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# PUBLIC INSURANCE AND PSYCHOTROPIC PRESCRIPTION MEDICATIONS FOR MENTAL ILLNESS

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**ABSTRACT**

Mental illnesses are prevalent in the United States and globally. Cost is a critical barrier to treatment receipt. We study the effects of recent and major eligibility expansions within Medicaid, a public insurance system for the poor in the U.S., on psychotropic prescription medications for mental illness. We estimate differences-in-differences models using administrative data on medications for which Medicaid was a third-party payer over the period 2011 to 2017. Our findings suggest that these expansions increased psychotropic prescriptions by 22.3%. We show that Medicaid, and not patients, financed these prescriptions. For states expanding Medicaid, the total cost of these prescriptions was $30.8M. Expansion effects were experienced across most major mental illness categories and across states with different levels of patient need, system capacity, and expansion scope. We find no evidence that Medicaid expansion reduced a proxy for serious mental illness: suicide.



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## 1. Introduction

Mental illnesses are prevalent in the United States and other developed countries (World Health Organization 2017). For example, in 2015 17.9% of U.S. adults met the diagnostic criteria for any mental illness and 4% met criteria for a serious mental illness (Center for Behavioral Health Statistics and Quality 2016). The American Psychiatric Association (APA), the primary professional organization of psychiatrists in the U.S., defines mental illness as 'health conditions involving changes in thinking, emotion or behavior (or a combination of these' (American Psychiatric Association 2015). Further, according to the APA, mental illnesses are 'associated with distress and/or problems functioning in social, work or family activities'.

Mental illnesses thus impose heavy burdens on afflicted individuals as these illnesses harm overall health, employment, and relationships (World Health Organization 2017). In addition to imposing costs internalized by afflicted individuals, mental illnesses levy costs on broader society (Frank and McGuire 2000). Each year mental illnesses cost the U.S. economy $504B in healthcare expenditures, disability payments, and a less productive work force (Insel 2015).[1] Mental illness prevalence is not homogenous across the population; less advantaged groups are more likely to suffer from such illnesses (World Health Organization 2017). Within the U.S., mental illness prevalence is particularly high among low income and uninsured individuals (Center for Behavioral Health Statistics and Quality 2016).

Although they impose substantial costs, mental illnesses can be effectively treated. For example, primary care providers prescribe psychotropic medications (medications used to treat mental illnesses such as anxiety, depression, and psychosis) and provide brief counseling (Olfson 2016). Additionally, specialty providers (e.g., psychiatrists and psychologists) provide intensive

[1] The authors inflated this number from the original estimate ($467B in 2012 dollars) to 2017 dollars using the Consumer Price Index.

psychopharmacological and psychosocial treatment in outpatient or inpatient settings (Olfson 2016). Despite established treatment efficacy (American Psychiatric Association 2006), there is a substantial amount of unmet need for mental illness treatment in the U.S. According to National Survey on Drug Use and Health (NSDUH) data, in 2015, more than half of U.S. adults who could benefit from mental illness treatment did not receive any treatment (Center for Behavioral Health Statistics and Quality 2016). Unmet need for mental illness treatment is particularly high for uninsured individuals (Garfield et al. 2011). Among those who sought care but did not receive it, the most commonly reported reason for failure to receive care was inability to pay for treatment (Center for Behavioral Health Statistics and Quality 2016). Thus, expanding affordable insurance coverage to low-income, uninsured individuals may remove cost-related barriers to unmet mental illness treatment needs and, in turn, reduce mental illness.

In 2010, the U.S. federal government implemented the Affordable Care Act (ACA). This Act, arguably the most substantial U.S. healthcare legislation in a generation, was designed to address perceived inadequacies within the healthcare delivery system. A primary objective of the Act was to reduce the level of uninsurance. The ACA increased insurance coverage through three levers: premium subsidies for private insurance, mandates that required employers to offer insurance and individuals to hold insurance, and expanded eligibility for Medicaid; a public insurance system that finances healthcare services for the poor (Frean, Gruber, and Sommers 2017). In 2009 the uninsurance rate was 15.4% (Cohen, Martinez, and Ward 2010); by 2017 the rate had fallen to 9% (Zammitti, Cohen, and Martinez 2017). Another objective of the ACA was to mitigate ‘underinsurance’: insurance that provides inadequate coverage of healthcare services. In particular, the ACA required that most insurance plans, including plans for individuals who gained eligibility through Medicaid expansion, cover ten benefit classes, including mental illness treatment and prescription medications (Garfield, Lave, and Donohue 2010). Historically, insurance has covered mental health services less generously than general healthcare services.

We explore the effects of ACA-related Medicaid expansions that occurred between 2011 and 2017 on psychotropic medications prescribed in outpatient settings for which Medicaid was a third-party payer. While we do not directly capture medication use, prescriptions arguably provide a reasonable clinical proxy for such use (Lehmann et al. 2014) and are commonly used as a proxy in economics (Richards et al. 2017; Bradford and Lastrapes 2013). Analyses of pre-ACA data suggest that individuals who gained eligibility through these expansions had elevated need for mental illness treatment (Garfield et al. 2011; Cook et al. 2016), which implies that newly insured populations may benefit from these expansions.

Psychotropic medication treatment is endorsed by providers – in professional practice treatment guidelines these medications are recommended as a component of treatment for most major mental illnesses (American Psychiatric Association 2017) – and common – in 2015, 36.7% of U.S. adults with mental illness used psychotropic medications (Center for Behavioral Health Statistics and Quality 2016). Moreover, access to insurance that covers these medications likely leads to a substantial reduction in out-of-pocket price faced by uninsured individuals seeking treatment; reimbursement rates for mental healthcare providers (e.g., psychologists) range from $67 to $114 per visit (Mark et al. 2017) [2] and thus this modality of care is likely unaffordable for many low-income and uninsured individuals.

We couple administrative data on the universe of prescriptions obtained in outpatient non-specialty settings, and purchased through retail and online pharmacies for which Medicaid was a third-party payer between 2011 and 2017 with differences-in-differences models to

[2] This estimate is derived from commercial insurance claims and may depart from costs faced by an uninsured patient with mental illness.

estimate Medicaid effects. Our findings suggest that, post-expansion, psychotropic prescriptions increased 22.3% in expansion states relative to non-expansion states, and effects increased over time in the post-expansion period. The costs of the new prescriptions to state Medicaid programs were roughly $2.2M per quarter. Effects were relatively homogeneous across psychotropic class and state characteristics that proxy for patient need, system capacity, and expansion scope.

The paper proceeds as follows. Section 2 offers a discussion of Medicaid, with emphasis on the ACA expansions we study, and the related literature. Data, variables, and methods are outlined in Section 3. Results are reported in Section 4. Extensions to the main analysis and robustness checking are reported in Section 5. A discussion is provided in Section 6.

## 2. Medicaid and related literature

### *2.1 Medicaid and ACA-related expansions*

Established in 1965, Medicaid is the primary insurer for low-income families, low-income elderly Medicare beneficiaries, and disabled individuals in the U.S., covering 77 million individuals in 2017 (Sommers and Grabowski 2017). Medicaid is a joint federal-state program. States historically had ample latitude to determine specific eligibility criteria and benefit design within federal standards. Prior to the ACA, most states limited Medicaid eligibility to the disabled and low-income parents; other low-income groups were not eligible for coverage. Medicaid is characterized by low patient cost-sharing and coverage of a relatively expansive list of healthcare services, including mental illness services (Kaiser Family Foundation 2017). Of particular relevance to our study, comparison of plans suggests that Medicaid may provide more generous coverage for mental illness services than many private insurance plans (Garfield, Lave, and Donohue 2010; Rosenbaum et al. 2015).

Beginning in 2014, as part of the ACA, Medicaid was expanded in 31 states and the District of Columbia (as of March, 2018) to cover parents and other non-disabled adults with incomes up to 138% of the Federal Poverty Level [FPL] (Kaiser Commission on Medicaid and the Uninsured 2016).[3] Categorical restrictions were removed. Individuals who gained eligibility through these Medicaid expansions are referred to as 'newly eligible' and are insured by 'expansion' plans that generously cover both mental illness treatment and prescription medications (Garfield, Lave, and Donohue 2010). Originally, the ACA legislated that the Medicaid expansion was to occur nationally. However, in 2012, the U.S. Supreme Court ruled that states would have the discretion whether to expand Medicaid.

