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Using Difference-in-Difference to Assess the Impact of Medicaid Policy on the Addiction Treatment Provider Workforce and Prescribing Behavior in Virginia

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A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at Virginia Commonwealth University.

by
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List of Abbreviations

ACA	Affordable Care Act
ACS	American Community Survey
ARTS	Addiction and Recovery Treatment Services
ASAM	American Society of Addiction Medicine
ATT	Average treatment effects on the treated
ATT(g,t)	Group-time treatment effects
BIPOC	Black, Indigenous, and People of Color
BWP	Buprenorphine waivered provider
CDC	Center for Disease Control and Prevention
CHIP	Children's Health Insurance Program
CMS	Centers for Medicare and Medicaid Services
DATA	Drug Addiction Treatment Act
DGM	Data generating mechanism
DMAS	Department of Medical Assistance Services
DRIPW	Doubly-robust inverse probability weighting
ETWFE	Extended two-way fixed effects
FDA	Food and Drug Administration
FPL	Federal poverty level
GAP	Governor's Access Plan
GCN	Generic code number
GTE	Greater than or equal
HIC3	Hierarchical Ingredient Code
IMD	Institutions for mental disease
IPUMS	Integrated Public Use Microdata Series
LTE	Less than or equal to
MC	Monte Carlo
MCO	Managed care organization
MD/DO	Doctors of Medicine/Doctors of Osteopathic Medicine
MHPAEA	Mental Health Parity and Addiction Equity Act
MOUD	Medications for treatment of opioid use disorder
NA	No anticipation
NDC	National Drug Code
NPI	National Provider Identifier
NP/PA	Nurse Practitioners/Physician Assistants
N-SSATS	National Survey of Substance Abuse Treatment Services
NUCC	National Uniform Claim Committee
OBOT	Office-Based Opioid Treatment
OLS	Ordinary least squares
OTP	Opioid treatment program
OUD	Opioid use disorder
PA	Prior authorization
PT	Parallel trends
RUCA	Rural-Urban Commuting Area
SAMHSA	Substance Abuse and Mental Health Service Administration
SD	Standard deviation
SUD	Substance use disorder
TCU	Texas Christian University
TVC	Time-varying confounders
TWFE	Two-way fixed effects
Tx	Treatment

Abstract

This dissertation is unified by both policy opportunity and estimation method, examining the impact of Medicaid behavioral health policy in Virginia using a difference-in-differences framework. Medicaid-covered substance use disorder (SUD) treatment benefits in Virginia were significantly enhanced through a Section 1115 SUD waiver, which are required to be budget neutral. Therefore, it is critical to understand whether the objectives of the waiver are met using reliable, unbiased estimation methods. This dissertation includes two empirical research projects evaluating the impact of Medicaid policy on removing barriers in access to care and one methodological project comparing the performance of alternate approaches to difference-in-difference estimation with health policies implemented at different points in time.

Chapter 1 uses data from the National Survey of Substance Abuse Treatment Services (N-SSATS) to assess whether the enhancement in SUD treatment benefits increased Medicaid participation among treatment facilities in Virginia compared to states without similar changes in SUD benefits. Results indicate Virginia's Section 1115 SUD waiver significantly increased facility acceptance of Medicaid as payment for SUD treatment services relative to comparison states. As this study has a single treated unit, the robustness of the difference-in-difference results was tested using two approaches appropriate for small samples—the synthetic control method and a form of outlier analysis with state-specific linear detrending.

Chapter 2 builds on methodologic advances on staggered difference-in-differences in the econometrics literature, using Monte Carlo simulations and an empirical application to compare the performance of two-way fixed effects (TWFE) with three alternatives to estimating overall treatment effects using extended two-way fixed effects (ETWFE). Across variation in sample size, treatment timing, treatment effects, and comparison groups, ETWFE outperforms TWFE, and weighting ETWFE estimates by the scaled share of units and time treated outperforms weighting by either units or time alone. The simulations and empirical application illustrate the bias of the TWFE-based effect of Medicaid expansion on insurance coverage will likely increase as more states implement Medicaid expansion, necessitating adoption of unbiased estimation approaches like ETWFE.

Chapter 3 uses ETWFE to examine whether the removal of the prior authorization (PA) requirement increases the prescribing rate for buprenorphine—one of the three medications approved by the Food and Drug Administration (FDA) for treatment of opioid use disorder (OUD). Compared to providers subject to PA requirements for the duration of the study, providers significantly increased their buprenorphine prescribing rates after the removal of the PA requirement. The gains in prescribing rates were observed in three of four cohorts of providers and increased over the post-treatment period. These findings suggest removing the PA requirement for buprenorphine is an effective policy solution for Medicaid programs to increase provider prescribing capacity and improve buprenorphine access.

Introduction

The opioid overdose crisis in the U.S. remains a national public health challenge, with nearly three in four of the 91,596 drug overdose deaths in 2020 attributed to opioids.[1] The societal costs of opioids is an estimated $78.5 billion each year in healthcare, substance use treatment, criminal justice, and lost productivity costs.[2] Over 40 million people reported a past year substance use disorder (SUD) in 2020; however, only 6.5% received any SUD treatment.[3] Nearly 3 million people are estimated to have an opioid use disorder (OUD), but only 11% received evidence-based medications for treatment of opioid use disorder (MOUD) like buprenorphine or methadone in 2020.[3]

Medicaid plays an important role in addressing the challenges of the opioid crisis, covering 20 percent of non-elderly adults in the United States but 40 percent of non-elderly adults diagnosed with OUD.[4] As the prevalence and cost per case of OUD vary significantly between states, Section 1115 SUD waivers approved by the Centers for Medicare and Medicaid Services (CMS) enable states to tailor their Medicaid benefits to meet the specific health needs of their residents.[5] However, 1115 waivers are required to be budget neutral, meaning states cannot spend more on waiver-covered benefits than they otherwise would in the absence of the waiver.[6] Therefore, it is critical to evaluate whether Medicaid policies covered by 1115 waivers have the intended effect by using reliable, unbiased estimation methods.

This research aims to improve understanding of the role for Medicaid policy in reducing structural barriers in access to care for Medicaid members with SUD and OUD. The first paper assesses whether Virginia's Section 1115 SUD waiver increased access to care by incentivizing treatment facilities in Virginia to accept Medicaid as payment for SUD treatment services. The second paper examines different methodological approaches to quantifying the impact of a

policy that was implemented at multiple time points. Recent methodological advances have identified the commonly used two-way fixed effects (TWFE) approach produces biased estimates, so it is critical to examine alternative estimation methods to ensure policy evaluation produce high quality, reliable results.[7] The third paper applies an alternative to TWFE, extended two-way fixed effects (ETWFE) to examine a component of Virginia's Section 1115 SUD waiver intended to alleviate barriers to MOUD treatment by no longer requiring a subset of providers to submit prior authorization requests in order to prescribe buprenorphine. Although Virginia Medicaid has largely removed prior authorization requirements for prescribing buprenorphine over the five years since the implementation of the waiver, these findings may be beneficial for policymakers in other states who may be considering alternative ways to improve access to care and incentivize the provision of evidence-based treatment for OUD.[8]

Chapter 1: Changes in Substance Use Disorder Treatment System Capacity Following Enhancement of Medicaid-covered Substance Use Disorder Treatment Benefits

INTRODUCTION

The U.S. Department of Health and Human Services allows states to create Section 1115 substance use disorder (SUD) demonstrations to tailor Medicaid benefits according to state-specific needs.[9] Virginia's Section 1115 SUD demonstration—"Building and Transforming Coverage, Services, and Supports for a Healthier Virginia" and formerly titled "The Virginia Governor's Access Plan (GAP) and Addiction and Recovery Treatment Services (ARTS) Delivery System Transformation"—was implemented on April 1, 2017. The ARTS benefit includes an Institutions for Mental Disease (IMD) waiver approved by CMS to allow Medicaid to pay for services in residential treatment or inpatient detoxification settings. Medicaid has historically been prohibited from covering SUD treatment services provided in residential or inpatient settings.[10] The ARTS benefit expanded Medicaid-covered benefits to include the continuum of care recommended by the American Society of Addiction Medicine (ASAM) guidelines; enhanced treatment provider payment rates for services; created SUD-specific care coordination services; and created provider and agency trainings about the changes to Medicaid-covered SUD benefits;[11,12] therefore, the ARTS benefit represents a significant expansion in the scope of treatment services available to Medicaid members in Virginia.[13]

Prior evaluations of the ARTS benefit identified an improvement in acute care use related to opioid use disorder (OUD) following implementation of the ARTS benefit and descriptively identified an increase in SUD treatment provider supply;[14,15] however, the improvements in capacity and access may reflect broader national trends in provider capacity and patient demand for SUD services. For example, the number of counties containing at least one provider waivered to prescribe buprenorphine, which is one of three evidence-based medications for treatment of

opioid use disorder (MOUD), increased from 54.9 percent in 2016 to 65 percent in 2018, suggesting an increase in demand for services.[16] An analysis of news media coverage of the opioid crisis identified a substantial increase in media mentions of MOUD in the two years prior to the ARTS benefit, which may have raised public awareness about the opioid crisis.[17]

This study seeks to parse changes in treatment capacity observed in Virginia from broader national trends, which requires identifying states similar to Virginia without a similar change to Medicaid-covered SUD benefits. One major Medicaid policy differentiating Virginia from many other states is Medicaid expansion, which Virginia implemented nearly two years after ARTS on January 1, 2019. Eight states also implemented Section 1115 SUD waivers prior to Medicaid expansion, but these state-specific policy changes occurred at least four years prior to ARTS implementation. Although Virginia is unique in the approach and timing of restructuring benefits for SUD prior to expansion of Medicaid eligibility, non-expansion states with similarly limited SUD treatment benefits and low provider supply in the period before ARTS may serve as a comparable comparison group to approximate how treatment capacity and access may have changed in the absence of the ARTS benefit.

There is mixed evidence about how the SUD treatment labor supply responds to federal and state policy changes to coverage of SUD treatment. For example, the Mental Health Parity and Addiction Equity Act (MHPAEA) of 2010 required employer-sponsored private insurance for companies with at least 51 employees, Medicaid managed care plans, and Children's Health Insurance Program (CHIP) plans to provide payment for SUD and mental health services comparable with other medical treatment services. Following MHPAEA, there was a significant increase in Medicaid acceptance among SUD treatment providers.[18] Similarly, comparisons of states with and without IMD waivers revealed a significant increase in Medicaid acceptance

among SUD treatment providers in states with IMD waivers.[10] While this suggests increasing the generosity of SUD benefits resulted in increased acceptance of Medicaid among SUD treatment providers, an examination of changes in hiring among SUD treatment providers following Medicaid expansion did not find a meaningful increase in hiring attempts in expansion states.[19] Similarly, preliminary analysis of the impact of Medicaid expansion on SUD treatment provider acceptance of Medicaid found no significant change among SUD treatment providers.[20] Although new Medicaid members eligible through expansion are estimated to have higher rates of SUD compared to Medicaid members with other eligibility, policy changes affecting the volume of Medicaid-covered patients in need of SUD services may be insufficient to drive structural changes like increasing the SUD treatment provider workforce.[21]

The objective of this study is to build on prior evaluations of the impact of the ARTS benefit by identifying comparison states to estimate the causal effect of enhancing Medicaid-covered SUD benefits on facility acceptance of Medicaid in Virginia. As the opioid epidemic has been declared both a national and state-level public health emergency, it is important for states to assess the impact of specific policy changes to determine whether policy changes have the intended effect. Estimating the effect of Virginia's ARTS benefit on SUD treatment capacity may be especially beneficial for states with unmet needs for SUD services among their Medicaid population.

Conceptual Framework

The Donabedian Structure-Process-Outcome quality of care conceptual framework was used to assess the impact of the ARTS benefit on the SUD treatment capacity in Virginia relative to states without a similar change to Medicaid-covered SUD benefits. In the Donabedian framework, the *structural* component represents broad characteristics of the health care setting,

including material and human resources and organizational structure.[22] Prior research identified lack of training, administrative burdens, and low reimbursement rates as significant structural barriers to provision of SUD treatment.[23] As the ARTS benefit enhanced reimbursement rates, created workforce trainings on Medicaid-covered services, and introduced wrap-around services like SUD care coordination, these structural changes influence downstream *processes* performed within the health system. The *process* component of the Donabedian framework reflects the action of care provision within the health system, which indicates the removal of structural barriers should result in increased Medicaid acceptance.[22] Improvements in SUD treatment processes are expected to subsequently improve *outcomes* for members with SUD by increasing access to an evidence-based continuum of care. The focus of this study is to examine whether the structural changes represented by the ARTS benefit significantly affected SUD treatment processes in Virginia as evidenced by increased acceptance of Medicaid among facilities offering SUD treatment.

Research Questions and Hypotheses

Q1. Did the ARTS benefit significantly affect SUD treatment capacity in Virginia compared to states without similar changes to Medicaid-covered SUD benefits?

H1. *Acceptance of Medicaid as payment for SUD treatment increased* following implementation of ARTS compared to states without similar policy changes in SUD benefits.

H2. *The number of SUD treatment facilities per 100,000 population increased* following implementation of the ARTS program compared to states without similar policy changes in SUD benefits.

METHODS

Data Sources

The primary data source for this analysis is the Substance Abuse and Mental Health Service Administration's (SAMHSA) National Survey of Substance Abuse Treatment Services (N-SSATS), which is an annual census of substance use treatment facilities.[24] The survey includes all public and private treatment facilities in SAMHSA's Inventory of Behavioral Health Services and facilities newly identified during the first three to five months of the field period. The N-SSATS is fielded each year from the last business day of March through December, where the last business day of March is used as the survey reference date. SAMHSA collects responses via web, phone, and mail, and provides several rounds of reminders, achieving an average response rate of 92 percent over the study period. This study uses the facility-level, public-use NSSATS files from 2010—the year the MHPAEA went into effect—through 2019, which is the year Medicaid expansion was implemented in Virginia. The MHPAEA implementation year was chosen as baseline because the parity law significantly increased SUD treatment providers' acceptance of Medicaid.[18] State-by-year demographic characteristics were incorporated from the U.S. Census Bureau's American Community Survey (ACS) to account for variation in state population and composition.[25,26] The age-adjusted overdose death rate in the baseline year was included from the Center for Disease Control and Prevention's (CDC) WONDER Underlying Cause of Death Database as a proxy for severity of the opioid crisis by state.[1]

Study Design

A pooled cross-sectional study design was used to assess the impact of the ARTS benefit on facility acceptance of Medicaid for SUD treatment services in Virginia relative to states without similar changes in SUD benefits. Although Virginia was the 14th state to implement an

1115 SUD waiver, Virginia was the 33rd state to implement Medicaid Expansion.[27,28] The control group consisted of 13 non-expansion states that had not implemented an 1115 SUD waiver by 2019 (see Appendix Figure 9), including nine Southern states (Alabama, Florida, Georgia, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, and Texas), two Midwestern states (Missouri and South Dakota), and two Western states (Idaho and Wyoming).[9,27] Failure to account for Medicaid expansion when identifying potential control states may bias results, as changes in program eligibility affect the demographic composition and health needs of the state Medicaid populations.

Dependent Variable

Two dependent variables were defined to assess changes in SUD treatment capacity after ARTS at the facility and state levels. The primary dependent variable assessed whether there was a change in the percentage of facilities accepting Medicaid for SUD treatment. Facility acceptance of Medicaid was defined as a binary indicator variable, where 1 indicates facility acceptance of Medicaid as payment for SUD services. The secondary dependent variable assessed whether there was a growth in the overall number of SUD treatment facilities, where a state-level measure of the number of SUD treatment facilities per 100,000 population was defined by aggregating the facility-level dataset at the state-by-year level. N-SSATS respondents with missing values for Medicaid acceptance were removed from all analyses (1.6 percent of respondents in Virginia and 1.7 percent of respondents in comparison states).

Independent Variables

The primary independent variable in the analysis of the impact of the ARTS benefit on the SUD treatment capacity is an indicator for facilities in Virginia, where the ARTS benefit was implemented in Virginia in 2017. Facility characteristics include ownership status as private, for-

profit, private non-profit, or government-owned; other forms of payment accepted, including private insurance, other non-Medicaid forms of public insurance (e.g., Medicare, Tricare, or other state-financed health insurance), self-pay, and charity care;[10] and SUD treatment services offered, including outpatient, residential, hospital inpatient, or MOUD. Annual state-level factors associated with the SUD provider supply and demand for SUD services were included from the ACS and CDC WONDER, including sex, age, race/ethnicity, level of urbanization, educational attainment, percentage of the state population below the poverty level, unemployment rate, and age-adjusted overdose death rate in the baseline year.[1,29–31]

Empirical Approach

Differences between facility and state-level characteristics of Virginia and control states during the pre-ARTS period were summarized with descriptive statistics and tested using Wald chi-square tests clustering the standard error at the state. Time-varying confounding was further assessed by testing whether each independent variable varied over time during the pre-ARTS period and whether the association between each independent variable and Medicaid acceptance varied over the pre-ARTS period.[32] Difference-in-differences regression was used to estimate the treatment effect of the ARTS benefit on the probability of facility acceptance of Medicaid relative to states without similar changes in SUD benefits. Treatment effects were estimated using a linear probability model of the form:

$$y_{st} = \beta_0 + \beta_1 VA * year_{17} + \beta_2 VA * year_{18} + \beta_3 VA * year_{19} + \beta_4 VA + \alpha_t + \delta_s + \boldsymbol{X}_{st}\gamma + \boldsymbol{Z}_{ist}\lambda + \varepsilon_s \quad (1)$$

The intercept β_0 represents the mean outcome among control states without SUD benefit expansions in the baseline year. The parameters β_1 through β_3 represent the parameters of interest as separate treatment effects by interacting the Virginia indicator with year dummy variables for

each post-treatment year, 2017–2019. Fixed effects for year and state were included to account for secular trends and time-invariant state differences, respectively represented by α and δ. The parameter γ represents a vector of pre-treatment, state-level characteristics, and λ represents a vector of facility-level characteristics. State-level characteristics are fixed at their 2016 values for all post-treatment state-years to avoid "post-treatment bias," which could bias results by partially adjusting away treatment effects.[33] Residuals (ε_s) were clustered at the state-level to account for correlation among facilities within the same state. As the SUD treatment landscape varies significantly by setting and treatment intensity, the above model was estimated among all treatment facilities and stratified by specific treatment contexts, including facilities offering outpatient treatment, opioid treatment programs (OTPs), and facilities offering SUD treatment in languages other than English.

Although the 13 control states did not implement 1115 SUD waivers over the study period, the control states needed similar pre-treatment or parallel trends in Medicaid acceptance in order for the difference-in-difference results to be interpretable as a valid counterfactual of what would have happened in Virginia in the absence of ARTS. The parallel trends assumption was assessed both visually and statistically by testing whether the pre-treatment trends in Medicaid acceptance were significantly different between facilities in Virginia and control states Whether facilities in Virginia anticipated the ARTS program (i.e., the "no anticipation" assumption) was tested by lagging the ARTS indicator and testing whether there was a significant difference in Medicaid acceptance between facilities in Virginia and control states in the year prior to ARTS. Treatment effects from model 1 were estimated first by only including state and year fixed effects, and then successively adding in potentially confounding variables to account for variation in state-level economic conditions, state-level demographic factors, and

facility-level ownership. Facility characteristics that may have been affected by the policy change such as the types of SUD treatment services offered were not included as covariates to avoid post-treatment bias.

The secondary hypothesis that the ARTS policy increased treatment capacity as defined by the number of treatment facilities per 100,000 population was tested by collapsing the facility-level observations to a state-by-year panel dataset and using outlier analysis to estimate whether the change in the outcome significantly differed for Virginia relative to the average change among the control states:

$$y_{s,t} - \bar{y}_{s,t<2017} = \beta_0 + \beta_1 VA + \varepsilon_s \quad (2)$$

State-specific linear detrending was estimated by first taking the difference between the observed outcome in each year and the pre-treatment average outcome for each state. The state-specific detrended outcome was then regressed on an indicator variable for the treated unit, Virginia, for each year in the study period. The resulting t-statistic for the parameter estimate of the Virginia indicator tests whether the change in Virginia is an outlier relative to the average change in the comparison states. Cluster robust standard errors were not used for outlier analysis.[34,35]

Sensitivity Analysis

Several sensitivity analyses were used to assess the robustness of observed treatment effects of ARTS on facility acceptance of Medicaid in Virginia. First, the robustness of the linear probability model was assessed using a nonlinear difference-in-differences model with a Bernoulli distribution and logit link function. Second, the robustness of the facility-level findings was tested using two state-level approaches. A balanced panel dataset of state-by-year observations was collapsed from the primary facility-level dataset. The outlier analysis approach described above was used to assess whether the post-treatment change in percentage of Medicaid

acceptance in Virginia significantly differed from the average change in the control states.[34,35] Treatment effects were also estimated using a synthetic control approach balancing on pre-treatment state demographic characteristics, economic conditions, and the age-adjusted overdose death rate for 2012.[36] Lastly, the robustness of the results to the choice of comparison states was assessed by estimating treatment effects compared to all non-expansion states, regardless of 1115 SUD waivers (n=15), and only Southern, non-expansion states (n=9).

RESULTS

Table 1 presents descriptive facility and state-level characteristics for Virginia and comparison states for the pre-ARTS period, 2013–2016. The pre-ARTS years 2010–2012 were excluded from all analyses due to violations of the parallel trends assumption in the years immediately following implementation of MHPAEA in 2010 (Appendix Figure 11). Facilities in Virginia had similar levels of Medicaid acceptance for SUD treatment (60.3 percent) to comparison states (58.2 percent) during the pre-ARTS period; however, facilities in Virginia were more likely to accept private insurance, other non-Medicaid public insurance, and self-pay for SUD treatment compared to facilities in comparison states. Fewer facilities in Virginia reported private, for-profit ownership (27.3 percent) compared to comparison states (40.5 percent). Facilities had similar levels of outpatient and hospital inpatient SUD treatment, but facilities in Virginia had fewer residential treatment centers (18.5 compared to 24.7 percent in comparison states) and more facilities offering MOUD (46.4 compared to 32.6 percent in comparison states). Comparing state-level characteristics, Virginia and comparison states were similar in terms of sex, age, percentage of population living in an urban area, and age-adjusted drug overdose death rate; however, Virginia had a lower average percentage of the population identified as Hispanic ethnicity, and more favorable economic indicators with a lower pre-ARTS

average poverty rate, unemployment rate, and percentage of the population with less than a high school education.

Table 1. Descriptive characteristics of the sample in the pre-ARTS period, 2013–2016.

Characteristics[1]	Facilities in Virginia n=897		Facilities in Control States n=12,689		p-value[2]
Facility-level characteristics (n, %)					
Accepted payment types					
Medicaid	541	60.3	7,385	58.2	0.649
Private	665	74.3	8051	64.2	<0.001
Other Public	600	66.9	7022	55.3	<0.001
Self-pay	842	94.5	11562	91.4	0.006
Charity care	593	66.1	8970	70.7	0.066
Ownership					
Private, for-profit	245	27.3	5143	40.5	<0.001
Private, non-profit	217	24.2	6029	47.5	<0.001
Government	435	48.5	1517	12.0	<0.001
SUD treatment services offered					
Any outpatient	767	85.5	10630	83.8	0.112
Any residential	166	18.5	3135	24.7	<0.001
Any hospital inpatient	57	6.4	834	6.6	0.745
Any MOUD	416	46.4	4136	32.6	<0.001
Number of MOUD					<0.001
0	481	53.6	8536	67.4	
1	163	18.2	1809	14.3	
2	114	12.7	1093	8.6	
3	107	11.9	1017	8.0	
4	32	3.6	209	1.7	
MOUD offered					
Buprenorphine with naloxone	243	27.1	2429	19.2	<0.001
Buprenorphine without naloxone	196	21.9	1610	12.7	<0.001
Methadone	117	13.0	1422	11.2	0.280
Naltrexone	284	31.7	2432	19.2	<0.001
State-level characteristics (mean, SD)					
Male	49.2	0.05	49.1	0.52	0.575
Age GTE 65	13.2	0.49	14.4	2.46	0.213
Race/ethnicity					
Non-Hispanic White	63.7	0.46	62.4	11.80	0.744
Non-Hispanic Black	18.9	0.04	16.8	8.7	0.423
Hispanic	8.5	0.23	15.0	11.8	0.048
Non-Hispanic Other	9.0	0.27	5.7	3.02	0.123
Urbanicity	75.5	0.00	74.4	11.53	0.812
Poverty rate	11.4	0.33	16.5	1.79	0.015
Unemployment rate	6.6	0.49	8.8	1.8	0.003
Educational attainment less than high school	11.9	0.42	14.3	2.39	0.017
Age-adjusted drug overdose death rate	12.8	2.4	15.3	4.44	0.081

[1]Differences in discrete, facility-level variables reported in frequencies and percentages and continuous, state-level variables reported in means and standard deviations (SD); [2]p-values from Wald chi-square tests with standard errors clustered at the state level

As the difference-in-difference approach requires similar trends across the pre-treatment period, the time-varying differences of each facility and state-level independent variable are presented in Appendix Table 7. Significant differences in the association between each covariate and acceptance of Medicaid or differences in the covariate trends between Virginia and comparison states inform the appropriate regression adjustment required for difference-in-differences estimation.[32] A summary of level and trend differences is presented in Appendix Table 8, and a decision tree summarizing how differences between levels and trends informed regression adjustment is included in Appendix Table 9.

