 1.
B. Calculate the predicted rate for each quality episode (steps 4 and 5).
    a. Step 4. Determine presence or absence of the risk factors for each patient (technical specifications for risk factors are in Section III): If dichotomous risk

factor covariates are used, assign covariate values, either ‘0’ for covariate condition not present or ‘1’ for covariate condition present, for each quality episode for each of the covariates as reported at SOC/ROC, as described in the section above.

b. Step 5. Calculate the predicted rate for each quality episode with the following formula:

    i. [1] Episode-level predicted QM rate = $\frac{1}{1+e^{-X}}$

        1. Where e is the base of natural logarithms and X is a linear combination of the constant and the logistic regression coefficients times the covariate scores (from Formula [2], below).

    ii. [2] Quality measure triggered (yes=1, no=0) = $B_0 + B_1 COVA + B_2 COVA + \cdots + B_N COVN$

        1. Where B_0 is the logistic regression constant, B_1 is the logistic regression coefficient for the first covariate, $COVA$is the episode-level rate for the first covariate, B_2 is the logistic regression coefficient for the second covariate, and $COVB$ is the episode-level rate for the second covariate, etc. The regression constant and regression coefficients are provided in the Recalibrated Risk Adjustment Model_Risk Factors_Model Fit_Coefficients document.

C. Calculate the agency’s monthly predicted rate (step 6)

a. Step 6. Once a predicted QM rate has been calculated for all quality episodes, calculate the mean agency-level predicted QM rate by averaging all episode-level predicted values for that agency for each month.

D. Calculate national predicted rate (step 7)

a. Step 7. Calculate the monthly national predicted rate: Once a predicted QM value has been calculated for all episodes, calculate the mean national-level predicted QM rate by averaging all episode-level predicted values for each month. Note that the sample will include only those quality episodes with non-missing data for the component covariates.

E. Calculate the agency's monthly risk-adjusted rate (step 8)

a. Step 8. Calculate the agency-level monthly risk-adjusted rate based on the agency-level monthly observed quality measure rate (step 3), agency-level monthly mean predicted quality measure rate (step 6), and national monthly mean predicted QM rate (step 7), using the following formula: [3] agency risk adjusted rate = agency observed rate + national predicted rate – agency predicted rate

F. Calculate the agency's 12-month risk adjusted rate (step 9)

a. Step 9. Calculate the 12-month risk-adjusted rate by averaging the agency's monthly risk-adjusted rate (step 8) weighting by the HHA's number of episodes in each month over the 12 month period. If the adjusted rate is greater than 100%, the adjusted rate is set to 100%. Similarly, if the result is a negative number the adjusted rate is set to zero.

Appendix Table C 4: Number of Treated (Managed Long Term Services and Supports) and Untreated (Fee for Service Long Term Services and Supports) Home Health Providers by Year

Year	MLTSS	FFS-LTSS
2013	2340	1225
2014	2309	1228
2015	2109	1166
2016	1967	1121
2017	1910	1100
2018	1822	1034
2019	1746	980

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.]

Appendix Table C 5: Covariate Mean Values by Treatment Status

Covariate	Treatment	Control
Medical Social Services	0.94	0.89
Occupational Therapy Services	0.97	0.96
Nursing Care Services	1	1
Physical Therapy Services	1	1
Speech Pathology Services	0.95	0.92
Home Health Aide Services	0.99	0.98
Ownership Type: Public	0.08	0.12
Ownership Type: Private For-Profit	0.7	0.62
Ownership Type: Private Nonprofit	0.22	0.26
Percent in Poverty	12.12	12.38
Percent Black	12.98	14.23

Urban Influence Code	2.35	3.38
Percent Over 65	14.65	14.12
State Expansion Status	0.56	0.08
State Non-Expansion Status	0.49	0.46
N	14203	7854

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.]