Economic theory suggests that Medicaid expansions, by reducing out-of-pocket prices, will increase the quantity of prescriptions demanded by the newly enrolled suffering from mental illnesses (Grossman 1972). Moreover, increased awareness of mental illness treatment and its benefits may occur with Medicaid expansion either through public information campaigns or through connection with general healthcare providers, which may increase demand for psychotropic medications. There are numerous factors that may mute expansion effects. For instance: mental illness and treatment stigma, new patients' unfamiliarity with the healthcare delivery system, limited participation in Medicaid by healthcare providers, well established mental healthcare provider shortages, and so forth (Decker 2012; Center for Behavioral Health Statistics and Quality 2016; Bishop et al. 2014; Thomas et al. 2009). Thus, the extent to which Medicaid expansions lead to increases in psychotropic medication prescriptions is ultimately an empirical question. We attempt to provide empirical evidence on this relationship.

[3] Maine and Virginia have passed legislation to expand Medicaid. At the time of writing an effective date for these expansions has not been determined.

Although we emphasize the newly eligible population in our study, other groups experienced changes in insurance status and eligibility post-ACA. Through 'welcome mat' effects, individuals who were previously eligible enrolled in Medicaid (Frean, Gruber, and Sommers 2017). In all states income eligibility was increased by five percentage points on January 1st 2014; this increase occurred with the federal government's transition to the 'Modified Adjusted Gross Income' [MAGI] criteria for determining program eligibility[4] ('MAGI effects'). For expansion states the post-ACA income threshold is 138% of FPL (133% plus 5 percentage points) and for non-expansion states the post-ACA income threshold is 5 percentage points above the state's Medicaid income threshold in March 2010. These groups are not referred to as newly eligible and are not covered by expansion plans (Garfield, Lave, and Donohue 2010). However, most states covered mental illness treatment in traditional Medicaid plans.

In our main analyses, we leverage variation in Medicaid eligibility for the newly eligible group, but we note the possibility that welcome mat and MAGI effects may differ across expansion and non-expansion states. In extensions, we explore and discuss the extent to which our effects reflect increased prescriptions within the newly eligible population specifically.

A robust and growing literature shows that the ACA Medicaid expansions lead to large decreases in the uninsured rate (Wherry and Miller 2016; Frean, Gruber, and Sommers 2017; Sommers et al. 2015; Miller and Wherry 2017). Moreover, Medicaid expansions increased access to care as measured by having a personal doctor, receiving an annual check-up, and ability to pay for needed treatment (Sommers et al. 2016; Sommers et al. 2017; Miller and Wherry 2017); improved financial security (Hu et al. 2016); and improved health (Simon, Soni,

[4] More details are available at https://www.healthcare.gov/glossary/modified-adjusted-gross-income-magi/ [accessed August 17th, 2017].

and Cawley 2017; Winkelman and Chang 2017; Koma et al. 2017).[5] These expansions did not lead to substantial crowd-out of private insurance (Frean, Gruber, and Sommers 2017; Courtemanche et al. 2017; Kaestner et al. 2017; Wherry and Miller 2016).

*2.2 Medicaid and mental illness*

There is relatively little evidence on the effect of Medicaid expansion on mental illness treatment; especially compared with the volumes literature on general healthcare services. In particular, little is known on the effects of Medicaid expansions on psychotropic medications.

Three studies examine the effects of Medicaid on overall mental illness service use and unmet treatment need using variation afforded by pre-ACA state expansions and offer conflicting findings. Golberstein and Gonzales (2015) find little effect of Medicaid on mental illness service use using the Medical Expenditure Panel Survey. On the other hand, Wen, Druss, and Cummings (2015) utilize NSDUH data to show Medicaid expansions increased the probability of receiving mental illness treatment and reduced reports of unmet treatment need. Similarly, Baicker et al. (2017) leverage a randomized control trial in the state of Oregon and show that individuals randomized to Medicaid were more likely to use psychotropic medications than individuals randomized to the control group.

While these studies are important, they have primarily focused on traditional Medicaid populations and/or the pre-ACA period. Moreover, their findings are mixed.[6] In comparison, we examine expansions to non-traditional populations – low-income and non-disabled adults – in a very recent time period across U.S. states. Moreover, while two of the three above-noted

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[5] We note that not all studies demonstrate self-assessed health gains. See, for example, Courtemanche et al. (2017) and Miller and Wherry (2017).

[6] We suspect that differences in Medicaid expansion modelling may play a role. Golberstein and Gonzales impute Medicaid eligibility following Cutler and Gruber (1996). Wen et al use indicator variables for a Section 1115 waivers. See Hamersma and Kim (2013) for a discussion of approaches to modelling pre-ACA Medicaid expansions. Differences in covariates, time periods, and survey frame may also play a role. Finally, Baiker et al rely on a lottery that occurred in a single state.

studies examine mental illness treatment overall, we examine psychotropic medications. Psychotropic medications filled by physicians and other providers in outpatient and primary care settings can allow individuals to fulfill standard work and family commitments and are likely less stigmatized, therefore are perhaps more appealing to patients than other forms of treatment (e.g., care received in specialty treatment facilities such as a residential rehabilitation center).

To the best of our knowledge, only one study examines the effect of ACA-related Medicaid expansions on mental illness treatment. In an extension to their main analyses of overall prescriptions, Ghosh, Simon, and Sommers (2017), using all payer claims data, find that psychotropic medications increased 19%, post-expansion, in expanding states relative to non-expanding states (overall prescriptions increased by the same percent). Our analysis builds on the Ghosh, Simon, and Sommers (2017) analysis in at least seven important ways. (i) We assess heterogeneity across major classes of psychotropic medications. (ii) We estimate the costs of increases in psychotropic medication and changes in prescribing patterns. (iii) We test the extent to which state Medicaid programs vs. patients assumed the financial responsibility of increased psychotropic medication prescriptions. While Medicaid co-payments are federally regulated to low levels (e.g., a maximum of $4 to $8 for enrollees up to 150% FPL), previous research shows that such cost-sharing can reduce medication use within Medicaid (Soumerai et al. 1991; Soumerai et al. 1987; Abdelgawad and Egbuonu-Davis 2006; Ridley and Axelsen 2006).[7] (iv) We examine how Medicaid effects vary across state characteristics that proxy for patient need, system capacity, and expansion scope. (v) We are able to study a much longer-term expansion effects as we have access to 42 months post-expansion (vs. 15 post-expansion months examined

[7] We note that a small number of states use Waivers that allow higher cost-sharing. On the other hand, several groups (e.g., pregnant women) are exempt from cost-sharing.

by Ghosh and colleagues). (vi) We attempt to isolate effects for the newly eligible population. (vii) We investigate the effects of Medicaid expansion on serious mental illness.

## 3. Data and methods

### *3.1 Prescription medications*

We draw data on Medicaid-financed prescription medications from the State Drug Utilization Database (SDUD). The Centers for Medicaid and Medicare (CMS) compile the SDUD using state data supplied by Medicaid programs. The SDUD includes the universe of outpatient prescription medications purchased at retail and online pharmacies and covered under the Medicaid Drug Rebate Program for which Medicaid serves as a third-party payer (U.S. Department of Health and Human Services 2012). The purpose of the SDUD is to allow the Department of Health and Human Services to determine state and federal rebates from approximately 600 pharmaceutical companies involved in the Medicaid Drug Rebate program.

While the SDUD has included information from fee-for-service since its inception, data on prescriptions financed by managed care plans were added to the SDUD in March 2010 (U.S. Department of Health and Human Services 2012). We use data from 2011 onward to examine both fee-for-service and managed care given the movement toward managed care within Medicaid over time (Hurley and Somers 2003). For example, in most expansion states over 80% of newly eligible enrollees are enrolled in managed care plans and in some expansion states the share is over 90% (Paradise 2017).[8]

We use SDUD data in all quarters between Q1 2011 and Q2 2017, yielding 24 periods of data for each state and DC: 12 periods pre-2014 and 14 periods post-2014. We exclude Kansas

[8] In our main analysis, for states that have both types of plans we sum across fee-for-service and managed care plans to construct our prescription count variable. If a state offers just one type of plan (fee-for-service or managed care) we simply sum across that plan. More details are available on request.

and Rhode Island due to odd missing data patterns. We study overall prescriptions for medications with indications for mental illnesses, and consider heterogeneity across major psychotropic groups: anti-depressants, anti-anxiety medications, anti-psychotics, mood stabilizers, and stimulants.[9] Medications are listed in Table 1.

To form the set of medications to examine, we first use medications provided by the National Institute of Mental Health to identify the medications in each psychotropic class. Next we refer to each medications' Medline webpage to broaden the list of included medications. Only medications with Food and Drug Administration (FDA) indicators for treatment of adult mental illness are included in our analyses.[10] We identify medications in the SDUD with crosswalks between National Drug Codes (Roth 2017).

