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Abstract

The 2014 Medicaid expansion excluded Americans over 65, but they could still be affected via spillover effects. Using Medicare administrative data, we test for spillovers in Medicaid coverage and Medicare spending among Medicare beneficiaries. We analyze two separate birth cohorts: those under 65 in 2014, who could have been induced by the expansion to take up Medicaid before joining Medicare; and those 65 or older in 2014, whose Medicaid eligibility was never affected by the expansion. Our analysis shows that spillovers only flowed into the under-65 cohort, with Medicaid coverage increasing and average Medicare spending falling for this group, with little effect on health conditions. A lack of an effect on those over 65 in 2014 suggests Medicare beneficiaries were not crowded out of health care access by the expansion. Instead, those under 65 used Medicaid to satisfy "pent-up" demand, consuming care they would have otherwise consumed later under Medicare.

Keywords: Health Insurance, Medicare, Medicaid expansion, ACA, Spillovers

JEL Codes: I13, I18

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1. Introduction

In January 2014, as part of the Patient Protection and Affordable Care Act (ACA), the US federal government implemented one of its most ambitious expansions of Medicaid eligibility in recent history. This expansion transformed the program from one that was not open to most adults into one where all adults in expansion states were eligible if their incomes were below 138 percent of the Federal Poverty Level (FPL). Given the large scale of this intervention into the health care system, spillover effects of the expansions into untargeted populations are likely substantial. Frean, Gruber, and Sommers (2017), for instance, argue half of the increase in health insurance coverage among non-elderly Americans caused by the ACA Medicaid expansion came from individuals who were already eligible before the ACA. This impact on the take-up of coverage can also spill over into behavior. Medicaid coverage affects the cost and access to health care, so untargeted individuals who take up coverage may change their health care consumption choices. Moreover, these consumption changes may flow over and affect others since health care markets create invisible linkages between participants via price mechanisms and supply constraints. Thus, the potential impacts of the ACA Medicaid expansion reach well beyond targeted populations.

In this paper, we use administrative data to comprehensively examine spillovers from the ACA Medicaid expansion into one of the largest populations not targeted by the ACA: Medicare beneficiaries. We measure effects on three categories of outcomes: Medicaid coverage among Medicare enrollees – known as "dual coverage"; average Medicare spending per beneficiary; and enrollee health conditions. Because the ACA coverage expansions exclude individuals older than 65, any changes in Medicaid coverage among Medicare enrollees are not due to eligibility changes of the ACA expansion. Instead, changes come from individuals taking up coverage they were already eligible for through non-ACA pathways, a phenomenon known as the "welcome mat" or "woodwork" effect. The most commonly cited underlying mechanism for this effect is increased awareness about Medicaid eligibility from improved government outreach and publicity efforts and reduced barriers to enrollment (Aizer and Grogger 2003; Aizer 2007; Frean, Gruber, and Sommers 2017). In addition, McInerney, Mellor, and Sabik (2021) suggest that previous experience with Medicaid before becoming a Medicare beneficiary also plays a role, calling this phenomenon the "on-ramp" effect.

Spending effects that flow from coverage changes induced by the expansion represent further spillovers and can be divided into two types: supply-side and demand side. Supply-side spillovers could occur if the capacity of health care providers is insufficient and unable to expand quickly enough to handle the additional demand for health care the expansion creates,[1] resulting in other health care consumers being crowded out from access to health care. In the context of Medicare, this crowd-out effect would be reflected by reduced health care consumption by beneficiaries.

Demand-side spillovers emanate from changes in individuals' cost to consume health care, and we focus on two subtypes. First is the downward-sloping-demand effect. For those who take up Medicaid coverage, out-of-pocket costs for health care are lower because Medicaid covers all or some of a Medicare beneficiary's cost-sharing. Downward-sloping demand curves imply that this lower cost may result in beneficiaries consuming additional health care under Medicare. Second is the intertemporal-substitution effect, where those who take up Medicaid before they turn 65 due to the ACA expansion could consume health care under Medicaid that they otherwise would have consumed later when they joined Medicare. This substitution over time implies a reduction of consumption under Medicare.

In investigating spillovers into spending, we follow a strategy similar to the one used by McInerney, Mellor, and Sabik (2021) and conduct separate analyses by birth cohort since the intertemporal-substitution effect could only apply to those exposed to the Medicaid expansion before Medicare eligibility. The first cohort consists of those who were 65 or older when Medicaid expanded in 2014 ("post-65" beneficiaries) and thus were not exposed to the Medicaid expansion before they enrolled in Medicare. The second consists of those who were still under 65 in 2014 and could have been induced to substitute care intertemporally from Medicare to Medicaid ("pre-65" beneficiaries). Thus, post-65 beneficiaries could only be affected by the supply-side and downward-sloping-demand effects, while the pre-65 individuals could be influenced by both in addition to intertemporal substitution. Moreover, separating the cohorts this way also allows us to examine changes in Medicaid coverage separately and provide further understanding of the results of McInerney, Mellor, and Sabik (2021), which showed sizable spillovers in coverage (on-ramp effects) for pre-65 beneficiaries, but did not provide separate estimates for post-65 enrollees.

[1] The Oregon Health Insurance Experiment showed a clear increase in health care use after Medicaid enrollment (Finkelstein et al. 2012; Baicker et al. 2013). Evidence on health care use from the ACA expansion includes increased use of preventive services, likelihood of having a personal physician, use of prescription drugs, and (for the first year) increased overnight hospital stays (Sommers et al. 2015; Miller and Wherry 2017; Simon, Soni, and Cawley 2017; Ghosh, Simon, and Sommers 2019).

Using variation in Medicaid expansion adoption across states in a difference-in-differences (DD) framework, and combining that with changes in Medicare eligibility across age cohorts in a triple-difference (DDD) research design, we find that dual coverage increased following the expansion, but only among pre-65 beneficiaries. Measuring at about four percentage points, it represents a roughly 14.8 percent increase in dual enrollment in expansion states. For spending, because we do not find a dual coverage effect for post-65 individuals, this group could not be impacted by the downward-sloping-demand effect and could only reflect supply-side crowds out. However, on both the extensive and intensive margins, we find no evidence of supply-side spillovers for post-65 beneficiaries. For the pre-65 enrollees, we find that health care consumption in Medicare (combined inpatient, outpatient, and physician services spending) fell by 461 dollars on average, an effect of about eight percent of pre-ACA spending. This comes entirely along the intensive margin, with the likelihood of accessing any care, the extensive margin, being unaffected. For Part D, we find no effect on spending overall, but pre-65 beneficiaries are more likely to have prescription spending of some type (that is, along the extensive margin) in expansion states. Thus, overall we find the ACA Medicaid expansion spilled over into Medicare exclusively through those exposed to it before they turned 65, and the estimated spending effects reflect individuals substituting care intertemporally from Medicare to Medicaid. Additionally, we cannot rule out that this group was also affected by the downward-sloping-demand effect; but if it was, it was dominated by intertemporal substitution. Lastly, our examination of health measures finds limited evidence of effects on health in the pre-65 individuals whose health consumption was altered.

Our work contributes to and builds upon recent empirical literature focused on whether health care reforms have supply-side spillovers ("negative spillovers" in this literature) that result in the crowd out of Medicare enrollees' access to medical care. Studying the Massachusetts health reform, Joynt et al. (2013) and Joynt et al. (2015) argue it did not induce negative spillovers in Medicare and instead improved care quality with only a modest increase in costs. In contrast, Bond and White (2013) find the same reform decreased the number of primary care visits per Medicare beneficiary, though only on the intensive margin. Examining expansions of Medicaid and the State Children's Health Insurance Program to parents in the late 1990s and early 2000s along with changes in employer-provided insurance availability, Glied and Hong (2018) find insurance coverage expansions led to lower Medicare spending per beneficiary. This was a similar finding to that of McInerney, Mellor, and Sabik (2017), who study individual-state Medicaid expansions pre-dating the ACA. Both studies, however, present suggestive evidence that the spending crowded out in these cases was low-value care. In a study more closely related to ours, Carey, Miller, and Wherry (2020) examine the impact of the ACA Medicaid expansion on primary care access and overall spending in Medicare for post-65 beneficiaries, estimating precise zero effects using administrative claims data.

Our paper plays a unique role in this literature, providing a possible reconciliation for findings that, on the surface, seem to disagree. By splitting our sample by birth cohort, we find evidence consistent with Carey, Miller, and Wherry (2020), Joynt et al. (2013), and Joynt et al. (2015) that there was no crowd out of access from these recent health care reforms, while also obtaining negative spending effects like Bond and White (2013), Glied and Hong (2018), and McInerney, Mellor, and Sabik (2017) among our younger cohort. Moreover, our finding of heterogeneity by cohort provides an alternative explanation for spending reductions to that given by Glied and Hong (2018) and McInerney, Mellor, and Sabik (2017). Rather than low-value care being crowded out, an argument that depends on the health care market being inefficient enough that low-value care could be widespread in the first place, our explanation of voluntary intertemporal substitution is consistent with near-elderly individuals without insurance coverage experiencing "pent-up" demand for health services. Such a phenomenon has been observed in a variety of settings among new Medicare enrollees (Decker and Rapaport 2002; McWilliams et al. 2003; Card, Dobkin, and Maestas 2008; 2009; Chatterji, Nguyen, and Yoruk Forthcoming).

Finally, we note that this study also provides additional insight into coverage spillovers from the ACA expansion found by McInerney, Mellor, and Sabik (2021). Using American Community Survey data, they found dual coverage increased by about almost two percentage points among the overall Medicare population, but by four percentage points among the pre-65 enrollees. They didn't, however, separately estimate an effect for post-65 enrollees. In our study, we use administrative data and estimate separate effects for both pre-65 and post-65 beneficiaries, which allows us to confirm their results on pre-65 enrollees' coverage, but also show that there was no change among those already 65 when Medicaid expanded. Thus, the coverage pathway they called the on-ramp effect was not merely "an important mechanism," as they put it, in coverage spillovers to Medicare – it was the *only* mechanism.

