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Early Effects of the Affordable Care Act on Health Care Access, Risky Health Behaviors, and Self-Assessed Health

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The goal of the Affordable Care Act (ACA) was to achieve nearly universal health insurance coverage through a combination of mandates, subsidies, marketplaces, and Medicaid expansions, most of which took effect in 2014. We use data from the Behavioral Risk Factor Surveillance System to examine the impacts of the ACA on health care access, risky health behaviors, and self-assessed health after two years. We estimate difference-in-difference-in-differences models that exploit variation in treatment intensity from state participation in the Medicaid expansion and pre-ACA uninsured rates. Results suggest that the ACA led to sizeable improvements in access to health care in both Medicaid expansion and nonexpansion states, with the gains being larger in expansion states along some dimensions. However, we do not find clear effects on risky behaviors or self-assessed health.

JEL Classification: I12, I13, I18

1. Introduction

The goal of the Patient Protection and Affordable Care Act (ACA) was to achieve nearly universal health insurance coverage in the United States through a combination of policies largely implemented in 2014 (Obama 2016). Several recent studies, including Frean, Gruber, and Sommers (2017) and Courtemanche et al. (2017), have shown that the ACA led to gains in insurance coverage. The objective of this article is to evaluate whether or not such coverage increases translated to changes in access to care, risky health behaviors, and, ultimately, short-run health outcomes.

A number of 2014 ACA provisions involved overhauling nongroup insurance markets in an effort to ensure that one's health history did not provide a barrier to obtaining coverage. Specific regulations included guaranteed issue laws, which forbid insurers from denying coverage on the basis of an applicant's health status, and modified community rating, which imposes uniform

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premiums regardless of observable applicant characteristics aside from age and smoking status. In addition, the federal government established a health insurance marketplace to facilitate insurance purchases for individuals and small businesses. Each state was given the option of establishing its own insurance marketplace, and 15 did so in 2014 (KFF 2014).

These reforms alone would likely lead to an adverse selection death spiral, with the influx of high-cost beneficiaries causing relatively low-cost beneficiaries to drop their coverage, thus driving up premiums for those remaining in the insurance pool (Courtemanche and Zapata 2014). This concern motivated another component of the ACA: the individual mandate. Beginning in 2014, individuals deemed to be able to afford coverage but electing to remain uncovered were penalized. The largest penalty that could be imposed was the maximum of either the total annual premium for the national average price of a bronze exchange plan, or \$285 (\$975) in 2014 (2015).[1] In addition, an employer mandate, which required employers with 100 or more full-time equivalent employees to offer "affordable" coverage to at least 95% of their full-time employees and their dependents (children up to age 26) or face a penalty, took effect in 2015 (Tolbert 2015).

The remaining challenge associated with promoting universal coverage—affordability—was addressed by the ACA in 2014 in two ways. First, sliding scale subsidies in the form of premium tax credits (PTCs) became available to consumers in every state with incomes of 100 to 400% of the federal poverty level (FPL) who did not qualify for other affordable coverage. Second, in states that opted to expand Medicaid via the ACA, low-income adults (with incomes at or below 138% of the FPL) who were not elderly, disabled, or parents of a dependent child became eligible for Medicaid coverage. Previously, Medicaid eligibility was typically restricted to those with low incomes among specific groups (categories of eligibility), such as children, single parents, pregnant women, the disabled, and the elderly.[2] According to the Kaiser Family Foundation, 27 states participated in the Medicaid expansion in 2014, with three more implementing it in 2015 and another two in 2016.[3]

Theoretically, the expansion of insurance coverage brought about by the ACA should increase access to care because of the reduction in out-of-pocket costs, but this is not automatically the case. On the demand side, newly insured individuals may not have sufficient knowledge of the health care system to easily secure a regular primary care doctor. Somers and Mahadevan (2010) report that only 12% of adults have proficient health literacy. On the supply side, concerns have been raised about whether there are sufficient numbers of primary care physicians to treat all of these newly insured patients (Schwartz 2012; Glied and Ma 2015). While the federal government

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[1] The maximum increased to \$2085 in 2016. For more information, see https://www.healthcare.gov/fees/fee-for-not-being-covered/.

[2] Prior to the ACA, Medicaid income limits varied by category of eligibility, with the federal government setting income limit floors and/or ceilings across categories (Buchmueller, Ham, and Shore-Sheppard 2016). In many categories of eligibility, many states opted for income limits above 138% of the FPL, such as the income limit associated with infants and pregnant women. For example, the Omnibus Budget Reconciliation Act of 1987 allowed states to cover pregnant women and children in families with incomes at or below 185% of the FPL. For those in expansion states that were not categorically eligible for Medicaid or were in categories of eligibility with income limits at or below 138% of the FPL prior to the ACA, the Medicaid expansion increased their eligibility. For those in categories of eligibility with income limits above 138% of the FPL, their income limits generally remained unchanged, other than adjustments associated with the ACA's uniform implementation of modified adjusted gross income.

[3] The 2012 Supreme Court ruling on the ACA upheld the individual mandate (the primary mechanism to address selection issues) but made the Medicaid expansion optional for states. For further information on state decisions with respect to the Medicaid expansion, see KFF, Status of State Action on the Medicaid Expansion Decision, http://kff.org/health-reform/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act/.

increased Medicaid primary care reimbursement rates to Medicare levels in 2013 and 2014, only a few states fully maintained this "fee bump" in 2015.[4]

Insurance coverage expansions could influence risky health behaviors—such as smoking, drinking, and overeating—in either direction (Cawley and Ruhm 2012). On the one hand, improved access to care among the affected population could translate to improvements in health behaviors via information, accountability, or treatments such as smoking cessation drugs or weight loss programs. Conversely, insurance expansions can theoretically worsen health outcomes through ex ante moral hazard, as the reduction in financial risks associated with unhealthy behaviors incentivizes such behaviors. Moreover, income effects from gaining free or subsidized coverage could influence behaviors by enabling consumers to spend money they had budgeted for the direct purchase of health care on alcohol, cigarettes, and junk food or, conversely, on healthy food and gym memberships (Simon, Soni, and Cawley 2017).

The net effect of insurance expansions on population health depends on the changes in both access to care and health behaviors and, therefore, is also theoretically ambiguous. The extent to which increased health care utilization translates to better population health depends on the distribution of affected individuals' initial locations along the health production function. Evidence suggests that "flat of the curve" care—perhaps due to uncertainty over treatment effectiveness, the principal-agent nature of the patient-doctor relationship, fee-for-service reimbursement, lack of coordination across health care providers, or malpractice liability—is common in the United States (Garber and Skinner 2008). Moreover, the same issues with health literacy that could hamper efforts by the newly insured to find a primary care doctor could also limit their ability to understand and comply with treatment recommendations.[5]

The purpose of this article is to estimate the impact of the ACA's 2014 provisions on a variety of outcomes related to health care access, risky health behaviors, and self-assessed health. We separately identify the effects of the private and Medicaid expansion portions of the ACA by using an identification strategy developed in Courtemanche et al. (2017) to estimate the impact of the ACA on insurance coverage by exploiting differences across local areas in pretreatment uninsured rates. To be more specific, we estimate a difference-in-difference-in-differences (DDD) model with the differences coming from time, state Medicaid expansion status, and local area pretreatment uninsured rate. If our objective was merely to isolate the effect of the Medicaid expansion, this could potentially be achieved with a simpler difference-in-differences model comparing changes in states that expanded Medicaid to changes in nonexpansion states. However, identifying the impact of the other components of the ACA (e.g., mandates, subsidies, marketplaces) is more difficult due to their national nature. We therefore exploit an additional layer of plausibly exogenous variation arising from the fact that universal coverage initiatives provide the most intense treatments in areas with high uninsured rates.[6]

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[4] Medicaid reimbursement rates are typically lower that those associated with private insurance plans or Medicare (Buchmueller, Ham, and Shore-Sheppard 2016). The purpose of the fee bump was to encourage more primary care providers to start seeing Medicaid patients, but the temporary nature of the fee bump may have reduced its effectiveness relative to a permanent fee increase. For more on state plans with respect to Medicaid primary care reimbursement, see Snyder, Paradise, and Rudowitz (2014) and Advisory Board (2015).

[5] Previous literature has shown a relationship between health literacy and health outcomes including health status, chronic illness, and hospitalization (Cho et al. 2008; Berkman et al. 2011).

[6] Finkelstein (2007) uses a similar strategy to identify the impacts of another national program, Medicare, on health care spending. Miller (2012a) also uses this approach to estimate the impact of the Massachusetts reform on emergency room utilization without control states.

Our data come from the 2011 to 2015 waves of the Behavioral Risk Factor Surveillance System (BRFSS), with the sample restricted to nonelderly adults. The BRFSS is well suited for our study for three reasons. First, it includes a wide range of questions on health care access and self-assessed health. Second, with over 300,000 observations per year, it is large enough to precisely estimate the effects of state-level interventions. Third, it was among the first large-scale health data sets to release data from 2015, allowing us to examine two calendar years of data after the full implementation of the ACA.

Our results suggest that the ACA substantially improved access to health care among nonelderly adults. Gains in insurance coverage were 8.3 percentage points in Medicaid expansion states compared to 5.3 percentage points in nonexpansion states, while reductions in cost being a barrier to care were 5.1 percentage points in expansion states and 2.6 percentage points in nonexpansion states. The ACA also increased the probabilities of having a primary care doctor and a checkup by 3.0 and 2.4 percentage points, respectively, in non-Medicaid-expansion states, with the effects not being statistically different in expansion states. Gains in access were generally largest among individuals with lower incomes.

However, the effects of the ACA on risky health behaviors and self-assessed health were less pronounced—at least after two years. For the full sample, we find no statistically significant impacts on any of the risky behavior or health outcomes in either Medicaid expansion or nonexpansion states. This general pattern of null results persists even among the lower-income subsample, though we do observe a marginally significant improvement in mental health in Medicaid expansion states for that group.

2. Literature Review

In this section, we review the literature on the impacts of expansions of insurance coverage. We divide the literature into studies focusing on coverage expansions prior to 2014 and those that examine the components of the ACA implemented in 2014.

Effects of Pre-2014 Insurance Interventions

There is an extensive literature spanning several decades examining the impacts of the receipt of both public and private health insurance on a variety of outcomes related to access to care, utilization, spending, risky health behaviors, and health. Additional outcomes considered in this literature include labor market participation, job lock, and other public program participation. Cutler and Zeckhauser (2000) provide a thorough review of the health insurance literature, while Buchmueller, Ham, and Shore-Sheppard (2016) review the literature on Medicaid and Gruber (2000) reviews the literature on health insurance and the labor market. Here, we provide a brief summary of the evidence on the effects of insurance-related interventions on outcomes related to access, risky behaviors, and health.

Causally interpretable evidence on the impacts of health insurance coverage dates back to the RAND Health Insurance Experiment of the 1970s–1980s, which randomly assigned individuals to insurance plans with different coinsurance rates and deductibles. Those assigned to a plan with no cost sharing incurred about 20% higher medical expenses than others (Manning et al. 1987). However, on average, this additional utilization did not translate to statistically significant effects on self-assessed health, smoking, or weight (Brook et al. 1983).

