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THE IMPACT OF INSURANCE EXPANSIONS ON THE ALREADY INSURED: THE AFFORDABLE CARE ACT AND MEDICARE

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ABSTRACT

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The Impact of Insurance Expansions on the Already Insured: The Affordable Care Act and Medicare

By Colleen M. Carey, Sarah Miller, and Laura R. Wherry*

Some states have not adopted the Affordable Care Act (ACA) Medicaid expansions due to concerns that the expansions may impair access to care and utilization for those who are already insured. We investigate such negative spillovers using a large panel of Medicare beneficiaries. Across many subgroups and outcomes, we find no evidence that the expansions reduced utilization among Medicare beneficiaries, and can rule out all but very small changes in utilization or spending. These results indicate that the expansions in Medicaid did not impair access to care or utilization for the Medicare population.

JEL: I11 (Analysis of Health Care Markets), I13 (Health Insurance, Public and Private), H51 (Government Expenditures and Health)

Many states have opted not to expand Medicaid eligibility under the Affordable Care Act (ACA), even though the federal government will pay for a high share of expansion costs, and despite evidence that these expansions substantially improve insurance coverage, health, and access to care for low-income individuals.

One rationale offered for not implementing the eligibility expansions is that they may strain already-overburdened health care providers and result in worse care for those who are already insured. In particular, many predicted that a surge in demand for primary care under the ACA coverage expansions would exacerbate a national shortage in the primary care workforce.[1] The effects of insurance expansions on use of care are well documented, with sizable increases in health care utilization associated with insurance coverage, at least in the short-term. Thus, if physicians are unable to accommodate the new demand for health services, the Medicaid expansions may drive up wait times and worsen health care access for the already insured. These concerns may be especially acute in areas that experienced health professional shortages prior to the expansions, or areas where the increase in Medicaid eligibility is particularly large.

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[1]For example, analyses in Huang and Finegold (2013) and Hofer et al. (2011) forecasted the need for 4,000 to 7,000 additional physicians in the primary care workforce to meet increased demand under the ACA.

Concerns about such "negative spillovers" for the already insured population are not unfounded. Recent research in economics predicts that not only would the ACA expansions generate such spillovers, but that they would be large. McInerney et al. (2017) examine pre-ACA Medicaid expansions to non-elderly adults and find they caused substantial reductions in care, particularly among beneficiaries eligible for both Medicare and Medicaid coverage (i.e., the "dually eligible"). Estimates from McInerney et al. (2017) imply that expansions as large as those seen under the ACA would reduce medical spending among dually-eligible beneficiaries by nearly 20 percent in expansion states. Glied and Hong (2018) similarly document large negative spillovers on the Medicare population resulting from Medicaid coverage expansions; their estimates predict reductions in physician services spending among Medicare beneficiaries of about 6.5 percent in expansion states, with substantially larger reductions predicted in lower income areas where the increase in coverage was large.[2]

The robust evidence documenting that Medicaid coverage results in substantial increases in the utilization of medical care among its recipients lends further credence to these concerns. The Centers for Medicare and Medicaid Services (CMS) actuarial report states that 9.1 million newly eligible adults enrolled in Medicaid in 2015 (Centers for Medicare and Medicaid Services, 2016a). Estimates from the Oregon Health Insurance Experiment imply that these newly-enrolled individuals will demand slightly more than 2 additional office visits, resulting in about 19.7 million additional office visits demanded in their first year of coverage. If visits with Medicaid patients were substituted for visits with Medicare patients one-for-one, we would expect about 0.73 fewer visits for each Medicare beneficiary living in expansion states during that year. If such negative spillovers are concentrated on disadvantaged populations, such as the dually eligible or those living in low income areas, these effects could be considerably larger.

This paper provides the first evidence on whether the ACA coverage expansions generated these expected negative spillovers in utilization for the already insured. Our research design compares changes in the utilization of Medicare beneficiaries in states that did and did not expand Medicaid. Medicare beneficiaries in the two groups of states had very similar utilization patterns prior to the expansion, but beneficiaries in expansion states were exposed to significantly larger demand shocks as a result of the ACA. Medicare beneficiaries are an ideal population in which to examine the possibility of negative spillovers: Medicare beneficiaries have high health needs, and may therefore be more sensitive to issues related to provider availability, and the Medicare program itself experienced little change at the time of the Medicaid expansions.[3] We observe the utilization of a panel of nearly 4 million Medicare beneficiaries between 2008 to 2015, ensuring adequate power to detect even very small negative spillovers.

[2]See Section V and Appendix Section A for more discussion of these calculations.

[3]There were several changes to Medicare in the ACA implemented over the sample period but these changes were national in scope, differed in timing from the Medicaid expansions, and are not expected to affect Medicaid expansion states differently than non-expansion states. Specifically, the ACA altered payment formulas for both fee-for-service and Medicare Advantage to slow the pace of spending growth; initiated a number of payment demonstrations in fee-for-service Medicare, including the Accountable Care Organization model introduced in 2013; enhanced Part D benefits by gradually closing the "coverage gap" between 2011 and 2020; and eliminated cost-sharing for preventive services and an annual wellness visit starting in 2011.

We focus our attention on the utilization of primary care services, motivated in part by concerns about the adequacy of the primary care workforce to meet increased demand under the ACA. According to the National Ambulatory Medicare Care Survey, Medicare beneficiaries represent about 25 percent of the patients of a typical primary care physician and about 67 percent of physicians who treat Medicare beneficiaries also see Medicaid and uninsured patients, suggesting that substitution between these patient groups may be relevant for these physicians. Existing research confirms that, in states that implemented the ACA Medicaid expansions early, primary care physicians were particularly affected, and increased the number of Medicaid patients they saw by 10 percent in the first year following the policy (Bhole and Curto, 2016). Furthermore, at the time of the Medicaid expansions, primary care services were reimbursed at parity between Medicare and Medicaid. Services with equal reimbursement are of special interest because our theoretical model predicts that utilization of such services should fall among the already-insured with the onset of the Medicaid expansions. We adapt the model described by Sloan et al. (1978) to our setting to show how health care providers would respond to a Medicaid coverage expansion accompanied by parity in reimbursement rates, as occurred with the ACA for primary care services. Under reimbursement parity, providers who supply equally profitable patients with visits on a first-come, first-serve basis can be expected to see fewer Medicare beneficiaries and more Medicaid beneficiaries when an expansion occurs. We therefore focus on services for which the ACA increased Medicaid fees to the Medicare level; it is exactly these types of services for which we would expect the crowding out of Medicare beneficiaries to be the most likely.

In contrast to the concerns that have been expressed by policymakers and the media, we find a tightly-bounded null effect of coverage expansions on primary care utilization. Our difference-in-differences model implies no difference in the utilization of primary care services after the expansions occurred among Medicare beneficiaries in expansion and non-expansion states. Our estimates for the effect on the quantity of primary care visits is *positive* in sign and close to zero. The 95 percent confidence interval bounds the effect at a decline of less than one-third of a visit, a 2.9 percent change relative to the 9.4 such visits Medicare beneficiaries have on average per year. The worst case scenario of a one-to-one substitution between Medicaid and Medicare patients is easily ruled out by this lower bound. Since providers may have privileged existing patients over new patients, we also examine new patient visits among Medicare beneficiaries, and find a similarly precise zero. We are able to reject reductions in new primary care visits larger than 0.7 percent. Event studies for these outcomes show parallel trends in the variables prior to expansion, supporting the assumption that underlies our difference-in-differences model.

These null results extend to several Medicare subpopulations that we hypothesize are most likely to experience negative spillovers. First, we examine dually-eligible Medicaid-Medicare beneficiaries who we expect are more likely to be treated by the same providers as the Medicaid expansion population. In addition, evidence from pre-ACA state Medicaid expansions for low-income adults indicates that negative spillovers were concentrated among this group (McInerney et al., 2017). Second, we examine Medicare beneficiaries who reside in high-impact zip codes that had low incomes and high rates of uninsurance prior to the ACA expansions; we show that these zip codes experienced larger demand shocks. Third, we examine Medicare beneficiaries residing in counties designated as Health

Provider Shortage Areas (HPSAs), where we expect that access to providers is especially likely to be impaired. Last, we examine whether the effects of the expansion differ among sick and healthy subsamples that we construct based on the presence of diagnostic codes in the pre-period. We expect that negative spillovers may be found either among the sick, with much higher health care demand, or the healthy, if health providers concentrate unavoidable reductions on the healthy. Our null results are replicated across each of these subsamples, and again our large sample sizes allow us to reject meaningful effects. We also assess the robustness of our findings to different sample definitions and when focusing on states with the largest expansions, as measured by the share of the population gaining eligibility, but continue to find no evidence of negative spillovers.

We also explore several other outcomes designed to characterize negative spillovers, if present. Our results indicate no change associated with Medicaid expansion in Medicare beneficiaries' total number of physician services, number of days between physician services, number of services with a physician extender (such as a nurse practitioner), physician services expenditures, or total medical expenditures. We also assess survey evidence on access to care as reported by Medicare beneficiaries. We find no evidence of changes in either appointment availability or in-office wait times. Finally, we repeat the analysis using a synthetic control research design, but again find no evidence of differential utilization between expansion and non-expansion states.

Thus, overall we present robust, well-identified, tightly-bounded evidence that the Medicaid expansions did not lead to economically-meaningful utilization reductions in the Medicare population. Our findings have significant policy implications and suggest that the 14 states who have not yet expanded Medicaid may be able to do so without impairing access to care in the wider population. These findings are consistent with a recent study by Polsky et al. (2017) that finds no evidence of a change in the availability of appointments for primary care among the privately insured between 2012-2013 and 2016 in a 10-state audit study. Our paper stands in contrast, however, to two previous analyses finding negative spillovers for Medicare beneficiaries following pre-ACA state Medicaid expansions for low-income adults (McInerney et al., 2017; Glied and Hong, 2018). In addition, the 95 percent confidence intervals on our estimates allow us to rule out changes of the magnitude estimated in this previous work. Our findings of no negative spillovers are especially notable since the coverage expansions we study are large and sharp, compared to many pre-ACA state expansions.

Finally, showing that the health care industry can accommodate Medicaid expansion without affecting Medicare utilization has implications for our understanding of the sector's industrial organization. There are several ways to explain our findings. There would be no negative spillovers in Medicare if the Medicaid expansion did not greatly increase the health care demand of those who gained coverage; however, a growing number of papers find that, indeed, the expansion population increased their service utilization.[4] A

[4]For example, Miller and Wherry (2017) and Simon et al. (2017) find the Medicaid coverage increases are associated with increased use of some preventive services. Wherry and Miller (2016) find the expansions associated with higher rates of physician office visits in the first year. Sommers et al. (2015) find that the expansions were associated with increased reports of having a personal physician. Ghosh et al. (2017) find the expansions are associated with increased use of prescription drugs. In their study of pre-ACA expansions to a similar population in Oregon, Finkelstein et al. (2012) and Baicker et al. (2013) document sizable increases in health care utilization, including primary and preventive care.

second explanation is that the increase in demand was met with the assistance of mid-level providers, such as physician assistants and nurse practitioners. States have expanded the scope-of-practice for these providers over time, granting them greater autonomy in patient care (Gadbois et al., 2015).[5] While we find no change in the care supplied to Medicare beneficiaries by mid-level providers, such providers may have primarily supplied care to the newly-insured Medicaid beneficiaries. Thirdly, while our model predicts that doctors should be indifferent between Medicaid and Medicare beneficiaries when reimbursement rates are identical, there may be non-pecuniary aspects of treating Medicaid patients that make them less attractive (e.g., additional paperwork or lags in payments, see Gottlieb et al., 2018a) or additional financial benefits to treating Medicare patients (further discussed in Section II). Finally, it may be the case that physicians reduced access to care for other groups, such as the uninsured or self-pay patients, in order to see more Medicaid patients (see, e.g., Chen, 2017). These types of negative spillovers would not be captured in our analysis of the Medicare population.

