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Johanna Catherine Maclean
Michael F. Pesko
Steven C. Hill

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The Effect of Insurance Expansions on Smoking Cessation Medication Prescriptions: Evidence from ACA Medicaid Expansions
Johanna Catherine Maclean, Michael F. Pesko, and Steven C. Hill
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ABSTRACT

We explore the effects of recent Medicaid expansions on Medicaid-financed prescriptions for evidence-based smoking cessation medications. We estimate differences-in-differences models using administrative data on the universe of prescription medications sold in retail and online pharmacies for which Medicaid was a third-party payer. Our findings suggest that expansions increased smoking cessation prescriptions by 36% with heterogeneity across medication class. We provide evidence that these prescriptions were primarily financed by Medicaid programs and not patients, and that our estimates reflect increases in prescriptions among newly eligible populations and not other populations that enrolled in Medicaid due to Affordable Care Act-related changes. Overall our findings suggest that the recent Medicaid expansions allowed newly insured low-income smokers to access efficacious cessation medications.

Johanna Catherine Maclean
Department of Economics
Temple University
Ritter Annex 869
Philadelphia, PA 19122
and NBER
catherine.maclean@temple.edu

Michael F. Pesko
Department of Economics
Andrew Young School of Policy Studies
Georgia State University
PO Box 3992
Atlanta, GA 30302-3992
mpesko@gsu.edu

Steven C. Hill
Center for Financing, Access and Cost Trends
Agency for Healthcare Research and Quality
540 Gaither Road
Rockville, MD 20850
shill@ahrq.gov

1. Introduction

Smoking is the largest preventable cause of morbidity and mortality in the United States, leading to more than 480,000 deaths each year and accounting for 30% of all cancer deaths annually (Centers for Disease Control and Prevention 2017). Despite the well-established health harms of smoking, the adult smoking rate remains stubbornly high at 15% (National Center for Health Statistics 2017). There is socioeconomic disparity in smoking: lower income groups are more likely to smoke than higher income groups (National Center for Health Statistics 2017). In particular, within Medicaid, a public insurance program that finances healthcare services for low-income individuals, the smoking rate is 30% (National Center for Health Statistics 2017).

This high smoking rate within the Medicaid population is troubling from a public finance perspective as smoking-attributable Medicaid costs (e.g., cancer treatments, chronic bronchitis, and emphysema) will be borne predominately by taxpayers. These costs are non-trivial: smoking-related diseases accounted for 15%, or $45B,[1] of annual Medicaid expenditures between 2006 and 2010 (Xu et al. 2015). Smoking cessation has been shown to both improve health and reduce healthcare expenditures (Centers for Disease Control and Prevention 2010, Warren et al. 2014, Richard, West, and Ku 2012), suggesting that promoting cessation within Medicaid broadly could confer substantial benefits to both government payers and enrollees.

Beginning in 2014, the Affordable Care Act (ACA) provided enhanced federal matching funds for states to expand their Medicaid programs to low-income (up to 138% of the federal poverty level [FPL]), non-elderly, non-disabled adults.[2] For all enrollees, the ACA requires Medicaid programs to cover Food and Drug Administration (FDA)-approved prescription

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[1] Inflated from the original estimate, $39B in 2010 dollars, to 2017 dollars using the Consumer Price Index.
[2] For states that expanded Medicaid, the federal government financed 100% of the costs for newly eligible beneficiaries between 2014 and 2016. After that time, the federal contribution declines to 90% by 2020 and remains at that level (Kaiser Family Foundation 2014).

cessation medications with little to no cost-sharing (DiGiulio et al. 2016). These medications are efficacious (Biazzo et al. 2010, Ruger and Lazar 2012, Zhu et al. 2000, Aubin et al. 2008, Cummings and Hyland 2005, Stead et al. 2012). For instance, a recent meta-analysis found that prescription cessation medications helped 50-80% smokers quit versus a placebo (Cahill et al. 2013). While efficacious, these medications are expensive: prices range from $100 to $500 per prescription for an uninsured smoker.[3] Low income uninsured smokers may have limited ability to access to these medications. Indeed, Lillard et al. (2007) show that uninsured smokers are less likely to use cessation medications in quit attempts than privately insured smokers.

Prior to the ACA, Medicaid eligibility was generally limited to children, poor parents, pregnant women, and the disabled ('traditional populations'). The newly eligible – primarily low-income, non-disabled, and childless adults – have higher smoking rates and less experience with the healthcare delivery system than traditional populations (DiGiulio et al. 2016, National Center for Health Statistics 2014),[4] suggesting that the newly eligible may benefit from coverage obtained through the ACA-related expansions. Recent estimates show that 2.3 million newly eligible smokers gained access to Medicaid through the ACA expansions (DiGiulio et al. 2016), which implies that the expansions have substantial scope to reduce smoking.

We examine the effect of ACA Medicaid eligibility expansions to newly eligible populations on utilization of Medicaid-financed prescription medications for smoking cessation

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[3] We conducted a non-systematic review of online sales of these medications for an uninsured smoker to retrieve our price estimates (e.g., https://www.goodrx.com/zyban; https://www.goodrx.com/chantix; https://www.goodrx.com/nicotrol; accessed April 26th, 2017).

[4] According to the National Center for Health Statistics official estimates, in 2013 (in advance of January 1st, 2014 when the majority of expansion states expanded Medicaid) the Medicaid smoking rate was 30%. This number should predominately reflect the smoking rate among traditional Medicaid eligible populations. DiGuilio and colleagues estimate a smoking rate of 38% among newly eligible enrollees. We note that these estimates are from different sources: the 2013 estimate is based on the National Health Interview Survey and the post-ACA estimate is based on the Centers for Disease Control and Prevention's Behavioral Risk Factor Surveillance Survey combined with the Centers for Medicare and Medicaid Services Medicaid Budget and Expenditure System.

approved by the FDA. To this end, we draw administrative data from the Medicaid State Drug Utilization Database (SDUD) between 2011 and 2015. These data cover the universe of prescription medication claims purchased from retail and online pharmacies for which Medicaid was a third-party payer. While prescription fills do not directly capture medication use, previous research documents that fills are a reliable proxy (Lehmann et al. 2014). Moreover, fills are common proxies within economics (Richards et al. 2017, Ghosh, Simon, and Sommers 2017).

We contribute to the small literature that examines the impact of expanding access to efficacious cessation medications within Medicaid in five important ways. (i) We examine the effect of expanding coverage to newly eligible Medicaid populations, which have very high smoking rates and have had little access to insurance previously. (ii) Our use of the SDUD offers us access to the universe of Medicaid-financed prescription medications sold through retail and online pharmacies, while previous studies have relied on survey data, which are vulnerable to measurement error, or claims data that lack detailed payer information. (iii) Our data contain information on Medicaid payments which we leverage to explore whether changes in prescriptions were financed by Medicaid or patients. (iv) Because the medications we study require a formal prescription from a healthcare provider, we are able to indirectly explore the newly Medicaid-eligible smokers' ability to access primary care services and to navigate the changing U.S. healthcare delivery system, with which they have little experience. (v) We study an important behavioral intermediary outcome: use of prescription cessation medications. The literature on the smoking effects of ACA Medicaid expansions, which has examined self-reported smoking in survey settings, is decidedly mixed (Simon, Soni, and Cawley 2017, Courtemanche et al. 2017a), perhaps because cessation takes time and studies rely on, at most, two years of post-2014 data. If cessation medications are a mechanism through which these insurance expansions assist smokers quit, then we should observe changes in prescriptions prior to changes in reported smoking status. Further, when assessing the value of a large-scale policy intervention such as the ACA, understanding how the intervention affected healthcare service use is important independent of intervention benefits (i.e., cessation).

Our results suggest that expanding Medicaid to newly eligible populations increased utilization of smoking cessation medications: post expansion Medicaid-financed prescription medications for smoking cessation increased by 36% in expansion states relative to non-expansion states with effects driven primarily by Zyban; the lowest cost FDA-approved prescription medication for cessation. A dynamic model documents that effects increased over time, which implies that we may observe more definitive evidence of smoking reductions attributable to Medicaid expansion as time passes. Further, we document that these cessation medications were primarily financed by state Medicaid programs and not patients. Finally, prescription effects appear to be driven by increases in newly eligible populations and not other groups that enrolled in Medicaid due to ACA-related policy changes.

This manuscript is organized as follows. Section 2 provides a review of the Medicaid program, outlines a conceptual model, and discusses the related literature. Data, variables, and methods are presented in Section 3. Main results are reported in Section 4, and extensions and sensitivity analyses are presented in Section 5. Finally, Section 6 provides a discussion.

2. Medicaid, conceptual framework, and prior literature

2.1 The Medicaid program

Medicaid finances healthcare services, including cessation services, for low income people. Medicaid is a federal and state program, with states establishing policies within a set of federal laws. Prior to the ACA, Medicaid eligibility for non-elderly adults in most states was limited to people with disabilities, pregnant women, and parents of poor children.