A substantial portion of the literature focuses on expansions of the Medicaid program. Evidence suggests that expansions for children and pregnant women in the 1980s and 1990s reduced low birthweight (Currie and Gruber 1996a), infant mortality (Currie and Gruber 1996b), and avoidable hospitalizations among children (Dafny and Gruber 2005). However, other studies suggest that these expansions increased smoking among pregnant women (Dave, Kaestner, and Wehby 2015) and had inconsistent effects on their health care utilization (Epstein and Newhouse 1998). Research has also found that Medicaid expansions for childless adults in the early 2000s increased self-reported access to care and health while reducing mortality, particularly related to HIV (Sommers, Baicker, and Epstein 2012; Sommers 2017). Studies of the randomized 2008 Oregon Medicaid lottery found that Medicaid increased health care access and utilization along a broad range of dimensions and led to large, immediate gains in self-assessed health (Finkelstein et al. 2012; Taubman et al. 2014). However, no evidence was found of changes in smoking, obesity, or clinical indicators of physical health (Finkelstein et al. 2012; Baicker et al. 2013). Tello-Trillo (2016) shows that a large Medicaid disenrollment in Tennessee reduced access to care and self-assessed health.

Another branch of the literature studies the impacts of Medicare, the universal coverage program for U.S. seniors. Evidence shows that health care utilization increases sharply at the age of eligibility (Lichtenberg 2002; Card, Dobkin, and Maestas 2008), while mortality among patients admitted to the ER falls sharply (Card, Dobkin, and Maestas 2009). However, other studies suggest that Medicare does not impact mortality more generally (Finkelstein and McKnight 2008) and slightly worsens smoking and drinking habits (Dave and Kaestner 2009).

Several studies have focused on the 2006 Massachusetts health care reform, a universal coverage initiative that featured a combination of insurance market reforms, mandates, and subsidies similar to the ACA. Kolstad and Kowalski (2012), Miller (2012a,b), and Van der Wees, Zaslavsky, and Ayanian (2013) all present evidence consistent with the reform improving access to primary care. Van der Wees, Zaslavsky, and Ayanian (2013) and Courtemanche and Zapata (2014) find that the reform also improved adults' self-assessed health, though an earlier study by Yelowitz and Cannon (2010) did not observe a statistically significant result. Courtemanche and Zapata (2014) also estimate that the reform reduced body mass index (BMI). Sommers, Kenney, and Epstein (2014) present evidence that the reform reduced mortality rates, though Kaestner (2015) disputes this finding.

Another series of articles investigates the effects of the first major insurance expansion to occur under the ACA: A mandate for insurers to cover dependents up to 26 years old that took effect in 2010. Evidence suggests that this dependent coverage expansion increased access to care (Sommers et al. 2013; Barbaresco, Courtemanche, and Qi 2015) and general health care utilization (Chua and Sommers 2014; Akosa Antwi, Moriya, and Simon 2015) but not utilization of preventive services (Barbaresco, Courtemanche, and Qi 2015). Chua and Sommers (2014), Barbaresco, Courtemanche, and Qi (2015), and Burns and Wolfe (2016) present evidence that the dependent coverage provision improved self-assessed health along some dimensions. Finally, Barbaresco, Courtemanche, and Qi (2015) document a reduction in BMI.

Kelly and Markowitz (2009) take a different approach to examining the causal effect of health insurance on BMI. Rather than investigating a particular policy change, they use Lewbel's estimator for instrumental variables without exclusion restrictions. They find that insurance increases BMI but not the probability of being obese.

To summarize, the available causally interpretable evidence suggests that health insurance can impact access to care, risky behaviors, and health outcomes but that the effects often vary substantially across contexts. For instance, the effects of insurance on self-assessed health appear to have been large and immediate in the cases of the Oregon Medicaid expansion and Massachusetts reform but more modest after the ACA dependent coverage expansion and virtually nonexistent in the RAND experiment. As another example, only the Massachusetts reform and the dependent coverage provision appear to have led to weight loss. These examples underscore the necessity of obtaining credible evidence on the effects of the 2014 components of the ACA rather than simply relying on results from other settings.

In particular, even evidence from the prior interventions that have the most in common with the ACA—Medicaid and the Massachusetts reform—may not be reliable indicators. In contrast to the narrower population targeted by Medicaid expansions, the ACA expanded coverage to a much broader range of low- and middle-income families and childless adults, with only part of the expansion occurring via Medicaid. Marketplace plans differ from traditional Medicaid in terms of cost sharing and provider networks. The effects of the Massachusetts reform and ACA could differ because of the relatively low prereform uninsured rate in Massachusetts, differences in the sociodemographic characteristics of those gaining coverage, the relative public enthusiasm surrounding the Massachusetts law compared to the ACA, and the fact that the entire expansion among adults was done though subsidized private coverage in Massachusetts as opposed to the mix of public and private used by the ACA (Gruber 2008).

Effects of the 2014 Components of the ACA

Much of the early evidence on the effects of the 2014 components of the ACA focuses on changes in coverage. At the national level, simple pre-post comparisons find increases in coverage of 2.8–6.9 percentage points, depending on the time frame, data set and population group (Long et al. 2014; Smith and Medalia 2015; Courtemanche, Marton, and Yelowitz 2016; Obama 2016; Barnett and Vornovitsky 2016; McMorrow et al. 2016).[7] Other recent work uses more sophisticated econometric techniques to isolate the impacts of different components of the ACA on coverage. Kaestner et al. (2017) and Wherry and Miller (2016) focus on the Medicaid expansions, while Frean, Gruber, and Sommers (2017) study the Medicaid expansions, subsidized premiums for marketplace coverage, and individual mandate. Using the identification strategy that we employ in this article, Courtemanche et al. (2017) aim to estimate the impact of the ACA more generally, finding that it increased coverage by an average of 5.9 percentage points in Medicaid expansion states compared to 2.8 percentage points in nonexpansion states in 2014.

A growing number of studies examine health-related outcomes besides insurance. Polsky et al. (2015), Shartzer, Long, and Anderson (2016), Sommers et al. (2015), Kirby and Vistnes (2016), and Sommers, Blendon, and Orav (2016) show that the timing of the ACA coincided with increased access to care, while Sommers et al. (2015) also document an improvement in self-assessed health. However, it is unclear whether estimates based only on time-series variation are able to disentangle causal effects of the ACA from other national shocks. Three articles use difference-in-differences (DD) approaches to examine the impacts of the 2014 ACA Medicaid

[7] Although, we focus our discussion on national studies, single-state investigations generally reach similar conclusions (Sommers, Kenney, and Epstein 2014; Golberstein, Gonzales, and Sommers 2015; Benitez, Creel, and Jennings 2016; Sommers et al. 2016).

expansion on access, health behaviors, or self-assessed health after two years.[8] Using data from the Gallup-Healthways Well-Being Index, Sommers et al. (2015) find evidence that the Medicaid expansion improved access along some dimensions but did not significantly affect self-assessed health. Abramowitz (2016) finds that the Medicaid expansion was associated with a *reduction* in self-reported overall health using data from the Current Population Survey Annual Social and Economic Supplement. Simon, Soni, and Cawley (2017) use data from the BRFSS and find that the Medicaid expansion increased some aspects of access and preventive care use among low-income childless adults. However, they find no evidence of effects on risky health behaviors or most of their self-assessed health measures.

Relative to these previous studies, our main contribution is to present causally interpretable evidence on the effects of the full ACA—as opposed to just its Medicaid portion—on access to health care, risky health behaviors, and self-assessed health. This is critical information in light of ongoing policy debates about the future of the ACA. While we adopt the DDD strategy of Courtemanche el al. (2017), our work is distinct because we examine outcomes beyond just insurance coverage, use a second year of posttreatment data, and use a different dataset (BRFSS instead of the American Community Survey [ACS]).

A secondary contribution of our work is to offer an alternative identification strategy for the impact of the Medicaid expansion that relies on weaker assumptions than the DD approach used previously. Specifically, we do not need to assume that any differential changes in the outcomes between the expansion and nonexpansion states in 2014 are attributable to Medicaid. Instead, our approach allows for other factors (e.g., underlying trends or enthusiasm for the other parts of the ACA) to contribute to this differential as long as they are not correlated with pretreatment uninsured rates.

3. Data

Our primary data source is the BRFSS, an annual telephone survey conducted by state health departments and the US Centers for Disease Control and Prevention that collects data on preventive services, risky behaviors, and self-assessed health for all 50 states and the District of Columbia. A random digit dialing method is used to select a representative sample of respondents from the noninstitutionalized adult population. The BRFSS is appealing for our study because its large number of observations, more than 300,000 per year, allows us to precisely estimate the effects of the ACA. This is important since only a fraction of the population is affected by the change in legislation, limiting plausible effect sizes.

Our main sample consists of 19- to 64-year-olds from the 2011–2015 waves. We exclude individuals older than 64 since the ACA was not intended to affect the health care coverage of seniors. We begin the sample in 2011 because that was the first year in which the BRFSS included cell phones in its sampling. As individuals who exclusively use cell phones are disproportionately young, this inclusion results in a discrete change in the sample means of many of our key variables (including insurance coverage) between 2010 and 2011. An additional benefit of excluding years prior to 2011 is that this limits the sample to years after the implementation of the ACA's

[8] Additionally, Sommers, Baicker, and Epstein (2012) find that early Medicaid expansions under the ACA in New York, Maine, and Arizona were associated with increases in access to care and self-assessed health.

dependent coverage expansion, preventing confounding from differences in state dependent coverage mandates prior to the ACA.

We utilize 11 different health-related dependent variables.[9] The first set relates to health care access since such access, specifically to primary care, has repeatedly been shown to be an important predictor of health outcomes (Starfield, Shi, and Macinko 2005). Our four access measures consist of dummy variables reflecting whether the respondent has any health insurance, had any medical care needed but not obtained because of cost in the previous year, has a primary care physician, and had a well-patient checkup (e.g., a physical) in the previous year. The next three outcomes relate to risky health behaviors: a binary indicator for whether one smokes, a count of alcoholic drinks consumed per month, and a continuous variable measuring the respondents' body weight in the form of BMI.[10] These are three of the leading causes of preventable death in the United States, costing 467,000, 64,000, and 216,000 lives, respectively, per year as of 2005 (Danaei et al. 2009; Cawley and Ruhm 2012). Another set of outcomes relates to self-assessed health status: a dummy for whether overall health is very good or excellent, days of the last 30 not in good mental health, days of the last 30 not in good physical health, and days of the last 30 with health-related functional limitations.[11] Self-assessed health variables, although subjective, have been shown to be correlated with objective measures of health, such as mortality (e.g., Idler and Benyamini 1997; DeSalvo et al. 2006; Phillips, Der, and Carroll 2010). While one might initially be skeptical that insurance expansions could meaningfully affect health in their first two years, prior evidence from the randomized Oregon Medicaid experiment (Finkelstein et al. 2012) and the Massachusetts universal coverage initiative (Van der Wees, Zaslavsky, and Ayanian 2013; Courtemanche and Zapata 2014) has shown that immediate gains in self-assessed health can indeed occur.