2. Background

Medicare is a public health insurance program for Americans aged 65 or older and for certain others under 65 with disabilities. In 2019, it covered 61.5 million Americans, including 3.8 million who were newly enrolled (CMS 2021). Traditional Medicare, or Fee-For-Service (FFS), consists of two parts: Part A covers inpatient hospital stays, skilled nursing facility care, hospice care, and home health care; Part B covers physician services, hospital outpatient care, laboratory tests, imaging, and durable medical equipment. Seniors also have the option of choosing a private Medicare Advantage (MA) plan, also known as Medicare Part C, which covers a broader range of services but within a more limited service network. Prescription drug coverage is provided under Part D, the most recent addition to Medicare. Under traditional Medicare, beneficiaries can still face high out-of-pocket costs without a cap. Those without any supplemental coverage are most likely to face substantial medical expenses. In 2016, for instance, traditional Medicare enrollees without supplemental coverage spent an average of $7,473 out-of-pocket for health care (Cubanski et al. 2019).

Like Medicare, Medicaid is also a government health insurance program, but it has traditionally targeted low-income individuals, and each state has its own, unique Medicaid program (in contrast to the nationally homogenous Medicare program). People who satisfy the eligibility criteria of both programs can be simultaneously enrolled in both Medicare and Medicaid. These "dual enrollees" (or "dual beneficiaries") [2] account for about 18 percent of Medicare beneficiaries – roughly eleven million people – as of 2019 (CMS 2021). For those who qualify, Medicaid coverage can significantly lower out-of-pocket costs for traditional Medicare beneficiaries by paying Part A and B premiums, deductibles, coinsurance, and copayments, while also providing services not covered by Medicare (such as dental, vision, and long-term care). For example, out-of-pocket costs averaged only $2,665 for dual enrollees in 2016 – almost $5,000 less than those without any supplemental coverage (Cubanski et al. 2019).

The ACA expansion of Medicaid resulted in a significant increase in the potential for interactions between the Medicaid and Medicare programs, both directly and indirectly. To address low take-up of benefits, the ACA was accompanied by significant publicity and outreach efforts, and its provisions streamlined enrollment and renewal processes (Wachino, Artiga, and Rudowitz 2014). While this was primarily intended to increase take-up of

[2] Most commonly, those enrolled in both programs are called "dual eligible," but we view this as a misnomer to avoid given that people eligible for Medicaid are often not enrolled.

coverage by those under 65, these efforts also could indirectly affect Medicare enrollees and induce them to take up Medicaid coverage for which they were already eligible.[3] Furthermore, the ACA expansion also directly affected the eligibility of near-elderly individuals who were under 65 after the expansion, and who would soon enroll in Medicare after turning 65. McInerney, Mellor, and Sabik (2021) found that, in 2014, Medicaid participation increased substantially – by about ten percentage points, or more than a third – in expansion states among low-income 63- and 64-year-olds.[4] Thus, the expansion created a situation where there could be a significant shifting of health care consumption from after 65 under Medicare to before 65 under Medicaid.

3. Empirical Methods

3.1 Research Design Overview and Data

The ACA Medicaid expansion was originally designed to include all states, but a Supreme Court ruling changed the law to give states the option to expand or not. We take advantage of the opt-in format of the expansion and use it as the basis for our research design. Expansion states represent a treatment group in our analysis and non-expansion states a control group that allows us to estimate the counterfactual outcome that would have been observed had the expansion states not actually been "treated" by the ACA. Thus, our basic research design is a DD. Furthermore, since some theoretical effects depend on birth cohort, we split our sample into two groups by birth year to estimate separate effects. We also take advantage of this split of the sample in a DDD framework that compares across the birth cohorts in addition to across states. Since both birth cohort groups could (at least in theory) be affected by the Medicaid expansion, care must be taken to understand what effect the DDD approach identifies. As we discuss more below, based on the results from our separate DD models, the DDD ends up identifying the same effect as the DD model for our pre-65 beneficiaries, but with the added benefits of an additional control group and a larger sample size.

For most states that opted-in to the ACA expansion, their expansions occurred in 2014. To perform a clean evaluation, our analysis focuses on these 2014 adopters along with those that never expanded through the end of our sample in 2017. Thus, our sample includes

[3] The Medicaid participation rate among eligible Medicare enrollees was under 50 percent before the ACA (Currie 2004; Pezzin and Kasper 2002; Remler and Glied 2003).

[4] Additionally, Bin Abdul Baten and Wehby (2021) found that, among 60- to 64-year-olds with incomes below the poverty line, the ACA Medicaid expansion increased the likelihood of having any coverage – not Medicaid specifically – by about 8.5 percentage points.

16 expansion and 18 non-expansion states.[5] Within these states, we use individual-level data from a five-percent random sample of the 2010 to 2017 Medicare population obtained from the Centers for Medicaid and Medicare Services (CMS). Included beneficiaries are chosen randomly and are observed each subsequent year of our sample, with new, randomly selected individuals added each year as they turn 65 and enroll in Medicare. Using Master Beneficiary Summary Files (MBSF), we observe demographics, coverage, chronic conditions, and health care spending on an annual basis. Our measures of health spending include the three largest components of traditional Medicare: inpatient under Part A and hospital outpatient care and phycian services under Part B. We also observe drug spending under Part D.[6] In sum, our analytical dataset is an unbalanced panel containing annual observations from 2010 to 2017, subject to several restrictions that we describe next.

First, given that one of our focuses is on those who had initial exposure to Medicaid expansions before 65, we limit our sample to those aged 65 to 69 (those over 69 during our sample period were all older than 65 when Medicaid expanded in 2014). Second, since movers may be different from non-movers in important ways, we exclude those who moved between states during the 2010 to 2017 period. Third, we drop individuals born in January and February of 1949 – that is, those who turned 65 in early 2014 – from the post-ACA period. Since the goal of splitting our sample by age group is to identify people who could have been exposed to the Medicaid expansion before they enrolled in Medicare, dropping these early 2014 birthdays removes those for whom this was unlikely. Fourth, since the expansion was targeted at lower-income Americans, we only include those from zip codes where the zip-code-level median income was below the United States median and whose zip-code-level poverty share was above the median of all zip-code-level poverty shares in the country. This also focuses our analysis on those on whom spillovers are most likely to be found, since higher-income individuals are less likely to face health care access issues, be sensitive to out-of-pocket spending, or have Medicaid affect their consumption of medical

[5] Expansion states: Arizona, Arkansas, Hawaii, Illinois, Iowa, Kentucky, Maryland, Michigan, Nevada, New Hampshire, New Mexico, North Dakota, Ohio, Oregon, Rhode Island, and West Virginia. Non-expansion states: Alabama, Florida, Georgia, Idaho, Kansas, Maine, Mississippi, Nebraska, North Carolina, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Wisconsin, and Wyoming. We exclude 2015 and 2016 expanders (Alaska, Indiana, Louisiana, Montana, and Pennsylvania), early opt-in states (California, Connecticut, Colorado, Minnesota, Missouri, New Jersey, Washington, and District of Columbia) and those that provided Medicaid coverage to childless adults and had income thresholds of 100 percent of the FPL or higher from 2010 through 2013 (Delaware, Massachusetts, New York, and Vermont, in addition to also-early-opt-in District of Columbia).

[6] We do not observe any Part C spending since the MBSFs are based on Medicare claims data, which excludes spending that occurs for beneficiaries enrolled in Medicare Part C.

care. Finally, we also lack enrollees born in 1950 and 1951 for the years 2015 and 2016, as our data was originally obtained for a different project where these records were not required. These individuals are included in our 2017 data, however.

Table 1 presents descriptive statistics for our final sample consisting of 384,255 observations. Our primary outcome is Medicare spending on combined inpatient, outpatient, and physician services. Before the ACA, individuals in our sample incurred, on average, $6,226 in such services annually in expansion states and $5,515 in non-expansion states.[7] These figures can be compared to the 2012 Medicare spending estimate for the same categories of $4,841 for 65- to 74-year-olds reported by Niu, Buntin, and Manchester (2015).[8] Defining dual enrollees as those Medicare beneficiaries with at least one month of Medicaid coverage in a year, they represent about 28 percent of our pre-ACA sample. This is much higher than the overall rate of dual enrollment 11.6 percent for 65- to 74-year-olds reported for 2013 by CMS Medicare-Medicaid Coordination Office (2020).[9] In both cases of spending and dual coverage, our larger estimates unsurprising given that our sample consists only of those residing in low-income, high-poverty zip codes. Additionally, *Table 1* also shows that the share of dual beneficiaries declines over time in our sample, as one would expect given the economy was recovering from the Great Recession over our time frame. Considering *Table 1* as a whole, it shows that the expansion and non-expansion states were different across several measures in the pre-ACA period, including in spending and various demographic characteristics. This underscores the importance of our DD and DDD research designs. We show below that our outcomes of interest exhibit parallel pre-trends, suggesting we can still obtain credible estimates in spite of the pre-existing differences across states.

3.2 Identifying Heterogeneous Effects by Birth Cohort

In designing our analysis to be able to identify separate effects for pre-65 and post-65 enrollees, our use of Medicare administrative data creates a constraint: we do not observe individuals before enrollment in Medicare. This limitation is important because the

[7] We pro-rate spending for those who were Medicare enrollees for less than a full year according to the formula: pro-rated annual spending = {(actual spending)/(number of enrolled months)}*12.
[8] Niu, Buntin, and Manchester (2015) reported $5,601 for 65- to 74-year-olds for total spending. We restrict this to the cateogries of inpatient, outpatient, and physician services represent (76.1% of the estimate), and express it in 2017 dollars using a CPI index increase factor of 1.1357 (the original was presented in 2009 dollars). Hence, our figure $4,841 = 5,601×0.761×1.1357.
[9] See data tables accompanying the CMS report at https://www.cms.gov/files/zip/medicaremedicaiddualenrollmenteverenrolledtrendsdata.zip.

individuals who were younger than 65 when Medicaid expanded in 2014 are not observable in our data during the pre-period. *Table 2* illustrates this issue and our ultimate solution. On the right side of the table showing the post-ACA period, the shaded cells show the birth years of pre-65 individuals who appear in our data in 2014 or later. As beneficiaries age over time and new individuals enroll each subsequent year, the share of pre-65 beneficiaries in the sample increases each year. Similarly, because our sample is limited to individuals who were under age 70, the share of the sample consisting of post-65 individuals falls each year. Thus, the post-ACA sample is split between those younger and older than 65 at expansion in the diagonal fashion shown in the right side of *Table 2*.

