Published in final edited form as:

Econ Inq. 2019 October ; 57(4): 1798–1820. doi:10.1111/ecin.12794.

Public insurance expansions and smoking cessation medications

Johanna Catherine Maclean^{a,*}, Michael F. Pesko^b, Steven C. Hill^c

^aAssociate Professor, Department of Economics, Temple University, Research Associate, National Bureau of Economics, Research Affiliate, Institute for Labor Economics, Ritter Annex 869 -- 1301 Cecil B Moore Avenue, Philadelphia PA, 19122.

^bAssistant Professor, Department of Economics, Andrew Young School of Policy Studies, Georgia State University, PO Box 3992, Atlanta GA, 30302-3992.

^cSenior Economist, Center for Financing, Access and Cost Trends, Agency for Healthcare Research and Quality, 5600 Fishers Lane, Mail Stop 07W41A, Rockville MD 20857.

Abstract

We study the effect of public insurance on smoking cessation medication prescriptions and financing. We leverage variation in insurance coverage generated by recent Affordable Care Act expansions to Medicaid. We estimate differences-in-differences models using administrative data on the universe of Medicaid-financed prescriptions sold in retail and online pharmacies 2011–2017. Our findings suggest that these expansions increased Medicaid-financed smoking cessation prescriptions by 34%. This increase reflects new medication use and a shift in payment from private insurers and self-paying patients to Medicaid. Adjusting our estimate for changes in financing implies that Medicaid expansion lead to a 24% increase in new medication use.

Keywords

Smoking cessation; prescriptions; healthcare; public insurance; expenditures; JEL classification; I1; I13; I18

1. Introduction

Smoking is believed to be the largest preventable cause of morbidity and mortality globally (World Health Organization 2013). In the United States, the focus of this study, 480,000 deaths are attributed to smoking annually (Centers for Disease Control and Prevention 2017) and smoking is linked with \$193B in healthcare costs each year (Xu et al. 2015).¹ Despite established health harms, the smoking rate remains high: 13.9% in the U.S. (National Center for Health Statistics 2018). The majority of smokers, 68.8%, want to quit but most cannot as smoking is addictive; the annual quit rate is 6.2% (Centers for Disease Control Prevention 2011).

*Corresponding author. catherine.maclean@temple.edu.

¹The cost estimate is inflated by the authors to 2018 dollars using the Consumer Price Index (CPI).

There is socioeconomic disparity in smoking: lower-income groups are more likely to smoke than higher-income groups (National Center for Health Statistics 2017). Smoking is particularly prevalent within the Medicaid population. Medicaid is a joint state-federal public insurance program that finances healthcare services for the poor in the U.S. The smoking rate is 30% among Medicaid-enrolled adults (National Center for Health Statistics 2017), which is troubling from a public finance perspective as smoking-attributable Medicaid costs will be borne predominately by taxpayers given low cost-sharing within Medicaid. These costs are non-trivial: smoking-related diseases accounted for 15% or \$45B of annual Medicaid expenditures over the period 2006 to 2010 (Xu et al. 2015).² Smoking cessation has been shown to reduce healthcare expenditures (Richard, West, and Ku 2012), suggesting that promoting cessation broadly within Medicaid could confer substantial benefits to both taxpayers and enrollees.

Beginning in 2014, the Affordable Care Act (ACA) provides generous federal funding for states to expand their Medicaid programs to low-income nonelderly, non-disabled adults ('newly eligibles'). For states that expanded Medicaid, the federal government financed 100% of the costs for newly eligible beneficiaries between 2014 and 2016. By 2020, the federal contribution will decline to 90%. The ACA requires Medicaid to cover Food and Drug Administration (FDA)-approved prescription cessation medications, and, for newly eligibles, Medicaid must cover these drugs with no cost-sharing (DiGiulio et al. 2016, Gates, Ranji, and Snyder 2014). These medications are efficacious (Cummings and Hyland 2005, Aubin et al. 2008, Biazio et al. 2010, Ruger and Lazar 2012, Stead et al. 2012) but are expensive without insurance. Prices range from \$100 to \$500 per prescription for an uninsured smoker in addition to the costs of a healthcare professional appointment required to obtain a prescription.³

While expanding Medicaid coverage can potentially improve public health, a concern is that such expansions may shift financing through 'crowd-out' of service use that would have been financed by private insurers and fewer uninsured patients paying out-of-pocket, leading to no change in public health but higher government expenditures. An additional potential concern associated with ACA Medicaid expansion is that insurance coverage may increase smoking through *ex-ante* moral hazard effects. Crowd-out and moral hazard effects may also interact.

In this study, we examine the effect of ACA Medicaid expansions to newly eligible populations on Medicaid-financed FDA-approved prescription medications for smoking cessation and associated changes in financing, both crowd-out from private insurance and reductions in out-of-payments from uninsured patients. The newly eligible have higher smoking rates than traditionally eligible Medicaid populations, are very low-income, and had little access to insurance (public or private) prior to the ACA (DiGiulio et al. 2016, National Center for Health Statistics 2014). Given these characteristics, the newly eligible may benefit from coverage obtained through ACA-related Medicaid expansions. We apply

²The estimate is inflated by the authors to 2018 dollars using the CPI.

³These cost estimates are based on the authors' non-systematic review of online discount prescription medication websites, details available on request.

differences-in-differences and event-study methods to administrative data that cover the universe of Medicaid-financed prescription medications purchased from retail and online pharmacies over the period 2011 to 2017. We find that, post-expansion, Medicaid-financed smoking cessation prescription fills increased by 34% in expansion states.

Our primary finding suggests the expansion increased overall smoking cessation medication utilization financed by Medicaid. However, understanding the extent to which this estimate reflects new use of medications vs. a change in financing for use that would have occurred absent Medicaid expansion is important. Using additional data sources to assess expansion effects on changes in medication financing (as opposed to initiation of new medication use), which we refer to as ‘cost-shifting’, we estimate that 70% of the increase in Medicaid-financed prescription fills was attributable to new use of cessation medications, and the remaining 30% was due to cost-shifting. Further, we document that 15% of the estimated cost-shifting was from private insurance and 85% from uninsured patients who previously paid out of pocket for cessation medications. Scaling our primary estimate of a 34% increase in Medicaid-financed prescriptions for such cost-shifting implies that the expansion increased new cessation prescription medication use by 24%. Our findings suggest that Medicaid expansion plausibly improved access to care by increasing use of effective medications but led to federal and state governments financing medication use that would have occurred absent the policy.

Our estimates are useful for both public finance and public health. In terms of public finance importance, policymakers are concerned about the budgetary impact of the newly eligible and we quantify the magnitude of the increased prescription medication use costs to state Medicaid programs, which is *ex-ante* uncertain due to the characteristics of the population that enrolled in Medicaid post-expansion and the providers who deliver care to this population. Additionally, increased use of expensive but effective cessation medications within a population with high smoking rates and limited access to insurance can improve public health. Thus, while one would expect an increase in Medicaid-financed prescriptions post-expansion, quantifying the magnitude of this change is important in terms of understanding the costs and benefits associated with a large increase in generous public insurance coverage for the poor. Further, understanding the extent to which expansions cost-shifting of service use is important for assessing the overall value of a costly public policy such as a large-scale public insurance expansion.

2. Medicaid, conceptual framework, and related literature

2.1 Medicaid

Medicaid is the primary health insurer for low-income nonelderly people in the U.S. It is a joint federal and state program, with states establishing policies within a set of federal laws and administering the program. The federal government provides matching funds to states to finance Medicaid. States that do not follow federal laws are at risk of losing federal match funds. Prior to the ACA, Medicaid eligibility for nonelderly 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 to adults with incomes below 138% of the Federal Poverty Level (FPL) or lose federal funding. This expansion would offer, in most states, eligibility to low-income childless adults for the first time. In 2012 the U.S. Supreme Court ruled that states are not required to expand eligibility to retain federal funding, leaving expansion optional to states. States' decisions to expand Medicaid with the ACA were largely determined by political factors (Sobel 2014). In particular, non-expansion states were more likely to have a Republican-controlled lower chamber than expansion states.

In 2014 the federal government subsidized states' expansions of Medicaid eligibility to most residents with incomes below 138% of FPL. Adults covered under this provision are referred to as 'newly eligible.' Several states opted to expand Medicaid to low-income childless adults prior to 2014. In 2010, six states used waivers to offer full Medicaid benefits to childless adults and 12 states offered limited benefits to this population. Many of the latter programs were closed to new applicants (Kaiser Commission on Medicaid and the Uninsured 2009). Using the ACA early expansion option, five states expanded eligibility prior to 2014. By January 2019, 34 states (including DC) had expanded their Medicaid programs under the ACA. Additional states passed expansion legislation at the time of writing, but the legislation has not been implemented.