We note that this list does not provide a complete enumeration of all psychotropic medications used to treat mental illness. We argue that the selected medications reflect a substantial share of medications plausibly available to a Medicaid patient. Our medication selection was further informed by one of the authors who is a practicing psychiatrist. Further, a limitation of studying psychotropic medications specifically, and prescription medications generally, is that some medications are used for treatment of other health conditions. For example, Zyban, a medication used to treat depression, is also used to treat smoking cessation (Maclean, Pesko, and Hill 2017).

*3.2 Medicaid expansions*

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[9] Anti-depressants are indicated for depression treatment (e.g., major depressive disorder), anti-anxiety medications are indicated for anxiety treatment (e.g., generalized anxiety, obsessive compulsive disorder, panic disorder), anti-psychotics are indicated for treatment of psychosis (e.g., schizophrenia), mood stabilizer medications are indicated for treatment of mood disorders (e.g., bipolar disorder), and stimulants are indicated for treatment of attention-deficit hyperactivity disorder (ADHD).

[10] https://www.nimh.nih.gov/health/topics/mental-health-medications/index.shtml (accessed May 5th, 2017).

Our classification of expansion states and expansion dates follows Maclean, Pesko, and Hill (2017) and is listed in Table 2. The majority of states expanded Medicaid on January 1st, 2014. Two states expanded later in 2014 (Michigan, New Hampshire). Five states expanded in 2015 or 2016 (Alaska, Indiana, Louisiana, Montana, Pennsylvania). Prior to 2011, four states (Delaware, Massachusetts, New York, Vermont) and the District of Columbia expanded Medicaid eligibility to cover parents and childless adults with full benefits through 100% FPL or higher, and continued to enroll new beneficiaries. We code these states as treated in all periods. We match Medicaid expansion dates to the SDUD by state, year, and quarter. For within-quarter expansions, we code the first quarter in which the expansion was in place.

*3.3 Outcomes*

We construct the number Medicaid-financed prescriptions for psychotropic medications per 100,000 18-64 year olds in the state. We use data from the U.S. Census on population and the Current Population Survey (CPS) on age (Flood et al. 2017). This population is the target of ACA-related Medicaid expansions (Frean, Gruber, and Sommers 2017).

*3.4 Empirical model*

Our differences-in-differences (DD) model is specified in Equation (1):

(1) $$P_{st} = \alpha_0 + \alpha_1 Ex_{st} + \alpha_2' X_{st} + S_s + \tau_t + \Omega_{st} + \varepsilon_{st}$$

$P_{st}$ is the psychotropic prescription rate in state *s* in state and in year/quarter (‘period’) *t*. $Ex_{st}$ is an indicator for whether or not a state has expanded its Medicaid program in period *t*. $X_{st}$ is a vector of time-varying state characteristics from the CPS: unemployment rate among non-elderly adults and demographics (age, sex, race, ethnicity, non-U.S. birth, and education). $S_s$ and $\tau_t$ are vectors of period fixed effects. State fixed effects control for time-invariant state factors while period fixed effects control for national trends in prescriptions. We also include state-specific linear time trends ($\Omega_{st}$). $\varepsilon_{st}$ is the error term. We use unweighted OLS and report 95% confidence intervals that account for within-state clustering.

*3.5 Internal validity testing*

A necessary assumption for the DD model to recover causal estimates is that the treatment and comparison groups would have followed the same trend in the post-treatment period, had the treatment states not been treated. This assumption is untestable as expansion states did expand Medicaid and thus we cannot observe these states in the untreated state post-expansion. We attempt to provide suggestive evidence on this assumption in two ways.

(i) We examine unadjusted trends in the pre-expansion period in our outcome variables for the treatment group and 2011-2013 for the comparison group. If we find that the outcomes appear to have trended similarly in the pre-treatment period across these groups, such trends provide suggestive evidence that the SDUD data satisfy the parallel trends assumption. (ii) Using pre-Medicaid expansion data for each expanding state and 2011-2013 data for non-expanding states,[11] we estimate the OLS regression model outlined in Equation (2):

(2) $$M_{st} = \gamma_0 + \gamma_1 Treat_s * Trend_t + \beta_2' X_{st} + S_s + \tau_t + \epsilon_{st}$$

$Treat_s * Trend_t$ is an interaction between the treatment group indicator and a linear time trend.[12] If we cannot reject the null hypothesis that $\gamma_1$ is zero, this finding provides support that our data satisfy the parallel trends assumption. States with substantial expansions prior to 2011 are excluded from validity tests (see Table 2).

## 4. Results

*4.1 Summary statistics*

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[11] We use 2011-2013 data for states that expanded Medicaid after January 1st, 2014 in validity testing (see Table 2).
[12] We do not include state-specific linear time trends in Equation (2) because these would be perfectly collinear with our main interaction testing for pre-expansion parallel trends between expanding and non-expanding states.

Table 3 reports summary statistics for expansion and non-expansion states in the period 2011-2013; states with substantial expansions before 2011 are excluded. In expansion states the quarterly number of psychotropic prescriptions per 100,000 non-elderly adults was 9,731 and in non-expansion states the number was 9,068. Turning to control variables, there were clear level differences between expanding and non-expanding states in the pre-expansion period; e.g., lower unemployment rates in expanding states. We control for these variables in all regressions.

*4.2 Internal validity testing*

Figure 1 reports graphical analysis of trends in outcomes aggregated to the quarter-treatment level; we exclude states that expand prior to January 1st, 2014. Psychotropic prescriptions in treatment and comparison states moved broadly in parallel pre-expansion. Post-expansion, psychotropic prescriptions increased in expanding states relative to non-expanding states, and differences between the groups grew over time.

Table 4 reports regression-based parallel trends testing. We cannot reject the null hypothesis that the treatment and comparison groups followed the same trend in psychotropic prescriptions pre-expansion: $\hat{\gamma}_1$ is not statistically different from zero and is small in magnitude (-30 or -0.3% relative to the pre-ACA mean in expansion states). 95% confidence intervals are relatively tight and allow us to rule out pre-expansion trend differences between expansion and non-expansion states of -1.3% and +0.7% (relative to the pre-ACA expansion state mean).

*4.3 Differences-in-differences regressions: Prescriptions*

Table 5 reports the main DD results. Post-expansion, psychotropic prescriptions increased by 2,174 per quarter per 100,000 non-elderly adults in expanding states relative to non-expanding states, or a 22.3% increase relative to the baseline mean prescription rate in expanding states in the pre-expansion period (9,731).

We next consider heterogeneity by psychotropic class. Relative to other insured individuals, those covered by Medicaid suffer from high rates of mood disorders, anxiety disorders, trauma-based disorders, psychotic disorders, and attention deficit/hyperactivity disorder (ADHD) (Adelmann 2003). Of these illnesses, depressive and anxiety disorders are the most prevalent among Medicaid enrollees, and the most likely to be treated in primary care settings similar to settings that lead to prescriptions captured by the SDUD. Bipolar disorder and psychotic disorders are more likely to be treated in specialty settings, given the higher severity of both clinical symptoms and side effect profiles of mood stabilizers and antipsychotic medications. Primary care providers feel less comfortable than specialty providers to prescribe stimulant medications for the treatment of ADHD given the addictive potential of these medications and the difficulty of accurate ADHD diagnosis in adults (Morrill 2009). The FDA has approved psychotropic medications for all of these categories, and numerous studies confirm their effectiveness (Lieberman 2007; Gaynes et al. 2009; Stein et al. 2006; Fredriksen et al. 2013; Ling et al. 1998). Results are reported in Table 6.

As expected, anti-depressants and anti-anxiety medications were the most common prescriptions in the pre-expansion period: 2,996 and 2,616 per 100,000 per quarter. The quarterly number of prescriptions for anti-psychotic medications, mood stabilizers, and stimulants per 100,000 was 2,061; 1,212; and 847 respectively. Medicaid expansion increased prescriptions for anti-depressants, anti-anxiety, anti-psychotic, and stimulant medications by 1,077 (35.9%), 623 (23.8%), 251 (12.2%), and 146 (17.2%) per 100,000 non-elderly per quarter post-expansion in expansion states relative to non-expansion states. We find no statistically significant evidence that mood stabilizer prescriptions were altered by the expansions, although the coefficient estimate is positive and implies a 6.4% increase. These findings suggest that anti-depressant prescriptions and anti-anxiety medications were more responsive to the expansions than other medication classes. This pattern of results is in line with the fact that these illnesses are relatively common among Medicaid enrollees and are more likely to be treated in non-specialty settings.[13] While the point estimates and relative effect sizes do vary across psychotropic class, we note that confidence intervals surrounding the specific medication point estimates overlap.