We include a wide range of control variables. The controls from the BRFSS are dummy variables for age groups (five-year increments from 25–29 to 60–64, with 19–24 as the reference group), gender, race/ethnicity (non-Hispanic black, Hispanic, and non-Hispanic white, with "other" as the reference group), marital status, education (high school degree, some college, and college graduate, with less than a high school degree as the reference group), household income ($10,000–$15,000, $15,000–$20,000, $20,000–$25,000, $25,000–$35,000, $35,000–$50,000, $50,000–$75,000, and >$75,000, with <$10,000 as the reference group), number of children in the household (zero to four, with five or more as the reference group), whether the respondent reports a primary occupation of student, and whether the respondent is unemployed. We also control for the Bureau of Labor Statistics' seasonally adjusted monthly state unemployment rate as well as dummy variables

9 Note that we do not utilize the screening (e.g., colonoscopy, mammogram, pap test) variables available in the BRFSS because, in almost all states, they are only available in 2012 and 2014. This means that 2014 would be the only posttreatment year, which would be especially problematic since the questions use reflection periods of a year or greater (e.g., pap test in the past year). In other words, it is not clear that 2014 would be a true "posttreatment" year for these outcomes, since part of the reflection period for respondents surveyed in that year would occur prior to the ACA taking effect.

10 Results are robust to using an indicator for obesity ($BMI \geq 30$) rather than continuous BMI. Self-reports of weight and height are well-known to suffer from measurement error, but studies implementing a correction method involving validation data from the National Health and Nutrition Examination Survey have repeatedly shown that adjusting for this error does not affect the signs and significance of coefficient estimates (e.g., Cawley 2004; Courtemanche, Pinkston, and Stewart 2015).

11 We also run alternative specifications where the dependent variable is equal to a dummy for whether overall health is good or better, a dummy for whether overall health is excellent, and a summary index of health that incorporates the three health behaviors, overall self-assessed health, and the three self-assessments that pertain to physical/mental health. These additional specifications produce results that are consistent with those presented in Table 3.

for whether states set up their own insurance exchanges and whether these exchanges experienced glitches (KFF 2014; Kowalski 2014).

A critical variable for our identification strategy is the uninsured rate in the respondent's "local area" in the pretreatment year of 2013, which we compute within our BRFSS sample. The BRFSS does not contain county-level identifiers continuously throughout our period of analysis, making it impossible for us to compute county-level uninsured rates. Instead, we use information collected on type of location within a state. The BRFSS reports whether the respondents reside in the center city of an MSA, outside the center city of an MSA but inside the county containing the center city, inside a suburban county of the MSA, or not in an MSA. However, no location information was collected from cell-phone respondents. We use this location variable to construct four subgroups within each state: within a central city, suburbs, non-MSA, and location unavailable (i.e., cell phone sample). Based on these within-state classifications, we calculate the pretreatment average uninsured rates by location (considering "cell phone" to be a location for the sake of convenience) within a state. To ensure that each area contains a sufficient number of respondents to reliably compute pretreatment uninsured rates, we combine the seven areas with fewer than 200 respondents in 2013 with other areas.[12] After doing this, there are 194 areas with 2013 uninsured rates computed from 219 to 5804 respondents, with the average being 1475 and the median being 1205.

Our Medicaid expansion variable comes from the Kaiser Family Foundation, a nonprofit organization that collects a vast array of health policy information. This information includes whether a state implemented the Medicaid expansion as well as whether this expansion was done through private insurance via a Section 1115 waiver. Expanding under the Section 1115 waiver, as done by Arkansas, Iowa, and Michigan, introduced cost sharing and premiums for enrollees and could therefore have had different effects than expanding via traditional Medicaid. We attempted to test for such differences, but statistical power was insufficient to draw meaningful conclusions; we therefore simply classify the Section 1115 waiver states as being Medicaid expanders. Thus, a total of 27 states (including the District of Columbia) participated in the 2014 Medicaid expansion and 30 states (including the District of Columbia) expanded by the end of 2015.

In our main specifications, we simply classify the 30 states that expanded Medicaid by 2015 as the treatment group for the Medicaid expansion and the other 21 as the control group. The majority of the expansion states implemented their expansion in January 2014, with some exceptions. Michigan's expansion took effect in April 2014 and New Hampshire's in August 2014. In 2015, Indiana and Alaska expanded Medicaid in February and September, respectively. States are classified as part of the treatment group beginning the month of their expansion.

Table 1 provides pretreatment means and standard deviations of the dependent variables, while Supporting Information Appendix Table A1 does the same for the controls.[13] We also report the summary statistics stratified into four groups based on whether the respondent's state expanded Medicaid and whether her local area's pretreatment uninsured rate was above or below the median for individuals in the sample. According to Table 1, 79% of the sample had insurance at baseline. For both the high- and low-uninsured rate subgroups, individuals in Medicaid expansion states were slightly more likely to have insurance prior to 2014 than those in nonexpansion

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[12] Specifically, we combine the central city and suburban parts of Wyoming into one area, and we do the same for Vermont, South Dakota, and Montana. We also combine the suburban and rural parts of Massachusetts, Arizona, and California.

[13] The online appendix is available at http://www2.gsu.edu/~ecojhm/courtemanche_et_al_SEJ_online_appendix.pdf.

Table 1. Means and Standard Deviations of Dependent Variables by State Medicaid Expansion Status and Pretreatment Uninsured Rate

	Full Sample	Medicaid Expansion; ≥ Median Baseline Uninsured	Medicaid Expansion; < Median Baseline Uninsured	Nonexpansion; ≥ Median Baseline Uninsured	Nonexpansion; < Median Baseline Uninsured
Any insurance coverage	0.788	0.791	0.868	0.710	0.805
	(0.409)	(0.407)	(0.339)	(0.454)	(0.396)
Primary care doctor	0.741	0.745	0.826	0.682	0.754
	(0.439)	(0.436)	(0.378)	(0.465)	(0.431)
Cost barrier to care in past year	0.192	0.202	0.144	0.241	0.187
	(0.394)	(0.401)	(0.351)	(0.427)	(0.389)
Well-patient doctor visit in past year	0.627	0.586	0.673	0.632	0.629
	(0.484)	(0.492)	(0.469)	(0.482)	(0.483)
Overall health very good or excellent	0.535	0.511	0.565	0.505	0.544
	(0.499)	(0.499)	(0.496)	(0.499)	(0.498)
Days not in good physical health in past month	3.660	4.489	3.940	4.149	4.099
	(7.964)	(8.639)	(8.073)	(8.362)	(8.326)
Days not in good mental health in past month	4.118	4.486	3.758	3.755	3.678
	(8.210)	(8.960)	(8.127)	(8.154)	(8.095)
Days with health-Related limitations in Past month	2.518	3.066	2.553	2.572	2.570
	(6.797)	(7.505)	(6.877)	(6.463)	(6.975)
BMI	27.875	28.002	27.848	28.202	28.187
	(6.282)	(6.331)	(6.208)	(6.462)	(6.435)
Smoking status	0.216	0.212	0.195	0.218	0.244
	(0.412)	(0.408)	(0.396)	(0.420)	(0.429)
Drinks per month	14.285	13.080	13.782	14.103	13.740
	(35.824)	(32.600)	(32.187)	(37.640)	(35.173)

Note: Standard deviations in parentheses.

states. Residents of Medicaid expansion states and states with pre-ACA uninsured rates below the median (column 3) had, on average, better health care access and self-assessed health than their counterparts even before the ACA was implemented. They were also more educated, more likely to be used, and had higher incomes according to Supporting Information Appendix Table A1. Our econometric design will account for these baseline differences.

Figures 1–3 show how the average values of the outcome variables change across the sample period for four groups stratified by state Medicaid expansion status and local area pretreatment uninsured rate (above or below the median). The graphs show that the pretreatment trends were generally similar along these dimensions for most outcomes. Later, we will test our DDD model's identifying assumptions more formally through an event study analysis.[14] In the posttreatment period (2014 and 2015), Figure 1 shows improvements in all access measures for all four groups, with the exception of the well-patient checkup variable for the least heavily treated group (low baseline uninsured rate in non-Medicaid-expansion state). For all access outcomes, the gains are largest among the most heavily treated group (high uninsured rate, Medicaid expansion state).

[14] Some divergence in pretreatment trends is evident for self-reported overall and physical health as well as functional limitations. However, the event-study regressions will generally indicate that our econometric model performs well for these outcomes, suggesting that our DDD strategy—which will account for differential trends between Medicaid expansion and nonexpansion states as well as the control variables discussed above—adequately accounts for the sources of these divergent trends.

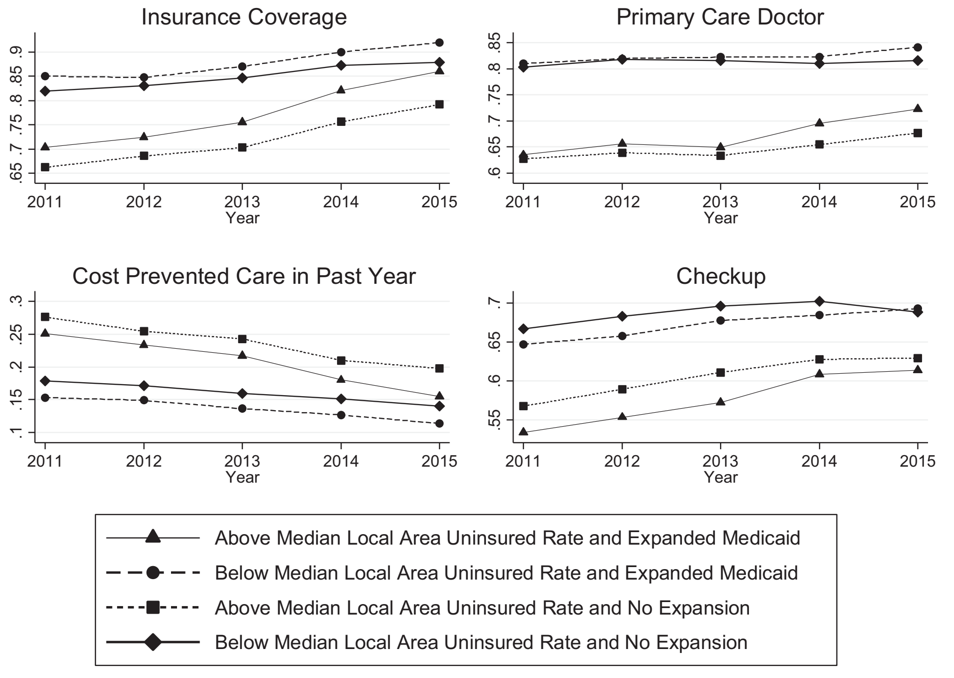

Figure 1. Changes in Health Care Access Variables over Time by State Medicaid Expansion Status and Local Area Pretreatment Uninsured Rate.