As of January 1, 2014, section 2502 of the ACA requires states to cover FDA-approved smoking cessation products, both prescription and over-the-counter (OTC) drugs, for all Medicaid enrollees. Prescription cessation medications Zyban/bupropion ('Zyban') and Chantix/varenicline ('Chantix'), and nicotine replacement therapies (NRT) such as patches, gum, lozenges, nasal sprays, and inhalers are included (Food and Drug Administration 2015). NRT patches, gum, and lozenges are available OTC, while nasal sprays and inhalers ('Nicotrol') require a prescription. While many state Medicaid programs were already providing coverage for these services prior to the ACA (DiGiulio et al. 2016), use of these medications within Medicaid was limited: just 10% of Medicaid smokers received any cessation medications (Ku, Bruen, et al. 2016). States may not use cost sharing for these drugs for newly eligibles and pregnant women, but states may use cost sharing for other Medicaid enrollees. In our main analyses, we examine Medicaid expansion effects on prescription cessation medications: Zyban, Chantix, and Nicotrol. We expect that these medications, because they require a consultation with a healthcare provider which is likely challenging for an uninsured smoker, are more likely to be affected by Medicaid expansion than OTCs.

2.2 Conceptual framework

Two frameworks predict that Medicaid expansion will increase smoking cessation prescription fills. First, in the Grossman (1972) model, consumers do not demand healthcare services *per se*, instead they demand the health improvements attributable to service use. 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 seeking to improve health by quitting

smoking could demand prescription cessation medications. Medicaid – by reducing the out-of-pocket price – should increase the quantity of cessation medications demanded.

Second, Adda and Cornaglia (2006) propose a framework in which the demand for tobacco products is derived from the consumer's demand for nicotine. That is, the consumer does not demand tobacco products *per se*, instead the consumer demands nicotine contained in the tobacco product. In this framework, for some smokers, tobacco products and smoking cessation prescription medications may act as substitutes, as both contain nicotine (or through their pharmacological profiles, outlined later in the manuscript, affect brain functioning in a similar manner). For such smokers, gaining Medicaid coverage may reduce the out-of-pocket prices for smoking cessation prescription medications below the price of tobacco products, which may lead to an increase in prescriptions. We note that our data will not allow us to discriminate between the Grossman (1972) and Adda and Cornaglia (2006) frameworks, but both imply an increase in smoking cessation medication prescription rates post-expansion.

The decline in price is likely non-trivial for the newly eligible population as the ACA mandates zero cost-sharing for these enrollees (DiGiulio et al. 2016, Gates, Ranji, and Snyder 2014). In terms of healthcare professional appointments, which are necessary to obtain a prescription, cost-sharing is capped at 10% of costs by federal mandates (Kaiser Commission on Medicaid and the Uninsured 2013). The cost of a cessation prescription for an uninsured smoker can be as high as \$500 (not including healthcare professional fees required to obtain a prescription). Given that the newly eligible population is low-income – in 2019 for a family of two residing in the contiguous U.S. 138% FPL is \$23,336 in income per year – accessing expensive prescriptions without insurance is likely financially infeasible for many.

Medicaid may alter demand for cessation prescription medications through several additional mechanisms. *Ex-ante* moral hazard suggests that, by lowering smoking-attributable healthcare costs, insurance may incentivize individuals to increase smoking and/or delay cessation (Klick and Stratmann 2006). If insurance acts as an in-kind income transfer, and if smoking is a normal good (Kenkel, Schmeiser, and Urban 2014), the newly eligible may increase smoking. The literature (discussed in Section 2.3) does not suggest that ACA Medicaid induced these behaviors. Other ACA changes (e.g., enrollment process simplification) may amplify or diminish Medicaid effects (Frean, Gruber, and Sommers 2017). Limited provider and patient knowledge of Medicaid prescription cessation medication coverage may mute effects (McMenamin, Halpin, and Bellows 2006, McMenamin et al. 2004). Finally, as noted earlier, expanding Medicaid could, through private coverage crowd-out and reduced self-payments by uninsured patients, lead to changes in medication financing rather than changing use.

Crowd-out behaviors, if present, may interact with moral hazard. For instance, if Medicaid coverage reduces the costs of healthcare treatment associated with smoking-related health conditions (e.g., lung cancer) relative to private coverage, then those consumers who drop private coverage to take up the newly available Medicaid coverage may increase smoking, which may decrease smoking cessation medication use, at least in the short run examined in

this study. On the other hand, if the out-of-pocket costs of smoking cessation prescription medications are lower with Medicaid than with private insurance for those ‘crowded-out’ individuals, then use of cessation medications may increase. Overall, any combination of crowd-out and moral hazard complicates the insurance-smoking cessation prescription medication relationship.

2.3 Literature

Several studies show that pre-ACA Medicaid expansions increased cessation medications utilization (prescriptions and OTCs) and reduced smoking (Liu 2010, 2009, Adams et al. 2013, Witman 2013, Richards et al. 2017, Ku, Brantley, et al. 2016). These studies focus on traditionally eligible populations, predominantly parents and the disabled, and the extent to which these findings can generalize to newly eligible populations is unclear. In particular, the newly eligible population has higher smoking prevalence than populations eligible prior to the ACA (DiGiulio et al. 2016, National Center for Health Statistics 2014). Higher smoking prevalence may indicate that the newly eligible have greater need for cessation medications. Further, the pre-ACA Medicaid literature focuses on expanding coverage for cessation services, holding income eligibility thresholds constant, which implies that the populations affected by these earlier expansions most likely already had insurance coverage (which became more generous post-expansion in terms of cessation medications) and, plausibly, experience interacting with the healthcare system. The newly eligible have had less access to insurance and thus many may face challenges accessing care even after gaining coverage due to their inexperience.

While we emphasize the income eligibility change in our study, the ACA is a large and multifaceted policy in the broader U.S. healthcare delivery system. Thus, prior studies of coverage expansions for particular services reflect much more incremental changes and are unlikely to provide credible estimates of ACA Medicaid effects. For instance, private coverage crowd-out is much more of an empirical concern with a large-scale expansion of income eligibility vs. an increase in coverage for particular set of services to be used by a sub-set of the Medicaid population. Our study of the newly eligible population, and this very large and recent policy change is important for understanding the role of insurance in smoking cessation.

The ACA Medicaid literature is vast. We focus on studies that use quasi-experimental methods, typically differences-in-differences, to estimate effects on medications and smoking. Four studies have investigated prescription medications but none have examined prescription cessation medications. These studies show increases in prescriptions post-expansion.

Using data between 2013 and 2015, Ghosh, Simon, and Sommers (2019) show that Medicaid-financed medications overall increased by 19%; the authors do not study cessation medications. In terms of behavioral health outcomes (substance use disorders [SUD] and mental illness), expansion increased Medicaid-financed prescriptions for SUDs by 55% to over 100% (Wen et al. 2017, Maclean and Saloner 2019, Meinhofer and Witman 2018) and mental illness by 21% (Maclean et al. 2018). In terms of non-prescription medication utilization, Cotti, Nesson, and Tefft (2018), use Nielson scanner data and document that the

probability that a household purchased an OTC cessation medication increased 50% post-expansion.

Smoking cessation prescription medications are important to study, separate from other classes of medications, given that smoking is distinct from general health (e.g., cancer, hypertension, diabetes) and other behavioral health (e.g., mental illness, SUDs) outcomes in terms of addiction, the associated costs to the individual and society (for instance, SUDs are linked with crime and violence (Caulkins, Kasunic, and Lee 2014), which are not typical features of nicotine addiction), utility derived from engaging in the behavior (e.g., smoking, unlike many general health conditions such as diabetes, provides utility to the individual consuming the product), and treatment costs (the ACA requires zero cost-sharing for smoking cessation medications for newly eligible population but this is not the case for all medications). Finally, prescription medications, unlike the OTC medications examined by Cotti, Nesson, and Tefft (2018), require a consultation with a healthcare provider to secure a prescription, which adds to the financial and time cost of these medications for enrollees.

The estimated effects of ACA Medicaid expansion on smoking outcomes are quite mixed. Using two years of post-expansion data, Simon, Soni, and Cawley (2017) find that Medicaid expansion reduced past 30-day smoking among childless nonelderly adults by 6% in expansion states relative to non-expansion states. However, a follow-up study by these authors using three years of post-expansion data found more limited effects on smoking (Cawley, Soni, and Simon 2018). On the other hand, Cotti, Nesson, and Tefft (2018) find that cigarette purchases decline post-expansion using household scanner data, and Koma et al. (2017) show increased smoking cessation among low-income smokers post-expansion. Finally, Courtemanche and colleagues (Courtemanche et al. 2017, 2018), using a triple difference estimator, find no evidence that these expansions affected smoking outcomes.

