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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

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Some states that have not adopted the Affordable Care Act (ACA) Medicaid expansions have stated 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 suggest 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 increase insurance coverage and access to care for low-income individuals.[1] 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.[2] The moral hazard effects of insurance expansions 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

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[1]The ACA Medicaid expansion was funded by the federal government 100% in 2014-2016. Beginning in 2017, the federal share gradually decreases each year until reaching 90% in 2020. From this point forward, states are responsible for 10% of the expansion costs, which remains a lower state share when compared to the federal-state split for the regular Medicaid population.

[2]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.

large.

This paper provides the first evidence on whether the ACA coverage expansions generated negative spillovers in utilization for the already insured. Our research design compares 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 live in areas that experienced 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, but 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.

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. 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 groups may be relevant for these physicians.[4] 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 unambiguously predicts that utilization of such services should fall among the already-insured with the onset of the Medicaid expansions (Section II). Using both a difference-in-differences and synthetic controls empirical implementation, we find a tightly-bounded null effect of coverage expansions on primary care utilization. These null results are apparent across several Medicare subpopulations most likely to experience spillovers, and for measures of utilization that span settings and types of care. Overall, we conclude that the Medicaid expansions did not generate economically meaningful spillovers in the Medicare population.

The framework for our analysis comes from the model described by Sloan et al. (1978). We adopt this model 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

[3]There were several changes to Medicare in the ACA. The Act altered payment formulas for both fee-for-service and Medicare Advantage to set a slower pace of spending growth. The Act initiated a number of payment demonstrations in fee-for-service Medicare, including the Accountable Care Organization model introduced in 2013. Part D benefits were enhanced by the closing of the "coverage gap" gradually between 2011 and 2020. Perhaps most relevant to our paper, Medicare began covering preventive services and an annual wellness visit at no cost-sharing in 2011, and between 2011 and 2015 primary care providers in Medicare received a 10% increase in the Medicare portion of reimbursements of some primary care services under the Primary Care Incentive Program. However, all of these changes were national in scope and not expected to affect Medicaid expansion states differently than non-expansion states.

[4]These statistics are calculated by the authors from the 2015 National Ambulatory Medical Care Survey and exclude pediatricians.

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, our difference-in-difference model implies that no difference in the utilization of primary care services occurred among Medicare beneficiaries in expansion and non-expansion states. Our confidence interval bounds the effect at a decline of less than one-third of a visit, a 3.1% change relative to the 9.4 such visits Medicare beneficiaries have on average per year. An event study provides support for the parallel trends assumption for our difference-in-difference analysis. We also test the expansion states against a synthetic control and find no detectable effect. In our synthetic control analysis, our 95 percent confidence interval rejects reductions in primary care visits larger than 1.6%. 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 1.3% and 1.7% under the difference-in-differences and synthetic control models, respectively.

We hypothesize that any negative spillovers are likely to be concentrated among certain subpopulations of Medicare beneficiaries defined by whether they are dually eligible for Medicaid, their geographic location, and their individual health status. Therefore, in addition to examining changes in utilization for the full Medicare population, we also examine changes in five different subgroups. 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 any negative spillovers were concentrated among this group (McInerney et al., 2017). Second, we examine Medicare beneficiaries who reside in low-income zip codes that had 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 in both our difference-in-difference and synthetic cohort analyses, and again our large sample sizes allow us to reject meaningful effects. We also assess the robustness of our findings to different sample definitions, including focusing on the states with the largest eligibility gains under the expansions, but continue to find no evidence of negative spillovers.

We also explore several other outcomes designed to characterize negative spillovers, if present. Our results suggest no change associated with Medicaid expansion in Medicare beneficiaries' total number of office visits, number of office visits with a physician extender (such as a nurse practitioner), number of days between visits, total expenditures, or expenditures on office-based care. We also assess survey evidence on changes in care due to wait times as reported by Medicare beneficiaries. We find no evidence of changes in care associated with either appointment availability or in-office wait times. Thus, overall we present robust, well-identified, tightly-bounded evidence that there was no effect on the utilization of Medicare beneficiaries associated with the ACA Medicaid expansions.

Our findings have significant policy implications and suggest that the 16 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 their 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 implied 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 could accommodate the 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.[5] Buchmueller et al. (2016) find evidence that dental demand shocks 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. While we show no change in the care supplied to Medicare beneficiaries by mid-level providers, such providers may have primarily supplied care to newly-insured Medicaid beneficiaries. Also, 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). 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 (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

[5] For example, Miller and Wherry (2017) and Simon et al. (2017) find the expansions 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.

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 private insurance demand, measured using firm size composition.[6] Finally, the authors examine whether there are changes in physician behavior or patient experiences 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 coverage. Surprisingly, despite receiving fewer physician visits per year, avoidable hospitalizations are lower for Medicare patients in these areas.

Prior work on insurance expansions under Massachusetts health care reform finds mixed 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 zipcodes 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 costs 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. 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. Similarly, Huh (2017) finds that providing Medicaid coverage for dental services induces dentists to move in to a state. Finally, Baker and Royalty (2000) study an expansion in Medicaid prenatal eligibility that was accompanied

[6] He et al. (2015) have similar findings using changes in county unemployment rates. They find that inpatient care for Medicare patients increases in response to rising unemployment rates among providers with a greater share of private patients.

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. Conceptual 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 (highest) 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 decreases with quantity, again, at some point 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, non-publicly insured 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 private 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 Medicaid patient, and Medicaid patients are substituted for lower paying non-public patients at the margin. In this scenario, Medicare patients may find themselves replaced, at least to some extent, by newly-insured Medicaid patients for all three types of physicians represented by MC-1, MC-2, and MC-3.[7]

[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

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 reimbursement parity (primary care services) may begin providing more services to newly-insured Medicaid beneficiaries. 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]

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, this reimbursement parity was in effect during the first year of Medicaid expansion in 2014.[9] However, the policy was not reauthorized by Congress and expired after 2014. 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).[10] This meant that 7 of the 29 expansion states still had this policy in effect during 2015, or the second year of Medicaid expansion. In our main empirical analyses, we estimate the reduced form policy effect in 2014 and 2015 for all expansion states. In subsequent analyses, however, we re-estimate the models using data through 2014 only, the year in which we predict the negative spillovers would be largest.

Because of the magnitude of the Medicaid expansions, it is reasonable to expect a change in providers' marginal patient as suggested by the model. Firstly, as described in the Introduction, the ACA expansions were associated with economically-meaningful increases in service demand from the newly insured (Miller and Wherry, 2017; Simon et al., 2017; Wherry and Miller, 2016; Sommers et al., 2015; Ghosh et al., 2017). In addition, the ACA expansions were as large as, if not larger than, the earlier expansions studied in related empirical work (McInerney et al., 2017; Glied and Hong, 2018). We also 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 and 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

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 second 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).

[9]Several states experienced delays in the implementation of the fee bump, although providers were paid retroactively for primary care rendered to Medicaid beneficiaries during these delays (Alexander and Schnell, 2017).

[10]We are excluding Connecticut in the count from Tollen (2015) since other sources indicate that the state did not actually extend the fee bump in 2015.

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 primary care services. 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 for new patient visits if there were additional administrative burdens associated with Medicaid reimbursement. 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.[11] 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.[12] To do so, we rely on the American Community Survey (ACS), on ongoing survey conducted by the U.S. Census Bureau that includes information on health insurance coverage. 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.[13] The

[11] As discussed earlier, Garthwaite (2012) and He and White (2013) find evidence suggesting that physicians reduce their labor supply in response to CHIP expansions, while Chen (2017) finds an increase in total hours worked.

[12] Buchmueller et al. (2017), Simon et al. (2017), Sommers et al. (2015), Miller and Wherry (2017), Wherry and Miller (2016), Hu et al. (2018), and others, all document this pattern.

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

restricted-use version also contains information on the county and (starting in 2012) zip code of residence of respondents, allowing us to estimate a first stage for the subsamples of individuals living in Health Provider Shortage Area (HPSA) counties and in high-impact zip codes. We define high impact zip codes as those that are in the top quartile when ranked 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 prior to the ACA. We apply the relevant survey weights in our analysis and use survey years 2008 through 2015 except where noted.

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% random sample of enrollees observed from 2008 to 2015.

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.[14] We count the number of visits that include primary care services, as well as the total work-related Relative Value Units (RVUs) of primary care services received by each beneficiary each year. Medicare measures the amount of care provided on the basis of RVUs; these units are meant to capture the resources each type of care is expected to use. In particular, RVUs associated with primary care services correspond to visits of different lengths, with longer and more complicated visits receiving a higher number of RVUs. If physicians shorten the average length of time spent with each Medicare beneficiary in response to the Medicaid expansions, this behavior should be reflected in changes in primary care RVUs. As such, we consider RVUs to be a key outcome that allows us to accurately measure changes in the quantity of health care services received.

We also examine a subset of visits with primary care services and reimbursement codes for new patient evaluation (HCPCS codes 99201-99205). 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 office visits without restricting them to be related to primary-care services.[15] Since the use of mid-level providers is hypothesized to be a

[14]The affected codes in Medicare are HCPCS codes 99201-99499, 90460, 90461, 90471, 90472, and 90473 (Kaiser Commission on Medicaid and the Uninsured, 2012). Technically, 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 reimbursements; in Medicare, 82% of affected services are provided by qualifying providers, so we do not incorporate this qualification.

[15]We refer to each claim in the Carrier file as an "office visit," although in fact some claims may represent laboratory, ambulance, or ambulatory surgical services.

pathway for supply expansion, we note whether a visit included any services provided by a non-MD (primarily nurse practitioners or physician assistants). 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 visits 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 office-based (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 A.1 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, 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 all eight years.

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 are 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.[16]

We also examine individuals living in zip codes in which a high percentage of the population was low income and uninsured before the ACA. We define this sample using zip code level data from the 5-year 2008-2012 ACS file. We rank all zip codes by the share of the adult population under age 65 who are uninsured and have an income less than 138 percent of the FPL. We define the top quartile as “high-impact” zip codes.[17] We show that a larger portion of individuals living in these zip codes gained insurance coverage through the ACA Medicaid expansions; it is therefore reasonable to believe that negative spillovers may be larger there.

[16] Only 16% of dually eligible individuals are living in institutions such as nursing homes, and most likely, seeing providers whose practices are focused on the institutionalized population; the remainder are community dwelling individuals sharing providers with the general population (Coughlin et al., 2012).

[17] Note that this is the same zip code definition used in Hu et al. (2018).

Additionally, individuals residing in health-provider shortage areas may also be likely to experience negative spillovers, if the health care industry in these areas is already operating at capacity. Thus, we examine individuals residing in counties designated in 2010 as Health Professional Shortage Areas for primary care practitioners by the U.S. Health Resources and Services Administration.[18]

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 of the index indicating worse health.[19] 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.

Table 1 shows descriptive statistics and sample sizes for our Medicare claims data. The first row describes the share of individuals in each subsample treated by the Medicaid expansion, described in more detail below. It is clear that in all subsamples we observe a large number of both treated and untreated individuals. The following rows report the means of our outcome variables. A typical Medicare beneficiary has about 9.4 primary-care visits per year. However, dually-eligible beneficiaries and beneficiaries with 2 or more chronic diseases have considerably more primary care visits on average; 11.8 and 12.5, respectively. 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 zipcodes 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 enhances our ability to assess and 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

[18]2010 HPSA designations for each county are drawn from the 2015-2016 Area Health Resource File. We consider a county to have HSPA designation if the entire county was indicated to be a geographically defined HPSA. For detailed information regarding HPSA Primary Care Designation Criteria, please refer to the HRSA website at http://bhpr.hrsa.gov/shortage/hpsas/designationcriteria/designationcriteria.html.

[19]See Charlson et al. (1987) for more information on how the index is calculated.

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 a state that opted to expand Medicaid eligibility in 2014 or 2015, and zero otherwise.[20] States with previous expansions of Medicaid, or similar coverage, for adults with incomes up to 100% FPL before 2014 are excluded from the analysis.[21] 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.[22] The standard errors are clustered at the state level.

Our parameters of interest are estimates of β_y. We set the coefficient on β_{-1} equal to zero to use the year immediately prior to expansion as our reference year. The remaining β_y coefficients capture how outcomes evolved in states that opted to expand Medicaid relative to those that did not. 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-difference estimation of the impact of the ACA. This indicates that before the ACA, expansion and non-expansion states experienced similar trends in outcome variables, and only diverged after the ACA Medicaid expansions were enacted.

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.

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

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 nonexpansion states, rather than estimating changes relative to the year immediately prior to implementation.

[20] 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 considered 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).

[21] 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. 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.

[22] We group together $y = -6$ and $y = -7$ since only the 3 states expanding in 2015 have a nonzero value for $y = -7$.

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 distinctly 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). Because the comparison units are weighted to match the treated unit, there is no concern about differing pre-trends.

We construct confidence intervals in the synthetic control analysis via a permutation test. This test assigns the status of expansion state to 21 states selected at random and re-estimates the synthetic control model as if the randomly selected states had actually expanded Medicaid.[23] This provides us with a distribution of null effects. We center this distribution at our estimated effect and report the 97.5th and 2.5th empirical quantiles as the end points of our confidence interval.

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 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.[24] However, if these changes are correlated

[23]We select 21 states to form the placebo treatment group to mirror the 21 expansion states that form the true treatment group.

[24]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 dual-eligibles, 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. In addition, some states extended the increased reimbursement policy into 2015, sometimes with additional state-specific characteristics (certain providers). If these state policies were correlated with expansion status and affected the utilization of dually-eligible beneficiaries, then changes in outcomes among the dually-eligible will reflect the combined effect of both the state-specific fee-bump policy 2013-2015 and

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 varied 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 by a nationally representative sample of individuals. We then dropped expansion states whose eligibility change was less than 10 percentage points.[25] 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.[26] 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 2014 to 2015.

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 National Health Interview Survey (NHIS), which contains state of residence information. In each year, the NHIS questions a single, 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. These results are consistent with existing work showing relative increases in coverage in expansion states (for example, Buchmueller et al., 2017; Simon et al., 2017; Sommers et al., 2015; Miller and Wherry, 2017 among others). We conduct this analysis using the restricted-use version of the American Community Survey as described in Section III.

Figure 2 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 Medicaid expansion in 2014-2015.

[25]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.

[26]Under Wisconsin's BadgerCare Reform waiver, the state expanded eligibility to childless low-income adults with incomes up to 100% FPL in 2014. The state already covered low-income parents under its BadgerCare Plus program.

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. Note that we only observe zip code of residence in the ACS micro data beginning in 2012.[27]

The difference-in-differences coefficients are reported in Table 2. 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, the estimated effects of the expansions are considerably larger: we observe an increase in Medicaid coverage of 10.7 percentage points and in insurance coverage of 9 percentage points. Among those living in counties designated as health professional shortage areas (Panel C), we see increases in Medicaid enrollment of 4.9 percentage points and decreases in uninsurance of 3.1 percentage points, which is only slightly larger than what we observe overall. However, we might expect that spillovers would still be particularly strong in these areas given low provider availability.

According to existing results in the literature, insurance coverage changes of this size could potentially result in large spillovers.[28] For example, McInerney et al. (2017) find that a one percentage point increase in Medicaid eligibility for noneldery adults reduces average spending among dually eligible Medicare beneficiaries by 3.3 percent and by 0.7 percent for all beneficiaries. 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. Similarly, 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 office-based spending among Medicare beneficiaries; in the high impact zip codes, where we observe a 9 percentage point increase in insurance coverage, we would expect an over 23 percent decrease in the Medicare portion of office-based spending.[29] In the next section, we directly test for such impacts on the utilization of Medicare beneficiaries.

[27]This is the first year that zip codes were available on the 1-year ACS file using ZCTA definitions from the 2010 Census.

[28]The measures of spending mentioned in this paragraph differ slightly from those used in our analysis. McInerney et al. (2017) focus on a measure of total spending constructed from the Medicare Current Beneficiary Survey Cost and Use files, which rely on self-reports and administrative records to report spending on all medical services, including services not covered by Medicare. These files also report spending for both fee-for-service beneficiaries and Medicare Advantage enrollees. Our data only include services covered by Medicare, and we are limited to fee-for-service beneficiaries. However, McInerney et al. (2017) find their effects are driven by the Medicare portion of total spending, and our null results persist when we restrict our attention to the Medicare portion of total spending for fee-for-service beneficiaries. Glied and Hong (2018) rely on data on office-based spending for fee-for-service beneficiaries from the Dartmouth Atlas that only include the Medicare portion of spending, and do not include any cost-sharing amounts. However, ignoring the small deductible for office-based care (adjusted each year for inflation, equal to $110 in 2010), our measure of office-based spending is simply 1.25 times the Medicare portion since cost-sharing is 20% for these services.

[29]Glied and Hong (2018) instrument for changes in insurance coverage with an indicator of large Medicaid eligibility expansion (changes in state-level eligibility exceeding 5 percentage points) to examine the impact of changes in insurance coverage driven by Medicaid expansion on office-based spending. They find that a one percentage-point increase in insurance coverage is associated with a decrease in office-based spending of 2.6 percent.

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 nonexpansion states. Despite these coverage increases, we show in this section that Medicare 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 is parity in reimbursements between the Medicare and Medicaid programs, which implies a reduction in utilization in our theoretical model. We first present event studies validating the parallel trends assumption of a difference-in-difference model, and then show a formal pre-post difference-in-difference coefficient.

Figure 3 reports an event study for all (Panel A) and several subsamples of Medicare beneficiaries for work-related RVUs for primary care, which captures the quantity of primary care services supplied. Each figure reports the estimates from Equation (1), where the coefficient for the year immediately prior to expansion has been normalized to zero. The coefficients on the other years represent the deviation of the outcome in the specified year relative to expansion from the mean difference between expansion and non-expansion states. 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.

Figure 3 depicts our main results: 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 3, we report the corresponding difference-in-difference 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 0.4% reduction 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 (1.42 RVUs) and represents only a 1.8% decline relative to the sample mean.[30] Moreover, our coefficients are precisely estimated. Evaluating the lower bound of the 95% CI relative to the mean, we are able to rule out a decline in RVUs of more than 5.1% in the full sample. The largest possible reduction across all samples is a 7.8% reduction in the number of primary care RVUs among residents in high-impact zipcodes, representing fewer RVUs than would be needed for a single office visit. 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.

Figure 4 and 5 next 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

[30] The number of work RVUs for code 99203, a 30-minute new patient office visit, is 1.42.

of care supplied, although it does not capture some important characteristics of care, such as the length of the visit, that 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 zipcode subsamples in the first year of expansion. 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 1.5 percent a 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 3 repeat the DID analysis for these two other measures of primary care service utilization. The DID estimates are extremely small, representing less than 1% 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 4 applies the same research design to a broader set of outcomes. In Panel A, we demonstrate that the Medicaid expansion was not associated with changes in the total number of office visits 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 that 0.7 in the number of office visits, or a 2.7 percent decline over the sample mean 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.[31] The corresponding event studies (in Appendix Figure A.2), are generally supportive of the parallel trends assumption, with possibly the exception of the sick subsample.