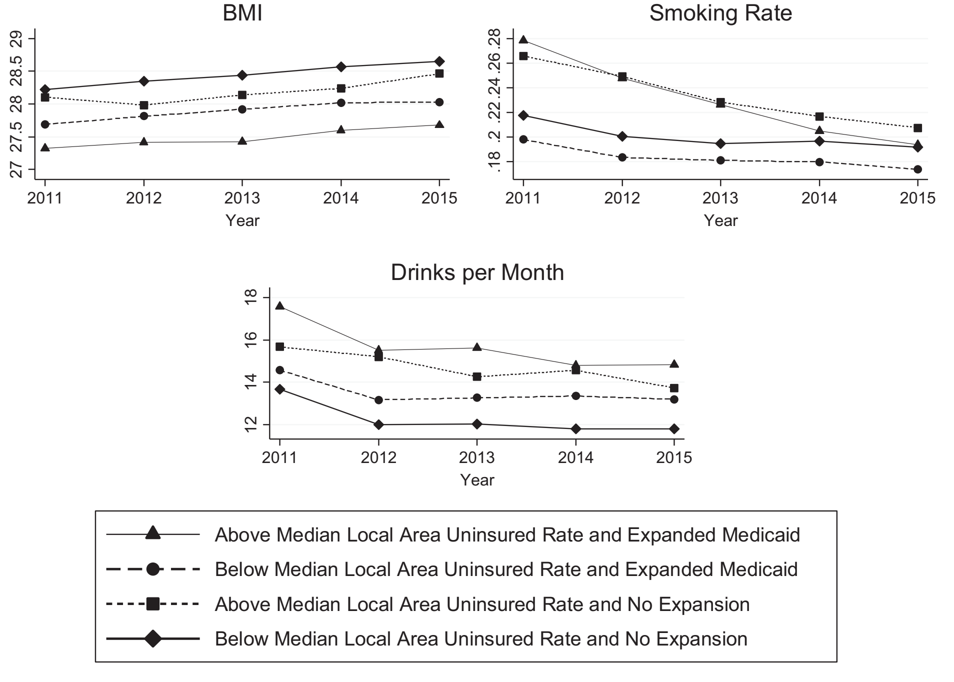

Figure 2. Changes in Health Behavior Variables over Time by State Medicaid Expansion Status and Local Area Pretreatment Uninsured Rate.

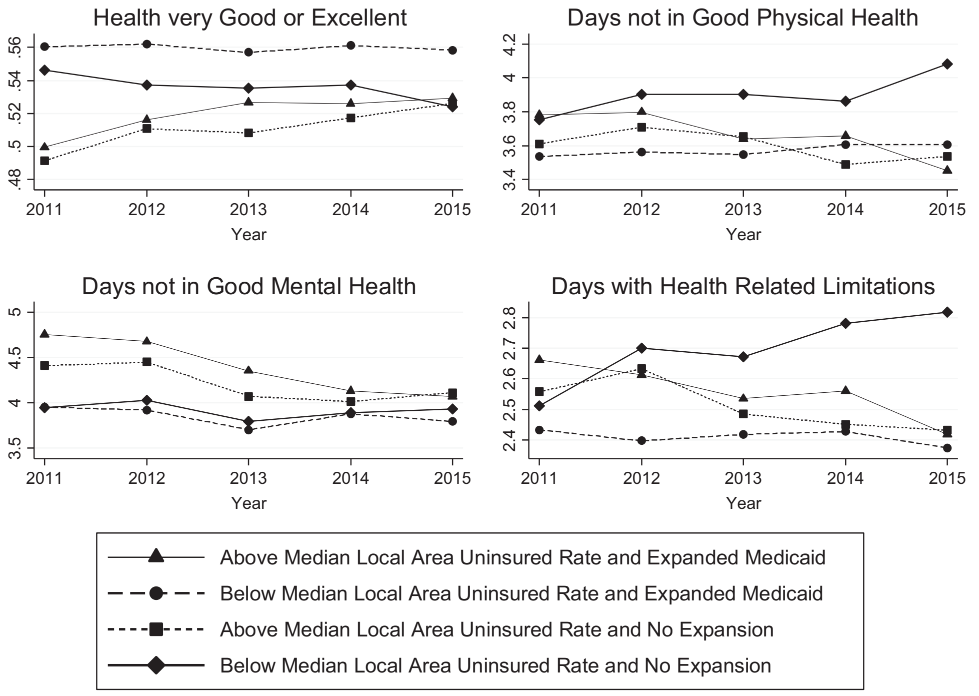

Figure 3. Changes in Self-Assessed Health Variables over Time by State Medicaid Expansion Status and Local Area Pretreatment Uninsured Rate.

In Figures 2 and 3, we see that changes in the health behavior and self-assessed health outcomes after the ACA do not appear to exhibit as clear a pattern. In most cases, the changes appear to simply reflect the continuation of pretreatment trends, while in other cases (e.g., days not in good physical health, days with functional limitations), some groups experience gains and others losses. In sum, the graphs provide preliminary evidence that the ACA's effects on health care access were more pronounced than those on behaviors and health. We next turn to econometric analysis to investigate these effects more rigorously.

4. Econometric Models

For each outcome, our econometric objectives are to estimate the effects of both the fully implemented ACA (including the Medicaid expansion) and the ACA without the Medicaid expansion. A major challenge in doing so is to disentangle the impacts of the nationwide components of the ACA (e.g., exchanges, mandates, subsidies) from underlying year-to-year fluctuations that would have occurred even in the law's absence. We adopt the strategy Courtemanche et al. (2017) use to identify the impact of the ACA on health insurance coverage, which exploits variation across space in the intensity of treatment arising from differential pretreatment uninsured rates. Adding this layer of geographic variation allows us to include time period fixed effects while still identifying the effects of the national (private) portion of the law. For simplicity, we refer to this method as a DDD, although we acknowledge that it is slightly different from a conventional DDD as pretreatment uninsured rate is a continuous variable.

Assuming that the extent of a geographic area's treatment is proportional to its baseline uninsured rate, the DDD model is

$$
\begin{aligned}
y_{\text{iast}} = & \gamma_0 + \gamma_1(\text{UNINSURED}_{as} * \text{POST}_t) + \gamma_2(\text{MEDICAID}_s * \text{POST}_t) \\
& + \gamma_3(\text{UNINSURED}_{as} * \text{MEDICAID}_s * \text{POST}_t) + \gamma_4 \boldsymbol{X}_{\textbf{iast}} + \theta_{at} + \alpha_{as} + \varepsilon_{\text{iast}}
\end{aligned} \tag{1}
$$

where

- y_{iast} is the outcome for individual *i* in area type (central city, rest of MSA, non-MSA, cell phone) *a* in state *s* in time period (month/year) *t*,
- POST_t is an indicator for whether period *t* is in the posttreatment period of January 2014 or later,
- X_{iast} is a vector of control variables,
- MEDICAID_s is an indicator for whether state *s* participated in the ACA's Medicaid expansion,
- UNINSURED_{as} is the pretreatment (2013) uninsured rate in area type *a* within state *s*,
- θ_{at} represents time fixed effects for each month or year * area type combination (e.g., central city in January 2011); these not only control for time as flexibly as possible but also allow time trends to evolve differentially across individuals living in central city, suburban, and rural areas as well as those with only cell phones,
- α_{as} represents fixed effects for each geographic area (e.g., central city in Alabama),
- and $\varepsilon_{\text{iast}}$ is the error term.

Note that POST_t is not included in the model since it is captured by the time fixed effects, while the terms $\text{UNINSURED}_{as} * \text{MEDICAID}_s$ are not separately included since they are captured by the area fixed effects.

In Equation 1, the effect of the ACA without the Medicaid expansion is given by γ_1 * UNINSURED_{as}, which means it is assumed to be zero in a (hypothetical) area with a 0% uninsured rate at baseline and to increase linearly as the pre-ACA uninsured rate rises. (We have also experimented with nonlinear functional forms for the uninsured rate and found that they do not reveal any meaningful new information.) The identifying assumption is that, in the absence of the treatment, any changes in the outcomes that would have occurred in 2014–2015 would not have varied differentially by area uninsured rates, conditional on the controls. We do *not* need to assume that there would have been no changes at all in the outcomes without the ACA (conditional on the controls), as would be the case in a pre–post comparison that did not utilize the variation in pretreatment uninsured rates.

The effect of the Medicaid expansion is given by $\gamma_3 * \text{UNINSURED}_{as}$. As with the other components of the ACA, the impact of the Medicaid expansion is now assumed to vary linearly with the state's baseline uninsured rate. (Again, we found that considering nonlinear functional forms did not reveal new information). As the Medicaid expansion should not causally affect insurance coverage in an area with a 0% baseline uninsured rate, we consider γ_2 to reflect unobserved confounders rather than capturing part of the expansion's causal effect. This interpretation follows Miller (2012a) and Courtemanche et al. (2017). The identifying assumption for the impact of the Medicaid expansion is therefore that, without the ACA, differential changes in the outcomes in 2014–2015 between Medicaid expansion and nonexpansion states would not have been correlated with 2013 uninsured rates. This is a weaker assumption than would be required by a DD model, in which case one would have to assume that, conditional on the controls, there would have been *no* differential changes across expansion and nonexpansion states.

Robustness Checks

We also conduct a number of robustness checks. The first several vary the set of control variables to address the possible concern that some of them could be endogenous to the ACA. Recall that the baseline model includes the following controls: demographic (age, gender, and race/ethnicity), family (education, marital status, and number of children), economic (income, employment and student status, and unemployment rate), and health insurance exchange (interactions of year = 2014 with whether the state set up its own exchange and whether the exchange had glitches). The first four robustness checks include only subsets of these variables: demographic controls only, demographic and family controls, demographic and economic controls, and demographic and exchange controls.

Next, recall that we do not know geographic area type (central city, suburbs, or rural) for individuals interviewed on a cell phone, necessitating our combining of all such individuals into a separate group within each state. The next robustness check aims to ensure that this decision does not meaningfully influence the results by dropping those interviewed on cell phones, ensuring the availability of the area type variable for everyone in the sample.

The following set of robustness checks addresses the potential concern that interacting POST_t and $\text{MEDICAID}_s{*}\text{POST}_t$ with the same uninsured rate variable may be problematic since the Medicaid and private portions of the ACA applied to different income ranges (at or below 138% of the FPL for Medicaid, above 138% in Medicaid expansion states, and above 100% in nonexpansion states for the exchanges/subsidies). The first such check interacts POST_t with the pre-ACA uninsured rate for respondents above 100% of the FPL and $\text{MEDICAID}_s{*}\text{POST}_t$ with the rate for those at or below 138%. Additional specifications use a 100% cutoff for both groups and a 138% cutoff for both groups.

Next, we consider alternative approaches to computing pretreatment uninsured rates that utilize a larger number of individuals per area than our baseline strategy. This addresses possible concerns about using groups narrower than state to construct this key variable. First, we pool all three pretreatment years when computing baseline uninsured rates rather than just using 2013 to increase the number of individuals in each area. Second, we drop the substate classifications and compute pretreatment uninsured rates at the state level (using just 2013).

In another robustness check, we drop 19–25-year olds. Since this age group was treated by the 2010 ACA dependent coverage provision, their treatment status is somewhat ambiguous. With that said, Courtemanche et al. (2017) find that this age group still experienced large coverage gains in response to the 2014 ACA provisions, so we do not expect dropping 19- to 25-year-olds to meaningfully impact our results.

The remaining robustness checks deal with the potentially ambiguous Medicaid expansion treatment status of some states, as many states partially expanded Medicaid under the ACA prior to 2014. One approach restricts the sample to only the nine treatment states and 20 control states that did not have some form of Medicaid expansion prior to January 2014, as classified by Kaestner et al. (2017). Another uses the same nine treatment states but the full control group. Next, we only exclude the five states that Kaestner et al. (2017) describe as having comprehensive early Medicaid expansions prior to 2014. Our final robustness check drops the states that expanded Medicaid in 2014 or 2015 but whose expansion was not effective as of January 1, 2014.