I. Previous Literature

Given concerns over provider constraints affecting access to care, a number of recent studies have examined whether expanded insurance coverage has negative effects on the already-insured. Most closely related to our study, two separate papers examine expansions in state Medicaid programs prior to the ACA and find evidence of negative spillovers for Medicare beneficiaries. McInerney et al. (2017) use a simulated eligibility approach to examine the effects of changes in state eligibility rules for non-elderly adults in the 2000s. The authors find that increased Medicaid eligibility leads to reductions in total spending and health care utilization among dually-eligible Medicare beneficiaries, but do not find similar effects for non-duals. The authors find no evidence of adverse health effects for dual beneficiaries on mortality or measures of self-reported health. Also using a simulated eligibility approach, Glied and Hong (2018) find significant reductions in office-based Medicare spending and surgical discharges associated with increased state Medicaid eligibility for parents. The authors also find evidence of decreases in Medicare utilization associated with increased demand from the privately insured driven by changes in firm size composition.[6] Finally, the authors examine whether there are changes in physician behavior or patient experience associated with insurance expansion. They find little evidence that changes in coverage rates affect physician labor supply or income, wait time for appointments, or patient satisfaction, but do find evidence of fewer annual physician visits for Medicare patients in areas with more insurance expansion. Surprisingly, despite receiving fewer physician visits per year, avoidable hospitalizations decrease for Medicare patients in these areas.

Prior work on insurance expansions under Massachusetts health care reform finds mixed

[5]In prior work, Buchmueller et al. (2016) find evidence that dental demand shocks after expanded Medicaid coverage were accommodated in part by supply increases by mid-level providers. Carrillo and Feres (2018) provide further evidence that physicians and nurses may be close substitutes for the provision of some services in their study of physician supply policy in Brazil.

[6]He et al. (2015) have similar findings using changes in county unemployment rates. They find that inpatient care for Medicare patients rises with county unemployment rates, especially among providers with a greater share of privately insured patients.

evidence on effects for the Medicare population. Bond and White (2013) find a modest decrease in the number of primary care visits per Medicare beneficiary in zip codes with high rates of uninsurance prior to reform, or those with the largest expected demand shocks. The authors do not find, however, any change in the proportion of beneficiaries with at least one primary care visit. Joynt et al. (2015) find no evidence of reductions in the number of office visits, quality of office-based care, or total cost of care for chronically ill Medicare patients. In addition, Joynt et al. (2013) find no evidence of an increase in preventable hospitalizations among Medicare beneficiaries, even in areas with larger insurance expansion.

Studies of spillover effects in the context of other types of public insurance expansions are also less clear in their predictions. Garthwaite (2012) finds that pediatricians decreased their labor supply in response to the Children's Health Insurance Program (CHIP) expansions, demonstrating a potential pathway for reduced utilization among already-insured children. He and White (2013) also find a decrease in pediatrician work hours associated with CHIP expansions. In contrast, Chen (2017) finds that physicians increase their total hours of care supplied in response to Medicaid eligibility expansions, and Chen et al. (2018) suggest that CHIP reauthorization led pediatricians to pursue further training and accept private practice positions (relative to hospital employment). White (2012) finds no evidence of a change in the use of physician services following CHIP expansions among children who do not themselves gain coverage, but does find *positive* spillovers associated with an increase in physician reimbursement fees. In the context of the dental industry, Buchmueller et al. (2016) find that Medicaid coverage of dental benefits results in modestly higher wait times in offices and for appointments. The authors find this effect is concentrated, however, in states with more restrictive policies regarding the scope of practice for dental hygienists, suggesting that the availability of mid-level providers might be important in determining spillovers.

Finally, Baker and Royalty (2000) study an expansion in Medicaid prenatal eligibility that was accompanied by increased provider fees. The authors find that the expansions increased utilization among poor patients but that this increase occurred in public settings rather than private physician practices. This suggests that any spillovers for the already-insured might be minimal or limited to those patients using certain providers.

II. Model

Our predictions about the impact of the ACA Medicaid expansions on Medicare enrollees are adapted from the widely-used model of Sloan et al. (1978). This model assumes that providers face a downward sloping demand curve for private patients and a fixed unit price for treating Medicaid and Medicare patients. Previous research (e.g., Decker, 2009; Hahn, 2013) shows that the reimbursement rate for Medicare is, in general, substantially higher than that of Medicaid. We depict graphically in panel A of Figure 1 what a marginal revenue curve for providers might look like in such a model: some portion of the marginal revenue curve is downward sloping (corresponding to the private market), and at some point it intersects with the fixed Medicare reimbursement rate for that service. The Medicare reimbursement rate is represented by the first (higher) horizontal portion of the marginal revenue curve. At some point, a provider might have served all of her Medicare customers and again begins to take on private patients. As marginal revenue continues to decrease with quantity, the private marginal revenue curve may intersect with the (lower) Medicaid reimbursement rate.

Providers differ in their production functions. Some providers, such as those represented by the curve MC-1 in the figure, exhibit a high-cost, low quantity style in which they see relatively few patients but charge a high per-visit fee. These providers profit-maximize when they see a mix of Medicare and private patients, but do not see Medicaid patients. Other providers, such as those represented by MC-2 and MC-3, have lower marginal costs and lower per-visit fees, and provide a larger number of visits. These providers see a mix of Medicaid, Medicare, and privately-insured patients. Only the lowest cost providers, depicted as MC-3, will see all available Medicaid patients; MC-3's marginal patient is a lower-paying, uninsured patient.

First, we consider the case where Medicaid coverage is expanded but the fees paid by Medicaid for medical services remain lower. This is represented by the extension of the Medicaid portion of the marginal revenue curve by the dotted line in panel A. In the absence of a fee bump, only providers like MC-3 are affected. After the expansion, these providers substitute low-paying, non-publicly insured patients for Medicaid patients. In this case, Medicare patients are completely unaffected by the coverage expansion.

Next, consider the case where Medicaid coverage is expanded *and* there is parity in fees between Medicare and Medicaid. We depict such a case in panel B of Figure 1. In this case, the marginal revenue curves of all providers are affected, all providers are now indifferent between seeing a Medicare and a Medicaid patient, and Medicaid patients are substituted for lower-paying non-public patients at the margin. In this scenario, existing patients, including both patients covered by Medicare and those already insured by Medicaid, may find themselves replaced, at least to some extent, by newly-insured Medicaid patients.[7]

Thus, this model of provider production implies that expansions in Medicaid may not affect access or utilization in Medicare for the majority of health care services where Medicare reimbursement exceeds Medicaid reimbursement. However, the providers of services with fee parity (primary care services) may begin providing more services to newly-insured Medicaid beneficiaries (for example, see Alexander and Schnell, 2017). In this scenario, Medicaid patients may be substituted for Medicare patients, which should manifest in lower utilization of primary care services among Medicare beneficiaries in states with Medicaid expansions.[8]

[7]Note that this may be offset somewhat since affected physicians will provide more visits in response to the higher marginal reimbursement rate; however, the size of this increase in the quantity supplied will depend on the shape of the marginal cost curve.

[8]For ease of presentation, we assume zero crowd-out of higher-paying, private insurance coverage under expanded Medicaid in the two scenarios described here. The presence of this type of crowd-out would not change the predictions of these models regarding negative spillovers for the Medicare population. We still only arrive at a prediction of decreased utilization under the fee-parity scenario, although it may be offset by an increase in the supply of services to Medicare patients by some physicians if Medicaid crowds-out private coverage for patients in private plans with reimbursement that exceed Medicare levels. These physicians would also decrease the total quantity of services they provide. See discussions in McInerney et al. (2017) and Garthwaite (2012).

A. *Implications for the ACA Medicaid Expansions*

The predictions from this model are directly relevant to the policy environment surrounding the ACA Medicaid eligibility expansions. To address concerns about provider willingness to participate in the expansions, the ACA required states to raise Medicaid primary care payment rates to Medicare levels in 2013 and 2014, referred to as the "Medicaid primary care fee bump." Therefore, fee parity was in effect for primary care services during the first year of Medicaid expansion in 2014.[9]

Because of the magnitude of the Medicaid expansions, it is reasonable to expect a change in providers' marginal patient as suggested by the model. First, as described in the Introduction, the ACA expansions were associated with economically-meaningful increases in service demand from the newly insured (Ghosh et al., 2017; Miller and Wherry, 2017; Simon et al., 2017; Sommers et al., 2015; Wherry and Miller, 2016). In addition, the ACA expansions were as large as, if not larger than, the earlier expansions studied in related empirical work (Glied and Hong, 2018; McInerney et al., 2017). We examine in subanalyses four situations where the demand shock for providers was especially large. We examine zip codes where the ACA expansions were particularly large, as well as geographic areas where the health care industry has little or no excess capacity. We also examine dually-eligible beneficiaries, who are more likely to share providers with the low-income adults gaining coverage under the expansion. Finally, we consider a subset of states where the change in eligibility due to the expansion was particularly large.

However, there are also many ways in which provider behavior may differ from what is predicted here. Providers may serve Medicaid patients out of a charity motive (e.g. Glied and Hong, 2018), or be unable to perfectly implement the market segmentation described in the model. In that case, the Medicaid expansion may crowd out Medicare beneficiaries' utilization of all services, not just services with fee parity. On the other hand, established patient relationships may lead providers to favor Medicare patients even when the reimbursement rates are equal. To address this concern, we examine new patient visits among Medicare beneficiaries; after the ACA expansions, Medicare beneficiaries seeking care from a new provider should have been directly in competition with newly-insured Medicaid beneficiaries.

Still, providers may have continued to prefer Medicare beneficiaries even for new patient visits if there were additional benefits not captured in the service reimbursement rate. One example would be higher administrative burdens associated with Medicaid reimbursement (e.g. Gottlieb et al., 2018b). Additionally, some physicians may have received bonus payments for services to Medicare patients under a primary care incentive program in effect between 2011 and 2015. Under this program, qualifying primary care physicians received a quarterly payment equal to 10 percent of Medicare's payments for a subset of primary care services. However, a national survey indicated that only half of primary care physicians were aware of the program, with just one-quarter reporting receiving a bonus payment (Andrews, 2015). In addition, Chen et al. (2018) find little evidence of physician

[9] The policy expired after 2014. At least fifteen states and the District of Columbia elected to extend the fee bump into 2015 and continue to pay increased primary care fees (Tollen, 2015). This means that 7 of the 29 expansion states still had this policy in effect during 2015, the second year of Medicaid expansion. Since fee parity was not fully in effect in 2015, in our robustness checks we re-estimate the models using data through 2014 only, the year in which we predict the negative spillovers would be largest.

response to the incentive program.

Furthermore, this model does not allow providers to consider the expected future revenue from different patients based on their characteristics. For instance, providers may not want to establish relationships that will become less lucrative after the end of Medicaid reimbursement parity. Or, providers may continue to favor Medicare patients if their expected health care needs (and therefore, expected revenue) surpass those of newly eligible Medicaid patients. For this reason, we also examine changes in utilization separately for healthy and sick Medicare beneficiaries.