The question then arises of how to set up our pre-period data to allow us to implement a DD-style analysis? We address this question by splitting the pre-ACA data in a fashion that is symmetric with the split in the post-ACA period. By symmetric we mean that each year of the pre-ACA period has the same split of ages as the corresponding year in the post-ACA period. Thus, to estimate the effect of the ACA Medicaid expansion on Medicare's pre-65 beneficiaries we use only the birth-year cohorts listed in the shaded cells of *Table 2*, and to estimate post-65 enrollee effects we use only the cohorts listed in the unshaded cells. That is, post-ACA observations of individuals born in the years 1949 through 1952 are combined with pre-ACA observations of those born from 1945 to 1948 to create the sample we use to estimate pre-65 beneficiary effects. Analogously, we create a sample to estimate post-65 beneficiary effects by combining post-ACA records for those born from 1945 through 1948 with pre-ACA records of enrollees with 1941 to 1944 birthdays. While this approach is unique, it allows us to split our Medicare data so we can estimate heterogeneous effects while still implementing a DD framework.

3.3 Estimation

Our base method of estimating effects for both the pre-65 and post-65 enrollees follows a standard DD approach, with our econometric model taking the following form:

$$Y_{ist} = \delta \, EXP_s \times POST_t + X'_{ist}\Theta + \mu_t + \lambda_s + \varepsilon_{ist} \quad (1)$$

Here Y_{ist} represents dual enrollment, Medicare spending (inpatient, outpatient, and physician services), drug spending (Part D), and health conditions for individual *i* in state *s* and year *t*. EXP_s indicates individuals living in an expansion state, and $POST_t$ identifies the years after

2013. $EXP_s \times POST_t$ is equal to one for those who lived in an expansion state after the time of expansion and zero, otherwise. X_{ist} contains variable controls at the individual, zip-code, and state levels. These include binary controls for females, white race, the presence of any of 27 chronic conditions, and a set of dummy variables for birth year. It also includes zip-code-level gross-rent median price, percentage of those with social security benefits, average family size, and median age. At the state level, we include the annual unemployment rate. λ_s captures time-invariant differences across states, and μ_t controls for changes over time that are common to all states. Finally, ε_{ist} represents the error term. In all estimates of equation (1), we calculate standard errors that allow for state-level clustering.

The primary parameter of interest in equation (1) is δ, which represents the DD effect of the Medicaid expansion. We summarize the theoretical effects of the Medicaid expansion on Medicare beneficiaries in terms of δ in *Table 3*. For dual (Medicaid) coverage, the ACA increased outreach and publicity efforts to inform people of their potential eligibility while also streamlining enrollment and renewal. Moreover, the ACA expansion also may have induced Medicaid take-up by already eligible individuals before turning 65, giving them direct, personal experience with the program and creating an "on ramp" to Medicaid coverage after 65. These mechanisms all imply a positive theoretical effect on dual coverage from the expansion for both pre-65 and post-65 Medicare enrollees.

For spending, there are three potential theoretical mechanisms. The first two affect both groups but have opposing theoretical effects. Downward-sloping health care demand implies that insurance take-up would induce more spending, a positive effect for δ, while increased health care use by new Medicaid enrollees implies crowd-out of access for Medicare beneficiaries, a negative implied effect for δ. We emphasize here that the downward-sloping demand effect is contingent on individuals taking up dual coverage. This implies that if the expansion did not affect dual coverage, then there would be no downward-sloping demand effect on spending. The third effect, intertemporal substitution, only applies to pre-65 individuals. If they take up Medicaid before they turn 65, this could allow them to consume health care under Medicaid that they would have consumed later via Medicare, implying a negative effect for δ. This effect also depends on the first-stage effect of Medicaid take-up, but in this case only for individuals under 65 years old, as otherwise they would not be able to consume care under Medicaid before turning 65. We return to this point in the results section below, noting that previous work has provided evidence of this first-stage effect.

Considering the above predictions as a whole, for the outcome of dual coverage we unambiguously expect increases in Medicaid coverage for both age cohorts. Additionally, the effect for pre-65 individuals should be larger than for the post-65 group given the on-ramp effect does not apply to the post-65 cohort. In contrast, for spending the predicted net effect is ambiguous in direction for both groups and will depend on which effects dominate. Between the two groups, however, there is a clear prediction for the effect on the pre-65 group to be less than that of the post-65 group because the intertemporal substitution effect is negative and only applies to pre-65 enrollees.

In addition to estimating the above DD specification, we push our data a bit harder and take advantage of the fact that both the pre- and post-65 groups have theoretical effects in common, and that the pre-65 group is affected by one extra mechanism for both the dual coverage and Medicare spending outcomes. We use the post-65 group to estimate the effects that are shared between the groups, and then subtract these estimates from the estimated effects for the pre-65 groups. Assuming our theoretical understanding of the effects facing these groups are correct, this approach identifies the on-ramp effect for dual coverage and the intertemporal substitution effect for spending in the pre-65 group. We implement this strategy by pooling the data for both groups and estimating a DDD-style econometric model. To differentiate the age groups, we define the indicator $PRE65_g$, which equals one for the sample we use to estimate effects on pre-65 beneficiaries (those in the shaded cells of *Table 2*) and zero for the post-65 enrollees (the unshaded cells of *Table 2*).[10] Our DDD model then takes the form:

$$Y_{igst} = \beta EXP_s \times POST_t \times PRE65_g + \alpha_1 EXP_s \times POST_t + \alpha_2 PRE65_g \times EXP_s + X'_{igst}\Theta + \mu_t + \lambda_s + \eta_b + \varepsilon_{igst} \quad (2)$$

where the variables remain the same as in equation (1), except for the birth-year fixed effects, η_b (which included in our DD models but not separately indicated in equation (1)), and the newly added $PRE65_g$.[11] The coefficient β is our primary interest as it represents the difference between the separate effects for the pre-65 and post-65 groups. The theoretical

[10] These samples are described in detail in Section 3.2.

[11] We do not include a $PRE65_g$ term in equation (2) since it is collinear with the combined year and birth-year fixed effects. Similarly, we do not include a $POST_t \times PRE65_g$ term since it is collinear with the birth-year fixed effects.

effects that it captures are summarized in the last row of *Table 3*: the on-ramp effect for dual coverage and the intertemporal-substitution effect for Medicare spending.

We estimate all regressions using Stata version 14 via the “reghdfe” command, and adjust for clustering at the state level in all standard errors (Bertrand, Duflo, and Mullainathan 2004; Correia 2016; StataCorp 2015).

4. Results

4.1 Effects of Medicaid Expansion on Dual Enrollment

Our main estimates of spillover effects on Medicare from the ACA Medicaid expansion are presented in *Table 4*. Panels A and B contain our DD results for the post-65 and pre-65 groups, respectively, while Panel C contains our DDD estimates using a sample that pools both groups. We focus first on our estimates of effects on dual coverage, contained in column 1. For the post-65 group, Panel A shows that we obtain a statistically insignificant estimate of approximately zero. This null effect on coverage implies that the outreach, publicity, and reduction of enrollment barriers associated with the Medicaid expansion did not change dual coverage among Medicare enrollees. In contrast, our estimate for the pre-65 group in Panel B is 3.9 percentage points and is statistically significant ($p < 0.05$). This estimate represents an increase of about 14 percent among pre-65 beneficiaries relative to a baseline of 27.9 percent dual coverage before the expansion. We interpret this pre-65 result as coming entirely through the on-ramp spillover pathway given that our estimate for the post-65 group implies there was no net impact from outreach, publicity, and streamlined enrollment procedures.

These DD results rely on the assumption that the non-expansion states represent a reasonable representation of the counterfactual for the expansion states. We provide some evidence for this assumption by estimating event study versions of equation (1) where the average difference between expansion and non-expansion states is allowed to vary over time. That is, we substitute the $EXP_s \times POST_t$ term in equation (1) with EXP_s multiplied by a full set of year indicators (except 2013, the comparison year), allowing for direct comparison of time trend differences between the expansion and non-expansion states in the pre-period. *Figure 1* presents estimates for the associated coefficients for these event study terms. In the case of both age groups, the pre-65 on the top and the post-65 on the bottom, we find the differences between expansion and non-expansion states were stable from 2010 to 2013, suggesting that the underlying assumption of our DD analysis is credible. For the post-expansion estimates, we find our event study estimates align with our primary, DD results in column 1 of *Table 4*.

We next return to our main results on dual coverage in column 1 of *Table 4*, where estimates of our DDD specification in equation (2) are presented in Panel C. As noted above, we argue that the DDD approach specifically identifies spillovers that occur through the on-ramp pathway. However, our null estimate on Medicaid coverage for the post-65 group implies that these estimates will also reflect essentially the entire spillover effect felt by pre-65 beneficiaries. Consistent with this, the statistically significant estimate produced by the DDD of 4.1 percentage points ($p < 0.05$) is similar to our DD result for the pre-65 group. It implies an increase in dual coverage of almost 15 percent compared to a pre-ACA baseline of 27.7 percent. Our estimate here is consistent with the four percentage point estimate obtained by McInerney, Mellor, and Sabik (2021) for the on-ramp effect via a similar methodology, but a with different data source in the American Community Survey. Therefore, we both find the on-ramp mechanism is the main spillover pathway from the ACA Medicaid expansion into dual enrollment among Medicare enrollees.