The ACA originally mandated all states expand Medicaid or lose federal Medicaid funding. In 2012 the Supreme Court ruled that states were not required to expand eligibility to retain federal funding. This ruling left Medicaid expansion optional to states. The decision to expand Medicaid was largely determined by the party controlling the state's lower legislative chamber (Sobel 2014, Courtemanche et al. 2017b).

Starting in 2014, the federal government generously subsidized states' expansions of Medicaid eligibility to most residents with incomes below 138% of the FPL; the threshold is 133% and there is an additional 5 percentage point disregard in both expansion and non-expansion states.[5] By 2017, 32 states, including the District of Columbia, had expanded their Medicaid programs under the ACA, and adults covered under this provision are referred to as 'newly eligible.' For non-expansion states, starting January 1st, 2014, the 5 percentage point disregard ('disregard') increased income eligibility thresholds for traditional groups by 5 percentage points of FPL over the state's March 2010 income thresholds. Populations that gained coverage through this increase are not termed newly eligible.

Prior to the ACA, states had substantial discretion in deciding what services to cover in their Medicaid programs, and coverage of cessation products varied considerably (Singleterry et al. 2014). Starting in January 2014, section 2502 of the Act required states to cover FDA-

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[5] In most states, childless adults of any income level were not eligible for Medicaid prior to the ACA. In early 2010, only 6 states (Arizona, Delaware, Hawaii, Massachusetts, New York, and Vermont) used federal waivers to offer full Medicaid benefits to childless adults, and although 12 states offered limited benefits to this population, many of these latter programs were closed to new applicants (Kaiser Commission on Medicaid and the Uninsured 2009). Using the early expansion option of the ACA, 5 additional states (California, Connecticut, Minnesota, New Jersey and Washington) expanded eligibility to childless adults before 2014. In 2010, the District of Columbia, however, was the only jurisdiction to use the early expansion option to provide for full benefits to 200% of FPL; others had income thresholds below 138% FPL or limited eligibility to adults who were in older state-funded programs.

approved cessation products, both prescription and over-the-counter (OTC) drugs, for all Medicaid enrollees. These products include the prescription cessation medications Zyban/bupropion (‘Zyban’) and Chantix/varenicline (‘Chantix’), and nicotine replacement therapies (NRT) such as patches, gum, lozenges, nasal sprays, and inhalers (Food and Drug Administration 2015). NRTs are available OTC and through formal prescriptions: patches, gum, and lozenges are available OTC while nasal sprays and inhalers require a prescription. The ACA does not regulate utilization management techniques for these medications but the Act does encourage removing cost-sharing (Kaiser Commission on Medicaid and the Uninsured 2012).

The ACA Medicaid changes most relevant to our study are the increased number of enrollees and improved coverage for prescription cessation medications. These changes have implications for a study of ACA Medicaid expansions on smoking among the newly eligible.

Increases in the number of Medicaid enrollees are attributable to coverage increases in three groups: newly eligible adults, previously eligible individuals who opted to take up Medicaid post-expansion; i.e., ‘welcome mat’ effects (Sonier, Boudreaux, and Blewett 2013), and individuals who became eligible in non-expansion states due to the disregard. Our data – administrative counts of Medicaid-financed prescription medications – do not allow us to distinguish between these groups, but we attempt to isolate newly eligible effects.[6] We next review the available evidence and our approaches to isolating newly eligible effects.

Frean, Gruber, and Sommers (2017) show that welcome mat effects are similar across expansion and non-expansion states. Our empirical model (differences-in-differences) can account for common shocks to expansion and non-expansion states through inclusion of period fixed effects. Under the assumption that welcome-mat effects are common across states, we are

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[6] This limitation is common within the ACA-Medicaid expansion literature that relies on administrative or survey data (Simon, Soni, and Cawley 2017, Miller and Wherry 2017, Wherry and Miller 2016, Wen et al. 2017).

able to empirically account for these effects.[7] Because the income disregard changes affect populations at different levels of the income distribution (i.e., within 5 percentage points of the state's March 2010 income eligibility threshold), these effects are not subsumed by the state or period fixed effects included in our differences-in-differences models. Hence, our baseline regression model cannot discriminate between newly eligible and those individuals who became eligible due to the income disregard changes.

To dig deeper into this question, we estimate three extensions to the main analysis. Specifically, we (i) estimate a triple difference-style model that leverages variation in pre-ACA Medicaid total enrollment and post-ACA newly eligible enrollment to isolate effects for the newly-eligible from other groups that enrolled in Medicaid due to the ACA; (ii) separately study managed care (MC) Medicaid financed prescriptions as the newly-eligibles are disproportionally enrolled in MC plans (Kaiser Commission on Medicaid and the Uninsured 2016);[8] and (iii) select a sample of states for which we can separate newly eligible effects from income disregard effects. While each of these explorations of the data has important limitations and caveats, collectively the results generated in these extensions imply that our results are primarily attributable to changes in prescription medications within newly eligible enrollees.

The ACA increased coverage for cessation services in states that had limited coverage of these services before the Act was implemented. For traditional populations, increased coverage for cessation medications plausibly increased service use. These changes, if present, imply that our estimates will capture both increased utilization among the newly eligible enrollees and among traditional enrollees that gained improved coverage for cessation services. We address

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[7] Moreover, Frean, Gruber, and Sommers (2017) show that the ACA Medicaid expansions did not lead to measureable crowd-out of private insurance.

[8] For instance, in 2017 27 expansion states had MC plans and in 25 of these states, 80% or more of the newly eligible were enrolled in MC plans and in 17 states over 90% were enrolled in MC plans (Paradise 2017).

this issue by focusing on states that experienced no change in coverage for these medications during our study period; thus effects in this sample should largely reflect effects for populations that enrolled in Medicaid and not previously eligible populations that gained improved coverage for cessation services. We find that our results are not sensitive to this sample selection, which suggests that our findings are not fully explained by continuously enrolled populations.[9]

In summary, while we cannot fully rule out the possibility that our estimates of newly eligible effects are contaminated by other groups affected by ACA Medicaid changes, we contend that our estimates largely reflect changes among the newly eligibles.

2.2 Conceptual framework

The Grossman (1972) model of the demand for health and healthcare services motivates our study. In the Grossman model consumers do not demand healthcare services *per se*, but instead they demand the health improvements attributable to utilization of such services.

In our study, consumers seeking to improve health by quitting their smoking addiction could plausibly demand smoking cessation prescription medications. Consumers maximize a utility function given the price of healthcare services and other goods, preferences, a health endowment, a health production function, other factors that determine health, and a budget constraint. Consumers are assumed to respond to healthcare price changes in a manner comparable to other goods and services. We investigate the effect of recent Medicaid expansions on demand for smoking cessation medications. Medicaid – by reducing the out-of-pocket price – should increase the quantity of cessation medications demanded by consumers.

[9] A comparable concern is that early expansion states (Table 1) were not required to cover cessation services between their expansion date and January 1st, 2014. As discussed later in the manuscript, we have excluded these states from our analysis and results are not appreciably different (see Table 16).

There are several factors unique to either Medicaid or addictive goods such as cigarettes which may mute price effects. (i) Insurance does not always reduce out-of-pocket prices as many providers do not accept Medicaid, indeed a major concern among ACA policy makers was lack of provider participation in Medicaid (Decker 2012). (iii) *Ex ante* moral hazard suggests that, by lowering smoking-attributable healthcare costs, insurance may incentivize individuals – who no longer face the full costs of their health behaviors – to delay or deter smoking cessation (Klick and Stratmann 2006).[10] (iv) If insurance acts as an in-kind income transfer to the newly eligible, and if smoking is a normal good (Kenkel, Schmeiser, and Urban 2014), the newly eligible may in fact *increase* smoking. (v) Finally, other ACA-related changes (e.g., communication efforts, simplification of the enrollment process) may amplify Medicaid effects through non-price channels (Frean, Gruber, and Sommers 2016).

The question of whether, and to what extent, ACA Medicaid expansions lead to changes in prescription cessation medications is ultimately an empirical question. While our differences-in-differences methods will not allow us to explore these specific pathways, our objective is to provide evidence on the net effect. Moreover, the decision to expand Medicaid is the most direct policy lever under consideration among states.

2.3 Literature

We review literature on the price elasticity of demand for cessation medications and the related Medicaid literature.

While there is a substantial economic literature that estimates the elasticity of demand for cigarettes (Tauras et al. 2016, Maclean, Webber, and Marti 2014, Pesko et al. 2016, Decicca et al. 2008, Chaloupka and Warner 2000, Gallet and List 2003), a relatively small set of studies has

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[10] We note that an *ex post* moral hazard story would suggest that cessation medication use will increase post-expansion in expanding states.

estimated price elasticities for cessation medications. These studies are informative for our work as insurance reduces the out-of-pocket price for cessation medications. Tauras and Chaloupka (2003) estimate that the average own-price elasticity of demand for a nicotine patches and gum were -2.3 and -2.5 respectively, suggesting that smokers' demand for these products is highly elastic. Paterson et al. (2008) and Marti (2012) show, using choice experiments, that smokers were less likely to choose a cessation medication when its price was experimentally increased. While not formally estimating a price elasticity of demand, Keeler et al. (2002) document that when a cessation medication became available OTC, rather than requiring a prescription from a provider which is costly to the consumer, the quantity demanded of this product increased.