Finally, it is possible that the provider supply curves are flat enough to fully accommodate the increase in demand. If this were true, we would expect to see an increase in physician hours after Medicaid expansion or other practice changes; we are unaware of any evidence on this question in the context of the ACA Medicaid expansions. One possible change in practice behavior that could accommodate the demand increase is an increase in the use of mid-level providers, such as physician assistants or nurse practitioners, to provide primary care services. We are able to test for this empirically by examining whether there is an increase in Medicare visits with any services provided by non-MDs.

III. Data

A. Coverage Impact of Medicaid Expansions

We first replicate results already documented in several papers to establish that states that expanded Medicaid eligibility through the ACA experienced larger increases in insurance coverage than states that did not.[10] To do so, we rely on the American Community Survey (ACS) for the years 2008-2015 (U.S. Census Bureau, 2018). We use the restricted-use version, which contains approximately 50 percent more sample than the public-use version and gives us about 2.7 million non-elderly adults per year, or about 22 million respondents total.[11] We apply the relevant survey weights in our analysis and use survey years 2008 through 2015 except where noted.

We assess insurance coverage changes and, later, Medicare utilization in two geographic subsamples. The first subsample targets areas where the insurance expansions were likely the largest. Using the public-use 5-year ACS file for 2008-2012, we order zip codes by the fraction of the population that is both low income (in households with incomes under 138 percent of the Federal Poverty Level) and uninsured, and define the top quartile as "high impact" zip codes (U.S. Census Bureau, 2016). The 1-year ACS only includes zip codes beginning in 2012, so we assess insurance coverage for this subsample over the years 2012-2015. The second geographic subsample targets areas where the health care industry is thought to be operating at capacity or experiencing a supply shortfall. We define these areas using counties designated in 2010 as Health Professional Shortage Areas (HPSA) for primary care practitioners by the U.S. Health Resources and Services Administration[12]

[10]Buchmueller et al. (2017), Simon et al. (2017), Sommers et al. (2015), Miller and Wherry (2017), Wherry and Miller (2016), Miller et al. (2019), and others, all establish this fact.

[11]Census disclosure restrictions require that we round sample sizes to the nearest 10,000.

[12]2010 HPSA designations for each county are drawn from the 2015-2016 Area Health Resource File. We consider a county to have HPSA designation if the entire county was indicated to be a geographically defined HPSA.

(Health Resources and Services Administration, 2016).

B. *Utilization of Medicare Beneficiaries*

We measure the health care utilization of Medicare beneficiaries from administrative Medicare claims data from the Centers for Medicare and Medicaid Services (CMS) for a 20 percent random sample of enrollees observed from 2008 to 2015 (Center for Medicare and Medicaid Services, 2016).

Our primary outcome is the utilization of primary care services, which is derived from the Part B (Carrier) file that records the provider and exact services associated with each claim using Healthcare Common Procedure Coding System (HCPCS) codes. We characterize a service as "primary care" if it experienced increased reimbursement in Medicaid in 2013 and 2014.[13] These services are principally Evaluation and Management services, which are usually rendered in an office visit but can also include physician services rendered in a hospital. Our first measure is the total work-related Relative Value Units (RVUs) of primary care services received by each beneficiary each year. Each HCPCS code has a corresponding RVU value which we calculate using the physician fee schedule relative value files (Centers for Medicare and Medicaid Services, 2016b). RVUs are the basis for Medicare billing and are designed to capture the resources that a service is expected to use; services for more complex patients that take longer are assigned more RVUs. If providers in expansion states spend less time with Medicare beneficiaries at each visit, this measure should capture that reduction in care.[14] Our next measure is the number of primary care visits, which we measure as the number of Part B claims with primary care services. This measure should capture whether Medicare beneficiaries change their number of encounters with primary care providers.

We also examine visits with reimbursement codes for new patient evaluation (HCPCS codes 99201-99205), a subset of primary care services. Providers operating at or near capacity may prioritize existing patient relationships; thus, Medicare beneficiaries may be more directly in competition with the newly-insured for new patient visits. In 2010, Medicare ceased payment for certain HCPCS codes ("consultation codes") that previously included new patient visits; this policy change results in a doubling of the observed number of new patient visits identified using the HCPCS codes for new patient evaluation from 2009 to 2010. We thus exclude 2008 and 2009 when analyzing this outcome to ensure a uniform variable definition.

We also record the total number of physician services without requiring those services to

[13]The affected codes in Medicare are HCPCS codes 99201-99499, 90460, 90461, 90471, 90472, and 90473 (Kaiser Commission on Medicaid and the Uninsured, 2012). The increased reimbursement applies only to qualifying providers, where qualification is based on specialty or on the affected codes accounting for more than 60% of the provider's Medicaid reimbursements. Since we do not have information on Medicaid claims, we are unable to know whether providers who do not qualify via specialty meet the claims threshold. In Medicare, if we define "qualifying provider" based on these codes accounting for at least 60% of the provider's *Medicare* reimbursements, the vast majority (82%) of affected services are provided by qualifying providers. For this reason, we examine the utilization of all primary care services. However, we also consider the subset of primary care services provided by this definition of qualifying providers as a robustness check in Appendix Figure A11; results are very similar.

[14]This interpretation assumes that there is no change in strategic billing by physicians in response to the Medicaid expansion. While, we cannot rule out this possibility, we note that providers are subject to audits to determine the accuracy of their Evaluation and Management coding.

be related to primary care.[15] Since the use of mid-level providers is a hypothesized pathway for supply expansion, we also examine the total number of services billed by non-MD providers (primarily nurse practitioners or physician assistants).[16] In this administrative dataset, we do not observe the amount of time an individual waited for an appointment since requesting it. Instead, we use the number of days between observed physician services as a proxy for wait times. Finally, we examine spending, both to facilitate comparisons with previous work and to detect the presence of any downstream impacts of reduced access to primary care. Our first measure is overall expenditure on all services (inpatient, outpatient, and office-based; Medicare portion and cost-sharing); our second is expenditure on physician (i.e. Part B) services (Medicare portion and cost-sharing). Both measures are converted to real 2015 dollars using the Medical Services Consumer Price Index.

These outcomes are available only for Medicare beneficiaries enrolled in traditional fee-for-service (FFS) Medicare; we do not observe utilization information among those enrolled in Medicare Advantage (MA). Medicare Advantage penetration rates differ dramatically across states, and are slightly higher (27%) in Medicaid expansion states than in non-expansion states (25%). Appendix Figure A9 assesses the presence of a differential trend in Medicare Advantage enrollment between expansion and non-expansion states prior to the ACA. We find that MA enrollment is falling slightly in the expansion states in the pre-period,[17] potentially inducing compositional changes in our sample of fee-for-service enrollees. To avoid any influence on our estimates, we study a balanced panel of individuals who are present in the FFS sample for all eight years. The balanced panel is slightly older (1.5 years) than the unbalanced panel and underrepresents those in their mid-to-late 60s who join Medicare during the sample period.[18]

[15]We refer to each claim in the Carrier file as a "physician service," although in fact some claims may represent laboratory, ambulance, or ambulatory surgical services. Similar to our primary care physician measures, these services may also be rendered in the hospital setting.

[16]Medicare reimburses services provided by non-MDs in two ways: direct billing (at 85% of the physician payment rate) or "incident-to" billing. Non-MD services that are provided in certain circumstances may be billed "incident-to" using the physician's National Provider Identifier and qualify for the full rate. It is not possible to identify services that are provided by a non-MD if they are billed "incident-to"(Buerhaus et al., 2015; DesRoches et al., 2013; Perloff et al., 2016). Thus, our measure reflects only non-MD services that are directly billed. Because a non-MD must direct bill services under certain circumstances (e.g., any new medical issue that a patient brings up), we still expect our measure to capture greater utilization of non-MD services.

[17]The post-period coefficients among the dually eligible are the most concerning due to their large magnitude. This increase is likely due to automatic enrollment in combined Medicaid-Medicare managed care plans in nine states under a demonstration program called the Financial Alignment Initiative implemented under the ACA. Enrollment in such plans is indistinguishable from Medicare Advantage enrollment in our data. The states that implemented this program include four (CA, IL, MI, and OH) that expanded Medicaid, three (SC, TX, and VA) which did not, and two (MA and NY) that we exclude from our sample because of pre-existing high Medicaid eligibility. The differential enrollment in MA disappears if we drop these states.

[18]In results available upon request, we examined utilization among repeated cross-sections of 67-69 year old FFS beneficiaries in the years 2008-2015. We did not include 65 and 66 year olds in case those individuals enter Medicare at different levels of health due to exposure to ACA coverage at ages before 65. In this population we generally find that the Medicaid expansion did not affect the utilization of Medicare beneficiaries, although in a few cases the analysis is inconclusive due to non-parallel trends in the pre-period.

C. Subsample Analyses

In addition to examining outcomes for the full Medicare FFS population, we also consider several subsamples that may be more likely to experience spillover effects from the Medicaid expansions. For these subsample analyses, we similarly require that the beneficiaries are in the defined subgroup for all eight years of the 2008-2015 period in order to be included in the analysis.

The first such sample we consider is beneficiaries who are dually eligible for Medicare and Medicaid, primarily low-income seniors and non-elderly adults with disabilities. This population receives assistance from Medicaid to help pay for premiums and cost-sharing for Medicare covered services. Given their low incomes, they may be more likely to be affected by the ACA Medicaid expansions if they share neighborhoods and providers with the low-income adults gaining Medicaid coverage.

Next, we hypothesize that the increase in Medicaid coverage was particularly high in "high-impact" zip codes with high rates of low-income uninsured individuals before the ACA. Since spillovers would be expected to be largest where the demand shock from the newly insured is largest, we assess the utilization of Medicare beneficiaries who reside in these zip codes. In addition, the health systems in counties with a HPSA designation may be particularly unable to expand supply. Medicare beneficiaries in such counties constitute another geographic subsample.

Finally, we divide the Medicare population into sick and healthy subsamples based on the highest Charlson Comorbidity Index recorded for each individual in the pre-period (since changes in utilization during the post period could change the presence of ICD codes that underlie the Charlson Index). This index is based on the number of health conditions recorded in an individual's medical claims, with higher values indicating worse health Charlson et al. (1987). Individuals with a Charlson Index of at most one during 2008-2013 are categorized as "healthy"; if supply-constrained providers can "triage" their patients, utilization reductions may be most common among this population. On the other hand, "sick" beneficiaries (those with a Charlson index of at least two in any pre-period year) consume a disproportionate quantity of Medicare services and so may bear the brunt of any service reductions. The distribution of the highest Charlson Index in the pre-period is similar in expansion and non-expansion states and is reported in Appendix Figure A10.

Figure 2 describes our analytical samples in more detail. Panel A shows the number of observations (person-years) of the full balanced sample and then each subsequent sample for expansion and non-expansion states. Recall that each subsample is also balanced so the number of individuals is the same in the pre- and post-periods. Our full sample contains more than 29 million person-years. All samples are well represented in both expansion and non-expansion states, with the high-impact zip codes somewhat more prevalent in the non-expansion states and the HPSAs more concentrated in expansion states. Panels B-D report the levels of our key outcomes for each subsample by expansion and non-expansion status across the pre- and post-periods. Utilization generally climbs between the pre- and post-periods, which is expected given the aging of our fixed panel. A typical Medicare beneficiary has 9 primary care visits at which they receive 14 RVUs of primary care during the pre-period. However, dually-eligible beneficiaries and beneficiaries with 2 or more chronic diseases have 2-3 more primary care visits on average. This demonstrates that these classifications indeed capture more clinically vulnerable subgroups with a higher demand for services. In contrast, we see lower usage of primary care services for beneficiaries living in high-impact zip codes or HPSA counties, which may reflect supply-side constraints.