Appendix Table C 6: The Urban Influence Code

Metropolitan Counties	
Code	
1	In large metro area of 1+ million residents
2	In small metro area of less than 1 million residents
Nonmetropolitan Counties	
3	Micropolitan area adjacent to large metro area
4	Noncore adjacent to large metro area
5	Micropolitan area adjacent to small metro area
6	Noncore adjacent to small metro area and contains a town of at least 2,500 residents
7	Noncore adjacent to small metro area and does not contain a town of at least 2,500 residents
8	Micropolitan area not adjacent to a metro area
9	Noncore adjacent to micro area and contains a town of at least 2,500 residents

10	Noncore adjacent to micro area and does not contain a town of at least 2,500 residents
11	Noncore not adjacent to metro or micro area and contains a town of at least 2,500 residents
12	Noncore not adjacent to metro or micro area and does not contain a town of at least 2,500 residents

Note. SOURCE: [U.S. Department of Agriculture Economic Research Service]

Appendix Section B: Aggregating Group-Time Average Treatment on the Treated Estimators

Simple Aggregation Weights

The Simple scheme aggregates ATTs across all groups and time periods according to sample size to produce a single ATT. The Simple weighting function is denoted below,

$$\theta = \frac{1}{k}\sum_{g=2}^{7}\sum_{j=2}^{6} 1\{g \leq t\} ATT(g,t) P(G = g | C \neq 1)$$

Group Aggregation Weights

The Group scheme aggregates the ATTs for panels in each treatment group, across all periods. The Group weighting function is denoted below,

$$\theta = \frac{1}{T - g + 1}\sum_{t=2}^{6} 1\{g \leq t\} ATT(g,t)$$

Dynamic Aggregation Weights

The Dynamic scheme aggregates the ATTs for each period relative to the first period of treatment, across all cohorts. Therefore, all panels that were treated by e periods relative to treatment are grouped together and weighted proportional to sample size. It is very similar to an event study approach. By providing the ATT for the several pre and post-treatment periods, this aggregation method sheds light on treatment effect heterogeneity across time. The Dynamic weighting function is denoted below,

$$\theta = \sum_{g=2}^{7} 1\{g + e \leq \tau\} ATT(g, g + e) P(G = g | G + e \leq \tau, C \neq 1)$$

Where e represents the number of periods relative to treatment (e = 0 indicates the period of treatment). τ represents the total number of periods (7) and G is the first time period that a panel receives treatment.

Appendix Table C 7: Mean Patient Health Outcome Index and Provider Quality Index for all Home Health Providers by Year

	Patient Health Outcome		**Provider Quality**	
Year	**FFS-LTSS**	**MLTSS**	**FFS-LTSS**	**MLTSS**
2013	53.77	53.92	74.52	74.95
2014	54.76	55.12	75.85	76.27
2015	55.74	55.75	76.21	76.87
2016	57.47	57.73	76.46	76.96
2017	61.24	61.26	78.53	78.72
2018	64.55	64.69	79.56	79.49
2019	67.30	67.20	79.87	80.01

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.]

Appendix Table C 8: The Mean Difference in Difference and Regression Adjusted Percentage Point Estimates of the Effect of Managed Long Term Services and Supports on Patient Health Outcomes and Provider Quality (Dynamic Aggregation Weights)

	Patient Health Outcome		**Provider Quality**	
Period to Treatment	**Mean DD**	**Regression Adjusted**	**Mean DD**	**Regression Adjusted**
-5	0.264	0.222	0.131	0.050
-4	-0.149	-0.159	-0.116	-0.257
-3	0.746***	0.431	0.263	0.259
-2	0.017	-0.253	-0.413**	-0.806
-1	-0.333	-0.075	-0.079	-0.091
0	-0.098	-0.062	-0.058	-0.158
1	-0.013	-0.047	0.040	0.163
2	-0.135	-0.333	-0.065	-0.158
3	-0.320	-0.723**	-0.161	-0.490**
4	-0.295	-0.736***	-0.329	-0.639***
5	-0.315	-0.777**	-0.374	-0.671***
6	0.323	0.070	0.038	-0.068

Note. SOURCE: [Author's Analysis of Home Health Quality Reporting Program for the years of 2013 to 2019.] NOTES: [Regression adjusted estimates include covariates (provider services offered, ownership type, % county population black, % county population in poverty, % county population over 65, urban influence code, state expansion status, mltss level of care needed to enroll.) Provider and time fixed effects have been implemented for both mean DD and regression adjusted estimates. Stars indicate significance at 1%, 5% and 10%.]

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