If providers' schedules are crowded by the newly insured, Medicare beneficiaries may be asked to wait longer between office visits. Appendix Figure A.3 suggests this proxy for "wait times" was trending similarly in the two types of states. The point estimates in Panel B of Table 4 suggest *decreases* in this measure for all samples other than the dually eligible, rather than increases as one would predict. None of the estimates is statistically significant. Under the implied 95% confidence intervals, the largest possible increase in the days between visits across samples is 2% among the dually eligible, from 22.8 to 23.3 days.

Panel C of Table 4 reports expenditures on office-based services in expansion and non-expansion states. 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 office-based spending that range from 1.4% (sick) to 4.1% (healthy) of the mean. We may compare these estimates to the predicted decrease in office-based spending

[31] The authors estimate that a one percentage point change in Medicaid eligibility is associated with a 3.1 percent decrease in the number of medical provider events for the dually eligible. We again apply a 70% take-up rate to the estimated 4.2 percentage point increase in Medicaid coverage to determine the predicted change in medical provider events under the ACA expansions.

of 6.5% for Medicare beneficiaries overall, and the 23% decrease for beneficiaries in high-impact zipcodes described earlier based on estimates from Glied and Hong (2018). The event studies in Appendix Figure A.4 generally support our difference-in-difference design.

There are two outcomes of potential policy interest where our event studies suggested significant differences in trends between the expansion and non-expansion states. We measure the share of office visits among Medicare beneficiaries where some services are provided by a non-MD (mid-level) provider, such as an NP or PA. Unfortunately, as is apparent in Appendix Figure A.5, there is a clear downward trend in this outcome in expansion states over our time period. Similarly, overall expenditures across all settings of care shows a substantial upward trend in expansion states in Appendix Figure A.6.[32] Since these event studies suggest the assumptions of the difference-in-difference 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 when evaluating individual pre-expansion interaction terms for most variables we consider. However, for at least two outcomes – the use of mid-level providers and overall expenditure – there is some visual evidence of differential pre-treatment trends for the expansion and non-expansion states. In this subsection, we 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)).

Figure 6 demonstrates the observed trends in the expansion states and the synthetic control unit in the number of primary care RVUs each year, with Figure 7 and 8 plotting similar trends for the number of PCP visits and new patient visits, respectively. For all three outcomes, the synthetic control unit tracks very closely with the expansion states prior to the implementation of the ACA expansions and continues to do so in 2014 and 2015, after the expansions are implemented. The post-expansion difference between the expansion and non-expansion states is very close to zero.

Table 5 presents the synthetic control estimates of the effects of the expansions with the associated confidence intervals. Panel A shows the results for primary care RVUs. Consistent with the visual evidence presented in Figure 6, we find no effect of the ACA Medicaid expansions on Medicare beneficiaries' use of primary care and our point estimates are very small. For all Medicare beneficiaries, our point estimate indicates a change in primary care RVUs of 0.011 per year, less than a tenth of a percent of the sample mean; our confidence intervals allow us to reject a decrease in primary care RVUs of more than 0.264, only 1.7% of the sample mean. Although our precision varies across the different subgroups, for all subgroups we estimate a change in RVUs that is less than 3% of the sample mean, and our confidence intervals are such that we can reject a decrease in primary care RVUs of more than 0.68 for the dually eligible, or about 3% relative to the sample mean.

Panels B and C of Table 5 present the results for other measures of primary care use: the number of primary care visits and the number of new patient visits. Consistent with

[32]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.

results from the previous section, we find no change in either of these outcomes either overall and for any of the subgroups we examine. There does not appear to be any evidence of an effect of the Medicaid expansions on the use of primary care services by Medicare beneficiaries.

Table 6 presents the synthetic control estimates for other outcomes derived from the claims data. We first discuss the two outcomes for which we did not calculate difference-in-difference coefficients due to non-parallel pretrends: the number of visits with non-physician providers (Panel A) and overall expenditures (Panel B). The estimated coefficients for the use of non-physician providers are all negative, and a null effect is rejected only for the case of Medicare beneficiaries in HPSA counties. This reduction in non-MD visits appears to be driven by a relative increase in non-MD visits among beneficiaries in the non-expansion states (see Appendix Figure A.7). Although the effect is statistically significant, it is quite small, indicating a reduction in visits with non-MD providers of about 0.1 visits per year. We also note that there appears to be some differences between the treated group and the synthetic control even in the pre-expansion period. We find no change in the total number of office-based visits for this subsample (and the coefficient is positively signed, see Panel C); the negative effect on non-MD visits for this subgroup therefore suggests a shift towards more physician-delivered care.

Across all subgroups, we find no evidence of a decrease in overall medical expenditures associated with expansion (Panel B and Appendix Figure A.8). The estimated coefficients for models of total expenditures are not statistically significant and suggest, if anything, a small increase in expenditures. We are able to reject decreases larger than \$88 for all beneficiaries, approximately 1% of the sample mean. The largest lower bound we observe across the subgroups is a decrease of \$381 among the dually eligible, approximately 3% of average expenditures for this subsample. In contrast, as discussed earlier, results from McInerney et al. (2017) predict a decrease in spending of 4.2% overall and of almost 20% among the dual eligible.

The results for office-based expenditures (Panel C and Appendix Figure A.9) are similar. We find no statistically significant effect of the Medicaid expansions on spending on office-based care for any subgroup, and our point estimates are quite small. For the entire sample, we estimate an average decrease of \$38, about 1.2 percent of the sample mean, and across all subgroups we can reject decreases in spending larger or equal to 4% of the sample means.

Finally, the synthetic cohort analysis suggests no negative spillovers when we consider the number of office visits (Panel D and Appendix Figure A.10) and the days between such visits (Panel E and Appendix Figure A.11). We report a statistically significant *decrease* in the days between visits for the healthy subgroup of beneficiaries, those with a Charlson score of 0 or 1. Our point estimate indicates a decrease in the length between visits of 0.6 days, or a 1.9% decline over the mean of 30.5 days. Across all subgroups, there is no significant evidence of a negative spillover of the form of an increase in days between visits. The largest increase included in our 95% confidence intervals is still less than a one day increase (0.9 days, or 3.8% of the mean) for beneficiaries in high-impact zipcodes.

Overall, the results are consistent with those reported in the previous section. In particular, we do not find compelling evidence that the utilization of care among Medicare beneficiaries was affected by the Medicaid expansions, either overall or for any of the subgroups we examine. Our confidence intervals allow us to rule out changes larger than 5 percent in size for most outcomes.

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, we verify that our results hold for our primary-care related outcomes even when we drop 2015. Appendix Table A.1 reports the difference-in-difference coefficients for primary care RVUs, primary care visits, and new patient visits excluding 2015. The null hypothesis of no effect is never rejected and, as in Table 3, we can reject all but economically-small estimates.

Excluding Dual-eligibles. — As discussed in Section III, the increased reimbursement for primary care services may have affected utilization by duals 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 3, Panel B). Still, in this section we repeat our analysis of primary care utilization for each of our subsamples excluding duals.

Appendix Figures A.12-A.14 report the event studies for high-impact zip codes, HPSA counties, and the 0/1 and 2+ Charlson subsamples excluding duals. The non-dual share of these subsamples represents between 71% (high-impact zip-codes) to 82% (0/1 Charlson), 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 Medicare beneficiaries. None of the difference-in-difference 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 A.2 reports the difference-in-difference coefficients for primary care RVUs, 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.

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.[33] The

[33]Because the NHIS is a repeated cross-section, rather than a panel, we cannot limit the data to only those who were continuously enrolled in Medicare over the entire period. Instead, we focus on the over-65

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 9 plots the event study coefficients for these outcomes. There appear to be similar pre-trends across the treated and comparison states prior to the implementation of the ACA Medicaid expansions, and no visible divergence following implementation. Table 7 shows the result from the difference-in-difference 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 to 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. 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 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 (McInerney et al., 2017; Glied and Hong, 2018), who find large effects of pre-ACA Medicaid eligibility expansions on the use of care among Medicare beneficiaries. Our analysis does not directly speak as 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 remains unclear from these results what changes providers *did* make in order to see the newly-insured, only that any changes undertaken by providers did not affect the care received by Medicare patients. One possibility is more reliance on administrative staff and

population, almost all of whom should be Medicare eligible, rather than all Medicare enrollees. This avoids the possibility that the expansions themselves are changing who enrolls in Medicare and introducing bias through sample selection.

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 type 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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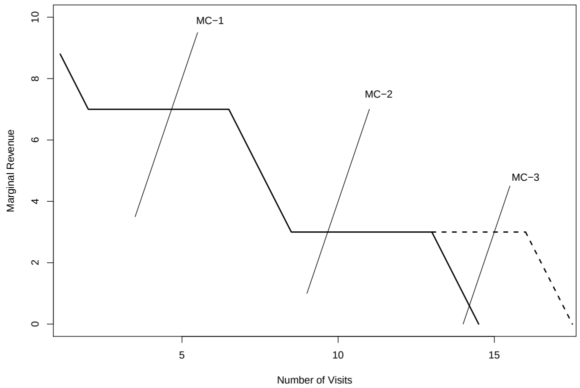
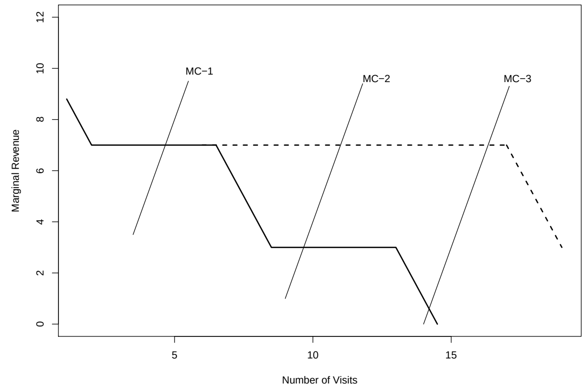
Figure 1. : Provider Response to Medicaid Expansions



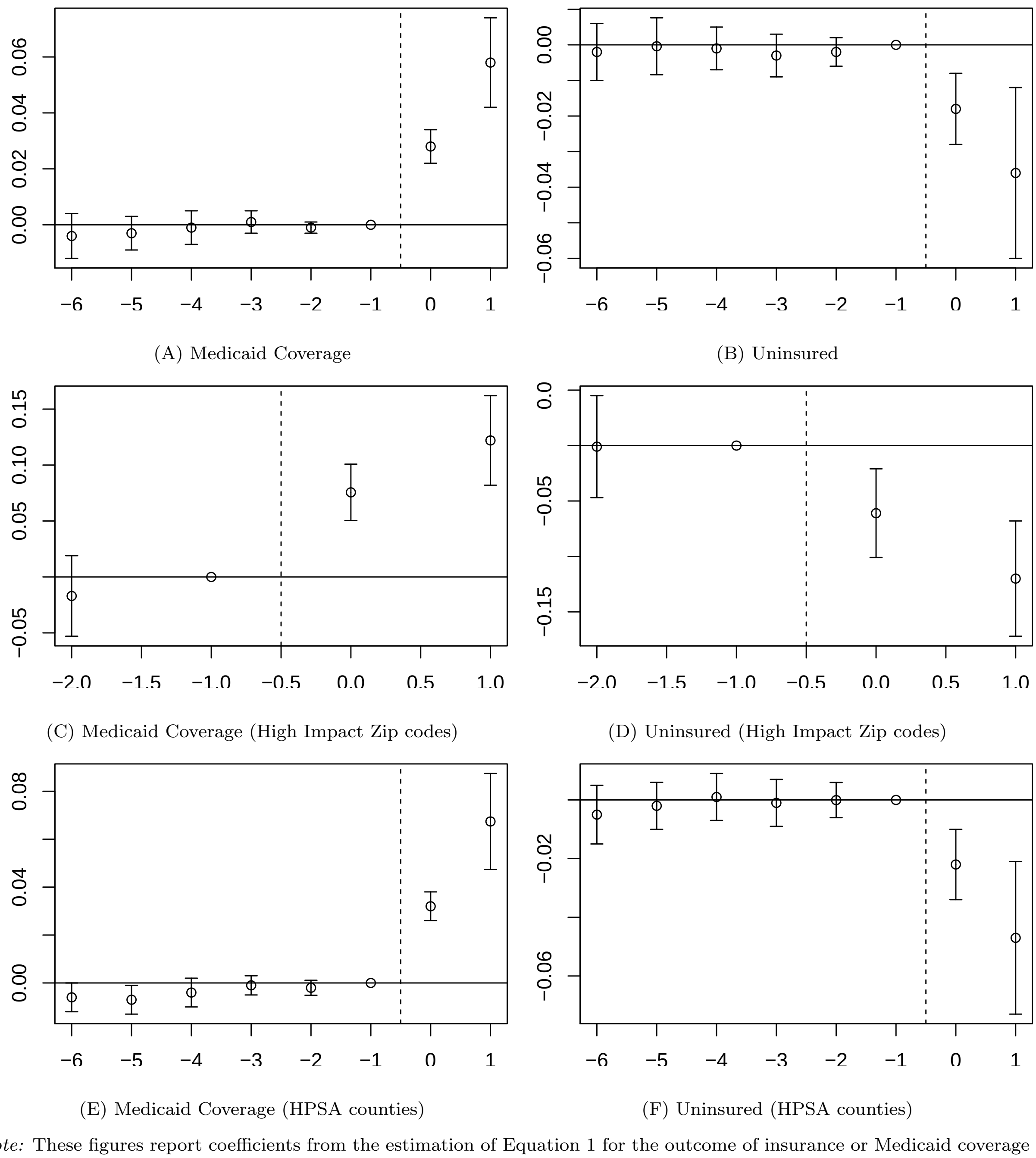
(A) No Fee Bump



(B) Fee Bump

Note: Figure depicts marginal revenue and marginal cost curves for providers before (solid) and after (solid and dashed) Medicaid expansion in the case there is no fee bump (first panel) and with a fee bump (second panel).

Figure 2. : Trends in Insurance Coverage in Expansion vs. Non-Expansion States, 2008-2015



(A) Medicaid Coverage

(B) Uninsured

(C) Medicaid Coverage (High Impact Zip codes)

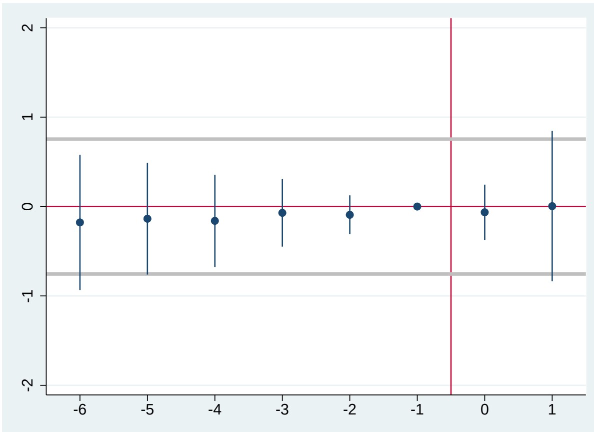
(D) Uninsured (High Impact Zip codes)

(E) Medicaid Coverage (HPSA counties)

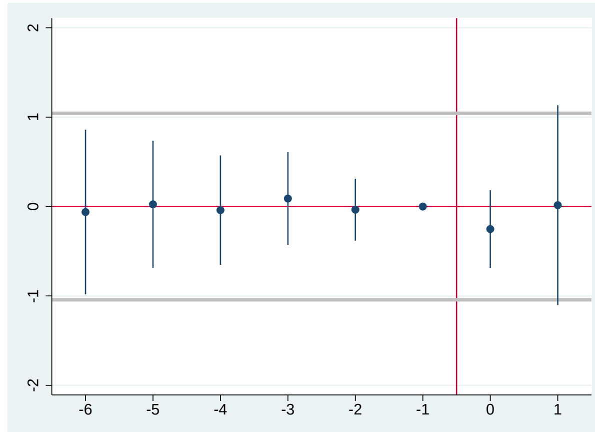
(F) Uninsured (HPSA counties)

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.