Difference-in-differences results of the impact of ARTS on facility acceptance of Medicaid as payment for SUD treatment are presented in Table 2. There was sufficient evidence of parallel pre-ARTS trends for all models, as all tests for pre-trends and no anticipation were non-significant. Only controlling for secular trends and time-invariant state differences through state and year fixed effects indicates there was a 4.6 percentage point increase in Medicaid acceptance in the first year of ARTS (95% CI: 2.4, 6.7), a 10.8 percentage point increase in Medicaid acceptance in the year following ARTS implementation (95% CI: 9.3, 12.3), and finally a 13.2 percentage point increase in the second year following ARTS implementation (95% CI: 11.4, 15.0). Controlling for differences in the poverty rate, percentage of the population identified as Black, Indigenous, and People of Color, and facility ownership type resulted in final treatment effect estimates of the impact of ARTS rising from a 4.9 percentage point increase in 2017 (95% CI: 3.3, 6.5), an 8.0 percentage point increase in 2018 (95% CI: 5.8, 10.1), and a 10.9 percentage point increase in 2019 (95% CI: 8.4, 13.4) (Figure 1, plot A). Nonlinear difference-in-differences, state-by-year level comparisons, and variations in comparison states yielded comparable treatment effect estimates (Table 2; Appendix Figure 12).

Table 2. Average treatment effects on the treated (ATTs) of ARTS on acceptance of Medicaid as payment for substance use treatment.

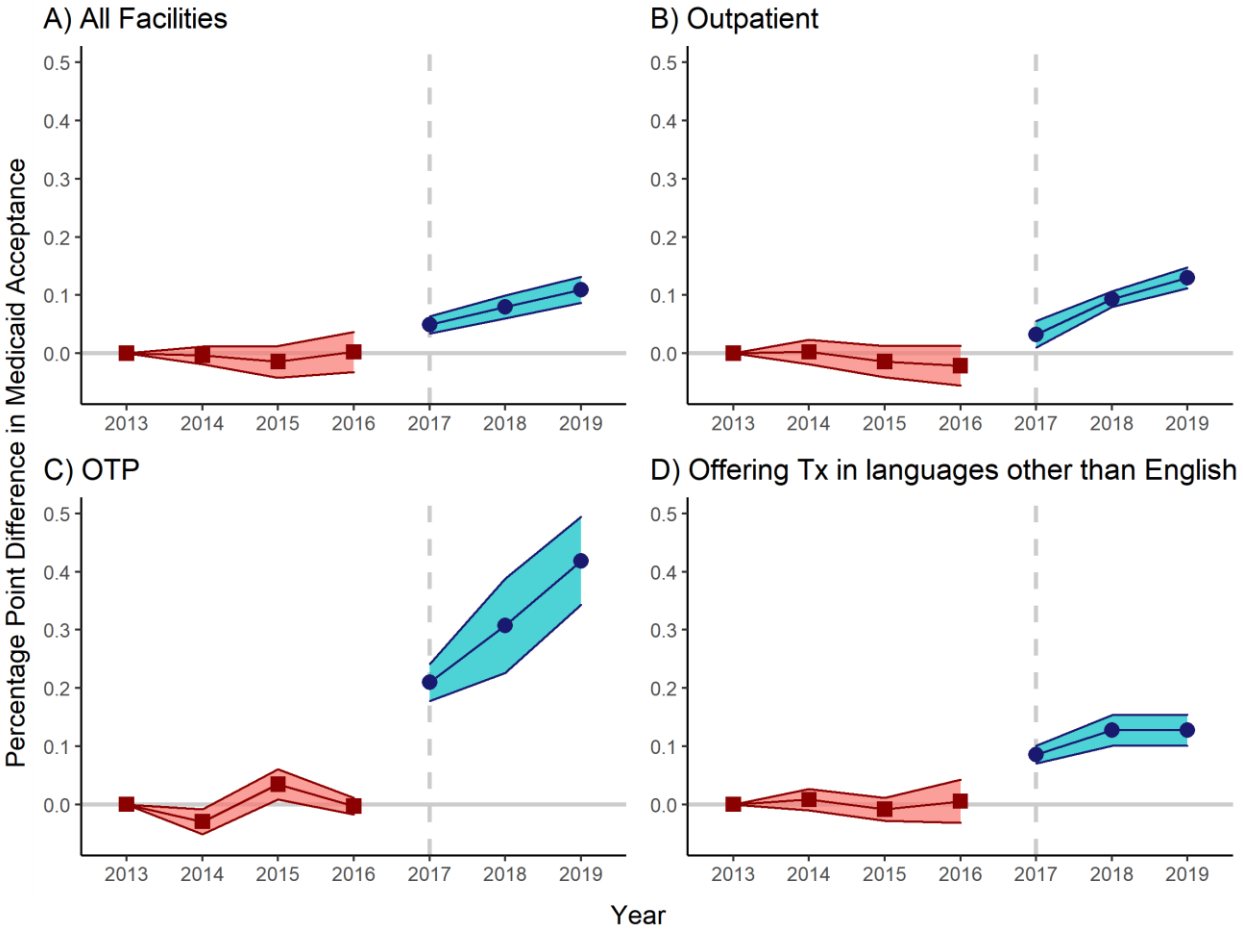
Model		ATTs vary across time			Pre-trends p-value	No anticipation p-value
		2017	2018	2019		
Linear probability models						
1	State and year fixed effects	0.046	0.108	0.132	0.842	0.662
		(0.010)	(0.007)	(0.008)		
2	M1 + poverty rate	0.039	0.102	0.125	0.496	0.280
		(0.008)	(0.010)	(0.011)		
3	M2 + % BIPOC	0.050	0.112	0.136	0.648	0.782
		(0.006)	(0.008)	(0.007)		
4	M3 + for-profit ownership	0.049	0.080	0.109	0.269	0.060
		(0.007)	(0.010)	(0.011)		
Non-linear model (Bernoulli distribution with logit link function)						
5	State and year fixed effects	0.045	0.107	0.131	0.859	0.659
		(0.010)	(0.007)	(0.009)		
State-by-year level comparisons						
6	Outlier analysis with state-specific linear detrending	0.049	0.111	0.131	--	--
		(0.045)	(0.035)	(0.039)		
7	Synthetic control[1]	0.048	0.111	0.142	--	--
		--	--	--		
Sensitivity to comparison states – all non-Medicaid expansion states (n=15)						
8	State and year fixed effects	0.047	0.109	0.132	0.985	0.975
		(0.010)	(0.007)	(0.009)		
9	M8 + poverty rate	0.042	0.104	0.127	0.755	0.498
		(0.007)	(0.010)	(0.011)		
10	M9 + % BIPOC	0.046	0.108	0.131	0.867	0.888
		(0.005)	(0.006)	(0.006)		
11	M10 + for-profit ownership	0.046	0.076	0.107	0.497	0.131
		(0.007)	(0.009)	(0.012)		

Model		ATTs vary across time			Pre-trends p-value	No anticipation p-value
		2017	2018	2019		
Sensitivity to comparison states – all Southern, non-Medicaid expansion states (n=9)						
12	State and year fixed effects	0.047	0.109	0.134	0.605	0.959
		(0.011)	(0.008)	(0.010)		
13	M12 + poverty rate	0.041	0.102	0.127	0.505	0.466
		(0.007)	(0.011)	(0.012)		
14	M13 + % BIPOC	0.041	0.102	0.127	0.448	0.401
		(0.006)	(0.008)	(0.009)		
15	M14 + for-profit ownership	0.040	0.073	0.109	0.376	0.597
		(0.010)	(0.011)	(0.016)		

[1]Synthetic-Virginia comprised of Texas (0.51), Tennessee (0.20), South Carolina (0.15), Missouri (0.08), and South Dakota (0.06) balanced on state urbanicity, percent BIPOC, number of facilities per 100,000 population, age-adjusted overdose death rate in 2012, and percentage of facilities offering methadone.

The variation in the percentage point increases in Medicaid acceptance for SUD treatment in Virginia relative to control states are visually presented overall and by three treatment contexts in Figure 1. As stratifying facilities by treatment context resulted in much smaller sample sizes (Appendix Table 10), the context-specific models controlled for the fewest covariates required to meet the parallel trends assumption. Facilities offering outpatient treatment and OTPs had sufficiently parallel trends adjusting for state and year fixed effects; however, facilities offering treatment in languages other than English required adjustment for poverty rate, percentage of population identified as Black, Indigenous, and People of Color, and for-profit ownership status (Appendix Table 11). The 2013–2016 pre-ARTS average Medicaid acceptance in Virginia varied widely by treatment context: 76 percent of facilities offering treatment in languages other than English (n=375), 64 percent of all outpatient settings (n=767), and only 26 percent of OTPs (n=106). Despite the variation in pre-ARTS Medicaid acceptance, there were significant, positive increases in Medicaid acceptance for SUD treatment across all three treatment contexts (Figure 1, plots B through D).

Figure 1. ATTs of ARTS on facility acceptance of Medicaid as payment for substance use treatment overall (A) and by treatment context (B through D).



The results of the state-level analysis examining whether there was a significant increase in the number of facilities per 100,000 population are visually presented in Appendix Figure 13. Although the state-level outlier analysis of the percentage point increase in Medicaid acceptance in Virginia was significant relative to comparison states in 2018 and 2019 (Appendix Figure 13, plot A), there was so significant difference in the log-number of facilities per 100,000 population (Appendix Figure 13, plot B).

DISCUSSION

This study identified a significant increase in SUD treatment facility acceptance of Medicaid after ARTS, increasing by 8.0 percentage points on average over the post-treatment period from a baseline level of 60.3 percent of facilities (95% CI [6.2, 9.7]) overall and across a variety of treatment contexts; however, there was no change in the number of treatment facilities per 100,000 population in the post-treatment period, indicating that the gains in Medicaid participation are among the existing SUD treatment workforce as opposed to a facility-level expansion in the treatment delivery system.

States seeking to improve patient outcomes and increase Medicaid participation among their SUD treatment workforce may benefit from quasi-experimental evidence of the impact of Medicaid-covered SUD treatment benefits, as this study provides additional systems-level context to previously observed changes in favorable patient utilization following the implementation of ARTS. The increase in treatment capacity supports recent findings of increased utilization of initiation and engagement of OUD treatment and MOUD among Medicaid members with OUD in Virginia, where OUD treatment rates increased nearly four-fold to 26 percent between 2016 and 2018, and MOUD use doubled from 32 to 63 percent.[13] Similarly, the increase in treatment capacity supports a similar downstream change in patient outcomes, where ARTS was associated with decreased acute care utilization among Virginia Medicaid members with OUD compared to members without SUD.[14]

Although this study provides additional evidence that the provisions in the ARTS benefit significantly reduced structural barriers to access to care for Medicaid members with SUD, there are several notable limitations to this study. First, as the ARTS benefit includes many components like increased provider reimbursement rates, provider trainings, and the IMD waiver for residential and inpatient treatment, this study does not disentangle the overall effect to

identify the impact of any single component within ARTS. Second, although there may have been an increase in Medicaid acceptance, the N-SSATS does not include questions related to payer mix. This study does not account for the volume of Medicaid patients receiving treatment or any cost shifting that result from Medicaid acceptance. Lastly, although the inclusion of state-level demographics improves the comparability of the treated and comparison states, this study is unable to assess whether the ARTS benefit impacted racial disparities in access to care within the SUD treatment system.[37–39] Future research should investigate change to SUD benefits affects racial disparities in access to treatment or whether variation in reimbursement rates results in any cream skimming by treatment facilities.

Medicaid-covered behavioral health benefits play an important role in Medicaid participation among treatment facilities. State Medicaid agencies with unmet population health needs for behavioral health and SUD treatment may consider the comparability of their state-specific reimbursement rates relative to states with higher Medicaid participation and engage with their provider community to identify and address barriers to Medicaid participation.

Chapter 2: Implications of Using the Extended Two-Way Fixed Effects Method for Estimating the Effects of Health Care Policies with Staggered Implementation Dates

INTRODUCTION

Difference-in-difference is a quasi-experimental tool to evaluate interventions or policy changes occurring in observational settings when randomized controlled trials are unethical or infeasible.[40,41] Comparing the change in outcomes before and after a policy change among the treated group (difference 1) to the same change among a control group not affected by the policy change (difference 2), the difference-in-difference estimator (difference 1–difference 2) provides evidence of the effect of a policy change or treatment. The control group provides counterfactual evidence of what would have happened to the treated group in the absence of the policy change, as long as the control group's trend in outcomes in the pre-treatment time period is similar to the treated group. This key assumption is often referred to as either the parallel trends or pre-trends assumption and is a requirement to attribute the observed change in outcomes to the policy change.

Medicaid expansion provides a ubiquitous example of a policy change often analyzed using difference-in-differences and introduces the challenge of evaluating policies implemented at different time points. As of 2019, 33 out of 50 states opted to expand Medicaid coverage to 138% of the federal poverty level (FPL) at four different time points: 26 in 2014, 3 in 2015, 2 in 2016, and 2 in 2019 (see Appendix Figure 15).[27] The staggered timing of expansion means there were 17 states who had not implemented Medicaid expansion as of 2019, allowing for a control group of untreated states. Taking the difference between the mean change in the percentage of the populations with insurance coverage observed in the 33 expansion states after expansion (difference 1) and the mean change in coverage in the 17 non-expansion states (difference 2), provides an estimate of the effect of expansion on insurance coverage (difference-in-difference).

An extensive body of research has documented the significant increase in insurance coverage among Medicaid expansion states, where early research identified a 5.2 percentage point reduction in the uninsured rate after expansion.[42,43] Studies using a difference-in-differences framework to analyze the expansion effect are frequently estimated either only including the 26 states that expanded in 2014 or using a two-way fixed effects (TWFE) model with a time-varying indicator to account for variation in expansion timing across states;[44,45] however, both approaches have non-trivial drawbacks. Dropping states that expanded Medicaid eligibility after 2014 omits a growing number of late-expansion states from overall treatment effects estimates. Recent methodological research has also identified that applying TWFE to the case of staggered treatment timing produces biased estimates.[7]

Two-way fixed effects

A TWFE model formulation includes a *time-varying treatment indicator* variable defined as one beginning in the first treated period for treated units, controlling for *unit fixed effects* to account for time-invariant differences between treated and control units, and *time fixed effects* to account for changes to secular trends that would have affected all units.[7,46] For time-varying indicator *w*, units *i*, and time *t*, ordinary least squares (OLS) can be used to estimate the treatment effect, β_1:

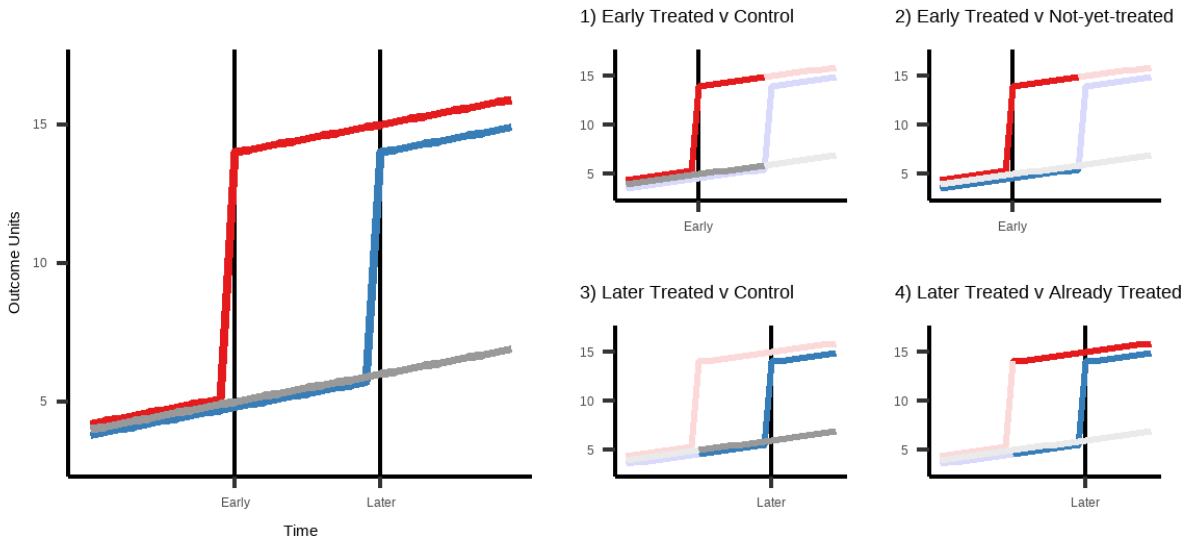
$$y_{it} = \beta_0 + \beta_1 w_{it} + \gamma_i + \delta_t + \varepsilon_{it} \quad (1)$$

In the case when all treated units are treated at a common time, TWFE provides the same treatment effect estimate as another common difference-in-difference specification using an interaction between a treatment group indicator and a post-treatment time dummy:

$$y = \beta_0 + \beta_1 treatment_i \times post_t + \beta_2 treatment_i + \beta_3 post_t + \varepsilon_{it} \quad (2)$$

When treated units are treated at different times, TWFE was commonly used to estimate a single treatment effect; however, Goodman-Bacon's decomposition of the TWFE estimate revealed the parameter estimate associated with the time-varying treatment indicator is actually a variance-weighted average of each pairwise comparison of treated and control groups at each treatment time point—including problematic comparisons of newly treated units with units who have already been treated as if they were valid controls.[7] Figure 2 illustrates a simple case of three groups, where a control group is never treated (grey), and two treatment groups are treated at two different time points (red, treated early; blue, treated later), yielding four distinct, pairwise comparisons.

Figure 2. Illustration of variation in treatment timing and comparisons included in TWFE overall treatment effect estimate.



Note: Adapted from Goodman-Bacon 2021; comparisons 1–3 are valid but comparison 4 is a forbidden comparison and biases the overall treatment effect estimate

When the early treatment group is treated, it is valid to compare the early treatment group to the control group (comparison 1) and the later treatment group (comparison 2), as the later

treatment group is untreated at the time the early treatment group is treated. When the later treatment group is treated, it is valid to compare the later treatment group to the control group (comparison 3); however, a forbidden comparison emerges by comparing the later treatment group to the early treatment group, as the early treatment group has already been treated (comparison 4).[7,47] The inclusion of the forbidden comparison of later treated units to units that have already been treated biases the TWFE overall treatment effect estimate.

In the wake of Goodman-Bacon's decomposition of the TWFE estimate and identification of the bias of TWFE in the case of staggered treatment timing, several estimation methods emerged in the economics literature as alternatives to TWFE.[46–52] These new estimation methods differ in approach, avoiding the forbidden comparison problem by reshaping the dataset as an event-study, using long-differencing as opposed to regression, and providing packages for implementation in commonly used statistical software. Although software packages may reduce the learning curve required to implement these new solutions, an alternative approach utilizing regression-based tools familiar to applied researchers may be of interest. Applied researchers facing a trade-off between the ease of implementing TWFE using OLS and the challenge of implementing newer, black-box estimation methods may be tempted to assume the forbidden comparison problem is minor enough to ignore. Continuing to use TWFE to estimate treatment effects in the case of staggered treatment timing therefore represents a practical-knowledge gap warranting further examination.

Extended Two-Way Fixed Effects

The extended two-way fixed effects (ETWFE) approach to staggered difference-in-differences provides a way to avoid the forbidden comparison problem of TWFE by introducing more flexibility into the model by deliberately defining terms for each treated group and post-

treatment time periods.[35,46] Three sets of variables are required to estimate ETWFE from a standard panel dataset. First, a time-varying treatment indicator, *w*, is defined as one for all time periods on and after treatment (i.e., as in TWFE, described above). Second, a set of cohort-specific indicators, d_g, are defined for each treatment group, *d*, unified by their initial treatment time, *g*. Untreated units are defined as cohort d_{inf}, where "inf" suggests a treatment time of infinity. The inclusion of an indicator for untreated units provides a helpful way to confirm whether a dataset contains any control units ($d_{inf} = 1$ for some units) or whether all units are eventually treated ($d_{inf} = 0$ for all units), which has implications for model formulation and treatment effects estimation. Third, a set of time dummies are defined for each time period in the panel. Appendix Table 12 provides an illustrative example of how these three sets of variables are defined in the simple case illustrated in Figure 2.

In the case when a study contains a control group, ETWFE is implemented with *group-time treatment effects*—defined as an interaction between each cohort, *d*, first treated in time period *g*, with all time dummies on and after each cohort's first treatment period, $f_{t \geq g}$—and fixed effects for each cohort and time:

$$y = \left[d_g \times f_{t \geq g}\right] + d_g + f_t \quad (3)$$

If covariate adjustment is required, equation 3 also incorporates the time-varying treatment indicator, *w*. In the case when all units are eventually treated, equation 3 should include neither group-time treatment effects nor cohort fixed effects for the cohort treated last.

The ETWFE group-time treatment effects provide flexible building blocks that can be aggregated in different ways to estimate an overall treatment effect, cohort-specific average treatment effects, or event-time treatment effects, depending on the study objectives. Callaway and Sant'Anna provide a discussion of several aggregation approaches and Wooldridge

recommends using a linear combination of the group-time treatment effects to estimate an overall treatment effect.[46,48] Cohort-specific average treatment effects aggregate the group-time treatment effects for each cohort, averaging over the number of post-treatment periods per cohort. Finally, event-time average treatment effects are aggregated in relative time to stack each cohort's post-treatment periods to estimate how the intensity of the treatment effect changes on average over the post-treatment period for all treated cohorts, regardless of when they were initially treated.

The primary purpose of this study is to compare the performance of TWFE and ETWFE in the case of staggered difference-in-differences, including three alternatives to weighting the ETWFE overall treatment effect to compare different approaches to weighting the ETWFE group-time treatment effects to estimate aggregated treatment effects.

METHODS

The performance of TWFE and three weighted ETWFE effects was compared using Monte Carlo simulations under a variety of data-generating mechanisms (DGMs) and an empirical application to compare estimates the effect of Medicaid expansion on insurance coverage.

Simulation Study

Monte Carlo simulations varying total sample size, treatment timing, treatment effects, and comparison groups were used to compare basic TWFE to three ETWFE overall treatment effects (Table 3). The first ETWFE weighting scheme used the linear combination approach proposed by Wooldridge, which is the same as weighting the cohort-specific average treatment effects by the *share of time treated* (as proposed by Callaway and Sant'Anna).[46,48] The second scheme weighted the cohort-specific average treatment effects by the *share of units treated*. The

third weighting scheme combined the other two by weighting the cohort-specific average treatment effects by a *scaled combination of the share of time and units treated*. Appendix Table 13 provides an illustration of these three weighting schemes applied to the simple case in Figure 2.

Table 3. Data-generating mechanisms (DGMs) included in the Monte Carlo simulations comparing the performance of TWFE and three ETWFE overall treatment effect estimates in the case of staggered treatment timing, 2010–2019.

Conditions	Settings		Number of variations
Sample size	50, 100, and 200 total units (60% treated in simulations including control units)		3
Treatment timing	Equal allocation	20%, 20%, and 20%	3
	More units treated earlier	30%, 20% and 10%	
	More units treated later	10%, 20%, and 30%	
Treatment effects	Null	$\mu \sim N(0, 0)$, all trends increasing at rate of 0 per year	3
	Constant	$\mu \sim N(2, 0.04)$, all trends increasing at rate of 0.5 per year	
	Heterogeneous	$\mu_{2014} \sim N(3, 0.04)$, increasing at rate of 0.3 per year $\mu_{2015} \sim N(2, 0.04)$, increasing at rate of 0.15 per year $\mu_{2016} \sim N(1, 0.04)$, increasing at rate of 0.075 per year	
Comparison group	Including untreated control group and all units eventually treated		2

The DGMs included in Table 3 were chosen to approximate real-world scenarios, where difference-in-difference may be employed on state-level policy changes (N=50) or larger samples in the case of individual-level interventions (N=200); treatment cohorts may vary in size, as in Medicaid expansion where most units are treated early; treatment effects may be consistent or vary across treated cohorts; and all units may eventually be treated, which

precludes the estimation of the cohort treated last or any treatment effects of earlier treated cohorts during the year the final cohort is treated (i.e., as there are no longer any comparison units). Appendix Figure 14 graphically illustrates the variation in treatment timing, treatment effects, and comparison groups.

Each Monte Carlo simulation utilized a simulated panel dataset, which enables the calculation of each unit's counterfactual treatment effect, regardless of whether each unit was ultimately allocated to a treated cohort. The true treatment effects were calculated as the difference between each treated unit's treated outcome and their untreated outcome (i.e., a difference which would not be possible to calculate with a "real-world" dataset because counterfactual outcomes are unknown). Beyond the settings described in Table 3, each simulation included unit and time fixed effects normally distributed with a mean of zero and variance of one and autocorrelated errors (ρ=0.95). Each simulation proceeded by generating a dataset based on the DGM settings in Table 3 including control units and running a full analysis to estimate and store the four treatment effects of interest—three ETWFE overall treatment effects estimates applying the three weighting schemes described above and the TWFE overall treatment effect estimate. To maintain the sample size and treatment timing of treated units in the case when all units are eventually treated, untreated units were then dropped from the dataset and a full analysis was run to estimate the four treatment effects for the DGMs where all units are eventually treated. Therefore, one simulated dataset allowed estimating two DGMs within a single replication.

The number of replications needed per simulation was determined by running a small number (n=10) of replications per DGM to estimate the variance of the four treatment effects of interest. The maximum variance out of the four estimates was used to calculate the number of

replications needed to ensure a Monte Carlo standard error of bias no larger than 0.005 for the case including untreated units and when all units are eventually treated.[53] The maximum number of replications needed between the two comparison group settings was then rounded up to the nearest hundred to provide a conservative estimate of replications to ensure the results are not attributable to the Monte Carlo standard error. The number of replications for each DGM are included in Appendix Table 14.

The performance of the four treatment effects of interest was compared in terms of absolute bias and coverage of confidence intervals.[53] Unbiasedness is a key property of an estimator, and absolute bias was defined as the absolute difference between the treatment effect estimates and the true treatment effect. Coverage was defined as the percentage of simulations where the confidence intervals for the treatment effect estimates contained the true treatment effect. As 95% confidence intervals were estimated for each replication, the resulting coverage was expected to be approximately 95%, where bias would result in lower coverage.