4.2 Effects of Medicaid Expansion on Medicare Spending

Before discussing the rest of *Table 4*, which contains results on the effect of the expansion on Medicare spending, we note that our estimate of a null effect for Medicaid coverage in the post-65 group impacts the theoretical relationship between the expansion and Medicare spending for that group. We wrote above that the Medicaid coverage could reduce the cost of health care for Medicare enrollees, potentially leading to additional consumption. If enrollees, however, do not take up Medicaid coverage in response to the expansion, their cost of health care is not affected. Thus, we cannot expect (at least theoretically) to observe a downward-sloping-demand effect for the post-65 group given our results so far obtained. This implies that the only theoretical relationship remaining between the expansion and Medicare spending for post-65 beneficiaries is the crowd-out effect, which does not work through dual coverage but through supply-side capacity constraints that may bind in response to additional consumption by those not enrolled in Medicare. Therefore, we interpret the estimates we obtain for the effect on spending on the post-65 group as estimates of the crowd-out (negative spillovers) effect.

Table 4 presents results for three variations of spending measures: column 2 contains effects on combined Medicare spending (inpatient, outpatient, and physician services), while columns 3 and 4 present combined spending on the intensive and extensive margins, respectively. Spending on the extensive margin means that an enrollee had positive Medicare spending, and, hence, it is a binary outcome. Spending on the intensive margin is conditional on there being some positive spending, so our samples for these estimates are restricted to only those individuals who had some consumption on the extensive margin.

Our findings in Panel A for post-65 enrollees are positive and not statistically significant across all three measures. Taken together, these results suggest that the increased pool of Medicaid patients under age 65 caused by the ACA did not crowd out access to care for Medicare enrollees. For total spending in column 2, we obtain a point estimate of $99.4 and a 95 percent confidence interval that rules out negative spillovers of larger than $269 per enrollee, or about 4.5 percent of baseline spending of $5,967.[12] This result is consistent with the most similar estimate in the literature, provided by Carey, Miller, and Wherry (2020). They obtained a point estimate of $56.12 for spillovers on overall spending due to the ACA expansion using a sample of post-65 beneficiaries.[13] Moreover, our estimate is within their estimate's 95 percent confidence interval, and theirs is in ours, and in both cases the confidence intervals rule out large crowd-effects among the Medicare population.

Moving next to our results for the pre-65 group in Panel B, we note that since we do not find evidence of crowd-out effects for the post-65 population, we presume there was also no effect (or only a small one) on pre-65 enrollees, given that the mechanism of supply-side capacity constraints cannot be targeted at specific groups. Thus, we interpret our pre-65 results as reflecting only downward-sloping demand and intertemporal substitution. The theoretical net effect of these effects is ambiguous as their predicted impacts have opposite directions, but this also implies that any estimate would represent a lower bound for the effect that dominates.

Our result for total spending in column 2 suggests Medicare enrollees in expansion states, on average, consumed $461 less health care ($p < 0.05$) through Medicare after the expansion, roughly 8.3 percent of baseline spending of $5,572. Breaking this result down to the extensive and intensive margins, we find the decrease in spending was larger on the intensive margin (column 3) at -$554, or 8.6 percent compared to a baseline of $6,409. Access – as measured by the extensive margin in column 4 – saw a slight increase of almost a percentage point, though this estimate is only significant at the ten-percent level. These

[12] Our confidence intervals use a critical value of 2.035, which comes from a t-distribution with 33 degrees of freedom. Our degrees of freedom calculation is based on 34 states (clusters) in our sample.
[13] See their Table A.3 in their online appendix.

results suggest that downward-sloping demand may have induced a small increase in the likelihood of accessing care for Medicare enrollees, but intertemporal-substitution effects resulted in them being treated less intensively, on average. Overall, intertemporal substitution ended up being the dominant effect.

We emphasize here that our interpretation of these results as being driven by intertemporal substitution relies on the existence of an increase in Medicaid take-up in response to the ACA expansion among those *close to 65, but not yet over 65*. McInerney, Mellor, and Sabik (2021) provide such evidence, having found that low-income 63- and 64-year-olds experienced a ten percentage-point increase in Medicaid enrollment in response to the ACA Medicaid expansion.[14] Thus, a substantial share of the low-income population could have increased its consumption of health care under Medicaid shortly before turning 65 and enrolling in Medicare, satisfying a logical requirement inherent in our intertemporal substitution interpretation.

McInerney, Mellor, and Sabik's estimate can also help us increase our understanding of our estimated $461 decrease in spending if we recognize that our estimate is an average across our entire pre-65 group, despite that not all beneficiaries in the group had taken up Medicaid before turning 65 to consume medical care. We can estimate the effect on those who *actually* enrolled in Medicaid in response to the ACA prior to 65 by dividing our -$461 spending estimate by McInerney, Mellor, and Sabik's 0.1 estimate. This implies an average treatment effect on Medicare spending for those who enrolled in Medicaid before turning 65 due to the ACA of about -$4,610 (= -$461/0.1). This represents a substantial decrease in spending, but two factors suggest that it could be a reasonable estimate. First, there is likely to be a strong selection effect, where people who expected to have high health care costs were the ones who decided to take up Medicaid coverage before 65 (and Medicaid does not exclude individuals on the basis of their health). The spending distribution in Medicare is significantly skewed, so costs for individuals in the higher parts of the spending distribution can be quite high.[15] Second, a significant portion of those who took up coverage in response to the ACA are ones who were likely to eventually become dual enrollees after entering the Medicare program. This can be seen from our 3.9 percentage point estimate for the increase

[14] McInerney, Mellor, and Sabik's estimate is much larger than our estimate of the dual coverage effect for the pre-65 group in column 1 of Table 4 because their estimate is based on take-up by individuals who gained new eligibility *and* that of those who had already been eligible. Meanwhile, our estimate consists only of those taking up coverage for which they were already eligible, since eligibility criteria for those over 65 were not affected by the ACA.

[15] About five percent of Medicare beneficiaries account for 41 percent of Medicare spending in 2012 (Bynum et al. 2017).

in dual coverage among the pre-65 group. Such individuals have much higher health care spending levels than average, suggesting sizable intertemporal substitution could be feasible for a substantial share of those who took up Medicaid.[16]

For spending we also estimate event study versions of equation (1) to evaluate the pre-trends in our outcomes. Results from these models are presented in *Figure 2*, ***Figure 3***, and *Figure **4***. In each of these figures, the coefficients in the years before 2014 are close to zero without statistical significance both in the pre-65 (top panels) and post-65 (bottom) groups. Additionally, the post-period estimates are largely consistent with our DD results. For the overall and intensive margin spending outcomes, our pre-65 group estimates are negative in the post-period for each year except one, while for the pre-65 group estimates are close to zero each post-period year. Our estimators for these outcomes, however, are less precise, resulting in estimates that are not statistically significant at the 5 percent level. For the extensive margin outcome, we obtain positive estimates for each post-period year for the pre-65 group, with one year being statistically significant at the 5 percent level, while estimates are all close to zero for the post-65 group. Thus, overall these figures support our use of the DD approach and send a similar message as to the effects of the ACA expansion.

Panel C in *Table 4* presents our spending estimates using our DDD model, equation (2). As we noted above, because we estimated a null effect of the expansion on dual coverage for the post-65 group, we cannot interpret our estimates for the post-65 group on spending as reflecting a downward-sloping demand effect. In contrast, our estimate for dual coverage for the pre-65 group was not null, so the downward-sloping-demand effect could be reflected in our spending estimates for that group. This pair of dual coverage estimates affects the interpretation of our DDD model because, had there been a "first stage effect" on dual coverage for the post-65 group, the DDD model would "subtract out" the downward-sloping-demand effect from the estimates for the pre-65 group, giving us a clean estimate of the intertemporal-substitution effect alone (as summarized in the last panel of *Table 3*). Since there was not a first-stage effect for the post-65 enrollees, though, our DDD model will not subtract out any downward-sloping-demand effect on the pre-65 group. Instead, our DDD estimate will reflect the same mechanisms as the estimates for the pre-65 group: a combined effect of downward-sloping demand and intertemporal substitution. In other words, it represents an alternative estimate of the effect of the expansion on the pre-65 group. As

[16] For instance, Keohane et al. (2018) show that the average spending per dual beneficiary between ages 65 and 74 was $12,118 in 2015. Moreover, top end spending by dual enrollees is even more extreme, as the top 10 percent averaged about $86,300 in Medicare consumption in 2009 (Hayford et al. 2013).

compared to our DD model, however, the DDD allows us to pool our data, allowing for potentially more accurate estimation.

Column 2 presents our DDD estimate of the effect of exposure to the Medicaid expansion before 65 on Medicare spending. We find that annual medical spending per beneficiary fell by an average of \$590 ($p < 0.05$), representing a 10.2 percent decrease over baseline spending of \$5,767. Converting this estimate to one reflecting the effect on only those individuals who *actually* took up Medicaid before 65, as we did for our estimate from Panel B, results in a reduction in spending of approximately \$5,900 (= \$590/0.1). Columns 3 and 4 show that the overall spending reduction was driven primarily on the intensive margin, with our estimate of the likelihood of accessing any Medicare services being statistically insignificant. We reiterate here that our negative spending estimate implies that, to the extent downward-sloping demand affected the pre-65 group, intertemporal substitution was the dominant effect. Further, because we cannot observe the downward-sloping demand effect, our estimates may understate the magnitude of intertemporal substitution among the pre-65 enrollees.