3. Data, variables, and methods

3.1 Prescriptions

We use the Medicaid State Drug Utilization Database (SDUD). State Medicaid programs collect these prescription data to bill pharmaceutical manufacturers for rebates, and the states pass this information to the Centers for Medicaid and Medicare Services (CMS), which creates the SDUD. Data in the SDUD include all outpatient prescription medications covered under the Medicaid Drug Rebate Program (MDRP) for which Medicaid serves as a third-party payer (U.S. Department of Health and Human Services 2012). The purpose of SDUD is to allow the U.S. Department of Health and Human Services (DHHS) to monitor pharmaceutical rebates for Medicaid-financed prescriptions fills and re-fills ('prescriptions'). Other data sources are used by DHHS to determine the rebate amounts. The MDRP requires roughly 600 pharmaceutical companies participating in Medicaid to pay state programs rebates associated with Medicaid-financed prescriptions. While we do not capture medication use, clinical research documents that prescriptions are a reliable proxy for medication use (Lehmann et al. 2014) and prescriptions are a common proxy within economics (Richards et al. 2017, Bradford and Lastrapes 2013). State Medicaid programs have been required to submit data on fee-for-service (FFS) prescriptions quarterly to the SDUD since 1992. Prescriptions obtained through managed care (MC) plans were added to

the SDUD in March, 2010 (U.S. Department of Health and Human Services 2012). We use data from Q1 2011 to Q4 2017 to include both FFS and MC prescriptions in our analysis (Wen et al. 2017). We aggregate data from FFS and MC to create a single quarter of data. The majority of the newly eligible are enrolled in MC plans, for instance in most expansion states over 80% (Kaiser Commission on Medicaid and the Uninsured 2016), and thus including MC data is important. Government audits confirm that reporting quality is high in the SDUD (Levinson 2011). Unfilled prescriptions are not included in the dataset.

We focus on prescription medications approved by the FDA for cessation (Food and Drug Administration 2015) covered by Medicaid under the ACA in our main analyses. We emphasize prescriptions vs. other cessation medications in our main analysis as prescriptions require a formal consultation with a healthcare provider and are therefore more difficult to access without insurance coverage for lower-income groups, such as the newly eligible population.

We study Zyban, Chantix, and Nicotrol, which are the only FDA-approved prescription cessation medications. We study branded Chantix and its generic version Varenicline, and branded Nicotrol, for which there is no generic. We study the following medications for Zyban: proprietary name Zyban, Buprobán, or Bupropion, extended release with the strength 150 mg.⁴ Zyban is also indicated for attention deficit disorder and depression, but we study the formulations most likely to be used for smoking cessation (Hughes, Stead, and Lancaster 2007). We cannot account for the common practice of off-label use; that is healthcare providers writing prescriptions for medications to treat health conditions for which the FDA has not approved the medication. Multiple use and off-label use are limitations to the broader literature that studies prescription medications, and are not unique limitations of our study.

Each of the prescription medications we study offers the patient a slightly different cessation mechanism, in particular how the medication allows the smoker to reduce addiction to nicotine (the addictive ingredient in tobacco products). A key distinction between the medications is that Zyban and Chantix do not contain nicotine while Nicotrol does contain this ingredient. Zyban eases nicotine withdrawal symptoms (e.g., anxiety, headaches, and irritability) and reduces nicotine cravings by blocking chemicals in the brain. The specific process through which this medication operates on the brain is not entirely clear. Chantix and nicotine act on the same areas of the brain, leading to similar pharmacological experiences, and Chantix eases withdrawal symptoms. Unlike nicotine, Chantix is not addictive and blocks other chemicals in the brain which reduce the enjoyment from consuming nicotine. Finally, Nicotrol contains nicotine, but at a lower dose than found in tobacco products to allow the patient to slowly reduce nicotine intake until the patient is able to quit. Nicotrol also eases withdrawal symptoms.

⁴Other variants of Zyban are: Forfivo (brand), Wellbutrin (brand), Aplenzin (brand), Bupropion (brand), and alternative strengths of Bupropion (generic). These medications are less likely to be used for smoking cessation purposes and are not included. For more information, please see: <https://www.webmd.com/smoking-cessation/understanding-nicotine-withdrawal-treatment#1> and <https://www.webmd.com/smoking-cessation/quit-smoking-aids-know-your-options#1> (accessed February 23th, 2019).

We create these categories using crosswalks between National Drug Codes (NDC) for brand name and generics (Roth 2017). Specific NDCs used in variable construction are available from the corresponding author. Our main analysis combines all medications, but we also study each medication separately. For data points (NDC-state-quarter-year) with fewer than 11 prescription fills, the number of fills and expenditures are censored in the SDUD for confidentiality; 49% of cells are censored in our sample of medications. In these cases, we assign a value of five for prescription fills and for payments we assign the average cessation medication payment for data points with 11 prescriptions divided by two. We report in a sensitivity check that our prescription filling results are robust to assigning different values for missing cells: zero (the lowest possible censored value) and ten (the highest possible censored value). We also show that our payment findings are robust to alternative imputation approaches, i.e., assuming the lowest and highest possible censored value. While we note data censoring as a study limitation, our results are robust to imputing a wide range of plausible censored values.

A concern with the SDUD is differential reporting by state Medicaid programs across newly eligible and previously eligible enrollees, which could lead to systematic reporting differences across expansion and non-expansion states. We believe that such systematic reporting by states is unlikely. First, Medicaid-financed prescriptions for the newly eligible and other enrollees receive the same rebate through the MDRP. Thus, we do not expect that healthcare providers or states differentially report prescriptions to CMS to be included in the SDUD by newly eligible status as the costs of under-reporting (through lost rebates) is identical across enrollees. Second, as noted earlier in this section, the SDUD is used by DHHS to monitor Medicaid-financed prescriptions, other data sources are used by the agency to calculate rebate amounts. There is no direct link between reporting to SDUD and state Medicaid programs' eventual rebates, which further minimizes any gains to states from differentially reporting fills for newly eligibles. In sum, to the best of our knowledge there is no incentive for enrollee eligibility status, or Medicaid expansion, to influence states' reporting of data to the SDUD.

3.2 Medicaid expansion

Appendix Table 1 summarizes our Medicaid expansion state classification. The majority of expansions occurred January 1st, 2014. The analysis excludes five states that, prior to 2011, substantially expanded their Medicaid eligibility to cover parents and childless adults with full Medicaid benefits up to 100% FPL or higher and remained open to new enrollees. We match Medicaid expansion dates to the data on state-year-quarter. Our classification closely follows Wherry and Miller (2016), and Simon, Soni, and Cawley (2017).

3.3 Outcomes

We construct the number of prescriptions for all three medications. We convert this variable to prescriptions per 100,000 state residents 18 to 64 years, the age group most affected by Medicaid expansion (Simon, Soni, and Cawley 2017). We could have used the number of Medicaid enrollees in the denominator. However, this variable construction would lead to an endogenous numerator and denominator in our outcome variable, which would complicate interpretation of regression coefficients as we could not be certain whether changes reflected

changes in prescriptions, the enrolled population, or both. Reliance on population, which is not influenced by Medicaid expansion (Goodman 2017), alleviates this difficulty. However, as we show in sensitivity analyses, our results are robust to scaling by Medicaid enrollment.

We construct total payments and Medicaid payments. We convert payments to 2017 dollars using the Gross Domestic Product deflator (Dunn, Grosse, and Zuvekas 2018). We do not scale payment outcomes by population as we are interested in overall payments, but we control for the nonelderly adult population in these regressions.

Payment estimates from the SDUD are overstated because the data set does not include rebates from pharmaceutical companies to Medicaid. Government estimates suggest that states recoup 45% (13%) of branded (generic) medication payments in the form of rebates (Levinson 2011). We use this information to adjust our payment estimates. Zyban is off-patent in all years of our study while Chantix and Nicotrol are single source brand name medications. Our calculation is as follows. Pre-expansion in expansion states; Zyban accounted for 82% of the medications in our sample while Chantix accounted for 17% and Nicotrol accounted for 1%. We assume that Medicaid recoups 13% of Zyban (mostly generic medications) and 45% of Chantix and Nicotrol payments (branded medications). Overall for these three medications, we assume that Medicaid recoups, on average, 19% of payments in the form of rebates ($19\% = [0.82 \times 0.13] + [0.17 \times 0.45] + [0.01 \times 0.45] \times 100\%$).