*4.4 Differences-in-differences regressions: Prescription costs of Medicaid expansions*

We next construct total and Medicaid payments for the psychotropic medications so that we can study the costs of increased psychotropic medications post-expansion. We also examine the extent to which state Medicaid programs, and not patients, financed the increased prescriptions. We convert payments to 2017 terms using the Consumer Price Index. We consider overall costs of these medications and control for the state population age 18 to 64 years in all payment regressions. To account for dependent variable skewness, we estimate generalized least squares models with a Poisson link selected using a modified Park test for healthcare cost data outlined by Manning and Mullahy (2001).[14] Results are reported in Table 7.

Our cost estimates are overestimated because the SDUD does not include state rebates from pharmaceutical companies participating in the Medicaid Rebate Program. Government estimates just prior to the ACA suggest that states recoup roughly 45% of their payments in the form of rebates (Levinson 2011).[15] Our estimates of the effect of expanding Medicaid on costs can be scaled by this number to reduce measurement error. In addition to rebates paid to states

[13] We note that anti-psychotics have been increasingly used to treat depression in primary care settings (Wright, Eiland, and Lorenz 2013), which may explain the rise in these prescriptions in primary care settings.
[14] Details available on request.
[15] We were not able to locate a more recent estimate. Data on rebates is not easily assessable to the public. More details are available on request.

through the Medicaid Rebate Programs, many states directly negotiate with pharmaceutical companies for supplemental rebates. If states, by expanding Medicaid, were better able to negotiate for supplemental rebates then we will be unable to separate expansion effects (e.g., increased utilization of services) from negotiation effects (e.g., higher rebates).

We find that, post-expansion, total pre-rebate payments increased by $4.2M per quarter in expansion states relative to non-expansion states; Medicaid pre-rebate payments increased by $4.0M. 95% confidence surrounding these point estimates overlap. Compared to the pre-ACA mean in expansion states, these estimates translate to a 9.7% increase in total payments and a 9.4% increase in Medicaid payments. If we assume that state Medicaid programs received rebates that accounted for 45% of psychotropic payments, then the corrected increase in Medicaid payments attributable to the expansion was $2.2M per quarter. Given the similarity in the total and Medicaid coefficient estimates, we assume that Medicaid, and not patients, provided the vast majority of the expansion-attributable prescriptions. This finding is arguably not surprising given the federally regulated low cost-sharing levels in Medicaid.

The estimated relative increase in prescriptions (22.3%) is larger than the estimated relative increase in payments (9.4%). For comparison, by the end of our study period (Q2 2017), total Medicaid enrollment increased by 30% over the pre-ACA baseline (Kaiser Family Foundation 2018). To explore whether this difference in effect size is driven by prescribing patterns, we estimate two auxiliary regressions using Equation (1): (i) the ratio of total payments to the number of pills dispenses (a proxy for price) and (ii) the number of pills per prescription (a proxy for dosage). The purpose of estimating these regressions is to examine whether Medicaid expansion leads to changes in the types of medications prescribed (e.g., less costly medications) and/or doses (e.g., more pills per prescription). We report results in Table 8.

We find no statistically significant evidence that Medicaid expansion lead to changes in these variables in expansion states relative to non-expansion states. While we cannot test this hypothesis as data on supplemental rebates are – to the best of our knowledge – not publically available, we postulate that states ability to better negotiate with pharmaceutical companies for supplemental rebates allows them to offset some of the prescription costs or reduced costs through some other means. Moreover, if we compare the tails of our 95% confidence intervals surrounding these estimates the implied increases are more similar. For example, the bottom tail of the 95% confidence interval surrounding the prescription medication estimate implies a 14.0% increase while the top tail of the 95% confidence interval surrounding the Medicaid payment estimate implies a 16.9% increase in payments.

## 5. Extensions and robustness

We next explore heterogeneity in Medicaid effects across state characteristics, probe the stability of our findings through a number of robustness checks, and investigate the effects of Medicaid expansion on a measure of serious mental illness: suicide.

### *5.1 Heterogeneity across state characteristics*

Medication expansion effects may vary across state features, for example patient need for mental illness treatment, access to primary care, mental illness co-morbidities, and uninsurance. These characteristics plausibly reflect differences in the potential benefits to states from Medicaid expansion and capacity of states' healthcare delivery systems to support a large-scale insurance expansion. Documenting such heterogeneity is important for policymakers in the states that have not expanded Medicaid in determining whether expanding could benefit their constituents and for considering the distributional effects of a large policy shift across states.

We explore such heterogeneity by estimating separate regressions for states at/above and below the national median for (i) prevalence of serious mental illness among adults from the NSDUH, (ii) uninsurance rate among adults 18 to 64 from the American Community Survey (Ruggles et al. 2017),[16] (iii) ratio of primary care doctors to Medicaid beneficiaries using data from the Area Health Resource File and CMS, and (iv) adult smoking rates from the Centers for Disease Control and Prevention's Behavioral Risk Factor Surveillance Survey.[17] We use 2010 data to construct these variables to avoid stratifying our sample on an endogenous variable.

Results are reported in Appendix Tables 1 (treatment need), 2 (uninsurance), 3 (primary care access), and 4 (smoking). While point estimates do vary across samples, 95% confidence intervals overlap, suggesting that the effects of Medicaid expansion were relatively homogenous across state characteristics. For example, in states with above the median mental illness prevalence the increase in prescriptions was 2,297 per quarter and in states with below the median mental illness prevalence the increase was 1,736; but 95% confidence intervals overlap.

*5.2 Policy endogeneity*

State policies are determined by the political economy within the state. An important empirical concern is therefore reverse causality. For example, state legislatures, concerned with rising mental illness or other related factors, may implement policies, such as the decision to expand Medicaid, in an attempt to reverse these trends. Such a phenomena implies that outcomes may induce changes in policies rather than policies inducing changes in outcomes.

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[16] We rely on public use NSDUH data from 2009/2010. Our proxy is: 'Serious mental illness (SMI) is defined as having a diagnosable mental, behavioral, or emotional disorder, other than a developmental or substance use disorder, that met the criteria found in the 4th edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-IV) and resulted in serious functional impairment.'

[17] Smoking is highly correlated with mental illness. Adults with mental illness consume 30% of all cigarettes in the U.S. (Substance Abuse and Mental Health Services Administration 2013) .

We estimate an event study to examine reverse causality following Autor (2003). We include a series of variables for each time period before and after expansion (policy leads and lags) in Equation (1). These lead and lag variables are constructed by interacting each period indicator with an indicator for expansion states.[18] We set Q4 2013 as the index period. State-specific linear time trends are excluded from the event study following Wolfers (2006). In particular, Wolfers notes that models with dynamics (such as the leads and lags in an event study) should not include state-specific trends as such trends can muddle interpretation of the estimates of dynamic effects. We drop states with substantial expansions before 2011. Results are presented graphically in Figure 2. We report the coefficient estimates and associated 95% confidence intervals that account for within-state clustering for each lead/lag.

The event studies do not reveal evidence of reverse causality: the coefficient estimates on the leads are small and imprecise, and alternate in sign. Post-expansion the estimated coefficients are positive and precise. The event study results suggest that increases in prescriptions increased over time. This pattern of results is not surprising as the newly eligible must take up Medicaid and make an appointment with a provider prior to filling a prescription.[19]

*5.3 Weighting*

We estimated unweighted regressions, however, the economics field has not yet reached consensus regarding the use of weights in analyses seeking to estimate causal effects (Angrist and Pischke 2009; Solon, Haider, and Wooldridge 2015). Given the lack of consensus, we re-estimate Equation (1) using the state population ages 18 to 64 years as weights. Weighted results are reported in Appendix Table 5 and are not appreciably different from unweighted results.

[18] Coded one if the state expanded Medicaid during our study period and zero otherwise (Lovenheim 2009).
[19] Another concern in policy analyses is program induced migration (Moffitt 1992). To the best of our knowledge, Medicaid expansion did not induce such behavior (Goodman 2017).

*5.4 Controlling for between-state heterogeneity*

A critical concern in differences-in-differences analyses is adequately accounting for between-state differences. In Equation (1) we use state fixed effects and state-specific linear period time trends. However, this specification imposes a specific structure on the unobservables that may be incorrect. On the other hand, if there are no important state-level unosbervables, then Equation (1) throws away variation that could be used for identification.

We next probe the sensitivity of our results to alternative approaches to controlling for between-state heterogeneity. Specifically, we rely on (i) state- and period-fixed effects, (ii) state-specific quadratic time trends, (iii) state-by-year fixed effects, and (iv) state-specific linear time trends and a larger set of state time-varying characteristics from the CPS (poverty rate,[20] Veteran status, marital status, family size, number of children in the family, work-limiting disability, and any activity limitation). Results are reported in Appendix Table 6 and are not appreciably different from our main findings and 95% confidence intervals generally overlap.