Appendix Table 1 breaks down our spending results in *Table 4* into separate categories: one for inpatient and another for combined outpatient and physician services. These results are consistent with *Table 4,* but it does reveal one interesting feature of our results. For the post-65 group, we obtain a statistically significant ($p < 0.05$) positive estimate for the intensity of treatment in inpatient settings. While this estimate reflects only a small share of our sample, it provides another detail that further strengthens the case against the theory that provider capacity constraints induced by the ACA expansion resulted in crowd out of access for Medicare enrollees.

4.3 Robustness Checks and Heterogeneity by Race and Gender

We next turn to the question of the robustness of our results. Here we focus on our DDD results since our DDD estimates are essentially a composite of our DD results for both age cohorts. In *Table 5* we present estimates after several sample adjustments for comparison with our main results.[17] In Panel A, we exclude 2014 from our sample to rule out the chance our estimators could be picking up an artifact of the expansion's implementation. In Panel B,

[17] We also provide a set of robustness checks in *Appendix Table 2* of our post-65 group's DD results from Panel A of *Table 4*, all of which are consistent with our main findings.

we exclude individuals with at least one month of retiree drug subsidy (RDS), a program that subsidizes employer and union health plans that provide retiree drug coverage to former employees after their enrollment in Medicare. Individuals who are part of a health plan receiving an RDS are likely to have had high-quality health insurance before retirement, and potentially supplementary coverage afterwards. Thus, they might be less likely to take up Medicaid coverage either before or after age 65. In Panel C, we add three states that opted into Medicaid expansion earlier than 2014: New Jersey, Colorado, and Connecticut. In our main analysis, we exclude early opt-in states given they would have a lower treatment "dose" in 2014 as compared to other states that did not expand early. However, these states' early expansions were quite small: New Jersey expanded to 23 percent of the FPL, Colorado to 10 percent, and Connecticut to 56 percent (Kaiser Family Foundation 2012). Thus, these states still had very large expansions in 2014 that are relatively comparable to the other states in our treatment group. In Panel D, we exclude 2010 since it was still near the height of the Great Recession, and Medicare enrollees' supplementary coverage and spending behavior could have been different as compared with later years.

As we noted in Section 3.1, our data is missing enrollees who were born in 1950 and 1951 from our data for 2015 and 2016. We examine whether this creates imbalance in our sample that affects our results in Panel E. There we drop a corresponding set of enrollees from our pre-period data: those born in 1946 and 1947 from the years 2011 and 2012. In Panel F, we exclude Michigan and New Hampshire, which expanded Medicaid in April and August of 2014, respectively, and so received a lower treatment dose than the states that expanded in January 2014. In Panel G, we change the method we use to limit our sample to lower-income, higher-poverty individuals. In our main sample, we compare zip-code level income and poverty to the national medians. In this analysis, we compare to the national means. Considering jointly the results of all of these variations to our main analysis in Panels A to G, we see that they are all consistent with our main results in Panel C of *Table 4*. For dual coverage, we estimate an effect of the expansion of about four percentage points in each case, with all but one being statistically significant at the 5 percent level (and that remaining on has a p-value of 0.059). For spending, we obtain estimates implying reductions in Medicare spending of between $445 and $653 overall and between $506 and $717 on the intensive margin. All of these are statistically significant at conventional levels, with most satisfying the 5 percent significance level. For the extensive margin, we obtain statistically insignificant estimates in all cases that range from 0.003 to 0.006, which are magnitudes that are consistent with our main results.

Table 6 summarizes heterogeneous effects by race and gender derived from our DDD specification in equation (2). The results in this table can be grouped into two sets in terms of overall similarity. The first contains the white and female sub-groups, where we observe positive estimates in dual enrollment that are not statistically significant. Despite being insignificant statistically, the point estimates for these sub-groups, 0.028 for white enrollees and 0.024 for females represent meaningful effect sizes, and large effects cannot be ruled out. At the same time, for these sub-groups we estimate substantial Medicare spending reductions that are statistically significant and are driven by the intensive margin. The total Medicare spending decrease is \$717 and \$1,051 for white beneficiaries and females, respectively, and \$833 and \$1,187 on the intensive margin (all with $p < 0.05$).

The second set of similar results is for males and non-white enrollees. For these sub-groups, we find evidence that the on-ramp effect increased dual enrollment in Medicaid and Medicare by 6.7 and 8.5 percentage points, respectively (both $p < 0.05$). These imply large effect sizes in percentage terms. For males, an estimate of 6.7 percentage points represents an effect of 35 percent, and for non-white enrollees, 8.5 percentage points corresponds to a 16 percent effect. In terms of spending, we obtain statistically insignificant estimates for both sub-groups that are also small in almost all cases. That said, it should be noted that our estimators for these sub-groups are much less precisely estimated than our main results, so we cannot rule out large spending effects.

The combined results in *Table 6* are in some ways consistent with our overall results on spillovers, but also show there was additional heterogeneity among sub-groups even within the differences we show across the pre- and post-65 age cohorts. The larger spending and smaller dual coverage results for females and white individuals are consistent with the intertemporal substitution effect being the dominant effect for these groups, as in our main results (recall that intertemporal substitution depends on Medicaid take-up before turning 65, not after). For non-white enrollees and men, the much larger dual coverage estimates and small spending estimates suggest that either downward-sloping demand effects offset intertemporal substitution, or both had negligible impacts on these groups. The large dual coverage estimates here are of particular note as they underscore just how strong the on-ramp effect can be towards influencing take-up of supplemental Medicaid coverage for some enrollees. Lastly, when considering this table as a whole, it is interesting that though there is some variation in the way spillovers were felt across sub-groups, all sub-groups felt spillover effects of *some* type from the ACA expansion, a testament to their scope.

4.4 Part D Drug Spending

As Medicare's Part D program pertains to pharmaceutical benefits and differs from traditional Medicare in that it is optional for most enrollees, in *Table 7* we separately examine spillover effects on drug spending under Part D. Here we return to the same presentation format as in *Table 4*, with estimates based on DD models for both the pre-65 and post-65 groups, and using a DDD approach for the sample with both groups pooled. As shown in Panel A, for post-65 beneficiaries we estimate qualitatively small effects that are not statistically significant across all outcomes. This is consistent with our interpretation of our main results that negative spillovers from the ACA Medicaid expansion are either small or non-existent among Medicare beneficiaries. For pre-65 enrollees, we can consider the results in both Panels B and C. This is because our null results for the post-65 group imply the DDD estimates in Panel C will entirely reflect the effects felt by the pre-65 group. In both panels, we again obtain small and statistically insignificant estimates for overall spending and along the intensive margin. For the extensive margin, however, we estimate beneficiaries were 1.4 (Panel B) and 1.6 (Panel C) percentage points more likely to make use of Part D coverage (both $p < 0.01$). These results differ from our findings for spending in inpatient, outpatient, and physician services in that they do not suggest an intertemporal substitution effect, or at least not one that dominates any additional consumption caused by the downward-sloping-demand effect. One possible explanation is that pharmaceuticals may be more difficult to substitute across time than other medical services. For spending on the extensive margin, our positive effects may be partially explained by the fact that dual beneficiaries are automatically enrolled in Part D (as compared to those not dually enrolled, for whom it is optional). Since the expansion induced an increase in dual coverage, the share of Medicare enrollees enrolled in Part D also increases, in contrast to Parts A and B, where the share enrolled stays constant regardless of changes in dual coverage. Thus, our estimates here may not reflect a behavioral change among enrollees in terms of when or what they consume, but instead a change in who is paying for it.

4.5 Health Conditions

Above we covered the question of whether the ACA Medicaid expansion led to spillovers in Medicare in terms of dual coverage and spending under Medicare, but the question still remains as to whether the spillovers we found resulted in any measurable changes in the health of enrollees. We investigate this question using our DDD estimation strategy (equation (2)), which reflects effects on our pre-65 group, since it is the only one for which we found any coverage and spending impacts, and these represent a necessary "first-stage" for us to expect health effects. We report these results in *Table 8*, where each estimate comes from a different regression on a different, binary outcome that indicates whether a beneficiary experienced that condition at some point in a year (measured at the end of the year).

Our results for 26 separate health conditions in *Table 8* suggest changes in dual enrollment and Medicare resulted in few impacts on measures of Medicare enrollee health. We observe statistically significant decreases for two chronic conditions ($p < 0.01$): chronic kidney disease and colorectal cancer. On the other hand, we observe an increase in the likelihood of acquired hypothyroidism ($p < 0.1$). Given that our negative estimates among these significant results have a much higher level of significance, and one would likely consider chronic kidney disease and colorectal cancer to be more severe health conditions than hypothyroidism, one might interpret these results as suggesting some improvement in health. Given the large number of outcomes in which we find no effect, however, we consider our findings here to represent evidence of little or no overall health impact.

5. Limitations

Finally, some limitations of our study are worth noting. First, we are not able to link our administrative records for Medicare enrollees to information about them before age 65. Thus, we cannot directly observe Medicaid take-up or consumption before that age. We address this issue by splitting our sample by age cohort, which allows us to apply economic reasoning based on the ages individuals were exposed to the expansion to draw inferences about behavior before 65. Second, due to a data collection issue, our sample is missing records for Medicare beneficiaries born in 1950 and 1951 in the years 2015 and 2016. We perform a robustness check for our estimates where we drop an analogous group of enrollees from the pre-period, finding similar results. This suggests that this missing information may not be critical to our conclusions. Lastly, the administrative Medicare data we use lacks individual-level information on socio-economic status, preventing us from directly controlling for it in our models. To confront this problem, we control for zip-code level measures of income, poverty, and other demographics, and employ DD models to account for group-level differences in these characteristics (to the extent they affect coverage and spending). While we argue our results are credible in spite of these weaknesses, readers should keep them in mind in interpreting our results.

