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The Effects of State Medicaid Expansions for Working-Age Adults on Senior Medicare Beneficiaries†

By Melissa McInerney, Jennifer M. Mellor, and Lindsay M. Sabik*

Do Medicaid expansions to working-age adults affect healthcare spending and utilization among older Medicare beneficiaries? Although economic theory provides conflicting predictions about the presence and direction of such spillover effects, it does identify circumstances when spillovers can reduce Medicare spending. Using data on Medicaid expansions during the 2000s and microdata from the Medicare Current Beneficiary Survey, we find that a 1 percentage point rise in the share of working-age adults eligible for Medicaid has modest effects on the average Medicare beneficiary's spending, but reduces average spending by $477 among dual eligibles. Importantly, we find no evidence of adverse health effects. (*JEL* G22, H75, I12, I13, I18, I38, J14)

In the next decade, provisions in the Affordable Care Act (ACA) will expand public health insurance coverage to as many as 13 million persons per year (Congressional Budget Office 2014). Recent findings from the Oregon Health Insurance Experiment show that adults who are newly covered by Medicaid use more healthcare (Finkelstein et al. 2012, Taubman et al. 2014). Similarly, analysis of Massachusetts' comprehensive coverage expansion finds significant reductions in treatment delays and unmet healthcare needs (Long and Stockley 2011). In addition to these direct effects on the newly insured, persons already covered by health insurance could experience spillover effects from Medicaid expansions, such as reductions or delays in care (Boskin 2013). Given the needs of the aged and disabled, spillovers that reduce access for Medicare beneficiaries may be particularly problematic if they also adversely affect health.

Economic theory, however, provides conflicting predictions about the presence and direction of spillovers from Medicaid expansions to Medicare healthcare

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provision. While no single theoretical model fits this exact type of spillover, insights can be drawn from models of physician behavior in two-payer settings or in the presence of heterogeneous payment arrangements. For example, in a mixed economy model of physician behavior, Medicaid expansions can increase the amount of healthcare supplied to the Medicare population, assuming that Medicaid expansions crowd out private insurance (e.g., Garthwaite 2012). Alternatively, in the absence of crowd out, the supply of healthcare to Medicare beneficiaries would not change since Medicaid reimburses at lower rates than Medicare. Elsewhere in the literature, theories about the spillover effects of restrictive insurance coverage (Glied and Zivin 2002) suggest a scenario where Medicaid expansions could decrease the healthcare supplied to Medicare beneficiaries. In addition to these competing theoretical predictions, the few existing empirical studies on this topic produce mixed results (Sommers, Baicker, and Epstein 2012; Bond and White 2013; Joynt et al. 2013; Glied 2014; Joynt et al. 2015).

Our paper makes several key contributions to the small prior literature on spillovers from Medicaid expansions to Medicare beneficiaries. First, we review the extant theoretical literature to develop a set of testable predictions about spillover effects under different assumptions of crowd-out, physician interaction with both Medicaid and Medicare patients, and reimbursement rate differences between Medicaid and Medicare. This conceptual framework suggests that negative spillover effects (used here to refer to scenarios in which Medicaid expansions *reduce* the healthcare supplied to Medicare beneficiaries) can arise from physicians' decisions to reduce the intensity of treatment offered to all patients in response to an increased number of patients covered by more restrictive insurance coverage, under the fixed-cost hypothesis of Glied and Zivin (2002). Thus, we predict that negative spillovers will be more likely to occur for those Medicare patients seen by physicians who also treat large numbers of working-aged Medicaid patients, and residing in states where Medicaid reimburses at much lower rates than Medicare.

Second, we use microdata from the Medicare Current Beneficiary Survey (MCBS) Cost and Use files and state data from various sources to test the specific predictions regarding spillover effects. Since our conceptual framework suggests that spillover effects depend on the degree to which Medicare beneficiaries use the same healthcare providers as newly covered Medicaid recipients, we use discharge data to document that low-income Medicare enrollees who are covered by both Medicare and Medicaid (i.e., dual eligibles or duals) are more likely than other Medicare beneficiaries to be treated by physicians who also treat working-age Medicaid patients. We then test for spillover effects in separate samples of duals and non-duals in the MCBS.[1] Our conceptual framework also suggests that spillover effects will be larger when Medicaid payments are less generous, and we find some evidence that reductions in Medicare spending are larger in states with lower ratios of Medicaid to Medicare payments. To our knowledge, our study is the first to test for heterogeneous spillover effects.