*5.5 Alternative coding schemes for Medicaid expansion*

There are several alternative Medicaid expansion coding schemes used in the literature (Frean, Gruber, and Sommers 2017; Simon, Soni, and Cawley 2017; Wherry and Miller 2016). Our review of these coding schemes suggests that there is little disagreement in terms of what states expanded Medicaid, but rather what states had expanded Medicaid prior to 2014. To assess the extent to which our findings are driven by a particular coding scheme, we also test alternative approaches to coding ACA Medicaid expansion. Specifically, we re-estimate Equation (1) using the following coding schemes: (i) we drop states with substantial pre-2011

[20] We note that there was a change in the CPS income questions over our study period (Czajka and Rosso 2015) which may affect the poverty rate measurement. We have estimated region models without the poverty rate, but with other controls in the long covariate set, and results are very similar.

expansions following Wherry and Miller (2016), and (ii) we use a coding scheme outlined in Maclean and Saloner (2017). We also drop California and re-estimate Equation (1) to ensure that our results are not driven by a large state with a strong pre-ACA Medicaid presence. Results are broadly robust to the use of these alternative coding schemes and are reported in Appendix Table 7. Indeed, 95% confidence intervals surrounding the point estimate overlap

*5.6 Isolating effects for the newly eligible from other Medicaid populations*

Equation (1) relies on variation in states' decision to expand Medicaid to newly eligible populations, but this design may confound newly eligible effects with effects for other groups that enrolled in Medicaid with the ACA (i.e., welcome mat effects and MAGI effects). We next explore whether our main findings are reasonable estimates for the newly eligible.

First, we estimate a triple-difference style model that leverages changes in the newly eligible enrollees using CMS Medicaid enrollment data; we exclude 2017 from this analysis as enrollment data by newly eligible status was not available at the time of writing. We augment Equation (1) with an interaction between an indicator for an expansion state, the post period (this variable varies across expansion states depending on when they expanded Medicaid; for non-expansion states this period is 2014 to 2016), and the newly eligible as a percent of increased enrollment. The newly eligible share of increased enrollment between 2013 and each period is defined as follows:

$$(3) \qquad Percent_{st} = \frac{New_{st}}{Total_{st} - Total_{s,2013}} * 100\%$$

Where $New_{st}$ is the number of newly eligible enrollees in state *s* in period *t*, $Total_{st}$ is the total number of enrollees in state *s* in time *t*, and $Total_{s,2013}$ is the total number of enrollees in state *s* in 2013 (the year before the Medicaid expansion). This variable is coded as zero in all years prior to expansion for expansion states and for non-expansion states in all periods. We constrain

$Percent_{st}$ to lie between 0% and 100% in all years[21] and exclude early expanding states from this analysis. The Medicaid expansion indicator captures the effect of Medicaid expansion on all Medicaid enrollees other than the newly eligible (welcome mat and MAGI effects) and the three-way interaction captures the effect for the newly eligible. We do not include the main effect for $Percent_{st}$ as it is collinear with other variables in the regression. We note that the $Percent_{st}$ is obviously influenced by whether or not a state expands Medicaid, which implies that results generated in this specification are likely vulnerable to over-controlling bias.

Results are reported in Appendix Table 8. The coefficient on the expansion variable, which captures effects for previously eligible and those individuals who gained eligibility through MAGI changes, is 1,011 and implies that prescriptions increased 10.4% per quarter within these groups in expansion states relative to non-expansion states in the post-expansion period. The coefficient estimate on the three-way interaction, which captures effects for the newly eligible, is precise and positive: 12. As a way of interpreting this coefficient estimate, we multiply it by the mean value of $Percent_{st}$ in the post-expansion period among expansion states (74.3%). Based on this calculation, we find that, post-expansion, prescriptions among newly eligible populations increased by 891 per 100,000 non-elderly residents each quarter (9.2%). Given the similarity between the two estimates, we cannot rule out the possibility that increases in prescriptions between the newly eligible and other groups that enrolled in Medicaid due to ACA-related changes are the same. Indeed, the 891 estimate for the newly eligible lies within the 95% confidence for the non-newly eligible. Moreover, examination of the top tail of the 95%

[21] We set $Percent_{st}$ to zero for a few states that had declines in total enrollment, perhaps due to the improving economy over the study period. We capped $Percent_{st}$ at 100%, because, in readily available CMS enrollment summaries, early expansion enrollees were reclassified in 2014 as newly eligible, implying that the newly eligible grew more than total enrollment.

confidence intervals suggest increases of 21.6% and 16.8% for non-newly eligibles and newly eligibles respectively which are in line with our main point estimates.

Next, we focus on Medicaid managed care prescriptions rather than prescriptions from both managed care and fee-for-service programs combined as we do in our main analysis because most newly eligibles are enrolled in managed care plans while traditional populations are more likely to be enrolled in fee-for-service plans (Kaiser Commission on Medicaid and the Uninsured 2016). Over 80% of newly eligible enrollees are covered by managed care in most expansion states, and over 90% in some states (Paradise 2017). We include an interaction between the Medicaid expansion indicator and an indicator for managed care-financed prescriptions and re-estimate Equation (1); with the unit of observation being a state-year-quarter-program (managed care vs. fee-for-service). Sample sizes are not double our main sample size as not all states report managed care and fee-for-service data in all years (e.g., some states do not have managed care programs in some years). Details available on request.

Results are reported in Appendix Table 9. The coefficient estimate on Medicaid expansion is imprecise and wrong-signed (negative). However, the interaction term estimate (expansion interacted with managed care) is positive, precise, and large in magnitude. We interpret these findings to imply that increased prescriptions predominately occurred within managed care Medicaid and not fee-for-service Medicaid. Given that the newly eligible are substantially more likely to be enrolled in managed care, these findings imply that our main effects are likely attributable to the newly eligible.

Considering our approaches to isolate effects for the newly eligible from other populations that enrolled in Medicaid concurrent with the ACA roll-out suggests that our main effects are not fully attributable to other groups. Our findings from these analyses are in line with recent work by Biener, Zuvekas, and Hill (2017). The authors show that, post-expansion, primary care visits increased among newly-eligibles but such visits were not altered among previously eligibles. Of relevance to our work, primary care settings are a likely location from which a psychotropic medication prescription may originate. Overall, we find no evidence to suggest that our main point estimates do not offer a reasonable estimate for the newly eligble.

### *5.7 Mental illness*

Thus far in the analysis we have examined the effect of Medicaid expansion on psychotropic medications and our results provide convincing evidence that prescriptions for these medications increased post-expansion. However, we are also interested in the effects of Medicaid expansion on mental illness. To explore such effects, we turn to the National Vital Statistics System (NVSS). The NVSS provides the universe of deaths in the U.S. and lists the underlying cause of death. We use this information to calculate the quarterly number of suicides among adults 18 to 64 years in each state in our sample between 2011 and 2016 (public use data for 2017 was not available at the time of writing). We convert the number of suicides to the rate per 100,000 non-elderly adults each quarter in each state. We match Medicaid expansion dates to the NVSS using the same procedure outlined for the SDUD. We estimate Equations (1), DD, and (2), parallel trends, in the NVSS. Results are reported in Appendix Table 10. Our parallel trends testing provides suggestive evidence that the NVSS data are able to satisfy the parallel trends assumption. We find no statistically significant evidence that suicides declined in expanding states relative to non-expanding states in the post-expansion period. However, we note that suicides are a relatively rare outcome and that previous investigations into Medicaid effects imply that other measures of mental health have improved post-expansion.

## 6. Discussion

Lower income populations are at elevated risk for mental illness and are less likely to have insurance. Public insurance expansions can allow such populations to obtain insurance coverage and, in turn, receive effective treatment for mental illness. We examine the effect of a large-scale and recent public insurance expansion that covered mental illness services and prescription medications in the U.S. Specifically, we leverage within-state variation in Medicaid eligibility generated by provisions in the ACA over the period 2011 to 2017 to study changes in Medicaid-financed prescriptions for psychotropic medications obtained in outpatient, non-specialty settings.

We find that post-expansion the number of Medicaid-financed psychotropic prescriptions increased by 22.3% in expanding states relative to non-expanding states. For comparison, by the end of our study period (Q2 2017), total Medicaid enrollment increased by 30% relative to the pre-ACA baseline (Kaiser Family Foundation 2018). The increases in prescriptions were observed across nearly all classes of psychotropic medications and effects were relatively homogeneously experienced across state characteristics that proxy for patient need, system capacity, and expansion scope. The costs of increased psychotropic prescriptions for state Medicaid programs were \$2.2M per quarter, which translates to \$30.8M for states that expanded Medicaid January 1st, 2014. Medicaid, and not patients, financed these costs. An event study shows that expansion effects increased with time, suggesting that the newly eligible with mental illness were able to maintain relationships with primary care providers and manage their illnesses. Given cognitive and social challenges faced by many with mental illness, this pattern was not obvious *ex ante*. We provide suggestive evidence that our findings reflect changes in prescribing patterns among the newly eligible and cannot be fully explained by changes in prescribing experienced by other populations that enrolled in Medicaid due to ACA changes more broadly (e.g., welcome mat effects). Finally, we do not find evidence that Medicaid expansion altered suicide rates. However, suicides likely reflect serious mental illness and it may take time for Medicaid to alter such outcomes.