IV. Empirical Model

A. Medicaid Expansion

To document the impact of the Medicaid expansions on the use of care among Medicare beneficiaries, we estimate a difference-in-differences model. This approach better fits our context than the "simulated eligibility" methods used in other papers in this area since we look at expansions that occurred in states with a clearly defined before and after period, rather than gradual expansions rolled out over time. Comparing expansion and non-expansion states lets us evaluate whether pre-expansion trends differed across the expansion and non-expansion states.

We use the below event-study model to assess the evolution of outcomes in expansion and non-expansion states both before and after expansion.

$$(1) \qquad Y_{ist} = \beta_s + \beta_t + Expansion_s \times \left[\sum_{y=-6}^{-2} \beta_y I(t - t_s^* = y) + \sum_{y=0}^{1} \beta_y I(t - t_s^* = y) \right] + \epsilon_{ist}.$$

Here, β_s denotes state fixed effects and β_t denotes year fixed effects. The variable $Expansion_s$ equals 1 if individual i is living in one of the 24 states that opted to expand Medicaid eligibility in 2014 or 2015, and zero otherwise.[19] Four states and D.C. that had previous expansions of Medicaid, or similar coverage, for adults with incomes up to 100% FPL before 2014 are excluded from the analysis.[20] We would expect less of a demand shock in these states with generous pre-ACA Medicaid programs and, by excluding them, aim to increase our ability to detect negative spillovers under the ACA expansions. The remaining 21 states that did not expand Medicaid in 2014 or 2015 have values of zero for the $Expansion_s$ variable. For the expansion states, indicator variables $I(t - t_s^* = y)$ measure the time relative to the implementation year, t_s^*, of the expansion in each state. We group together $y = -6$ and $y = -7$ since only the 3 states expanding in 2015 have a nonzero value for $y = -7$. The standard errors are clustered at the state level.

Our parameters of interest are estimates of β_y that show how outcomes evolved in states

[19] In our analyses, states that expanded Medicaid in 2014 are AR, AZ, CA, CO, CT, HI, IL, IA, KY, MD, MI, MN, NJ, NM, NV, ND, OH, OR, RI, WA, and WV. Michigan implemented their expansion in April 2014 with the remainder of states expanding in January 2014. States that we consider to have 2015 expansions are NH (implemented August 15, 2014), PA (January 1, 2015), and IN (February 1, 2015). We do not consider AK to be a 2015 expander state given its late implementation date (September 1, 2015).

[20] We excluded only those states that extended coverage to childless adults with incomes at least as high as 100% FPL, with benefit packages that were comparable to Medicaid, and with open enrollment during each year of the pre-period. Four states (DE, MA, NY, VT) met these criteria and were excluded from the analysis. We also excluded DC, which took up the new ACA option to cover low-income adults in 2010. Our results are unchanged if we instead include these five states. CA, CT, and MN were also early implementers of the ACA expansions but did not fully expand eligibility to 138% FPL until January 1, 2014. See appendix of Miller and Wherry (2017) for additional details and source information.

that opted to expand Medicaid relative to those that did not, with the year immediately prior to expansion as the reference year. If coefficients on years prior to the Medicaid expansion (i.e., β_{-6} through β_{-2}), are close to zero and not statistically significant, this provides support for the parallel trends assumption under our difference-in-differences estimation of the impact of the ACA. This indicates that, before the ACA, expansion and non-expansion states experienced similar trends in outcomes. Any estimated divergence in outcomes between the two groups of states during the post period (β_0 and β_1) are then attributed to the policy change.

We next estimate a difference-in-differences (DID) model that examines the average change in outcomes following expansion in the expansion states compared to the nonexpansion states.

$$Y_{ist} = \beta_s + \beta_t + \beta_0 Expansion_s \times I(t - t_s^* \geq 0) + \epsilon_{ist}. \tag{2}$$

This specification differs from the event study analysis in that it estimates the average change in outcomes during the entire post-period compared to the entire pre-period in the expansion states compared to the non-expansion states, rather than estimating changes relative to the year immediately prior to implementation.[21]

B. Additional Analyses

In addition to the main analyses described above, we conduct several additional analyses to examine the robustness of our findings to alternative methods, samples, or measures of negative spillovers.

First, for at least two outcomes, we find that the pre-period trends were meaningfully different in expansion and non-expansion states prior to the ACA expansions under our event study model. For this reason, and as an additional check on our findings under the DID model, we conduct a separate analysis using the synthetic control method described in Abadie et al. (2010). This approach forms a control unit by weighting the non-expansion states to closely resemble the expansion states in the pre-period, and then attributes any divergence in outcomes after implementation to the impact of the expansions. This method has been used in many settings, including the study of the ACA Medicaid expansions (Hu et al., 2018). Following previous studies, we group all expansion states into one aggregate treated unit. This method requires a common "pre-intervention" time period, so we restrict this analysis to the 21 states that expanded by April of 2014, following Hu et al. (2018). By weighting comparison units to match the treated unit, this approach aims to minimize differing pre-trends. Details on this approach are found in Appendix Section B.

Next, we examine the sensitivity of our findings to different policy parameters. As described in Section II, Medicaid primary care reimbursements were set to Medicare levels in 2013 and 2014, but not all expansion states opted to extend this parity reimbursement policy through 2015. Our conceptual model predicts that negative spillovers are less likely to occur if Medicare reimbursement rates exceed those of Medicaid. For this reason, we have re-run our analyses evaluating spillovers through the first year of Medicaid expansion

[21]Recent work by Goodman-Bacon (2018) suggests that DID models can be misleading if the treatment timing varies across states and treatment changes the slope of the outcome variable. However, we demonstrate in Appendix Section C that this is not a major concern in our setting.

only (i.e. 2014), when Medicare and Medicaid reimbursement rates were equal for primary care services in all states. In addition, there is heterogeneity across states in terms of how the fee bump policy (in all years, 2013 through 2015) affected the dually eligible in practice, depending on pre-existing state reimbursement policies for this population. Detailed information on these pre-existing state policies for duals and the implied changes under the ACA fee bump policy is difficult to locate.[22] However, if these changes are correlated with state expansion status, this might affect our findings. For this reason, in Section V.C we also re-run the analyses excluding duals from each of the sample populations.

We next examine whether there are spillovers for Medicare beneficiaries in the states with the largest Medicaid expansions. The increase in eligibility for Medicaid associated with the expansions varies across states based on what eligibility criteria were in place prior to the ACA. It may be possible that Medicare beneficiaries in the most affected states experienced negative spillovers, even though there were no spillovers associated with the expansions overall. We used information on pre-ACA eligibility rules from the Kaiser Family Foundation and data from the Current Population Survey to determine how large of an eligibility increase would be experienced in each state using a nationally representative sample of individuals. We then dropped expansion states whose eligibility changes were less than 10 percentage points.[23] We also excluded Wisconsin from the pool of non-expansion states, because it enacted an ACA expansion "look alike" program at the same time the ACA expansions were implemented. This left us with a pool of expansion states that experienced eligibility changes of, on average, 23 percentage points, and a pool of non-expansion states that experienced eligibility changes of, on average, 2.5 percentage points from 2013 to 2014.

Other studies have shown that individuals gaining health insurance coverage can have elevated short-run utilization due to pent-up demand for care (e.g. Lo et al., 2014; Miller and Wherry, 2017). To examine whether spillovers are more intense right after expansion or, alternatively, grow over time as more individuals take-up coverage, we examine changes in primary care utilization over half-year, rather than annual, intervals in additional analyses.

Finally, because our administrative data do not have direct measures of wait times for care, we also rely on survey data to examine reports of delays in care associated with the Medicaid expansion. We use the restricted-use version of the 2008-2015 waves of the National Health Interview Survey (NHIS), which contains state of residence information (National Center for Health Statistics, 2016).[24] In each year, the NHIS questions a single,

[22]Prior to the increase, providers in some states already received Medicare-level reimbursement for dually-eligible beneficiaries, obtaining 80% of allowed charges from Medicare and 20% patient cost-sharing from the state Medicaid program. However, many states did not pay the full 20% cost-sharing for the dually eligible, or erected other administrative barriers to receiving the full payment (Medicaid and CHIP Payment and Access Commission, 2015). Thus, the onset of the fee bump in 2013 had potentially varying impacts for the dually eligible by state.

[23]This led us to exclude CA, CT, HI, IA, IN, MD, and MN. For comparison's sake, Glied and Hong (2018) and McInerney et al. (2017) both discuss large Medicaid expansions as states with eligibility changes of greater than 5 percentage points.

[24]Although designed to be nationally representative, the NHIS sample is drawn from each state and D.C. and, by pooling years of data, may be used to construct precise state-level estimates for most states (Centers for Disease Control and Prevention, 2019). In our context, this is unlikely to be a concern since we are pooling states and years to estimate the average effect of Medicaid expansion for a group of 25 states.

randomly-chosen adult from each surveyed household about delays in care due to long wait times. We use the responses among those 65 or older as an alternative measure of negative spillovers to Medicare beneficiaries.

V. Results

A. Medicaid Expansion States had Significantly Larger Coverage Increases

We first show that states that expanded Medicaid eligibility through the ACA experienced larger increases in insurance coverage than states that did not; as a result, it is reasonable to believe that Medicare beneficiaries that lived in these states were at higher risk of being crowded out of medical care.

Figure 3 displays the event study coefficient estimates of Equation (1). Coverage in non-expansion states and expansion states evolved similarly during the years prior to the expansions but diverged starting in the first year of implementation, with increases in Medicaid coverage and decreases in uninsurance in expansion states relative to non-expansion states.

The difference-in-differences coefficients are reported in Table 1. We find a statistically significant increase in Medicaid enrollment in expansion relative to non-expansion states of 4.2 percentage points. The increase in total insurance coverage is more modest, at 2.5 percentage points. Among individuals living in the high-impact zip codes (Panel B), the estimated effects of the expansions are considerably larger as we expected: we observe an increase in Medicaid coverage of 10.7 percentage points and in insurance coverage of 9 percentage points. Among those living in HPSA counties (Panel C), we see increases in Medicaid enrollment of 4.9 percentage points and decreases in uninsurance of 3.1 percentage points. Thus HPSA counties have about-average ACA coverage expansions, but are expected to have supply constraints that might make negative spillovers for Medicare beneficiaries more likely.

According to existing results in the literature, insurance coverage changes of this size could potentially result in large spillovers.[25] For example, McInerney et al. (2017) find that a one percentage point increase in Medicaid eligibility for nonelderly adults reduces average spending by 0.7 percent for all beneficiaries and 3.3 percent among the dually eligible. Assuming the take-up rate is approximately 70 percent (Buettgens and Kenney, 2016), this implies that a 4.2 percentage point increase in Medicaid coverage would result in a decrease in spending of approximately 4.2 percent overall and by almost 20 percent among dually-eligible beneficiaries. The results in Glied and Hong (2018) imply that the 2.5 percentage point increase in insurance coverage that we observe in expansion states would result in a 6.5 percent reduction in physician services spending among Medicare beneficiaries. If we extrapolate these estimates to changes in coverage observed in sub-state geographies, we could expect an over 23 percent decrease in physician services spending for beneficiaries in high-impact zip codes, where we observe a 9 percent increase in insurance

[25] The measures of spending and the populations studied mentioned in this paragraph differ slightly from those used in our analysis. However, we do not expect that these differences would lead one to anticipate different effects in our context. Please see Appendix Section A for detailed information on the comparison between these studies and our own.

coverage. In the next section, we directly test for such impacts on the utilization of Medicare beneficiaries.