6. Conclusion

It has been nearly ten years since the ACA expansion of Medicaid, and since then we have obtained a better understanding of many of its direct impacts. We are still learning, however, about the ways it has indirectly affected people. Spillovers from health insurance expansions have been a point of anxiety for some policymakers and health system commentators, particularly with respect to those that might affect Medicare. One common concern has been the following question: "If an expansion results in more people trying to access health care services, would access to care for Medicare beneficiaries be reduced?" While several sets of authors have studied this question, previous research has produced conflicting answers (Joynt et al. 2013; Bond and White 2013; Joynt et al. 2015; McInerney, Mellor, and Sabik 2017; Glied and Hong 2018; Carey, Miller, and Wherry 2020). In this paper, we consider these indirect effects and the ACA Medicaid expansion, performing a comprehensive evaluation of its spillovers into the Medicare population, studying effects on dual enrollment, Medicare spending, and health conditions.

Despite causing an influx of newly insured patients, we do not find the ACA Medicaid expansion caused undesirable consequences on Medicare enrollees. Indeed, for individuals already over 65 at the time of the expansion, we find no evidence of coverage or spending effects, implying they were not crowded out of accessing care. Instead, we document that those who were under 65 at the time of the ACA expansion experienced an increase of 14.8 percent in the likelihood of being dual enrollees after turning 65. We also show that they experienced reductions in Medicare spending of 10.2 percent on average, which we argue is mainly due to Medicaid giving access to care earlier than if they had to wait until turning 65 and enrolling in Medicare. We find little evidence, however, that these changes in spending and coverage influenced the likelihood of having common chronic health conditions – at least over the course of four post-expansion years. Overall, our findings suggest that, rather than negatively affecting Medicare enrollees, spillovers from the expansion had a positive effect, resulting in greater insurance coverage and improved timing of health care consumption.

Our results also help reconcile disagreements among the previous work in this literature. We agree with those finding no provider capacity constraints that reduce access (Carey, Miller, and Wherry, 2020; Joynt et al., 2013; Joynt et al., 2015) and with those that some saw reduced health care consumption (Bond and White 2013; McInerney, Mellor, and Sabik 2017; Glied and Hong 2018).[18] We are able to agree with both by focusing on the heterogeneity of spillovers by the age at which an individual was exposed to the ACA expansion, thereby elucidating the important role of intertemporal substitution. This underscores the importance of (what we would call) an under-researched mechanism: the effects of the exposure to Medicaid expansion before age 65.

Our findings also have some important policy implications related to health care consumption by the elderly and near-elderly. First, we find that the expansion led to individuals substituting care across time, which suggests that some near-elderly were not obtaining needed care in the years shortly before turning 65. This emphasizes the importance of timely treatment of senior and near-elderly populations – so-called "treatment on time" or preemptive care. The ACA helped those in expansion states address these needs, but they may still exist for the near-elderly in non-expansion states. Second, we find that the take-up of dual coverage did not lead to large amounts of additional consumption of care under Medicare via downward-sloping-demand effects. This is encouraging because it suggests Medicare beneficiaries were not missing out on a lot of needed care or prescriptions for reasons due to out-of-pocket costs (an especially notable result given that we focus on a population from lower-income areas). Hence, it seems that *some* near-elderly Americans may be lacking the ability to obtain care, but both Medicaid and Medicare appear to help address this problem when they are available. Third, our results imply that the actual cost to the government of the ACA Medicaid expansion is smaller than would be suggested by the fiscal outlays that pay for Medicaid since those outlays result in lower average costs for Medicare. In making further decisions about changes to health care programs, therefore, policymakers should not assume spillovers always increase costs. In some cases, as with the ACA Medicaid expansion, they might reduce them.

7. References

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[18] Our findings are particularly in congruence with those of Bond and White (2013) in that we both find consumption changes are driven entirely on the intensive margin.

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Table 1: Descriptive statistics

	Pre-ACA			Post-ACA		
	[1]		[2]	[3]		[4]
	Expansion		Non-expansion	Expansion		Non-expansion
Dual coverage	.275		.277	.229	***	.206
Spending – Inpatient, Outpatient, & Physician Services ($)	6,226	***	5,515	6,033	***	5,400
Having positive spending (binary)	.886	***	.898	.899	***	.909
Age	67.0	***	67.0	67.3	***	67.4
Female	.601	**	.595	.587		.588
White	.786	***	.725	.771	***	.737
State unemployment rate (%)	8.66	***	8.24	5.40	***	5.08
Zip-code level characteristics:						
Rent median price ($)	711	***	735	718	***	740
With social security benefits (%)	35.3		35.4	35.1	***	35.2
Average family size	3.14	***	3.24	3.13	***	3.22
Median age	39.2	***	38.8	39.1	***	38.8
Number of observations	65,375		119,576	74,454		124,850

Notes: The sample is restricted to beneficiaries from lower-income, higher-poverty zip codes, as described in Section 3.1. Spending is defined as the sum of inpatient, outpatient, and physician services spending. We annualize spending for new Medicare enrollees aged 65 as their spending reflects only part of a year. Spending values are converted to 2017 dollars using the CPI. Statistical difference, via two-tailed t-test, indicated by asterisks: * = p-value < 0.1; ** = p-value < 0.05; *** = p-value < 0.01.

Table 2: Summary of birth cohorts' role in research design

Each cell identifies a set of birth-year by calendar-year observations; Shaded cells mark observations used to estimate effects on our Pre-65 beneficiaries; Unshaded cells mark those used for Post-65 effects.

Individuals born in 1949 or later were under 65 after 2014; Those born before 1949 were 65 by the time Medicaid expanded in 2014.

	Pre-ACA				**Post -ACA**			
Age	2010	2011	2012	2013	2014	2015	2016	2017
65	Born 1945	Born 1946	Born 1947	Born 1948	Born 1949	Born 1950*	Born 1951*	Born 1952
66	Born 1944	Born 1945	Born 1946	Born 1947	Born 1948	Born 1949	Born 1950*	Born 1951
67	Born 1943	Born 1944	Born 1945	Born 1946	Born 1947	Born 1948	Born 1949	Born 1950
68	Born 1942	Born 1943	Born 1944	Born 1945	Born 1946	Born 1947	Born 1948	Born 1949
69	Born 1941	Born 1942	Born 1943	Born 1944	Born 1945	Born 1946	Born 1947	Born 1948

Note: Asterisks (*) indicate where Medicare enrollees born in 1950 and 1951 are missing in 2015 and 2016 datasets.

Table 3: Summary of predicted effects and possible mechanisms by group

Group on which effects apply and estimation sample	Research Design	Predicted Effect by Outcome (Proposed Mechanism)	
		Medicaid/Dual Coverage	Medicare Spending
Group: Post-65 enrollees *Sample:* Post-65 enrollees & pre-ACA counterparts	DD (Equation (1))	Increase (Outreach, publicity, streamlined enrollment and renewal)	Increase (Downward-sloping demand) Decrease (Crowd out) *(Dual coverage null effect for post-65 group implies downward-sloping demand does not apply as it relies on take-up of Medicaid)*
Group: Pre-65 enrollees *Sample:* Pre-65 enrollees (& pre-ACA counterparts)	DD (Equation (1))	Increase (Outreach, publicity, streamlined enrollment and renewal) Increase (On-ramp effect)	Increase (Downward-sloping demand) Decrease (Crowd out) Decrease (Intertemporal substitution)
Group: Pre-65 enrollees *Sample:* All enrollees	DDD – Difference between above two DDs (Equation (2))	Ex-ante prediction: Increase (On-ramp effect)	Ex-ante prediction: Decrease (Intertemporal substitution) *(Dual coverage and spending null results for post-65 group imply predicted direction is unclear; could reflect intertemporal substitution, downward-sloping demand, or both)*

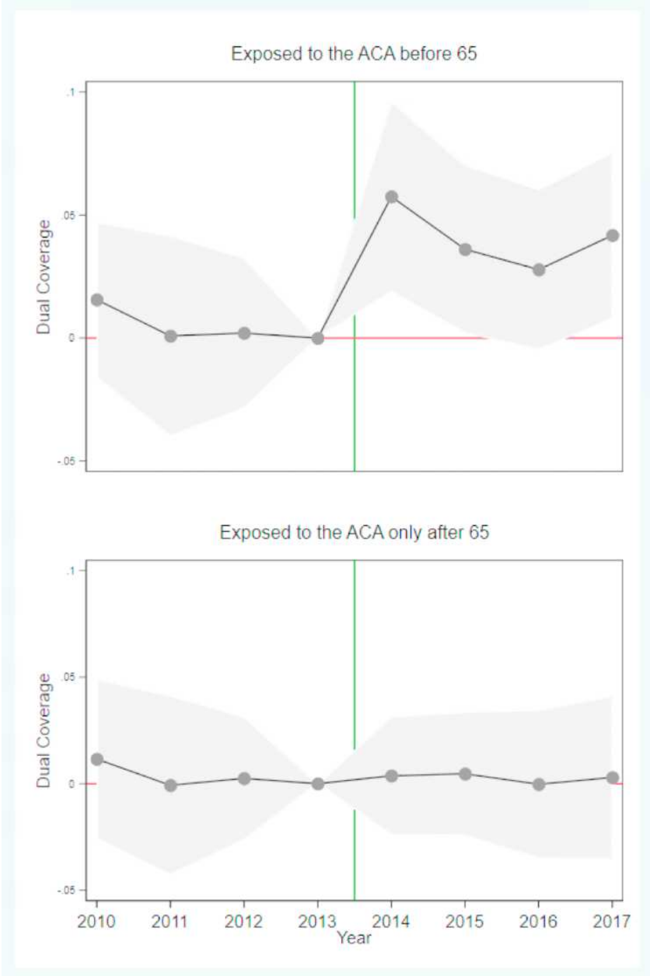
Notes: Post-65 enrollees are those who were older than 65 when Medicaid expanded. Pre-65 enrollees were under 65 at the time of the expansion.