Empirical Application

The primary objective for the empirical application was to estimate the effect of Medicaid expansion on the percentage of insured lower income, non-elderly adults without children in the US using both TWFE and ETWFE to contrast the treatment effects using these two estimation approaches to difference-in-difference in the case of staggered treatment timing. The secondary objectives were to demonstrate how ETWFE can be used to test for parallel trends; control for a continuous, pre-treatment covariate or heterogeneous linear trends; and lastly, estimate hypothetical treatment effects as if all remaining non-expansion states had expanded in 2019 (i.e., to demonstrate the case when all units are eventually treated).[35]

Data

A panel dataset of state-by-year estimates of the percentage of low income, non-elderly adults without children was constructed for the years 2010–2019 from the publicly available Integrated Public Use Microdata Series (IPUMS) USA data from the U.S. Census Bureau's American Community Survey.[25,54] IPUMS USA allows for the identification of a nationally representative sample of adults who may be most likely to benefit from Medicaid expansion: non-elderly adults aged 26–64 who are ineligible for young adult coverage through their parent's plan and not yet Medicare eligible; with household incomes under 100% of the FPL who would not qualify for a premium subsidy through an Affordable Care Act Marketplace plan; and who have no children in the household, as parents of minor children may be subject to different income thresholds when determining Medicaid eligibility compared to households without children.

As applied health services researchers may wish to estimate TWFE as a rough estimate of overall treatment effects, TWFE was first used to estimate of the impact of the Medicaid expansion on insurance coverage by regressing the percentage of insured adults on the time-varying treatment indicator of Medicaid expansion implementation, state fixed effects, and time fixed effects, with standard errors clustered at the state level. The Goodman-Bacon decomposition was used to quantify the contribution of the forbidden comparisons of late expansion states to states that have already expanded eligibility in the overall TWFE estimate.[55]

ETWFE was estimated assuming unconditional parallel trends for comparison with the TWFE estimates by regressing the percentage of insured adults on group-time treatment effects—interactions between each treated cohort indicator and all of time dummies on and after each cohort was initially treated—cohort fixed effects, and time fixed effects, with standard errors clustered at the state level. The group-time treatment effects were then aggregated by

weighting the cohort-specific average treatment effects by the scaled shares of time and units treated (an example of weight calculation is included in Appendix Table 13).

Parallel Trends Assumption

The ETWFE treatment effects are only interpretable if the parallel trends assumption is met, which can be empirically tested and potentially corrected in the case of significant pre-trends.[35] Parallel trends were first empirically tested by expanding the regression model above to include interactions between each treated cohort indicator and all of the time dummies preceding each cohort's initial treatment, excluding the first year to avoid the dummy trap. An F-test of all pre-treatment interaction terms provides a test of whether pre-trends are jointly different from zero.

The unconditional pre-trends assumption can be relaxed to assume conditional parallel trends by incorporating pre-treatment covariates associated with differences in pre-treatment trends in insurance coverage between expansion and non-expansion states.[32] To illustrate covariate adjustment, the 2010 state unemployment rate was incorporated to control for a continuous, pre-treatment covariate assumed to be associated with differences in pre-treatment trends in insurance coverage between expansion and non-expansion states.[56] First, average unemployment rates were demeaned by cohort by subtracting each treated cohort's average unemployment rate from each state's actual unemployment rate. The time-varying treatment indicator, *w*, demeaned unemployment rate, *DX*, and observed unemployment rate, *X*, were then added to model 3 as:

$$y = [w_{it} \times d_g \times f_{t \geq g}] + [w_{it} \times d_g \times f_{t \geq g} \times DX_i] + [d_g \times X_i] + [f_{t>t=1} \times X_i] + d_g + X_i + f_{t>t=1} \quad (4)$$

Model 4 includes treatment effect terms adding an additional interaction with *w*; the treatment effect terms including *w* interacted with the demeaned unemployment rate, *DX*; the cohort-specific indicators and time dummies all interacted with the observed unemployment rate, *X*; and main effects for the cohort-specific indicators, observed unemployment rate, and time dummies. Conditional pre-trends can then be tested as above, including interactions between each treated cohort indicator and all of the time dummies preceding each cohort's initial treatment and using an F-test on the pre-treatment terms.

Violations of parallel trends may also be corrected by adjusting for heterogeneous linear trends. Returning to model 3, incorporate interactions between each treated cohort indicator and a continuous term for time, *T*:

$$y = \left[d_g \times f_{t \geq g}\right] + d_g + f_{t>t=1} + \left[d_g \times T\right] \quad (5)$$

An F-test of the interactions between each treated cohort indicator and a continuous term for time, *T,* was used to test whether these cohort-specific linear trends significantly differ from zero. A non-significant result indicates controlling for these heterogeneous linear trends is sufficient to assume conditional parallel trends; however, a significant result may warrant an alternate solution such as excluding treated cohorts with significantly different linear trends.

Assuming a hypothetical scenario where the 17 non-expansion states actually had implemented Medicaid expansion in 2019 allows for an illustration of the case of staggered difference-in-differences when all units are eventually treated. The general process of contrasting TWFE and ETWFE overall estimates and testing and correcting for violations of parallel trends controlling for heterogeneous linear trends as described above was repeated with one key difference: in the case where all units are eventually treated, the terms involving the last treated cohort (i.e., the 2019 cohort) were no longer included in the model and the aggregation of the

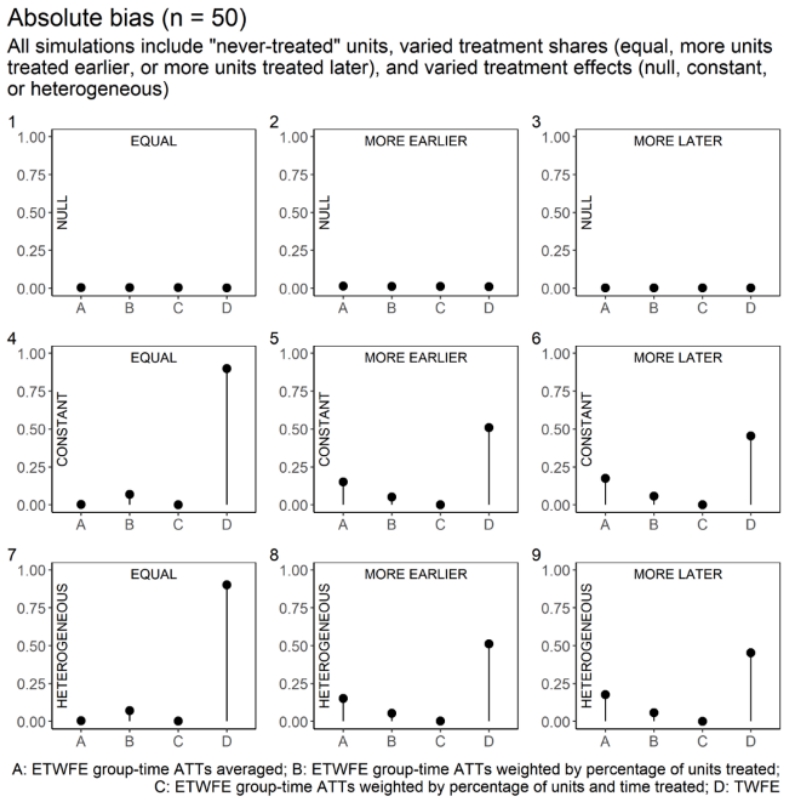
group-time treatment effects for cohorts treated before 2019 did not include the group-time treatment effects for the year the last treated cohort was treated (i.e., group-time treatment effects including the time dummy for 2019 were excluded from the aggregation of treatment effects). These terms should be included in the regression equation, but should not be interpreted as true treatment effects, as there were no longer any comparison units once the last treated cohort received treatment. Therefore, only the *feasible* treatment effect terms should be included in the overall, cohort-specific, or event-study treatment effect estimates.[57]

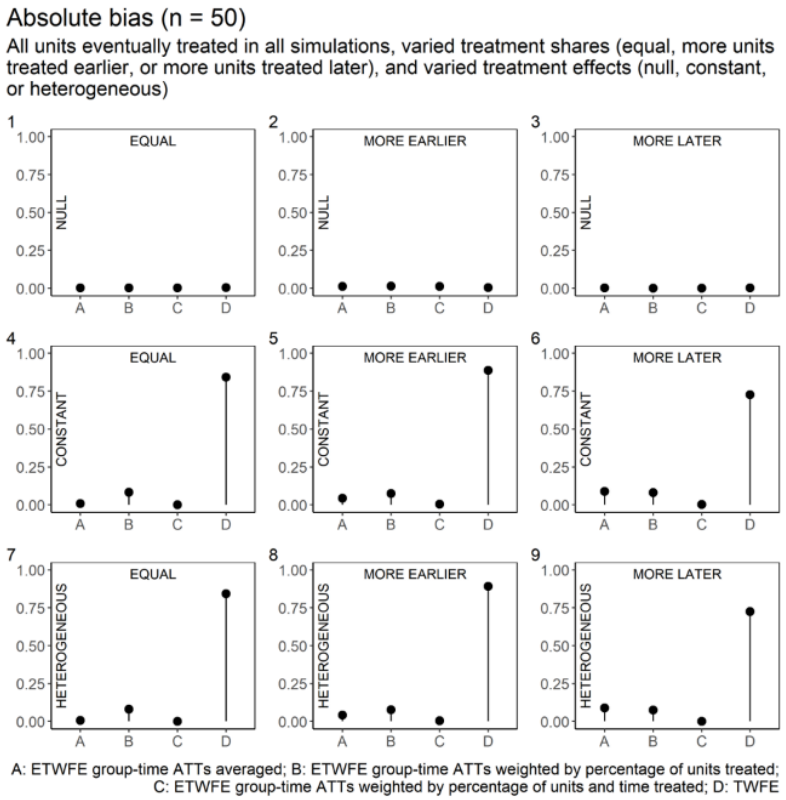
RESULTS

Simulation Study

Across all DGMs, ETWFE estimates weighted by the scaled combination of units and time treated outperformed basic TWFE and the two other ETWFE weighting schemes. All four estimators were minimally biased in the case of null treatment effects; however, bias emerged in the presence of treatment effects, particularly for TWFE. The three ETWFE estimates all outperformed TWFE, with a maximum absolute bias of 0.179 across all DGMs compared to 0.899 for TWFE. Among the three ETWFE weighting schemes, ETWFE estimates weighted by the percentage of time treated was most strongly biased in the case of variation in treated cohort sizes. ETWFE estimates weighted by the percentage of units treated were uniformly biased across DGMs. Tabular results are presented in Appendix Table 15 and graphically for sample sizes of 50 in Figure 3, 100 in Appendix Figure 16, and 200 in Appendix Figure 17. The lollipop plots in Figure 3 present the absolute value of the absolute bias, and the direction of the absolute bias is discernable in Appendix Table 15. In all DGMs with non-null treatment effects, the TWFE estimates were biased toward null.

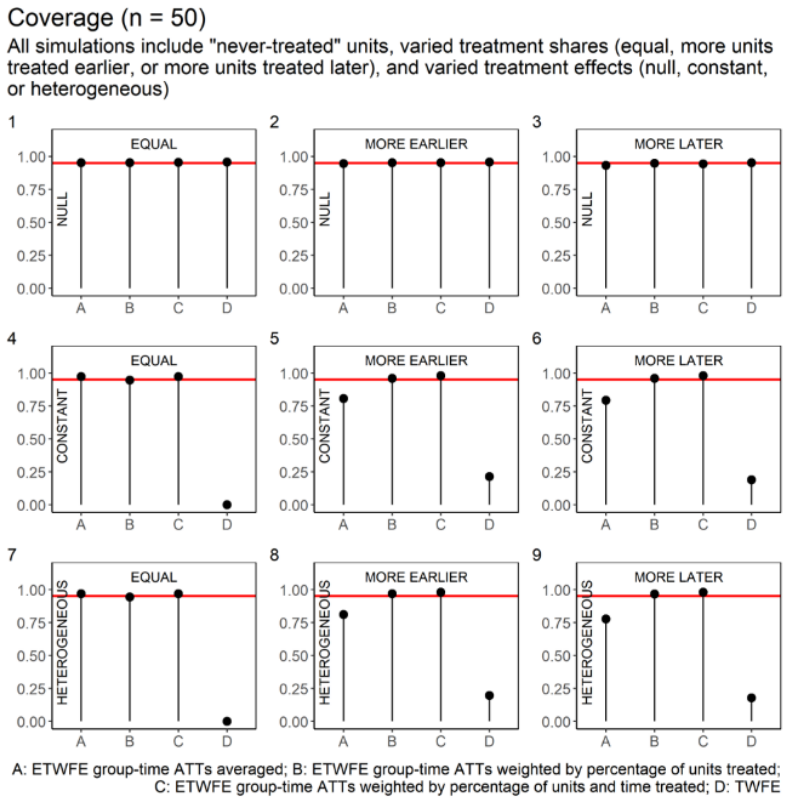
Figure 3. Absolute bias of TWFE and ETWFE estimators across DGMs with sample size of 50.

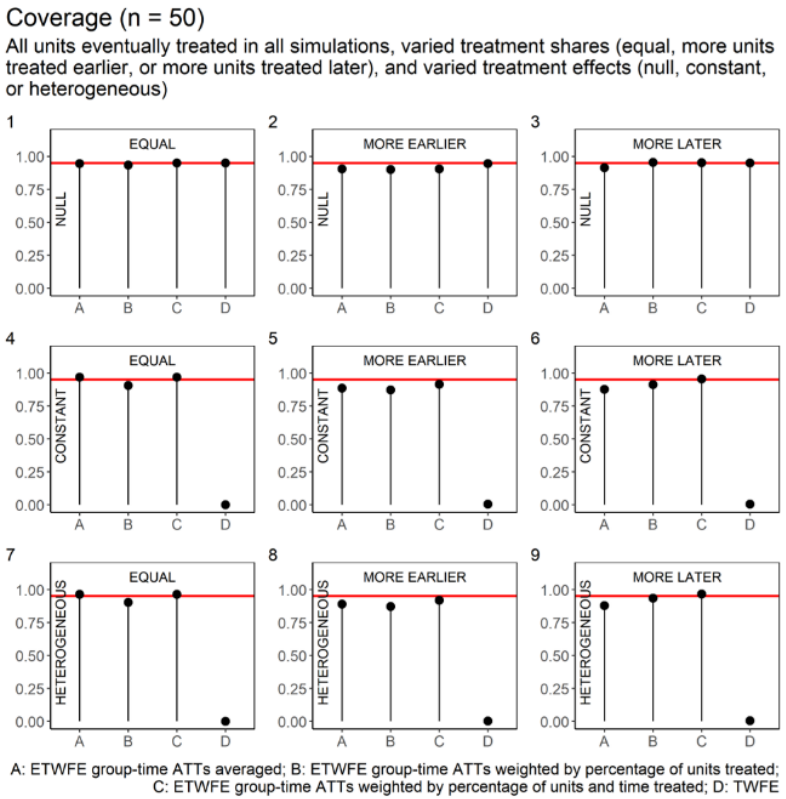




Similar to absolute bias, ETWFE estimates weighted by the scaled combination of units and time treated outperformed basic TWFE and the two other ETWFE weighting schemes as measured by the percentage of 95% confidence intervals containing the true treatment effect. In the case of null treatment effects, the four estimators had comparable coverage; however, coverage deteriorated rapidly for TWFE estimates in the presence of treatment effects. The poor coverage of TWFE was particularly egregious in simulations of non-null treatment effects when all units are eventually treated, as none of the TWFE confidence intervals contained the true treatment effect. Comparisons of the three ETWFE weighting schemes indicate coverage is affected by variation in treated cohort sizes, overall sample size, and comparison group. In DGMs including an untreated group, ETWFE weighted by share of time treated resulted in under-coverage of the true treatment effect increasingly with the overall sample size. In DGMs where all units are eventually treated, ETWFE weighted by the share of units treated resulted in under-coverage of the true treatment effect increasingly with the overall sample size. Tabular results of coverage are presented in Appendix Table 16 and graphically for sample sizes of 50 in Figure 4, 100 in Appendix Figure 18, and 200 in Appendix Figure 19. The lollipop plots of coverage of 95% confidence intervals containing the true treatment effect include a red line at 95% for reference of the target coverage.

Figure 4. Coverage of TWFE and ETWFE estimators across DGMs with sample size of 50.





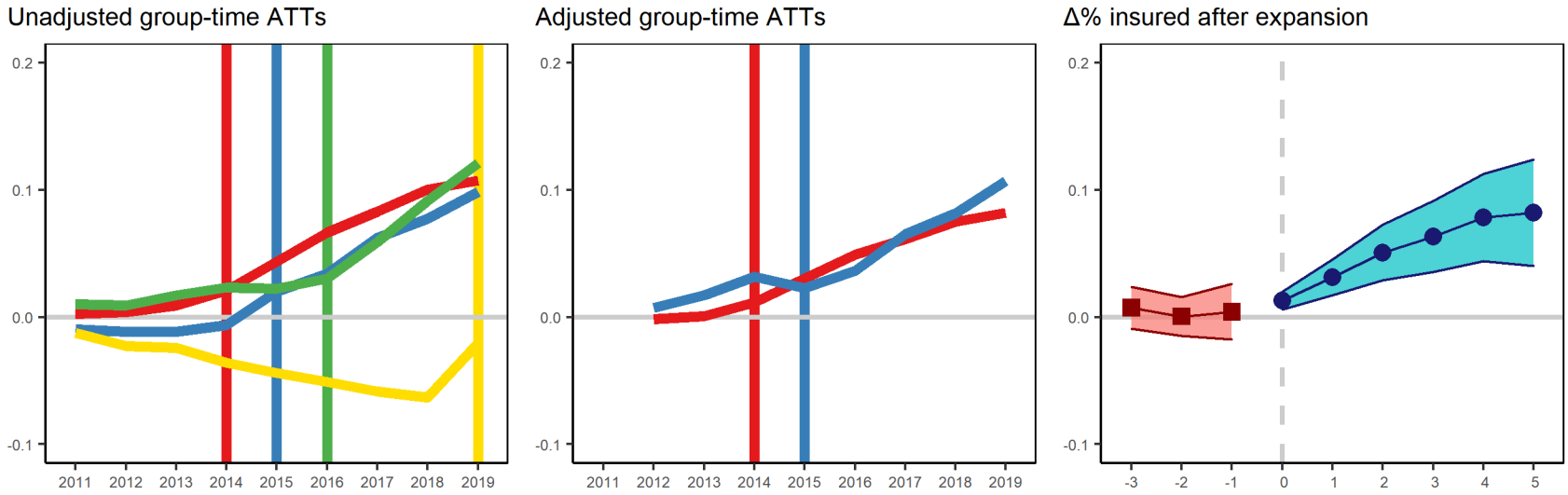
Empirical Application

Assessing the effect of Medicaid expansion on insurance coverage among low-income, childless adults ages 26–64 in states who implemented expansion by 2019 involved four cohorts of states—26 states in 2014, 3 in 2015, 2 in 2016, and 2 in 2019—leaving 17 non-expansion states as a control group of untreated states (see Appendix Figure 15). The TWFE rough estimate suggested a 5.9 percentage point increase in the percentage of insured adults after expansion (95% confidence interval (CI): 4.2–7.7); however, the Goodman-Bacon decomposition of the TWFE estimate reveals 7% of the TWFE estimate is attributable to the forbidden comparison of states newly implementing expansion against states who had previously expanded Medicaid eligibility, which contribute a -1.1 percentage point difference-in-difference estimate to the overall TWFE estimate.[55] The downward bias the TWFE estimate is more evident by comparing to the ETWFE estimate, which suggests Medicaid expansion increased insurance coverage by 6.8 percentage points (95% CI: 4.9–8.8).

Plotting the unadjusted ETWFE group-time treatment effects reveals a clear violation of the parallel trends assumption (Figure 5). Visual comparison of the four cohorts suggests the 2019 states had different pre-expansion trends in insurance coverage compared to the other three cohorts. A formal test of the unconditional parallel trends assumption confirms this visual assumption, as an F-test of interactions between each cohort indicator and their pre-expansion time dummies was jointly significant. Two potential corrections for violations of parallel trends indicated the four cohorts did not have conditional parallel pre-expansion trends in insurance coverage. First, controlling for each state's 2010 unemployment rate was insufficient to meet the conditional parallel trends assumption, as an F-test of interactions between each cohort indicator and their pre-expansion time dummies controlling for unemployment rate was jointly significant.

Second, controlling for heterogeneous linear trends by controlling for interactions between each cohort indicator and a linear time term only have sufficiently parallel trends after dropping the 2016 and 2019 expansion cohorts. An F-test of the interactions between each of the 2014 and 2015 cohort indicators with a linear time term failed to reject the null hypothesis of conditional parallel trends, which indicates it is appropriate to proceed with estimation of the ETWFE treatment effect for the effect of Medicaid expansion on insurance coverage only for the 2014 and 2015 cohorts, adjusting for heterogeneous linear trends.

Figure 5. Empirical application of ETWFE to the change in percentage of insured low-income, childless adults aged 26–64 after Medicaid expansion.



Notes: ATTs = average treatment effects on the treated; Δ% = change in percentage

The group-time treatment effects for the 2014 and 2015 cohorts adjusting for heterogeneous linear trends are graphically presented in the second plot in Figure 5 and in Table 4. The 11 group-time treatment effects—six post-implementation or treatment years for the 2014 cohort and five treatment years for the 2015 cohort—were aggregated in several ways to estimate different average treatment effects. First, the cohort-specific average treatment effects were estimated as a linear combination of the group-time treatment effects by cohort, where there was an average increase of 5.2 percentage points (95% CI: 2.6–7.7) in insurance coverage among the 2014 expansion states and a 6.3 percentage points (95% CI: 3.7–8.8) among the 2015 expansion states. Second, the event-study average treatment effects increased over the post-expansion period from a 1.3 percentage point increase during the implementation year (95% CI: 0.6–2.0) to 8.2 percentage points five years post-expansion (95% CI: 4.0–12.4). The event-study change in insurance coverage after expansion is graphically presented in years relative to expansion in the third plot in Figure 5. Third, there was an overall average increase of 5.3 percentage points in insurance coverage after expansion (95% CI: 2.8–7.7). Excluding the 2016 and 2019 cohorts, the TWFE estimate of 6.3 percentage points (95% CI: 4.4–8.3) is higher than the ETWFE estimate, likely biased due to both the forbidden comparisons as previously discussed and the violation of parallel trends identified in the unadjusted estimates.

Table 4. Percentage point change in insurance coverage among low income, childless adults aged 26–64 in 29 states that implemented expansion in 2014 and 2015 compared to 17 states that did not implement expansion by 2019.[1]

	Coef.	SE	*p*-value	95% CI	
Group-time ATTs					
Cohort Expanded in 2014 (n=26)					
$\text{Exp}_{2014} \times \text{Year}_{2014}$	1.2	0.4	0.006	0.4	2.0
$\text{Exp}_{2014} \times \text{Year}_{2015}$	3.1	0.8	<0.001	1.5	4.6
$\text{Exp}_{2014} \times \text{Year}_{2016}$	4.9	1.2	<0.001	2.5	7.3
$\text{Exp}_{2014} \times \text{Year}_{2017}$	6.1	1.5	<0.001	3.0	9.2
$\text{Exp}_{2014} \times \text{Year}_{2018}$	7.5	1.9	<0.001	3.7	11.3
$\text{Exp}_{2014} \times \text{Year}_{2019}$	8.2	2.1	<0.001	3.9	12.5
Cohort Expanded in 2015 (n=3)					
$\text{Exp}_{2015} \times \text{Year}_{2015}$	2.3	0.8	0.008	0.6	3.9
$\text{Exp}_{2015} \times \text{Year}_{2016}$	3.7	1.0	0.001	1.6	5.7
$\text{Exp}_{2015} \times \text{Year}_{2017}$	6.5	1.3	0.000	4.0	9.1
$\text{Exp}_{2015} \times \text{Year}_{2018}$	8.2	1.8	<0.001	4.5	11.8
$\text{Exp}_{2015} \times \text{Year}_{2019}$	10.7	1.9	<0.001	6.8	14.6
Overall ATT[1](n=29)	5.3	1.2	<0.001	2.8	7.7
Group-specific ATTs[1]					
Cohort Expanded in 2014 (n=26)	5.2	1.3	<0.001	2.6	7.7
Cohort Expanded in 2015 (n=3)	6.3	1.3	<0.001	3.7	8.8
Event-time ATTs[1]					
-3 years pre-exp. (n=3)	0.7	0.8	0.393	-1.0	2.4
-2 years pre-exp. (n=29)	0.0	0.8	0.957	-1.5	1.6
-1 year pre-exp. (n=29)	0.4	1.1	0.713	-1.8	2.7
0 Exp. implementation (n=29)	1.3	0.4	0.001	0.6	2.0
1 year post-exp. (n=29)	3.1	0.7	<0.001	1.7	4.6
2 years post-exp. (n=29)	5.1	1.1	<0.001	2.8	7.3
3 years post-exp. (n=29)	6.3	1.4	<0.001	3.5	9.2
4 years post-exp. (n=29)	7.8	1.8	<0.001	4.3	11.4
5 years post-exp. (n=26)	8.2	2.1	<0.001	3.9	12.5

[1]*Overall ATT calculated as weighted sum of group-specific ATTs weighted by share of treated units and treated periods; all estimates adjusted for heterogeneous linear trends and clustered standard errors at the state. States that implemented expansion in 2016 (Louisiana and Montana) and 2019 (Maine and Virginia) were excluded due to violations of parallel trends.*

In the hypothetical case when all states implemented expansion by 2019, the parallel trends assumption was only met after dropping the 2016 cohort and controlling for

heterogeneous linear trends of the 2014 and 2015 cohorts. Weighting these feasible group-time treatment effects by the scaled shares of units and time treated resulted in an overall average increase of 4.6 percentage points in insurance coverage after expansion (95% CI: 2.5–6.6). In contrast, the TWFE estimate is 3.9 percentage points (95% CI: 2.7–5.2), and the Goodman-Bacon decomposition reveals the forbidden comparisons contribute a -0.04 difference-in-difference estimate with a larger weight of 21.4% to bias the overall TWFE estimate towards the null.[55]

DISCUSSION

This study demonstrates that ETWFE outperformed TWFE in the case of staggered difference-in-differences regardless of how the group-time treatment effects were aggregated and secondly, that weighting the ETWFE group-time treatment effects by the scaled shares of units and time treated yielded an overall average treatment effect with lower bias and better coverage of 95% confidence intervals compared to separately weighting by shares of units or time alone. The empirical application of ETWFE to the staggered implementation of Medicaid expansion suggests expansion increased insurance coverage by 5.3 percentage points, which aligns with early estimates derived using TWFE (5.2 percentage points).[42] The TWFE estimate of the expansion effect in this study was only 1 percentage point higher than the ETWFE estimate. The low bias of the TWFE estimate in this case is likely attributable to the majority of states expanding in 2014—79% of all 33 states that expanded by 2019—and the exclusion of the 2016 and 2019 cohorts from the analysis due to violations of parallel trends; however, as more states expand Medicaid, thus creating additional cohorts of late-expansion states, the bias of the TWFE estimate of the effect of expansion on insurance coverage will likely increase. Comparing the difference in the absolute bias of the MC simulations of sample size 50, the absolute bias

increased once all units were treated, especially in the case of varied treatment shares (Figure 3). In terms of coverage, once all states implement Medicaid expansion, the confidence intervals of the TWFE expansion effect estimate will be much less likely to contain the true expansion effect. As the coverage results of the MC simulations of sample size 50 indicate in Figure 4, coverage degrades in the absence of untreated units, regardless of variation in treatment shares. Therefore, it is critical to adopt estimation approaches that can accommodate staggered policy adoption and provide unbiased treatment effects.