Several studies show that pre-ACA Medicaid expansions – which primarily affected traditional populations – increased utilization of cessation medications and reduced smoking. The extent to which these findings can generalize to the ACA's newly eligible adults is unclear. Liu (2009) documents higher smoking cessation rates among non-elderly adult women residing in states with a higher composite index of Medicaid coverage for cessation medications. Liu (2010) shows that Medicaid coverage increased quit attempts while Greene, Sacks, and McMenamin (2014) find that reduced cost-sharing led to higher cessation rates among Medicaid recipients. Adams et al. (2013) and Jarlenski et al. (2014) show that Medicaid participation, and enrollment and coverage policies reduced maternal smoking. Witman (2013) documents that expanded Medicaid coverage reduced smoking by 6% among low-income individuals who report ever smoking. Richards et al. (2017) use insurance claims show that coverage increased utilization by 20 prescriptions for Zyban and Chantix per 10,000 persons.[11]

[11] A limitation of the Richards study is that the authors were not able to explore payer source.

The early literature evaluating the ACA Medicaid expansions finds evidence that prescription medications increased, but no study examines cessation medications.[12] Medicaid-financed prescription utilization increased by 19% in expansion states relative to non-expansion states (Ghosh, Simon, and Sommers 2017). Wen et al. (2017) show that utilization of Medicaid-financed buprenorphine (a medication used to treat opioid use disorder) prescriptions increased by 70% in expansion states relative to non-expansion states. Maclean and Saloner (2017) find that use of Medicaid-financed medications used to treat a wider set of substance use disorders, all FDA-approved medications to treat alcohol and drug use disorders (but not smoking cessation), increased by 43% in expansion states relative to non-expansion states. Finally, Maclean et al. (2017) show that, post-expansion, prescriptions for psychotropic medications used to treat mental illness increased 22% in expanding states relative to non-expanding states. Importantly, these papers do not attempt to isolate effects for the newly eligible and, instead, plausibly capture effects for newly eligible, previously eligible populations that enrolled due to welcome mat effects, and populations that become eligible due to income disregard changes.

The effects of the ACA Medicaid expansions on smoking are mixed which is not entirely surprising as studies rely on just two years of post-2014 data and the transition from smoking to cessation is time-intensive. Longitudinal evidence suggests that a smoker attempts to quit on average 30 times or more before successfully quitting for 1 year or longer (Chaiton et al. 2016). Simon, Soni, and Cawley (2017) find that Medicaid expansion reduced past 30-day smoking among childless non-elderly adults by 6% in expansion states relative to non-expansion states.

[12] Young-Wolff et al. (2017) use a pre-post design to provide evidence that cessation medication use increased post-expansion among Medicaid enrollees in the Kaiser Permanente Northern California system.

Smoking propensities were not altered by the expansion in any other group. Courtemanche et al. (2017a), using the same data,[13] find no evidence that these expansions affected smoking.

3. Data, variables, and methods

3.1 Prescription medication data

We use the Medicaid State Drug Utilization Database (SDUD). The SDUD is compiled by the Centers for Medicaid and Medicare (CMS) from administrative data submitted by state Medicaid programs and include all states' data for outpatient prescription medications (initial fills and refills) 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). Since 1992, state Medicaid programs have been required to submit data on the number and type of prescriptions filled and refilled each quarter to the Centers for Medicare & Medicaid Services (CMS) in exchange for federal matching funds.

We use data from 2011 to 2015 in our study, and have 20 periods of data for each state: 12 pre-2014 and 8 post-2014. While the SDUD has data from fee-for-service (FFS) Medicaid since 1992, prescription fills and refills obtained through managed care (MC) plans were added in March, 2010 (U.S. Department of Health and Human Services 2012). We use data from 2011 onward to ensure that we are able to include both FFS and MC data (Wen et al. 2017).[14] The ability to measure MC data when analyzing the ACA Medicaid expansions is imperative as the majority of the newly eligible are enrolled in MC plans (Kaiser Commission on Medicaid and

[13] Simon et al use a differences-in-differences model comparable to the one we estimate while Courtemanche et al take a third difference based on pre-ACA insurance rates within a local area. We are unable to implement the Courtemanche et al method in our state-level data that focuses on a single payer (Medicaid).

[14] Specifically, we aggregate data from both FFS and MC Medicaid to create a single quarter of data.

the Uninsured 2016). We exclude five states – Arizona, Hawaii, Ohio, Rhode Island, and Virginia – that display odd data patterns.[15]

We focus on prescription medications approved by the FDA for cessation (Food and Drug Administration 2015): Zyban, Chantix, and Nicotrol (an NRT with inhaler and spray versions).[16] We create these categories using crosswalks between National Drug Codes (NDCs) for brand name and generics obtained from the National Bureau of Economic Research (Roth 2017).[17] Our main analysis combines all medications, but we also study each medication separately. There are two main limitations of the SDUD data. (i) They capture aggregate counts of prescription initial fills and refills, and do not provide information on patient or provider characteristics.[18] (ii) They do not include rebates by manufacturers to the states.

3.2 Medicaid expansion

Table 1 summarizes our classification of expansion states. The majority of expansion states implemented their expansion on January 1st, 2014. Two states expanded Medicaid later in 2014 (Michigan and New Hampshire) and five states expanded in 2015 or 2016 (Alaska, Indiana, Louisiana, Montana, and Pennsylvania) and we refer to these states as 'late expansion states'. Prior to 2011, four states (Delaware, Massachusetts, New York, and Vermont) and the District of Columbia substantially expanded their Medicaid eligibility to cover both parents and childless adults with full Medicaid benefits up to 100% FPL or higher and remained open to new

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[15] Including these five states does not change our results in a meaningful way. Details available on request.

[16] Specific medication classifications that we use to identify these medications are available on request from the corresponding author. Zyban can be used for multiple purposes including cessation, depression, attention deficit disorder, obesity, and substance use (https://www.fda.gov/downloads/drugs/drugsafety/ucm089835.pdf (accessed March 31st, 2017). We wish to focus on Zyban that is prescribed to aid cessation. Thus, in our main analyses we include the following medications: proprietary name Zyban, Buproban, or bupropion, extended release, with the strength 150mg. In unreported analyses, we have applied more inclusive definitions which, we suspect, include Zyban prescriptions for other purposes. Results, available on request, are comparable.

[17] http://www.nber.org/data/national-drug-code-data-ndc.html (accessed April 14th, 2017).

[18] Thus we cannot study the number unique prescription users, because some patients may try more than one medication. However, we are interested in overall changes in service use.

enrollees. The SDUD data are available at the quarter level, and thus we match Medicaid expansion dates to this dataset based on state-year-quarter. Our classification closely follows Wherry and Miller (2016), and Simon, Soni, and Cawley (2017).

3.3 Outcome variables

We construct the number of prescriptions filled and refilled for all three medications.[19] We construct total payments and Medicaid and non-Medicaid payments, which allows us to test whether Medicaid or patients financed medications. We convert payments to 2015 dollars using a healthcare cost Gross Domestic Product (GDP) deflator (Dunn, Grosse, and Zuvekas 2018). For each state, we divide each outcome variable by the total population between the ages of 18 to 64 years age (the group most likely to be affected by ACA Medicaid expansions (Simon, Soni, and Cawley 2017)) of age using data drawn from the American Community Survey (ACS) (Flood et al. 2017) and the University of Kentucky Center for Poverty Research Center (2016).[20]

3.4 Control variables

We control for a range of state-level variables in our regressions. First, we merge in tobacco cigarette and electronic cigarette regulations: taxes and bans on use in public places (restaurants, bars, and private worksites) from the Centers for Disease Control and Prevention (2016) STATE System. These variables reflect the costs of related goods. We use the tax in dollars per package of 20 tobacco cigarettes and an indicator for an e-cigarette tax. For public

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[19] The specific NDCs used for these outcomes are available on request.

[20] We could have standardized our outcomes by the number of Medicaid enrollees. However, we chose not to use this standardization as the number of enrollees is clearly influenced by a states' decision to expand Medicaid. Thus, we would have an endogenous numerator and denominator which complicates interpretation of our regression coefficients as there would be two changing objects (cessation medications and enrollees).

use bans, we construct separate variables for tobacco cigarettes and e-cigarettes that capture the count of the number of venues in which a state imposes a ban on product use.

Second, we merge in data on state-level demographics from the Annual and Social and Economic Supplement to the Current Population Survey (CPS): average age, sex, race, Hispanic ethnicity, and education. Third, we link the annual seasonally adjusted unemployment rate in the state from the Bureau of Labor Statistics Local Area Unemployment Database and the poverty rate (University of Kentucky Center for Poverty Research Center 2016) to the SDUD.