3.4 Empirical model

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

$$C_{s,t} = \alpha_0 + \alpha_1 \text{Expand}_{s,t} + X_{s,y} \alpha_1 + \theta_s + \tau_t + \Delta_{s,t} + \varepsilon_{s,t} \quad (1)$$

$C_{s,t}$ is the number of prescription fills and refills per 100,000 non-elderly adults in state s in period t . $\text{Expand}_{s,t}$ is an indicator for whether or not a state has expanded its Medicaid program under the ACA in quarter t . $X_{s,y}$ is a vector of state-year level characteristics that vary at the annual (not quarterly) level: demographics from the Annual and Social and Economic Supplement to the Current Population Survey (ASEC) (age, sex, race/ethnicity, foreign birth, education, veteran status, and poverty status) and the unemployment rate among those ages 18 to 64 years calculated by the authors from the ASEC (Flood et al. 2017). θ_s and τ_t are vectors of state and year-by-quarter fixed effects, and $\Delta_{s,t}$ is a vector of state-specific linear time trends. $\varepsilon_{s,t}$ is the error term. We do not include the smoking rate as a control in Equation (1) as this variable is plausibly influenced by Medicaid expansion (Simon, Soni, and Cawley 2017). Including such variables can lead to over-controlling bias (Angrist and Pischke 2009).

We estimate regressions for number of prescription fills using OLS and payment regressions using a generalized linear model (GLM) with a gamma distribution, based on a modified Park test outlined by Manning and Mullahy (2001). In all specifications, we report average marginal effects. All coefficient estimates have the same interpretation: the average change in the outcome attributable to a state transitioning from no expansion to Medicaid expansion. We cluster standard errors by the state (Bertrand, Duflo, and Mullainathan 2004).

3.5 Internal validity

A necessary assumption for the DD model to recover causal estimates is that the treatment group (expansion states) and the comparison group (non-expansion states) 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 three ways.

(i) We examine unadjusted trends in the pre-expansion period in outcomes for treatment and comparison groups. If we find that the outcomes trended similarly in the pre-treatment period across these groups, such trends provide suggestive evidence that the SDUD data satisfy the parallel trends assumption. (ii) Using pre-expansion data only, we estimate Equation (2):

$$C_{s,t} = \gamma_0 + \gamma_1 Treat_s * Time_t + X_{s,y} \gamma_2 + \theta_s + \tau_t + \mu_{s,t} \quad (2)$$

where $Treat_s * Time_t$ is an interaction between an indicator for expansion states and a linear time trend; all other variables are as defined earlier. If we cannot reject the null hypothesis that γ_1 is zero, this finding provides support that the SDUD can satisfy the parallel trends assumption.

(iii) We estimate an event study in the spirit of Autor (2003) as outlined in Equation (3):

$$C_{s,t} = \delta_0 + \sum_{j=-12}^{-2} \sigma_j (Rel_qt_{s,j} = 1) + \sum_{k=0}^{14} \rho_k (Rel_qt_{s,k} = 1) + X_{s,y} \delta_1 + \theta_s + \tau_t + \pi_{s,t} \quad (2)$$

We first center the data around the expansion quarter for expansion states ($Rel_qt_{s,j}$ and $Rel_qt_{s,k}$). Second, for expansion states, we construct leads and lags around the expansion quarter. Non-expansion states are coded as zero for all lead and lag variables (Lovenheim 2009). The reference period in the event study is the quarter prior to Medicaid expansion. The vector σ_j reflects the effect of Medicaid expansion at quarters 12 to 2 in the pre-policy period while the vector ρ_k captures effects in the expansion quarter through 14 quarters post-expansion.

We exclude data points more than 12 quarters in advance and 14 quarters following when the state expanded Medicaid, because cells (in event-time) are small outside this range (Lovenheim 2009, Maclean, Popovici, and Stern 2018). We exclude state-specific linear trends from the parallel trends testing in (ii) and the event study (Wolfers 2006) and states with substantial expansions before 2011 are excluded.

4. Results

4.1 Summary statistics

Table 1 reports summary statistics for pre-expansion and non-expansion states 2011 to 2013. States with substantial expansions before 2011 are excluded. We report p -values from t -tests assessing the statistical significance of the differences between the groups. The annual number of cessation medication prescriptions per 100,000 nonelderly adults was 257 per quarter in expansion states and 205 per quarter in non-expansion states. While not identical, the two groups of states were similar across observed characteristics.

4.2 Internal validity testing

Figure 1 provides graphical analysis of trends in cessation prescription medications over our study period. We center the data around the expansion quarter for expansion states and Q2 2014 for non-expansion states; the average expansion quarter for adopting states (excluding early adopters). We impose endpoint restrictions: periods more than 12 quarters in advance and 14 quarters after the expansion year/Q2 2014 are dropped. Cell sizes outside this range are small. However, results, available on request, are robust to including these periods. In the years prior to expansion/Q2 2014, prescriptions 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. Further, this finding suggests that any changes in expansion states prior to ACA Medicaid expansion, such as changes in cost-sharing, had minimal effect on cessation prescriptions. Post-expansion/Q2 2014, we observe that the expansion and non-expansion states appear to have followed different trends. In expansion states, Medicaid-financed prescriptions increased, while in non-expansion states prescriptions were relatively stable.

We report results generated in formal regression-based parallel trends testing in Table 2. We cannot reject the null hypothesis that expansion states and non-expansion states followed similar trends in smoking cessation prescription fills over the period 2011 to 2013: $\hat{\gamma}_1$ is not statistically different from zero. Moreover $\hat{\gamma}_1$ is small in magnitude (0.33, or 0.1% relative to the baseline mean of 257) and the standard error estimate is relatively small (2.6).⁵

We report event study results graphically in Figure 2; coefficient and standard error estimates for all lead/lag variables are reported in Appendix Table 2. The coefficient estimates are small in magnitude and not statistically distinguishable from zero (95% confidence intervals include zero) in the pre-expansion period. Coefficients become positive and statistically distinguishable from zero in the post-expansion period, and increase in magnitude over time. In particular, we observe a ramp up period of roughly one year. While we cannot study the specific channels that drive this pattern we observe in the data, the increasing effect sizes over time in the post-expansion period are in line with Medicaid enrollment continuing to rise as eligible adults learn about the expansion as well as enrollees being better able to access services over time. For instance, improved access may occur due

⁵Thus, we can rule out differences in pre-expansion trends greater than -1.9% and $+1.1\%$ between expansion and non-expansion states (calculations based on comparison of tails of the 95% confidence interval with baseline mean).

to increased benefit awareness (among patients and providers) and/or ability to navigate the healthcare delivery system (e.g., enroll in Medicaid, locate a physician, schedule an appointment, and fill a prescription). On the other hand, enrollees, through interaction with the healthcare system and/or information on smoking cessation that occurred with the roll-out of ACA Medicaid expansion or other components of the ACA, may over time decide to stop smoking and, in turn, access prescription cessation medications. This ramp up may also be influenced by crowd-out and/or moral hazard effects.

For more comparison with our DD-style model, we also report estimates generated in an event study that includes state-specific linear trends. Results are broadly robust to including these time trends; see Figure 3. However, we note that there is more evidence of differential pre-trends in expansion and non-expansion states. The pre-trends suggest that, pre-expansion, expansion states were trending downward relative to non-expansion states. This pattern of results works against our main finding: increased fills post-expansion. Further, as noted by Wolfers (2006), including trends in models with dynamics, such as an event study, can complicate the interpretation of regression coefficients.

We interpret the results of our internal validity testing to imply that our data is plausibly able to satisfy the parallel trends assumption. We report DD regressions for the remainder of the study to provide a concise summary of treatment effects.

4.3 Baseline regression results

Table 3A reports our main DD results for number of fills. We report results based on specifications which include a progressively larger set of controls: Model (1) state- and period-fixed effects only, Model (2) adding time-varying state-level variables, and Model (3) adding state-specific linear time trends. Appendix Table 3 reports a full set of coefficient estimates from our full specification: Model (3). Coefficient estimates decline in magnitude as we include progressively more controls for state-level heterogeneity; for instance the coefficient estimate on Medicaid expansion generated in Models (1) and (2) are nearly double the estimate in Model (3).

Post-expansion, and in our preferred Model (3), we find that expansion states experienced an increase of 88 Medicaid-financed prescriptions per 100,000 nonelderly adults per quarter relative to non-expansion states. Compared to the baseline mean in expansion states pre-expansion, this estimate reflects a 34% increase. We report coefficient estimates generated in Model (3) in the remainder of the paper for brevity.