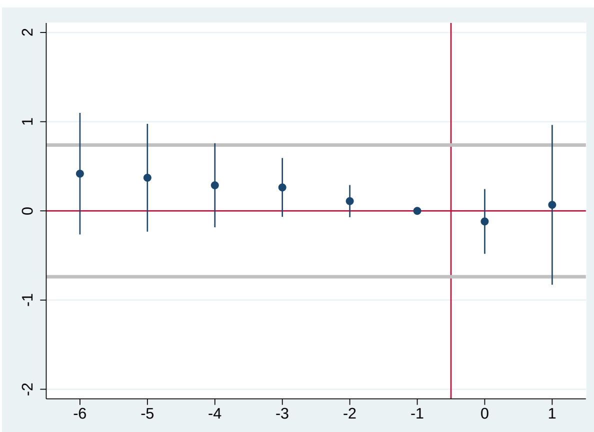
Figure 3. : Trends in Primary Care RVUs in Expansion vs. Non-Expansion States, 2008-2015



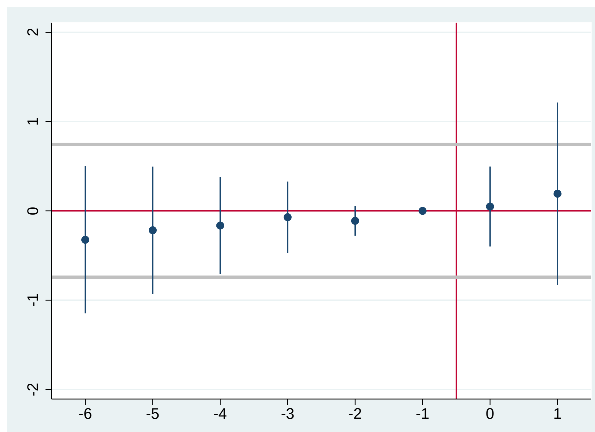
(A) All Medicare



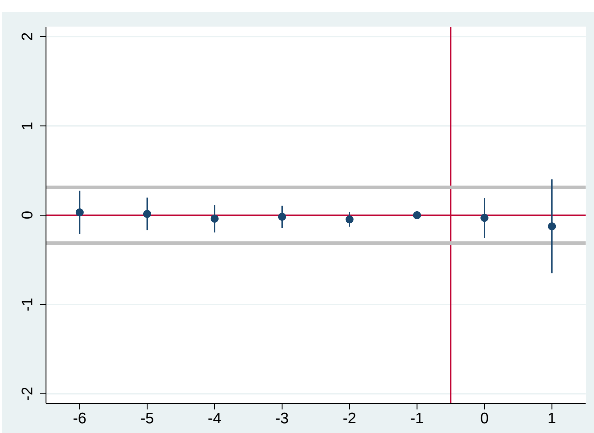
(B) Dually Eligible



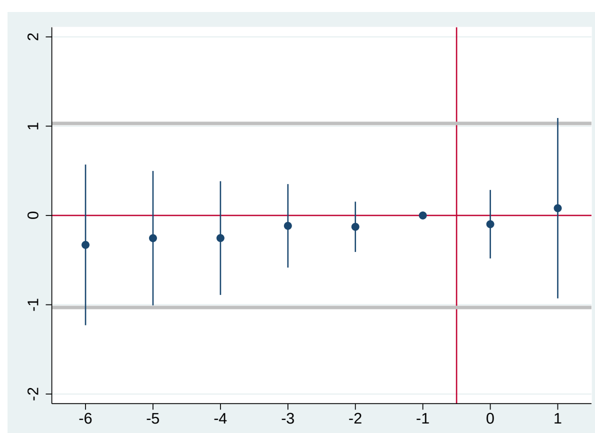
(C) High-impact zip codes



(D) HPSA counties



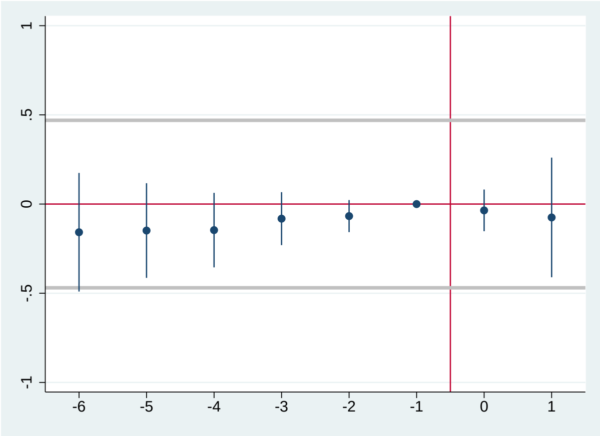
(E) Charlson score of 0 or 1



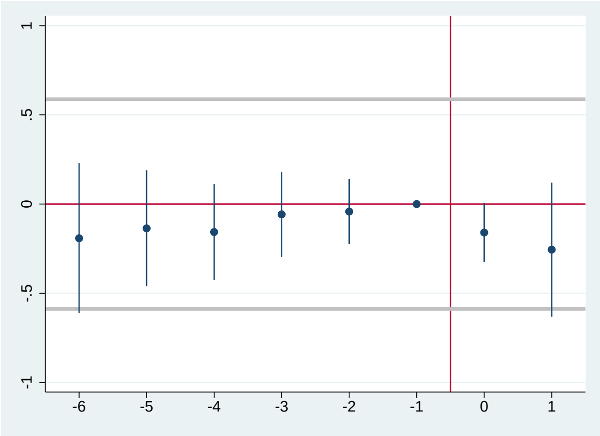
(F) Charlson score of 2 or more

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.

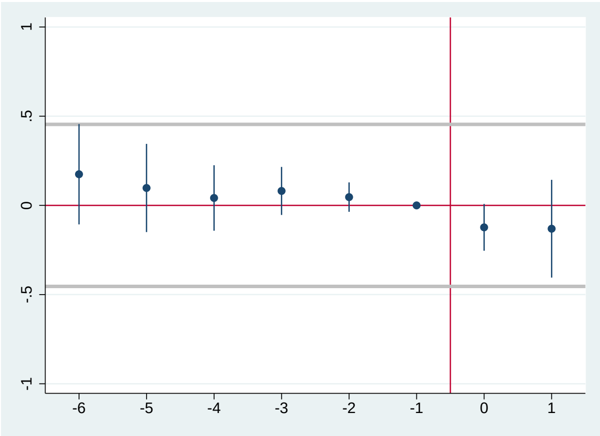
Figure 4. : Trends in Primary Care Visits in Expansion vs. Non-Expansion States, 2008-2015



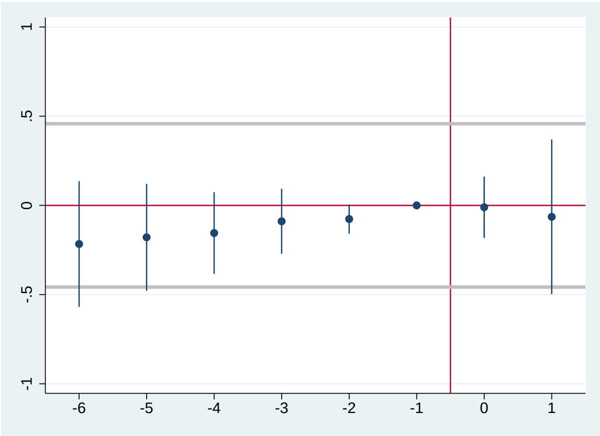
(A) All Medicare



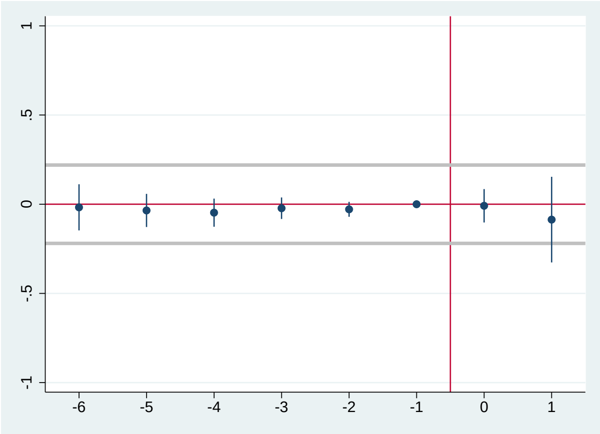
(B) Dually Eligible



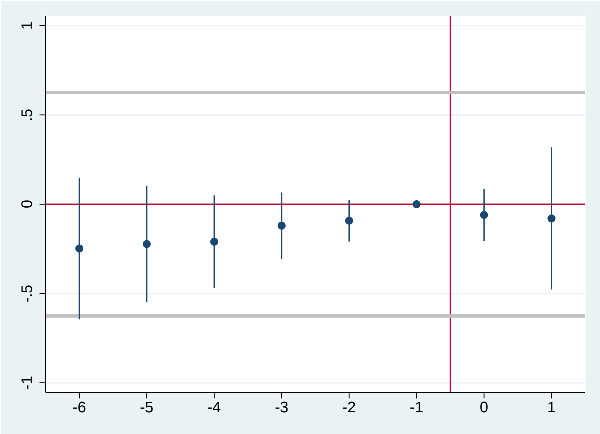
(C) High-impact zip codes



(D) HPSA counties



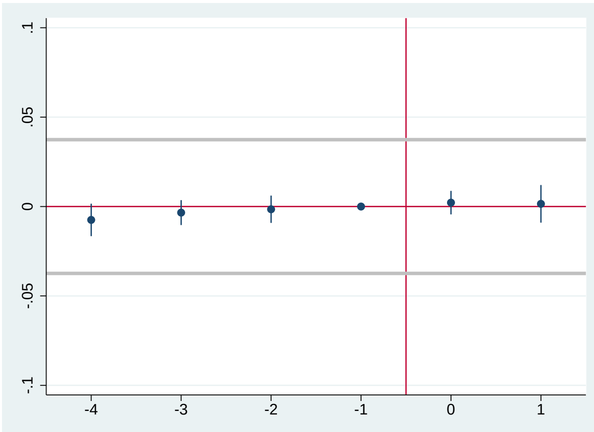
(E) Charlson score of 0 or 1



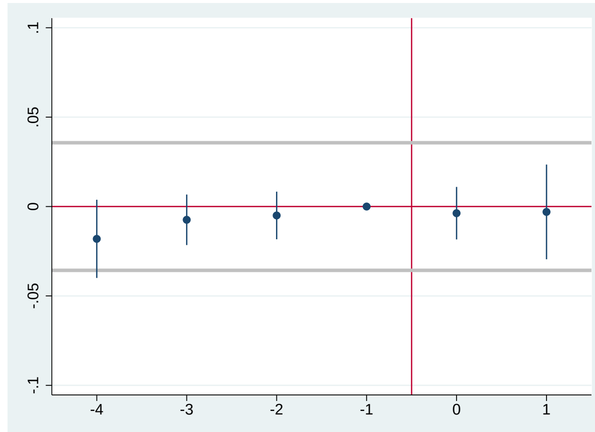
(F) Charlson score of 2 or more

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.

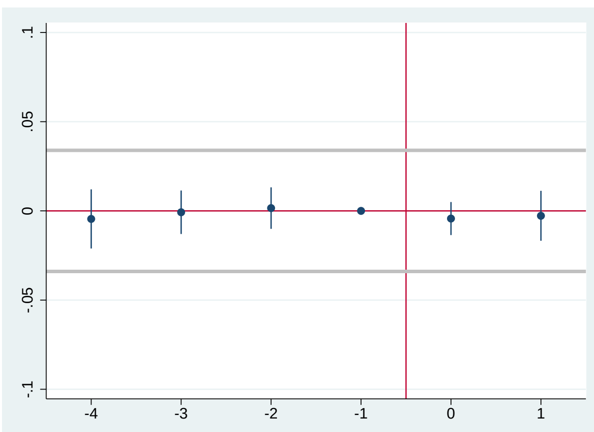
Figure 5. : Trends in New Patient Visits in Expansion vs. Non-Expansion States, 2008-2015



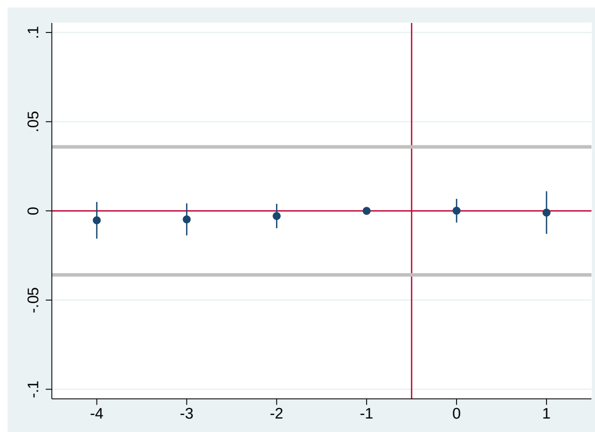
(A) All Medicare



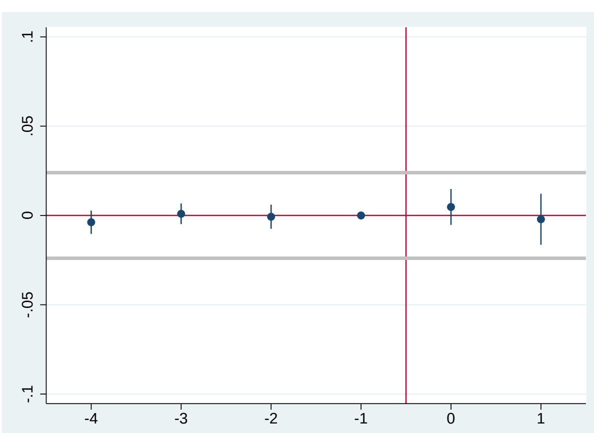
(B) Dually Eligible



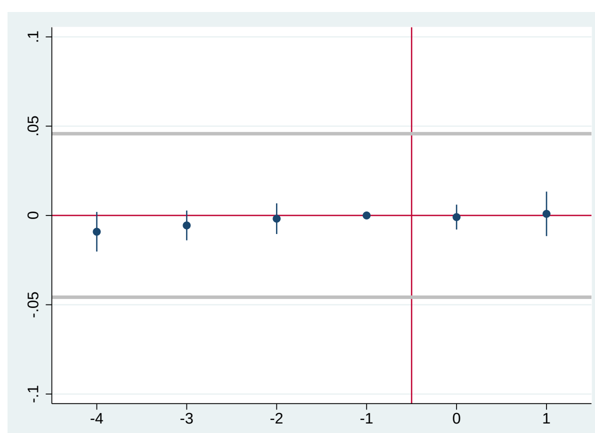
(C) High-impact zip codes



(D) HPSA counties



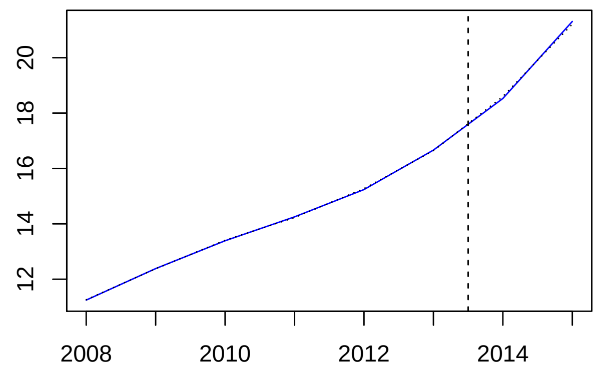
(E) Charlson score of 0 or 1



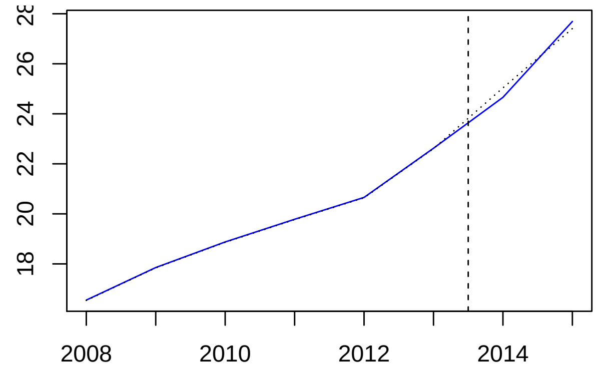
(F) Charlson score of 2 or more

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.

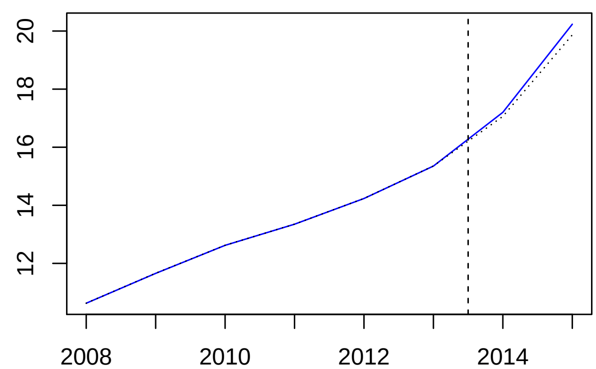
Figure 6. : Primary Care RVUs in Expansion States (solid) vs. Synthetic Control (dotted), 2008-2015



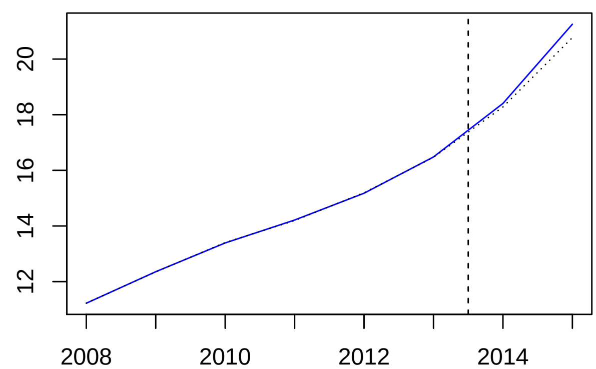
(A) All Medicare



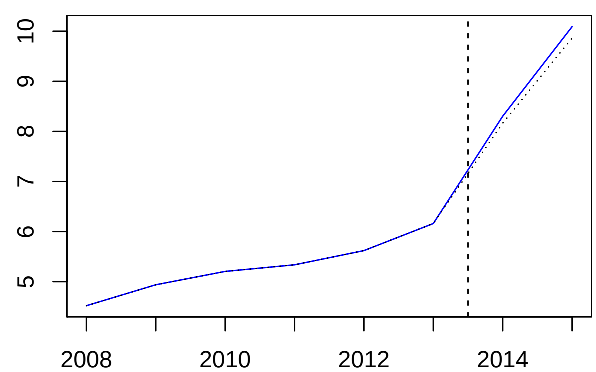
(B) Dually Eligible



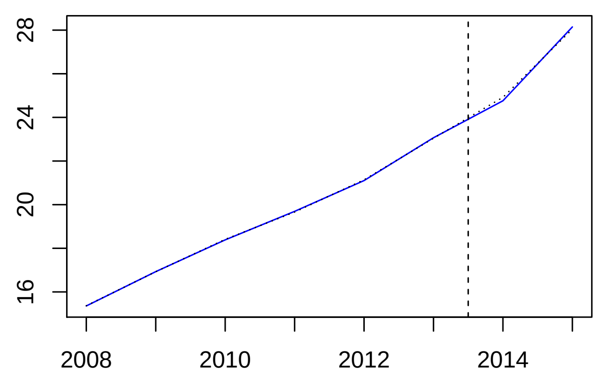
(C) High-impact zip codes



(D) HPSA counties



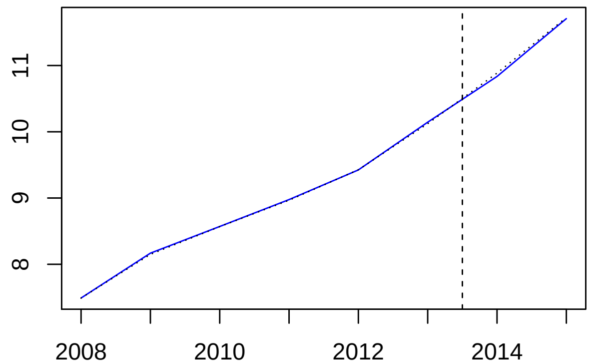
(E) Charlson score of 0 or 1



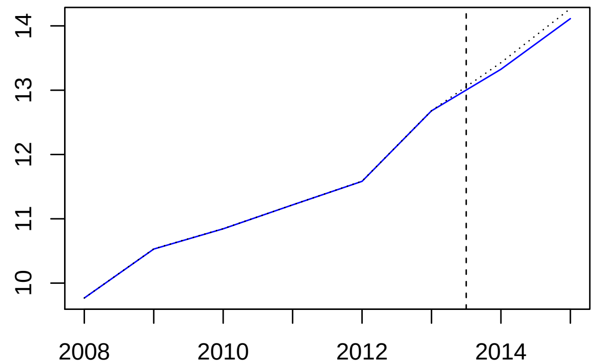
(F) Charlson score of 2 or more

Note: These figures report trends from 2008 to 2015 in the outcome variable in the expansion states relative to a synthetic control unit created by weighting the non-expansion states for the outcome of primary care RVUs. See the text for more details.