Third, we use the MCBS data to examine the impact of Medicaid eligibility expansions on health status as well as spending and utilization. This allows us to assess the

[1] We do not observe payer-mix for the providers who treat MCBS respondents.

welfare implications of observed spillovers. Further, we perform subgroup analyses to test whether Medicare beneficiaries who experience larger changes in spending also experience larger health effects.

Finally, we examine spillovers using a more comprehensive set of Medicaid expansions to working-age adults than previously considered. Most prior studies examine spillover effects in Massachusetts only (e.g., Bond and White 2013, Joynt et al. 2013, Joynt et al. 2015). The sole prior national study examines only large State Children's Health Insurance Program (SCHIP, or currently CHIP) expansions to adults (Glied 2014). In contrast, we identify the effects of public insurance expansions across the entire United States using variation in eligibility arising from all state-level Medicaid and CHIP expansions to working-age adults during the 2000s. Thus, we are able to include other large expansions that the prior literature did not include, such as IowaCare, Insure Oklahoma, the New Mexico State Coverage Insurance program, and the Healthy Indiana Plan, all implemented under 1115 waivers during the mid-2000s. Further, while most studies use a binary indicator for an expansion, we use a measure of simulated eligibility, defined as the percent of a nationally representative sample of adults aged 20–64 that would be eligible for Medicaid based on the eligibility criteria in place in each state and year. Using this continuous measure allows the intensity of the expansions to vary across states.

To preview our results, we find that a 1 percentage point rise in the share of working-age adults eligible for Medicaid reduces annual healthcare spending by \$87 per Medicare beneficiary. However, consistent with the prediction that spillovers will be more significant among Medicare patients treated by physicians who also treat large numbers of Medicaid patients, we find larger reductions in annual healthcare spending of \$477 per person among dual eligibles.

Importantly, we find little evidence that reductions in healthcare spending represent negative spillovers in a normative sense. That is, we do not find evidence that duals' health, as measured by self-reported health status and mortality, worsens following a state Medicaid expansion. Thus, our overall results are consistent with declines in spending that reduce so-called "overuse" or "overtreatment" among Medicare enrollees. We also show that our results for spending cannot be explained by changes in the types of duals who take-up Medicaid coverage following expansions, by differential mortality among duals in expansion states, or by preexisting trends.

The leading explanation for our results is that they stem from physician responses to changes in fixed practice components brought on by changes in the heterogeneous payment environment. That is, when physicians who treat both Medicare and Medicaid patients experience increases in the share of Medicaid patients (whose coverage is more restrictive), these physicians reduce treatment intensity for all patients. This explanation is supported by some evidence that medical provider spending reductions are larger for duals residing in states with less generous Medicaid payments relative to Medicare.

I. Prior Literature on Medicaid Expansions

Numerous studies show that enrollment in public insurance increases individuals' healthcare use. For example, low-income adults covered by Oregon's 2008

Medicaid expansion used more preventive care, outpatient care, and prescription drugs, and had a higher likelihood of having a hospitalization (Finkelstein et al. 2014, Taubman et al. 2014). Relatedly, Wisconsin's 2009 expansion of public insurance to low-income childless adults increased outpatient and emergency department visits among the newly insured, although it reduced inpatient hospitalizations (DeLeire et al. 2013).

Since our study looks at the impact of Medicaid eligibility expansions (as opposed to Medicaid participation), an important question is whether *eligible* individuals who live in states with expanded Medicaid eligibility use more healthcare. This may not always be the case if eligible individuals fail to take-up coverage or if expansions crowd-out employer-sponsored or nongroup insurance coverage. Nonetheless, several findings suggest that state eligibility expansions do increase healthcare coverage and utilization among eligible persons.