5. Results

Tables 2 and 3 report the results from the baseline DDD regression for each outcome. The top panel presents the coefficient estimates and standard errors for the variables of

Table 2. Effects of ACA on Health Care Access and Health Behaviors

	Insurance Coverage	Primary Care Doctor	Cost Barrier	Checkup	BMI	Smoker	Alcoholic Drinks per Month
Coefficient Estimates of Interest							
Medicaid expansion* post	−0.013	0.005	0.019	0.005	0.003	−0.022[a]	0.087
	(0.008)	(0.011)	(0.010)	(0.016)	(0.113)	(0.009)	(0.538)
Post* pretreatment uninsured	0.259[c]	0.148[b]	−0.127[c]	0.119[a]	−0.087	−0.0006	3.290
	(0.030)	(0.049)	(0.031)	(0.051)	(0.405)	(0.046)	(2.119)
Medicaid expansion* post* pretreatment uninsured	0.152[b]	0.007	−0.123[b]	0.060	−0.040	0.054	−0.607
	(0.045)	(0.065)	(0.042)	(0.067)	(0.528)	(0.045)	(2.397)
Implied Effects of ACA at Mean Pretreatment Uninsured Rate							
ACA without Medicaid expansion	0.053[c]	0.030[b]	−0.026[c]	0.024[a]	−0.018	−0.0001	0.667
	(0.006)	(0.010)	(0.006)	(0.010)	(0.082)	(0.009)	(0.429)
Medicaid expansion	0.031[c]	0.001	−0.025[b]	0.012	−0.008	0.010	−0.123
	(0.009)	(0.013)	(0.009)	(0.014)	(0.107)	(0.009)	(0.486)
Full ACA (with Medicaid expansion)	0.083[c]	0.031[b]	−0.051[c]	0.036[c]	−0.026	0.011	0.544
	(0.010)	(0.011)	(0.010)	(0.009)	(0.094)	(0.007)	(0.571)
Pretreatment mean and standard deviation of outcome	0.811	0.742	0.183	0.635	27.951	0.208	14.285
	(0.391)	(0.437)	(0.386)	(0.481)	(6.375)	(0.406)	(35.824)
Sample size	1,322,370	1,321,567	1,071,238	1,072,537	1,264,243	1,300,819	1,225,053

Notes: Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. BRFSS sampling weights are used. All regressions include state * location type and year * location type fixed effects as well as the controls.
[a]Indicates statistical significance at 5% level.
[b]Indicates statistical significance at 1% level.
[c]Indicates statistical significance at 0.1% level.

Table 3. Effects of ACA on Self-Assessed Health

	Very Good or Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Coefficient estimates of interest				
Medicaid expansion* post	0.009	−0.171	0.006	−0.334[a]
	(0.008)	(0.111)	(0.210)	(0.165)
Post* pretreatment uninsured	0.028	−0.584	−0.396	−0.595
	(0.0327)	(0.555)	(0.763)	(0.695)
Medicaid expansion* post* pretreatment uninsured	−0.036	0.336	−0.337	1.114
	(0.041)	(0.537)	(0.878)	(0.811)
Implied Effects of ACA at Mean Pretreatment Uninsured Rate				
ACA without Medicaid expansion	0.006	−0.118	−0.080	−0.121
	(0.006)	(0.112)	(0.155)	(0.141)
Medicaid expansion	−0.007	0.068	−0.068	0.226
	(0.008)	(0.108)	(0.178)	(0.164)
Full ACA (with Medicaid expansion)	−0.002	−0.050	−0.149	0.105
	(0.010)	(0.126)	(0.126)	(0.154)
Pretreatment mean and standard deviation	0.537	3.634	4.071	2.500
	(0.499)	(7.948)	(8.169)	(6.777)
Sample size	1,321,799	1,309,624	1,310,641	1,316,271

Notes: Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. BRFSS sampling weights are used. All regressions include state * location type and year * location type fixed effects as well as the controls.
[a]Indicates statistical significance at 5% level.
[b]Indicates statistical significance at 1% level.
[c]Indicates statistical significance at 0.1% level.

interest, while the bottom panel gives the implied effects of the ACA at the average pretreatment uninsured rate. Indicators of statistical significance at the 0.1%, 1%, and 5% level are also shown.

We begin our discussion with the outcomes related to access: insurance coverage, having a primary care doctor, cost being a barrier to care in the past 12 months, and checkup in the past 12 months, which are in the first four columns of Table 2. Because the cost barrier and checkup variables reflect information from the past 12 months, treatment status in 2014 is ambiguous for these outcomes. For instance, for someone interviewed in March 2014, only three of the 12 months that determine one's answer to these questions are actually in the post-treatment period. We therefore drop 2014 in the regressions for these outcomes, explaining their smaller sample size.

The results suggest that the private portion of the ACA increased access to care along all observable dimensions. The top panel of the table shows that each percentage point increase in exposure to the ACA (measured by the baseline uninsured rate) increased the probability of having health insurance coverage by 0.259 percentage points, the probability of having a primary care doctor by 0.148 percentage points, and the probability of having a regular checkup by 0.119 percentage points, while reducing the probability of cost being a barrier to care by 0.127 percentage points. These impacts translate to increases in the probabilities of having insurance coverage, a primary care doctor, and a well-patient checkup of 5.3, 3.0, and 2.4 percentage points, respectively, and a reduction in the probability of cost being a barrier to care of 2.6 percentage points at the sample mean pretreatment uninsured rate. The ACA therefore led to sizeable improvements in access even in states that did not expand Medicaid.

The Medicaid expansion led to additional gains in access along some dimensions. At the average pretreatment uninsured rate, it increased insurance coverage by a statistically significant 3.1 percentage points and reduced the probability of reporting cost being a barrier to care by 2.5 percentage points. We do not find significant effects on having a primary care doctor or a well-patient checkup, though the magnitude for checkup is an economically meaningful 1.2 percentage points—around two-fifths as large as the effect on insurance.[15] Our finding that the Medicaid expansion increased access along some but not all dimensions is broadly consistent with the results from the DD studies by Sommers et al. (2015) and Simon, Soni, and Cawley (2017).[16]

Combining the effects of the private and Medicaid components shows that the fully implemented ACA led to large gains in all access measures. At the average pretreatment uninsured rate, insurance coverage increased by 8.3 percentage points, probability of having a primary care doctor rose by 3.1 percentage points, probability of cost being a barrier to care fell by 5.1 percentage points, and probability of having a checkup rose by 3.6 percentage points. Based on the sample means for the outcomes reported in Table 2, these results imply that the full ACA reduced the uninsured rate by 44% while also reducing the number of people without a primary care doctor by 12%, those with foregone care because of cost by 28%, and those not having an annual checkup by 10%. These magnitudes equate to 21%, 7.1%, 13.2%, and 7.5% of the respective outcomes' standard deviations.

The remaining three columns of Table 2 report the results for the three health behavior variables: BMI, probability of being a smoker, and drinks per month. We observe no statistically significant effects of the private portion, Medicaid expansion, or overall ACA on any of these outcomes. The signs are mixed, with the full ACA reducing BMI but increasing smoking and drinking. The magnitudes are relatively small compared to those for the access outcomes. The estimated effects of the full ACA at the mean pretreatment uninsured rate on BMI, smoking, and drinking are just −0.1%, 5.3%, and 3.8% of the corresponding sample means and −0.4%, 2.7%, and 1.5% of the corresponding standard deviations. Our findings for the Medicaid expansion are consistent with the null effects on risky behaviors found by Simon, Soni, and Cawley (2017) using DD methods and a sample of only low-income adults.

Table 3 displays the results for the self-assessed health outcomes. We find no statistically significant effects of either the private or Medicaid components of the ACA on any of the outcomes. Again, the signs are mixed, with the point estimates suggesting that the full ACA improved

[15] Both of the access variables for which we did not find statistically significant effects of the Medicaid expansion relate to primary care. One possible explanation is that newly enrolled Medicaid recipients may still have trouble accessing primary care, perhaps due to the temporary nature of the ACA Medicaid fee bump (MACPAC 2015) leading to a smaller than expected change in physician Medicaid participation and/or some degree of access crowd-out due to the concurrent expansion of private (i.e., marketplace) coverage. However, as the magnitude of the estimated effect on checkups is meaningfully large despite its statistical insignificance, we are reluctant to strongly push this explanation.

[16] The only noteworthy differences for specific access outcomes are that we find evidence of an effect on cost being a barrier to care but not having a primary care doctor, whereas the reverse is true for Sommers et al. (2015) and Simon, Soni, and Cawley (2017). In our view, the difference in results for cost being a barrier to care is not a major discrepancy, as Sommers et al. (2015) and Simon, Soni, and Cawley (2017) find the same signs and magnitudes that are only slightly smaller than ours—their estimates just do not quite reach statistical significance. The discrepancy in results for primary care doctor is more substantial, as our point estimate is essentially zero. In unreported regressions (available upon request), we replicated Simon, Soni, and Cawley's DD model and restriction of the sample to those with incomes below 100% of the FPL. We found that the estimated increase in the probability of having a primary care doctor shrinks roughly in half (from about 4 to 2 percentage points) and becomes slightly statistically insignificant if we add the control for the state setting up its own exchange. This suggests some upward bias in the DD estimate due to unobserved differences in state attitudes toward the ACA, which we control for with our DDD approach.

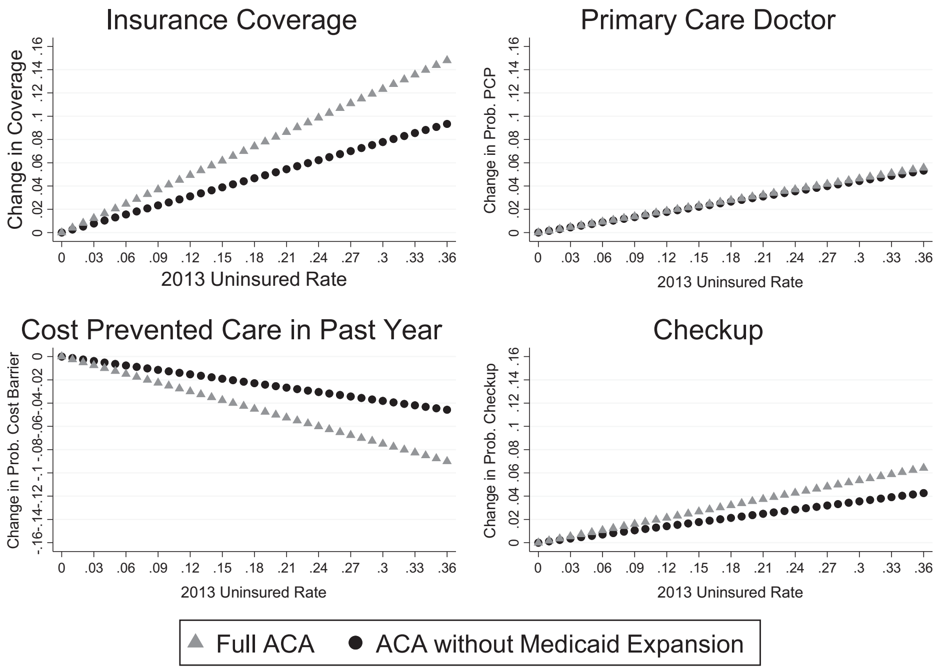

Figure 4. ACA's Effects on Access Outcomes at Pretreatment Uninsured Rates.

physical and mental health but worsened overall health and days with functional limitations. The magnitudes are relatively small, as implied effects of the full ACA represent just −0.37% (–0.4%), −1.4% (–0.6%), −3.7% (–1.8%), and 4.2% (1.5%) of the pretreatment means (standard deviations) of very good or excellent health, days not in good physical health, days not in good mental health, and days with health-related limitations, respectively. Our small and insignificant estimates contrast with the large, early improvements in these same self-assessed health outcomes seen after the Massachusetts health care reform (Van der Wees, Zaslavsky, and Ayanian 2013; Courtemanche and Zapata 2014) and randomized Oregon Medicaid experiment (Finkelstein et al. 2012). However, our null results for the Medicaid expansion are consistent with the lack of clear improvements in self-assessed health found by the DD studies in the ACA Medicaid expansion literature (Sommers et al. 2015; Abramowitz 2016; Simon, Soni, and Cawley 2017).