The conclusions of this study that ETWFE outperformed TWFE and that weighting the ETWFE group-time treatment effects by combined shared of units and time treated over other weighting approaches are not without limitations. First, the MC simulations in this study were based on simulated datasets generated from linear models and thus appropriately analyzed using OLS. Future research should compare the performance of ETWFE incorporating non-linear DGMs. Additionally, both the MC simulations and empirical application used balanced panel datasets. As pseudo-panel datasets with repeated cross-sections as opposed to repeated observations of the same units are often analyzed using difference-in-differences, additional research is needed to examine the performance of ETWFE in the pseudo-panel case, particularly in terms of identifying optimal average treatment effect weighting approaches. The findings in this study are also not generalizable to the case when treated units leave the treatment group before the end of the study period.[35,46] Given the impending end to the "maintenance of eligibility" policy allowing for continuity of Medicaid coverage during the COVID-19 pandemic, examining the performance of ETWFE in the case of early exits may aid future policy evaluation efforts. As eligibility redeterminations may result in the loss of Medicaid coverage, variation in policy unwinding may benefit from the use of the ETWFE to examine the effect of the end of

this policy.[58] Future research should also more thoroughly examine pre-Medicaid expansion differences between the cohorts of expansion states and non-expansions states. Although this simple example demonstrated ETWFE with two expansion cohorts, there may be alternative approaches satisfying the conditional parallel trends assumption, allowing the inclusion of the additional expansion cohorts in the overall expansion effect estimate. In conclusion, the bias of the TWFE approach encountered in difference-in-differences with variation in treatment timing is avoidable by implementing ETWFE, which allows estimation of average treatment effects by cohort, relative time, and overall, using regression-based tools familiar to health services researchers.

Chapter 3: Assessing the Impact of Removing the Prior Authorization Requirement for Buprenorphine on Prescribing Behaviors of Outpatient Opioid Use Disorder Treatment Providers Participating in Medicaid

INTRODUCTION

Medicaid is disproportionately affected by the opioid epidemic, insuring two in ten non-elderly adults in the United States but four in ten non-elderly adults diagnosed with opioid use disorder (OUD).[4] As overdose deaths contribute to recent declines in U.S. life expectancy and seven in ten overdose deaths are attributable to opioids, it is critical to identify policies that incentivize provision of evidence-based OUD treatment including medication for opioid use disorder (MOUD).[59] Buprenorphine is one of the three MOUD approved by the Food and Drug Administration (FDA) for treatment of OUD.[60] Buprenorphine is a partial opioid agonist considered to improve safety and reduce risk of misuse due to a "ceiling effect" on respiratory suppression.[61,62] Buprenorphine was the first MOUD approved by the FDA for use in an office-based setting under the Drug Addiction Treatment Act (DATA) of 2000; however, providers are required to complete additional training and receive a waiver from the Substance Abuse and Mental Health Service Administration (SAMHSA) in order to prescribe buprenorphine.[61,62]

Prior authorization (PA) requirements are a utilization management tool used by health insurers to reduce unnecessary utilization; however, requiring utilization review prior to approving a prescription for buprenorphine may add an administrative disincentive for providers and delay access to evidence-based treatment for patients seeking outpatient OUD treatment. A 2018 survey of DATA-waivered prescribers identified insurance requirements, including PA, as one of the most frequently cited barriers to prescribing buprenorphine.[63] Only limited time and perceived low patient demand were more commonly identified as barriers to prescribing buprenorphine, which means the burden posed by insurance PA is the most common regulatory

barrier to buprenorphine uptake.[63] A similar qualitative study of providers concluded that PA requirements can negatively impact patient care and patient motivation to remain in treatment when faced with delays in accessing medication.[64] Removal of the PA requirement for buprenorphine is one potential policy solution to increase access to evidence-based treatment for OUD.[23,65,66]

Virginia's ARTS benefit provides an opportunity to assess the impact of removing PA requirements for buprenorphine on outpatient OUD treatment, as providers credentialed as preferred Office-Based Outpatient Treatment (OBOT) providers were waived from the PA requirement for buprenorphine following implementation of ARTS on April 1, 2017.[67,68] Independent buprenorphine-waivered prescribers (BWP) were required to submit PA to prescribe buprenorphine until February 1, 2019, which allows a 22-month period where similar groups of providers were differentially affected by PA requirements for buprenorphine. The purpose of this study is to assess the impact of eliminating the PA requirement on buprenorphine prescribing of providers who were and were not subject to PA requirements for buprenorphine.

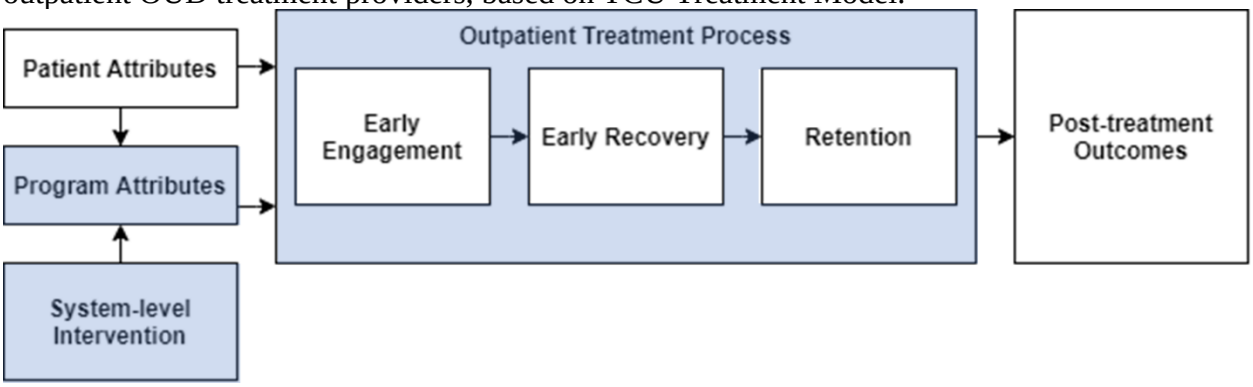
Conceptual Framework

The Texas Christian University (TCU) Treatment Model will be used to assess the impact of removing the PA for buprenorphine as a health system-level intervention affecting the downstream processes and outcomes of SUD treatment.[69] The TCU Treatment Model was developed to reflect the longitudinal nature of SUD treatment, where both patient and provider attributes jointly affect the development of a therapeutic relationship in early engagement, continued treatment program participation characterizes early recovery, and longer term retention in treatment reflects stabilized recovery. The TCU Treatment Model illustrates that

longer treatment engagement supports progression through the treatment process to stabilized recovery and subsequently more favorable post-treatment outcomes.

The removal of the PA for buprenorphine reflects a health system-level policy change theorized to affect provider prescribing behaviors by eliminating the time and effort required by the managed care organizations' utilization review process. Providers who are not subject to PA for buprenorphine are therefore more likely to include buprenorphine as a component of outpatient OUD treatment programs. Patients seeking OUD treatment with a provider not subject to the PA for buprenorphine are less likely to face potential delays when filling prescriptions due to untimely utilization review. The increase in access to buprenorphine thus facilitates continued pharmacotherapy during OUD treatment, which is associated with positive post-treatment outcomes, such as lower risk of overdose or acute care utilization.[29,70] The focus of this study is highlighted in blue in Figure 6, which is the structural change represented by the removal of the PA requirement for buprenorphine and its impact of prescribing behaviors as a reflection of the outpatient treatment process broadly defined.

Figure 6. Conceptual framework to assess the impact of PA removal on prescribing behaviors of outpatient OUD treatment providers, based on TCU Treatment Model.



Research Questions and Hypotheses

Q1. Did the elimination of the PA requirement for buprenorphine impact prescribing behaviors of outpatient OUD treatment providers participating in Medicaid?

H1. *Buprenorphine prescribing rates increased* following elimination of the PA requirement for buprenorphine among providers not subject to PA relative to providers required to submit PA for buprenorphine.

METHODS

Data Sources

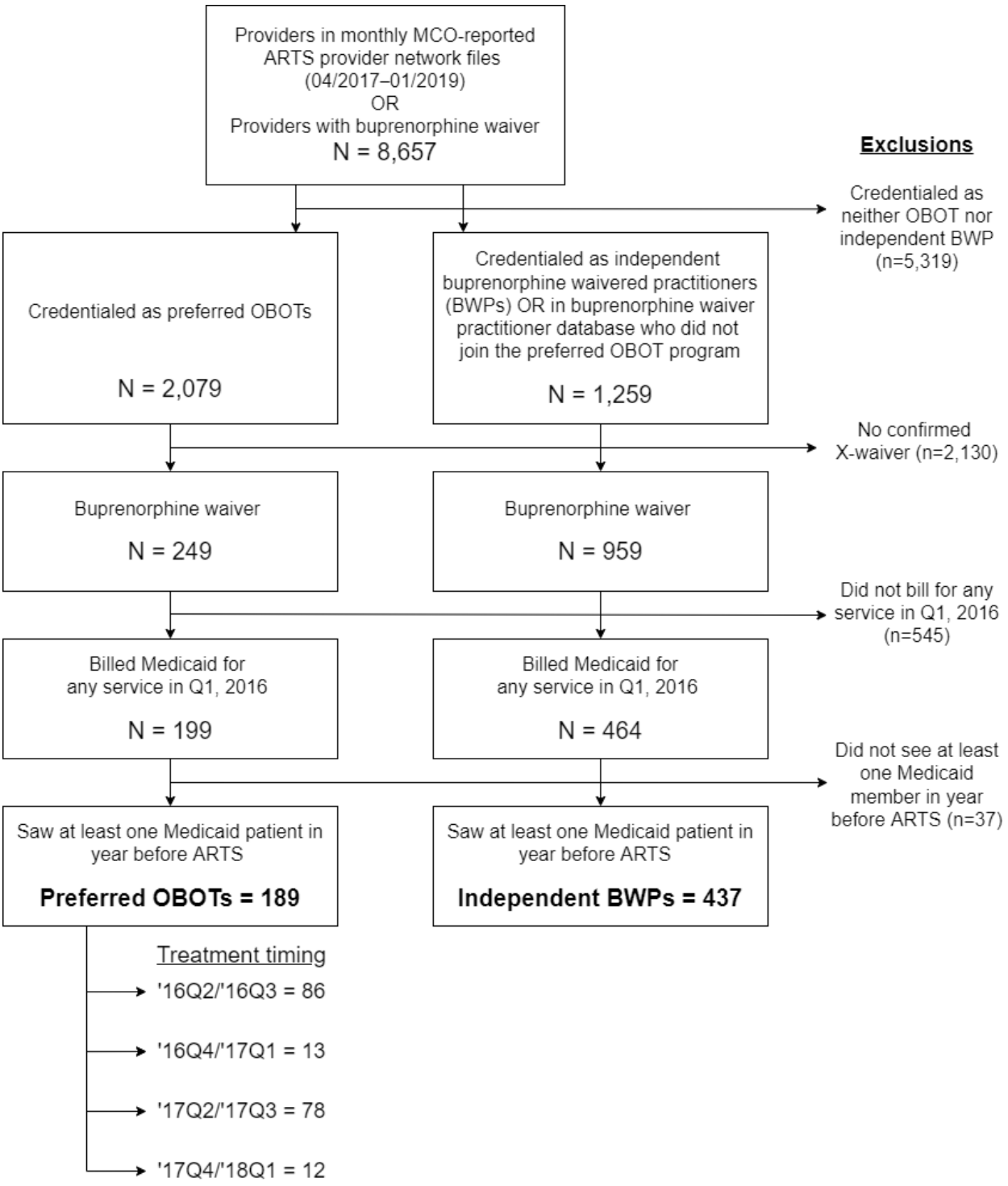
The data sources for this study included administrative claims databases and provider network files from the Virginia Department of Medical Assistance Services (DMAS) and a database of DATA waivered providers authorized to prescribe buprenorphine.[71] The administrative claims databases contain all claims adjudicated by DMAS and the six managed care organization (MCOs) contracted to provide managed care to the majority of Medicaid members. The provider network files are monthly extracts produced by the MCOs to report their full rosters of credentialed ARTS providers by level of care. The DATA waiver information was validated using the SAMHSA Buprenorphine Pharmacy Lookup tool.[72]

Study Design

A quasi-experimental, staggered difference-in-difference study design was used to assess the impact of eliminating the PA requirement for buprenorphine on prescribing behaviors of outpatient OUD treatment providers participating in Medicaid. The study period spanned April 1, 2016 through January 31, 2019, allowing one year before the earliest PA requirement removal, and 22 months of staggered PA requirement removal. The sample selection process is presented in Figure 7, where waivered, individual prescribers with established Medicaid participation were

identified from provider network files, administrative claims, and waiver databases. Providers participating in Medicaid were first identified from monthly MCO-reported ARTS provider network rosters. The treatment group included providers credentialled as preferred OBOTs on the provider network files between April 2017 through January 2019. The control group included those credentialed as independent BWPs on the provider network files or included in the waiver database, as long as they were not credentialed as a preferred OBOT at any point in the study period. As the focus of this study is on changes in prescribing rates of individual prescribers, those identified as preferred OBOTs or independent BWPs in the provider network files were excluded if their National Provider Identifier (NPI) was not in the buprenorphine waiver database. The validation of each provider's waiver ensures the sample excludes individuals who were unable to prescribe buprenorphine—such as care coordinators or behavioral health specialists—and practice-level NPIs where buprenorphine prescriptions could not be attributable to a single prescriber. Inclusion required treatment and control providers to have established Medicaid participation as evidenced by billing Medicaid for any service in the quarter preceding the beginning of the study period and seeing at least one member in the year prior to the earliest PA removal and launch of the ARTS benefit. The control group was untreated for the duration of the study period as the PA requirement for independent BWPs was only removed after the end of the study period.

Figure 7. Sample selection of treatment and control providers participating in Medicaid.



A panel dataset was constructed from administrative claims databases in six-month intervals at the provider level; therefore, the unit of analysis for this study is the provider-half-year. Paid claims for buprenorphine were included for all providers who met the sample inclusion criteria if they were billed as the prescribing provider for pharmacy claims and the servicing provider for office-based medication administration claims. Buprenorphine was identified from the National Drug Code's (NDC) therapeutic class, which is the first three digits of the Hierarchical Ingredient Code (HIC3) in combination with the Generic code number (GCN), where buprenorphine was identified as an HIC3 of "H3W" combined with a GCN sequence number included in the following list: 29312, 29313, 51640, 51641, 66635, 66636, 70259, 70262, 72449, 72450, 72451, 73424, 73425, 76981.

A primary concern of observational research is the selection bias inherent in the lack of randomized exposure.[40,41] In this study, providers chose whether to pursue credentialling as preferred OBOTs and members chose where to seek treatment, which means unobserved provider and member characteristics may impact selection into the treatment group. Selection bias was addressed through the use of a quasi-experimental study design to control for confounding due to unobserved, time-invariant provider characteristics and propensity score weighting to balance on observed provider characteristics associated with participation in the preferred OBOT program.

Dependent Variable

The dependent variable for this study was the daily buprenorphine prescribing rate, which was based on the total number of days covered for all members with buprenorphine prescriptions per provider. Daily possession of buprenorphine was identified based on each claim's date of service and the days' supply on pharmacy claims, where members with overlapping fills were

allowed to accumulate a surplus supply on a rolling basis. Office-administered buprenorphine possession was based only on the claim's date of service. Each day of buprenorphine possession was then attributed to the responsible provider—the prescribing provider for pharmacy claims and servicing provider for office-based medication administration claims. The total member-days of buprenorphine possession attributable to each provider was then aggregated in half-year intervals and divided by the total number of days per time period as each interval did not contain the same number of days. All half-years contained 183 days except for the six month period beginning October 1, 2017, which contained 182 days and the six month period beginning October 1, 2018, which was truncated and only contained 123 days. Therefore, this outcome measure can be interpreted as the average daily number of members with buprenorphine possession. For example, 300 member-days of buprenorphine possession in a half-year of 183 days equates to an average of 1.6 covered members per day during that interval.

Independent Variables

Attributes of both providers rendering outpatient OUD treatment services and patients seeking treatment will be included as independent variables. The primary independent variable was an indicator of whether a provider was credentialed as a preferred OBOT and thus not subject to the PA requirement for buprenorphine. Each provider's DATA waiver prescribing limit was identified to account for variation in potential patient volume and time with DATA waiver.[63] A provider role variable was defined using standard claims billing codes from the National Uniform Claim Committee (NUCC) Health Care Provider Taxonomy Code Set to distinguish between providers who are Doctors of Medicine and Doctors of Osteopathic Medicine (MD/DO) from Nurse Practitioners and Physician Assistants (NP/PA).[71,73] The NUCC taxonomy classification was included to account for differences in provider taxonomy. Lastly, an

indicator was defined to identify early-adopters who prescribed buprenorphine in the quarter prior to the study period, as these providers adopted evidenced-based MOUD treatment for their patients with OUD prior to the Virginia General Assembly legislation that approved the ARTS benefit.[74]

Demographic and health characteristics of patients who received buprenorphine during each time period were included as percentages of patients seen during each six month interval to account for variation in provider case-mix. Demographic attributes of patients included sex; age as of the first day of each time period; race/ethnicity; and urbanization of residential address, all sourced from enrollment files.[29,30,75] The level of urbanization was based on the patients' residential zip codes and mapped to Rural-Urban Commuting Area (RUCA) codes, where higher values are indicative of higher degrees of rurality.[76] A zip code was considered urban if it mapped to the top three metropolitan RUCA codes. An indicator of prior mental health diagnosis included all Medicaid-billed diagnoses from the year prior to treatment, where non-substance use related, single-level mental health diagnoses were included from the Healthcare Cost and Utilization Project's Clinical Classification Software.[77]

Empirical Approach

Differences between preferred OBOTs and independent BWPs in the year prior to the preferred OBOT program were summarized with descriptive statistics and tested using Wald chi-square tests clustering the standard error at the provider. Staggered difference-in-differences was used to estimate the impact of eliminating the PA requirement for buprenorphine on prescribing behaviors of outpatient OUD treatment providers participating in Medicaid. As providers were credentialed as preferred OBOTs at different time points during the study period, extended two-way fixed-effects (ETWFE) was used to estimate the group-time treatment effects and

aggregated to overall, cohort-specific, and event-study average treatment effects.[35,46] The ETWFE model was estimated using pooled ordinary least squares for preferred OBOT cohort *d*, who was initially credentialed in time period *g*, each post-credentialing period $f_{t \geq g}$, with fixed effects for cohort and time:

$$prescribing\ rate = \left[d_g \times f_{t \geq g}\right] + d_g + f_{t>1} \quad (1)$$

Standard errors were clustered at the provider level in accordance with the level of the treatment. The interaction terms between each cohort indicator and each of their post-credentialing half-year dummies are the group-time treatment effects, providing a specific treatment effect for each cohort on and after their first six-month interval as a preferred OBOT. Linear combinations of these group-time treatment effects were then used to estimate overall, cohort-specific, and event-study average treatment effects. The parallel trends assumption was empirically assessed by including heterogeneous linear trends and testing whether these trends were significantly different from zero, where parallel trends was supported by an F-test that failed to reject the null hypothesis of parallel trends conditional on heterogeneous linear trends.

Sensitivity Analysis

Several sensitivity analyses were used to assess the robustness of the results. First, a small number of preferred OBOTs who exited the program early were dropped from the analysis (n=9). Although ETWFE can account for early exit from a treatment program, the nine providers who exited the OBOT program early would have represented four very small treatment cohorts if retained. For example, three of the exiting providers were each the only provider to exit early in a half-year interval. The second sensitivity analysis stratified the model by providers who were early prescribers to compare how treatment effects differed for those prescribing buprenorphine before versus after the passage of the legislation approving the ARTS benefit.[74] Lastly, a doubly-

robust, inverse probability weighting approach was used to balance the treated and control groups on provider characteristics associated with OBOT program participation and patients characteristics associated with seeking care with an OBOT.[48,78] The propensity score model included provider waiver limit, taxonomy, indicator identifying early prescribers of buprenorphine, and patient demographic and health characteristics.

RESULTS

Pre-treatment differences between preferred OBOTs and independent BWPs are summarized in Table 5, where the provider groups saw a comparable mean number of Medicaid members over the two, six-month periods prior to the implementation of the ARTS benefit and preferred OBOT program. In the year before ARTS, 34% of independent BWPs and 60% of preferred OBOTs saw any Medicaid members diagnosed with OUD, seeing a mean of 2.7 and 5.2 members with OUD, respectively. Preferred OBOTs and independent BWPs differed in terms of baseline levels of buprenorphine prescribing, with 32% of preferred OBOTs and 14% of independent BWPs prescribing any buprenorphine during the pre-ARTS period. The preferred OBOTs buprenorphine prescribing rate covered an average of 1.8 members per day compared to 0.8 members per day among independent BWPs during the pre-ARTS period.

Table 5. Characteristics of preferred Office-Based Opioid Treatment providers (OBOTs) and independent buprenorphine waiver providers (BWPs) in the two half-years prior to ARTS.

	Preferred OBOTs (n = 378)		Independent BWPs (n = 874)		*p*-value[1]
Provider Characteristics					
Providers who saw any Medicaid members, n (%)	373	(98.7)	846	(96.8)	0.063
Mean number of members (SD)	114.7	(147.5)	117.4	(146.8)	0.828
Providers who saw any Medicaid members diagnosed with OUD, n (%)	225	(59.5)	293	(33.5)	<0.001
Mean number of members (SD)	5.2	(10.2)	2.7	(10.5)	0.004
Providers who prescribed or administered buprenorphine, n (%)	120	(31.8)	120	(13.7)	<0.001
Mean number of members (SD)	3.6	(8.9)	1.8	(8.7)	0.018
Mean number of buprenorphine-covered days (SD)	332.8	(924.3)	141.3	(681.4)	0.010
Mean buprenorphine-covered members per day (SD)	1.8	(5.1)	0.8	(3.7)	0.010
Role, n (%)					0.341
MD/DO	346	(91.5)	778	(89.0)	
NP/PA	32	(8.5)	96	(11.0)	
Taxonomy classification, n (%)					<0.001
Psychiatry & Neurology	194	(51.3)	236	(27.0)	
Family Medicine	36	(9.5)	160	(18.3)	
Internal Medicine	54	(14.3)	126	(14.4)	
Emergency Medicine	16	(4.2)	106	(12.1)	
NP/PA	32	(8.5)	96	(11.0)	
Obstetrics & Gynecology	26	(6.9)	36	(4.1)	
Other MD/DO	20	(5.3)	114	(13.0)	
Waiver limit, n (%)					<0.001
30	196	(51.8)	640	(73.2)	
100	114	(30.2)	164	(18.8)	
275	68	(18.0)	70	(8.0)	
Patient Characteristics, mean (SD)					
% Female	0.25	(0.39)	0.10	(0.28)	<0.001
% Non-Hispanic white	0.29	(0.44)	0.12	(0.32)	<0.001
% Age LTE 29	0.09	(0.21)	0.04	(0.14)	<0.001
% Age 30–49	0.21	(0.35)	0.09	(0.26)	<0.001
% Age GTE 50	0.02	(0.06)	0.01	(0.05)	0.024
% Urban residential zip code	0.23	(0.38)	0.09	(0.27)	<0.001
% Prior mental health diagnosis	0.24	(0.38)	0.11	(0.28)	<0.001

[1]p-values from Wald chi-square tests and F-tests with standard errors clustered at the provider level; cluster size for preferred OBOTs = 189, independent BWPs = 437.

Preferred OBOTs and independent BWPs also differed in taxonomy classification and waiver limit. The provider groups had similar distributions of clinical roles, with physicians comprising roughly 90% of each group of prescribers. Preferred OBOTs were more likely to have a taxonomy classification in psychiatry and neurology (51%) and obstetrics and gynecology (7%) compared to independent BWPs (27% and 4%, respectively). Independent BWPs were more likely from family medicine (18% compared to 9% among OBOTs) and emergency medicine (12% compared to 4%). The distributions of waiver limits skewed higher among preferred OBOTs, where nearly half of OBOTs had waiver limits over 30 patients (48%). Roughly one in four independent BWPs had a waiver limit over 30 patients in the pre-treatment period (27%).