Our findings contribute to the growing literature investigating the effects of the ACA-related Medicaid expansions. In line with previous research we show that these expansions increased use of healthcare services (Ghosh, Simon, and Sommers 2017; Sommers et al. 2016; Miller and Wherry 2017; Wherry and Miller 2016; Maclean, Pesko, and Hill 2017; Wen et al. 2017). Simon, Soni, and Cawley (2017) and Winkelman and Chang (2017) show that self-assessed health, which is believed to capture aspects of mental health (Horn, Maclean, and Strain 2017), improved within expanding states. Our findings suggest that increased access to psychotropic medications may provide evidence on one potential pathway through which the expansion lead to improved self-assessed health. Other factors, for example reduced financial strain (Hu et al. 2016), likely play a role in Medicaid-related mental health promotion as well.

Policymakers and providers have historically been concerned that mental healthcare demand is more price-elastic than other forms of healthcare, suggesting that expanding coverage for these services will lead to unsustainable costs healthcare (Frank and McGuire 2000). Interestingly, within the context of ACA-related Medicaid expansions, we find that increases in psychotropic medication prescriptions were smaller than increases in prescriptions for smoking cessation (36%) and substance use disorder prescriptions (55% to 70%) (Maclean and Saloner 2017; Maclean, Pesko, and Hill 2017; Wen et al. 2017), but were similar to increases in overall prescriptions (19%) (Ghosh, Simon, and Sommers 2017).

Our study has limitations. (i) We lack data on patients and providers, and cannot explore issues such as whether the medicine was prescribed appropriately and if it improved mental illness-related outcomes. (ii) The SDUD does not include manufacturer rebates to states and thus we have error in our payment variables. We have used government estimates on the size of Medicaid rebates to states (U.S. Department of Health and Human Services 2012) to scale our cost estimates which may mitigate some error. (iii) We have information on a single payer.

Our analysis suggests that public insurance expansions allow low-income individuals with mental illnesses to access valuable healthcare services. Reforms that curtail such access could worsen health outcomes for such individuals and, given the established negative externalities associated with mental illness (Insel 2008), have implications for broader society in terms of crime, increased healthcare costs, a less productive workforce, and so forth. Policymakers may wish to consider these costs to society when framing the future of Medicaid.

**Table 1. Psychotropic medications**

| Class: | Medications |
|---|---|
| Antidepressant | Aplenzin, Budeprion, Bupropion, Celexa, Citalopram, Cymbalta, Duloxetine, Effexor, Escitalopram, Fluoxetine, Forfivo, Lexapro, Paroxetine, Paxil, Pexeva, Prozac, Rapiflux, Sarafem, Selfemra, Sertraline, Venlafaxine, Wellbutrin, and Zoloft. |
| Anti-anxiety | Alprazolam, Ativan, Buspar, Buspirone, Clonazepam, Klonopin, Lorazepam, Niravam, and Xanax. |
| Anti-psychotic | Abilify, Aripiprazole, Chlorpromazine, Clozapine, Clozaril, Etrafon, Fazaclo, Fluphenazine, Geodon, Haldol, Haloperidol, Invega, Latuda, Lurasidone, Olanzapine, Paliperidone, Perphenazine, Permitil, Prolixin, Quetiapine, Risperdal, Risperidone, Seroquel, Symbyax, Thorazine, Trilafon, Triavil, Ziprasidone, and Zyprexa. |
| Mood stabilizer | Depakene, Depakote, Divalproex sodium, Eskalith, Lamictal, Lamotrigine, Lithane, Lithium, Lithobid, Stavzor, Valproate sodium, and Valproic acid. |
| Stimulant | Adderall, Amphetamine, Aptensio, Concerta, Dexedrine, Dextroamphetamine, Dextrostat, Lisdexamfetamine, Metadate, Methylin, Methylphenidate, Procentra, Quillichew, Quillivant, Ritalin, and Vyvanse. |

*Notes*: Data source is National Institute of Mental Health: https://www.nimh.nih.gov/health/topics/mental-health-medications/index.shtml and Medline websites (https://www.medline.com/) for specific medications (e.g., Aplenzin) embedded in the website (both websites accessed June 10th, 2017). Overall psychotropic medications include the union of the classes listed in this table. More details available on request from the corresponding author.

**Table 2. State Medicaid eligibility expansions**

| State: | Medicaid expansion date |
|---|---|
| *States with substantial expansions before 2011* | |
| Delaware | Before 2011 |
| District of Columbia | Before 2011 |
| Massachusetts | Before 2011 |
| New York | Before 2011 |
| Vermont | Before 2011 |
| *States with substantial expansions in 2011-2014* | |
| Arizona [a,b] | 1/1/2014 |
| Arkansas | 1/1/2014 |
| California [c] | 1/1/2014 |
| Colorado | 1/1/2014 |
| Connecticut [d] | 1/1/2014 |
| Hawaii [b] | 1/1/2014 |
| Illinois | 1/1/2014 |
| Iowa | 1/1/2014 |
| Kentucky | 1/1/2014 |
| Maryland | 1/1/2014 |
| Michigan | 4/1/2014 |
| Minnesota [d] | 1/1/2014 |
| Nevada | 1/1/2014 |
| New Hampshire | 8/15/2014 |
| New Jersey [d] | 1/1/2014 |
| New Mexico | 1/1/2014 |
| North Dakota | 1/1/2014 |
| Ohio [b] | 1/1/2014 |
| Oregon | 1/1/2014 |
| Rhode Island [b] | 1/1/2014 |
| Washington [e] | 1/1/2014 |
| West Virginia | 1/1/2014 |
| *Late expansion states (post-2014)* | |
| Alaska | 9/1/2015 |
| Indiana | 2/1/2015 |
| Montana [f] | 1/1/2016 |
| Louisiana [f] | 7/1/2016 |
| Pennsylvania | 1/1/2015 |

*Notes*: Medicaid expansion dates derived from Simon et al. (2017). 'Substantial' expansions covered both parents and childless adults up to at least 100% FPL, were open to new enrollees, and had full Medicaid benefits. Maine adopted the Medicaid expansion through a ballot initiative in November 2017, but at the time of writing an effective date for the expansion had not been announced.

[a] Expanded eligibility prior to 2011 but closed to new enrollees in 2011.

[b] Excluded, with Virginia, from the analysis due to data quality issues.

[c] From 2011 through 2013, some but not all California counties expanded eligibility, and income eligibility thresholds varied by county.

[d] Expanded eligibility prior to 2014 but with low eligibility thresholds.

[e] Expanded eligibility prior to 2014 but only to people who had previously enrolled in a state program.

[f] Non-expansion during the entire study period, 2011-2015.

**Table 3. Summary statistics for expansion and non-expansion states: SDUD 2011-2013**

| Sample: | Expansion states | Non-expansion states | Difference (*p*-value)* |
|---|---|---|---|
| *Mental illness prescriptions per 100,000* | | | |
| Prescriptions | 9,731 | 9,068 | 0.0516 |
| *State-year level characteristics* | | | |
| Unemployment rate (18 to 64 years) | 0.06 | 0.071 | 0.0053 |
| Age | 37.76 | 37.24 | 0.0001 |
| Female | 0.492 | 0.490 | 0.0109 |
| Male | 0.508 | 0.510 | 0.0109 |
| White | 0.807 | 0.799 | 0.4755 |
| African American | 0.087 | 0.135 | 0.0000 |
| Other race | 0.107 | 0.065 | 0.0000 |
| Hispanic | 0.120 | 0.093 | 0.0050 |
| Born outside the U.S. | 0.106 | 0.077 | 0.0000 |
| Less than high school | 0.324 | 0.340 | 0.0000 |
| High school | 0.237 | 0.238 | 0.7693 |
| Some college | 0.226 | 0.230 | 0.0369 |
| College degree | 0.214 | 0.193 | 0.0000 |
| Observations | 308 | 216 | -- |

*Notes*: Unit of observation is the state-year-quarter. States with substantial expansions before 2011 excluded from the analysis (see Table 2).
*Two-tailed *t*-tests applied.