B. Utilization in Expansion and Non-Expansion States

Primary Care Services. — In the previous section, we showed that Medicaid expansion states had significantly larger coverage increases compared to non-expansion states. Despite these coverage increases, we show in this section that Medicare beneficiaries' utilization of primary care services was not differentially affected in expansion states. We focus on primary care services because at the time of the Medicaid expansion there was parity in reimbursements for these services between the Medicare and Medicaid programs, which implies a reduction in utilization among Medicare beneficiaries in our theoretical model. We first present event studies validating the parallel trends assumption of a DID model, and then show a formal pre-post DID coefficient.

Our main results are demonstrated in Figure 4, which reports the event study coefficients for all (Panel A) and several subsamples of Medicare beneficiaries for work-related RVUs for primary care. To facilitate comparisons, we use the same y-axis scaling for every sample, and a gray bar represents an increase or decrease of five percent of the mean of that outcome for the sample. As may be seen in the Figure, the utilization of primary care services among Medicare beneficiaries was similar in expansion and non-expansion states prior to the ACA Medicaid expansions and exhibited no divergence after the expansions were implemented. This finding holds in all six samples considered. Not only are post-expansion event study coefficients not statistically significant, but they are very small in magnitude.

In Panel A of Table 2, we report the corresponding DID coefficients. We find no statistically significant evidence of reductions in primary care RVUs associated with Medicaid expansion for any of the samples. All coefficients are small relative to the mean of the outcome; for example, our point estimate for the impact on the number of PCP RVUs for all Medicare beneficiaries represents a 0.5 percent increase from the mean of 15.09 PCP RVUs per year. The largest point estimate we find, a reduction of 0.27 RVUs per year for patients living in high-impact zip codes, is equivalent to less than one-fifth of the RVUs required for a single 30-minute new patient office visit (which is paid at 1.42 RVUs) and represents only a 1.8 percent decline relative to the sample mean. Moreover, our coefficients are precisely estimated. Evaluating the lower bound of the 95 percent CI relative to the mean, we are able to rule out a decline in RVUs of more than 4.9 percent in the full sample. Thus, despite the differential exposure of Medicare beneficiaries to the largest coverage expansion since the introduction of Medicare and Medicaid, we can reject all but very modest reductions in the utilization of these beneficiaries.

Figures 5 and 6 present the results for the number of PCP visits and the number of new patient visits. The number of visits is also an important measure of the quantity of care supplied, although it does not capture some important characteristics of care, such as the length of the visit, which are measured implicitly with RVUs. There is some suggestion of a slight decrease in the number of PCP visits for the dually eligible and high-impact zip code subsamples in the first year of expansion, which could be the result of a small amount of pent-up demand among the expansion population. The estimates are only marginally significant at the 10 percent level, however, and are not significant during the second expansion year. Importantly, the size of the reductions observed in the first year after expansion are extremely small, representing less than a 1.4 percent reduction relative to the mean number of primary care visits in these groups. We do not see any evidence suggestive of a change in new patient visits.

Panels B and C in Table 2 repeat the DID analysis for these two other measures of primary care service utilization. The DID estimates are extremely small, representing less than 1 percent of the mean for all but one of the group-outcome combinations, and all estimates are statistically indistinguishable from zero. Overall, it appears that Medicare beneficiaries have no problem obtaining primary care and new patient visits despite a likely increase in the demand for new patient visits among the Medicaid expansion population.

Other Services. — Table 3 applies the same research design to a broader set of outcomes. The corresponding event studies (in Appendix Figure A12-A14), are generally supportive of the parallel trends assumption for the DID design. In Panel A, we demonstrate that the Medicaid expansion was not associated with changes in the total number of physician services among Medicare beneficiaries. Our point estimates suggest virtually no effect, and the standard errors are small enough to rule out economically meaningful impacts. The lower bound on our confidence interval for the dually eligible rejects decreases larger than 0.7 in the number of physician services, or a 2.7 percent decline for this subgroup. In contrast, the estimates reported in McInerney et al. (2017) predict that a Medicaid expansion the size of the ACA expansion would result in a reduction in medical encounters of approximately 18.6 percent among duals.

If providers' schedules are crowded due to the newly insured, Medicare beneficiaries may be asked to wait longer between physician services. The point estimates in Panel B of Table 3 suggest *decreases* in this measure for all samples other than the dually eligible, rather than increases that would suggest impediments to care. None of the estimates is statistically significant. Under the 95% confidence intervals, the largest possible increase in the days between physician services across samples is 1.4 percent among the dually eligible, from 22.8 to 23.3 days.

Panel C of Table 3 reports the effects of expansion on annual expenditures on physician services among Medicare beneficiares. As with other outcomes, confidence intervals around our estimates are small and always include zero. The lower bound of our confidence intervals rules out reductions in physician service spending that range from 1.4 percent (sick) to 3.8 percent (healthy) of the mean. We may compare these estimates to the predicted decrease in physician service spending of 6.5 percent for Medicare beneficiaries overall, and the 23 percent decrease for beneficiaries in high-impact zip codes described earlier based on estimates from Glied and Hong (2018).

There are two outcomes of potential policy interest where our event studies suggest significant differences in trends between the expansion and non-expansion states. First, we measure the share of services among Medicare beneficiaries provided by a non-MD (i.e. mid-level) provider. As is apparent in Appendix Figure A15, there is a clear downward trend in this outcome in expansion states over our time period. Similarly, overall expenditures across all settings of care show a substantial upward trend in expansion states in Appendix Figure A15.[26] Since these event studies suggest the assumptions of the difference-in-differences model are not satisfied, we examine them using an alternative method in the next section.

C. *Alternative Specifications and Measures*

Synthetic Control Estimates. — We find little evidence of differential pre-trends in the event study analyses for most outcomes we consider. However, for at least two outcomes – the use of mid-level providers and overall expenditures – there is some visual evidence of differential pre-treatment trends for the expansion and non-expansion states. We, therefore, apply an alternative method that forms a synthetic control unit to resemble the expansion states during the pre-period, thereby avoiding differential pre-trends (Abadie et al., 2010). The results are reported and discussed in Appendix Section B. Consistent with the analysis presented in the previous section, we find no evidence of negative spillovers when we apply this method.

Excluding 2015. — Our theoretical model predicts reductions for services with reimbursement parity, thus motivating our focus on primary care services bumped to parity reimbursement in 2013 and 2014, and in some states in 2015. Since not all states extended the fee bump in 2015, we verify that our results hold for our key outcomes when we drop this year. Appendix Table A4 reports the DID coefficients for primary care RVUs, primary care visits, and new patient visits for this analysis. The null hypothesis of no effect is never rejected and, as in Table 2, we can reject all but economically-small estimates.

Excluding Dually Eligible. — As discussed in Section III, the increased reimbursement for primary care services may have affected utilization by the dually eligible in a manner that varies by state. We have no reason to expect that pre-fee bump polices, or the decision to extend the fee bump into 2015, are correlated with expansion status; in addition, we do not see any noticeable changes in 2013 relative to 2012, or 2015 relative to 2014 in the utilization of primary care services among duals (Figure 4, Panel B). Still, in this section we repeat our analysis of primary care utilization for each of our subsamples excluding duals.

Appendix Figures A17-A19 report the event studies for high-impact zip codes, HPSA counties, and the 0/1 and 2+ Charlson subsamples excluding duals. Non-duals comprise at least 71% of each of these subsamples, so we still have adequate sample size. The event studies do not suggest that the inclusion of the duals was masking a true spillover onto non-dually eligible Medicare beneficiaries. None of the difference-in-differences coefficients for these subsamples is significant at the 5% level (results available upon request).

States with the Largest Eligibility Gains. — We next examine whether there is evidence of negative spillovers in states with the largest expansions in Medicaid. Appendix Table A5 reports the difference-in-differences coefficients for primary care RVUs,

[26] This trend persists even after adding a time-varying county-level control for the Medicare hospital wage index. The reported event studies do not include this control.

primary care visits, and new patient visits when we examine only states with larger Medicaid expansions (defined as eligibility gains of more than 10 percentage points). Even among these most affected states, we detect no evidence of negative spillovers, our point estimates are small, and our confidence intervals allow us to reject even modest reductions in utilization.

Estimates by Half-year Intervals. — By aggregating primary care utilization to half-year intervals (rather than annual), we can look for evidence of dynamic treatment effects – either larger spillovers to Medicare at first due to pent-up demand or increases in spillovers to Medicare over time as more eligible individuals take-up Medicaid coverage. In Appendix Figures A20-A22, we find that the treatment effects are essentially the same throughout the post-period. Figure 3 showed a larger increase in Medicaid coverage in the second year after expansion, so the finding of similar increases throughout the post-period adds support to our main finding that there were no spillovers to Medicare beneficiaries.

Alternative Measures of Crowding. — We further supplement our analysis on the use of care with survey-based measures on wait times. Our measures come from the NHIS from 2008 to 2015. We limit the sample to individuals age 65 and older and exclude those in early expansion states, consistent with the rest of our analysis.

The alternative crowding measures are based on two questions in the NHIS related to delays in care. The first question asks if the respondent delayed care due to an inability to get an appointment. The second question asks if the respondent delayed care because the time in the waiting room was too long. Figure 7 plots the event study coefficients for these outcomes. There appear to be similar pre-trends across the expansion and non-expansion states prior to the implementation of the ACA Medicaid expansions, and no visible divergence following implementation. Table 4 shows the result from the DID model. We detect no effect of the ACA Medicaid expansions on these measures of crowding. This is consistent with our previous analysis documenting no change in the use of care among Medicare beneficiaries.

VI. Discussion

A robust literature shows that gaining Medicaid eligibility causes the newly-insured to use more care. However, less is known about *how* physicians provide this care, given that health care provider capacity is constrained in the short run. Visits for the newly-insured may come from providers displacing existing patients, working longer hours, changing practice styles to increase volume, or some combination of these three. Getting inside this "black box" of the physician production function is difficult, in part due to data limitations.

The possibility that providers might be generating visits for the newly insured by displacing existing patients has generated considerable concern among policymakers and is one rationale states have given for foregoing the ACA Medicaid expansions. In this paper, we show that a mixed economy model of provider supply predicts that Medicare patients *may* become displaced as the result of the ACA Medicaid expansions, demonstrating an economic reason underpinning the policy concern. We examine whether this type of negative spillover occurred in primary care utilization for Medicare beneficiaries, since the ACA established equal primary care reimbursement for Medicaid and Medicare beneficiaries. However, our empirical results show that no such displacement occurred and our confidence intervals are small enough to rule out any economically meaningful effects. These results hold across multiple subgroups and specifications. We therefore can rule out one policy-relevant hypothesis about how primary care providers adjusted their supply to serve the newly insured – it is clear that they did not displace Medicare patients.

We note that our results contrast with those reported in analyses of previous Medicaid expansions (Glied and Hong, 2018; McInerney et al., 2017), who find large effects of pre-ACA Medicaid eligibility expansions on the use of care among Medicare beneficiaries, including office-based medical care. Our analysis does not directly speak to why our results differ. However, we speculate that it may be due to the fact that the ACA was a highly salient, publicly-debated policy that was announced several years in advance. Providers may have therefore been better able to anticipate the demand shock resulting from the expansions and adjust their practice patterns in a way that resulted in minimal disruption for existing patients.