Fourth, we control for state social policies targeting lower income populations. To this end, we turn to the University of Kentucky Center for Poverty Research Center (2016) and we control for the maximum Temporary Assistance for Needy Families (TANF) benefit for a family of four, the maximum Supplementary Nutrition Assistance Program (SNAP) benefit for a family of four, and the effective state minimum wage (i.e., the higher of the federal or state minimum wage). We also include an indicator for a Democratic governor; we treat the mayor of the District of Columbia as the *de facto* governor of this locality (Maclean and Saloner 2018). We convert all financial variables to constant 2015 dollars using the above-noted GDP deflator.[21]

3.5 Empirical model

Our differences-in-differences model is outlined in Equation (1):

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

C_{st} is the number of prescription fills and refills in state *s* in period *t*. $Expand_{st}$ is an indicator for whether or not a state has expanded its Medicaid program under the ACA in quarter *t*. X_{st} is a vector of state-year level characteristics. S_s and τ_t are vectors of state and year-by-quarter fixed effects. Inclusion of state fixed effects controls for time-invariant state-level unobservable

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[21] We do not attempt to control for state-year smoking rates, a determinant of demand for smoking cessation, because smoking rates are plausibly endogenous to the policy we study (Simon, Soni, and Cawley 2017).

factors and implies that our models are identified off within-state variation in Medicaid expansions. Year-by-quarter fixed effects control for secular trends in cessation medication use that affect the nation as a whole (e.g., national anti-smoking campaigns) and ACA-related changes that plausibly apply to all states (e.g., welcome-mat effects). We also include a vector of state-specific linear time trends (Ω_{st}). ε_{st} is the error term.

We estimate regressions using unweighted OLS and cluster standard errors around the state (Bertrand, Duflo, and Mullainathan 2004). The 46 clusters (after dropping 5 states with missing data) allow us to consistently estimate standard errors (Cameron and Miller 2015).

3.6 Validity

A necessary assumption for the DD model to recover causal estimates is that the treatment group (i.e., states with substantial expansions) and the comparison group (i.e., states without expansions) would follow the same trend in the post-treatment period, had the treatment states not been treated. However, this assumption is inherently untestable. We instead attempt to provide suggestive evidence on this assumption in two ways.

(i) We examine unadjusted trends in the pre-treatment period in our outcome variables for the treatment and comparison groups. If we find that the outcomes appear to trend 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 only pre-treatment data, we estimate regression models similar to Equation (1), except that we replace the $Expand_{st}$ variable with an interaction between the treatment group ($Treat_s$) and a linear time trend ($Time_t$) Akosa Antwi, Moriya, and Simon (2013). The model is presented in Equation (2):

(2) $$C_{st} = \gamma_0 + \gamma_1 Treat_s * Time_t + \gamma_2' X_{st} + S_s + \tau_t + \mu_{st}$$

If we cannot reject the null hypothesis that γ_1 is zero, then this finding provides further support that our SDUD data can satisfy the parallel trends assumption. In both validity tests, we exclude states with substantial expansions before 2011.

4. Results

4.1 Summary statistics

Table 2 reports summary statistics for both expansion and non-expansion states in the period 2011 to 2013. States with substantial expansions before 2011 are excluded from this comparison. We also report *p*-values from *t*-tests assessing the statistical significance of the differences between the two groups. The annual number of cessation medication prescription fills and refills per 100,000 non-elderly adults was 250 per quarter in expansion states and 208 per quarter in non-expansion states. While not identical, the two groups of states are generally similar across observed characteristics. However, there are some notable exceptions. On average, expansion states had higher tobacco cigarette taxes, more venue-specific smoking bans, lower poverty rates, and more generous social policies (e.g., higher effective minimum wages) and were more likely to have a Democrat governor than non-expansion states.

4.2 Validity

Figure 1 provides graphical analysis of trends in cessation prescription medication fills and refills over our study period. We aggregate the data to the year-treatment level to smooth out noise. In 2011-2013, prescription fills and refills appear to have moved broadly in parallel in the expansion and non-expansion states, which supports the hypothesis that the SDUD data satisfy the parallel trends assumption. However, in 2014-2015, we observe that the expansion and non-expansion states appeared to follow different trends. Overall, expansion states were relatively stable in terms of the number of prescription fills and refills reimbursed by Medicaid in the first and second year of the treatment, while non-expansion states appeared to trend *downwards* in reimbursed fills and refills in the second year of the treatment.

We report regression-based parallel trends testing in the left-hand panel of Table 3. We cannot reject the null hypothesis that expansion states and non-expansion states followed similar trends in prescription fills and refills in 2011-2013: $\hat{\gamma}_1$ is not statistically different from zero. Moreover, the estimate of γ_1 are is small and relatively precise, which allows us to rule out moderate violations of the parallel trends assumption in 2011-2013. In particular, the estimate of γ_1 is 2, relative to a baseline mean of 250, and the standard error estimate is 2.

4.3 Regression results

Table 3 (right-hand panel) reports our main DD results for our outcome variables. Post expansion, we find that expansion states experienced an increase of 89 prescription fills and refills per 100,000 non-elderly adults per quarter relative to non-expansion states. Compared to the baseline mean in expansion states before expansion, this estimate reflects a 36% increase.

We also consider payments – total, Medicaid, and non-Medicaid – for these prescriptions. As noted earlier in the manuscript, an important limitation of the SDUD is that it does not include rebates that Medicaid programs receive from pharmaceutical companies. This limitation is particularly important if expanding Medicaid allowed expansion states to bargain for higher rebates due to increased market power. However, even given this data limitation, we contend that this exercise is informative for studying whether Medicaid or patients carried the financial responsibility for the increase in prescription medications documented in Table 3.[22]

Results are reported in Table 4; the top panel reports regression-based testing of the parallel trends assumption while the bottom panel reports estimates generated in our DD models.

[22] For example, we contend that it is unlikely that expanding Medicaid allowed state Medicaid programs to negotiate rebates from pharmaceutical companies that fully offset payments that we observe in the SDUD.

Total payments per quarter on these medications was $15,375 per 100,000 in expansion states. Medicaid paid the vast majority – 99% in expansion states– of the costs of FDA-approved smoking cessation medications. The financial responsibility of Medicaid is perhaps not surprising as the program is characterized by low patient cost-sharing.

Results suggest that the SDUD satisfy the parallel trends assumption: coefficient estimates in the top panel are small in magnitude and imprecise in the total and Medicaid payments regressions. We find evidence that non-Medicaid payments were *increasing* 2011-2013 in expansion states relative to non-expansion states, thus we are cautious when interpreting DD results for this outcome. However, we include state-specific linear time trends in our DD models which can account for some (linear) differences in pre-trends.

Turning to estimates generated in our DD model, we observe that in expansion states relative to non-expansion states, total payments on these medications per 100,000 per quarter increased by $4,241, or 28% after the expansion. Estimates for Medicaid payments are similar: post expansion Medicaid payments rose by $4,295 per 100,000 non-elderly adults per quarter, or 28%, in expansion states relative to non-expansion states. The estimate in the non-Medicaid payment regression is large in magnitude (-$54) but imprecise. Collectively, our findings from the SDUD show increased cessation prescription use among the newly insured and that the newly insured were sheltered from bearing the full financial responsibility of these prescriptions.

There are two somewhat surprising findings generated in our payments analysis. First, the relative increase in total and Medicaid payments is greater than the relative increase in prescriptions. However, examination of the lower (upper) tail of the prescription medication (total/Medicaid payment) 95% confidence interval suggests that the two sets of variables experienced more analogous changes post-expansion in expanding states relative to non-expanding states. The lower tail of the prescription medication 95% confidence interval suggests a 21% increase and the upper tail of the total payments 95% confidence interval suggests a 49% increase. Thus, we cannot rule out the hypothesis that changes in prescriptions and payments were comparable post-expansion. Second, Medicaid payments increased by $54 more than total payments. 95% confidence intervals for these estimates overlap, and therefore we cannot rule out the possibility that total payments increased more than Medicaid payments.

5. Extensions and sensitivity analyses

We estimate several extensions to the main analyses and conduct sensitivity analyses.

5.1 Heterogeneity by medication

The three medications in our analyses vary in how much state Medicaid programs pay pharmacies – the average cost for Zyban, Chantix, and Nicotrol is $42, $191, and $235, respectively, per fill or refill in the SDUD during our study period. The medications also have different efficacy and side effects (Food and Drug Administration 2015). Such differences may lead to different effects of Medicaid expansion on prescriptions. We next explore heterogeneity by estimating separate regressions for each medication. Results are reported in Table 5.

Before we proceed to our heterogeneity analysis, we provide suggestive evidence that the SDUD can satisfy the parallel trends assumption for each cessation medication separately. We are unable to reject the null hypothesis that the expansion states and non-expansion states followed the same trend in these outcomes in the pre-expansion period. Turning to our DD estimates, we find that, post-expansion, Zyban prescription fills and refills increased 40% in expansion states relative to non-expansion states. We find no statistically significant evidence that prescription fills or refills for Chantix or Nicotrol increased in expansion states.