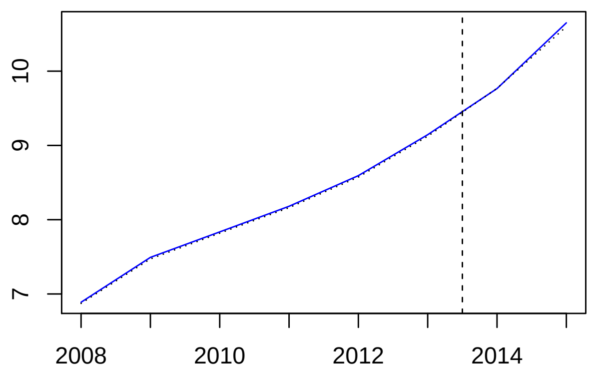
Figure 7. : Trends in PCP Visits in Expansion States (solid) vs. Synthetic Control (dotted), 2008-2015



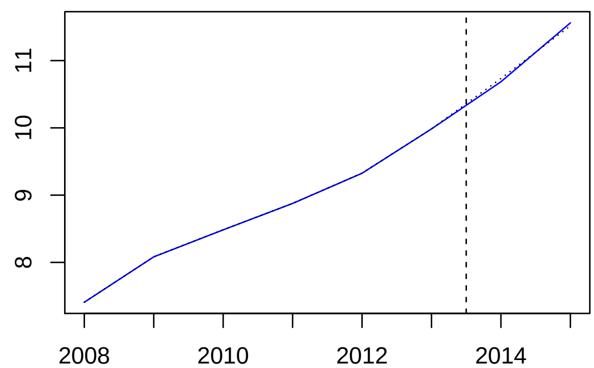
(A) All Medicare



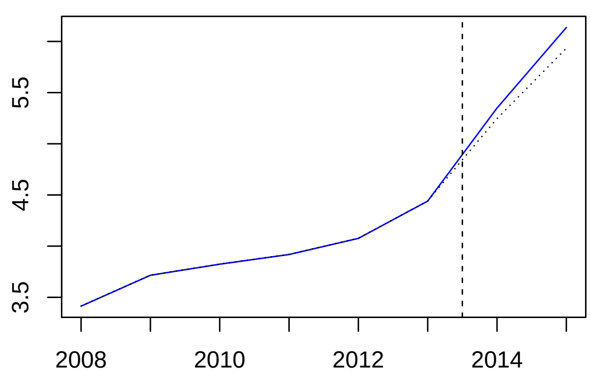
(B) Dually Eligible



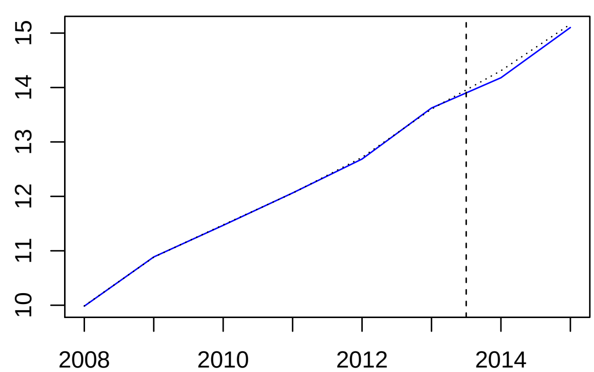
(C) High impact zip codes



(D) HPSA counties



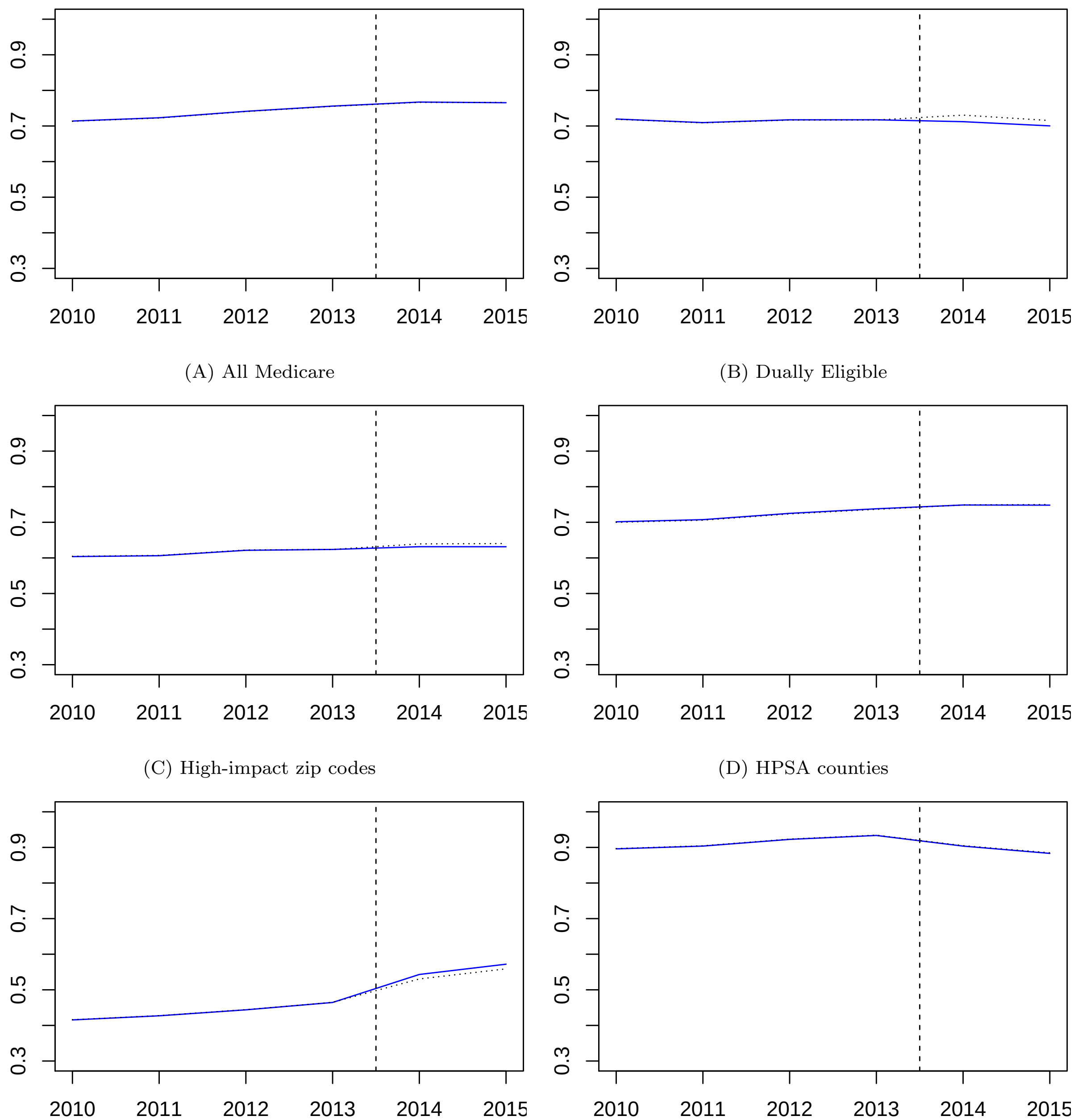
(E) Charlson score of 0 or 1



(F) Charlson score of 2 or more

Note: These figures report trends from 2008 to 2015 in the outcome variable in the expansion states relative to a synthetic control unit created by weighting the non-expansion states for the outcome of number of primary care visits. See the text for more details.

Figure 8. : New Patient Visits in Expansion States (solid) vs. Synthetic Control (dotted), 2008-2015



(A) All Medicare

(B) Dually Eligible

(C) High-impact zip codes

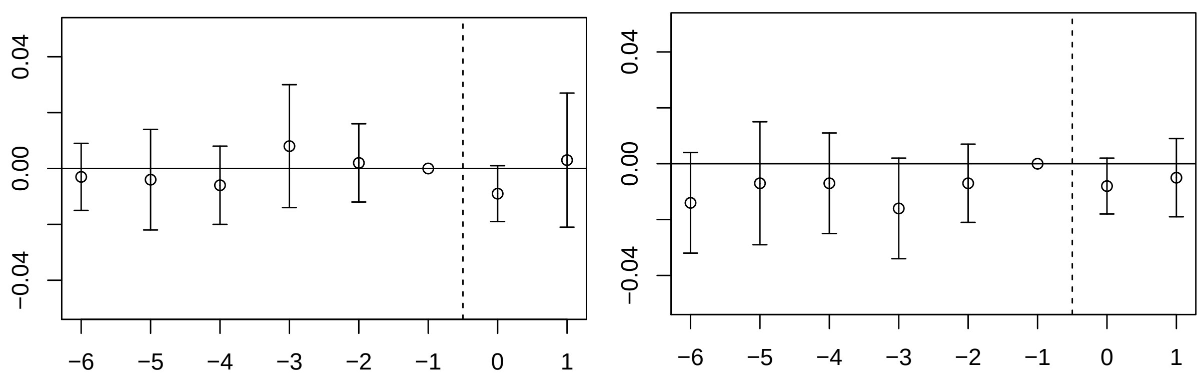
(D) HPSA counties

(E) Charlson score of 0 or 1

(F) Charlson score of 2 or more

Note: These figures report trends from 2008 to 2015 in the outcome variable in the expansion states relative to a synthetic control unit created by weighting the non-expansion states for the outcome of new patient visits. See the text for more details.

Figure 9. : 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 1—: Summary Statistics

Subsample:	All	Dually Eligible	High-Impact Zip Codes	HPSA Counties	0 or 1 Charlson Score	2+ Charlson Score
Share treated in 2014-2015	0.49	0.51	0.34	0.63	0.48	0.50
RVUs of PCP Services	15.09	20.86	14.77	14.91	6.24	20.61
Number of PCP Visits	9.39	11.76	9.08	9.18	4.39	12.51
Number of New Patient Visits	0.75	0.71	0.68	0.72	0.48	0.92
Number of Physician Visits	21.89	26.24	20.96	21.47	10.91	28.75
Days Between Physician Visits	22.61	22.78	23.53	23.15	30.45	19.19
Office-Based Expenditure	3,076	3,459	2,963	3,050	1,398	4,124
Number of Non-MD Visits	1.06	1.69	1.05	0.97	0.58	1.36
Overall Expenditure ($)	8,449	11,390	8,375	8,520	3,206	11,721
Obs: Most Variables	29,387,457	3,837,131	5,489,036	10,475,157	11,294,267	18,093,190
Obs: New Patient Visits	22,049,980	2,878,497	4,116,836	7,857,422	8,474,346	13,575,634
Obs: Days Between Visits	24,694,652	3,517,111	4,601,171	8,814,428	7,511,791	17,182,861

Note: This table reports the annual means of key variables between 2008 and 2015 across all subsamples considered in the analyses. New patient visits is observed 2010 to 2015 and days between visits is conditional on at least two visits, reducing the total number of person-year observations for these variables.

Table 2—: Difference-in-Difference Results: Health Insurance Coverage

Panel A: All		
	Uninsured	Covered by Medicaid
Post × Expansion	-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
Post × Expansion	-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
Post × Expansion	-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. 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 one-year ACS file beginning in 2012. Significance levels: *=10%, **=5%, ***=1%.

Table 3—: Difference-in-Difference 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	0.0600	-0.136	-0.267	0.234	-0.0596	0.138
	[-0.77,0.00]	[-1.11,0.83]	[-1.15,0.61]	[-0.73,1.19]	[-0.46,0.34]	[-0.86,1.14]
outcome mean	15.09	20.86	14.77	14.87	6.240	20.61
% of mean	0.40	-0.65	-1.81	1.57	-0.96	0.67
Panel B: Number of Primary Care Visits						
expansion×post	0.0393	-0.111	-0.200	0.0718	-0.0176	0.0669
	[-0.29,0.37]	[-0.51,0.29]	[-0.52,0.12]	[-0.32,0.47]	[-0.20,0.17]	[-0.33,0.46]
outcome mean	9.388	11.76	9.083	9.157	4.391	12.51
% of mean	0.42	-0.94	-2.20	0.78	-0.40	0.53
Panel C: New Patient Visits						
expansion×post	0.00469	0.00353	-0.00259	0.00251	0.00284	0.00350
	[-0.01,0.01]	[-0.02,0.03]	[-0.02,0.01]	[-0.01,0.01]	[-0.01,0.02]	[-0.01,0.01]
outcome mean	0.748	0.714	0.679	0.718	0.479	0.916
% of mean	0.63	0.49	-0.38	0.35	0.59	0.38
N	28,797,462	3,835,610	5,488,541	10,503,949	10,791,858	18,005,604

Note: This table reports difference-in-difference 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 4—: Difference-in-Difference 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 Office Visits						
expansion×post	0.254	-0.0972	-0.146	0.255	0.0973	0.322
	[-0.31,0.00]	[-0.70,0.51]	[-0.65,0.36]	[-0.43,0.94]	[-0.31,0.50]	[-0.32,0.97]
outcome mean	21.89	26.24	20.96	21.41	10.91	28.75
% of mean	1.16	-0.37	-0.70	1.19	0.89	1.12
Panel B: Days Between Office Visits						
expansion×post	-0.152	0.111	-0.171	-0.0414	-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: Office-Based Expenditures						
expansion×post	25.87	-16.84	-26.50	38.24	6.971	29.90
	[-50.22,101.96]	[-101.28,67.60]	[-91.71,38.71]	[-59.88,136.36]	[-57.24,71.18]	[-59.14,118.94]
outcome mean	3076	3459	2963	3041	1398	4124
% of mean	0.84	-0.49	-0.89	1.26	0.50	0.73
N	28,797,462	3,835,610	5,488,541	10,503,949	10,791,858	18,005,604

Note: This table reports difference-in-difference estimates comparing expansion and non-expansion states 2008 to 2015. Each column represents a different sample and each panel a different outcome. The days between visits measure is not defined for individuals with a single visit, 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. * signifies statistical significance at the 5% level.

Table 5—: Synthetic Control Estimates: 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						
Estimate	0.011	-0.047	0.253	0.304	0.183	-0.028
Confidence Interval:	[-0.264, 0.263]	[-0.68, 0.66]	[-0.47, 0.775]	[-0.061, 0.681]	[-0.172, 0.487]	[-0.372, 0.302]
outcome mean	15.09	20.86	14.77	14.87	6.240	20.61
% of mean	0.07	0.23	1.71	2.05	2.93	-0.14
Panel B: Number of Primary Care Visits						
Estimate	-0.033	-0.130	0.024	-0.002	0.153	-0.095
Confidence Interval:	[-0.151, 0.085]	[-0.431, 0.225]	[-0.275, 0.417]	[-0.169, 0.189]	[-0.009, 0.292]	[-0.248, 0.054]
outcome mean	9.39	11.76	9.08	9.16	4.39	12.51
% of mean	-0.35	-1.1	0.26	-0.02	3.5	-0.76
Panel C: New Patient Visits						
Estimate	0.00005	-0.017	-0.008	-0.001	0.013	-0.001
Confidence Interval:	[-0.013, 0.013]	[-0.037, 0.003]	[-0.034, 0.039]	[-0.014, 0.012]	[-0.001, 0.025]	[-0.018, 0.014]
outcome mean	0.748	0.714	0.679	0.718	0.479	0.916
% of mean	0.006	2.3	-1.2	-0.13	2.7	0.15

Note: This table presents estimates comparing states that expanded Medicaid eligibility in 2014 to a synthetic control unit. 95 percent confidence intervals are reported under the estimate. Significance levels: *=5%.

Table 6—: Synthetic Control Estimates: Other Services

Subsample:	All	Dually Eligible	High-Impact Zip Codes	HPSA Counties	0 or 1 Charlson Score	2+ Charlson Score
Panel A: Number of Non-MD Office Visits						
Estimate	-0.012	-0.029	-0.061	-0.097*	-0.025	-0.009
Confidence Interval	[-0.075, 0.061]	[-0.155, 0.12]	[-0.226, 0.096]	[-0.178, -0.016]	[-0.077, 0.031]	[-0.083, 0.08]
outcome mean	1.689	1.051	1.051	0.964	0.577	1.362
% of mean	-7.2	-2.3	-5.8	-10.0	-4.3	-0.65
Panel B: Overall Expenditures						
Estimate	56.12	38.71	242.98	191.55	96.36	282.83
Confidence Interval	[-88.81, 214.73]	[-380.77, 365.10]	[-121.11, 641.15]	[-31.01, 418.80]	[-58.91, 265.07]	[-10.38, 573.01]
outcome mean	8449	11390	8375	8520	3206	11721
% of mean	0.7	0.3	2.9	2.2	3.0	2.4
Panel C: Office-based Expenditures						
Estimate	-38.30	-41.38	13.67	42.36	42.07	-17.67
Confidence Interval	[-45.61, 54.73]	[-99.40, 96.58]	[-96.33, 89.49]	[-54.20, 74.32]	[-54.99, 58.70]	[-63.35, 71.96]
outcome mean	3076	3459	2963	3041	1398	4124
% of mean	-1.2	-1.2	0.5	1.4	3.0	-0.4
Panel D: Number of Office Visits						
Estimate	-0.106	-0.490	0.162	0.148	0.206	-0.136
Confidence Interval	[-0.484, 0.179]	[-1.16, 0.109]	[-0.822, 0.872]	[-0.225, 0.694]	[-0.257, 0.528]	[-0.551, 0.201]
outcome mean	21.89	26.24	20.96	21.41	10.91	28.75
% of mean	-0.48	-1.9	0.77	0.69	1.9	-0.47
Panel E: Days Between Office Visits						
Estimate	0.137	0.158	0.086	-0.165	-0.588*	0.139
Confidence Interval	[-0.165, 0.403]	[-0.28, 0.602]	[-1.305, 0.871]	[-0.848, 0.282]	[-1.058, -0.127]	[-0.132, 0.351]
outcome mean	22.61	22.78	23.53	23.15	30.45	19.19
% of mean	0.60	0.69	0.37	-0.71	-1.9	0.73

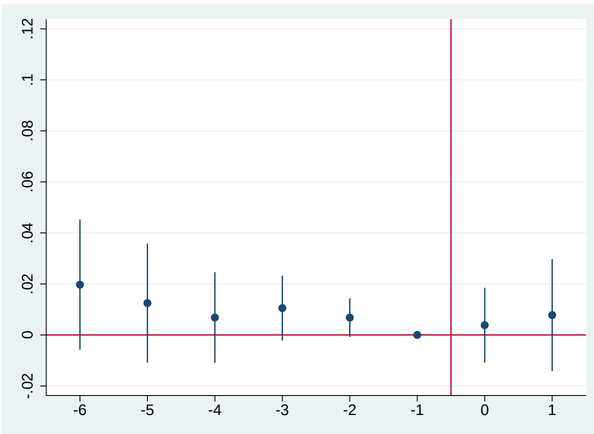
Note: This table presents estimates comparing states that expanded Medicaid eligibility in 2014 to a synthetic control unit. 95 percent confidence intervals are reported under the estimate. Significance levels: *=5%.