It is also possible that midlevel providers played an important role in meeting new Medicaid-induced demand. Evidence from the state of Michigan shows no effects of expansion on primary care appointment availability, even in provider shortage areas, but an increase in care by non-physician providers (Benitez et al., 2019). In addition, there was a significant increase in job ads for both physician and non-physician positions following the state's expansion (Benitez et al., 2019). The expanded use of non-physicians in primary care practices following the ACA has been elsewhere documented (e.g. Barnes et al., 2018). Notably, state scope of practice laws for nurse practitioners and physician assistants have also greatly expanded over time (Gadbois et al., 2015). These recent trends may be an additional reason our results differ from prior work examining pre-ACA state Medicaid expansions.

Another possibility is more reliance on administrative staff and patient support occupations, such as technicians, therapists, and aides, to help physicians and other health care professionals meet new demand. This would be consistent with growth in these types of health care occupations observed in Massachusetts following state reform (Staiger et al., 2011). Interviews with providers in that state also point to facility expansions and expanded practice hours as approaches taken to serve a greater number of patients (Ku et al., 2009). More work on the response of provider practice decisions to public health insurance expansions is an important topic for future research.

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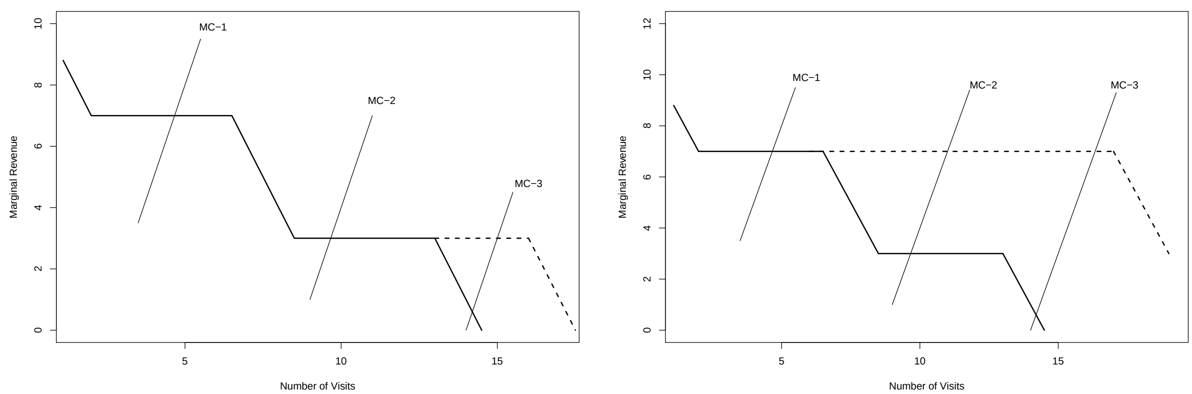
Figure 1. : Provider Response to Medicaid Expansions

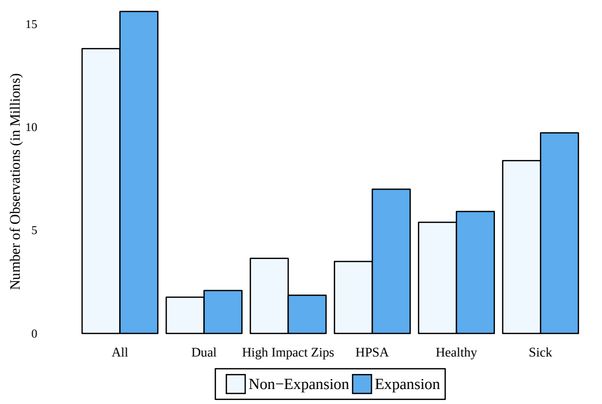


(A) Medicaid Expansion, No Fee Increase

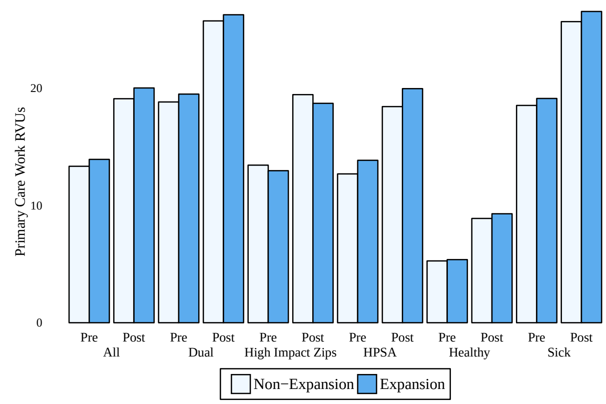
(B) Medicaid Expansion, Fee Parity

Note: Figure depicts marginal revenue curves faced by providers before (solid) and after (solid and dashed) Medicaid expansion in the case there is no fee increase (first panel) and with fee parity (second panel). The marginal cost curves for different types of providers are denoted by MC-1, MC-2, and MC-3.

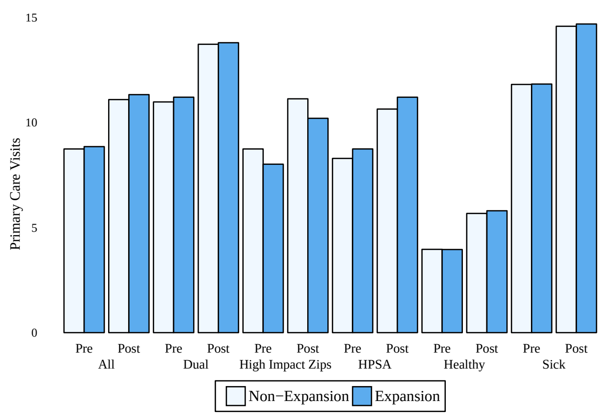
Figure 2. : Summary Statistics



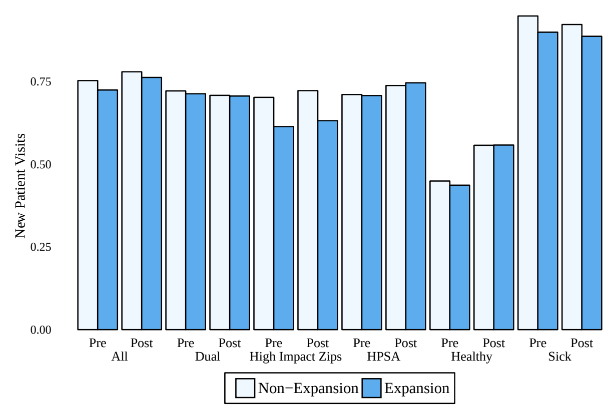
(A) Number of Observations in Sample and Each Subsamples



(B) Primary Care RVUs



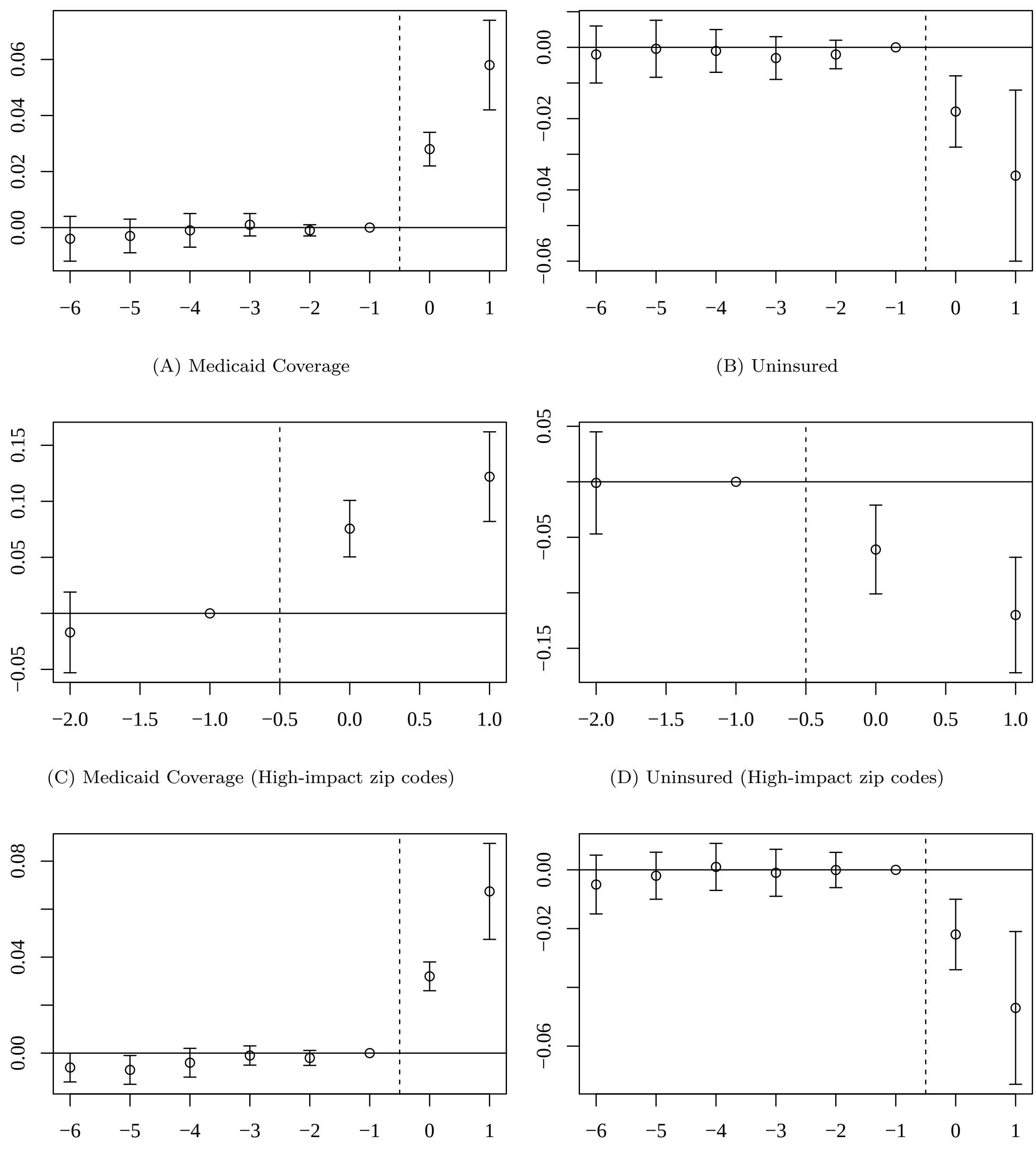
(C) Primary Care Visits



(D) New Visits

Note: Panel (A) reports the number of observations (person-years) in expansion and non-expansion states for our full sample (All) and five subsamples: dually eligible, those in high-impact zip codes (Low Inc Zips), those in health provider shortage areas (HPSA), those whose Charlson score in the pre-period never exceeds 0 or 1 (Healthy), and those whose Charlson score in the pre-period is at least 2 (Sick). Panels (B)-(D) report the means for key outcomes in expansion and non-expansion states for the full sample and six subsamples for the pre- and post-periods. For non-expansion states, the pre-period is defined as 2008-2013.