While our data do not allow us to explore the reasons behind the differential effects by cessation medication type, there are several possible reasons for this pattern of results. First, newly eligible smoking patients may be directed, perhaps through the use of differential co-payments or other forms of utilization management, toward less costly medications (Zyban) and away from more expensive medications (Chantix and Nictorol). Given the clinical literature does not suggest that these medications differ substantially in terms of their efficacy, this pattern of results could imply that the newly eligible are being treated in a cost-effective manner. Second, despite our efforts to focus on Zyban for smoking cessation rather than bupropion for depression, it is possible that some of the increase in our Zyban measure reflects it is use as an antidepressant (Maclean et al. 2017). Other possibilities include patient preferences, provider preferences for, or knowledge of, medications, or some other factor.

5.2 Event study

We estimate an event study to explore the possibility of policy endogeneity (Autor 2003). More specifically, we augment Equation (1) with a series interactions between period indicators for Q1 2011 through Q4 2015, and an indicator for expansion states (policy leads and lags). Q4 2013 is the omitted period in the event study. We omit the state-specific linear time trends following Wolfers (2006). We exclude states with substantial expansions before 2011.

We report event study results graphically in Figure 2; specific coefficient and standard error estimates are reported in Appendix Table 1. The event study findings are in line with estimates generated in the DD regression models. The coefficient estimates are small in magnitude and generally imprecise (95% confidence intervals include zero) before 2014. Coefficients become positive and precise in Q1 2014, and increase in magnitude over time. The increasing effect sizes in the post-period are in line with the hypothesis that patients are better able to access services over time, perhaps as they become aware of their benefits and/or are able to navigate the healthcare delivery system (e.g., enroll in Medicaid, locate a physician, schedule an appointment, and fill a prescription).[23] These increasing effect sizes over time suggests that cessation will also increases as time passes. Given that the ACA Medicaid literature on smoking effects is decidedly mixed (Simon, Soni, and Cawley 2017, Courtemanche et al. 2017a), our findings for a mechanism through which insurance may affect smoking is informative.

5.3 Weighting

There is controversy as to whether weighting is appropriate in studies that seek to estimate causal effects (Angrist and Pischke 2009, Solon, Haider, and Wooldridge 2015). Given this controversy, we re-estimate Equation (1) using population weights; we weight the regressions with the population ages 18 to 64 in each state. Results are reported in Table 6 and are not appreciably different from the unweighted results generated in our core model (Table 3).

5.4 Counselling services and over-the-counter cessation medications

Over time states have increasingly covered smoking cessation counseling (Singleterry et al. 2014); which may change the use of cessation medications, either by substituting for or encouraging medication adherence. Moreover, Medicaid expansion plans include coverage for OTC FDA-approved NRTs. Thus, our analysis may be vulnerable to omitted variable bias as we do not control for these related covered services.

We re-estimate Equation (1) including an indicator variable for whether a state-year-quarter covers counselling services (individual or group) or at least one FDA-approved OTC

[23] We note that another common concern in policy analyses is program induced migration. That is, some individuals may migrate from non-expansion states to expansion states and such behavior will bias regression coefficients (Moffitt 1992). To the best of our knowledge, there is no evidence that the ACA Medicaid expansions induced this type of cross-state migration behavior (Goodman 2017).

NRT (nicotine gum, lozenge, or patch). Table 7 reports results: coefficient estimates are not appreciably different from our core findings (Table 3).

To dig deeper into the importance of OTC NRTs, we re-estimate Equation (1) using the above-noted FDA-approved OTC NRTs as variables analogous to our prescription medication variables.[24] We examine common OTC NRTs (e.g., Nicoderm, Nicorette, generic brands for national box stores such as Walmart's brand Equate) using the method outlined in Section 3.1. Results are reported in Table 8. We also report regression-based testing of the parallel trends assumption in the left-hand panel of Table 8; this analysis suggests that the OTC data are able to satisfy this assumption. Turning to our DD estimates (right-hand panel), we find no statistically significant evidence that OTC NRT prescriptions fills and refills increased post expansion.

It is surprising that we find increases in prescription medications but no corresponding increases in the OTC NRTs post-expansion. We propose possible reasons. (i) OTC NRTs tend to be less expensive than prescription medications and OTC NRTs may be more affordable to uninsured smokers seeking to quit, but when prescription medication out-of-pocket prices decline with Medicaid coverage smokers switch to the latter, more expensive, product.[25] (ii) Many states offered OTC NRTs for free or at a heavy discount pre-ACA (Cummins et al. 2007) and uninsured smokers may have relied on these discounts pre-expansion and may continue to obtain NRTs outside Medicaid. (iii) The clinical literature suggests that prescription medications are more efficacious than OTC NRTs, thus newly insured smokers (perhaps in combination with healthcare providers) wishing to quit their smoking addiction may simply be selecting the

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[24] Medicaid programs typically require a prescription for OTC NRTs, hence we are able to study these medications in the SDUD. For example, see the New York (https://www.nysmokefree.com/subpage.aspx?pn=medications; accessed August 21st 2017) and Mississippi (https://medicaid.ms.gov/wp-content/uploads/2014/04/OTClistforWebEffective-10-1-2013.pdf; accessed August 21st, 2017) Medicaid plan documentation relating to OTC NRT coverage.

[25] See for example: https://www.larasig.com/node/6522 (accessed August 22nd, 2017).

optimal cessation method post-expansion. (iv) Coefficient estimates in Table 8 are positive while the standard error estimates are relatively large, thus we cannot definitively rule out the hypothesis that OTC NRT prescriptions increased post-expansion.

5.5 Utilization management techniques

The ACA did not regulate state Medicaid program use of utilization management techniques. According to the CDC (2016), the most common forms of utilization management are copayments, annual duration limits, and prior authorization. There is concern among healthcare scholars that such techniques may offset expansion effects (McAfee et al. 2015). To explore this possibility, we estimate a variant of Equation (1) that includes an indicator that takes on a value of one if the state applies one of the above-noted utilization management techniques, and zero otherwise. Results (Table 9) are not appreciably different from our core findings (Table 3), and suggest that utilization management techniques do not offset expansion effects.[26,27]

5.6 Primary care

A concern among policy makers is that, post-expansion, the newly eligible will not be able to access primary care given difficulties, pre-ACA, faced by Medicaid patients in accessing this type of care (Decker 2012). To explore the importance of access to primary care, where many of the prescriptions that we study here likely originate, we next estimate separate regressions for states with relatively high and relatively low access to primary care.

We combine data from the Area Resource File (ARF) on the number of physicians delivering primary care and data from CMS on the number of Medicaid enrollees in each state to

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[26] In unreported analyses we have used different coding schemes for utilization management and the results, which are available on request, are not appreciably different.

[27] Using data from the CDC STATE system, we have explored the extent to which expansion states have complied with the ACA cessation medication regulation that, post January 1st 2014, Medicaid must cover all three medications that we study here. Overall, states appear to be broadly in compliance with this requirement. However, we did find evidence that several states were not fully compliant (i.e., did not cover all medications). In unreported analyses, we excluded non-compliant states from the analysis sample. Results are robust and are available on request.

construct the ratio of physicians who might be delivering patient care to Medicaid enrollees. We classify states as 'high access' (at or above the median value of the primary physician-Medicaid enrollee ratio for the nation) or 'low access' (below the median value of the primary physician-Medicaid enrollee ratio for the nation). We use ARF and CMS data from 2010, in advance of the Medicaid expansions we study, to avoid stratifying the sample based on an endogenous variable (i.e., the number of enrollees).[28] Results are reported in Table 10.

We find similar effects (i.e., when comparing coefficient estimates to the pre-expansion means) in the low access sample than the high access sample. This is in line with Abdus and Hill (2017) who find no evidence of negative spillovers of the expansions on insured populations.

5.7 Isolating effects for the newly eligible

A limitation of the SDUD data, similar to many survey and administrative datasets used to study ACA Medicaid expansions, is that the data do not include information on patients to allow separation of the newly eligible from other Medicaid enrollees. We explore effects in different specifications and samples to tease out effects among newly eligible enrollees. Each of these extensions is subject to important limitations, which we outline below. We argue that considering results generated in these extensions collectively is informative for thinking through the extent to which we can interpret our main estimates (Table 3) as reflecting effects for the newly eligible or some combination of groups that enrolled in Medicaid due to ACA-related changes; i.e., newly eligible populations, previously eligible populations that enrolled in Medicaid due to welcome mat effects, and populations that become eligible for Medicaid due to changes in the income disregard.

[28] We note that the number of physicians is also potentially endogenous to the expansions we study. Indeed, the ACA allocated resources toward increasing healthcare workforce and infrastructure (Abdus and Hill 2017).