Several studies point out that the ‘strength’ of the ACA Medicaid expansion was determined by the state’s pre-ACA Medicaid generosity. We follow Simon, Soni, and Cawley (2017) and estimate a variant of Equation (1) that decomposes our Medicaid expansion variable into a mild partial expansion (DE, DC, MA, NY, and VT), partial expansion (CA, CT, HI, and MN), and a full expansion (all other states listed in Table 1).⁶ Mild partial expansion states had all implemented the policy change prior to 2011 (the first year of our study), therefore

⁶Our coding the strength of expansion variable departs from Simon et al in that we code Wisconsin as a non-expansion state as this state has not increased its income threshold beyond 100% of FPL. Simon et al code Wisconsin as a full expansion state. Details available on request.

we are only able to estimate partial and full expansion effects. Results are reported in Table 3B. While both coefficient estimates are positive, only the full expansion coefficient is statistically different from zero and suggests that prescriptions per 100,000 non-elderly increase by 94 or 37%.

As noted earlier in the manuscript, the ACA includes two key smoking-related policy changes: increased Medicaid income eligibility for a population with high smoking prevalence and improvements in coverage for FDA-approved smoking cessation medications. Coverage improvements are in terms of the number of included medications for all Medicaid enrollees, and zero cost sharing for newly eligible and pregnant women (see Section 2.1). Our estimate of a 34% increase in Medicaid-financed prescriptions reported in Table 3A conceivably captures the effects of both policy changes. We next estimate two variants of Equation (1) that plausibly allow us to isolate increased income eligibility effects from improved smoking cessation medication coverage effects. We note that these additional analyses are not likely to account for effects attributable to simplifications in the Medicaid enrollment processes, and/or improved information about the benefits of Medicaid and smoking cessation generally that may have occurred through ACA-related public outreach campaigns.

First, we exclude states that changed coverage for cessation prescription medications during our study period from our sample using coverage data from the Centers for Disease Control and Prevention (2018). Results are reported in Table 4A. The estimate generated in this sample is very similar to our main result: post-expansion quarterly Medicaid-financed prescriptions increased by 75 per 100,000 nonelderly adults (29%) in expansion states. While the absolute and relative effect sizes are somewhat smaller than our main estimates, 95% confidence intervals surrounding the point estimates overlap and thus we cannot rule out more similar effect sizes. Second, we control for whether a state Medicaid program covered each FDA-approved smoking cessation medication in Equation (1). More specifically, we include a separate indicator variable for any coverage of Zyban, Chantix, and Nicotrol. Results are essentially identical to our main estimate (Table 4B). This specification allows us to isolate the effect of ACA Medicaid expansion from improved coverage for the prescription medications we study within continuously eligible Medicaid populations. Post-expansion Medicaid-financed prescriptions increase by 88 per 100,000 nonelderly (34%) in expansion states. The coefficient estimates on the medication coverage variables are not statistically different from zero. These additional analyses suggest that our main DD estimate predominantly captures income eligibility effects for the newly eligible and not effects attributable to other coverage changes.

Table 5 shows estimates for payments. Medicaid financed the vast majority – 99% in expansion states prior to expansion – of FDA-approved smoking cessation medications. We observe that in expansion states relative to non-expansion states, quarterly total and Medicaid payments on these medications increased by 49% and 45% post-expansion. The corresponding estimates that are not adjusted for Medicaid rebates are: \$288,897 and \$266,048. The similarity in total and Medicaid payment coefficient estimates suggests that Medicaid – not patients – financed the increased prescriptions. Indeed, the 95% confidence intervals surrounding the point estimates overlap. In the final row of Table 5, we adjust our

payment estimates for average rebates assuming that states recoup 19% of Medicaid payments through the MDRP.

Our estimates for payments are larger (in relative terms) than our estimates for prescriptions. We raise the following possible reasons for this difference in relative effect sizes. First, prescription medication prices were rising over our study period (IQVIA Institute 2018), which could lead to this pattern of results. Second, the 95% confidence intervals surrounding the payments and prescription rate point estimates imply relative effect sizes that are more comparable, and indeed overlap. For instance, the bottom tail of the 95% confidence interval surrounding the total payments coefficient estimates implies a 13% increase, which lies below our prescription rate point estimate (34%). Thus, we do not wish to overstate any heterogeneity.

4.4 Separating new medication use from cost-shifting of ongoing medication use

We find that Medicaid-financed prescriptions increase 34% post-expansion in our preferred specification (Table 3A). However, to understand how Medicaid expansion influenced service use or simply shifted the financing of ongoing cessation medication use from patients and private insurers to the government, it is important to separate the extent to which this estimate reflects (i) new utilization of cessation medications and (ii) a change in the financing source for cessation medication use that would have occurred absent the policy (cost-shifting).

The former effect implies that Medicaid expansion may have improved public health directly by increasing the use of effective medications that can reduce smoking. The latter effect suggests that Medicaid expansion had no effect on public health (at least through prescription medication-assisted cessation) and instead altered the financing of cessation medication use that would have occurred without the expansion; i.e., crowd-out of medications financed by private insurers or a transfer from self-paying uninsured patients to Medicaid. Given the very low incomes of enrollees, shifting medication financing from self-paying uninsured smokers to Medicaid may be a worthwhile transfer even if it does not directly influence health. On the other hand, transferring financing from private insurers to Medicaid may be less desirable for social welfare. On net, because of these changes in financing, our primary estimates likely overstate Medicaid expansion effects on new use of cessation medications. We next attempt to explore the share of our estimate that is attributable to these two factors using additional data sources.

We first examine the importance of private coverage crowd-out and shifting payments from uninsured smokers to Medicaid following expansion. To do so, we use the 2011 to 2017 American Community Survey (ACS), a large government survey with information on insurance status, and regress the probability of private coverage and uninsurance on Medicaid expansion using Equation (1). We estimate linear probability models as our outcomes are binary (estimates reflect average marginal effects), and replace period fixed effects with year fixed effects in this analysis as the public use ACS does not include interview month or quarter. We focus on a sample of childless and non-disabled adults without Medicare coverage ages 18 to 64 years of age with family income 138% of FPL.⁷ In non-expansion states individuals with incomes between 100% and 138% of FPL can

receive subsidies to purchase private insurance, therefore we also estimate regressions on a comparable sample with incomes < 100% of FPL. Results are weighted by the ACS sample weights, but unweighted results are very similar and available on request. We estimate all specifications with our indicator for any Medicaid expansion and the strength of the expansion following Simon, Soni, and Cawley (2017). Results are reported in Table 6A (any expansion indicator) and Table 6B (strength of expansion indicators). We acknowledge that other studies have explored the effect of ACA Medicaid expansion on coverage rates, we simply wish to ensure that established relationships hold in our sample and specification.

In the specification with a single indicator for Medicaid expansion (Table 6A), we observe that the probability of being uninsured declined by five percentage points in both samples while the probability of holding private coverage declined by two percentage points following expansion, suggesting that only 28.6% of the increase in Medicaid enrollment crowded-out private insurance. Examination of the expansion strength indicator results suggests that effects are driven by states that experienced both a modest partial expansion and a full expansion. We note that in the < 100% of FPL sample, the coefficient estimate, while carrying a negative sign, on the full expansion indicator is not statistically different from zero in the any coverage specification. Similar to other studies within the ACA literature, we document at most only modest crowd-out of private insurance from Medicaid expansion (Frean, Gruber, and Sommers 2017, Kaestner et al. 2017, Courtemanche et al. 2017, Wherry and Miller 2016).

Next, we examine pre-ACA data on utilization of the prescription medications we study (Zyban, Chantix, and Nicotrol, and their generics) among low-income (< 138% of FPL) uninsured, Medicaid enrollee, and privately insured smokers.⁸ We use data on nonelderly adult smokers from the 2011 Tobacco Use Supplements (TUS) to the Current Population Survey merged with the 2011 ASEC. We follow a merge procedure outlined by Madrian and Lefgren (2000). We exclude respondents with Medicare coverage, but we do not exclude those with children or who are disabled as this latter group comprises most of the pre-ACA eligible population. We do not use more recent TUS data in a DD analysis because post-Medicaid expansion waves of the data do not include information on cessation medication utilization. Based on the merged 2011 TUS-ASEC data, we observe that use of prescription cessation medications (we consider all medications collectively) among low-income smokers is 1.6% for the uninsured, 9.4 % for Medicaid enrollees, and 5.9% for the privately insured.