Four cohorts of providers opted into the preferred OBOT program in the four half-year intervals after the implementation of the ARTS benefit (Appendix Table 17). Nearly half of providers were treated in the first six months of ARTS (46%). Thirteen preferred OBOTs were treated in the second six month interval (7%), and 94% of preferred OBOTs joined the program by the third interval. The final treated cohort was small, with only 12 providers (6%) joining the OBOT program in the fourth treated period.

The staggered difference-in-difference, ETWFE treatment effects of the impact of the PA removal on the buprenorphine prescribing rate are reported in Table 6. The parallel trends assumption was met conditional on heterogeneous linear trends. Overall, preferred OBOTs increased their buprenorphine prescribing by an average of 3.5 members per day compared to independent BWPs (95% CI: 2.2–4.8). Three of the four OBOT cohorts significantly increased their prescribing rate after joining the preferred OBOT program compared to independent BWPs, with the first treated cohort increasing by 3.7 members per day (95% CI: 1.8–5.7), the third

treated cohort increased by 2.8 members per day (95% CI: 1.5–4.1), and the final treated cohort increasing by 0.7 members per day (95% CI: 0.2–1.3) compared to the buprenorphine prescribing rate among independent BWPs.

Table 6. ATTs of the preferred OBOT program on buprenorphine prescribing rates.

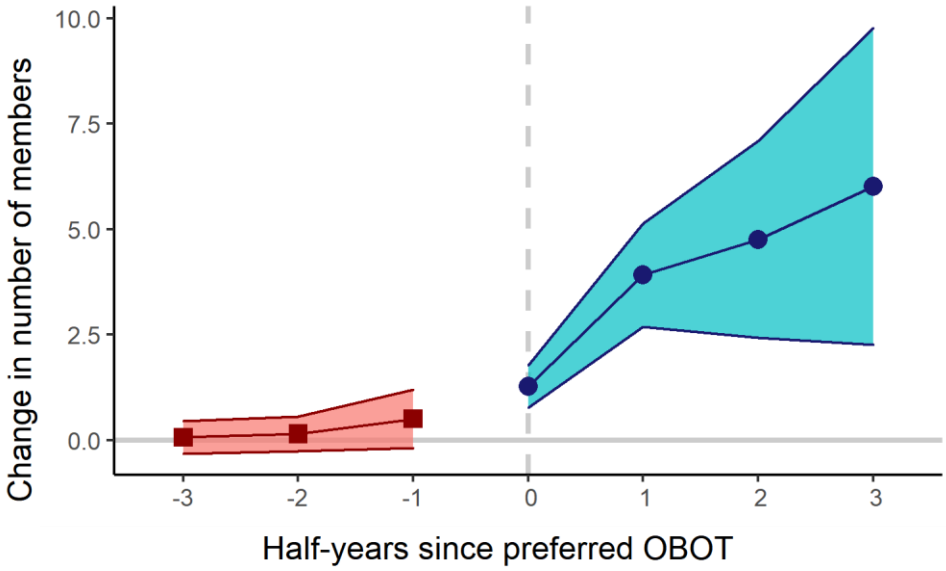
Buprenorphine-covered members per day	Coef.	SE	*p*-value	Pre-trends *p*-value
Group-time ATTs				0.1980
Cohort 17Q2/17Q3 (n=86)				
17Q2/17Q3	0.660	0.320	0.040	
17Q4/18Q1	3.697	0.862	<0.001	
18Q2/18Q3	4.505	1.239	<0.001	
18Q4/19Q1	6.020	1.918	0.002	
Cohort 17Q4/18Q1 (n=13)				
17Q4/18Q1	2.576	1.146	0.025	
18Q2/18Q3	6.364	3.771	0.092	
18Q4/19Q1	6.435	3.992	0.107	
Cohort 18Q2/18Q3 (n=78)				
18Q2/18Q3	1.823	0.474	<0.001	
18Q4/19Q1	3.747	0.887	<0.001	
Cohort 18Q4/19Q1 (n=12)				
18Q4/19Q1	0.745	0.302	0.014	
Overall ATT[1](n=189)	3.490	0.679	<0.001	0.1980
Group-specific ATTs[1]				0.1980
Cohort 17Q2/17Q3 (n=86)	3.720	0.997	<0.001	
Cohort 17Q4/18Q1 (n=13)	5.125	2.939	0.082	
Cohort 18Q2/18Q3 (n=78)	2.785	0.662	<0.001	
Cohort 18Q4/19Q1 (n=12)	0.745	0.302	0.014	
Event-time ATTs[1]				0.1980
-3 1.5 years before treatment (n=12)	0.066	0.196	0.737	
-2 1 year before treatment (n=90)	0.152	0.210	0.469	
-1 Six months before treatment (n=103)	0.503	0.351	0.153	
0 Treatment (n=189)	1.277	0.256	<0.001	
1 Six months after treatment (n=177)	3.915	0.626	<0.001	
2 1 year after treatment (n=99)	4.758	1.189	<0.001	
3 1.5 years after treatment (n=86)	6.019	1.918	0.002	

[1]*Overall ATT calculated as weighted sum of group-specific ATTs weighted by share of treated units and treated periods; group-specific ATTs calculated as average of group-time ATTs by cohort; event-time ATTs calculated as weighted sum of group-time ATTs weighted by share of treated units; all estimates adjusted for heterogeneous linear trends and cluster standard errors at the provider.*

To illustrate the dynamic effects, Figure 8 illustrates the event-study change in buprenorphine prescribing among preferred OBOTs relative to the independent BWPs. In the initial credentialing period, preferred OBOTs increased their prescribing by an average of 1.3 members

per day (95% CI: 0.8–1.8) and tripled to 3.9 members per day in the following six months (95% CI: 2.7–5.1) compared to independent BWPs. The trend in prescribing started to level out after one year, with preferred OBOTs increasing their prescribing rate to 4.8 members per day (95% CI: 2.4–7.1) and finally, 6.0 members per day (95% CI: 2.3–9.8) after 1.5 years as preferred OBOTs.

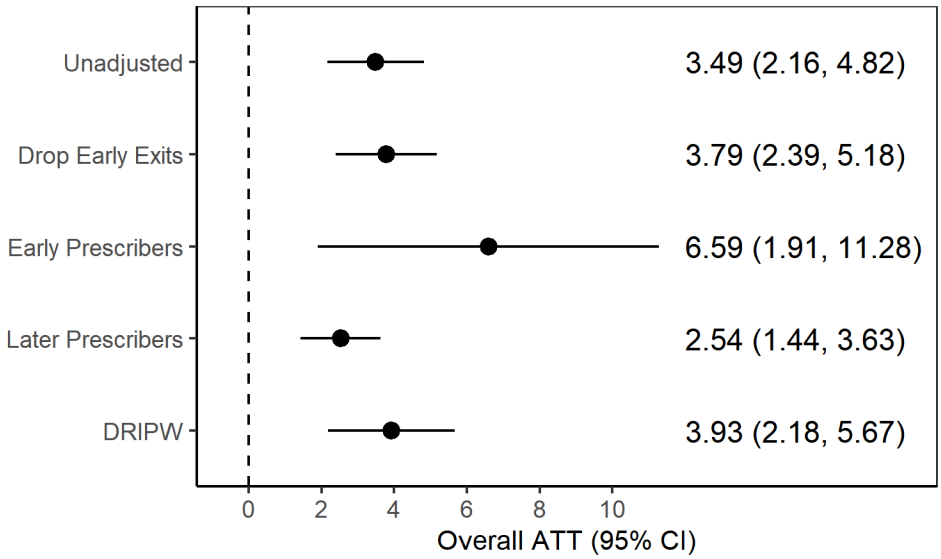
Figure 8. Event-study ATTs of change in buprenorphine prescribing rates among preferred OBOTs compared to independent BWPs in time relative to initial preferred OBOT credentialing.



The main results are robust to each of the three sensitivity tests, the results of which are displayed graphically in Figure 9 and in tabular format in Appendix Tables 18–20. Dropping the nine providers who exited the preferred OBOT program early slightly increased the point estimate of the change in the buprenorphine prescribing rate of preferred OBOTs relative to independent BWPs to 3.8 members per day (95% CI: 2.4–5.2). Stratifying the model by earliest buprenorphine prescription date revealed those who prescribed buprenorphine more than one year prior to the ARTS benefit increased their average prescribing rate by nearly three times as many members per day (6.6; 95% CI: 1.8–11.3) compared to those who were not early adopters

(2.5; 95% CI: 1.4–3.6). Lastly, the doubly-robust, inverse probability weighted treatment effects were also slightly larger than the main results, with preferred OBOTs increasing their prescribing rate by an average of 3.9 members per day (95% CI: 2.2–5.7) compared to independent BWPs. Despite the differences in point estimates, the overlapping 95% confidence intervals of all three sensitivity tests indicate these differences are not statistically different from the main results.

Figure 9. Comparison of overall ATTs of the preferred OBOT program on buprenorphine prescribing rates for the main results (unadjusted) and three sensitivity analyses.



Notes: The main results are labeled as "unadjusted" and reported in Table 6; results dropping the nine providers who exited the preferred OBOT program early are labeled "drop early exits" and reported in Appendix Table 18; results stratifying by earliest buprenorphine prescription date are labeled "early prescribers" and "later prescribers" and reported in Appendix Table 19; and doubly-robust, inverse probability weighted results accounting for observed provider and patient case-mix are labeled "DRIPW" and reported in Appendix Table 20.

DISCUSSION

The results of this study find that removing the PA requirement for buprenorphine is associated with a significant increase in buprenorphine prescribing rates among waivered prescribers participating in Medicaid, where preferred OBOTs increased prescribing to cover an average of 3.5 additional members per day after the PA removal compared to providers still

subject to PA requirements. Early in the preferred OBOT program, a prior qualitative evaluation of provider experiences identified frustration with delays of credentialing and reimbursement, where providers were reportedly still required by the MCOs to submit PAs for buprenorphine after credentialing.[79,80] In the current study, early-adopters who were credentialled in April 2017 only increased their prescribing rates by 0.7 members per day during their first six-months as preferred OBOTs. In contrast, providers who were credentialled in the second year of the program beginning in April 2018 increased their prescribing rates by an average of 1.8 members per day. After one year in the program, both cohorts of preferred OBOTs prescribed buprenorphine to an average of 3.7 members per day. These differences in initial prescribing rates of the two largest cohorts of preferred OBOTs suggest the implementation challenges faced by early-adopters may have been resolved by the second year of the program.

Although prescribing rates increased after the removal of the PA, this study has several limitations that preclude concluding that the PA removal is the only mechanism driving the increase. This study focused on the change in prescribing rates among those able to prescribe buprenorphine; however, other components of the clinic and care team surrounding the prescribers are not included in this study. The preferred OBOT model of care also includes care coordinators and behavioral health specialists, and these wrap-around services are likely to also positively affect treatment initiation and retention.[23,67] The Medicaid MCOs also serve as a referral source that may affect demand for preferred OBOT services by steering their Medicaid enrollees with OUD to preferred OBOTs for treatment services; however, all waivered prescribers can opt into the publicly available on the SAMHSA Buprenorphine Practitioner Locator.[81] Lastly, over the duration of the preferred OBOT program, participating providers developed an informal learning collaborative by supporting each other and sharing best practices.

Over the course of this study, preferred OBOTs increased their prescribing rates from an average of 1.3 members per day during the first six months of credentialing to 6.0 members per day after 1.5 years in the program. This positive trajectory of improved buprenorphine access over time may be attributable not only to the experience gained by individual providers but also the benefits of sharing their experiences with each other. This finding aligns with a previous survey of attitudes toward different approaches to increasing prescribing capacity, where partnering with an experienced provider was the most endorsed way to encourage waiver uptake among non-waivered providers (45.7%) and the second-most endorsed among under-prescribing waivered providers (29.7%).[82] Future research on the impact of Medicaid policy on buprenorphine access should assess the role of the full care team involved in patient care and account for the referral pathways of patients initiating treatment or complementing MOUD with other supportive components of comprehensive care.

Conclusion

The ongoing challenges of the opioid crisis and the varied approaches that Medicaid programs are using to address the crisis need evidence of what policies are driving positive outcomes and alleviating barriers to improved access to care. These studies provide evidence using quasi-experimental designs that changes to the Medicaid-covered SUD benefits included in Virginia's ARTS benefit have improved access to care across multiple levels of the healthcare system. First, using nationally representative survey data of the roughly 17,000 treatment facilities in the U.S., the percentage of facilities accepting Medicaid as payment for treatment services significantly increased in Virginia after the implementation of the ARTS benefit compared to non-expansion states without a similar enhancement in SUD benefits. Facilities in Virginia increased Medicaid acceptance by 8.0 percentage points in the year after ARTS from a baseline average of 60.3 percent of facilities. Although prior research has not found an association between Medicaid expansion and SUD treatment facility acceptance of Medicaid, the percentage of facilities accepting Medicaid in Virginia after expansion in 2019 continued to increase to 11 percentage point increase over baseline.[31] This represents a significant increase in access to care for Medicaid members with SUD, especially as Medicaid expansion enrollees are estimated to have higher rates of SUD compared to members enrolling due to other eligibility.[21]

Although the ARTS benefit was implemented on April 1, 2017, the preferred OBOT model of care was not adopted by providers in Virginia at a uniform time. Variation in timing of policy implementation has historically used a generalized difference-in-difference design and estimated with TWFE; however, recent economic literature identified TWFE produces biased results.[7,44] ETWFE outperformed TWFE across 54 DGMs varying in sample size, treatment timing, treatment effects, and comparison groups, and weighting ETWFE estimates by the scaled share of units and time treated outperforms weighting by either units or time alone. The

simulations and empirical application included in this study illustrate the bias of the TWFE-based effect of Medicaid expansion on insurance coverage will likely increase as more states implement Medicaid expansion, necessitating adoption of unbiased estimation approaches like ETWFE. The final study applied EWTFE to a component of the preferred OBOT model of care to assess whether the removal of the PA for buprenorphine alleviated a barrier to MOUD treatment by increasing prescribing rates. The results of this study suggest that removing the PA requirement for buprenorphine is associated with a significant increase in buprenorphine prescribing rates among waivered prescribers participating in Medicaid, where preferred OBOTs increased prescribing to cover an average of 3.5 additional members per day after the PA removal compared to providers still subject to PA requirements. Although Virginia Medicaid has largely removed PA requirements for prescribing buprenorphine over the five years since the implementation of ARTS, these findings may be beneficial for policymakers in other states who may be considering alternative ways to improve access to care and incentivize the provision of evidence-based treatment for OUD.[8]

Policymakers and stakeholders require strong evidence of the impact of healthcare policy changes to understand and continue investing in programs that are the drivers of favorable outcomes. Although an ongoing area of methodological research, future research on the impact of Medicaid behavioral health policy and its role in increasing access to care should tease apart broad policies like ARTS to understand the impact of smaller components—such as the PA removal through the preferred OBOT model of care. Additional research is needed to also parse the impact of overlapping policies like ARTS and Medicaid expansion, especially given the increase in provider acceptance identified after ARTS during the first year of expansion.

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APPENDIX

Table 7. Compare time-varying effects of covariates in the pre-ARTS period, 2013–2016.

Characteristics	H_0: The effect of X on the dependent variable is constant over time.		H_0: The difference in $\bar{X}$ between Virginia and control states is constant over time.	
	Test statistic[1]	p-value	Test statistic[1]	p-value
Facility-level characteristics				
Accepted payment types				
Medicaid	--	--	--	--
Private	5.3	0.149	41.2	<0.001
Other Public	13.2	0.004	53.8	<0.001
Self-pay	0.9	0.811	0.7	0.881
Charity care	20.6	<0.001	59.8	<0.001
Ownership				
Private, for-profit	6.7	0.084	59.4	<0.001
Private, non-profit	2.1	0.553	123.5	<0.001
Government	8.1	0.044	3.3	0.343
SUD treatment services offered				
Any outpatient	8.1	0.044	142.7	<0.001
Any residential	1.6	0.668	191.0	<0.001
Any hospital inpatient	6.0	0.113	32.7	<0.001
Any MOUD	12.4	0.006	37.3	<0.001
Number of MOUD	4.8	0.189	73.4	<0.001
MOUD types offered				
Buprenorphine with naloxone	4.4	0.222	81.9	<0.001
Buprenorphine without naloxone	2.3	0.510	88.6	<0.001
Methadone	8.6	0.034	61.0	<0.001
Naltrexone	3.1	0.370	51.2	<0.001
State-level characteristics				
Male	6.8	0.079	8.8	0.033
Age GTE 65	9.1	0.029	8.5	0.037
Race/ethnicity				
Non-Hispanic White	21.0	<0.001	20.4	<0.001
Non-Hispanic Black	14.4	0.002	4.4	0.217
Hispanic	19.4	<0.001	8.6	0.035
Non-Hispanic Other	7.2	0.067	19.0	<0.001
Urbanicity	13.6	0.003	16.6	<0.001
Poverty rate	2.4	0.500	39.0	<0.001
Unemployment rate	13.7	0.003	2.6	0.467
Educational attainment less than high school	6.3	0.099	4.3	0.233
Age-adjusted drug overdose death rate	5.6	0.135	5.9	0.115

[1]*p-values from Wald chi-square tests and F-tests with standard errors clustered at the state level*

Table 8. Summary of difference-in-difference time-varying confounding criteria in the pre-ARTS period, 2013–2016.

Characteristics	(1) $\bar{X}$ same in Tx and control	(2) Constant effect of X on Y over time	(3) Constant difference in $\bar{X}$ between Tx and control over time	Required regression adjustment
Facility-level characteristics				
Accepted payment types				
Private	☐	☒	☐	X
Other Public	☐	☐	☐	X·Year
Self-pay	☐	☒	☒	None
Charity care	☒	☐	☐	X·Year
Ownership				
Private, for-profit	☐	☒	☐	X
Private, non-profit	☐	☒	☐	X
Government	☐	☐	☒	X·Year
SUD treatment services offered				
Any outpatient	☒	☐	☐	X·Year
Any residential	☐	☒	☐	X
Any hospital inpatient	☒	☒	☐	None
Any MOUD	☐	☐	☐	X·Year
Number of MOUD	☐	☒	☐	X
MOUD offered				
Buprenorphine w/ naloxone	☐	☒	☐	X
Buprenorphine w/o naloxone	☐	☒	☐	X
Methadone	☒	☐	☐	X·Year
Naltrexone	☐	☒	☐	X
State-level characteristics				
Male	☒	☒	☐	None
Age GTE 65	☒	☐	☐	X·Year
Race/ethnicity				
Non-Hispanic White	☒	☐	☐	X·Year
Non-Hispanic Black	☒	☐	☒	X·Year
Hispanic	☐	☐	☐	X·Year
Non-Hispanic Other	☒	☒	☐	X
Urbanicity	☒	☐	☐	X·Year
Poverty rate	☐	☒	☐	X
Unemployment rate	☐	☐	☒	X·Year
Educational attainment less than high school	☐	☒	☒	None
Age-adjusted drug overdose death rate	☒	☒	☒	None

[1]*Time-varying confounders (TVC) have means that are not parallel over time or have a time-varying effect on Y*

Table 9. Criteria to determine appropriate regression adjustment strategy to estimate ATT of ARTS effect on Medicaid acceptance.

Criteria #1	Evidence	Criteria #2	Evidence	Criteria #3	Evidence	Regression Adjustment
Is X time-varying?	NO = AT7(4) p≥0.05 (or AT8(3) checked)			Is effect of X on Y time-varying?	YES = AT7(2) p<0.05 (AT8(2) unchecked)	X·Year
					NO = AT7(2) p≥0.05 (AT8(2) checked)	None
	YES = AT7(4) p<0.05 (or AT8(3) unchecked)	Is X associated with treatment?	YES = T1(5) p<0.05 (AT8(1) unchecked)	Is effect of X on Y time-varying?	YES = AT7(2) p<0.05 (AT8(2) unchecked)	X·Year
					NO = AT7(2) p≥0.05 (AT8(2) checked)	X
			NO = T1(5) p≥0.05 (AT8(1) checked)	Is effect of X on Y time-varying?	YES = AT7(2) p<0.05 (AT8(2) unchecked)	X·Year
					NO = AT7(2) p≥0.05 (AT8(2) checked)	None

Note: Decision tree based on Zeldow and Hatfield (2021). Table names abbreviated by number and column (e.g., AT7(4) indicates Appendix Table 7, column 4)

Table 10. Number of substance use treatment facilities in the sample, overall and by treatment context.

	Sample	2013	2014	2015	2016	2017	2018	2019
All facilities	VA	224	221	228	224	217	226	250
	Control	3,123	3,187	3,120	3,259	3,019	3,302	3,546
Treatment context[1]								
Outpatient	VA	188	189	192	198	188	199	219
	Control	2,617	2,659	2,604	2,750	2,559	2,808	3,012
OTP	VA	28	26	31	33	29	36	38
	Control	356	371	369	370	310	396	465
Languages other than English	VA	85	88	100	102	106	113	124
	Control	1,252	1,430	1,458	1,607	1,536	1,732	1,955

[1]OTP = opioid treatment program

Table 11. ATTs of ARTS on facility acceptance of Medicaid as payment for substance use treatment by treatment context.

Treatment context	Time-varying ATTs			(1) State and year fixed effects		(2) M1 + poverty rate		(3) M2 + % BIPOC		(4) M3 + for-profit ownership	
	2017	2018	2019	PT[1]	NA[1]	PT	NA	PT	NA	PT	NA
Outpatient	0.032 (0.012)	0.093 (0.007)	0.130 (0.009)	**0.061**	**0.128**	0.005	0.017	0.018	0.363	0.023	0.648
OTP	0.209 (0.016)	0.307 (0.041)	0.419 (0.039)	**0.067**	**0.442**	0.045	0.554	0.001	0.001	0.003	0.006
Languages other than English	0.085 (0.008)	0.127 (0.013)	0.128 (0.014)	0.005	0.315	0.002	0.656	0.004	0.352	**0.084**	**0.208**

[1]PT = Pre-trends p-value, NA = no anticipation p-value, OTP = opioid treatment program

Figure 10. Timeline of 1115 SUD waiver and/or Medicaid expansion implementation by state.

State	Waiver Begin	Waiver End	Expansion Begin
Utah	7/1/2002	6/30/2022	1/1/2020
California	9/1/2005	12/31/2021	1/1/2014
Delaware	1/1/1996	12/31/2023	1/1/2014
Maryland	7/1/1997	12/31/2021	1/1/2014
Massachusetts	7/1/1997	6/30/2022	1/1/2014
Rhode Island	7/1/2009	12/31/2023	1/1/2014
Vermont	10/1/2005	12/31/2021	1/1/2014
Michigan	1/1/2010	12/31/2023	4/1/2014
Kansas	1/1/2013	12/31/2023	
New Jersey	10/2/2012	6/30/2022	1/1/2014
Wisconsin	1/1/2014	12/31/2023	
Minnesota	10/18/2013	1/31/2025	1/1/2014
Washington	1/9/2017	12/31/2021	1/1/2014
West Virginia	1/1/2018	12/31/2022	1/1/2014
Illinois	7/1/2018	12/31/2025	1/1/2014
New Mexico	1/1/2019	12/31/2023	1/1/2014
Kentucky	4/1/2019	9/30/2023	1/1/2014
Ohio	3/15/2019	2/29/2024	1/1/2014
Arizona			1/1/2014
Arkansas			1/1/2014
Colorado	1/1/2021	12/31/2025	1/1/2014
Connecticut			1/1/2014
DC	1/1/2020	12/31/2024	1/1/2014
Hawaii			1/1/2014
Iowa			1/1/2014
Nevada			1/1/2014
New York			1/1/2014
North Dakota			1/1/2014
Oregon	4/8/2021	3/31/2026	1/1/2014
New Hampshire	7/10/2018	6/30/2023	8/15/2014
Pennsylvania	10/1/2017	9/30/2022	1/1/2015
Indiana	2/1/2015	12/31/2030	2/1/2015
Alaska	1/1/2019	12/31/2023	9/1/2015
Montana			1/1/2016
Louisiana	2/1/2018	12/31/2022	7/1/2016
Virginia	**4/1/2017**	**12/31/2024**	**1/1/2019**
Maine	1/2/2021	12/31/2025	1/10/2019
Nebraska	7/1/2019	6/30/2024	
North Carolina	11/1/2019	10/31/2024	
Mississippi			
Tennessee			
Idaho	4/17/2020	3/31/2025	1/1/2020
Oklahoma	12/22/2020	12/31/2025	
Alabama			
Florida			
Georgia			
Missouri			
South Carolina			
South Dakota			
Texas			
Wyoming			

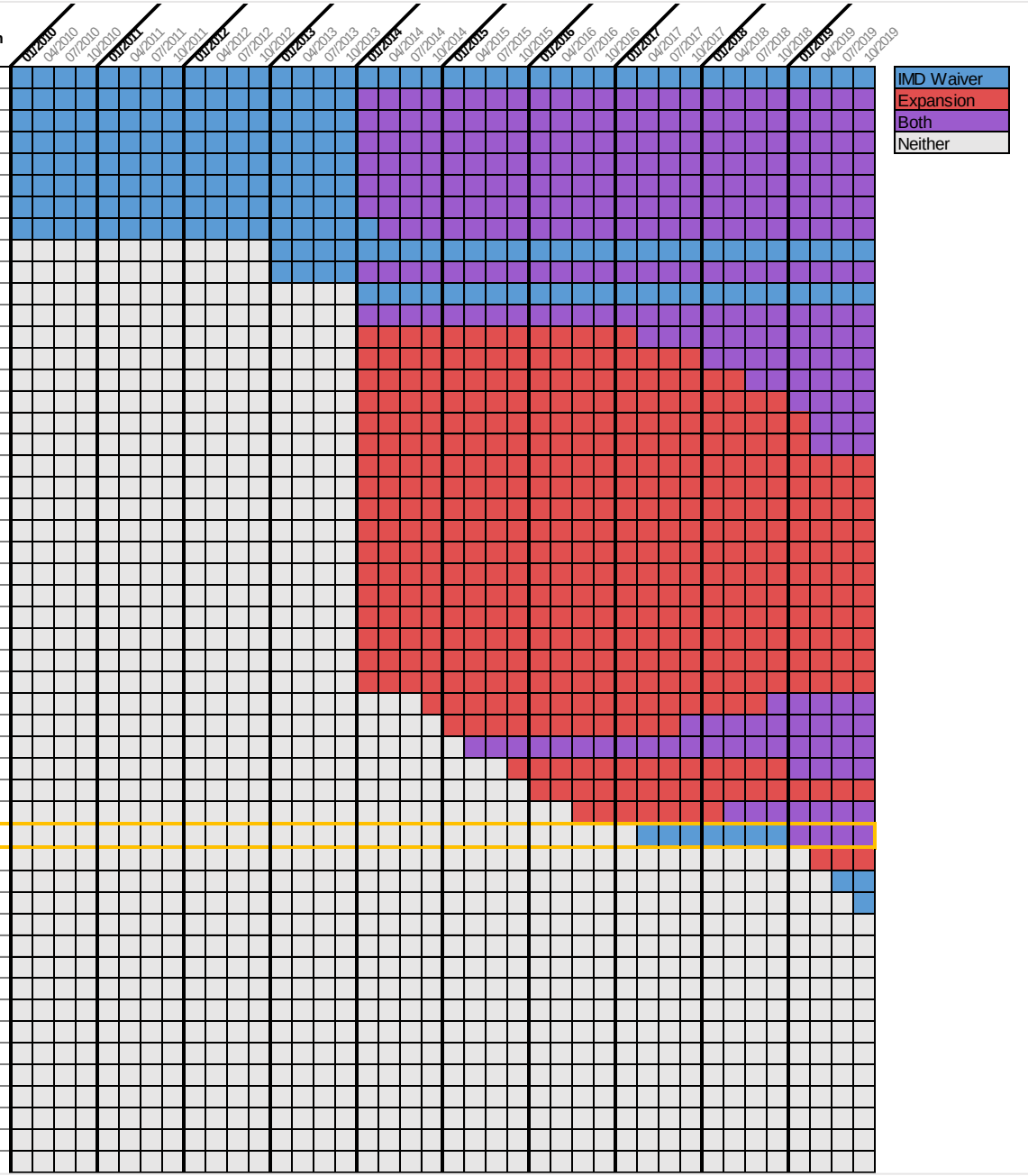


Figure 11. Visual inspection of parallel trends of Medicaid acceptance in the pre-ARTS period, 2010–2016.