The reported results in Tables 2 and 3 only compute impacts of the ACA at the mean pretreatment uninsured rate of 20.2%. Because area pretreatment uninsured rates varied widely, ranging from 3 to 36% with a standard deviation of 8%, this approach disguises a great deal of heterogeneity. Figure 4, therefore, shows how the predicted changes in our access outcomes vary across this range of uninsured rates in both expansion and nonexpansion states. The effects on the health behavior and self-assessed health outcomes are never significant at any uninsured rate, so we do not present similar graphs for them.

The predicted effect of the full ACA on the probability of having insurance coverage reached as high as 14.7 percentage points in the area with the highest pretreatment uninsured rate. Without the Medicaid expansion, this impact only reached 9.3 percentage points. The predicted impact of the full ACA on the probability of having a primary care doctor extends to 5.6 percentage points at the highest uninsured rate, with essentially no difference between Medicaid expansion and nonexpansion states. For the cost barrier and well-patient checkup outcomes, the maximum predicted effects of the ACA are 9 percentage points and 6.4 percentage points, respectively, in Medicaid expansion states and 4.5 and 4.3 in nonexpansion states.

Lastly, the results for the robustness checks are available in Supporting Information Appendix Tables A2–A12 (one table for each outcome).[17] In almost all cases, the findings from the baseline regressions persist across the various robustness checks.

6. Instrumental Variables

A natural question with interpretation of the reduced-form results from the preceding section is whether we can assume that the extensive margin of insurance coverage is the only mechanism through which the ACA affected the other outcomes. If this is true, then it would be reasonable to estimate an instrumental variables (IV) specification in which $\mathrm{UNINSURED}_{as}*\mathrm{POST}_t$ and $\mathrm{UNINSURED}_{as}*\mathrm{MEDICAID}_s*\mathrm{POST}_t$ are instruments and insurance coverage is the endogenous variable.[18] This assumption is difficult to test and may not hold if, for instance, areas with higher baseline uninsured rates also had higher rates of underinsurance (e.g., bare-bones privately purchased policies), in which case the intensive margin of coverage quality is another mechanism through which our treatment variables could affect the other outcomes. Moreover, general equilibrium effects are possible; for instance, in areas with large numbers of newly insured residents, continuously covered individuals may face increased difficulty accessing providers, while those working in the health care industry may experience positive income shocks. For these reasons, we prefer to emphasize our reduced-form approach, as it allows for all of these mechanisms. Nonetheless, IV results can be informative about how large the effects of coverage on the other health care access outcomes would need to be for the extensive margin of coverage to be the only relevant mechanism. Moreover, for the risky behavior and self-assessed health outcomes, they provide further insight as to whether the null estimates are large or small in magnitude.

Results from the IV model—with the full set of controls and fixed effects included—are presented in Table 4. In each column, we present the second-stage coefficient estimate for the health insurance variable along with its standard error, the first stage F statistic from the test of joint significance of the two instruments, and the p value for the overidentification test. In this case, the overidentification test essentially tests the null hypothesis that the estimated local average treatment effects of insurance would be statistically indistinguishable if either $\mathrm{UNINSURED}_{as}*\mathrm{POST}_t$ or $\mathrm{UNINSURED}_{as}*\mathrm{MEDICAID}_s*\mathrm{POST}_t$ were used as the sole instrument. A rejection of the null hypothesis could therefore mean either that the effect of gaining coverage via the Medicaid expansion is different from the effect of gaining coverage through the private component of the ACA (in which case the IV specification captures a weighted average of these two effects), or that the Medicaid and private expansions activate other mechanisms besides simply the extensive margin of coverage (in which case the IV specification would be inappropriate).

The results show that the estimated effects of insurance on the other access outcomes are large and highly significant. Specifically, insurance coverage increases the probability of having a primary care doctor by 45 percentage points and the probability of having a well-patient doctor

[17] The Supporting Information Appendix is available at http://www2.gsu.edu/~ecojhm/courtemanche_et_al_SEJ_online_appendix.pdf.

[18] We are not able to estimate an IV model with both private and Medicaid coverage as endogenous variables because the BRFSS does not contain information on source of coverage.

Table 4. Instrumental Variables Estimates of the Effects of Health Insurance

	Primary Care Doctor	Cost Barrier	Checkup	BMI	Smoker
Any insurance	0.446[c]	−0.469[c]	0.357[c]	−0.198	0.085
	(0.080)	(0.075)	(0.092)	(1.250)	(0.076)
Pretreatment mean and standard deviation of outcome	0.742	0.183	0.635	27.951	0.208
	(0.437)	(0.386)	(0.481)	(6.375)	(0.406)
Sample size	1,319,215	1,069,336	1,070,619	1,261,976	1,298,452
First-stage F statistic	618.16	704.79	703.35	630.63	626.53
Overidentification test P-value	0.001	0.144	0.721	0.932	0.072
	Drinks per Month	**Very Good or Excellent Health**	**Days Not in Good Physical Health**	**Days Not in Good Mental Health**	**Days with Health-Related Limitations**
Any insurance	9.018	0.022	−0.947	−1.676	0.168
	(7.866)	(0.092)	(1.520)	(1.615)	(1.268)
Pretreatment mean and standard deviation of outcome	14.285	0.537	3.634	4.071	2.500
	(35.824)	(0.499)	(7.948)	(8.169)	(6.777)
Sample size	1,265,867	1,319,344	1,307,254	1,308,281	1,313,883
First-stage F statistic	616.48	615.03	600.74	609.30	614.19
Overidentification test P-value	0.420	0.078	0.189	0.908	0.001

Notes: Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. BRFSS sampling weights are used. All regressions include state * location type and year * location type fixed effects as well as the controls.
[a]Indicates statistical significance at 5% level.
[b]Indicates statistical significance at 1% level.
[c]Indicates statistical significance at 0.1% level.

visit by 36 percentage points, while decreasing the probability of having foregone care by 47 percentage points. These are large effects that may be overstated because of the presence of other possible mechanisms, as discussed above. Nonetheless, in our view, effects of this general size are plausible, as they are in the same vicinity as the IV estimates from the Oregon Medicaid experiment. Specifically, Finkelstein et al. (2012) found that Medicaid coverage increased similar access outcomes by 20–34 percentage points after just one year. Our coefficient estimates imply that having insurance increases the probabilities of having a primary care doctor, a well patient checkup, and no unmet medical needs because of cost by 60%, 56%, and 57% of the corresponding pretreatment sample rates and 102%, 122%, and 74% of the standard deviations.

The estimated effects of insurance on risky behavior and self-assessed health outcomes are all statistically insignificant, as would be expected given the lack of significance in the reduced-form regressions. The magnitudes are quite small for three of the seven outcomes. Having insurance reduces BMI by only 0.2 units, or 0.7% of the sample mean and 3.1% of the standard deviation. In contrast, the magnitudes were much larger in the cases from the prior literature where health insurance coverage has been estimated to have a statistically significant impact on BMI. Courtemanche and Zapata (2014) found that insurance decreased BMI by 3.8 units using the Massachusetts reform as an instrument, while Barbaresco, Courtemanche, and Qi's (2015) results from the ACA's dependent coverage mandate imply that insurance reduced BMI by 1.6 units.

Similarly, our estimated effect of insurance coverage on the probability of being in very good or excellent health is just 2.2 percentage points, or 4.1% of the mean and 4.4% of the standard deviation. Again, this magnitude is far smaller than those from the prior studies where the effect of insurance on overall self-assessed health has been statistically significant. For instance, Finkelstein et al. (2012) found that, in the context of the Oregon Medicaid experiment, health insurance raised the probability of being in good or better health by 13 percentage points. Courtemanche and Zapata (2014) estimated that the effect of insurance gained from the MA reform on the probability of very good or excellent health was 23 percentage points. For days with health-related limitations, the magnitude of our IV estimate is just 0.17 days, or 6.7% of the mean and 2.5% of the standard deviation. The analogous estimates from Finkelstein et al. (2012) and Courtemanche and Zapata (2014) were −1.3 and −1.6 days, respectively.

For the remaining four outcomes, the magnitudes are more substantial if taken literally in spite of their statistical insignificance. Insurance coverage is estimated to increase the probability of being a smoker by 8.5 percentage points, or 41% of the sample mean and 21% of the sample standard deviation, and the number of drinks per month by 63% of the mean and 25% of the standard deviation. Yet, obtaining coverage reduces the number of days not in good physical and mental health by 0.95 and 1.68 days per month, respectively. These magnitudes represent 26% and 41% of the respective means and 12% and 21% of the standard deviations. They are also in the general vicinity of the statistically significant estimates from prior studies. For instance, Finkelstein et al. (2012) found that health insurance coverage reduced days not in good physical and mental health by 1.3 and 2.1 days, respectively, while Courtemanche and Zapata (2014) found reductions of 1.1 and 1.3 days for these outcomes.

In sum, the IV estimates provide new insights regarding magnitudes. If our ACA treatment variables only influence the outcomes via the extensive margin of insurance coverage, then the gains in health care access from obtaining health insurance coverage from the ACA are very large, but broadly consistent with those estimated elsewhere. The effects of ACA-induced insurance coverage on risky behaviors and self-assessed health, however, are less clear. All are statistically insignificant, with three being small, two being large in the direction of better health, and two being large in the direction of worse health. Together, these results suggest that the ACA did not discernably impact health in either direction in its first two years, but that future research with additional years of data and a larger sample size is necessary before reaching a definitive conclusion.

Our instruments generally perform well in the diagnostic tests. They generate first stage F statistics that are more than an order of magnitude above the weak instrument threshold of 10. The overidentification test only rejects the null hypothesis at the 5% level for primary care doctor and days with health limitations, and some rejections are to be expected given the potential for the Medicaid and private portions of the ACA to induce very different local average treatment effects of insurance coverage.