Table 4: Effects of Medicaid expansion on dual coverage and Medicare spending

	Coverage Effect [1]	Spending Effect [2]	Intensive Margin [3]	Extensive Margin [4]
	Dual Enrollment	Spending (Inpatient, Outpatient, & Physician Services)		
Panel A: Post-65 beneficiaries (DD)				
Post*Exp	-.002	99.4	97.7	.002
	(.018)	(181)	(193)	(.002)
Pre-ACA value	.274	5,967	6,492	.919
N. observations	213,476	213,476	196,481	213,476
Panel B: Pre-65 beneficiaries (DD)				
Post*Exp	.039**	-461**	-554**	.007*
	(.013)	(216)	(251)	(.004)
Pre-ACA value	.279	5,572	6,409	.869
N. observations	170,779	170,779	149,276	170,779
Panel C: All sample (DDD)				
Exp*Pre65*Post	.041**	-590**	-686**	.005
	(.018)	(270)	(298)	(.005)
Pre-ACA value	.277	5,767	6,451	.894
N. observations	384,255	384,255	345,757	384,255

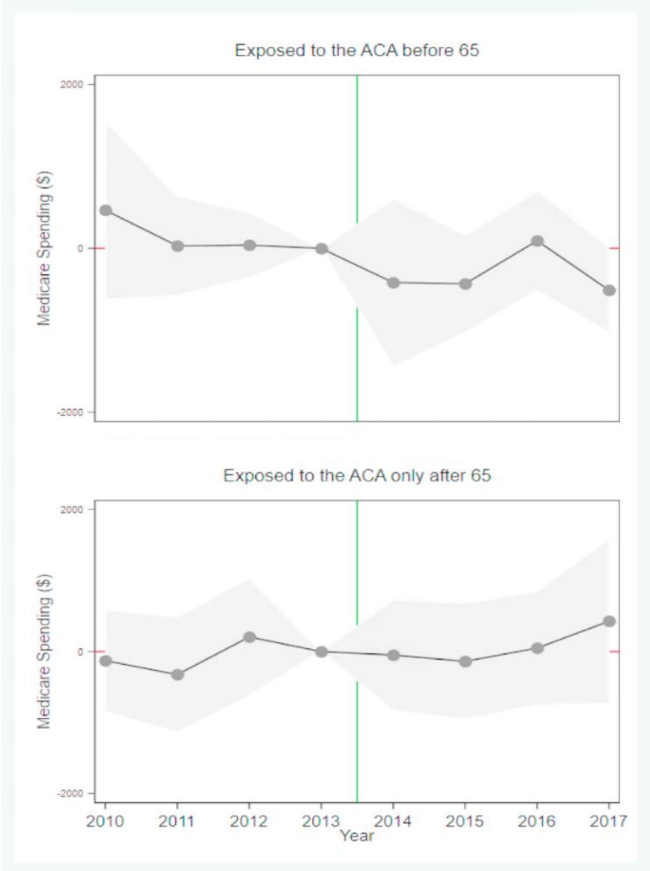
Notes: Panels A and B present estimates based on equation (1). Panel C estimates are based on equation (2). All models include year, state, and birth-year fixed effects, and other controls discussed in Section 3.3. Medicare spending is the combination of inpatient, outpatient, and physician services. The sample is restricted to beneficiaries from lower-income, higher-poverty zip codes, as described in Section 3.1. Intensive margin samples include only those enrollees who had positive Medicare spending in the given year. Parentheses denote standard errors, which are clustered at the state level. Results from two-tailed t-tests indicated by asterisks: * = p-value < 0.1; ** = p-value < 0.05; *** = p-value < 0.01.

Figure 1: Event Study Estimates of Dual Coverage Effects



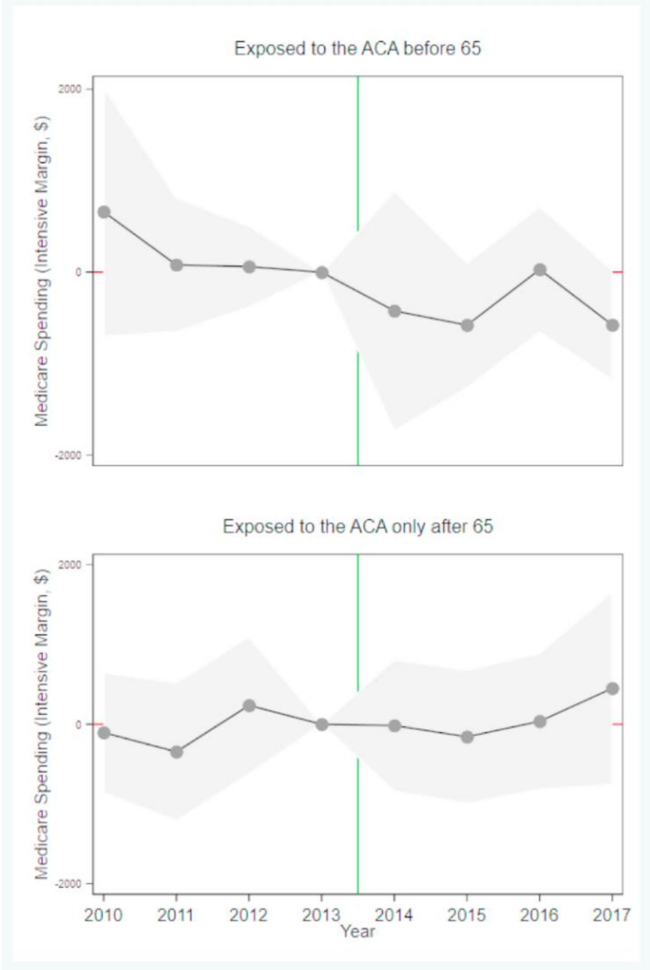
Note: The top panel in the figure contains results for the pre-65 group while the bottom has those for the post-65 group. Shaded areas represent 95 percent confidence intervals. The vertical line indicates the beginning of the Medicaid expansion.

Figure 2: Event Study Estimates of Medicare Consumption Effects



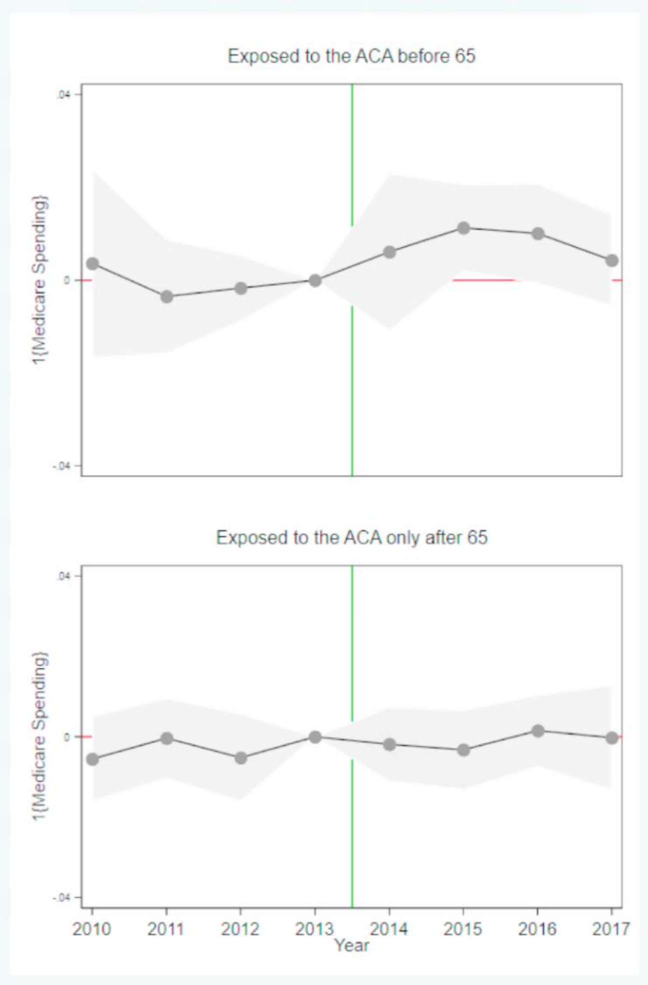
Note: The top panel in the figure contains results for the pre-65 group while the bottom has those for the post-65 group. Shaded areas represent 95 percent confidence intervals. The vertical line indicates the beginning of the Medicaid expansion.

Figure 3: Event Study Estimates of Medicare Consumption Effects – Intensive Margin



Note: The top panel in the figure contains results for the pre-65 group while the bottom has those for the post-65 group. For both graphs, we only include those whose Medicare spending is positive in the given year. Shaded areas represent 95 percent confidence intervals. The vertical line indicates the beginning of the Medicaid expansion.

Figure 4: Event Study Estimates of Medicare Consumption Effects – Extensive Margin



Note: The top panel in the figure contains results for the pre-65 group while the bottom has those for the post-65 group. For both graphs, the outcome variables are binary indicators. Shaded areas represent 95 percent confidence intervals. The vertical line indicates the beginning of the Medicaid expansion.