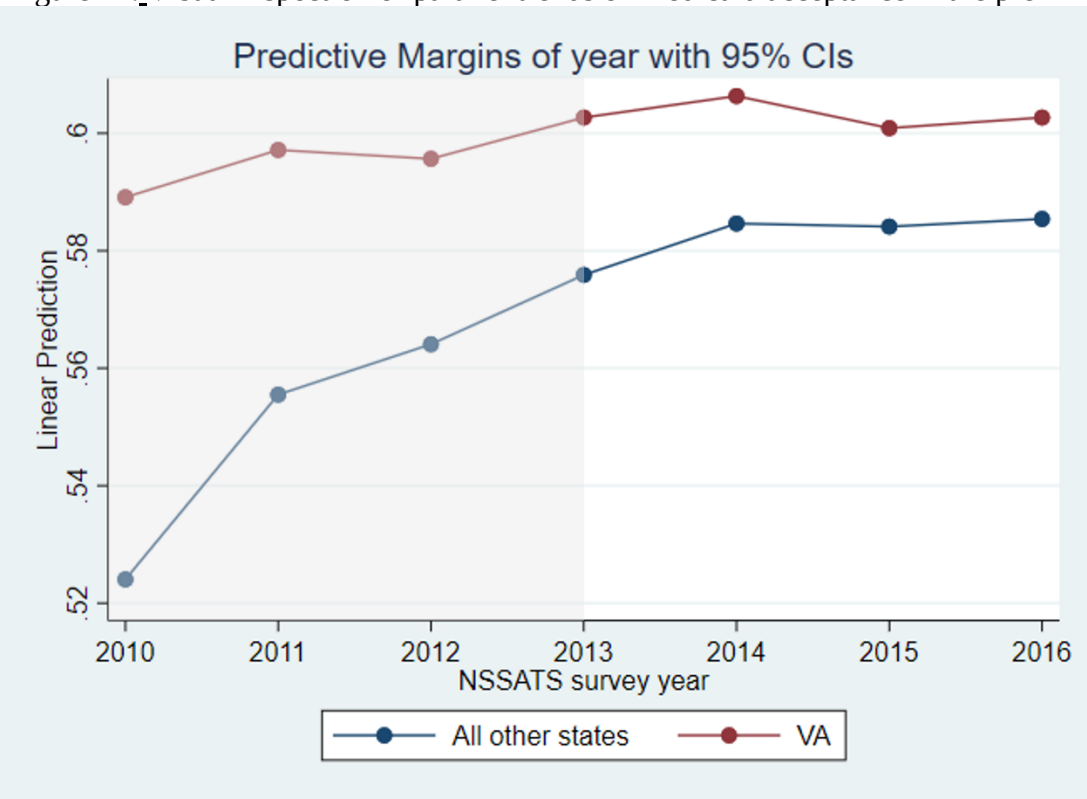
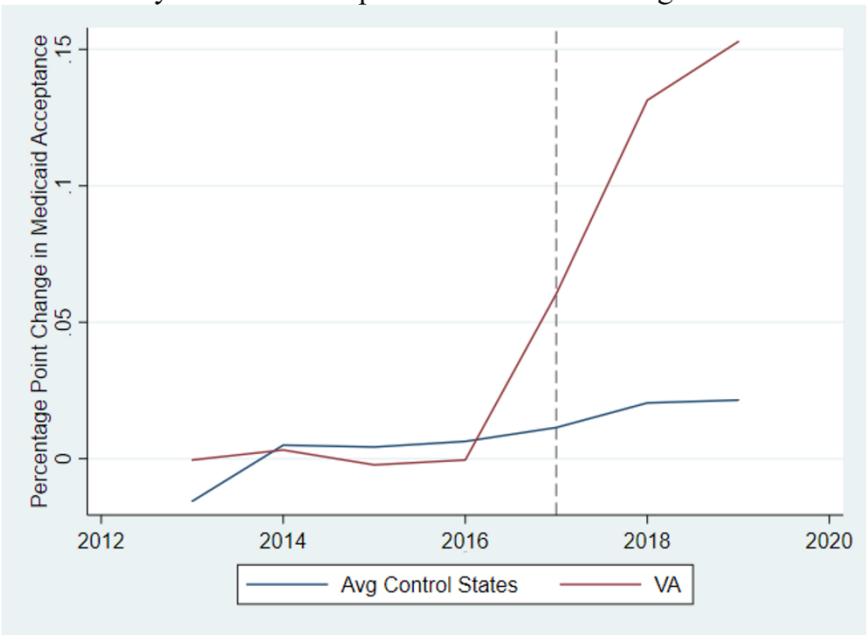


Figure 12. State-level sensitivity analysis results.

Outlier analysis with state-specific linear detrending



Synthetic control

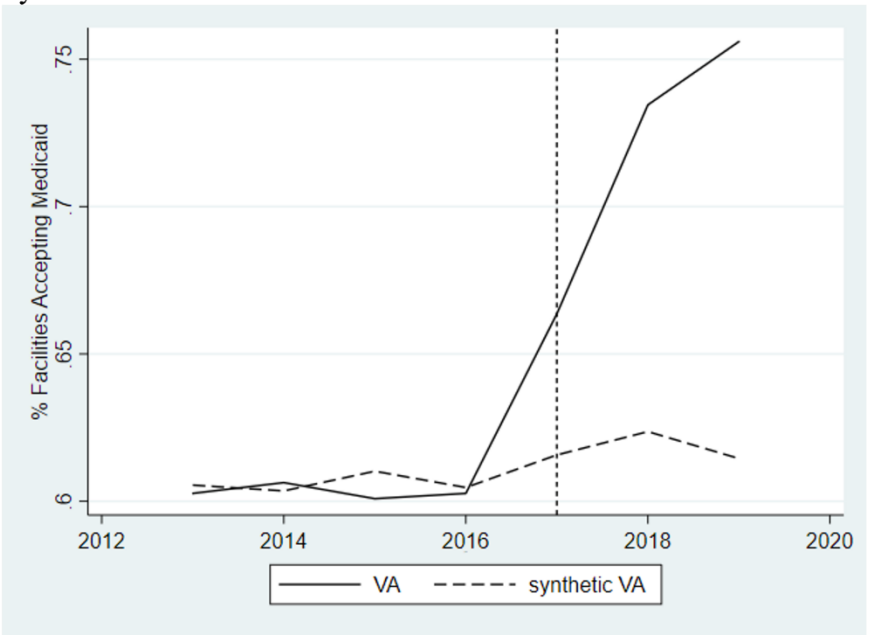


Figure 13. State-level analysis of changes in SUD treatment capacity after ARTS.

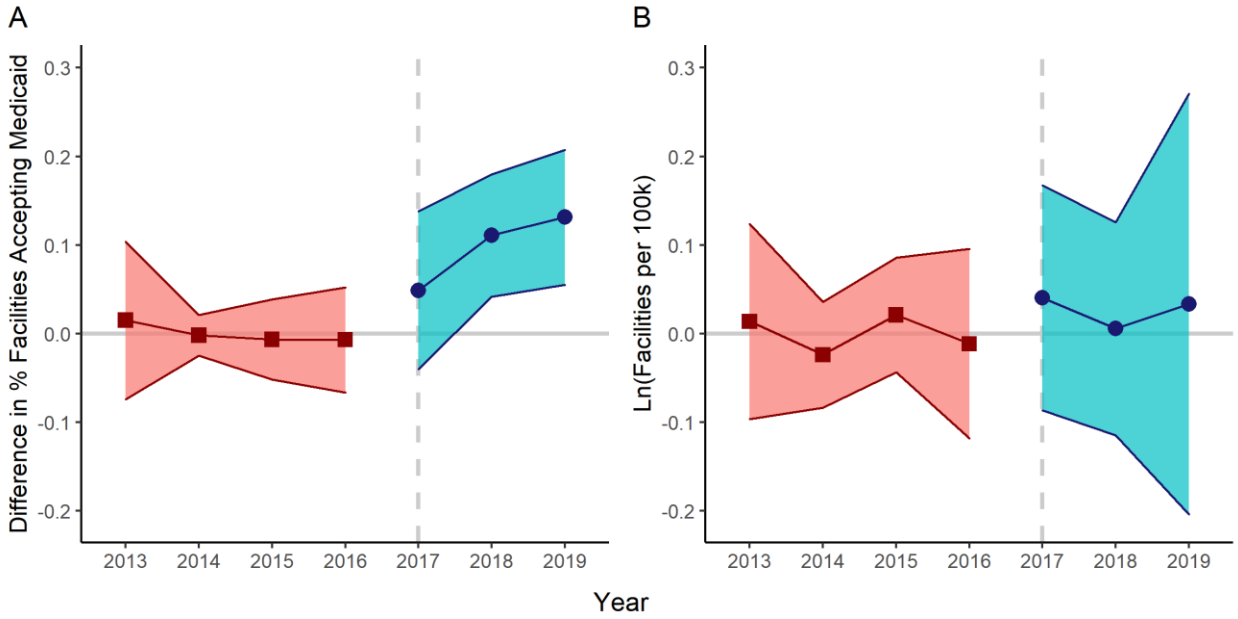


Table 12. Illustration of setting up variables required to estimate extended two-way fixed effects (ETWFE) from a panel dataset in the case of staggered treatment timing.

id	$year_t$	treatment $year_g$	w	d_{15}	d_{16}	d_{inf}	f_{14}	f_{15}	f_{16}
1	2014	2015	0	1	0	0	1	0	0
1	2015	2015	1	1	0	0	0	1	0
1	2016	2015	1	1	0	0	0	0	1
2	2014	2016	0	0	1	0	1	0	0
2	2015	2016	0	0	1	0	0	1	0
2	2016	2016	1	0	1	0	0	0	1
3	2014	.	0	0	0	1	1	0	0
3	2015	.	0	0	0	1	0	1	0
3	2016	.	0	0	0	1	0	0	1

Note: w = time-varying treatment indicator; d_g = cohort-specific treatment indicators for each treatment period, g, where $d_{inf\ identifies}$ units that are never treated (i.e., treatment timing = infinity); ft = time dummies for each year, t, in the panel

Table 13. Illustration of three weighting schemes to calculate overall treatment effects from ETWFE group-time treatment effects applied to the simple case in Figure 2.

Group-time treatment effects	d_{15}	d_{16}	
f_{14}	-	-	
f_{15}	14	-	
f_{16}	15	14	
Cohort-specific treatment effects (ATT(g,t))	**14.5**	**14**	
Steps to calculate ATT(g,t) weights			Row sum
Weight by time treated			
Number of time periods each cohort was treated	2	1	2 + 1 = 3
Share of time treated	2 / 3 = **0.667**	1 / 3 = **0.333**	
Weight by units treated			
Number of units treated in each cohort	75	25	75 + 25 = 100
Share of units treated	75 / 100 = **0.750**	25 / 100 = **0.250**	
Weight by scaled share of units and time treated			
Share time treated × share of units treated	0.667×0.759 = 0.5	0.333×0.250 = 0.083	0.5 + 0.083 = 0.583
Scaled shares of units and time treated	0.5 / 0.583 = **0.857**	0.083 / 0.583 = **0.143**	0.857 + 0.143 = 1
Overall treatment effect weighting ATT(g,t) by			Row sum = Overall treatment effect
Share of time treated	14.5×0.667 = 9.667	14×0.333 = 4.667	**14.333**
Share of units treated	14.5×0.750 = 10.875	14×0.250 = 3.500	**14.375**
Scaled shares of units and time treated	14.5×0.857 = 12.429	14×0.143 = 2.000	**14.429**

Table 14. Number of replications needed per simulated DGM.

DGM	Sample size	Treatment timing	Treatment effects	Comparison group				Max. reps needed per DGM	Final reps
				Never treated		All eventually treated			
				Var($\hat{\theta}$)	Reps needed for MC SE(Bias)<0.005	Var($\hat{\theta}$)	Reps needed for MC SE(Bias)<0.005		
01	50	equal	null	0.015	600	0.012	480	600	600
02	100	equal	null	0.008	320	0.005	200	320	400
03	200	equal	null	0.004	160	0.003	120	160	200
04	50	early	null	0.013	520	0.014	560	560	600
05	100	early	null	0.007	280	0.010	400	400	400
06	200	early	null	0.003	120	0.005	200	200	200
07	50	later	null	0.014	560	0.018	720	720	800
08	100	later	null	0.008	320	0.009	360	360	400
09	200	later	null	0.004	160	0.004	160	160	200
10	50	equal	constant	0.055	2200	0.015	600	2200	2200
11	100	equal	constant	0.024	960	0.007	280	960	1000
12	200	equal	constant	0.005	200	0.003	120	200	200
13	50	early	constant	0.039	1560	0.019	760	1560	1600
14	100	early	constant	0.018	720	0.012	480	720	800
15	200	early	constant	0.009	360	0.005	200	360	400
16	50	later	constant	0.026	1040	0.018	720	1040	1100
17	100	later	constant	0.012	480	0.010	400	480	500
18	200	later	constant	0.006	240	0.005	200	240	300
19	50	equal	heterogeneous	0.066	2640	0.014	560	2640	2700
20	100	equal	heterogeneous	0.031	1240	0.007	280	1240	1300
21	200	equal	heterogeneous	0.016	640	0.003	120	640	700
22	50	early	heterogeneous	0.110	4400	0.020	800	4400	4400
23	100	early	heterogeneous	0.048	1920	0.011	440	1920	2000
24	200	early	heterogeneous	0.026	1040	0.005	200	1040	1100
25	50	later	heterogeneous	0.056	2240	0.017	680	2240	2300
26	100	later	heterogeneous	0.028	1120	0.013	520	1120	1200
27	200	later	heterogeneous	0.013	520	0.006	240	520	600

Note: Variance of theta-hat estimated by running 10 simulations per DGM; DGM = data-generating mechanism, reps = replications, MC SE(Bias) = Monte Carlo standard error of bias, calculated as $\sqrt{\hat{\theta}} / \text{n simulations}$

Table 15. Absolute bias and Monte Carlo standard errors of bias for all DGMs.

DGM	Treatment effect	Treatment timing	Comparison group	ETWFE (A) Abs. Bias	ETWFE (A) MC SE	ETWFE (B) Abs. Bias	ETWFE (B) MC SE	ETWFE (C) Abs. Bias	ETWFE (C) MC SE	TWFE (D) Abs. Bias	TWFE (D) MC SE
Sample size = 50											
1	null	equal	never Tx	-0.004	0.005	-0.004	0.005	-0.004	0.005	**-0.002**	0.004
4	null	early	never Tx	0.004	0.005	0.003	0.005	**0.003**	0.005	0.003	0.004
7	null	later	never Tx	0.003	0.004	0.002	0.004	0.002	0.004	**0.002**	0.004
10	constant	equal	never Tx	-0.003	0.003	-0.069	0.003	**-0.001**	0.003	-0.899	0.002
13	constant	early	never Tx	-0.150	0.004	-0.051	0.004	**-0.001**	0.004	-0.509	0.005
16	constant	later	never Tx	0.175	0.005	-0.057	0.004	**-0.001**	0.005	-0.455	0.005
19	heterogeny	equal	never Tx	-0.004	0.003	-0.071	0.003	**-0.002**	0.003	-0.901	0.002
22	heterogeny	early	never Tx	-0.151	0.002	-0.053	0.002	**-0.002**	0.002	-0.512	0.003
25	heterogeny	later	never Tx	0.177	0.003	-0.057	0.003	**0.001**	0.003	-0.453	0.003
1	null	equal	all eventually Tx	0.003	0.004	**0.002**	0.004	0.003	0.004	0.004	0.003
4	null	early	all eventually Tx	-0.013	0.006	-0.014	0.006	-0.012	0.006	**-0.004**	0.004
7	null	later	all eventually Tx	0.003	0.005	**0.000**	0.004	0.001	0.004	0.002	0.003
10	constant	equal	all eventually Tx	-0.009	0.002	-0.082	0.002	**0.000**	0.002	-0.841	0.002
13	constant	early	all eventually Tx	-0.044	0.004	-0.075	0.004	**-0.004**	0.004	-0.888	0.003
16	constant	later	all eventually Tx	0.088	0.005	-0.080	0.004	**-0.003**	0.005	-0.727	0.003
19	heterogeny	equal	all eventually Tx	-0.008	0.002	-0.081	0.002	**0.001**	0.002	-0.843	0.002
22	heterogeny	early	all eventually Tx	-0.042	0.002	-0.076	0.003	**-0.004**	0.002	-0.891	0.002
25	heterogeny	later	all eventually Tx	0.090	0.003	-0.076	0.003	**0.001**	0.003	-0.724	0.002
Sample size = 100											
2	null	equal	never Tx	-0.008	0.004	-0.008	0.004	-0.008	0.004	**-0.006**	0.003
5	null	early	never Tx	-0.006	0.004	-0.007	0.004	-0.006	0.004	**-0.005**	0.004
8	null	later	never Tx	0.005	0.004	0.006	0.004	0.006	0.004	**0.005**	0.004
11	constant	equal	never Tx	**0.001**	0.003	-0.065	0.003	0.001	0.003	-0.860	0.003
14	constant	early	never Tx	-0.153	0.003	-0.053	0.004	**-0.001**	0.004	-0.510	0.005
17	constant	later	never Tx	0.179	0.005	-0.062	0.005	**-0.004**	0.005	-0.449	0.005
20	heterogeny	equal	never Tx	**-0.001**	0.003	-0.068	0.003	-0.001	0.003	-0.861	0.002
23	heterogeny	early	never Tx	-0.150	0.002	-0.048	0.002	**0.003**	0.002	-0.508	0.003
26	heterogeny	later	never Tx	0.173	0.003	-0.063	0.003	**-0.005**	0.003	-0.456	0.003
2	null	equal	all eventually Tx	**0.000**	0.004	0.001	0.004	0.000	0.004	-0.001	0.003
5	null	early	all eventually Tx	0.003	0.005	0.002	0.005	0.003	0.005	**0.002**	0.003
8	null	later	all eventually Tx	-0.001	0.005	-0.002	0.004	-0.001	0.004	**-0.001**	0.003
11	constant	equal	all eventually Tx	**0.001**	0.003	-0.084	0.002	0.001	0.003	-0.833	0.002
14	constant	early	all eventually Tx	-0.033	0.004	-0.067	0.004	**0.006**	0.004	-0.888	0.003
17	constant	later	all eventually Tx	0.096	0.005	-0.079	0.005	**0.002**	0.005	-0.721	0.003
20	heterogeny	equal	all eventually Tx	**0.003**	0.002	-0.080	0.002	0.003	0.002	-0.830	0.002

DGM	Treatment effect	Treatment timing	Comparison group	ETWFE (A) Abs. Bias	ETWFE (A) MC SE	ETWFE (B) Abs. Bias	ETWFE (B) MC SE	ETWFE (C) Abs. Bias	ETWFE (C) MC SE	TWFE (D) Abs. Bias	TWFE (D) MC SE
23	heterogeny	early	all eventually Tx	-0.038	0.002	-0.071	0.002	**0.003**	0.002	-0.891	0.002
26	heterogeny	later	all eventually Tx	0.088	0.003	-0.082	0.003	**-0.002**	0.003	-0.730	0.002
Sample size = 200											
3	null	equal	never Tx	**-0.002**	0.004	-0.002	0.005	-0.002	0.004	-0.002	0.004
6	null	early	never Tx	**-0.004**	0.004	-0.005	0.004	-0.006	0.004	-0.004	0.004
9	null	later	never Tx	**0.000**	0.004	-0.001	0.004	-0.001	0.004	0.000	0.004
12	constant	equal	never Tx	**0.003**	0.005	-0.064	0.005	0.003	0.005	-0.861	0.004
15	constant	early	never Tx	-0.153	0.003	-0.053	0.004	**-0.001**	0.004	-0.512	0.005
18	constant	later	never Tx	0.171	0.004	-0.059	0.004	**0.000**	0.004	-0.451	0.004
21	heterogeny	equal	never Tx	**0.005**	0.003	-0.062	0.003	0.005	0.003	-0.858	0.002
24	heterogeny	early	never Tx	-0.153	0.002	-0.052	0.002	**0.000**	0.002	-0.511	0.003
27	heterogeny	later	never Tx	0.167	0.003	-0.063	0.003	**-0.004**	0.003	-0.455	0.003
3	null	equal	all eventually Tx	0.004	0.003	0.003	0.004	0.004	0.003	**0.002**	0.003
6	null	early	all eventually Tx	-0.005	0.005	-0.005	0.005	-0.005	0.005	**-0.001**	0.003
9	null	later	all eventually Tx	0.002	0.005	0.004	0.004	0.003	0.004	**0.002**	0.003
12	constant	equal	all eventually Tx	**-0.003**	0.004	-0.087	0.004	-0.003	0.004	-0.841	0.003
15	constant	early	all eventually Tx	-0.037	0.004	-0.070	0.004	**0.004**	0.004	-0.890	0.003
18	constant	later	all eventually Tx	0.091	0.005	-0.078	0.004	**0.006**	0.004	-0.725	0.003
21	heterogeny	equal	all eventually Tx	**-0.002**	0.002	-0.085	0.002	-0.002	0.002	-0.834	0.002
24	heterogeny	early	all eventually Tx	-0.040	0.002	-0.074	0.002	**0.001**	0.002	-0.894	0.002
27	heterogeny	later	all eventually Tx	0.084	0.003	-0.082	0.003	**0.000**	0.003	-0.730	0.002

Notes: Minimum absolute bias presented in **bold** and determined prior to rounding; ETWFE = extended two-way fixed effects; TWFE = two-way fixed effects; MC SE = Monte Carlo standard error of bias; Tx = treatment; weighting scheme A weights group-time average treatment effects on the treated (ATTs) for percentage of time treated, B weights group-time ATTs for percentage of units treated, C weights group-time ATTs for scaled percentage of units and time treated, D = TWFE estimates; A–D absolute bias graphically displayed in Figure 2

Table 16. Coverage and Monte Carlo standard errors of coverage for all DGMs.