Our first approach to minimize contamination from welcome-mat and income disregard effects is to estimate a triple-difference style estimator to isolate effects for the newly eligible. We augment Equation (1) with a three-way interaction between an indicator for the expansion period, the post period (this varies across states depending on when the state expanded Medicaid), and the newly eligible as a percent of increased enrollment. We define the newly eligible share of increased enrollment between 2013 and each period in the following manner:

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

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. 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.[29] We exclude early expanding states from this analysis.[30] The Medicaid expansion indicator captures the effect of Medicaid expansion on all Medicaid enrollees other than the newly eligible (welcome mat and income disregard) 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. A limitation of this analysis is that the $Percent_{st}$ variable is likely influenced by Medicaid expansion, hence results generated in this specification may be vulnerable to over-controlling bias (Angrist and Pischke 2009). We report results in Table 11.

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[29] 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,' making it appear as though the newly eligible grew more than total enrollment.

[30] Including these states in the analysis does not alter our results appreciably (results available on request). We chose to exclude these variables as they did enroll newly eligible prior to January 1st, 2014 but our data source for enrollees (CMS) began tracking enrollment by newly eligible status in January 1st, 2014.

The interaction between the expansion indicator and post is not statistically different from zero in this specification; the confidence interval is quite wide and thus we cannot rule out increases in prescriptions for the non-newly eligible Medicaid populations. However, the coefficient on the three-way interaction is positive and precise. To interpret this coefficient estimate, we multiply it by the mean value of $Percent_{st}$ in the post-period among expansion states (82.85%). We find that, post-expansion, prescriptions among newly eligible populations increased by 90 per 100,000 non-elderly residents each quarter (36%). This estimate is very similar to that estimated in our main specification (Table 3; an increase of 89 prescriptions per quarter or 36%). We interpret these results to suggest that our findings are primarily attributable to increases in prescriptions among the newly eligible. This pattern is in line with findings for primary care visits (Biener, Zuvekas, and Hill 2017), a setting in which cessation medication prescriptions are likely to be written. The authors show that, post-expansion, primary care visits increased among newly-eligibles but such visits were not altered among previously eligibles.

As a second approach, we re-estimate Equation (1) in for prescriptions financed by MC plans and FFS Medicaid plans separately. Interviews with state Medicaid program directors reveal that most newly eligibles are enrolled in MC plans while traditional populations are more likely to be enrolled in FFS plans (Kaiser Commission on Medicaid and the Uninsured 2016). We argue that separating the data in this manner can allow us to provide estimates for samples that better reflect the newly eligible and other populations that enrolled in Medicaid. A limitation of this extension is that there is not a perfect correlation between newly eligible enrollees and Medicaid plan (MC vs. FFS); i.e., there are obviously newly eligibles enrolled in FFS plans, and previously eligibles and individuals who became eligible due to changes in the income disregard enrolled in MC plans. We are simply relying on the fact that the newly eligibles are much more likely to enroll in MC plans based on reports by state Medicaid program directors. For the analysis of MC plans we include only those state/year pairs that have MC data appearing in the SDUD (not all states have MC programs in all years of our study period) and we exclude the four expansion states that do not use MC for newly eligibles during our study period (Arkansas, Connecticut, Montana, and Vermont).

Results are reported in Table 12. The MC sample produces an estimate very similar to our main estimates although relative effect size is larger (Table 3). In particular, post-expansion, the number of quarterly prescription fills and refills in MC plans increased by 62 (55%) in expansion states relative to non-expansion states.[31] In terms of the FFS sample, we find no statistically significant evidence that prescriptions financed by FFS Medicaid increased in expanding states relative to non-expansion states. The effect size (both absolute and relative) is smaller in the FFS sample than the MC sample. We interpret these findings to suggest that our main DD estimates primarily capture effects for the newly eligible and not other groups that enrolled in Medicaid due to ACA-related changes.

Finally, we attempt to remove effects attributable those enrollees who became eligible due to the five percentage point increase in the income thresholds for both expansion and non-expansion states. This policy change occurred January 1st, 2014 for all states. Hence, for states that expanded Medicaid on January 1st, 2014, Medicaid expansion and the income disregard increase are perfectly collinear. To separate these two effects, we re-estimate Equation (1) excluding all states that expanded January 1st, 2014 and early expansion states (see Table 1). We exclude the early expansion states as, while these states did have major Medicaid expansions prior to the ACA-related expansions we study, the ACA expansion substantially increased the

[31] The relative effect size is larger as the baseline number of prescription fills and refills is lower in the MC Medicaid-financed sample than in the overall sample of prescriptions.

number of individuals eligible for Medicaid. Excluding these states allows us to recover effects for the newly eligible from effects for those individuals who became eligible due to the income disregard. However, an important limitation of this approach is that we leverage variation from the relatively small number of late expanding states (see Table 1). We interpret findings from this analysis cautiously, and encourage readers to do the same, as the findings could be attributable to unique attributes of the late expanding states rather than Medicaid expansion.

Results from this analysis are reported in Table 13. The estimated effects suggest that, post-expansion, quarterly prescription medications increased by 53 per 100,000 non-elderly adults in expansion states relative to non-expansion states. The 95% confidence intervals for this point estimate and our main point estimate (Table 3; 89) overlap. We interpret the similarity in the point estimates to imply that our main findings are largely attributable to the newly eligible.

In summary, while our data (administrative prescription counts) do not allow us to isolate those patients who became eligible due to the Medicaid expansions, findings from estimates generated in different specifications and samples collectively suggests that our estimates are not fully attributable to groups other than the newly eligible. These methods could be applied in other data sets that lack detailed income and demographic information.

5.8 States with no change in cessation medication coverage

The ACA required that all states cover FDA-approved smoking cessation medications. For states that did not cover these services prior to this Act, continuously enrolled traditional Medicaid populations in expansion and non-expansion states gained coverage. Thus, our DD estimates may reflect the combined effects of Medicaid expansion to newly eligible enrollees and cessation service coverage gains among the continuously enrolled. To isolate newly eligible effects we estimate Equation (1) excluding states that changed coverage for any of the medications we examine during our study period using data from Centers for Disease Control and Prevention (2016).[32]

Results are reported in Table 14. Estimates generated in this sample are very similar to our main results (Table 3): post expansion, quarterly prescription fills and refills increase by 72 per 100,000 or 28% in expansion states relative to non-expansion states. We interpret these results to suggest that our findings are primarily attributable to increases in enrollment, rather than improvements in coverage.

5.9 Controlling for between-state heterogeneity

In our primary specification, we control for between-state heterogeneity through the inclusion of time-varying state-level variables, states fixed effects, and state-specific linear time trends. While this specification is standard in policy analyses (Angrist and Pischke 2009), it does impose assumptions on the nature of between-state heterogeneity. For example, state-specific linear time trends can account for state-level unobservables that vary in a linear manner over time but the true trends in such unobservables may not be linear. On the other hand, if there are no important time-varying state-level variables, including these time trends 'throws away' variation that can be used for identification of treatment effects. Further, some of our time-varying state-level controls may be outcomes of Medicaid expansion, suggesting that our estimates suffer from over-controlling bias (Angrist and Pischke 2009).

To explore the robustness of our findings to different specifications, we next estimate variants of Equation (1) that sequentially exclude state-specific linear time trends, exclude time-varying state-level controls, and include state-specific quadratic time trends. Results are reported in Table 15 and are in line with estimates generated in our main specification. Indeed, 95%

[32] Excluded states include: Alabama, Alaska, Arkansas, California, D.C., Florida, Georgia, Kansas, Kentucky, Maine, Michigan, Nebraska, South Carolina, Texas, Washington, and West Virginia.

confidence intervals of all point estimates in Table 15 include the point estimate generated in our primary specification (Table 3, 89). Not surprisingly, coefficients are generally smaller in specifications that offer more control for between-state heterogeneity.

5.10 Alternative coding of ACA-related Medicaid expansion

We next report the robustness of our results to alternative approaches to coding ACA-related Medicaid expansion. First, we adopt the coding of early expansion states in Sommers et al. (2013) and Maclean and Saloner (2017); second we code states with substantial Medicaid prior to our study period (early expansion states in Table 1) as expanding January 1st, 2014 in the spirit of Courtemanche et al. (2017b); third we exclude states with substantial Medicaid expansions prior to our study period from the analysis sample following sensitivity analyses outlined by Wherry and Miller (2016), and Simon, Soni, and Cawley (2017);[33] and fourth we exclude California as this state has the largest number of Medicaid enrollees (University of Kentucky Center for Poverty Research Center 2016) and we wish to ensure that our findings are not driven by one specific state. Results are reported in Table 16. While the point estimates do vary across the various coding schemes, 95% confidence intervals for all specifications include our primary point estimate (89; Table 3) and we interpret these findings to imply that our results are not sensitive to the use of different approaches to coding ACA-related Medicaid expansion.

6. Discussion

In this study we offer new evidence on the effects of the Affordable Care Act (ACA) Medicaid expansions, specifically we examine the use of prescription medications for smoking cessation. The ACA-related expansions increased Medicaid enrollment by 27% (Gates et al.

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[33] This check excludes Massachusetts, which implemented a large-scale healthcare reform in 2006 that shares similarities with the ACA (Gruber 2008) and included smoking-cessation medications coverage for Medicaid enrollees (Land et al. 2010). Thus, we also explore the possibility that our main point estimates are partially driven by a previous large-scale healthcare reform.