Using these estimated rates of prescription medication use (which assumes that use rates are stable across insurance groups post-expansion) and our estimated 28.6% rate of private insurance crowd-out, we estimate that new Medicaid enrollees increased their prescription cessation medication use rates by 6.6 percentage points ($=28.6\% \times [9.4\% - 5.9\%] + 71.4\% \times [9.4\% - 1.6\%]$). We estimate that 70% ($=6.6/9.4 \times 100\%$) of our 34% increase in Medicaid-financed prescription medications is attributable to new use and the remainder is

⁷To define disability status, we make use of questions in the ACS that probe cognitive, ambulatory, independent living, self-care, vision, and hearing difficulties. If a respondent indicates any of these difficulties, we exclude them from the analysis sample. More details are available on request.

⁸TUS sample sizes are very small if we restrict the sample to < 100% of FPL.

attributable to use that would have occurred in the absence of the Medicaid expansions (cost-shifting). Our calculations further imply that 15% of the estimated 30% cost-shifting was attributable to private insurance and 85% was attributable to self-paying uninsured patients. Accounting for cost-shifting, we find that Medicaid expansion led to a net increase of 24% ($=34\%*0.70$) in cessation prescription fills. Thus, our estimates suggest that Medicaid expansion increased the use of effective smoking cessation prescription medications.

For the 1.6% of uninsured smokers previously paying out of pocket for cessation medications and gained Medicaid coverage, this shift in financing likely represented an important reduction in medical financial burden given the high costs of the medications we study and low income levels among the newly eligible. For instance, medication costs range from \$100 to \$500 in addition to healthcare professional consultation fees, and the annual income for a family of two earning 138% of FPL, the highest income level eligible under Medicaid expansion, is \$23,336 in 2019. The newly available funds may be allocated to other health-enhancing activities and/or may simply relieve strain related to financial problems (Maclean et al. 2015, Maclean, Webber, and French 2015). We do not wish to imply that there is no health benefit for these individuals, rather we argue that any health benefit operates through different pathways.

We note that if prescription cessation medication use rates among Medicaid enrollees increased or decreased following expansion, then this change will alter our estimates of new use. For example, conditional on having Medicaid, use rates may have increased due to better coverage of these medications (although we did not find evidence for this in Table 4A). Further, the pre-ACA Medicaid population was predominately composed of parents and the disabled, which might imply that our estimate of medication use from the TUS is too as these low traditionally eligible populations have lower smoking rates than the newly eligible. We can investigate the sensitivity of our estimate of new use due to the expansion if the policy also lead to such changes in medication use rates conditional on having Medicaid. We estimate that Medicaid expansion led to a net increase of 24% ($=34\%*0.70$) in cessation prescription fills, but this number could range from 15.2% to 32.3% if prescription cessation medication use conditional on having Medicaid also changed by 25% in either direction from the pre-expansion use rate. We argue that under reasonable assumptions, much of the increase in Medicaid-financed prescriptions that we estimate reflects new use and not cost-shifting.

We note that an assumption embedded in our analysis of crowd-out is that the size of crowd-out estimated in the ACS (which includes smokers and non-smokers as the ACS does not include information on smoking) is the same as the size of crowd-out for individuals who use smoking cessation medications. To the best of our knowledge, there is no evidence on whether ACA Medicaid expansion induced crowd-out that varied across smoking status. Thus, we may over- or under-estimate the relevant crowd-out for our purposes, which is a study limitation. Further, cost-sharing is likely lower in Medicaid than private insurance, suggesting that the price decrease will amplify crowd-out effects. Patients may also shift from less costly to more costly medications post-expansion due to price differences. Finally, while we have conducted sensitivity analyses, our estimates are limited by data availability

on the relevant sub-population of interest (i.e., smokers who drop private coverage when the public option is available).

5. Extensions and sensitivity analyses

5.1 Heterogeneity by prescription medication type and pre-ACA Medicaid coverage

The three medications in our analyses vary in how much state Medicaid programs pay pharmacies – the average payment per prescription adjusted for average rebates for Zyban, Chantix, and Nicotrol was \$38, \$147, and \$77, respectively in the SDUD over our study period. Additionally, the medications have different efficacy and side effects. Such differences may lead to different effects of Medicaid expansion on prescriptions. We next explore heterogeneity by estimating separate regressions for each medication (Table 7).

We find that, post-expansion, Medicaid-financed Zyban prescriptions increased 33% and Chantix Medicaid-financed prescriptions increased 41% (relative to baseline means) in expansion states relative to non-expansion states. We find no statistically significant evidence for Medicaid-financed Nicotrol prescriptions, although the coefficient estimate is positive suggesting an increase of 14% post-expansion.

We estimate separate regressions for states that, prior to passage of the ACA, covered and did not cover all three prescription medications that we study (Table 8). We measure coverage as of January 2010 using policy data from the CDC STATE system. We find similar increases in prescriptions post-expansion in both groups of states; indeed 95% confidence intervals overlap.

5.2 Over-the-counter cessation medications

The ACA requires that state Medicaid programs cover OTC FDA-approved NRTs. To explore expansion effects on Medicaid-financed prescriptions for OTC NRTs, we re-estimate Equation (1) using the FDA-approved OTC NRTs as variables analogous to our prescription medication variables. State Medicaid programs typically require a prescription for OTC NRTs if an enrollee wishes to have the medication reimbursed by Medicaid (Ku, Bruen, et al. 2016). Thus, we can accurately measure Medicaid-financed OTC NRTs in the SDUD but we will not capture OTC NRTs for which enrollees pay entirely out-of-pocket or finance through some other means (e.g., free medications). We examine common OTC NRTs (e.g., Nicoderm, Nicorette, generic brands) using the method outlined in Section 3.1.⁹ Like Nicotrol, OTC NRTs contain nicotine, but at a lower dose than tobacco products, and allow the smoker to reduce nicotine consumption and minimize withdrawal symptoms by acting on chemicals in the brain. We also combine Nicotrol and OTC medications, and re-estimate Equation (1). We find no statistically significant evidence that Medicaid-financed NRTs (OTC measured separately or combined with prescriptions) increased post-expansion (Table 9).

⁹Specifically, we include the following OTC brand medications: Commit, Habitrol, Nicoderm, Nicorette, Prostep, Thrive, and Kirkland Signature Quit. We also include the generic drug nicotine. We note there are other OTC NRT brands available, but we believe that we are able to capture a sizeable portion of this market with these medications, in particular as we include the generic version of nicotine, which is an ingredient in all OTC NRTs.

It is somewhat surprising that we find increases in prescriptions but no corresponding increases in the OTCs post-expansion. We propose possible reasons. OTC NRTs are less expensive than prescription medications and OTC NRTs may be more affordable to uninsured smokers seeking to quit, but when Medicaid coverage reduces prescription medication out-of-pocket prices then smokers switch to prescription products. The clinical literature suggests that prescription medications are more efficacious than OTC NRTs (Aubin et al. 2008), thus newly insured smokers (perhaps in combination with their healthcare providers) seeking to quit smoking may be selecting the optimal cessation method post-expansion. Alternatively, smokers may have continued to pay out-of-pocket for OTC NRTs, perhaps because they did not realize Medicaid would cover these medications with a prescription and/or because they had difficulty accessing a healthcare provider; access to providers is a well-established challenge within Medicaid (Decker 2012). Indeed, pre-ACA research documents that enrollee knowledge of Medicaid coverage for cessation services was poor (McMenamin et al. 2004). The pattern of no effect in Medicaid-financed claims and large positive effects for overall OTC purchases shown by Cotti, Nesson, and Tefft (2018) using ACA Medicaid variation in eligibility is in line with this explanation. Finally, we note that, while not statistically different from zero, our coefficient estimate implies a 10% increase in Medicaid-financed OTC cessation medication prescriptions post-expansion with the top tail of the 95% confidence interval implying an even larger increase. Filling rates for NRTs are low in the SDUD and we may be under-powered to detect effects.