7. Event Study Model

We next return to the reduced-form model and evaluate its key identifying assumptions. First, conditional on the controls, changes in our outcomes in 2014–2015 would not have been correlated with pretreatment uninsured rates in the absence of the ACA. Second, differential changes in 2014–2015 between Medicaid expansion and nonexpansion states would not have been correlated with pretreatment uninsured rates. We indirectly assess the plausibility of these assumptions by estimating an event study model that includes the interactions of the treatment variables with the full set of year fixed effects, with 2013 being the base year. The model is

$$
\begin{aligned}
y_{\mathrm{iast}} = {} & \theta_0 + \theta_1(\mathrm{UNINSURED}_{as} * Y2011_t) + \theta_2(\mathrm{UNINSURED}_{as} * Y2012_t) + \\
& \theta_3(\mathrm{UNINSURED}_{as} * Y2014_t) + \theta_4(\mathrm{UNINSURED}_{as} * Y2015_t) + \theta_5(\mathrm{MEDICAID}_s \\
& * Y2011_t) + \theta_6(\mathrm{MEDICAID}_s * Y2012_t) + \theta_7(\mathrm{MEDICAID}_s \\
& * Y2014_t) + \theta_8(\mathrm{MEDICAID}_s * Y2015_t) + \theta_9(\mathrm{UNINSURED}_{as} * \mathrm{MEDICAID}_s \\
& * Y2011_t) + \theta_{10}(\mathrm{UNINSURED}_{as} * \mathrm{MEDICAID}_s * Y2012_t) + \theta_{11}(\mathrm{UNINSURED}_{as} \\
& * \mathrm{MEDICAID}_s * Y2014_t) + \theta_{12}(\mathrm{UNINSURED}_{as} * \mathrm{MEDICAID}_s * Y2015_t) \\
& + \boldsymbol{\theta}_{13}\boldsymbol{X}_{\mathbf{iast}} + \alpha_{as} + \varepsilon_{\mathrm{iast}}
\end{aligned} \quad (2)
$$

where $Y2011_t$, $Y2012_t$, $Y2014_t$, and $Y2015_t$ are indicators for whether year t is 2011, 2012, 2014, or 2015, respectively. The tests for differential pretreatment trends (i.e., falsification tests) are provided by evaluating whether the coefficients on the "treatment" variables in the pretreatment years (θ_1, θ_2, θ_9, θ_{10}) are equal to zero.[19]

Table 5 presents the event study results for the seven outcomes related to health care access and health behaviors, and Table 6 presents similar results for the four outcomes related to self-assessed health using the full set of controls. In each table, the top panel presents the coefficient estimates of interest. Between the two tables, there are a total of 44 falsification tests (four parameters of interest in each of 11 regressions) and only three significant results at the 5% level. Three out of 44 is 6.8%, which is only slightly higher than would be expected by chance. These results, therefore, provide some reassurance about the validity of our model to estimate causal effects for the "true" ACA.

Another advantage of the event study specification is that it allows us to distinguish between the effects of the ACA in 2014 and 2015. The most notable result is that the coverage gains from the ACA appear to have increased in the second year relative to the first year, with the increase

[19] Recall that the coefficient on the $\mathrm{MEDICAID}_s{*}\mathrm{POST}_t$ variable in our main regression was assumed to capture unobserved confounders rather than part of the causal effect of the Medicaid expansion. We, therefore, do not consider θ_1 and θ_2 to provide additional falsification tests.

Table 5. Event Study Regressions for Health Care Access and Health Behaviors

	Insurance Coverage	Primary Care Doctor	Cost Barrier	Checkup	BMI	Smoker	Drinks per Month
Coefficient Estimates of Interest (2013 is base year)							
2011* Pretreatment uninsured	0.018	−0.057	0.094	−0.296[c]	0.602	0.035	−5.326
	(0.069)	(0.059)	(0.060)	(0.087)	(0.723)	(0.043)	(3.366)
2012* Pretreatment uninsured	−0.052	−0.022	−0.011	−0.173[a]	−0.950	0.022	0.926
	(0.067)	(0.081)	(0.056)	(0.066)	(0.622)	(0.046)	(4.642)
2014* Pretreatment uninsured	0.193[c]	0.115[b]	−0.129[b]	−0.0143	−0.435	0.010	8.360[a]
	(0.046)	(0.045)	(0.054)	(0.050)	(0.723)	(0.050)	(3.968)
2015* Pretreatment uninsured	0.310[c]	0.140	−0.106[b]	−0.021	−0.623	0.041	−3.211
	(0.066)	(0.117)	(0.052)	(0.088)	(0.614)	(0.041)	(3.336)
Medicaid expansion* 2011* Pretreatment uninsured	0.027	0.054	−0.047	0.096	−0.167	−0.046	0.462
	(0.075)	(0.074)	(0.045)	(0.081)	(0.834)	(0.048)	(5.479)
Medicaid Expansion* 2012* Pretreatment Uninsured	−0.001	0.113	−0.006	0.039	1.552	−0.094	−10.885
	(0.106)	(0.103)	(0.047)	(0.053)	(0.809)	(0.052)	(5.856)
Medicaid expansion* 2014* Pretreatment Uninsured	0.134[a]	0.011	−0.005	0.046	−0.391	0.019	−8.835[a]
	(0.062)	(0.074)	(0.050)	(0.060)	(0.916)	(0.050)	(4.117)
Medicaid Expansion* 2015* Pretreatment uninsured	0.197[a]	0.083	−0.142[b]	0.100	1.171	−0.024	−0.428
	(0.082)	(0.115)	(0.048)	(0.077)	(0.793)	(0.042)	(3.684)
Implied Effects of ACA at Mean Pretreatment Uninsured Rate							
ACA without Medicaid expansion in 2014	0.039[c]	0.023[b]	−0.026[a]	−0.003	−0.088	0.002	1.695[a]
	(0.009)	(0.009)	(0.001)	(0.010)	(0.146)	(0.010)	(0.805)
ACA without Medicaid expansion in 2015	0.063[c]	0.028	−0.022[a]	−0.004	−0.126	0.008	−0.651
	(0.013)	(0.024)	(0.010)	(0.018)	(0.124)	(0.008)	(0.677)
Full ACA (with Medicaid expansion) in 2014	0.066[c]	0.026	−0.027[c]	0.006	−0.009	0.006	−0.096
	(0.012)	(0.015)	(0.008)	(0.013)	(0.128)	(0.011)	(0.752)
Full ACA (with Medicaid expansion) in 2015	0.103[c]	0.045[b]	−0.050[c]	0.016	0.111	0.004	−0.564
	(0.013)	(0.014)	(0.012)	(0.010)	(0.182)	(0.007)	(0.845)

Notes: Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. BRFSS sampling weights are used. All regressions include state * location type and year * location type fixed effects as well as the controls.
[a]Indicates statistical significance at 5% level.
[b]Indicates statistical significance at 1% level.
[c]Indicates statistical significance at 0.1% level.

coming entirely from the private portion. Specifically, in 2014, the fully implemented ACA increased the probability of a nonelderly adult being insured by 6.6 percentage points, with 3.9 percentage points coming from the private portion and the remaining 2.7 percentage points from Medicaid. These magnitudes are similar to those estimated by Courtemanche et al. (2017) using the ACS data. In contrast, in 2015, the coverage gains from the full ACA jumped to 10.3 percentage points, with 6.3 percentage points coming from the private component and 4 percentage points coming from Medicaid.[20] Accordingly, the gains in primary care access and reductions in cost barriers also increased in 2015 relative to 2014, though these increases appear to have come entirely

[20] Our finding of additional coverage gains in 2015 is consistent with the Cohen, Martinez, and Zammitti (2016) descriptive examination of changes over time in coverage using the National Health Interview Survey (NHIS). They report in their table 17 that among nonelderly adults, the increase in those reporting coverages of any type was 4.1 percentage points between 2013 and 2014 and 3.5 percentage points between 2014 and 2015. For public (private) coverage, their table 18 (19) suggests the increase was 1.0 3.1 percentage point(s) between 2013 and 2014 and 1.2 (2.4) percentage points between 2014 and 2015.

Table 6. Event Study Regressions for Self-Assessed Health

	Days in Very Good or Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
Coefficient Estimates of Interest (2013 is base year)				
2011* Pretreatment uninsured	−0.062	0.922	1.492	1.056
	(0.047)	(1.606)	(1.191)	(0.687)
2012* Pretreatment uninsured	0.134	1.477	1.120	1.375[a]
	(0.076)	(1.664)	(0.769)	(0.650)
2014* Pretreatment uninsured	0.080[a]	−0.515	−0.213	−0.292
	(0.033)	(1.142)	(0.836)	(0.625)
2015* Pretreatment uninsured	0.088	0.358	1.300	0.370
	(0.072)	(0.907)	(0.649)	(0.761)
Medicaid expansion* 2011*	0.007	−1.221	−0.686	−1.093
Pretreatment uninsured	(0.070)	(1.457)	(1.309)	(0.735)
Medicaid expansion* 2012*	−0.087	0.447	−0.794	−0.361
Pretreatment uninsured	(0.089)	(1.383)	(0.846)	(0.552)
Medicaid expansion* 2014*	−0.081	0.715	−1.344	0.599
Pretreatment uninsured	(0.059)	(1.236)	(0.774)	(0.997)
Medicaid expansion* 2015*	−0.066	−0.072	−0.855	0.873
Pretreatment uninsured	(0.092)	(1.062)	(1.019)	(0.847)
ACA without Medicaid expansion	0.016[a]	−0.105	−0.043	−0.059
in 2014	(0.007)	(0.232)	(0.170)	(0.127)
ACA without Medicaid expansion	0.018	0.073	0.263	0.075
in 2015	(0.015)	(0.184)	(0.132)	(0.154)
Full ACA (with Medicaid expansion)	−0.0002	0.040	−0.316[a]	0.063
in 2014	(0.011)	(0.176)	(0.134)	(0.199)
Full ACA (with Medicaid Expansion)	0.004	0.058	0.090	0.252
in 2015	(0.014)	(0.175)	(0.150)	(0.168)

Notes: Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. BRFSS sampling weights are used. All regressions include state * location type and year * location type fixed effects as well as the controls.
[a]Indicates statistical significance at 5% level.
[b]Indicates statistical significance at 1% level.
[c]Indicates statistical significance at 0.1% level.

from the Medicaid expansion. The event study design also causes a few sporadic results to emerge for the health behavior and self-assessed health outcomes. In particular, the fully implemented ACA reduced days in poor mental health in 2014 (but not 2015). Such results, however, could simply be a byproduct of the large number of hypotheses tested by the event study models.

8. Analyses Stratifying by Income

One possible explanation for the large number of null results, particularly for the Medicaid expansion, might be that the full sample includes various groups of people with different probabilities of being treated by the ACA. In this section, we examine whether more effects show up if we "zoom in" on those with low to middle incomes who were most likely to receive free or subsidized insurance as a result of the ACA. Unfortunately, perhaps due to the demanding nature of the DDD specification and the need for each subsample to have sufficient numbers of individuals in each area to reliably compute pretreatment uninsured rates, splitting the sample into three or more groups results in estimates that are too imprecise to be useful. We therefore simply stratify into two groups of approximately equal size: those above and those below the median household income.