Figure 3. : Trends in Insurance Coverage in Expansion vs. Non-Expansion States, 2008-2015



(A) Medicaid Coverage

(B) Uninsured

(C) Medicaid Coverage (High-impact zip codes)

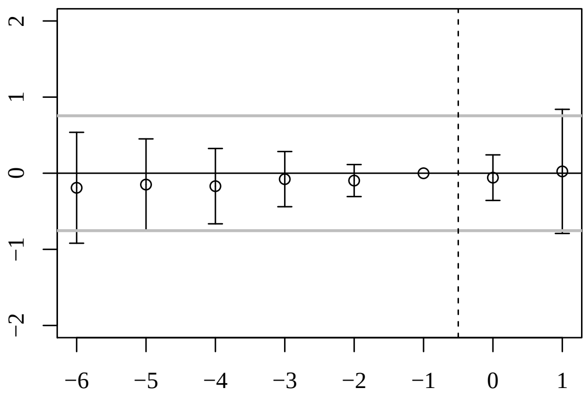
(D) Uninsured (High-impact zip codes)

(E) Medicaid Coverage (HPSA counties)

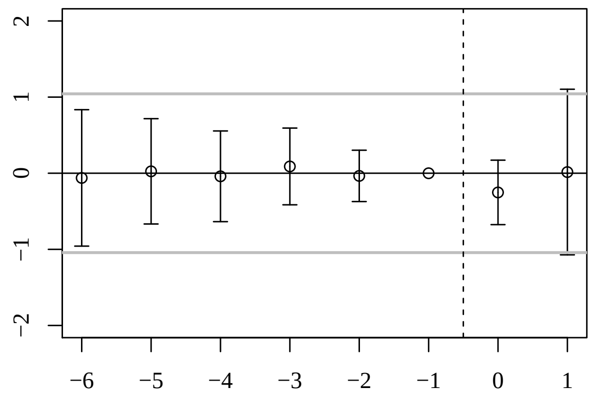
(F) Uninsured (HPSA counties)

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of insurance or Medicaid coverage in the American Community Survey, with the coefficients representing the deviation from the mean difference between expansion and non-expansion states in the six years before and two years after expansion. Panels C & D are limited to years 2012-2015 because zip codes only become available in the 1-year ACS in 2012.

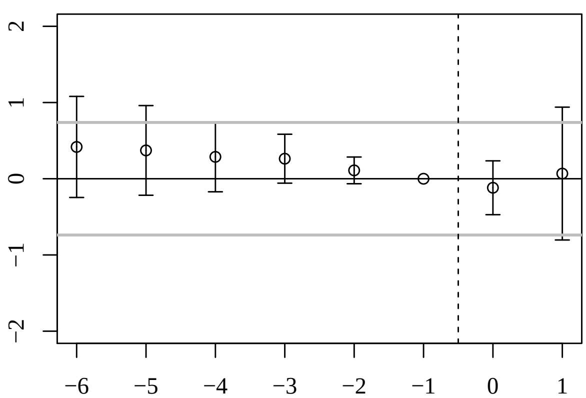
Figure 4. : Trends in Primary Care RVUs in Expansion vs. Non-Expansion States, 2008-2015



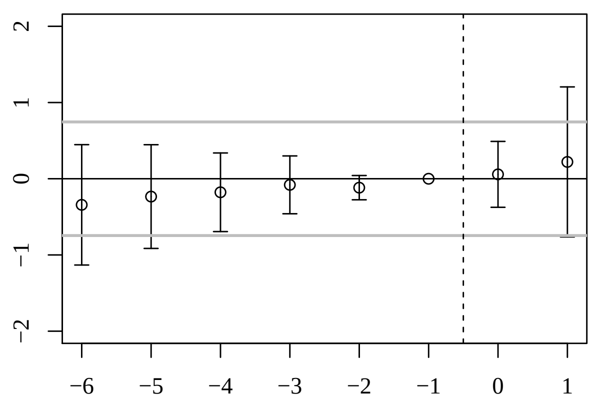
(A) All Medicare



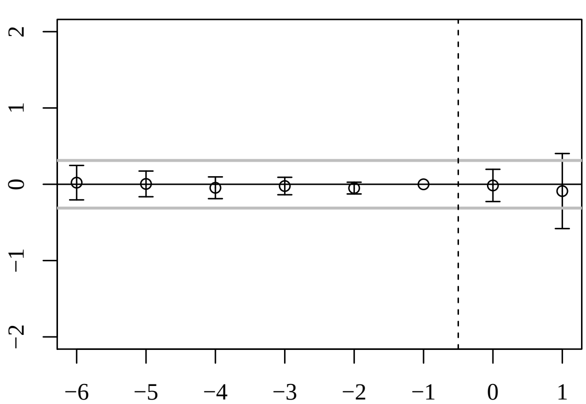
(B) Dually Eligible



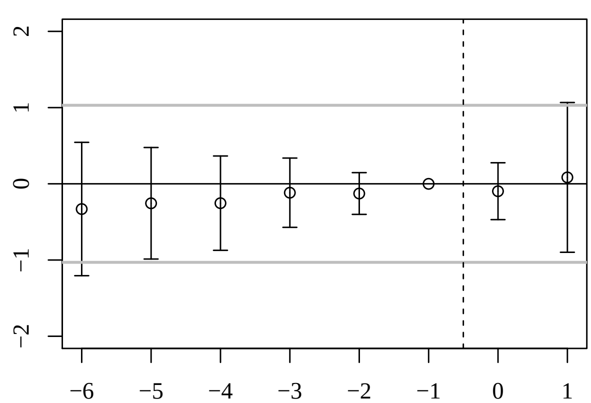
(C) High-impact zip codes



(D) HPSA counties



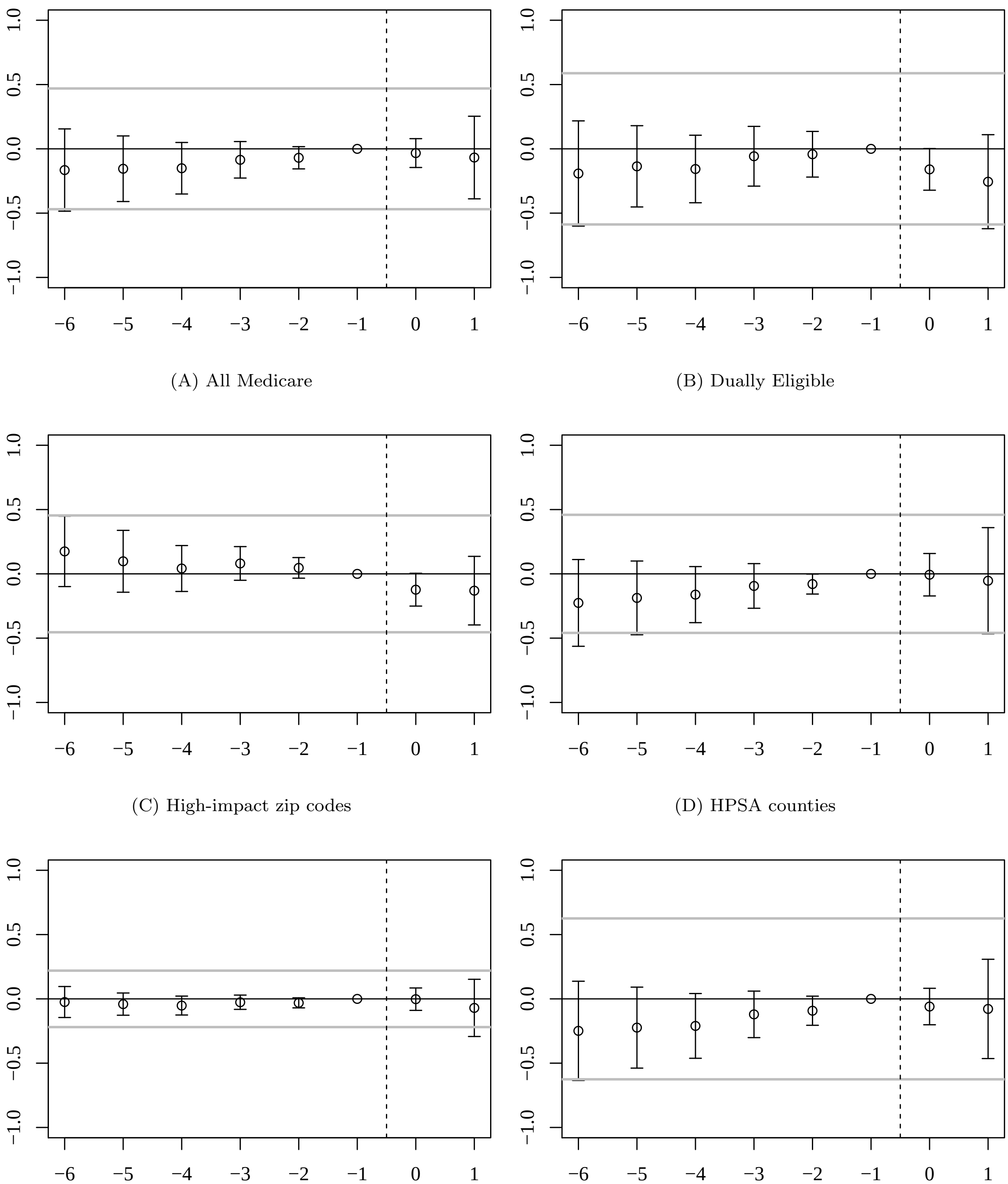
(E) Charlson score of 0 or 1



(F) Charlson score of 2 or more

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of primary care RVUs, with the coefficients representing the deviation from the mean difference between expansion and non-expansion states in the six years before and two years after expansion. The gray lines denote 5% of the mean for this subsample.

Figure 5. : Trends in Primary Care Visits in Expansion vs. Non-Expansion States, 2008-2015



(A) All Medicare

(B) Dually Eligible

(C) High-impact zip codes

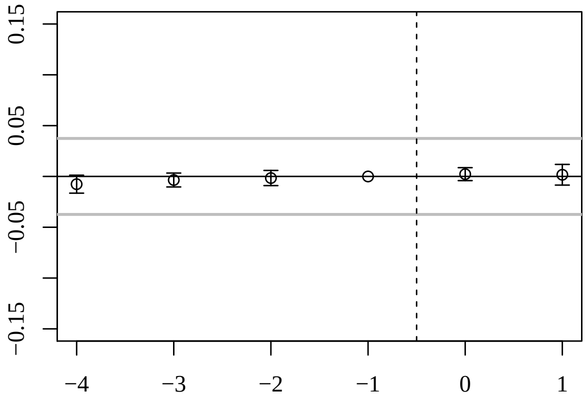
(D) HPSA counties

(E) Charlson score of 0 or 1

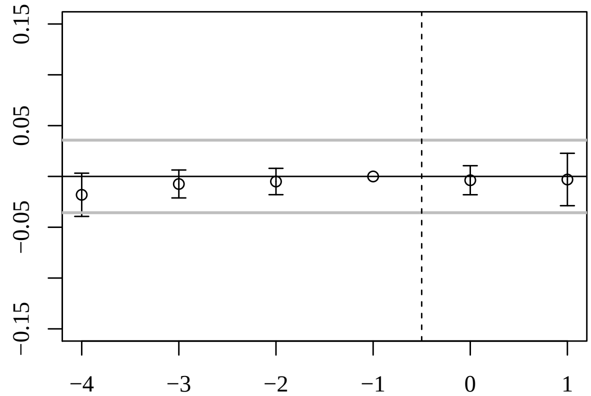
(F) Charlson score of 2 or more

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of primary care visits, with the coefficients representing the deviation from the mean difference between expansion and non-expansion states in the six years before and two years after expansion. The gray lines denote 5% of the mean for this subsample.