2016), and 38% of this population may smoke (DiGiulio et al. 2016). The expansions were therefore targeted at a group of adults with a high rate of uninsurance and smoking risk, and included a generous set of cessation medications. Our findings imply that, post-expansion, cessation medication fills and refills for which Medicaid was a third-party payer increased 36% in expansion states relative to non-expansion states, and this change was primarily driven by increased prescriptions for Zyban. Zyban is the least expensive prescription medication we study, which suggests that the newly eligible were being treated in a cost-effective manner, as the medications we study have similar efficacy. We provide evidence that this estimate is predominately driven by the newly eligible population and not other groups that enrolled in Medicaid with the broader ACA roll-out, nor improvements in coverage of prescription cessation products among populations continuously enrolled Medicaid over our study period (2011-2015). Increased prescriptions were financed by state Medicaid programs and not patients. Dynamic models suggest that prescriptions increased over time. Finally, and re-assumingly, our estimates are stable across numerous sensitivity checks.

We can compare our findings with previous ACA Medicaid studies that examine smoking. While Simon, Soni, and Cawley (2017) provide evidence that ACA Medicaid expansions reduced smoking by 6% among childless adults, Courtemanche et al. (2017a) find no evidence that smoking declined post-expansion among childless adults or any other population. Given that the effect of ACA Medicaid expansions on smoking remains unsettled, our study of cessation medications is particularly timely and offers new evidence on this question. Cessation takes time with the average smoker attempting to quit roughly 30 times before obtaining abstinence (Chaiton et al. 2016). If increased use of prescription medications is a principle mechanism through which expansions affect smoking, then – prior to changes in smoking – we would expect to see changes in medications. Given that we document increases in cessation medication fills and refills that accelerated over time, our findings suggest that we may expect more definitive evidence that smoking has declined in expansion states as more post-expansion data becomes available. Thus, our use of prescriptions facilitates better understanding of the mechanisms related to ACA Medicaid expansions and behavioral response of lower income smokers with little experience with the healthcare delivery system who seek to quit their smoking addiction. Finally, regardless of whether or not ACA Medicaid expansions affect smoking in the short- or long-run, assessing the effects of these expansions on service use is critical for understanding the overall value of this major policy shift.

We can also compare our estimates to three recent studies that have explored the effect of Medicaid expansion on prescription medication claims. Our finding of a 36% increase in Medicaid-financed smoking cessation medications is in the middle of estimates for the effect of the Medicaid expansion on prescription medications in general and specific types of prescription medications related to treatment of behavioral health outcomes. Our finding is larger than an increase of 19% found for all Medicaid-financed prescriptions (Ghosh, Simon, and Sommers 2017) and an increase of 22% identified for psychotropic medications (Maclean et al. 2017), but less than a 70% increase found for prescription drugs used to treat opioid use disorders (Wen et al. 2017). Our estimate is comparable to a 43% increase found for all FDA-approved medications used to treat alcohol and drug use disorders (Maclean and Saloner 2017).

Although several studies suggest Medicaid expansions have led to reductions in uninsurance, increased healthcare access and service use, improved health, and reduced financial instability (Miller and Wherry 2017, Wherry and Miller 2016, Hu et al. 2016, Simon, Soni, and Cawley 2017), the future direction of Medicaid is uncertain. Our study suggests that constraining Medicaid eligibility for the populations that gained access through the ACA-related expansions may reduce the use of smoking cessation medications. Reducing the use of such medications may in turn increase the total economic costs of smoking, currently estimated at $300B annually (Centers for Disease Control and Prevention 2017).

In summary, our findings offer new evidence on the early effects of Medicaid expansions on smokers. In combination with previous analyses that have explored the effects of expansions on other behavioral health outcomes, Medicaid expansions appear to have been associated with increased access to evidence-based services to a particularly policy-relevant group: low-income Americans who suffer from tobacco use, a chronic, addictive, costly, and harmful condition.

Table 1. Substantial state Medicaid expansions: 2011-2017

State	Medicaid expansion date
States with substantial expansions before 2011	
Delaware	Before 2011
DC	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 Wherry and Miller (2016) and Simon et al. (2017). ‘Substantial’ expansions covered both parents and childless adults up to at least 100% FPL, were open to new enrollees, and provided full Medicaid benefits.

[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 2. Summary statistics for expansion and non-expansion states: 2011-2013

Sample:	**Expansion states**	**Non-expansion states**	**Difference (*p*-value)***
Medicaid-financed cessation medications per quarter:			
Prescription fills and refills per 100,000 persons 18 to 64 years	250	208	0.0011
State-year level regulations and characteristics			
Tobacco cigarette taxes per package ($)	1.588	1.019	0.0000
Smoking bans (restaurants, bars, and private worksites)	2.333	2	0.0006
Any e-cigarette tax	0.048	0	0.0006
Vaping bans (restaurants, bars, and private worksites)	0.238	0.100	0.0276
Age	37.70	37.30	0.0033
Female	0.508	0.510	0.0291
Male	0.492	0.490	0.0291
White	0.824	0.805	0.0250
African American	0.082	0.131	0.0000
Other race	0.094	0.064	0.0000
Hispanic	0.122	0.089	0.0003
College degree	0.271	0.241	0.0000
Unemployment rate	7.738	7.080	0.0001
Poverty rate	13.43	15.05	0.0000
Maximum monthly TANF benefit for a family of4 ($)	593.7	424.0	0.0000
Maximum monthly SNAP benefit for a family of 4 ($)	705.4	698.9	0.0019
Effective minimum wage ($)	7.994	7.635	0.0000
Democratic governor	0.571	0.133	0.0000
Observations	252	240	--

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. States with substantial expansions before 2011 excluded from the analysis (see Table 1).

*Two-tailed *t*-tests applied.

Table 3. Effect of Medicaid expansions on cessation prescription medication fills and refills using differences-in-differences: 2011-2015

Outcome:	Prescription fills and refills
Panel A: Parallel trends testing+	
Mean value in expansion states, pre-expansion	250
Expansion state * time trend	2
	(2)
Observations	492
Panel B: Differences-in-differences	
Mean value in expansion states, pre-expansion	250
Expansion	89***
	(19)
Observations	920

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.
+Parallel trends testing regressions do not include state-specific linear trends and use data 2011-2013. Early expansion states excluded (see Table 1).
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 4. Effect of Medicaid expansions on cessation prescription medication payments using differences-in-differences: 2011-2015

Outcome:	Total payments	Medicaid payments	Non-Medicaid payments
Panel A: Parallel trends testing+			
Mean value in expansion states, pre-expansion	$15,375	$15,188	$187
Expansion state*time trend	141	120	21*
	(227)	(225)	(12)
Observations	492	492	492
Panel B: Differences-in-differences			
Mean value in expansion states, pre-expansion	$15,375	$15,188	$187
Expansion	4,241**	4,295***	-54
	(1,634)	(1,590)	(82)
Observations	920	920	920

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.
+Parallel trends testing regressions do not include state-specific linear trends and use data 2011-2013. Early expansion states excluded (see Table 1).
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 5. Heterogeneity in the effect of Medicaid expansions on cessation prescription medication fills and refills using differences-in-differences: 2011-2015

Regression model:	**Parallel trends+**	**Differences-in-differences**
Mean value in expansion states, pre-expansion	205	205
Zyban	1	83***
	(1)	(16)
Mean value in expansion states, pre-expansion	42	42
Chantix	1	7
	(2)	(7)
Mean value in expansion states, pre-expansion	3	3
Nicotrol	0	0
	(0)	(0)
Observations	492	920

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.

+Parallel trends testing regressions do not include state-specific linear trends and use data 2011-2013. Early expansion states excluded (see Table 1).

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

Table 6. Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills using differences-in-differences and applying population weights: 2011-2015

Outcome:	Prescription fills and refills
Weighted mean value in expansion states, pre-expansion	220
Expansion	85***
	(19)
Observations	920

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Regressions are weighted by the state population ages 18 to 64 years. Standard errors are clustered at the state level and are reported in parentheses.

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

Table 7. Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills using differences-in-differences and controlling for other-the-counter medication and counselling service coverage+: 2011-2015

Outcome:	Prescription fills and refills
Mean value in expansion states, pre-expansion	250
Expansion	91***
	(18)
Observations	916

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Over-the-counter medication policy data is missing for the District of Columbia and this state is excluded from the analysis. Standard errors are clustered at the state level and are reported in parentheses.

+ Over-the-counter medications include nicotine gum, patches, and lozenges. Counselling services include individual and group counselling.

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

Table 8. Effect of Medicaid expansions on over-the-counter (OTC) nicotine replacement therapies (NRT) fills and re-fills using differences-in-differences: 2011-2015

Outcome:	**OTC NRT fills and re-fills**
Panel A: Parallel trends testing+	
Mean value in expansion states, pre-expansion	62
Expansion state * time trend	3
	(3)
Observations	492
Panel B: Differences-in-differences	
Mean value in expansion states, pre-expansion	62
Expansion	5
	(14)
Observations	920

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.