5.3 Sensitivity analyses

We conduct several sensitivity analyses. Our findings are very stable, so we simply summarize these analyses. First, we weight regressions by the state non-elderly adult population (Appendix Table 4). Second, in our main analyses, we study the filling rate per 100,000 non-elderly adults, which imposes a specific functional form on how population enters the regression model. In Appendix Table 5 we report results in which our outcome is the prescription count, controlling for the state nonelderly population. Third, we scale prescriptions by Medicaid enrollees (vs. the nonelderly adult population) using CMS data maintained by the University of Kentucky Center for Poverty Research (2017) (Appendix Table 6). Fourth, we compare alternative specifications that differentially account for between-state heterogeneity: including state-specific quadratic time trends state-specific cubic time trends (Appendix Table 7). Fifth, we re-estimate Equation (1) using alternative Medicaid expansion classifications used in the economics literature; e.g., Courtemanche et al. (2017), Simon, Soni, and Cawley (2017), Wherry and Miller (2016), and Maclean and Saloner (2019). While the exact point estimates vary to some extent, 95% confidence intervals overlap (Appendix Table 8). Sixth, we explore the robustness of our results to different imputation approaches to address censored data points in the SDUD. Specifically, for prescriptions, we impute censored cells with a value of zero (the lowest possible censored value) and ten (the highest possible censored value). For payments, we assign the average cessation medication payment value for NDC cells with 11 fills (the smallest non-censored value in the SDUD) and a value of one cent (the lowest possible value), this procedure plausibly allows us to capture the full range of payments for the censored cells. Results are not appreciably different across imputation approaches (Appendix Tables 9A and 9B). Seventh, we sequentially remove each expansion state and re-estimate Equation (1) to

ensure that our findings are not driven by the experiences of one particular state (Appendix Table 10).

Another empirical concern in policy analyses is program induced migration; that is the implementation of Medicaid expansion may have led some individuals to migrate to expansion states (Moffitt 1992). To the best of our knowledge, there is no evidence that the ACA Medicaid expansion induced such behaviors (Goodman 2017). We confirm this null finding using data on past-year cross-state migration rates from the 2011 to 2017 ASEC (Appendix Table 11).¹⁰

6. Discussion

In this study we offer new evidence on the effects of ACA Medicaid expansion on the use of prescription medications for smoking cessation, and crowd-out from both private insurance and self-paying patients. The ACA-related expansions increased Medicaid enrollment by 27% (Gates et al. 2016), and 38% of this population smokes (DiGiulio et al. 2016). The expansions were targeted at a group of low-income adults with a high rate of both uninsurance and smoking risk, and included a generous set of cessation prescription medications. Our findings imply that, post-expansion, Medicaid-financed cessation prescriptions increased 34% in expansion states. We provide evidence that this estimate largely, but not fully, reflects new prescription cessation medication use. The majority of cost-shifting was likely from uninsured self-paying smokers (85%), and not private insurance companies (15%), to Medicaid. Given the low-income levels of enrollees, this transfer in financing from uninsured patients to Medicaid plausibly led to a reduction in medical financial burden. After accounting for private coverage crowd-out and cost-shifting from self-paying patients to Medicaid, we estimate that Medicaid expansion led to a 24% increase in new cessation medication prescription fills.

Thus, our findings suggest, within the context of smoking cessation, Medicaid expansion improved access to care and potentially improved public health by spurring new use of effective cessation medications. Some of the increased Medicaid-financed prescriptions reflects cost-shifting rather than new use and implies that Medicaid expansion shifted financing from patients and private insurance to state and federal governments. However, costing-shifting to Medicaid may reduce financial burden and associated stress for previously self-paying smokers and/or low-income individuals faced with the high costs of private insurance. For such individuals Medicaid eligibility may confer additional benefits not captured by changes in service use. We document that expansion effects are likely attributable to increased income eligibility and not mandated improvements in coverage for smoking cessation medications for all enrollees.

¹⁰We have also explored the effect of Medicaid expansion on current smoking and past-year quit attempts using the 2011–2017 Behavioral Risk Factor Surveillance Survey from the Centers for Disease Control and Prevention. We select a sample of low income non-elderly adults and estimate a DD-style regression model. We observe no statistically significant evidence that smoking outcomes were influenced by Medicaid expansion. Our findings are therefore in line with the previous studies that do not find statistically significant changes in smoking outcomes post-expansion (Courtemanche et al. 2017, 2018, Cawley, Soni, and Simon 2018). Full details are available on request.

Our study has limitations. We lack data on patients and providers. We use an approximation to adjust payment variables for average rebates between manufacturers and states. We have information on a single payer, Medicaid. We attempt to isolate the extent to which our estimates reflect new utilization and shifting cessation medication financing from other payers to Medicaid (i.e., crowd-out of private insurance and reductions in self-pay by uninsured smokers), but there may be remaining concerns that our ‘corrected’ estimate does not fully isolate new use due to assumptions that we imposed. In particular, our ability to correct our estimate of new utilization for these behaviors is limited by our data. We therefore interpret this analysis with some caution and suggest that readers do the same. Given the wide scope of the ACA, our estimated effects plausibly reflect other changes that occurred with this complex and wide-reaching piece of healthcare legislation. We note that many studies that investigate ACA Medicaid effects are also subject to this concern. Finally, some data points in the SDUD are censored, however, our estimates are robust to a range of alternative imputation procedures.

Our findings offer new evidence on the effects of public insurance expansions. Such expansions increased use of evidence-based healthcare services with a particularly policy-relevant group: low-income individuals who suffer from tobacco use, a chronic, costly, and harmful condition. However, while Medicaid expansion plausibly has the potential to improve public health through reduced smoking, the policy also shifted the financing of cessation efforts from private insurers and patients to state and federal governments.

The smoking-related literature on ACA Medicaid is growing and offering a more complete picture of public insurance effects on smoking outcomes. Our study documents that Medicaid-financed smoking cessation prescriptions increase post-expansion. We find that some of this increase is due to cost-shifting of ongoing medication use from private insurance and self-paying uninsured patients to Medicaid, most of the increase reflects new prescription medications. We find no evidence that Medicaid expansion increased over-the-counter nicotine replacement therapy medications (NRTs) financed by Medicaid, in contrast to a study by Cotti, Nesson, and Tefft (2018) using household scanner data and OTC purchased without a prescription. Our study of smoking outcomes does not suggest that Medicaid expansion lead to reductions in smoking among likely Medicaid beneficiaries, thus contributing an estimate to a mixed body of literature (Cotti, Nesson, and Tefft 2018, Courtemanche et al. 2017, Cawley, Soni, and Simon 2018, Koma et al. 2017, Courtemanche et al. 2018). Overall, the literature to date suggests that ACA Medicaid expansion increased service use but evidence on reduced smoking is mixed, at least in the first four years post-expansion.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

Acknowledgements:

Johanna Catherine Maclean and Michael F. Pesko were supported by a Research Scholar Grant – Insurance, RSGI-16-019-01 – CPHPS, from the American Cancer Society. We thank Joachim Marti, Sayeh Nikpay, Douglas Webber, and session participants at the International Health Economics Association World Congress and American

Society of Health Economists Annual Meeting for helpful comments. The views expressed in this paper are the authors' and do not reflect the views of the American Cancer Society, the Agency for Healthcare Research and Quality, the U.S. Department of Health and Human Services, the National Bureau of Economic Research, or the Institute for Labor Economics. All errors are our own.

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Figure 2. Event study of the effect of Medicaid expansions on smoking cessation prescription fills and refills per 100,000 non-elderly adults 2011–2017

Source: State Drug Utilization Data.

Notes: Unit of observation is a state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, and state and period fixed effects. The omitted period is the quarter prior to expansion for expansion states. Non-expansion states are coded zero in all state-quarter lag and lead variables. Data points more than 12 periods in advance of or 14 periods following Medicaid expansion are excluded. 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 Appendix Table 1). N=1,207. Mean value in expansion states in pre-expansion period is 257. Coefficient estimates are average marginal effects.

Figure 3. Event study including state-specific linear time trends of the effect of Medicaid expansions on smoking cessation prescription fills and refills per 100,000 non-elderly adults 2011–2017

Source: State Drug Utilization Data.

Notes: Unit of observation is a state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, state and period fixed effects, and state-specific linear time trends. The omitted period is the quarter prior to expansion for expansion states. Non-expansion states are coded zero in all state-quarter lag and lead variables. Data points more than 12 periods in advance of or 14 periods following Medicaid expansion are excluded. 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 Appendix Table 1). N=1,207. Mean value in expansion states in pre-expansion period is 257. Coefficient estimates are average marginal effects.

Table 1.

Summary statistics for expansion and non-expansion states: 2011–2013

Sample:	Expansion states	Non-expansion states	Difference (<i>p</i> -value)*
<i>Medicaid-financed cessation medications per quarter:</i>			
Prescription fills and refills per 100,000 persons 18 to 64 years	257	205	<0.0000
<i>State-year level characteristics</i>			
Age	37.8	37.2	<0.0000
Female	0.51	0.51	0.0250
Male	0.49	0.49	0.0250
White	0.81	0.80	0.5430
African American	0.09	0.13	<0.0000
Other race	0.11	0.07	<0.0000
Hispanic	0.12	0.09	0.0030
Born outside the U.S.	0.11	0.08	<0.0000
Less than high school	0.32	0.34	<0.0000
High school	0.24	0.24	0.7965
Some college	0.22	0.23	0.0068
College degree	0.21	0.19	<0.0000
Veteran	0.10	0.10	0.0018
Unemployment rate	0.08	0.07	0.0001
Poverty rate	0.14	0.15	0.0083
Observations	324	228	--

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 Appendix Table 1).* Two-tailed *t*-tests applied. The *p*-value from this test is reported.