Table 7—: Difference-in-Difference Results: Alternative Crowding Measures

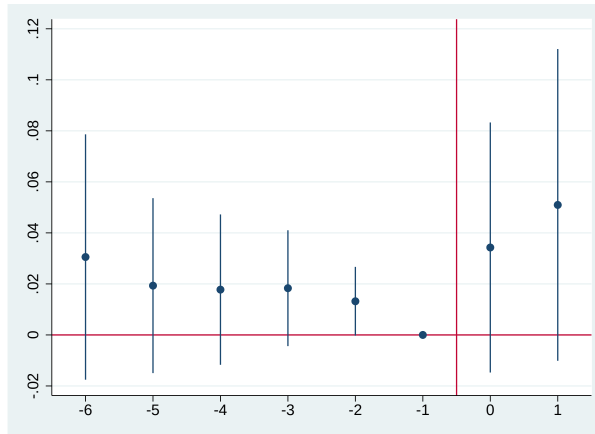
	Delayed Medical Care Due to No Appointment Availability	Delayed Medical Care Due to Long Wait Times in Waiting Room
Treated × Post Implementation	-0.004	0.001
	(0.004)	(0.004)
N:	48,508	48,508
Outcome Mean:	0.058	0.045
% of Mean:	-6.9	2.2

Note: This table presents difference-in-difference estimates using data from the 2008 to 2015 National Health Interview Survey from respondents age 65 and older. The dependent variable in Column 1 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 Column 2 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.

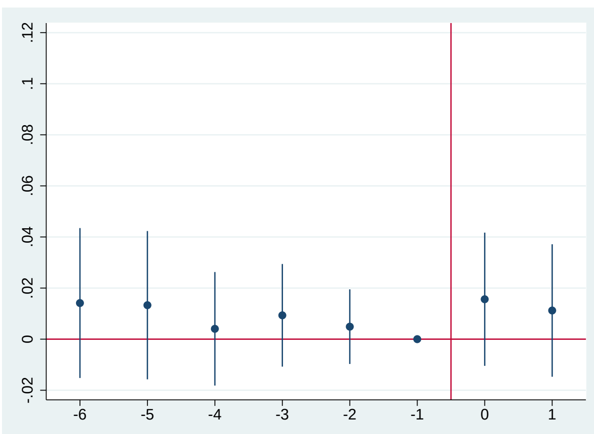
Figure A.1. : Trends in Medicare Advantage Enrollment in Expansion vs. Non-Expansion States, 2008-2015



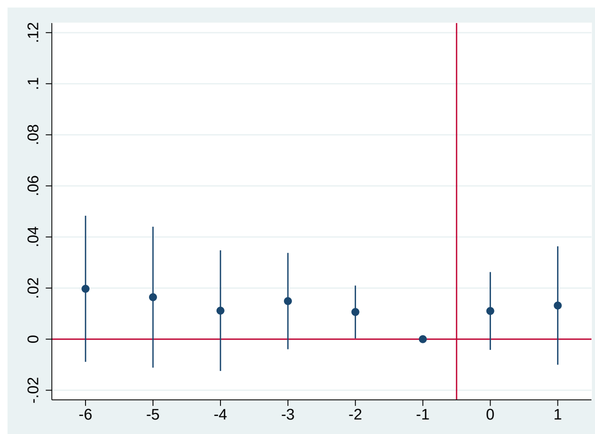
(A) All Medicare



(B) Dually Eligible



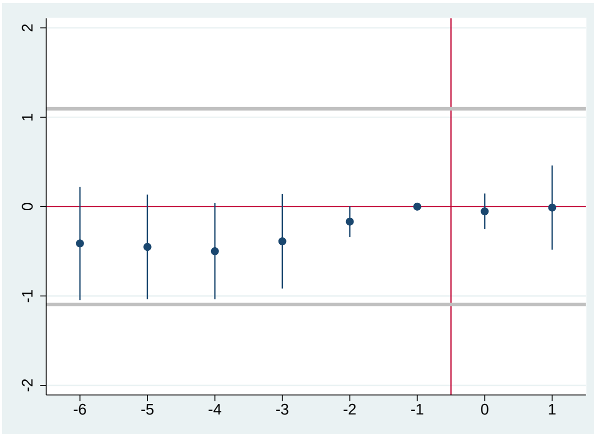
(C) High-impact zip codes



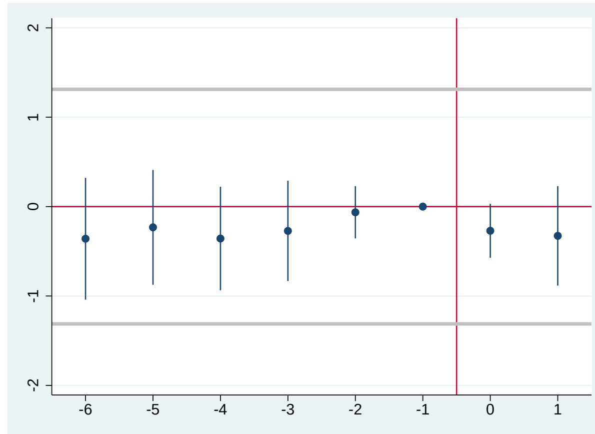
(D) HPSA counties

Note: These figures report coefficients from the estimation of Equation 1 for the outcome Medicare Advantage enrollment, with the coefficients representing the deviation from the mean difference between expansion and non-expansion states in the outcome in the six years before and two years after expansion. In contrast to all other analyses, these samples are not limited to fee-for-service enrollees.

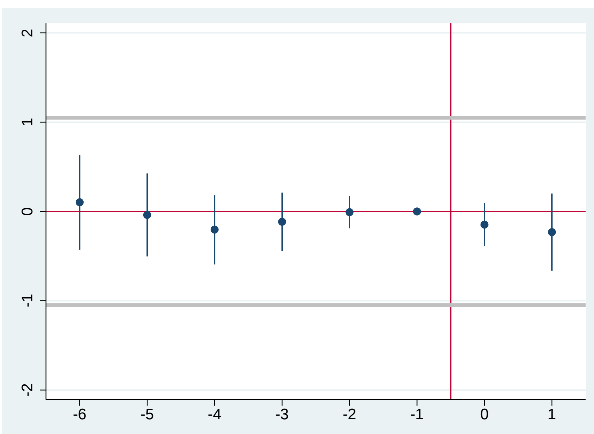
Figure A.2. : Trends in Number of Office Visits in Expansion vs. Non-Expansion States, 2008-2015



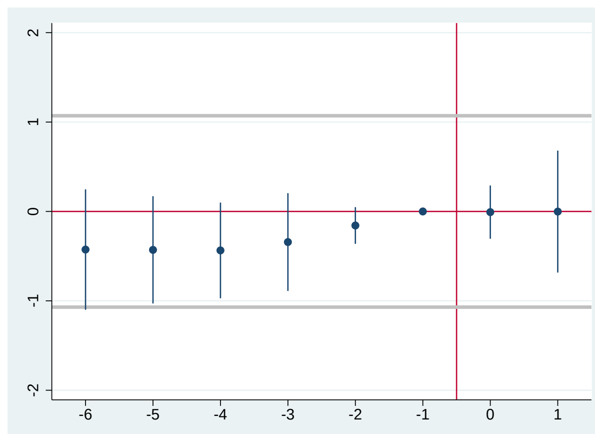
(A) All Medicare



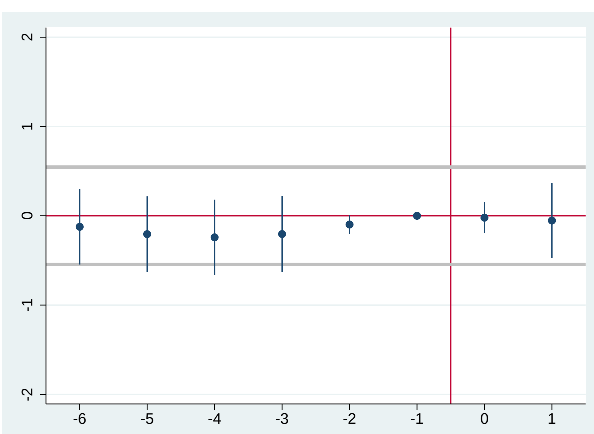
(B) Dually Eligible



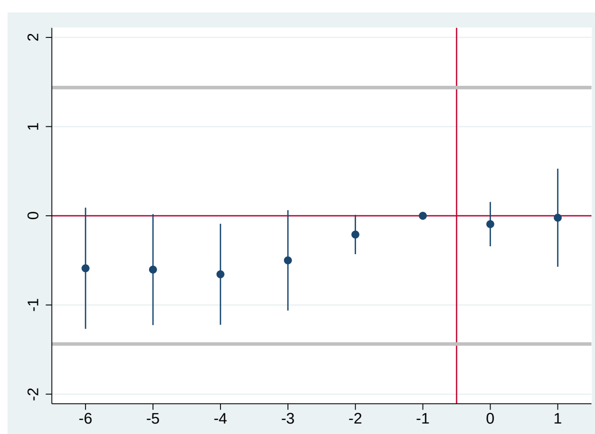
(C) High-impact zip codes



(D) HPSA counties



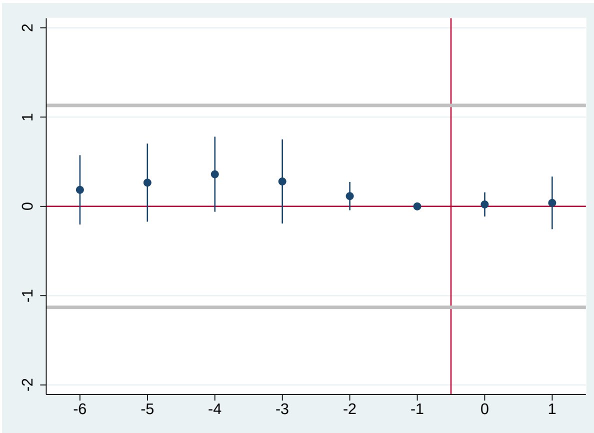
(E) Charlson score of 0 or 1



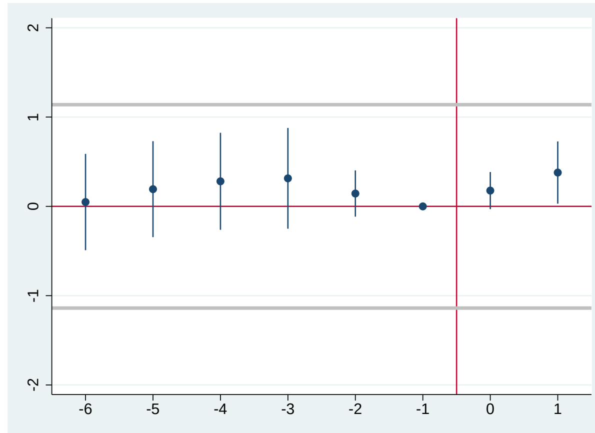
(F) Charlson score of 2 or more

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of number of office 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.

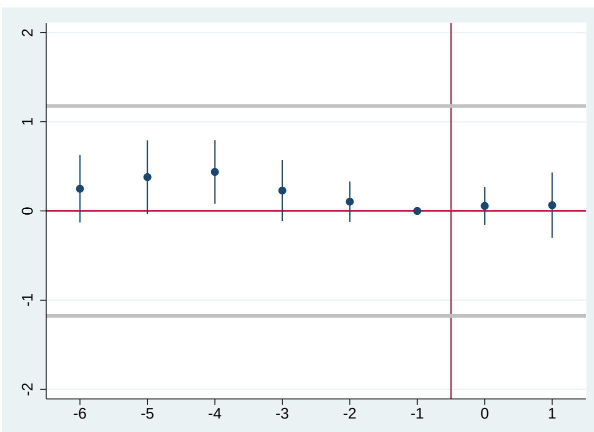
Figure A.3. : Trends in Days Between Visits in Expansion vs. Non-Expansion States, 2008-2015



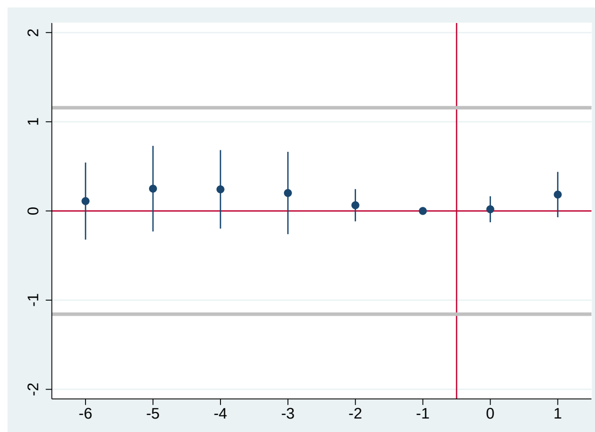
(A) All Medicare



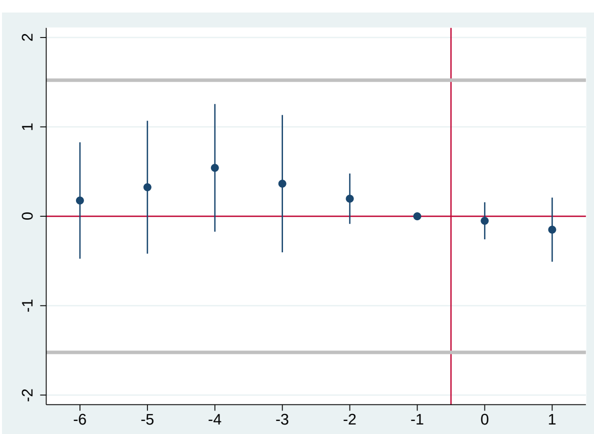
(B) Dually Eligible



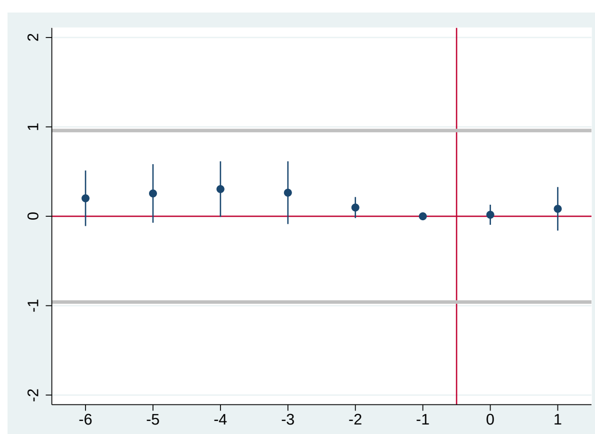
(C) High-impact zip codes



(D) HPSA counties



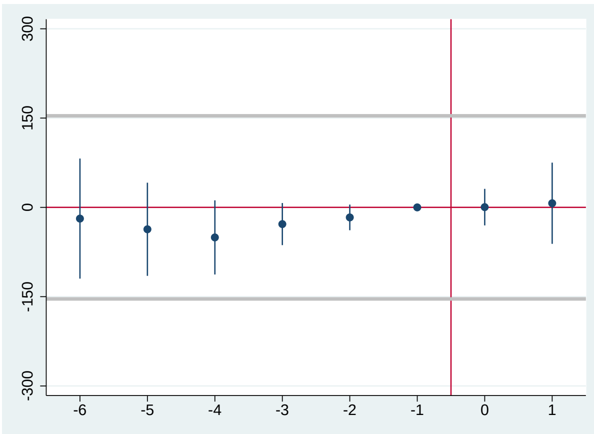
(E) Charlson score of 0 or 1



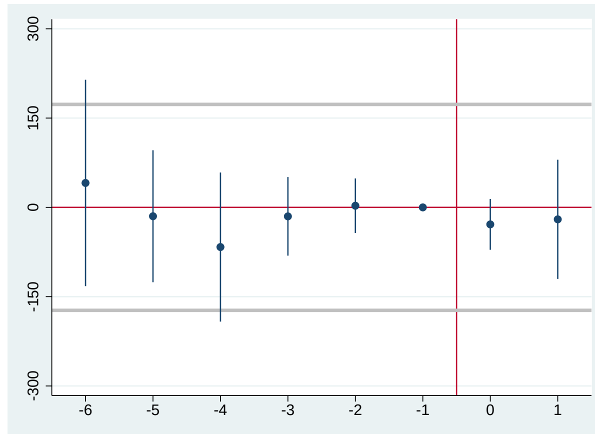
(F) Charlson score of 2 or more

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of days between 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.