+Parallel trends testing regressions do not include state-specific linear trends and use data 2011-2013. Early expansion states excluded (see Table 1).

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

Table 9. Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills using differences-in-differences and controlling for utilization management techniques+: 2011-2015

Outcome:	**Prescription fills and refills**
Mean value in expansion states, pre-expansion	250
Expansion	89***
	(19)
Observations	920

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.

+Utilization management techniques include co-payments, annual limits on duration, and prior authorization.

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

Table 10. Heterogeneity in Medicaid expansion effects on smoking cessation prescription medication fills and refills by access to primary care using differences-in-differences: 2011-2015

Outcome:	**Prescription fills and refills**
High access states	
Mean value in expansion states, pre-expansion	276
Expansion	90***
	(25)
Observations	460
Low access states	
Mean value in expansion states, pre-expansion	214
Expansion	91**
	(37)
Observations	460

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Access is defined as the ratio of physicians providing primary care to Medicaid enrollees in a state in 2010. High access is defined as at or above the national median in 2010. Low access is defined as below the national median in 2010. Standard errors are clustered at the state level and are reported in parentheses.

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

Table 11. Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills using a triple difference-style estimator: 2011-2015

Outcome:	Prescription fills and re-fills
Mean value in expansion states, pre-expansion	250
Expansion	1.167
	(41.613)
Post * expansion * percent	1.087**
	(0.474)
Mean percent, treatment group, post-expansion period:	82.85%
Observations	820

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses. Early expansion states excluded (see Table 1). Post = an indicator for the period after expansion. Expansion = an indicator for expansion in state *s* in period *t*. Percent = newly eligible enrollees as a percent of Medicaid enrollment increase between 2013 in state *s* and period *t*.

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

Table 12. Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills using differences-in-differences by program type: 2011-2015

Outcome:	Prescription fills and re-fills
Managed care Medicaid financed prescriptions+	
Mean value in expansion states, pre-expansion	113
Expansion	62***
	(23)
Observations	680
Fee-for-service Medicaid financed prescriptions	
Mean value in expansion states, pre-expansion	171
Expansion	33
	(25)
Observations	920

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.

+We exclude states that do not report managed care Medicaid financed prescriptions and four expansion states that do not enroll newly eligible populations in managed care plans during our study period. See text for more details.

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

Table 13. Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills using differences-in-differences excluding states that expanded Medicaid on January 1st, 2014 and early expansion states: 2011-2015

Outcome:	**Prescription fills and refills**
Mean value in expansion states, pre-expansion	262
Expansion	53***
	(15)
Observations	500

Source: State Drug Utilization Data.
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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses. States expanding Medicaid on January 1st and early expansion states excluded (see Table 1).
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 14. Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills using differences-in-differences using states that did not change cessation medication coverage over the study period: 2011-2015

Outcome:	**Prescription fills and refills**
Mean value in expansion states, pre-expansion	264
Expansion	72***
	(18)
Observations	600

Source: State Drug Utilization Data.
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 control for smoking policies, demographics, social policies, state-specific linear time trends, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 15. Effect of Medicaid expansions on cessation prescription medication fills and refills using differences-in-differences with different controls for between state heterogeneity: 2011-2015

Outcome:	**Prescription fills and refills**
Mean value in expansion states, pre-expansion	250
Exclude state-specific linear time trends	134***
	(25)
Exclude time-varying state-level controls	83***
	(21)
Include state-specific quadratic time trends	56***
	(19)
Observations	920

Source: State Drug Utilization Data.
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 control for and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 16. Effect of Medicaid expansions on cessation prescription medication fills and refills using differences-in-differences use different Medicaid expansion coding schemes: 2011-2015

Outcome:	**Prescription fills and refills**
Mean value in expansion states, pre-expansion+	250
Maclean and Saloner (2017a)	66***
	(21)
Observations	920
Treat all early expanding states as expanding January 1st, 2014 (see Table 1)	82***
	(18)
Observations	920
Exclude early expanding states (see Table 1)	90***
	(19)
Observations	820
Exclude California	88***
	(19)
Observations	900

Source: State Drug Utilization Data.

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 control for and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses.

+We use the coding scheme reported in Table 1 to calculate the mean values in expansion states in the pre-expansion states.

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

Appendix Table 1. Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills using an event study: SDUD 2011-2015

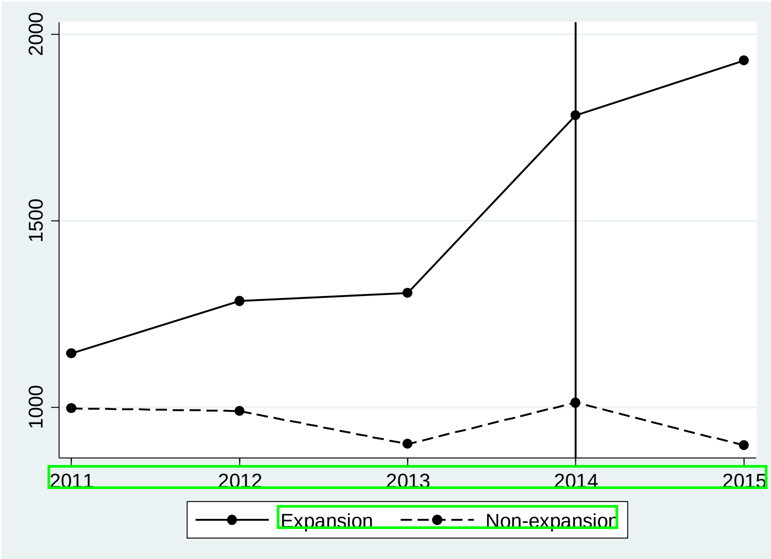
Outcome:	**Prescription fills and refills**
Mean value in expansion states, pre-expansion	250
Q1 2011	-20
	(27)
Q2 2011	-11
	(26)
Q3 2011	-56***
	(20)
Q4 2011	-25
	(19)
Q1 2012	3
	(19)
Q2 2012	12
	(20)
Q3 2012	4
	(18)
Q4 2012	-6
	(10)
Q1 2013	13
	(13)
Q2 2013	-6
	(13)
Q3 2013	10
	(8)
Q1 2014	34*
	(19)
Q2 2014	72**
	(35)
Q3 2014	146***
	(43)
Q4 2014	149***
	(34)
Q1 2015	143***
	(35)
Q2 2015	159***
	(35)
Q3 2015	149***
	(31)
Q4 2015	167***
	(32)
Observations	820

Source: State Drug Utilization Data.

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 control for smoking policies, demographics, social policies, and state and period fixed effects. Reference period is Q4 2013. Standard errors are clustered at the state level and are reported in parentheses. States with substantial expansions before 2011 excluded from the analysis (see Table 1).

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

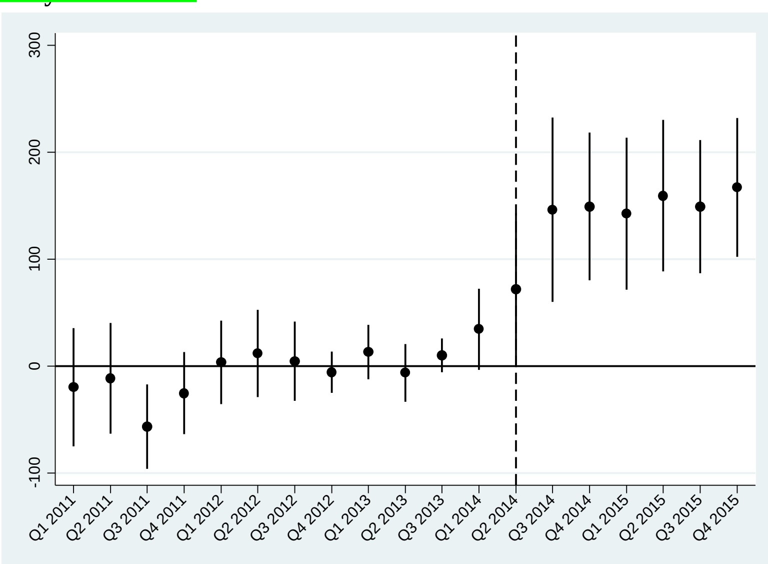
Figure 1. Trends in smoking cessation medication Prescription fills and refills: 2011-2015



Source: State Drug Utilization Data.

Notes: Unit of observation is the expansion-year. All outcomes are converted to a rate per 100,000 persons 18 to 64 years. States with substantial expansions before 2011 excluded from the analysis (see Table 1).

Figure 2. Effect of Medicaid expansions on smoking cessation prescription fills and refills using an event study: 2011-2015



Source: State Drug Utilization Data.

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 dummy variables include each year-quarter cell between Q1 2011 and Q4 2014, the omitted category is Q4 2013. All models control for smoking policies, demographics, social policies, 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 1). N=800. See Appendix Table 1 for coefficient and standard error estimates.

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