Table 7. Income Below Median Subsample

	Insurance Coverage	Primary Care Doctor	Cost Barrier	Checkup	BMI	Smoker
ACA w/o Medicaid	0.052[b]	0.041[b]	−0.016	0.046[a]	−0.006	−0.005
	(0.017)	(0.013)	(0.009)	(0.018)	(0.153)	(0.012)
Medicaid Expansion	0.067[b]	−0.002	−0.044[a]	−0.010	−0.091	0.019
	(0.026)	(0.021)	(0.017)	(0.023)	(0.209)	(0.014)
Full ACA (w/Medicaid)	0.119[c]	0.038[a]	−0.060[b]	0.035	−0.098	0.013
	(0.019)	(0.017)	(0.019)	(0.018)	(0.165)	(0.010)
Pretreatment Mean and	0.674	0.664	0.289	0.581	28.344	0.276
Standard Deviation	(0.469)	(0.472)	(0.453)	(0.493)	(6.766)	(0.447)
Sample Size	672,937	672,627	548,521	549,596	638,395	660,975
	Drinks per Month	Very Good or Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations	
ACA w/o Medicaid	1.728[a]	0.012	−0.316	−0.300	−0.388	
	(0.693)	(0.011)	(0.159)	(0.243)	(0.247)	
Medicaid Expansion	−1.399[a]	−0.002	0.187	−0.233	0.504	
	(0.643)	(0.015)	(0.245)	(0.300)	(0.360)	
Full ACA (w/Medicaid)	0.330	0.009	−0.129	−0.533[a]	0.116	
	(0.816)	(0.016)	(0.232)	(0.233)	(0.282)	
Pretreatment Mean and	12.508	0.426	4.798	5.276	3.482	
Standard Deviation	(37.676)	(0.495)	(9.052)	(9.262)	(7.969)	
Sample Size	640,349	672,765	663,572	664,825	668,102	

Notes: Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. BRFSS sampling weights are used. All regressions include state * location type and year * location type fixed effects as well as the controls. (Pretreatment Uninsured Rate = 0.316)
[a]Indicates statistical significance at 5% level.
[b]Indicates statistical significance at 1% level.
[c]Indicates statistical significance at 0.1% level.

Tables 7 and 8 report the results. It is reassuring that the sizeable gains in access were concentrated in the below-median-income subsample. The increase in insurance coverage from the full ACA was 11.9 percentage points for the lower-income group—with the majority of this increase coming from the Medicaid expansion—compared with 2.0 percentage points for the higher-income group. The gains in the other access outcomes appear to have been entirely concentrated among the lower-income subsample. For this group, the effects on having a primary care doctor and an annual checkup were driven mostly by the private portion of the ACA, while the reduction in cost barriers was driven mostly by the Medicaid expansion.

The results on risky health behaviors and self-assessed health generally show the same null effects we saw in Table 3, with a few exceptions. Most notably, one improvement in the self-assessed health outcomes emerges for the lower-income subsample: a reduction in days not in good mental health in expansion states. We also observe a few mixed results for health behaviors. The private portion of the ACA increased drinks per month, while the Medicaid expansion decreased drinking by a similar amount, leading to a null effect of the full ACA. Additionally, the Medicaid expansion increased smoking among the higher income subsample, a result that seems likely to be spurious since this group would not have qualified for Medicaid. We are reluctant to emphasize these few significant results as they do not seem to fit a broader pattern, and we would

Table 8. Income above Median Subsample

	Insurance Coverage	Primary Care Doctor	Cost Barrier	Checkup	BMI	Smoker
ACA w/o Medicaid	0.026[c]	0.010	−0.003	−0.006	−0.019	0.005
	(0.006)	(0.007)	(0.005)	(0.012)	(0.061)	(0.005)
Medicaid expansion	−0.007	−0.006	−0.004	0.010	0.084	0.015[b]
	(0.006)	(0.008)	(0.008)	(0.017)	(0.101)	(0.005)
Full ACA (w/Medicaid)	0.020[c]	0.005	−0.007	0.004	0.065	0.020[b]
	(0.006)	(0.006)	(0.008)	(0.012)	(0.110)	(0.007)
Pretreatment mean and	0.933	0.837	0.070	0.684	27.289	0.141
standard deviation	(0.249)	(0.369)	(0.255)	(0.465)	(5.580)	(0.348)
Sample size	649,433	648,940	522,717	522,941	625,848	639,844

	Drinks per Month	Very Good or Excellent Health	Days Not in Good Physical Health	Days Not in Good Mental Health	Days with Health-Related Limitations
ACA w/o Medicaid	−0.335	0.002	−0.064	0.095	−0.017
	(0.340)	(0.006)	(0.071)	(0.093)	(0.057)
Medicaid expansion	0.863	−0.009	0.048	0.112	0.046
	(0.452)	(0.008)	(0.111)	(0.137)	(0.084)
Full ACA (w/Medicaid)	0.528	−0.007	−0.017	0.207	0.029
	(0.509)	(0.008)	(0.111)	(0.115)	(0.085)
Pretreatment mean and	16.474	0.675	2.214	2.651	1.283
standard deviation	(33.297)	(0.468)	(6.000)	(6.330)	(4.605)
Sample size	627,768	649,034	646,052	643,712	648,169

Notes: Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. BRFSS sampling weights are used. All regressions include state * location type and year * location type fixed effects as well as the controls. (Pretreatment Uninsured Rate = 0.062)
[a]Indicates statistical significance at 5% level.
[b]Indicates statistical significance at 1% level.
[c]Indicates statistical significance at 0.1% level.

expect a couple of "effects" to emerge simply by chance given the large number of null hypotheses we are testing in these tables.[21] Overall, there is little evidence that the ACA influenced health behaviors or self-assessed health in the first two years, even for the lower-income subsample that experienced the largest gains in access.

9. Discussion

In this article, we used data from the Behavioral Risk Factor Surveillance System to examine the effects of the 2014 ACA provisions on health care access, risky health behaviors, and self-assessed health. Using a DDD strategy that exploits variation in time, pretreatment uninsured rates, and state Medicaid expansion status, we separately estimated the effects in both Medicaid

[21] We also stratified the sample by education, splitting the sample into those with less than a college degree and those with a college degree or higher. The patterns produced by our education stratification largely mimic the findings from our income stratification analysis, so are not reported. Additionally, we stratified the sample by age and found some evidence of gains in self-assessed health among the older half of the sample if we split it at the median. However, these results were not robust to small changes in the age cutoff used to split the sample. Results from the education and age subsample analyses are available on request.

expansion and nonexpansion states. The results suggest that the ACA improved access to care along all observable dimensions—including health insurance coverage, having a primary care doctor and a well-patient checkup in the past year, and cost barriers—in both expansion and nonexpansion states. The gain in coverage and reduction in cost barriers were significantly greater in expansion states. The magnitudes of the estimates imply effects of insurance on health care access that are at least as large as those observed in the Oregon Medicaid experiment.

Our lack of significant results for risky health behaviors suggests that the ex ante moral hazard, improved access to health-behavior-promoting medical care, and income effects brought about by insurance coverage either offset each other or are too small to be statistically detectable in our sample. The extent of ex ante moral hazard may be modest because the consumption value of good health may be a sufficient deterrent even if an individual is insulated from the financial consequences of illness. Improved access to medical care may be of only limited value with regard to health behaviors since they are generally not as easy to treat as acute conditions. Income effects may also be relatively small given the mixed results in the literature as to the causal impact of income on health behaviors and the potential for individuals to value in-kind spending on health insurance at less than its cost.[22]

Our inability to find clear evidence that the ACA improved self-assessed health contrasts the large, immediate gains in similar outcomes observed after the Oregon Medicaid experiment (Finkelstein et al. 2012) and the Massachusetts reform (Van der Wees, Zaslavsky, and Ayanian 2013; Courtemanche and Zapata 2014). The Oregon experiment was unique in that it was purely among low-income individuals who had demonstrated some interest in their health by actively registering for the lottery. The effects of the Massachusetts reform could plausibly differ from those of the ACA for several reasons, including differences in population demographics, the fact that the Massachusetts reform's insurance expansions for adults were done completely through private coverage as opposed to a mix of public and private coverage, and the greater prevalence of high deductibles in the ACA's private plans (Wharam, Ross-Degnan, and Rosenthal 2013). Another possible explanation is the relative lack of popularity of the ACA compared to these other interventions.[23] It has been hypothesized that the large, immediate gains in self-assessed health after insurance expansions may be attributable at least in part to a "warm glow" from gaining coverage (e.g., winning the lottery in Oregon, receiving insurance through a popular program in Massachusetts) rather than from actually utilizing additional medical care (Finkelstein et al. 2012; Courtemanche and Zapata 2014). Perhaps the amount of "warm glow" is smaller if the intervention bringing about the coverage is controversial, such as with the ACA.

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[22] See Cawley and Ruhm (2012) for an overview of the literature on the effect of income on risky health behaviors. Subsequent to their literature review, additional articles using natural experiments have continued to find mixed results (e.g., Adams, Blackburn, and Cotti 2012; Averett and Wang 2013; Kenkel, Schmeiser, and Urban 2014; Apouey and Clark 2015; Au and Johnston 2015; and Cowan and White 2015). Around 84% of individuals with a marketplace plan in 2015 qualified for an advance premium tax credit (PTC); conditional on qualifying, the advance PTC was \$272 per month. See CMS (2016). Gallen (2015) finds that each \$1.00 of Medicaid spending is valued at \$0.26–\$0.35 to participants.

[23] Blendon et al. (2008) report that in June 2008, two years after the implementation of the Massachusetts health care reform, 69% of residents supported the law. In contrast, a tracking poll conducted by the Kaiser Family Foundation stated that in December 2016, only 43% of adults viewed the ACA favorably. For further information on this poll, see KFF (2017).

Several caveats of our work provide directions for future research. For instance, investigation of clinical health outcomes is necessary to provide a fuller picture of the ACA's health effects. Additionally, future studies should continue to track the indicators used in our article over a longer period, as the effects of insurance on health could take many years to fully materialize or could require a larger sample to be statistically detectable. Next, our identification strategy implicitly assumes that effects of the ACA are concentrated among those who lacked coverage prior to the law's implementation. Future research should investigate whether impacts could also occur among, for instance, those who switched from catastrophic to more comprehensive coverage as a result of the ACA's minimum standards for insurance plans, or who experienced significant income shocks as a result of the subsidies or changes in premiums.[24] Finally, understanding the ACA's effects on health care access and health outcomes provides only part of the story with regard to evaluating the welfare effects of the law. For instance, protection against financial risk is a critical component of the gains from insurance, so the consumption-smoothing benefits of the ACA could confer a sizeable benefit even in the absence of discernable short-run health effects. Hu et al. (2016) found evidence from credit report data that the ACA's Medicaid expansion improved financial outcomes. But Pauly (2017) questions whether the poor should be allowed to purchase high-deductible marketplace plans. The ACA also contains a number of other components unrelated to insurance coverage, such as provider payment reforms and tax increases, that each represent a part of the overall picture. Thus, both the size and scope of the ACA have generated the need for a great deal of future research in order to better understand the multifaceted nature of its impacts.

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[24] For instance, 7.7% of nonelderly adults directly purchased individual coverage prior to the 2014 reforms (author's calculations using the ACS). For these individuals, the ACA's premium tax credit could directly substitute for household income devoted to health insurance. While many of these people likely experienced positive income shocks, some may have been spending less on insurance prior to the ACA, perhaps because they were purchasing noncomprehensive policies (Clemans-Cope and Anderson 2014). Thus, it is possible that the share of their budget spent on health insurance could have increased even in the presence of the subsidies.

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