DGM	Treatment effect	Treatment timing	Comparison group	ETWFE (A) Coverage	ETWFE (A) MC SE	ETWFE (B) Coverage	ETWFE (B) MC SE	ETWFE (C) Coverage	ETWFE (C) MC SE	TWFE (D) Coverage	TWFE (D) MC SE
Sample size = 50											
1	null	equal	never Tx	**0.953**	0.009	**0.952**	0.009	**0.955**	0.008	**0.957**	0.008
4	null	early	never Tx	0.945	0.009	**0.953**	0.009	**0.953**	0.009	**0.957**	0.008
7	null	later	never Tx	0.933	0.009	0.949	0.008	0.944	0.008	**0.953**	0.008
10	constant	equal	never Tx	**0.974**	0.003	0.946	0.005	**0.974**	0.003	0.000	0.000
13	constant	early	never Tx	0.808	0.010	**0.959**	0.005	**0.979**	0.004	0.214	0.010
16	constant	later	never Tx	0.793	0.012	**0.960**	0.006	**0.979**	0.004	0.189	0.012
19	heterogeny	equal	never Tx	**0.970**	0.003	0.945	0.004	**0.970**	0.003	0.000	0.000
22	heterogeny	early	never Tx	0.811	0.006	**0.969**	0.003	**0.981**	0.002	0.198	0.006
25	heterogeny	later	never Tx	0.777	0.009	**0.966**	0.004	**0.981**	0.003	0.179	0.008
1	null	equal	all eventually Tx	0.945	0.009	0.935	0.010	**0.950**	0.009	**0.950**	0.009
4	null	early	all eventually Tx	0.905	0.012	0.902	0.012	0.905	0.012	0.947	0.009
7	null	later	all eventually Tx	0.914	0.010	**0.955**	0.007	**0.953**	0.008	**0.951**	0.008
10	constant	equal	all eventually Tx	**0.968**	0.004	0.906	0.006	**0.969**	0.004	0.000	0.000
13	constant	early	all eventually Tx	0.886	0.008	0.872	0.008	0.916	0.007	0.005	0.002
16	constant	later	all eventually Tx	0.876	0.010	0.912	0.009	**0.955**	0.006	0.005	0.002
19	heterogeny	equal	all eventually Tx	**0.964**	0.004	0.904	0.006	**0.965**	0.004	0.000	0.000
22	heterogeny	early	all eventually Tx	0.890	0.005	0.873	0.005	0.920	0.004	0.003	0.001
25	heterogeny	later	all eventually Tx	0.878	0.007	0.935	0.005	**0.966**	0.004	0.005	0.002
Sample size = 100											
2	null	equal	never Tx	0.938	0.012	0.935	0.012	0.938	0.012	0.948	0.011
5	null	early	never Tx	0.933	0.013	0.940	0.012	0.948	0.011	0.933	0.013
8	null	later	never Tx	0.948	0.011	**0.955**	0.010	**0.955**	0.010	**0.958**	0.010
11	constant	equal	never Tx	**0.962**	0.006	0.923	0.008	**0.962**	0.006	0.000	0.000
14	constant	early	never Tx	0.661	0.017	**0.960**	0.007	**0.983**	0.005	0.011	0.004
17	constant	later	never Tx	0.638	0.021	0.932	0.011	**0.976**	0.007	0.016	0.006
20	heterogeny	equal	never Tx	**0.968**	0.005	0.923	0.007	**0.968**	0.005	0.000	0.000
23	heterogeny	early	never Tx	0.685	0.010	**0.964**	0.004	**0.983**	0.003	0.009	0.002
26	heterogeny	later	never Tx	0.668	0.014	0.918	0.008	**0.973**	0.005	0.013	0.003
2	null	equal	all eventually Tx	**0.958**	0.010	**0.960**	0.010	**0.958**	0.010	0.943	0.012
5	null	early	all eventually Tx	0.908	0.014	0.905	0.015	0.918	0.014	0.928	0.013
8	null	later	all eventually Tx	0.933	0.013	**0.953**	0.011	**0.955**	0.010	0.925	0.013
11	constant	equal	all eventually Tx	**0.969**	0.005	0.843	0.012	**0.969**	0.005	0.000	0.000
14	constant	early	all eventually Tx	0.928	0.009	0.903	0.010	0.949	0.008	0.000	0.000
17	constant	later	all eventually Tx	0.838	0.016	0.894	0.014	**0.968**	0.008	0.000	0.000
20	heterogeny	equal	all eventually Tx	**0.972**	0.005	0.845	0.010	**0.972**	0.005	0.000	0.000

DGM	Treatment effect	Treatment timing	Comparison group	ETWFE (A) Coverage	ETWFE (A) MC SE	ETWFE (B) Coverage	ETWFE (B) MC SE	ETWFE (C) Coverage	ETWFE (C) MC SE	TWFE (D) Coverage	TWFE (D) MC SE
23	heterogeny	early	all eventually Tx	0.923	0.006	0.892	0.007	**0.954**	0.005	0.000	0.000
26	heterogeny	later	all eventually Tx	0.872	0.010	0.880	0.009	**0.972**	0.005	0.000	0.000
Sample size = 200											
3	null	equal	never Tx	0.925	0.019	0.925	0.019	0.925	0.019	0.940	0.017
6	null	early	never Tx	0.945	0.016	**0.960**	0.014	**0.960**	0.014	**0.960**	0.014
9	null	later	never Tx	0.940	0.017	**0.955**	0.015	0.945	0.016	**0.950**	0.015
12	constant	equal	never Tx	**0.975**	0.011	0.860	0.025	**0.975**	0.011	0.000	0.000
15	constant	early	never Tx	0.410	0.025	0.908	0.014	**0.985**	0.006	0.000	0.000
18	constant	later	never Tx	0.377	0.028	0.903	0.017	**0.983**	0.007	0.000	0.000
21	heterogeny	equal	never Tx	**0.979**	0.005	0.881	0.012	**0.979**	0.005	0.000	0.000
24	heterogeny	early	never Tx	0.399	0.015	0.926	0.008	**0.985**	0.004	0.000	0.000
27	heterogeny	later	never Tx	0.397	0.020	0.882	0.013	**0.982**	0.005	0.000	0.000
3	null	equal	all eventually Tx	**0.970**	0.012	**0.960**	0.014	**0.970**	0.012	**0.965**	0.013
6	null	early	all eventually Tx	0.940	0.017	0.940	0.017	0.935	0.017	0.945	0.016
9	null	later	all eventually Tx	0.930	0.018	**0.960**	0.014	**0.955**	0.015	**0.960**	0.014
12	constant	equal	all eventually Tx	**0.970**	0.012	0.690	0.033	**0.970**	0.012	0.000	0.000
15	constant	early	all eventually Tx	0.928	0.013	0.845	0.018	**0.970**	0.009	0.000	0.000
18	constant	later	all eventually Tx	0.810	0.023	0.800	0.023	**0.963**	0.011	0.000	0.000
21	heterogeny	equal	all eventually Tx	**0.959**	0.008	0.696	0.017	**0.959**	0.008	0.000	0.000
24	heterogeny	early	all eventually Tx	0.914	0.008	0.841	0.011	**0.958**	0.006	0.000	0.000
27	heterogeny	later	all eventually Tx	0.817	0.016	0.782	0.017	**0.973**	0.007	0.000	0.000

Notes: Coverage reaching 95% presented in bold; ETWFE = extended two-way fixed effects; TWFE = two-way fixed effects; MC SE = Monte Carlo standard error of coverage; Tx = treatment; weighting scheme A weights group-time average treatment effects on the treated (ATTs) for percentage of time treated, B weights group-time ATTs for percentage of units treated, C weights group-time ATTs for scaled percentage of units and time treated, D = TWFE estimates; A–D coverage graphically displayed in Figure 3

Figure 14. Variation in treatment timing, treatment effects, and comparison groups included in simulation settings.

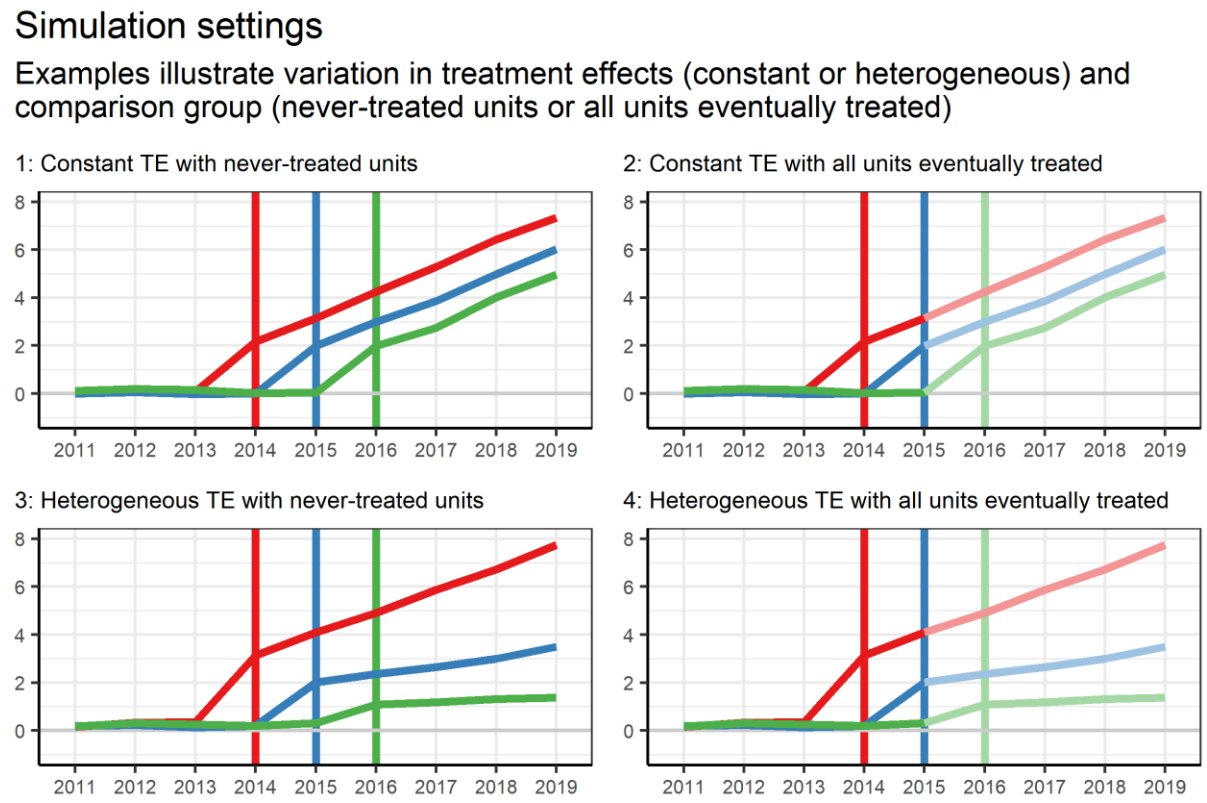


Figure 15. Variation in Medicaid expansion implementation through 2019.

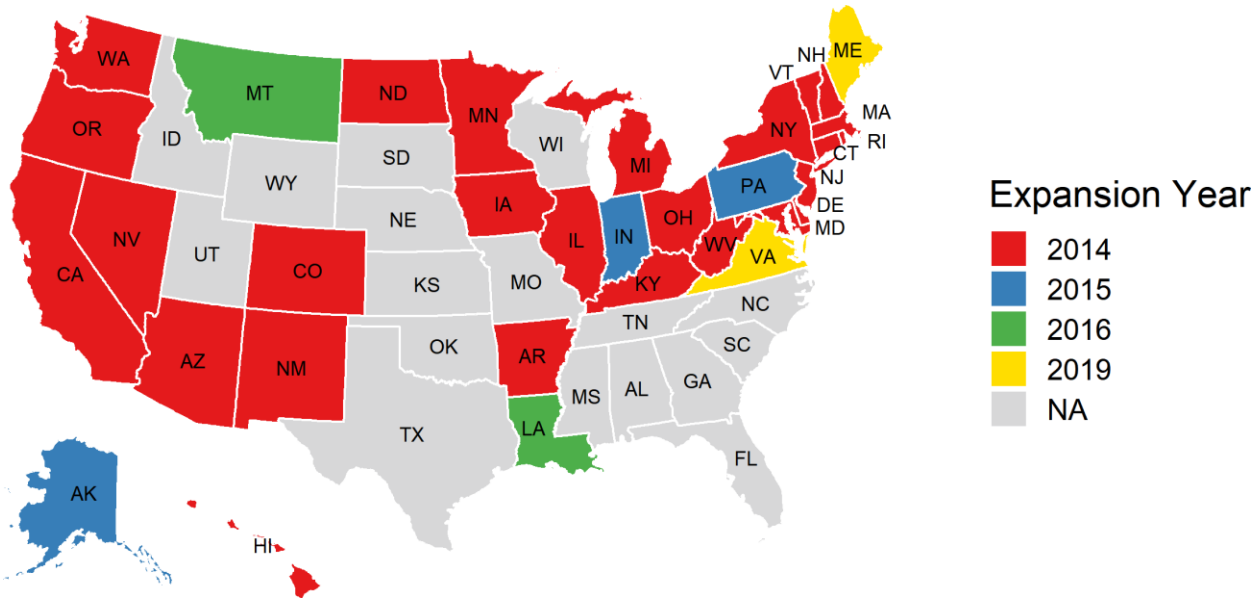
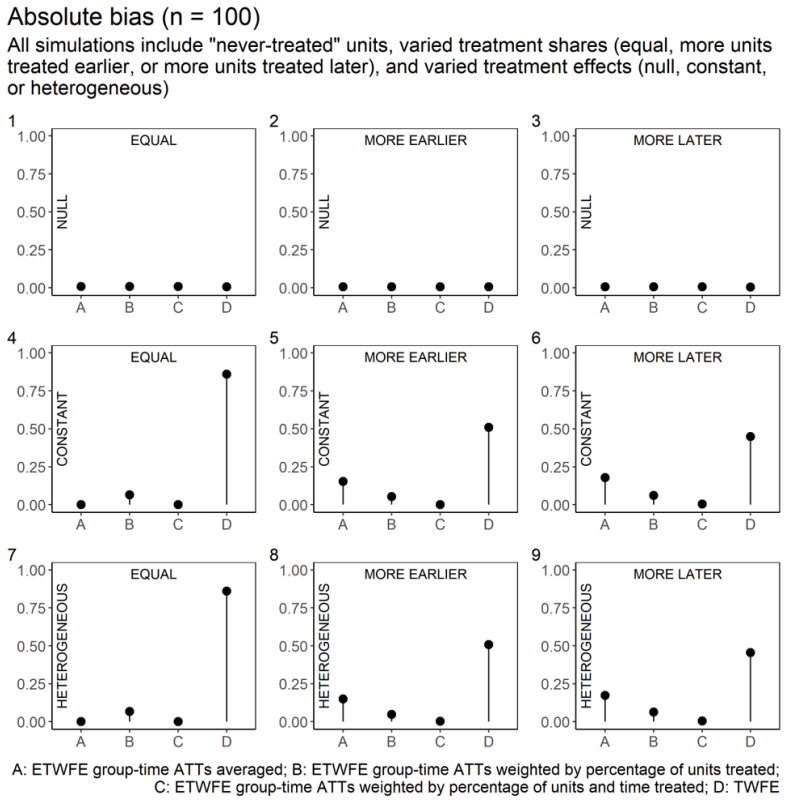


Figure 16. Absolute bias of TWFE and ETWFE estimators across DGMs with sample size of 100.



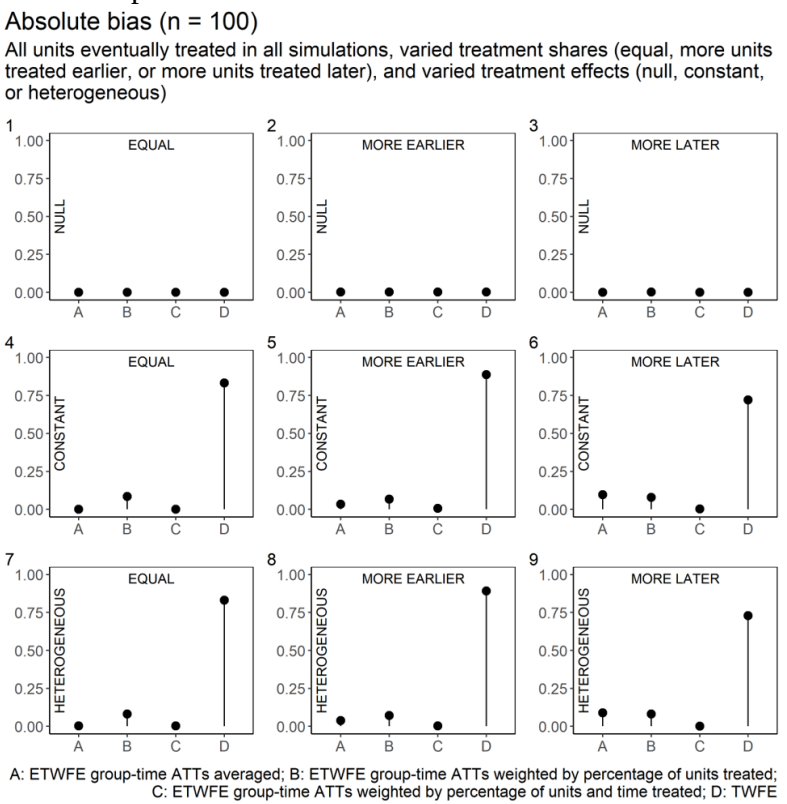
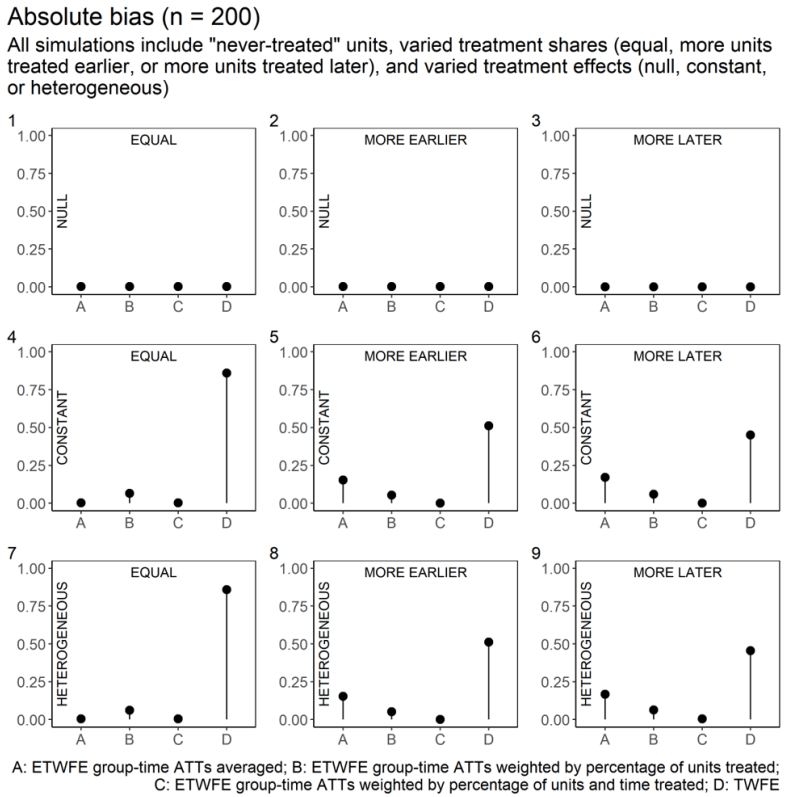


Figure 17. Absolute bias of TWFE and ETWFE estimators across DGMs with sample size of 200.



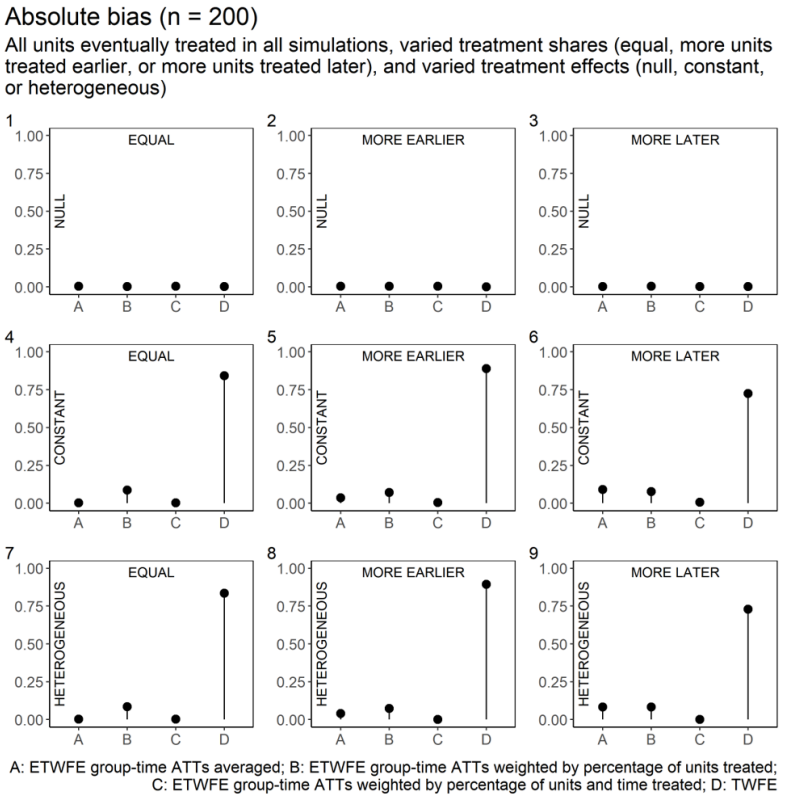
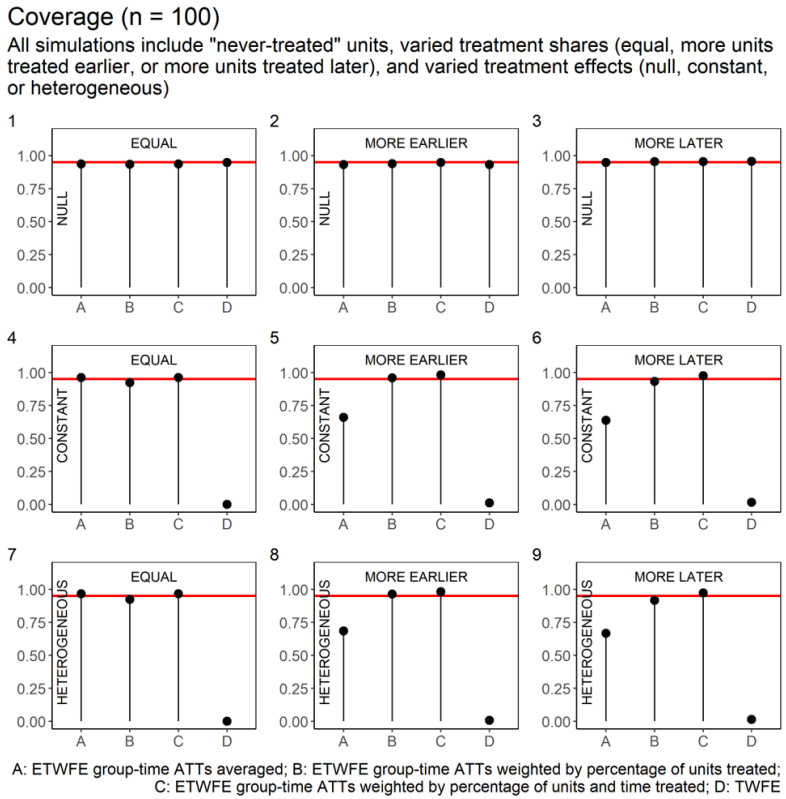


Figure 18. Coverage of TWFE and ETWFE estimators across DGMs with sample size of 100.



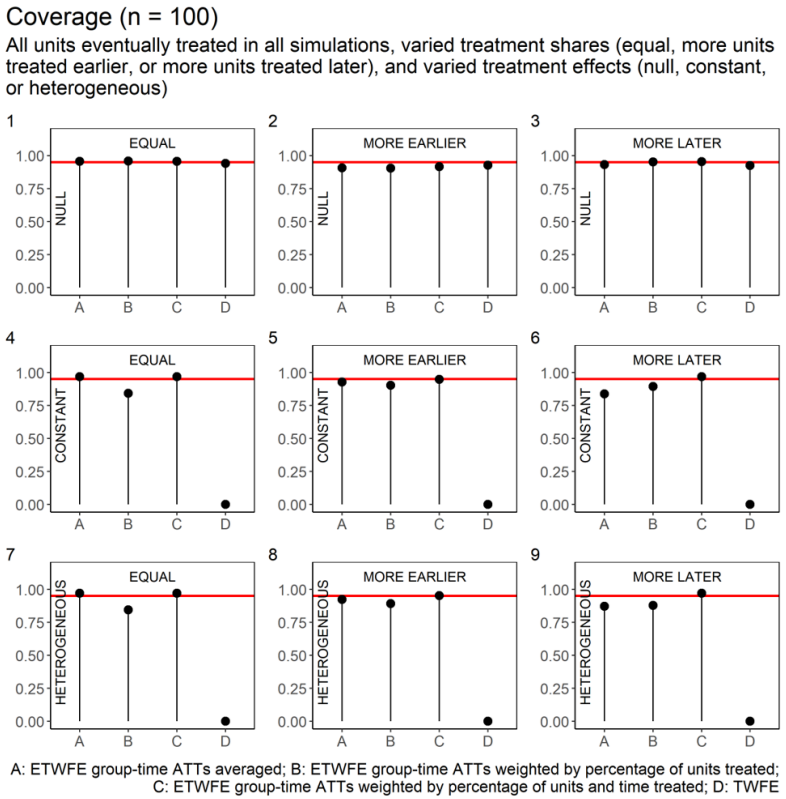
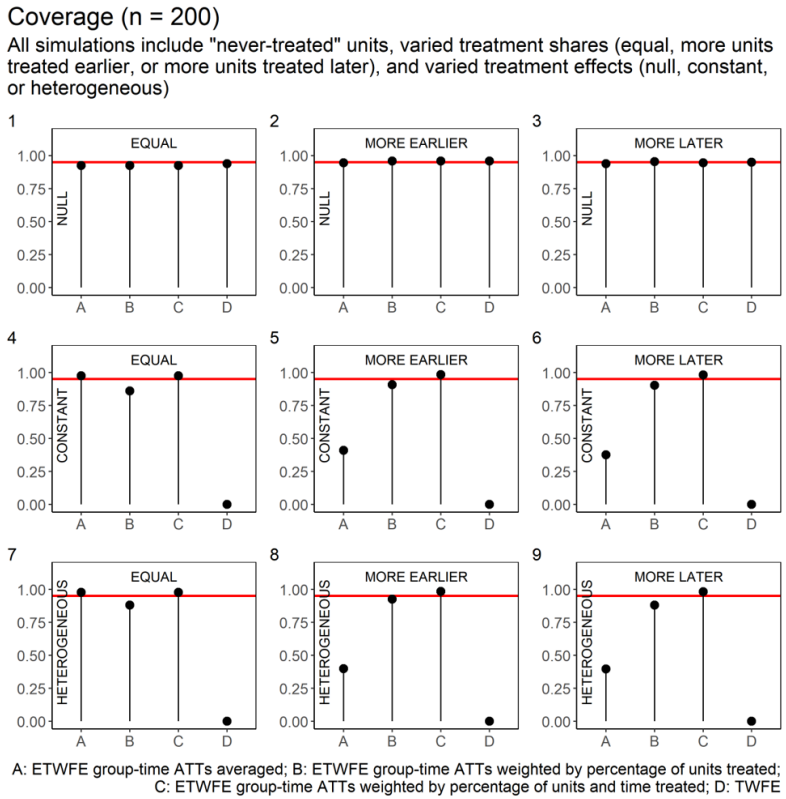


Figure 19. Coverage of TWFE and ETWFE estimators across DGMs with sample size of 200.