Table 2.

Parallel trends testing for cessation prescription medication fills and refills per 100,000 non-elderly adults in expansion and non-expansion states: 2011–2017

Outcome:	Prescription fills and refills
<i>Mean value in expansion states, pre-expansion period</i>	257
Expansion state * time trend	0.33 (2.62)
Observations	548

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, and state and period fixed effects. Standard errors are clustered at the state level and are reported in parentheses. Early expansion states excluded (see Appendix Table 1). Coefficient estimates are average marginal effects.

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

Table 3A.

Effect of Medicaid expansions on cessation prescription medication fills and refills per 100,000 non-elderly adults using differences-in-differences: 2011–2017

Outcome:	Prescription fills and refills
<i>Mean value in expansion states, pre-expansion period</i>	257
Model (1)	159.93 *** (27.93)
Model (2)	160.25 *** (27.48)
Model (3)	88.09 *** (30.76)
Observations	1,424

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

Model (1) includes state and period fixed effects.

Model (2) includes state characteristics listed in Table 1, and state and period fixed effects.

Model (3) includes state characteristics listed in Table 1, state and period fixed effects, and state-specific linear time trends.

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

Table 3B.

Effect of Medicaid expansions on cessation prescription medication fills and refills per 100,000 non-elderly adults using differences-in-differences and Medicaid expansion strength coding scheme following Simon et al (2017): 2011–2017

Outcome:	Prescription fills and refills
<i>Mean value in expansion states, pre-expansion period</i>	257
Partial expansion	46.79 (36.88)
Full expansion	94.14 *** (34.43)
Observations	1,424

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. See text for a discussion of expansion strength. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, state and period fixed effects, and state-specific linear time trends. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

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

Table 4A.

Effect of Medicaid expansions on smoking cessation prescription medication fills and re-fills per 100,000 non-elderly adults using differences-in-differences using states that did not change FDA-approved cessation medication Medicaid coverage over the study period: 2011–2017

Outcome:	Prescription fills and refills
<i>Mean value in expansion states, pre-expansion period</i>	262
Expansion	75.38** (35.99)
Observations	952

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, state and period fixed effects, and state-specific linear time trends. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

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

Table 4B.

Effect of Medicaid expansions on cessation prescription medication fills and refills per 100,000 non-elderly adults using differences-in-differences and controlling for Medicaid coverage of FDA-approved cessation medications: 2011–2017

Outcome:	Prescription fills and refills
Sample: all cessation medications	
<i>Mean value in expansion states, pre-expansion period</i>	257
Expansion	88.23 *** (31.11)
<i>Medicaid coverage for specific cessation medications</i>	
Medicaid covers Zyban	–30.84 (71.66)
Medicaid covers Chantix	20.32 (17.72)
Medicaid covers Nicotrol	7.81 (34.84)
Observations	1,424

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, state and period fixed effects, and state-specific linear time trends. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

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

Table 5.

Effect of Medicaid expansions on cessation prescription medication payments using differences-in-differences: 2011–2017

Outcome:	Total payments	Medicaid payments
<i>Mean value in expansion states, pre-expansion</i>	\$594,672	\$587,585
Mean estimate adjusted by 19% estimate to account for rebates (see text for details)	\$481,684	\$475,944
Expansion	\$288,897 ***	\$266,048 **
	(108,233)	(109,844)
Mean estimate adjusted by 19% estimate to account for rebates (see text for details)	\$234,007	\$215,499
Observations	1,424	1,424

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. All models estimated with GLM using a gamma distribution and control for state characteristics listed in Table 1, state non-elderly adult population, state and period fixed effects, and state-specific linear time trends. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

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

Table 6A.

Effect of Medicaid expansions on insurance coverage using differences-in-differences: 2011–2017

Outcome:	Private insurance	No insurance
Sample: 138% of FPL		
<i>Proportion in expansion states, pre-expansion period</i>	0.39	0.46
Expansion	−0.02 **	−0.05 ***
	(0.01)	(0.01)
Observations	891,054	891,054
Sample: < 100% of FPL		
<i>Proportion in expansion states, pre-expansion period</i>	0.38	0.45
Expansion	−0.02 **	−0.05 ***
	(0.01)	(0.01)
Observations	623,731	623,731

Source: American Community Survey (ACS).

Notes: Unit of observation is the person-state-year. We exclude disabled adults, elderly adults, and households with children. All models estimated with an LPM and control for personal characteristics, state characteristics listed in Table 1, state non-elderly adult population, state and year fixed effects, and state-specific linear time trends. Data are weighted by ACS sample weights. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

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

Table 6B.

Effect of Medicaid expansions on insurance coverage using differences-in-differences and Medicaid expansion strength coding scheme following Simon et al (2017): 2011–2017

Outcome:	Private insurance	No insurance
Sample: 138% of FPL		
<i>Proportion in expansion states, pre-expansion period</i>	0.39	0.46
Partial expansion	−0.04 *** (0.01)	−0.02 ** (0.01)
Full expansion	−0.05 *** (0.01)	−0.02 * (0.01)
Observations	891,054	891,054
Sample: < 100% of FPL		
<i>Proportion in expansion states, pre-expansion period</i>	0.38	0.45
Partial expansion	−0.04 *** (0.01)	−0.02 ** (0.01)
Full expansion	−0.06 *** (0.01)	−0.01 (0.01)
Observations	623,731	623,731

Source: American Community Survey (ACS).

Notes: Unit of observation is the person-state-year. See text for a discussion of expansion strength. We exclude disabled adults, elderly adults, and households with children. All models estimated with an LPM and control for personal characteristics, state characteristics listed in Table 1, state non-elderly adult population, state and year fixed effects, and state-specific linear time trends. Data are weighted by ACS sample weights. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

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

Table 7.

Heterogeneity in the effect of Medicaid expansions on cessation prescription medication fills and refills per 100,000 non-elderly adults using differences-in-differences: 2011–2017

Outcome:	Zyban	Chantix	Nicotrol
<i>Mean value in expansion states, pre-expansion period</i>	210	44	3
Expansion	69.56 ^{***}	18.10 ^{**}	0.43
	(25.83)	(7.38)	(0.35)
Observations	1,424	1,424	1,424

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, state and period fixed effects, and state-specific linear time trends. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

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

Table 8.

Heterogeneity in the effect of Medicaid expansions on cessation prescription medication fills and refills per 100,000 non-elderly adults by Medicaid coverage for cessation prescription medications pre-expansion using differences-in-differences: 2011–2017

Outcome:	Prescription fills and refills
Sample: Medicaid covered all prescription medications pre-expansion	
<i>Mean value in expansion states, pre-expansion period</i>	266
Expansion	80.26** (34.76)
Observations	1,004
Sample: Medicaid did not cover all prescription medications pre-expansion	
<i>Mean value in expansion states, pre-expansion period</i>	219
Expansion	96.48* (50.03)
Observations	420

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, state and period fixed effects, and state-specific linear time trends. Standard errors are clustered at the state level and are reported in parentheses. Pre-expansion Medicaid coverage is measured in January 2010 using CDC STATE policy data. Coefficient estimates are average marginal effects.

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

Table 9.

Effect of Medicaid expansions on over-the-counter (OTC) nicotine replacement therapies (NRT) fills and re-fills per 100,000 non-elderly adults using differences-in-differences: 2011–2017

Outcome:	NRT fills and re-fills
Sample: OTC NRTs only	
<i>Mean value in expansion states, pre-expansion period</i>	69
Expansion	7.12 (14.39)
Observations	1,424
Sample: OTC NRTs and Nicotrol	
<i>Mean value in expansion states, pre-expansion period</i>	71
Expansion	7.55 (14.51)
Observations	1,424

Source: State Drug Utilization Data.

Notes: Unit of observation is the state-year-quarter. All outcomes are converted to the rate per 100,000 non-elderly adults. All models estimated with OLS and control for state characteristics listed in Table 1, state and period fixed effects, and state-specific linear time trends. Standard errors are clustered at the state level and are reported in parentheses. Coefficient estimates are average marginal effects.

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