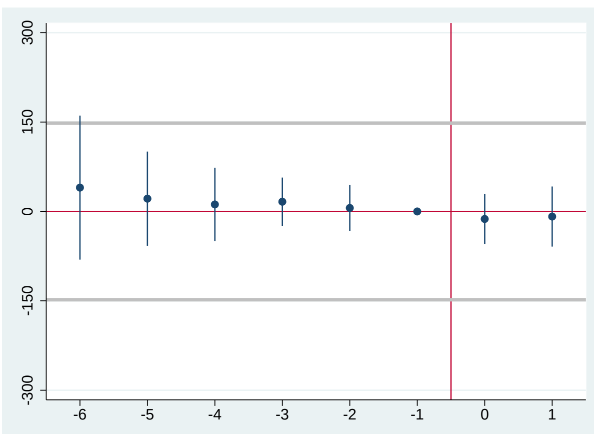
Figure A.4. : Trends in Office-Based Expenditures in Expansion vs. Non-Expansion States, 2008-2015



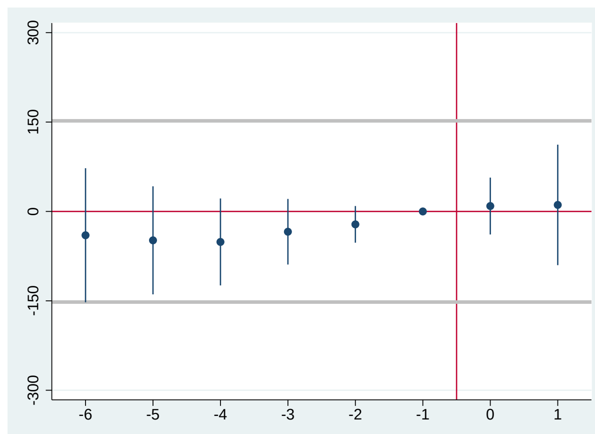
(A) All Medicare



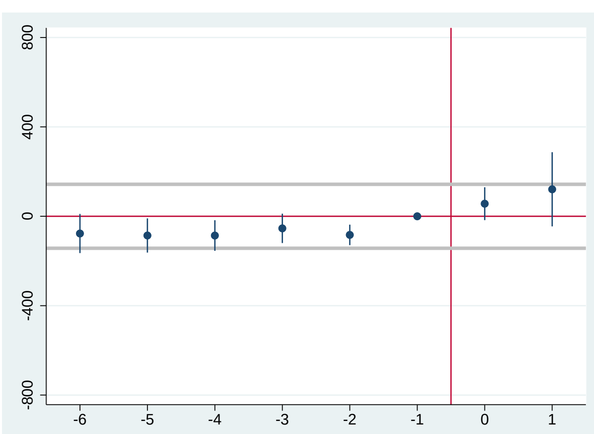
(B) Dually Eligible



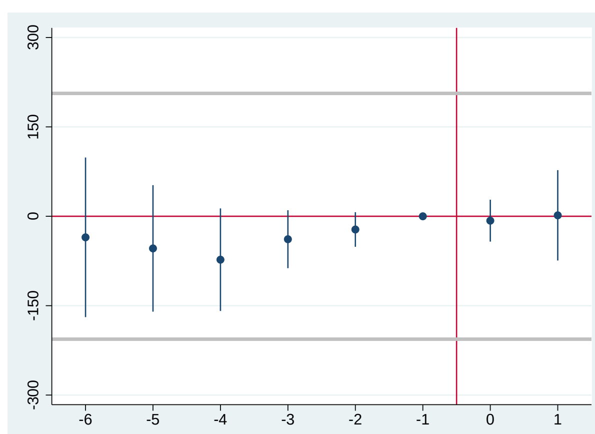
(C) High-impact zip codes



(D) HPSA counties



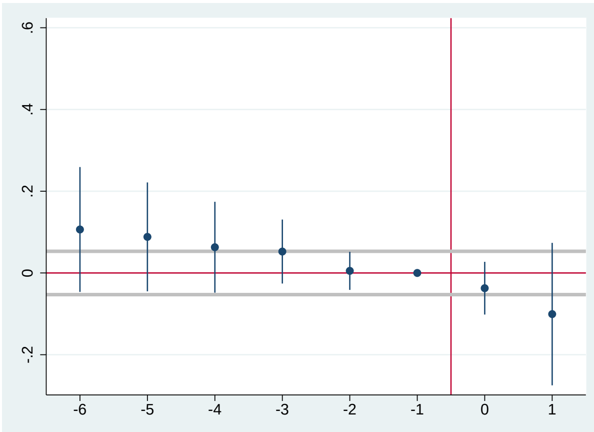
(E) Charlson score of 0 or 1



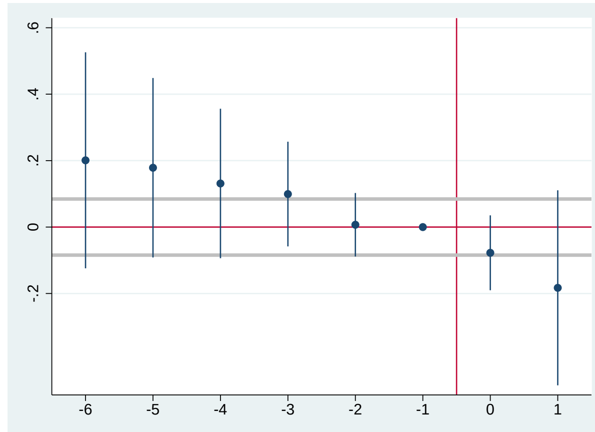
(F) Charlson score of 2 or more

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of office-based (Carrier) expenditures, 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.

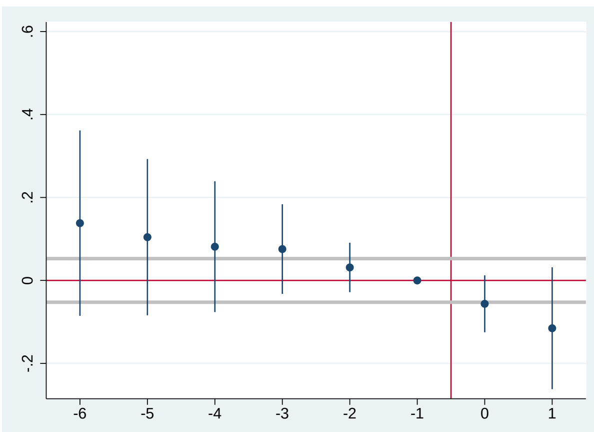
Figure A.5. : Trends in Non-MD Visits in Expansion vs. Non-Expansion States, 2008-2015



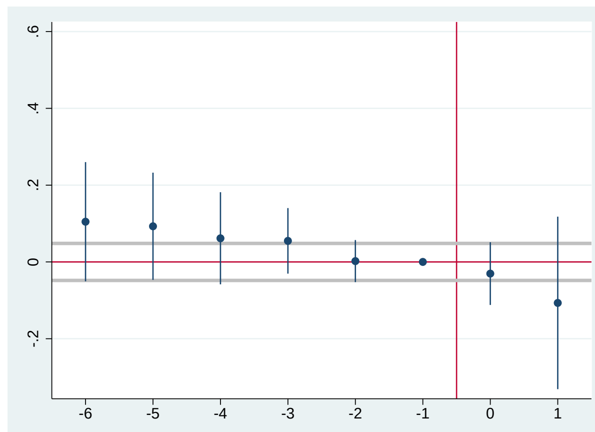
(A) All Medicare



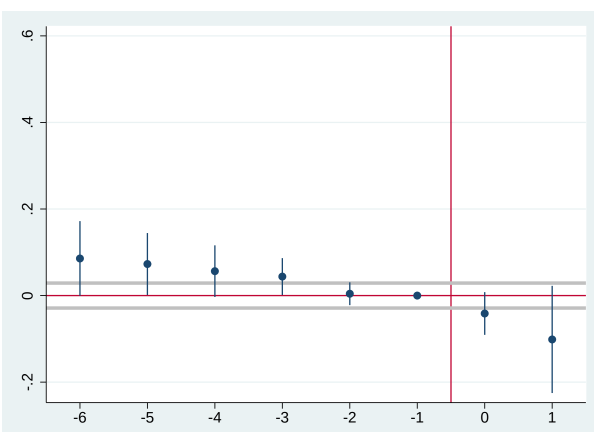
(B) Dually Eligible



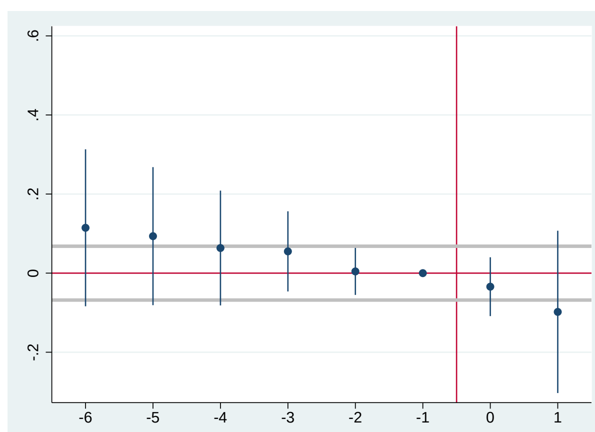
(C) High-impact zip codes



(D) HPSA counties



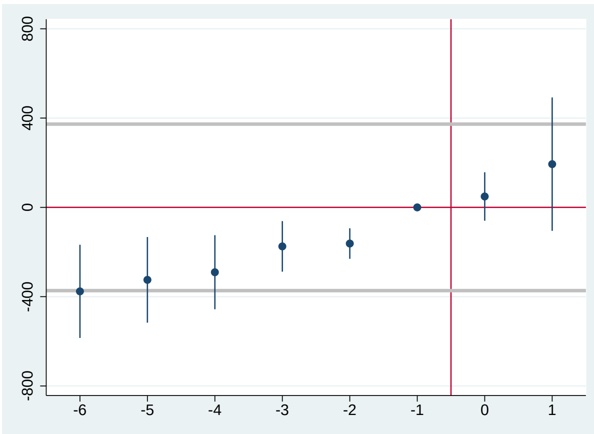
(E) Charlson score of 0 or 1



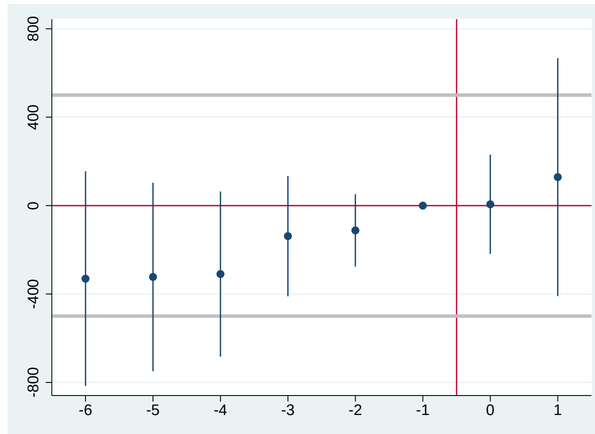
(F) Charlson score of 2 or more

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of office visits with services from a non-MD (mid-level) provider, 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.

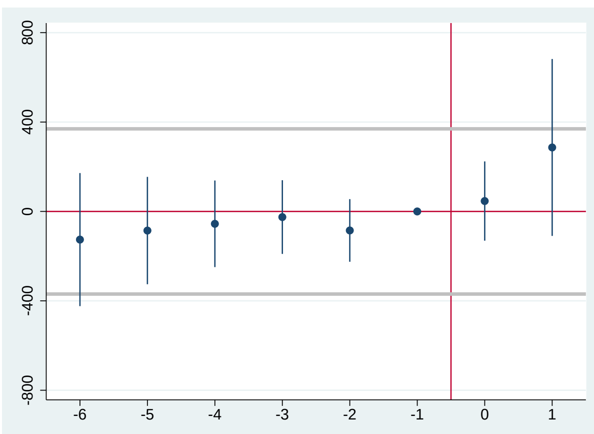
Figure A.6. : Trends in Overall Expenditures in Expansion vs. Non-Expansion States, 2008-2015



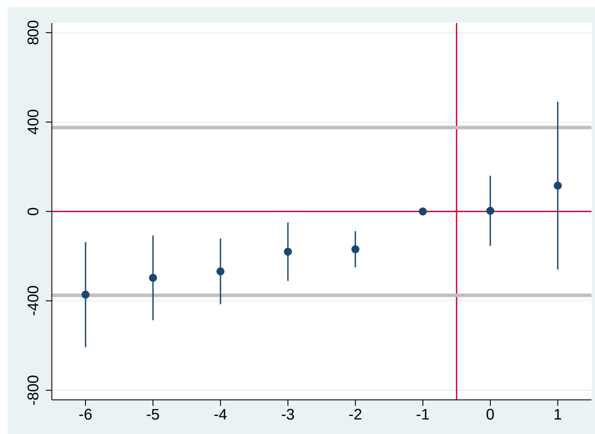
(A) All Medicare



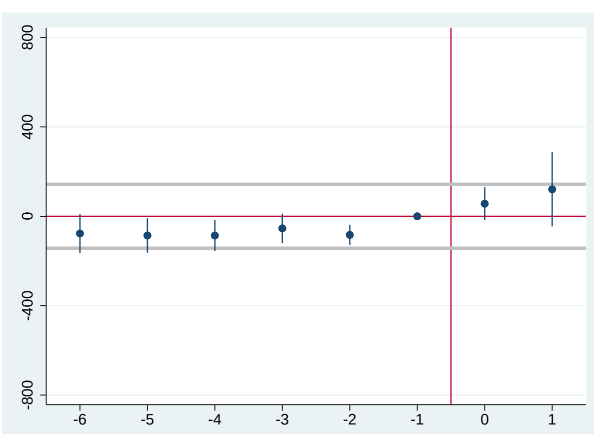
(B) Dually Eligible



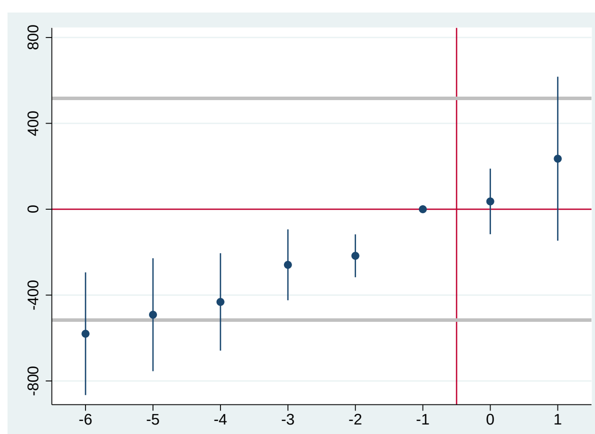
(C) High-impact zip codes



(D) HPSA counties



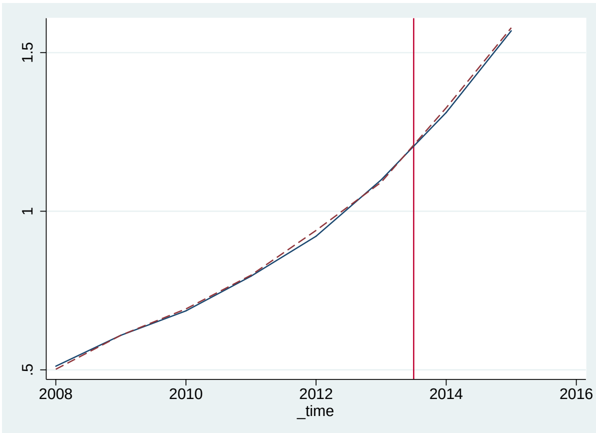
(E) Charlson score of 0 or 1



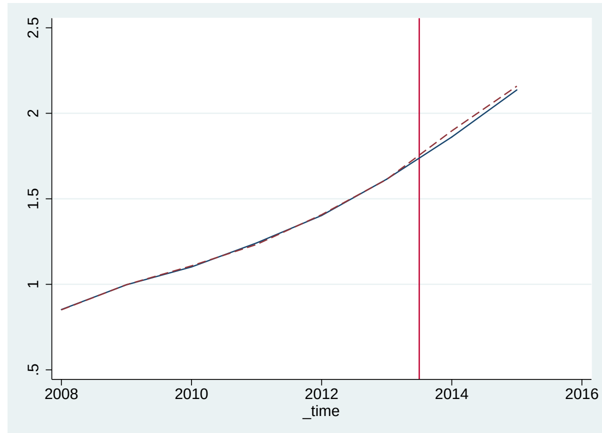
(F) Charlson score of 2 or more

Note: These figures report coefficients from the estimation of Equation 1 for the outcome of overall expenditures, 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.

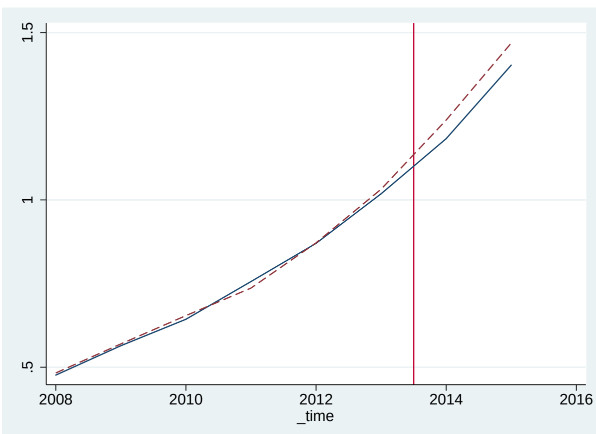
Figure A.7. : Trends in Non-MD Visits in Expansion States (solid) vs. Synthetic Control (dotted), 2008-2015



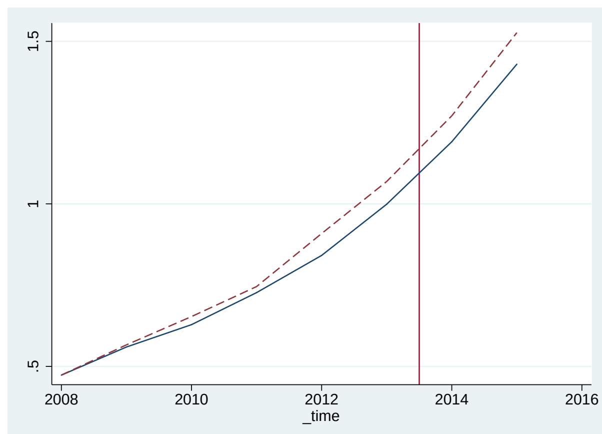
(A) All Medicare



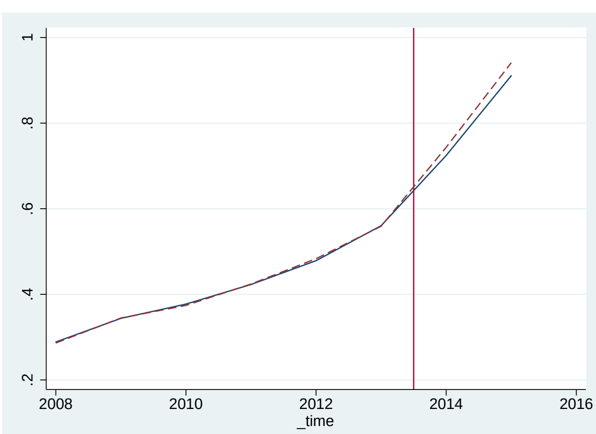
(B) Dually Eligible



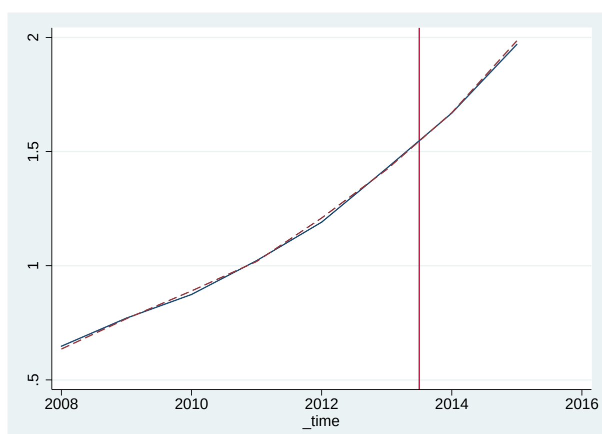
(C) High-impact zip codes



(D) HPSA counties



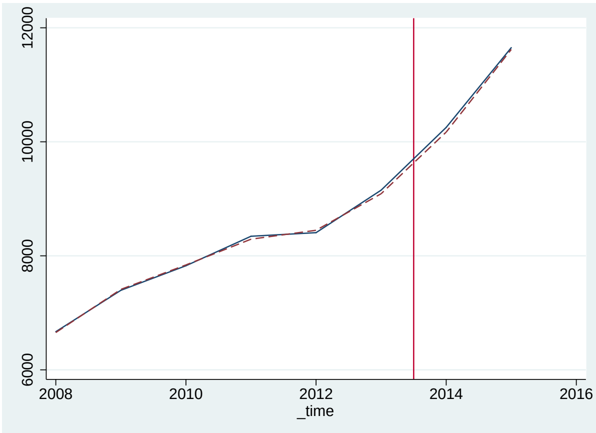
(E) Charlson score of 0 or 1



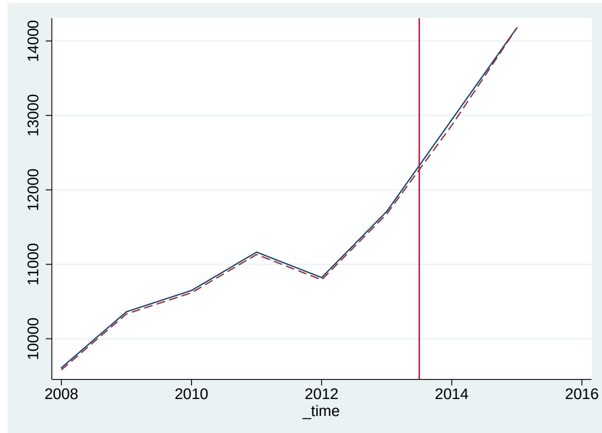
(F) Charlson score of 2 or more

Note: These figures report trends from 2008 to 2015 in the outcome variable in the expansion states relative to a synthetic control unit created by weighting the non-expansion states for the outcome of number of non-MD office visits. See the text for more details.