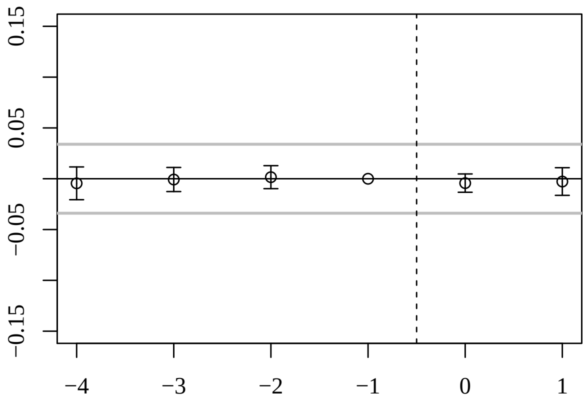
Figure 6. : Trends in New Patient Visits in Expansion vs. Non-Expansion States, 2008-2015



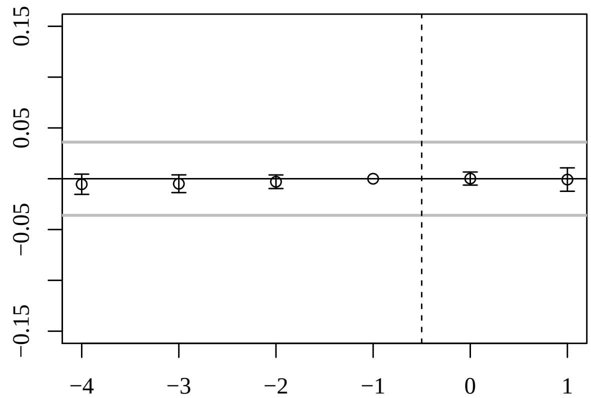
(A) All Medicare



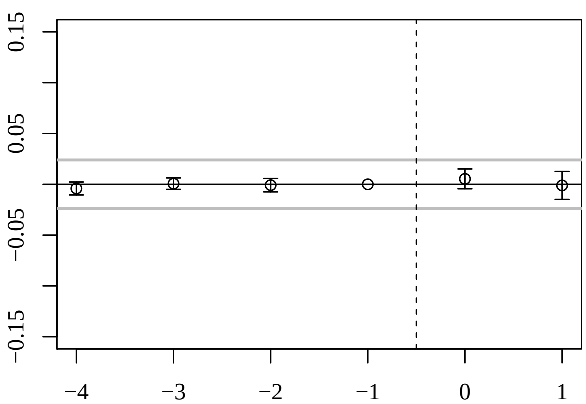
(B) Dually Eligible



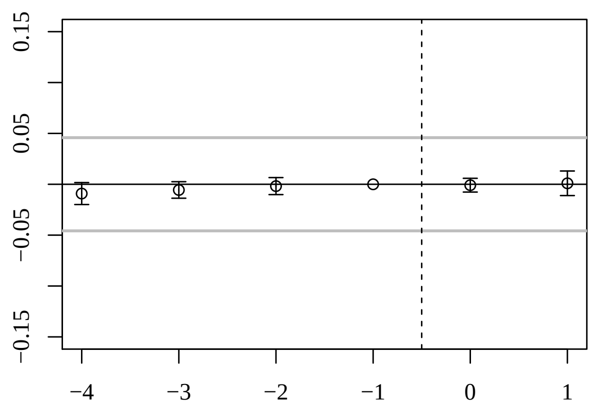
(C) High-impact zip codes



(D) HPSA counties



(E) Charlson score of 0 or 1



(F) Charlson score of 2 or more

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of new patient visits, with the coefficients representing the deviation from the mean difference between expansion and non-expansion states in the four years before and two years after expansion. The gray lines denote 5% of the mean for this subsample.

Table 1—: Difference-in-Differences Results: Health Insurance Coverage

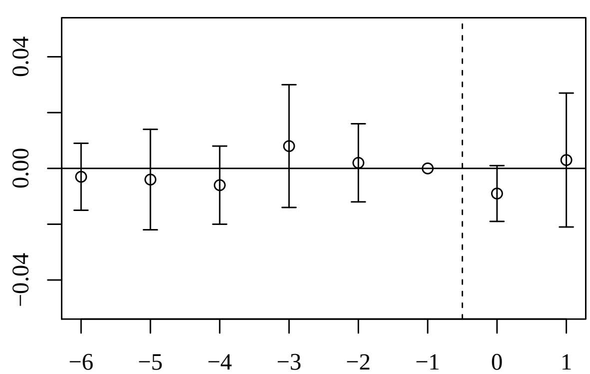
Panel A: All		
	Uninsured	Covered by Medicaid
Expansion × Post	-0.025	0.042
	(0.009)	(0.006)
Pre-ACA Mean:	0.2024	0.1098
N	21,990,000	21,990,000

Panel B: High-Impact Zip Codes		
	Uninsured	Covered by Medicaid
Expansion × Post	-0.090	0.107
	(0.017)	(0.017)
Pre-ACA Mean:	0.406	0.272
N	87,000	87,000

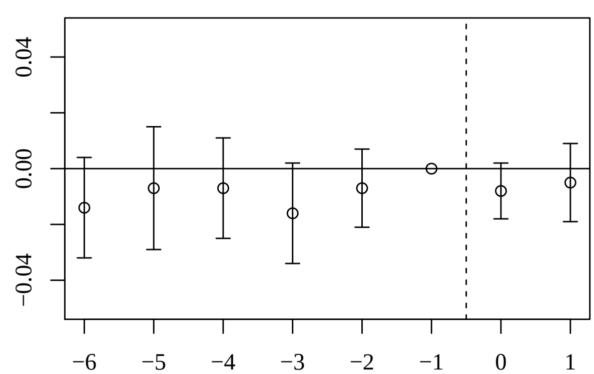
Panel C: Health Professional Shortage Area Counties		
	Uninsured	Covered by Medicaid
Expansion × Post	-0.031	0.049
	(0.010)	(0.007)
Pre-ACA Mean:	0.217	0.119
N	9,360,000	9,360,000

Note: This table displays the difference in differences estimates of the effect of the Medicaid expansion on the probability a respondent is uninsured (Column 1) or enrolled in Medicaid (Column 2) using data on non-elderly adults from the American Community Survey (2008-2015). Sample sizes in the American Community Survey are rounded to the nearest 10,000 due to disclosure requirements. Standard errors are in parentheses. Zip code is only available in the 1-year ACS file beginning in 2012.

Figure 7. : Trends in Rates of Delaying Care due to Appointment Availability and Wait Times, 2008-2015



(A) Delayed Needed Care due to Appointment Availability



(B) Delayed Needed Care Because Time in Waiting Room Too Long

Note: These figures report coefficients from the estimation of Equation 1 for the outcomes specified, with the coefficients representing the deviation from the mean difference between expansion and non-expansion states in the outcome in 2008 to 2015 among those 65 or older in the National Health Interview Survey.

Table 2—: Difference-in-Differences Results: Primary Care Services

Subsample:	All	Dually Eligible	High-Impact Zip Codes	HPSA Counties	0 or 1 Charlson Score	2+ Charlson Score
Panel A: Primary Care RVUs						
Expansion × Post	0.0784	-0.135	-0.267	0.260	-0.0312	0.140
	[-0.74,0.90]	[-1.11,0.84]	[-1.15,0.61]	[-0.69,1.21]	[-0.41,0.35]	[-0.86,1.14]
Outcome mean	15.09	20.86	14.77	14.91	6.240	20.61
% of mean	0.52	-0.65	-1.81	1.74	-0.50	0.68
Panel B: Number of Primary Care Visits						
Expansion × Post	0.0473	-0.111	-0.200	0.0828	-0.00362	0.0680
	[-0.27,0.37]	[-0.51,0.29]	[-0.52,0.12]	[-0.30,0.47]	[-0.18,0.17]	[-0.33,0.46]
Outcome mean	9.388	11.76	9.083	9.182	4.391	12.51
% of mean	0.50	-0.94	-2.20	0.90	-0.08	0.54
Panel C: New Patient Visits						
Expansion × Post	0.00486	0.00353	-0.00259	0.00269	0.00376	0.00354
	[-0.005,0.015]	[-0.023,0.030]	[-0.017,0.012]	[-0.008,0.013]	[-0.009,0.017]	[-0.008,0.015]
Outcome mean	0.748	0.714	0.679	0.720	0.479	0.916
% of mean	0.65	0.49	-0.38	0.37	0.78	0.39
N	28,758,522	3,835,554	5,488,541	10,475,157	10,754,291	18,004,231

Note: This table reports difference-in-differences estimates comparing expansion and non-expansion states 2008 to 2015. Each column represents a different sample and each panel a different outcome. Because of data availability as described in the text, the analysis that uses new patient visits excludes 2008 and 2009, thus reducing the number of observations for those regressions by approximately 40%. 95% confidence intervals, in brackets, from standard errors clustered at the state level.

Table 3—: Difference-in-Differences Results: Other Services

Subsample:	All	Dually Eligible	High-Impact Zip Codes	HPSA Counties	0 or 1 Charlson Score	2+ Charlson Score
Panel A: Number of Physician Services						
Expansion × Post	0.270	-0.0972	-0.146	0.276	0.127	0.324
	[-0.29,0.83]	[-0.70,0.51]	[-0.65,0.36]	[-0.40,0.95]	[-0.27,0.52]	[-0.32,0.97]
Outcome mean	21.89	26.24	20.96	21.47	10.91	28.75
% of mean	1.23	-0.37	-0.70	1.29	1.16	1.13
Panel B: Days Between Physician Services						
Expansion × Post	-0.152	0.111	-0.171	-0.0417	-0.333	-0.125
	[-0.48,0.18]	[-0.32,0.54]	[-0.48,0.14]	[-0.36,0.28]	[-0.77,0.11]	[-0.41,0.16]
Outcome mean	22.61	22.78	23.53	23.15	30.45	19.19
% of mean	-0.67	0.49	-0.73	-0.18	-1.09	-0.65
Panel C: Physician Services Expenditure						
Expansion × Post	27.81	-16.85	-26.50	40.75	11.35	30.20
	[-49.04,104.66]	[-101.29,67.59]	[-91.71,38.71]	[-57.70,139.20]	[-53.82,76.52]	[-58.96,119.36]
Outcome mean	3076	3459	2963	3050	1398	4124
% of mean	0.90	-0.49	-0.89	1.34	0.81	0.73
N	28,758,522	3,835,554	5,488,541	10,475,157	10,754,291	18,004,231

Note: This table reports difference-in-differences estimates comparing expansion and non-expansion states 2008 to 2015. Each column represents a different sample and each panel a different outcome. "Physician services" refers to the number of Part B claims, including those without primary care services, and "physician services expenditure" refers to total Part B expenditure on all services. The days between physician services measure is not defined for individuals with a single claim, so the number of observations for that outcome are reduced by approximately 25%. 95% confidence intervals, in brackets, from standard errors clustered at the state level.

Table 4—: Difference-in-Differences Results: Alternative Crowding Measures

	Delayed Medical Care Due to No Appointment Availability	Delayed Medical Care Due to Long Wait Times in Waiting Room
Expansion × Post	-0.004	0.001
	(0.004)	(0.004)
Outcome mean	0.058	0.045
% of mean	-6.9	2.2
N	48,508	48,508

Note: This table presents difference-in-differences estimates using data from the 2008 to 2015 National Health Interview Survey from respondents age 65 and older. The dependent variable in the first column equals 1 if the respondent reported that he or she delayed medical care because no appointment was available and 0 otherwise; the dependent variable in the second column equals 1 if the respondent reported that he or she delayed medical care due to a long wait time in the waiting room. Standard errors are clustered by state and reported in parentheses under the coefficient estimate.