Table 5: Robustness checks

	Coverage Effect [1]	Spending Effect [2]	Intensive Margin [3]	Extensive Margin [4]
	Dual Enrollment	Spending (Inpatient, Outpatient, & Physician Services)		
Panel A: Excluding 2014				
Exp*Pre65*Post	.039**	-616**	-701**	.005
	(.018)	(271)	(299)	(.005)
N. observations	328,868	328,868	296,420	328,868
Panel B: Excluding Medicare enrollees with RDS				
Exp*Pre65*Post	.043**	-594**	-689	.005
	(.018)	(274)	(301)	(.005)
N. observations	382,323	382,323	344,104	383,323
Panel C: Including states New Jersey, Colorado, and Connecticut				
Exp*Pre65*Post	.041**	-445*	-506*	.003
	(.016)	(231)	(257)	(.005)
N. observations	422,519	422,519	380,284	422,519
Panel D: Excluding 2010				
Exp*Pre65*Post	.038**	-520*	-610*	.006
	(.017)	(289)	(317)	(.005)
N. observations	347,296	347,296	312,725	347,296
Panel E: Excluding individuals who were born in 1946 and 1947 in 2011 and 2012				
Exp*Pre65*Post	.041**	-653**	-717**	.004
	(.018)	(251)	(273)	(.005)
N. observations	358,045	358,045	327,332	358,045
Panel F: Excluding Michigan and New Hampshire				
Exp*Pre65*Post	.041*	-553*	-652*	.004
	(.021)	(311)	(344)	(.005)
N. observations	358,736	358,736	322,866	358,736
Panel G: Changing income thresholds				
Exp*Pre65*Post	.039**	-483**	-560**	.005
	(.018)	(236)	(262)	(.005)
N. observations	361,222	361,222	324,463	361,222

Notes: All estimates are based on our DDD model in equation (2). All models include year, state, and birth-year fixed effects, and other controls discussed in Section 3.3. The sample is restricted to beneficiaries from lower-income, higher-poverty zip codes, as described in Section 3.1. Medicare spending is the combination of inpatient, outpatient, and physician services. Intensive margin samples include only those enrollees who had positive Medicare spending in the given year. Parentheses denote standard errors, which are clustered at the state level. Results from two-tailed t-tests indicated by asterisks: * = p-value < 0.1; ** = p-value < 0.05; *** = p-value < 0.01.

Table 6: Heterogeneous effects by race and gender

	Coverage Effect [1]	Spending Effect [2]	Intensive Margin [3]	Extensive Margin [4]
	Dual Enrollment	Spending (Inpatient, Outpatient, & Physician Services)		
White				
Exp*Pre65*Post	.028	-717**	-833**	.006
	(.018)	(284)	(309)	(.005)
N. observations	287,478	287,478	262,534	287,478
Non-White				
Exp*Pre65*Post	.085***	25.6	32.9	.007
	(.028)	(499)	(562)	(.009)
N. observations	96,777	96,777	83,223	96,777
Female				
Exp*Pre65*Post	.024	-1,051**	-1,187**	.008
	(.019)	(475)	(506)	(.008)
N. observations	227,460	227,460	208,722	227,360
Male				
Exp*Pre65*Post	.067**	48.5	44.5	-.001
	(.023)	(425)	(479)	(.008)
N. observations	156,795	156,795	137,032	156,795

Notes: All estimates are based on our DDD model in equation (2). All models include year, state, and birth-year fixed effects, and other controls discussed in Section 3.3. The sample is restricted to beneficiaries from lower-income, higher-poverty zip codes, as described in Section 3.1. Medicare spending is the combination of inpatient, outpatient, and physician services. Intensive margin samples include only those enrollees who had positive Medicare spending in the given year. Parentheses denote standard errors, which are clustered at the state level. Results from two-tailed t-tests indicated by asterisks: * = p-value < 0.1; ** = p-value < 0.05; *** = p-value < 0.01.

Table 7: Effects of Medicaid expansion on Medicare Part D spending

	Spending Effect [1]	Intensive Margin [2]	Extensive Margin [3]
Panel A: Post-65 beneficiaries (DD)			
Post*Exp	-50.5	-45.9	-.002
	(115)	(132)	(.003)
N. observations	213,476	194,054	213,476
Panel B: Pre-65 beneficiaries (DD)			
Post*Exp	26.4	21.8	.014***
	(55.0)	(60.8)	(.004)
N. observations	170,779	148,654	170,779
Panel C: All sample (DDD)			
Exp*Pre65*Post	79.3	70.9	.016***
	(121)	(139)	(.004)
N. observations	384,255	342,708	384,255

Notes: Panels A and B present estimates based on equation (1). Panel C estimates are based on equation (2). All models include year, state, and birth-year fixed effects, and other controls discussed in Section 3.3. The sample is restricted to beneficiaries from lower-income, higher-poverty zip codes, as described in Section 3.1. Intensive margin samples include only those enrollees who had positive Medicare Part D spending in the given year. Parentheses denote standard errors, which are clustered at the state level. Results from two-tailed t-tests indicated by asterisks: * = p-value < 0.1; ** = p-value < 0.05; *** = p-value < 0.01.

Table 8: Spillover effects of Medicaid expansion on selected health conditions

	Acute Myocardial infarction	Heart Failure	Ischemic heart disease	Atrial fibrillation	Cataract	Chronic kidney	Chronic obstructive pulmonary disease
Exp* Pre65*Post	-.001	-.009	-.004	-.004	.006	-.018***	-.006
	(.001)	(.006)	(.008)	(.004)	(.007)	(.005)	(.005)
N. observations	384,225	384,225	384,225	384,225	384,225	384,225	384,225
	Diabetes	Glaucoma	Hip fracture	Depression	Osteoporosis	Endometrial cancer	Anemia
Exp* Pre65*Post	-.009	-.010	-.0002	-.003	.003	-.001	-.002
	(.013)	(.006)	(.001)	(.006)	(.003)	(.001)	(.006)
N. observations	384,225	384,225	384,225	384,225	384,225	384,225	384,225
	Asthma	Hyper-lipidemia	Prostatic hyper-plasia	Hyper-tension	Acquired hypothyroidism	Prostate Cancer	Lung cancer
Exp* Pre65*Post	-.006	-.002	.004	-.008	.009*	-.002	.001
	(.004)	(.010)	(.003)	(.014)	(.005)	(.003)	(.002)
N. observations	384,225	384,225	384,225	384,225	384,225	384,225	384,225
	Rheumatoid Arthritis	Stroke	Breast Cancer	Colorectal cancer	Alzheimer		
Exp* Pre65*Post	.006	.001	.001	-.004***	-.001		
	(.009)	(.003)	(.003)	(.001)	(.002)		
N. observations	384,225	384,225	384,225	384,225	384,225		

Notes: Outcome variables are end-of-year indicators that are equal to one if a condition was found in beneficiary claims, even if claims coverage was not complete for the year, and zero otherwise. All regressions are based on our DDD model, equation (2), and include year, state, and birth-year fixed effects, and other controls discussed in Section 3.3 (except the chronic condition indicators, which are dropped from the independent variables of these models). The sample is restricted to beneficiaries from lower-income, higher-poverty zip codes, as described in Section 3.1. Parentheses denote standard errors, which are clustered at the state level. Results from two-tailed t-tests indicated by asterisks: * = p-value < 0.1; ** = p-value < 0.05; *** = p-value < 0.01.

Online Appendix

Appendix Table 1: Inpatient and outpatient health costs

	Consumption Effect		Intensive Margin		Extensive Margin	
	Inpatient	Outpatient & Physician Services	Inpatient	Outpatient & Physician Services	Inpatient	Outpatient & Physician Services
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Post-65 beneficiaries (DD)						
Post*Exp	78.4	21.0	1,606**	18.6	-.004	.002
	(140)	(105)	(624)	(112)	(.004)	(.002)
Pre-ACA value	3,133	2,834	21,618	3,092	.145	.917
N. observations	213,476	213,476	28,214	195,968	213,476	213,476
Panel B: Pre-65 beneficiaries (DD)						
Post*Exp	-240	-221**	-1,758	-274**	-.002	.007**
	(201)	(108)	(1,394)	(126)	(.004)	(.004)
Pre-ACA value	2,809	2,763	25,402	3,188	.111	.867
N. observations	170,779	170,779	18,132	148,879	170,779	170,779
Panel C: All Sample (DDD)						
Exp* Pre65*Post	-348	-242	-3,447**	-293	.002	.005
	(230)	(159)	(1399)	(179)	(.005)	(.005)
Pre-ACA value	2,968	2,798	23,282	3,140	.127	.891
N. observations	384,255	384,255	46,346	344,847	384,255	384,255

Notes: Panels A and B present estimates based on equation (1). Panel C estimates are based on equation (2). All models include year, state, and birth-year fixed effects, and other controls discussed in Section 3.3. The sample is restricted to lower-income, higher-poverty beneficiaries, as described in Section 3.1. Intensive margin samples include only those enrollees who had positive Medicare spending in the given year. Parentheses denote standard errors, which are clustered at the state level. Results from two-tailed t-tests indicated by asterisks: * = p-value < 0.1; ** = p-value < 0.05; *** = p-value < 0.01.

Appendix Table 2: Robustness checks for post-65 beneficiaries

	Coverage Effect [1]	Consumption Effect [2]	Intensive Margin [3]	Extensive Margin [4]
	Dual Enrollment	Spending (Inpatient, Outpatient, & Physician Services)		
Panel A: Excluding 2014				
Exp*Post	-.002	123	106	.002
	(.019)	(194)	(205)	(.003)
N. observations	166,830	166,830	153,970	166,830
Panel B: Excluding Medicare enrollees with RDS				
Exp*Post	-.003	103	99.7	.002
	(.018)	(183)	(196)	(.002)
N. observations	212,672	212,672	195,758	212,672
Panel C: Including states New Jersey, Colorado, and Connecticut				
Exp*Post	.0002	124	118	.003
	(.017)	(173)	(185)	(.005)
N. observations	234,625	234,625	216,005	234,625
Panel D: Excluding 2010				
Exp*Post	.003	86.3	94.2	.001
	(.017)	(182)	(19.5)	(.002)
N. observations	178,947	178,947	164,948	178,947
Panel E: Excluding individuals born in 1946 and 1947 from 2011 and 2012 datasets				
Exp*Post	-.002	99.4	97.7	.002
	(.018)	(181)	(193)	(.002)
N	213,476	213,476	196,481	213,476

Notes: All estimates are based on equation (1), using our post-65 sample. All models include year, state, and birth-year fixed effects, and other controls discussed in Section 3.3. The sample is restricted to lower-income, higher-poverty beneficiaries, as described in Section 3.1. Medicare spending is the combination of inpatient, outpatient, and physician services. Intensive margin samples include only those enrollees who had positive Medicare spending in the given year. Parentheses denote standard errors, which are clustered at the state level. Results from two-tailed t-tests indicated by asterisks: * = p-value < 0.1; ** = p-value < 0.05; *** = p-value < 0.01.