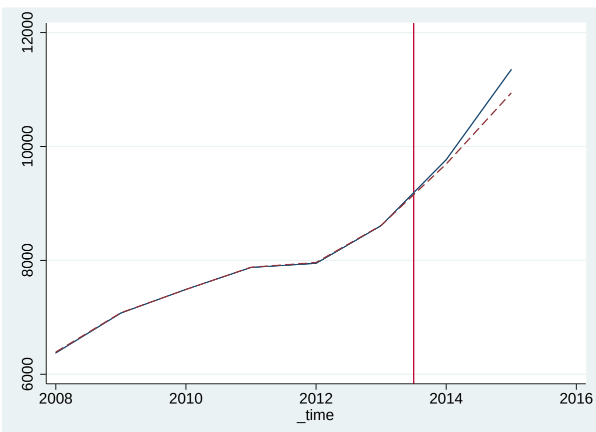
Figure A.8. : Overall Expenditures in Expansion States (solid) vs. Synthetic Control (dotted), 2008-2015



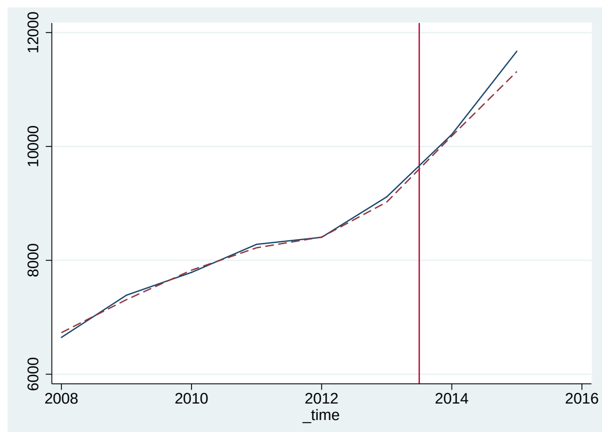
(A) All Medicare



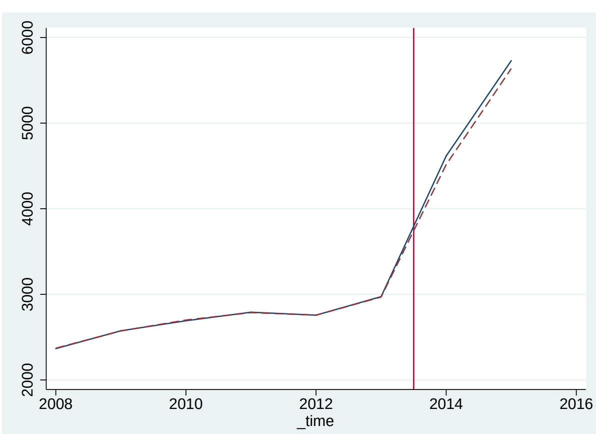
(B) Dually Eligible



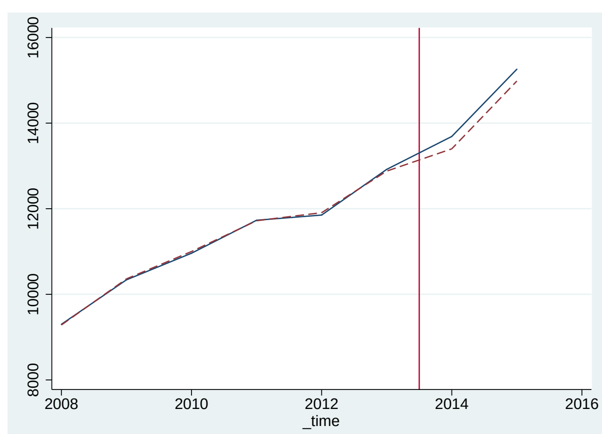
(C) High-impact zip codes



(D) HPSA counties



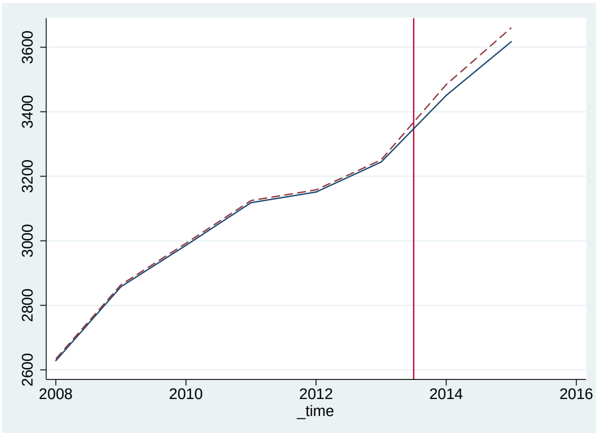
(E) Charlson score of 0 or 1



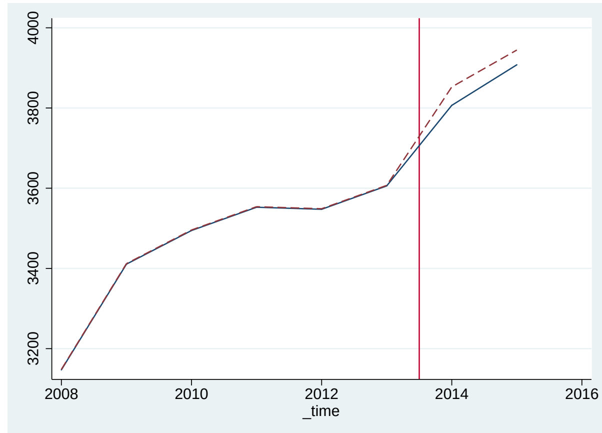
(F) Charlson score of 2 or more

Note: These figures report trends from 2008 to 2015 in the outcome variable in the expansion states relative to a synthetic control unit created by weighting the non-expansion states for the outcome of total expenditures. See the text for more details.

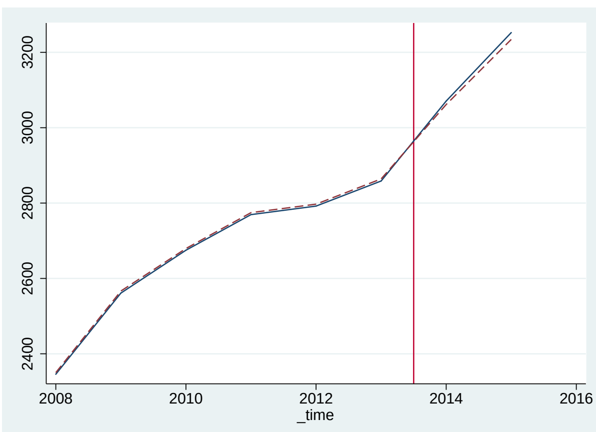
Figure A.9. : Office-based Expenditures in Expansion States (solid) vs. Synthetic Control (dotted), 2008-2015



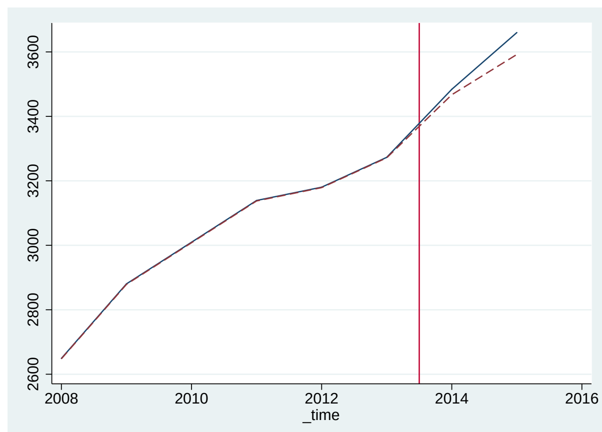
(A) All Medicare



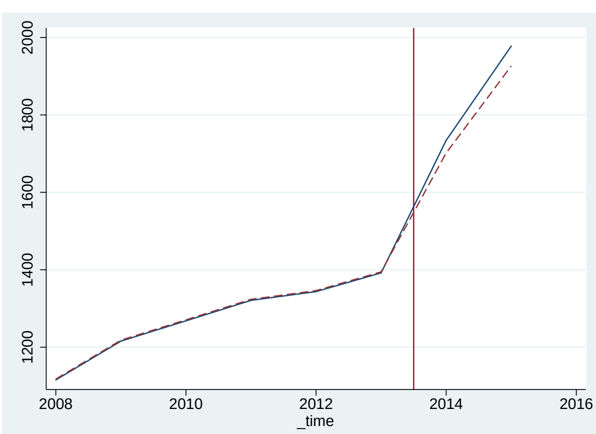
(B) Dually Eligible



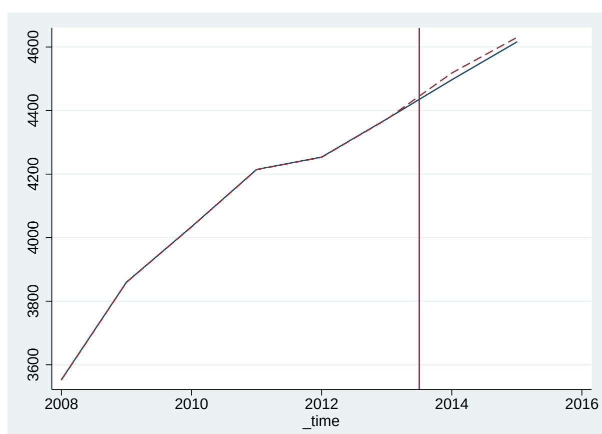
(C) High-impact zip codes



(D) HPSA counties



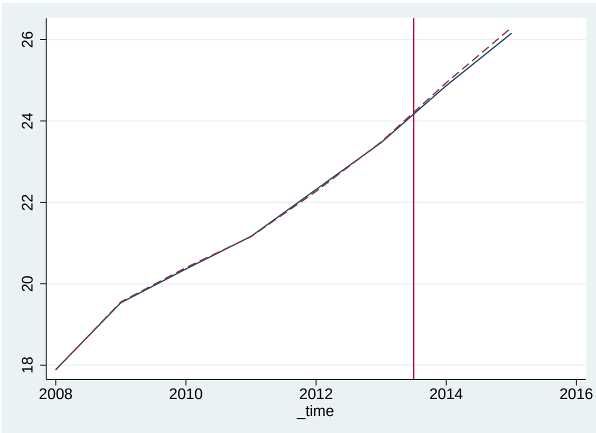
(E) Charlson score of 0 or 1



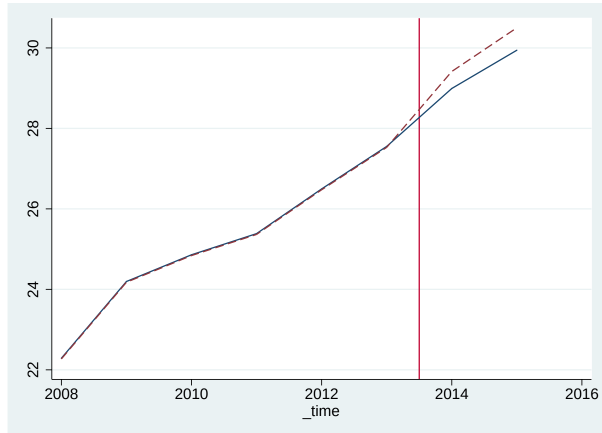
(F) Charlson score of 2 or more

Note: These figures report trends from 2008 to 2015 in the outcome variable in the expansion states relative to a synthetic control unit created by weighting the non-expansion states for the outcome of office-based expenditures. See the text for more details.

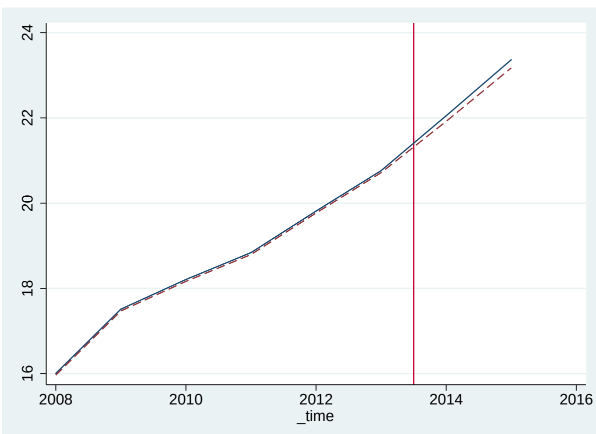
Figure A.10. : Number of Office Visits in Expansion States (solid) vs. Synthetic Control (dotted), 2008-2015



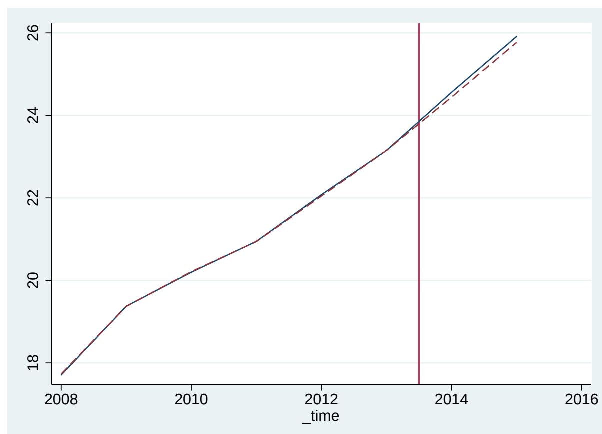
(A) All Medicare



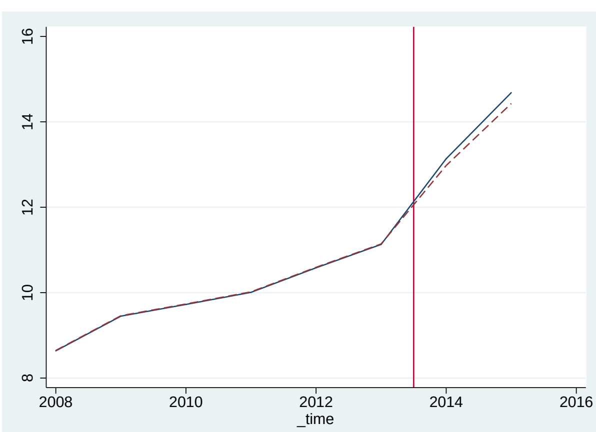
(B) Dually Eligible



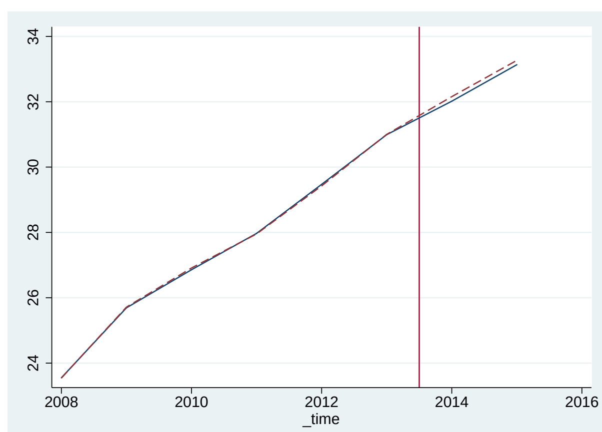
(C) High-impact zip codes



(D) HPSA counties



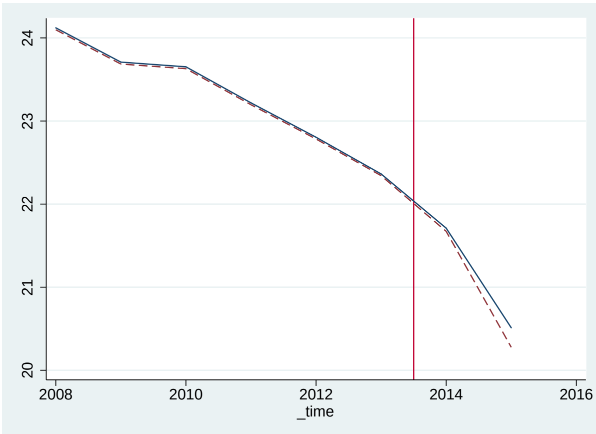
(E) Charlson score of 0 or 1



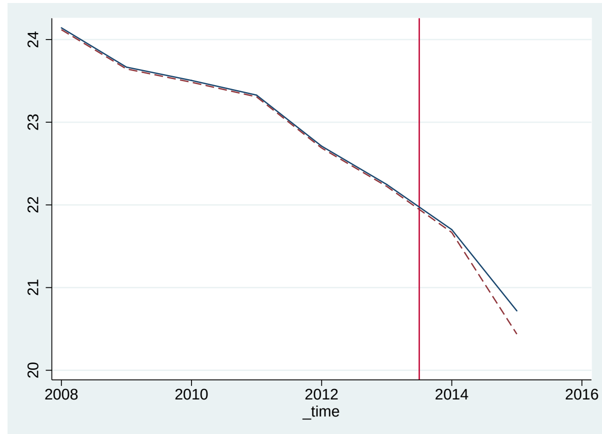
(F) Charlson score of 2 or more

Note: These figures report trends from 2008 to 2015 in the outcome variable in the expansion states relative to a synthetic control unit created by weighting the non-expansion states for the outcome of number of office visits. See the text for more details.