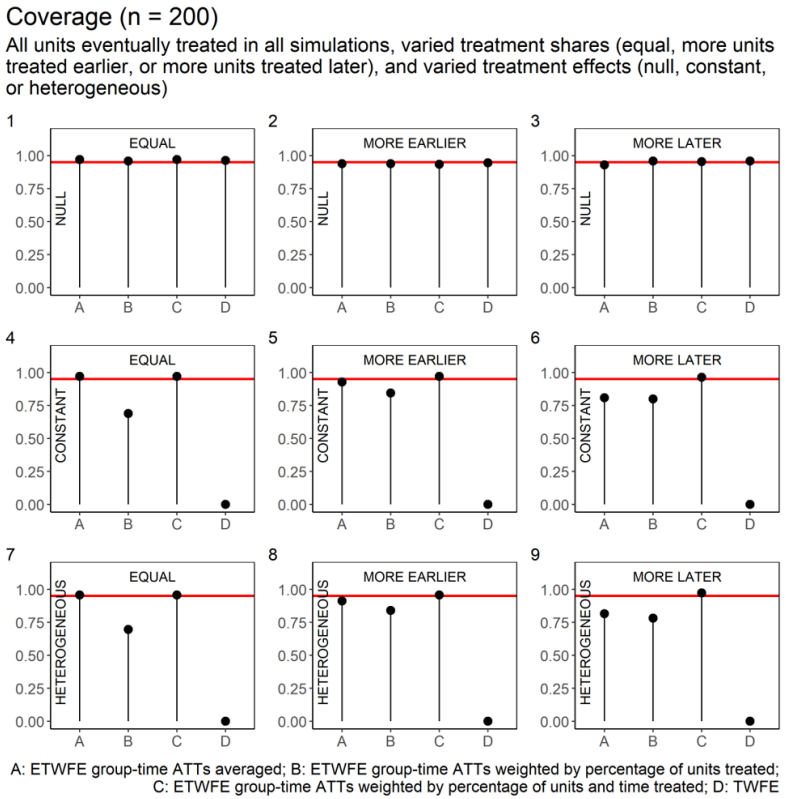


Table 17. Variation in treatment timing of providers participating in the preferred OBOT program.

	16Q1	16Q2/16Q3	16Q4/17Q1	17Q2/17Q3	17Q4/18Q1	18Q2/18Q3	18Q4/19Q1
Independent BWPs							
Never treated	437	437	437	437	437	437	437
Preferred OBOTs							
Newly Treated	0	0	0	86	13	78	12
Not-yet-treated	189	189	189	103	90	12	0
% Newly treated	0%	0%	0%	46%	7%	41%	6%
Cumulative treated							
% Cumulative treated	0%	0%	0%	46%	53%	94%	100%

Notes: ARTS implementation coincided with beginning of preferred OBOT program on 04/01/2017; Q1, 2019 only includes January, as the prior authorization requirement for buprenorphine was also waived for independent BWPs beginning 02/01/2019.

Table 18. ATTs of the preferred OBOT program on buprenorphine prescribing rates excluding providers who exited the program early (n=9).

Buprenorphine-covered members per day	Coef.	SE	*p*-value	Pre-trends *p*-value
Group-time ATTs				0.2671
Cohort 17Q2/17Q3 (n=78)				
17Q2/17Q3	0.724	0.337	0.032	
17Q4/18Q1	4.215	0.918	<0.001	
18Q2/18Q3	5.091	1.337	<0.001	
18Q4/19Q1	6.770	2.086	0.001	
Cohort 17Q4/18Q1 (n=12)				
17Q4/18Q1	2.818	1.214	0.021	
18Q2/18Q3	6.908	4.046	0.088	
18Q4/19Q1	6.974	4.287	0.104	
Cohort 18Q2/18Q3 (n=78)				
18Q2/18Q3	1.823	0.474	<0.001	
18Q4/19Q1	3.747	0.887	<0.001	
Cohort 18Q4/19Q1 (n=12)				
18Q4/19Q1	0.745	0.302	0.014	
Overall ATT[1] (n=180)	3.787	0.711	<0.001	0.2671
Group-specific ATTs[1]				0.2671
Cohort 17Q2/17Q3 (n=78)	4.200	1.074	<0.001	
Cohort 17Q4/18Q1 (n=12)	5.567	3.151	0.078	
Cohort 18Q2/18Q3 (n=78)	2.785	0.662	<0.001	
Cohort 18Q4/19Q1 (n=12)	0.745	0.302	0.014	
Event-time ATTs[1]				0.2671
-3 1.5 years before treatment (n=12)	0.066	0.196	0.737	
-2 1 year before treatment (n=90)	0.152	0.209	0.469	
-1 Six months before treatment (n=102)	0.507	0.354	0.152	
0 Treatment (n=180)	1.341	0.264	<0.001	
1 Six months after treatment (n=168)	4.190	0.649	<0.001	
2 1 year after treatment (n=90)	5.342	1.285	<0.001	
3 1.5 years after treatment (n=78)	6.770	2.086	0.001	

[1]*Overall ATT calculated as weighted sum of group-specific ATTs weighted by share of treated units and treated periods; group-specific ATTs calculated as average of group-time ATTs by cohort; event-time ATTs calculated as weighted sum of group-time ATTs weighted by share of treated units; all estimates adjusted for heterogeneous linear trends and cluster standard errors at the provider.*

Table 19. ATTs of the preferred OBOT program on buprenorphine prescribing rates stratified by early buprenorphine prescribing.[1]

Buprenorphine-covered members per day	Early buprenorphine prescribers[1] (Independent BWPs, n=52)				Later buprenorphine prescribers[1] (Independent BWPs, n=385)				
	n OBOTs	Coef.	SE	*p*-value	Pre-trends *p*-value	n OBOTs	Coef.	SE	*p*-value	Pre-trends *p*-value
Group-time ATTs					0.8384					0.1349
Cohort 17Q2/17Q3	28					58				
17Q2/17Q3		1.320	1.211	0.278			0.503	0.252	0.046	
17Q4/18Q1		3.646	2.571	0.159			3.265	0.772	<0.001	
18Q2/18Q3		6.172	3.757	0.104			3.947	1.007	<0.001	
18Q4/19Q1		8.523	4.731	0.075			5.504	2.105	0.009	
Cohort 17Q4/18Q1	2					11				
17Q4/18Q1		9.362	1.614	<0.001			1.257	0.507	0.013	
18Q2/18Q3		36.995	5.355	<0.001			0.791	0.565	0.162	
18Q4/19Q1		39.668	6.827	<0.001			0.440	0.458	0.337	
Cohort 18Q2/18Q3	10					68				
18Q2/18Q3		6.857	2.261	0.003			1.101	0.405	0.007	
18Q4/19Q1		12.534	4.135	0.003			2.494	0.781	0.001	
Cohort 18Q4/19Q1	2					10				
18Q4/19Q1		3.346	0.839	<0.001			0.354	0.326	0.277	
Overall ATT[2]	42	6.594	2.390	0.007	0.8384	147	2.535	0.561	<0.001	0.1349
Group-specific ATTs[2]					0.8384					0.1349
Cohort 17Q2/17Q3	28	4.915	2.887	0.092		58	3.305	0.939	<0.001	
Cohort 17Q4/18Q1	2	28.675	4.538	<0.001		11	0.829	0.367	0.024	
Cohort 18Q2/18Q3	10	9.695	3.068	0.002		68	1.798	0.580	0.002	
Cohort 18Q4/19Q1	2	3.346	0.839	<0.001		10	0.354	0.326	0.277	
Event-time ATTs[2]					0.8384					0.1349
-3 1.5 years before treatment	2	1.148	1.425	0.423		10	-0.087	0.037	0.017	
-2 1 year before treatment	12	1.566	1.667	0.350		78	-0.062	0.031	0.046	
-1 Six months before treatment	14	3.042	2.462	0.220		89	0.114	0.134	0.397	
0 Treatment	42	3.118	1.005	0.003		147	0.826	0.214	<0.001	
1 Six months after treatment	40	7.535	2.076	<0.001		137	2.683	0.503	<0.001	
2 1 year after treatment	30	8.405	3.542	0.020		69	3.388	0.850	<0.001	
3 1.5 years after treatment	28	8.523	4.731	0.075		58	5.504	2.105	0.009	

[1]*Early buprenorphine prescribers are waivered prescribers who billed Medicaid for buprenorphine in Q1, 2016—more than one year before the ARTS benefit; later buprenorphine prescribers first billed Medicaid for buprenorphine during the study period.*

[2]Overall ATT calculated as weighted sum of group-specific ATTs weighted by share of treated units and treated periods; group-specific ATTs calculated as average of group-time ATTs by cohort; event-time ATTs calculated as weighted sum of group-time ATTs weighted by share of treated units; all estimates adjusted for heterogeneous linear trends and cluster standard errors at the provider.

Table 20. Doubly-robust inverse probability weighted ATTs of the preferred OBOT program on buprenorphine prescribing rates.[1]

Buprenorphine-covered members per day	Coef.	SE	*p*-value	Pre-trends *p*-value
Group-time ATTs				0.3704
Cohort 17Q2/17Q3 (n=86)				
17Q2/17Q3	0.938	0.492	0.057	
17Q4/18Q1	3.061	1.245	0.014	
18Q2/18Q3	5.681	1.639	0.001	
18Q4/19Q1	7.526	2.331	0.001	
Cohort 17Q4/18Q1 (n=13)				
17Q4/18Q1	2.491	1.181	0.035	
18Q2/18Q3	6.966	4.448	0.117	
18Q4/19Q1	7.613	5.157	0.140	
Cohort 18Q2/18Q3 (n=78)				
18Q2/18Q3	1.910	0.511	<0.001	
18Q4/19Q1	3.867	0.974	<0.001	
Cohort 18Q4/19Q1 (n=12)				
18Q4/19Q1	0.954	0.564	0.090	
Overall ATT[2]	3.927	0.890	<0.001	0.3704
Group-specific ATTs[2]				0.3704
Cohort 17Q2/17Q3 (n=86)	4.302	1.326	0.001	
Cohort 17Q4/18Q1 (n=13)	5.690	3.558	0.110	
Cohort 18Q2/18Q3 (n=78)	2.888	0.727	<0.001	
Cohort 18Q4/19Q1 (n=12)	0.954	0.564	0.090	
Event-time ATTs[2]				0.3704
-3 1.5 years before treatment (n=12)	-0.037	0.119	0.754	
-2 1 year before treatment (n=90)	0.067	0.202	0.742	
-1 Six months before treatment (n=103)	0.335	0.175	0.056	
0 Treatment (n=189)	1.447	0.314	<0.001	
1 Six months after treatment (n=177)	3.703	0.809	<0.001	
2 1 year after treatment (n=99)	5.934	1.584	<0.001	
3 1.5 years after treatment (n=86)	7.526	2.331	0.001	

[1]Treatment model predictors included provider waiver limit; provider taxonomy; early buprenorphine prescribing indicator; percentage of patients with history of mental health diagnosis; percentage female patients, percentage of patients aged ≤ 29, 30–49, and ≥ 50; percentage of patients who identified as non-Hispanic white race/ethnicity; and percentage of patients residing in an urban zip code.

[2]Overall ATT calculated as weighted sum of group-specific ATTs weighted by share of treated units and treated periods; group-specific ATTs calculated as average of group-time ATTs by cohort; event-time ATTs calculated as weighted sum of group-time ATTs weighted by share of treated units; all estimates adjusted for heterogeneous linear trends and cluster standard errors at the provider.

CURRICULUM VITAE

ERIN L. BRITTON
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PROFILE

Experience	Cross-disciplinary training in statistics, economics, epidemiology, and health policy; Quasi-experimental research methods; Data management and database architecture; Healthcare claims, eligibility, provider, and survey data; Vital records; Electronic health records; Performance and quality measurement (PQA, HEDIS, STARS); Management and training of data analysts
Software Proficiencies	SQL, SAS, Stata, R, Javascript
Research Interests	Medicare and Medicaid policy, Mental health, Substance use disorder, Multimorbidity, Provider workforce, Vulnerable populations

EDUCATION

2022	Ph.D., Healthcare Policy and Research, School of Medicine, Virginia Commonwealth University, Richmond, VA.
	Dissertation: Using difference-in-difference to assess the impact of Medicaid policy on the addiction treatment provider workforce and prescribing behavior in Virginia
	Advisory Committee: Peter Cunningham, Ph.D. (Chair), Andrew Barnes, Ph.D., David Harless, Ph.D., Caitlin Martin, M.D., M.P.H.
2015	M.P.H., Public Health, School of Medicine, Virginia Commonwealth University, Richmond, VA.
	Thesis: Patterns of multimorbidity among nonelderly adults enrolled in the Virginia Coordinated Care program in 2010
2012	B.S., Sociology, Virginia Commonwealth University, Richmond, VA.

RESEARCH EXPERIENCE

2020–present Graduate Research Assistant, Virginia Commonwealth University, Department of Family Medicine & Epidemiology.
Advisor: Rebecca Etz, Ph.D.

- Quantitative analyst for the Larry A. Green Center's Quick COVID-19 Primary Care Survey to monitor the impact of COVID-19 on the U.S. primary care workforce.

RESEARCH EXPERIENCE

2018–present Graduate Research Assistant, Virginia Commonwealth University, Department of Health Behavior and Policy.
Advisor: Peter Cunningham, Ph.D.

- Contactor for the Virginia Department of Medical Assistance Services to evaluate Virginia's Section 1115 behavioral health waiver, Managed Long-Term Services and Supports (MLTSS) program, and Medicaid Expansion. Developed analytic files and survey samples from administrative claims and enrollment databases. Supported the development of electronic and mail surveys. Supported colleagues learning to use relational databases and complex administrative data using SAS and SQL. Consulted with state partners on development and enhancement of datasets.

2016–2018 Product Manager, Virginia Premier Health Plan, Analytics Department.

- Lead the development of analytic and reporting strategy for Medicare Advantage program from bid through program launch.

2015–2016 Senior Business Analyst, Virginia Premier Health Plan, Analytics Department.

- Evaluated CMS dual eligible demonstration program.

2014–2015 Graduate Research Assistant, Virginia Commonwealth University, Department of Family Medicine & Epidemiology.
Supervisor: Juan Lu, Ph.D., M.P.H., M.D.

- Evaluated patient characteristics associated with high-cost utilization.

2013–2014 Graduate Research Assistant, Virginia Commonwealth University, Department of Social and Behavioral Health.
Supervisor: Laura Siminoff, Ph.D.

- Developed qualitative codebook from audio files of patient-provider interaction to develop decision support tool for families with children with congenital and genetic anomalies.

2011–2012 Research Assistant, Virginia Commonwealth University, Department of Biostatistics.
Supervisor: Cynthia Sabo, M.S.

- Digitized archival data for the Mid-Atlantic Twin Registry.

PEER-REVIEWED PUBLICATIONS

1. Saunders H, **Britton E**, Cunningham PJ, Walker LS, Harrell A, Scialli A, Lowe J. Medicaid participation among practitioners authorized to prescribe buprenorphine. *Journal of Substance Abuse Treatmen*t. 2022; 133. https://doi.org/10.1016/j.jsat.2021.108513
2. Adepoju O, Liaw W, Chae M, Ojinnaka C, **Britton E**, Reves S, Etz R. COVID-19 and telehealth operations in Texas primary care clinics: Disparities in medically underserved area clinics. *Journal of Health Care for the Poor and Underserved*. 2021; 32(2): 948-957. https://www.muse.jhu.edu/article/794616
3. Barnes AJ, Cunningham PJ, Walker LS, **Britton E**, Sheng Y, Boynton M, Harper K, Harrell A, Bachireddy C, Montz E, Neuhausen K. Hospital use declines after implementation of Virginia Medicaid's Addiction and Recovery Treatment Services. *Health Affairs*. 2020; 39(2): 238–246. doi:10.1377/hlthaff.2019.00525

PEER-REVIEWED PUBLICATIONS

4. Lu J, **Britton E**, Ferrance J, Rice E, Kuzel A, Dow A. Identifying future high cost individuals within an intermediate cost population. *Quality in Primary Care*. 2015; 23(6): 318-326.

MANUSCRIPTS IN PROGRESS

1. Mellor J, Cunningham PJ, **Britton E**, Walker LS. Use of home and community-based services after implementation of Medicaid managed long term services and supports in Virginia.
2. Martin C, **Britton E**, Shadowen H, Bachireddy C, Harrell A, Zhao X, Cunningham PJ. Disparities in opioid use disorder related hospital use among postpartum Virginia Medicaid members.
3. Williams AR, **Britton E**, Hines AL, Thomson MD. Examining associations between primary care team-based care attributes, nutrition care delivery, and blood pressure control for patients with hypertension served in a large urban health system.
4. **Britton E,** Bachireddy C, Martin C, Walker LS, Harrell A, Barnes AJ, Cunningham PJ. Changes in initiation and continuity of pharmacotherapy for opioid use disorder after COVID-19 among Medicaid members in Virginia

REPORTS & POLICY BRIEFS

1. Cunningham PJ, Pierre-Louis S, Saunders H, **Britton E**, Farzana AU, Barnes AJ. Addiction and recovery treatment services: Evaluation report for state fiscal years 2019 and 2020. Delivered to Virginia Medicaid and published online. May 2022.
2. Saunders H, Cunningham P, Mellor J, **Britton E**, Salehian S, Mittler J, Marks S, Shadowen H, Guerra L, Barnes AJ, Zhao X. Commonwealth Coordinated Care Plus: Evaluation report 2017–2020. Delivered to Virginia Medicaid and published online. April 2022.
3. Cunningham PJ, Mueller M, **Britton E,** Pham H, Guerra L, Saunders H, Zhao X, Barnes AJ, Dihwa V. Addiction and recovery treatment services: Access, utilization, and quality of care (2016–2019). Delivered to Virginia Medicaid and published online. July 2021.
4. **Britton E**, Cunningham PJ. Diagnosis and treatment of substance use disorders among pregnant women covered by Medicaid. Delivered to Virginia Medicaid and published online. May 2020.
5. Cunningham PJ, Mueller M, **Britton E**. Addiction and Recovery Treatment Services: Access and utilization during the second year (April 2018–March 2019). Delivered to Virginia Medicaid and published online. February 2020.
6. Saunders H, Cunningham PJ, Guerra L, **Britton E**, Barnes AJ. Survey of member experiences with care coordination and health plans. Delivered to Virginia Medicaid. October 2019.
7. Barnes AJ, Snell M, Guerra L, Mueller M, Pham H, **Britton E**, Saunders H, Brooks M, Krist A, Cunningham PJ. Experiences prior to enrollment in Medicaid: New Medicaid expansion members describe health and healthcare experiences from the year before enrolling. Delivered to Virginia Medicaid and published online. October 2019.

INVITED PRESENTATIONS

1. Demographics and health characteristics among Medicaid members enrolled in Commonwealth Coordinated Care Plus in 2020. Virginia Department of Medical Assistance Services. October 2021, virtual.
2. Characteristics of Medicaid members with substance use disorders released from the Department of Corrections in 2020. Virginia Department of Medical Assistance Services. September 2021, virtual.
3. Overdose risk varies by outpatient treatment initiation setting among adult Medicaid members in Virginia. Virginia Department of Medical Assistance Services. February 2021, virtual.
4. Overdose risk varies by outpatient treatment initiation setting among adult Medicaid members in Virginia. Virginia Commonwealth University, Department of Health Behavior and Policy, Research Seminar. January 2021, virtual.

POSTER PRESENTATIONS

1. **Britton E,** Bachireddy C, Martin C, Walker LS, Harrell A, Barnes AJ, Cunningham PJ. Changes in initiation and continuity of pharmacotherapy for opioid use disorder after COVID-19 among Medicaid members in Virginia. AcademyHealth Annual Research Meeting, June 2022, Washington, DC.
2. **Britton E**, Saunders H, Cunningham PJ. Overdose risk varies by outpatient treatment initiation setting among adult Medicaid members in Virginia. AcademyHealth Annual Research Meeting, June 2021, virtual.
3. **Britton E**, Saunders H, Barnes AJ, Walker LS, Montz E, Cunningham PJ. Changes in prenatal substance use disorder treatment following implementation of Virginia Medicaid's Addiction and Recovery Treatment Services program. Addiction Health Services Research, October 2019, Park City, UT.
4. **Britton E**, Cluster detection of uninsured rates among nonelderly adults in the United States, 2016. Virginia Commonwealth University, Department of Statistics, Student Research Presentations, December 2018, Richmond, VA.
5. **Britton E**. Patterns of multimorbidity among nonelderly adults enrolled in the Virginia Coordinated Care program in 2010. Virginia Commonwealth University, Department of Family Medicine and Population Health, Student Research Presentations, May 2015, Richmond, VA.
6. Lu J, **Britton E**, Ferrance J, Rice E, Kuzel A, Dow A. Identifying future high cost individuals within an intermediate cost population. AAMC Health Workforce Research Conference, April 2015, Alexandria, VA.

TEACHING EXPERIENCE

1. Staggered difference-in-differences: Applications in Stata. Virginia Commonwealth University, Research Design and Proposal Preparation (HCPR 732). March 2022.
2. Data visualization. Virginia Commonwealth University, IVY (In Recovery) Lab Research, Teaching, and Research-in-progress meeting. February 2022.

TEACHING EXPERIENCE

3. The role of government in public health. Virginia Commonwealth University, Introduction to Public Health (DENH 411). August 2021.
4. Data literacy and data visualization. Virginia Commonwealth University, Departmental Seminar (SBHD 690/HCPR 699). April 2021.
5. Survey weights and variance estimation: Applications in SAS, Stata, and R. Virginia Commonwealth University, Survey Research Methods (HCPR 730). November 2020.
6. Introduction to survey research: Applications in R. Virginia State University, Data Science for the Public Good Program (DSPG) Workshop. October 2020.
7. Variable construction in SAS and Stata: Demonstration of the Kessler Psychological Distress Scale. Virginia Commonwealth University, Survey Research Methods (HCPR 730). September 2020.
8. Facilitated discussion with incoming Freshmen about *One Person, No Vote*, Virginia Commonwealth University's 2020 Common Book. August 2020.
9. Introduction to Survey Research Methods. Virginia Commonwealth University, Research Process in Occupational Therapy (OCCT 616). April 2020.
10. Survey weights and variance estimation: Applications in SAS and Stata. Virginia Commonwealth University, Survey Research Methods (HCPR 730). November 2019.

SERVICE

2020–present	Social Committee, Department of Health Behavior and Policy, Virginia Commonwealth University.
2019–present	Vice President, AcademyHealth Student Chapter, Virginia Commonwealth University.
2019, 2021	Panelist: Tips for Success in Graduate School, New Student Orientation, Department of Health Behavior and Policy, Virginia Commonwealth University.
2021	Healthcare Policy and Research Academic Program Review, Office of the Provost, Virginia Commonwealth University.
2020	Healthcare Policy and Research Curriculum Committee, Department of Health Behavior and Policy, Virginia Commonwealth University.
2020	Discussion Facilitator, Common Book Program, University College and the Office of the Provost, Virginia Commonwealth University.
2019–2020	Research Symposium Chair, Graduate Student Association, Virginia Commonwealth University.
2016–2017	Practicum Supervisor, Master of Decision Analytics program, Virginia Commonwealth University.
2015	Volunteer, Family Medicine Palliative Care Workshop, Virginia Commonwealth University.

SERVICE

2014–2015 Division of Epidemiology Curriculum Committee, Department of Family Medicine, Virginia Commonwealth University.

AWARDS

2022 Travel Grant, Office of the Provost, Virginia Commonwealth University

2022 Travel Award, School of Medicine, Virginia Commonwealth University

2021 School of Medicine Phi Kappa Phi Scholarship Nomination

2020 Code Red Award, Department Superlative, Department of Health Behavior and Policy

2014 Member, Phi Kappa Phi Honor Society

2011–2012 Academic Achievement Award

FUNDING

Title: Novel, High-Impact Studies Evaluating Health System and Healthcare Professional Responsiveness to COVID-19 01/01/2021–12/31/2023
PI: Rebecca Etz
Funder: Agency for Healthcare Research & Quality

The purpose of this study is to provide an ongoing analysis of the changes in primary care practice and patient outcomes due to the effects of the COVID-19 pandemic.
Role: Quantitative analyst (January 2021–present)

Title: Quick COVID-19 Primary Care Survey 06/01/2020–12/31/2020
PI: Rebecca Etz
Funders: The Morris-Singer Foundation and the Samueli Foundation

The purpose of this study is to better understand and monitor the response, challenges, and capacity of US primary care practices during the COVID-19 pandemic.
Role: Quantitative analyst (April 2020–December 2020)

Title: IAG-407 Appendix N 01/01/2018–06/30/2022
Commonwealth Coordinated Care Plus
PI: Peter Cunningham
Funder: Department of Medical Assistance Services

The purpose of this project is to assess the impacts of Commonwealth Coordinated Care Plus on Medicaid members, patient satisfaction, and the overall costs of services to Virginia's Medicaid program.
Role: Graduate research assistant (August 2018–present)

Title: IAG-407 Appendix K 07/01/2017–06/30/2022
Addiction Recovery and Treatment Services (ARTS)
PI: Peter Cunningham
Funder: Department of Medical Assistance Services

FUNDING

The purpose of this project is to assess the impacts of the Addiction and Recovery Treatment Services (ARTS) benefit by analyzing changes in SUD treatment access, utilization, and costs. Role: Graduate research assistant (August 2018–present)