**Table 4. Parallel trends test for psychotropic medication prescriptions per 100,000 non-elderly: SDUD 2011-2013**

| **Outcome:** | **Prescriptions** |
|---|---|
| *Mean value in expansion states, pre-expansion* | 9,731 |
| Treat * trend | -30 |
| | [-124; 64] |
| Observations | 524 |

*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, and state and period fixed effects. 95% confidence account for within-state clustering and are reported in square brackets. States with substantial expansions before 2011 excluded from the analysis (see Table 2).
***,**,* = statistically different from zero at the 1%,5%,10% level.

**Table 5. Effect of Medicaid expansion on psychotropic medication prescriptions per 100,000 non-elderly using differences-in-differences models: SDUD 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| *Mean value in expansion states, pre-expansion* | 9,731 |
| Expansion | 2,174*** |
| | [1,367; 2,981] |
| Observations | 1,270 |

*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets.
***,**;* = statistically different from zero at the 1%,5%, 10% level.

**Table 6. Effect of Medicaid expansion on psychotropic medication prescriptions per 100,000 non-elderly using differences-in-differences models: SDUD 2011-2017**

| Outcome: | Prescriptions |
|---|---|
| *Mean value for anti-depressant medications in expansion states, pre-expansion* | 2,996 |
| Anti-depressant medications | 1,077***<br>[674; 1,480] |
| *Mean value for anti-anxiety medications in expansion states, pre-expansion* | 2,616 |
| Anti-anxiety medications | 623***<br>[390; 855] |
| *Mean value for anti-psychotic medications in expansion states, pre-expansion* | 2,061 |
| Anti-psychotic medications | 251***<br>[133; 369] |
| *Mean value for mood stabilizer medications in expansion states, pre-expansion* | 1,212 |
| Mood stabilizer medications | 78<br>[-22; 178] |
| *Mean value for stimulant medications in expansion states, pre-expansion* | 847 |
| Stimulant medications | 146***<br>[87; 204] |
| Observations | 1,270 |

*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets.
***,**;* = statistically different from zero at the 1%,5%, 10% level.

**Table 7. Effect of Medicaid expansion on psychotropic medication prescription payments ($1,000s) using differences-in-differences models: SDUD 2011-2017**

| **Outcome:** | **Total payments** | **Medicaid payments** |
|---|---|---|
| *Mean value for all medications in expansion states, pre-expansion* | $43,551 | $42,349 |
| All medications | 4,244*** | 3,992** |
| | [1,131; 7,357] | [817; 7,166] |
| Observations | 1,270 | 1,270 |

*Notes*: Unit of observation is the state-year-quarter. All models estimated with a GLM model and control for state demographics, state population ages 18 to 64 years, state and period fixed effects, and state-specific linear time trends. Average marginal effects reported. 95% confidence account for within-state clustering and are reported in square brackets.
***,**,* = statistically different from zero at the 1%,5%, 10% level.

**Table 8. Effect of Medicaid expansion on psychotropic medication prices using differences-in-differences models: SDUD 2011-2017**

| **Outcome:** | **Price per pill** | **Pills/prescription** |
|---|---|---|
| *Mean value in expansion states, pre-expansion* | $14.78 | 263 |
| Expansion | 1 | -4 |
| | [-1; 3] | [-8; 1] |
| Observations | 1,270 | 1,270 |

*Notes*: Price is defined as the ratio of total payments to pills. Unit of observation is the state-year-quarter. All models estimated with OLS and control for state demographics, state population ages 18 to 64 years, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets.
***,**;* = statistically different from zero at the 1%,5%, 10% level.

**Appendix Table 1. Heterogeneity in Medicaid expansion effects on psychotropic medication prescriptions per 100,000 non-elderly by need for mental illness healthcare using differences-in-differences models: SDUD 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| **Sample: High mental illness care need states** | |
| *Mean value in expansion states, pre-expansion* | 10,327 |
| Expansion | 2,297*** |
| | [1,062; 3,532] |
| Observations | 650 |
| **Sample: Low mental illness care need states** | |
| *Mean value in expansion states, pre-expansion* | 8,893 |
| Expansion | 1,736*** |
| | [744; 2,728] |
| Observations | 620 |

*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets. Need for mental illness treatment calculated using National Survey of Drug Use and Health 2009/2010 state-level data.

***,**,* = statistically different from zero at the 1%,5%, 10% level.

**Appendix Table 2. Heterogeneity in Medicaid expansion effects on psychotropic medication prescriptions per 100,000 non-elderly by uninsurance rate using differences-in-differences models: SDUD 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| **Sample: High uninsurance rate states** | |
| *Mean value in expansion states, pre-expansion* | 9,499 |
| Expansion | 2,525*** |
| | [1,372; 3,677] |
| Observations | 676 |
| **Sample: Low uninsurance rate states** | |
| *Mean value in expansion states, pre-expansion* | 10,010 |
| Expansion | 1,764*** |
| | [612; 2,916] |
| Observations | 594 |

*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets. Uninsurance rates calculated using the American Community Survey 2010 data.

***,**,* = statistically different from zero at the 1%,5%,10% level.

**Appendix Table 3. Heterogeneity in Medicaid expansion effects on psychotropic medication prescriptions per 100,000 non-elderly by access to primary care using differences-in-differences models: SDUD 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| **Sample: High primary care access states** | |
| *Mean value in expansion states, pre-expansion* | 8,634 |
| Expansion | 2,417*** |
| | [1,495; 3,339] |
| Observations | 620 |
| **Sample: Low primary care access states** | |
| *Mean value in expansion states, pre-expansion* | 11,194 |
| Expansion | 1,918*** |
| | [503; 3,332] |
| Observations | 650 |

*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets. Access to primary care calculated using CMS and Area Resource File 2010 data.
***,**,* = statistically different from zero at the 1%,5%, 10% level.

**Appendix Table 4. Heterogeneity in Medicaid expansion effects on psychotropic medication prescriptions per 100,000 non-elderly by smoking status using differences-in-differences models: SDUD 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| **Sample: High smoking rate states** | |
| *Mean value in expansion states, pre-expansion* | 10,722 |
| Expansion | 2,359*** |
| | [1,307; 3,411] |
| Observations | 676 |
| **Sample: Low smoking rate states** | |
| *Mean value in expansion states, pre-expansion* | 8,714 |
| Expansion | 1,715*** |
| | [635; 2,796] |
| Observations | 594 |

*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets. Smoking rates calculated using Behavioral Risk Factor Surveillance Survey 2010 data.
***,**,* = statistically different from zero at the 1%,5%,10% level.

**Appendix Table 5. Effect of Medicaid expansion on psychotropic medication prescriptions per 100,000 non-elderly using differences-in-differences models using population weights: SDUD 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| *Weighted mean value in expansion states, pre-expansion* | 9,287 |
| Expansion | 1,687*** |
| | [906; 2,468] |
| Observations | 1,270 |

*Notes*: State populations ages 18 to 64 years serve as the weights. Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets.
***,**,* = statistically different from zero at the 1%,5%,10% level.

**Appendix Table 6. Effect of Medicaid expansion on psychotropic medication prescriptions per 100,000 non-elderly using differences-in-differences models with different controls for between-state differences: SDUD 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| *Mean value in expansion states, pre-expansion* | 9,731 |
| Model (1) | 3,031*** |
| | [2,013; 4,049] |
| Model (2) | 1,874*** |
| | [1,018; 2,729] |
| Model (3) | 1,624* |
| | [-226; 3,474] |
| Model (4) | 1,935*** |
| | [1,119; 2,751] |
| Observations | 1,270 |

*Notes*: The outcome variable in each regression is the number of prescription fills and refills. Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models estimated with OLS. 95% confidence account for within-state clustering and are reported in square brackets. Model (1) controls for state demographics, and state and period fixed effects. Model (2) controls for state demographics, state and period fixed effects, and state-specific quadratic time trends. Model (3) controls for state demographics, quarter fixed effects, and state-by-year fixed effects. Model (4) controls for an extended set of state demographics (see text), state and period fixed effects, and state-specific linear time trends.
***,**,* = statistically different from zero at the 1%,5%,10% level.

**Appendix Table 7. Effect of Medicaid expansion on psychotropic medication prescriptions per 100,000 non-elderly using differences-in-differences models using alternative Medicaid expansion coding schemes: SDUD 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| *Mean value in expansion states, pre-expansion* | 9,731 |
| Wherry & Miller exclusions | 2,195*** |
| | [1,381; 3,010] |
| Observations | 1,140 |
| *Mean value in expansion states, pre-expansion* | 10,744 |
| Maclean & Saloner coding scheme | 1,718*** |
| | [851; 2,585] |
| Observations | 1,270 |
| *Mean value in expansion states, pre-expansion* | 9,898 |
| Exclude California | 2,184*** |
| | [1,356; 3,013] |
| Observations | 1,244 |

*Notes*: Unit of observation is the state-year-quarter. See text for a discussion of the alternative Medicaid expansion coding schemes. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets.
***,**;* = statistically different from zero at the 1%,5%, 10% level.