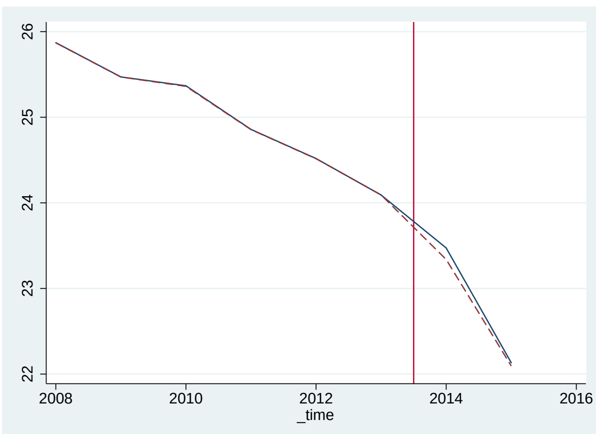
Figure A.11. : Days Between Visits in Expansion States (solid) vs. Synthetic Control (dotted), 2008-2015



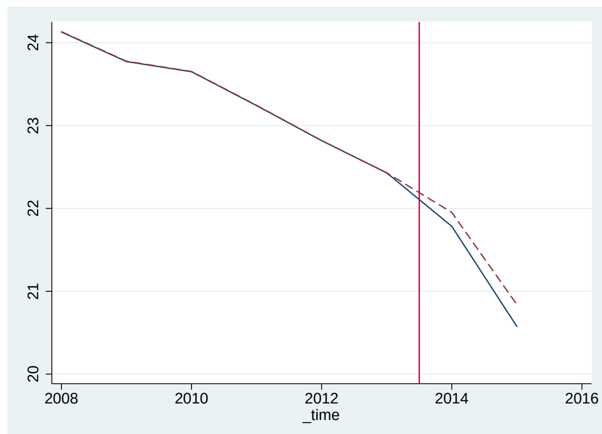
(A) All Medicare



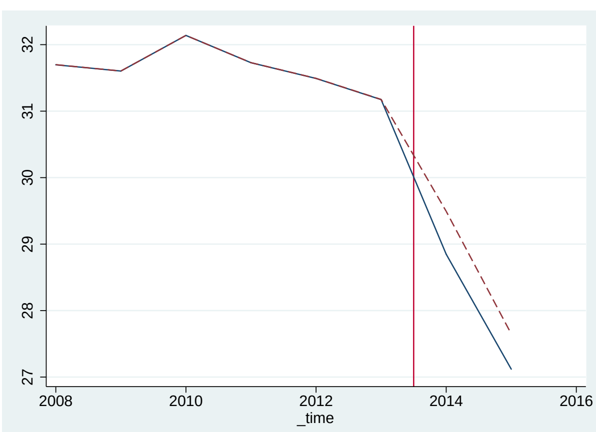
(B) Dually Eligible



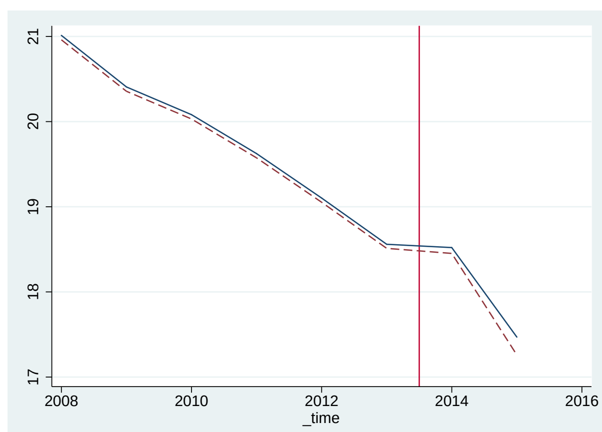
(C) High-impact zip codes



(D) HPSA counties



(E) Charlson score of 0 or 1



(F) Charlson score of 2 or more

Note: These figures report trends from 2008 to 2015 in the outcome variable in the expansion states relative to a synthetic control unit created by weighting the non-expansion states for the outcome of days between visits. See the text for more details.

Table A.1—: Difference-in-Difference Results: Primary Care Services, Excluding 2015

Subsample:	All	Dually Eligible	Low-Income Zip Codes	HPSA Counties	0 or 1 Charlson Score	2+ Charlson Score
Panel A: Primary Care RVUs						
expansion×post	-0.0711	-0.317	-0.358	0.0853	-0.0938	-0.0464
	[-0.73,0.59]	[-1.07,0.43]	[-1.01,0.29]	[-0.66,0.83]	[0.23,0.23]	[-0.84,0.74]
outcome mean	15.09	20.86	14.77	14.91	6.240	20.61
% of mean	-0.47	-1.519654842	-2.423832092	0.572099262	-1.503205128	-0.22513343
Panel B: Number of Primary Care Visits						
expansion×post	-0.0263	-0.185	-0.196	0.00120	-0.0409	-0.0184
	[-0.30,0.25]	[-0.53,0.16]	[-0.44,0.05]	[-0.31,0.31]	[-0.19,0.11]	[-0.35,0.31]
outcome mean	9.388	11.76	9.083	9.182	4.391	12.51
% of mean	-0.28	-1.57	-2.16	0.01	-0.93	-0.15
Panel C: New Patient Visits						
expansion×post	0.00216	-0.00356	-0.00337	0.000774	0.00241	8.42e-05
	[-0.01,0.01]	[-0.02,0.02]	[-0.02,0.01]	[-0.01,0.01]	[-0.01,0.02]	[-0.01,0.01]
outcome mean	0.748	0.714	0.679	0.720	0.479	0.916
% of mean	0.29	-0.50	-0.50	0.11	0.50	0.01
N	25,193,056	3,355,705	4,802,437	9,165,128	9,441,412	15,751,644

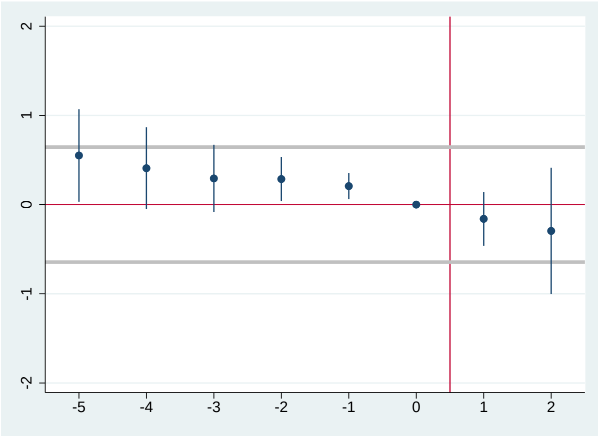
Note: This table reports difference-in-difference estimates comparing expansion and non-expansion states 2008 to 2014. Each column represents a different sample and each panel a different outcome. Because of data availability as described in the text, the analysis that uses new patient visits excludes 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. This table differs from Table 3 by excluding 2015.

Table A.2—: Difference-in-Difference Results: Primary Care Services, Large Expansions

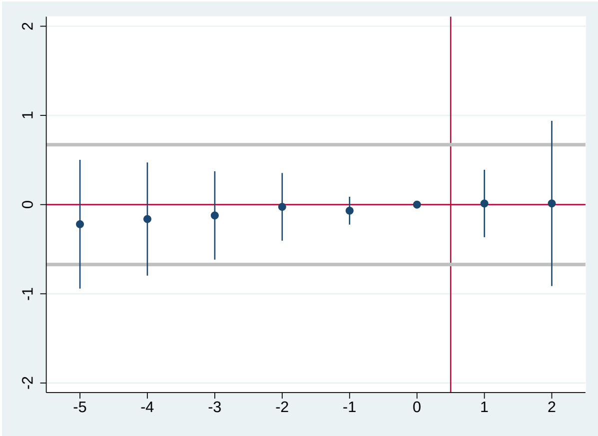
Subsample:	All	Dually Eligible	High Impact Zip Codes	HPSA Counties	0 or 1 Charlson Score	2+ Charlson Score
Panel A: Primary Care RVUs						
expansion×post	0.241	-0.353	-0.222	0.387	0.0684	0.330
	[-0.727 , 1.209]	[-1.494 , 0.788]	[-1.308 , 0.864]	[-0.716 , 1.49]	[-0.377 , 0.513]	[-0.828 , 1.488]
outcome mean	15.17	20.95	14.71	14.83	6.271	20.67
% of mean	1.6	-1.7	-1.5	2.6	1.1	1.6
Panel B: Number of Primary Care Visits						
expansion×post	0.116	-0.0879	-0.122	0.151	0.0214	0.160
	[-0.249 , 0.481]	[-0.486 , 0.31]	[-0.489 , 0.245]	[-0.239 , 0.541]	[-0.186 , 0.229]	[-0.273 , 0.593]
outcome mean	9.432	11.79	9.083	9.182	4.411	12.54
% of mean	1.2	-0.7	-2.16	1.7	0.5	1.3
Panel C: New Patient Visits						
expansion×post	0.0029	0.00715	0.000504	0.00245	0.000692	0.00299
	[-0.009 , 0.015]	[-0.017 , 0.031]	[-0.014 , 0.015]	[-0.009 , 0.014]	[-0.015 , 0.016]	[-0.009 , 0.015]
outcome mean	0.756	0.714	0.685	0.724	0.479	0.923
% of mean	0.4	1.0	0.1	0.3	0.50	0.3
N	22,655,029	2,838,571	4,966,233	5,480,615	6,294,075	10,710,121

Note: This table reports difference-in-difference 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. This table differs from Table 3 because it includes only states that had eligibility expansions of at least 10 percentage points in the treatment group, and excludes Wisconsin from the control group.

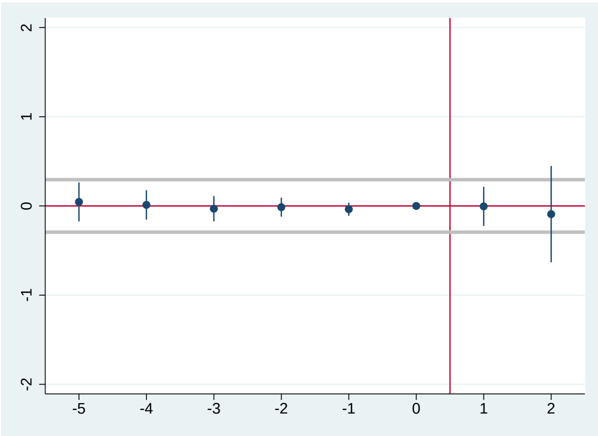
Figure A.12. : Trends in Primary Care RVUs in Expansion vs. Non-Expansion States Excluding Dual Eligibles, 2008-2015



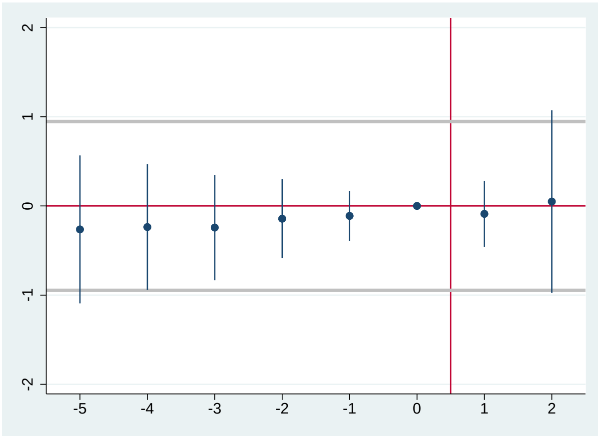
(A) Non-duals in High-impact zip codes



(B) Non-duals in HPSA counties



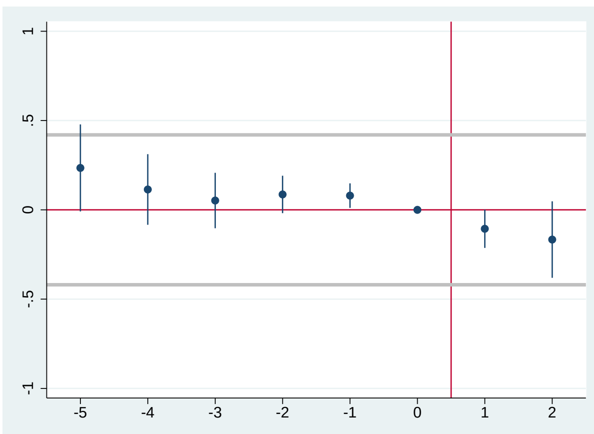
(C) Non-duals with Charlson score of 0 or 1



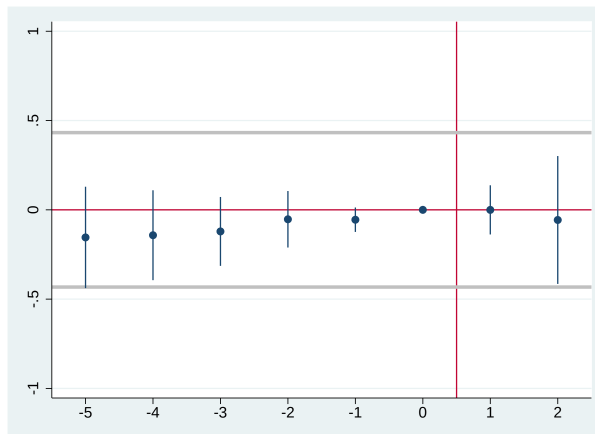
(D) Non-duals with Charlson score of 2 or more

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. This figure differs from Figure 3 in that it excludes the dually eligible.

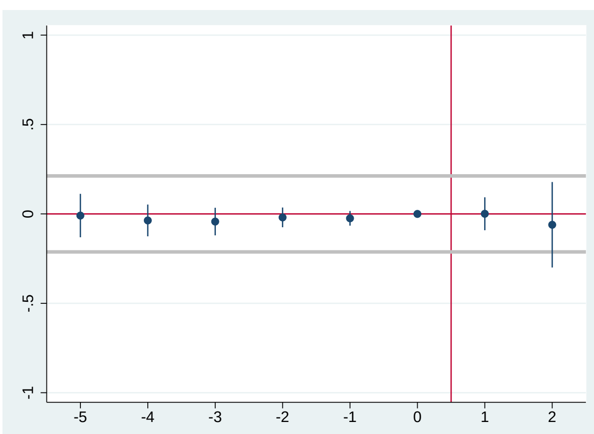
Figure A.13. : Trends in Primary Care Visits in Expansion vs. Non-Expansion States Excluding Dual Eligibles, 2008-2015



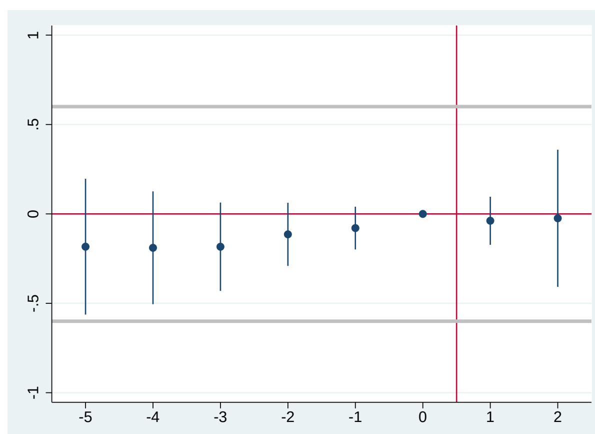
(A) Non-duals in high impact zip codes



(B) Non-duals in HPSA counties



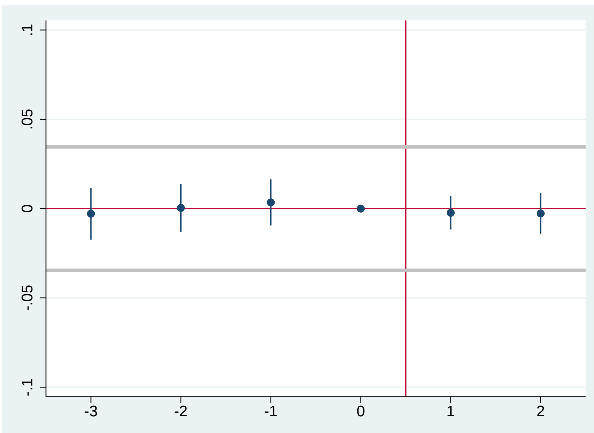
(C) Non-duals with Charlson score of 0 or 1



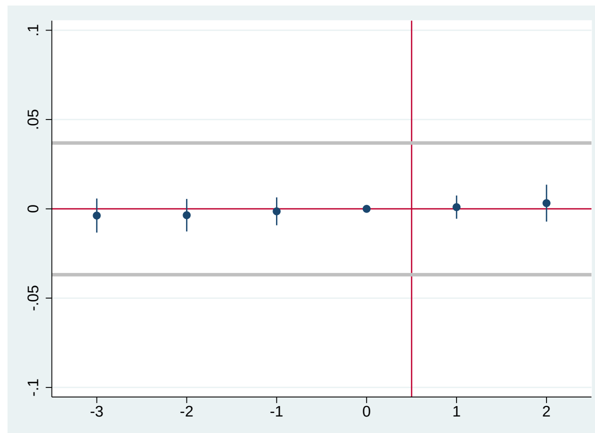
(D) Non-duals with Charlson score of 2 or more

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. This figure differs from Figure 4 in that it excludes the dually eligible.

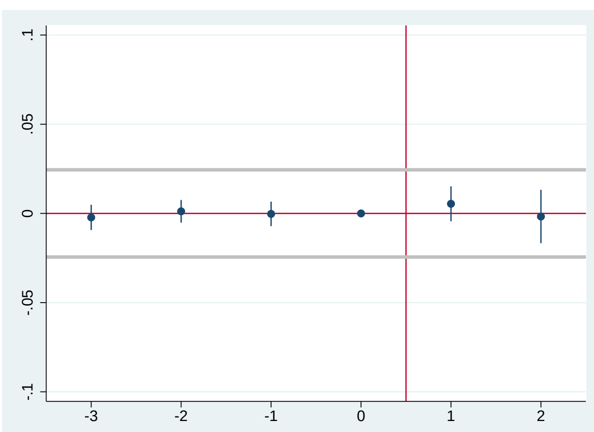
Figure A.14. : Trends in New Patient Visits in Expansion vs. Non-Expansion States Excluding Dual Eligibles, 2008-2015



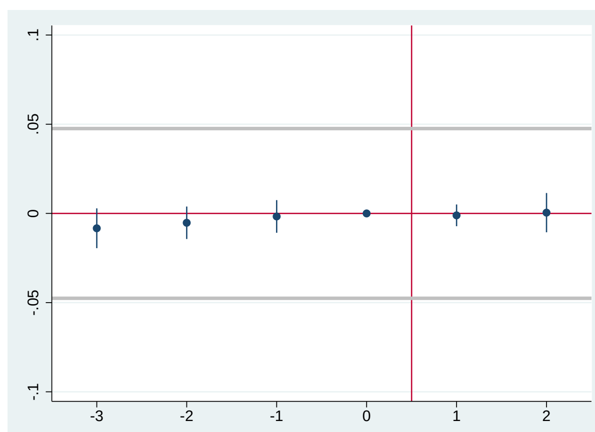
(A) Non-duals in High-impact zip codes



(B) Non-duals in HPSA counties



(C) Non-duals with Charlson score of 0 or 1



(D) Non-duals with 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. This figure differs from Figure 5 in that it excludes the dually eligible.