**Appendix Table 8. Effect of Medicaid expansion on psychotropic medication prescriptions per 100,000 non-elderly using a triple difference-style estimator: 2011-2016**

| **Outcome:** | **Prescriptions** |
|---|---|
| *Mean value in expansion states, pre-expansion* | 9,731 |
| Expansion * post | 1,011* |
| | [-87; 2,109] |
| Expansion * post * percent | 12** |
| | [2; 22] |
| Mean percent, treatment group, post-expansion period: | 74.27 |
| Observations | 1,052 |

*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets. Expansion = an indicator for expansion in state. Post = an indicator for the period after expansion. Percent = newly eligible enrollees as a percent of Medicaid enrollment increase between 2013 in state *s* and period *t*. States with substantial expansions before 2011 excluded from the analysis (see Table 2).
***,**,* = statistically different from zero at the 1%,5%,10% level.

**Appendix Table 9. Effect of Medicaid expansion on psychotropic medication prescriptions per 100,000 non-elderly using differences-in-differences and allowing and heterogeneity by Medicaid program type: 2011-2017**

| **Outcome:** | **Prescriptions** |
|---|---|
| *Mean value in expansion states, pre-expansion* | 5,947 |
| Expansion | -476 |
| | [-2,005; 1,052] |
| Expansion * managed care program | 4,074*** |
| | [1,130; 7,018] |
| Managed care program | -2,423* |
| | [-4,923 ;77] |
| Observations | 2,114 |

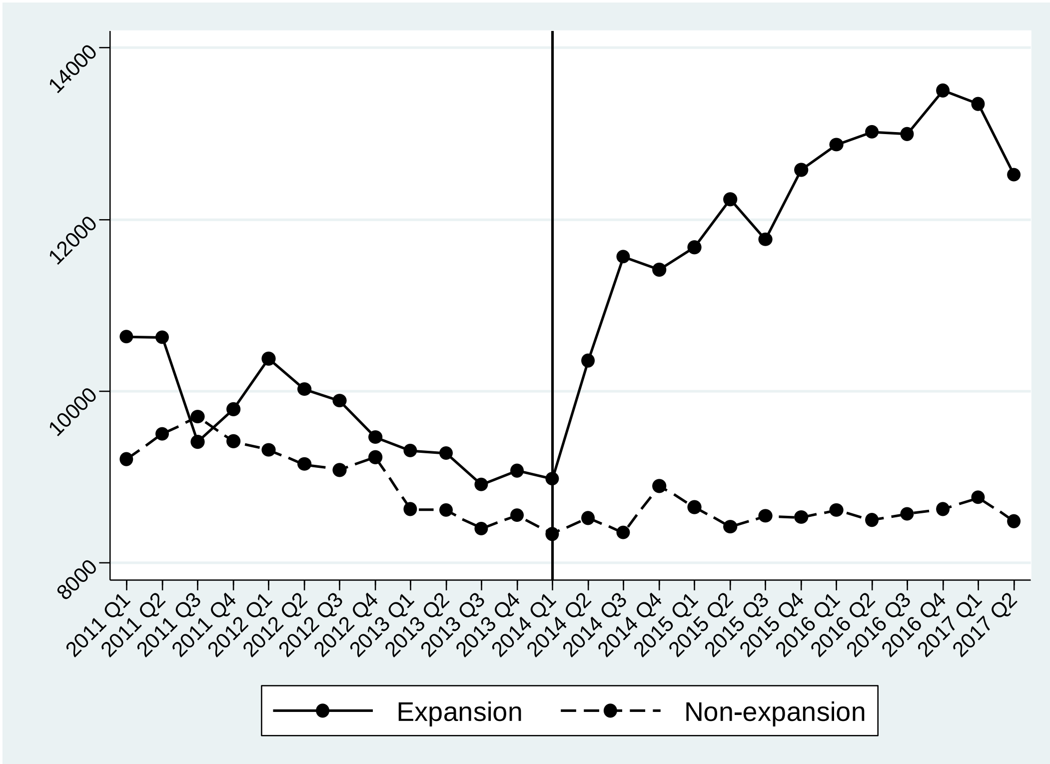
*Notes*: Unit of observation is a state-year-quarter-Medicaid program (managed care vs. fee-for-service). Only observations with both managed care and fee-for-service included in the analysis sample. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

**Appendix Table 10. Effect of Medicaid expansion on suicides per 100,000 non-elderly using differences-in-differences models: NVSS 2011-2016**

| **Outcome:** | **Suicides** |
|---|---|
| *Mean value in expansion states, pre-expansion* | 4.75 |
| Parallel trends test | -0.02 |
| | [-0.05,0.02] |
| Observations | 552 |
| Differences-in-differences | 0.15 |
| | [-0.07,0.37] |
| Observations | 1,224 |

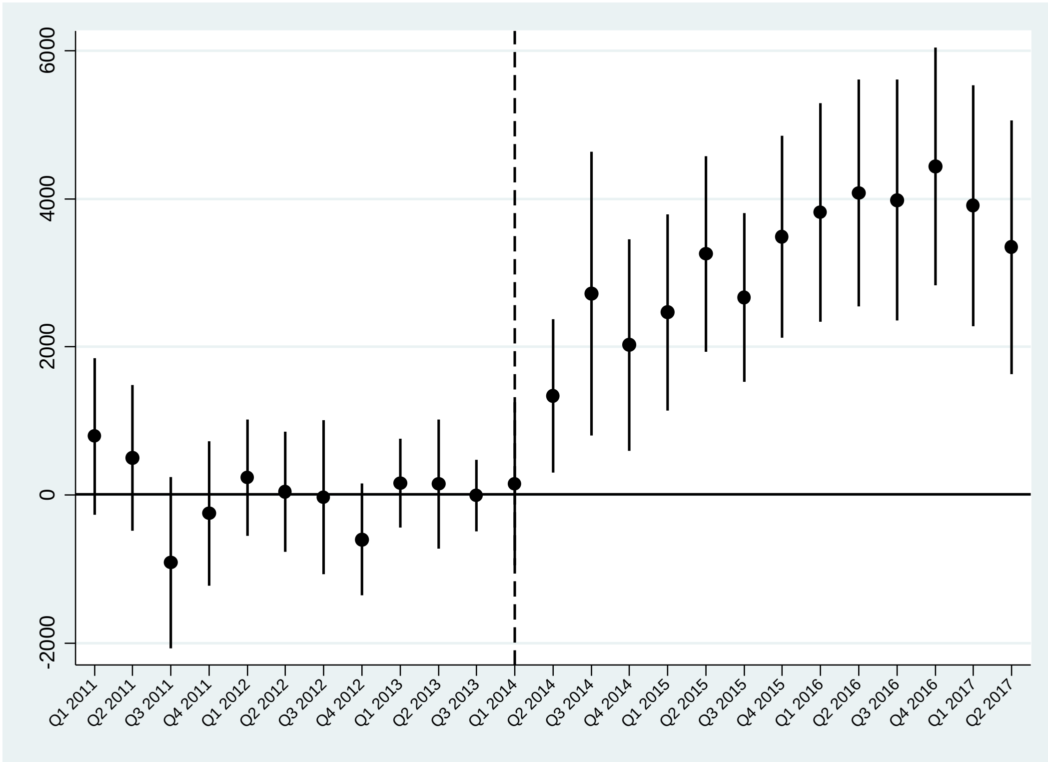
*Notes*: Unit of observation is the state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. All models are estimated with OLS and control for state demographics, state and period fixed effects, and state-specific linear time trends. 95% confidence account for within-state clustering and are reported in square brackets.
***,**;* = statistically different from zero at the 1%,5%, 10% level.

**Figure 1. Trends in all psychotropic medication prescriptions per 100,000 non-elderly in expansion and non-expansion states: SDUD 2011-2017**




*Notes*: Data is aggregated to the treatment-period level.

**Figure 2. Effect of Medicaid expansions on psychotropic medication prescriptions per 100,000 non-elderly using an event study model: SDUD 2011-2017**




*Notes*: Unit of observation is a state-year-quarter. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. Event study dummy variables include each year-quarter cell between Q1 2011 and Q2 2017, the omitted category is Q4 2013. All models are estimated with OLS and control for state demographics, and state and period fixed effects. 95% confidence intervals account for state-level clustering and are reported in vertical bars. States with substantial expansions before 2011 excluded from the analysis (see Table 2). N=1,140.

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