First, multiple studies show that state-level Medicaid expansions to parents lead to statistically significant increases in Medicaid participation and overall insurance coverage (e.g., Kronick and Gilmer 2002, Aizer and Grogger 2003, Busch and Duchovny 2005, Hamersma and Kim 2013, McMorrow et al. 2016). Second, estimates of crowd-out associated with adult Medicaid expansions are typically smaller than estimates associated with expansions to children (Buchmueller, Ham, and Shore-Sheppard 2015). This is not surprising because expansions to working-age adults are usually more modest than CHIP expansions to children; most expansions to these adults extend eligibility to persons at or below 200 percent of the federal poverty level (FPL), while 19 states cover children in families with incomes above 300 percent of the FPL through CHIP (KFF 2016). Thus, adults impacted by public insurance expansions are poorer than many children targeted by CHIP expansions, and these poorer adults are less likely to have private insurance in the first place. Accordingly, several studies find that parental Medicaid expansions have no statistically significant effects on private coverage for adults (e.g., Aizer and Grogger 2004, Busch and Duchovny 2003, Hamersma and Kim 2013), and recent analysis by Atherly et al. (2016) shows that the majority of new adults covered by Medicaid HIFA waivers (which encourage new state approaches to expand insurance among childless adults) had no private insurance in the prior year. McMorrow et al. (2016) examines state Medicaid expansions to parents taking place between 1997 and 2009 and does find evidence of crowd-out, but those estimates are lower than estimates based on CHIP expansions to children. McMorrow et al. (2016) estimate that 33 percent of the increase in Medicaid coverage was due to a decline in employer sponsored private insurance, while Gruber and Simon (2008) estimate that 60 percent of the increase in children's CHIP coverage was due to a drop in private coverage.

Third, several studies show that when states implement coverage expansions to working-age adults, their healthcare use increases. For example, Massachusetts' 2006 health reforms (which expanded coverage through a combination of Medicaid and subsidized private insurance) reduced reports of unmet healthcare needs due to cost and instances of delayed care, and increased the likelihood of having an office visit with a nurse practitioner or physician assistant (Long 2008; Long and Masi 2009; Long and Stockley 2011; Long, Stockley, and Dahlen 2012; Miller

2012; Long, Stockley, and Nordahl 2012).[2] Sommers, Baicker, and Epstein (2012), which focuses on Medicaid expansions to childless adults in New York, Arizona, and Maine, find that the expansions led to significant increases in insurance coverage and self-reported health and significant reductions in delays in care. Further, county-level mortality rates among 20–64-year-olds decreased in New York, relative to a comparison state that did not expand coverage.

In the prior literature that is directly related to our research question, there are a small number of studies that examine whether Medicaid expansions have spillover effects for the Medicare population.[3] Several focus on spillovers from Massachusetts' health reform to Medicare beneficiaries' healthcare use. Joynt et al. (2015) examine chronically ill Medicare beneficiaries and find no change in outpatient service use and an increase in average spending following the 2006 Massachusetts reforms. In a related study, Joynt et al. (2013) find that the Massachusetts reforms reduced preventable hospitalizations for Massachusetts' seniors, which they interpret as the absence of negative spillovers on Medicare beneficiaries. However, Bond and White (2013) find that although Medicare primary care use increased in Massachusetts overall, it decreased in the areas where the largest insurance expansions took place, evidence they interpret as negative spillovers. Sommers, Baicker, and Epstein (2012) find that seniors in three states with Medicaid expansions to childless adults experienced *increased* access to care as well as declines in cost-related care delays and mortality.[4] Finally, in the only prior national study, Glied (2014) uses state-level data to examine how CHIP expansions to adults impacted Medicare hospitalization and Part B Medicare spending. She finds that in states with substantial CHIP expansions, Medicare surgical discharge rates and Part B spending per beneficiary both fell by about 3 percent. To summarize, these prior studies provide mixed evidence for spillover effects and differ greatly in terms of their methods.

Our study differs from the bulk of the literature in our use of national data from all states and DC and a comprehensive measure of Medicaid eligibility expansions; because our eligibility measure is continuous, we can also account for differences in expansion size. Moreover, our work differs from all of the literature, including the national study by Glied (2014), in several important ways. First, we examine different mechanisms behind spillover effects. For example, we use individual-level data to test for heterogeneous effects (by type of Medicare beneficiary and by Medicaid payment generosity) that are predicted by the economic theory underlying our analysis. Second, we use a more comprehensive set of spending, utilization, and health measures for individuals enrolled in Medicare, and third, we examine the persistence of spillover effects over time. Lastly, we also use our data to rule out explanations for our findings that are not related to spillovers, such as differential selection, differential mortality, and preexisting trends.

[2] In contrast, Long and Stockley (2011) find that the New York Medicaid expansion did not increase access to or use of healthcare, although it did increase insurance coverage.

[3] A few studies have looked at spillover effects from Medicaid parental coverage expansions to children's healthcare use (e.g., Davidoff et al. 2003, Dubay and Kenney 2003). Garthwaite (2012) examines spillover effects from public insurance expansions to privately insured patients.

[4] Kolstad and Kowalski (2012) find related evidence that the Massachusetts expansion led to increases in non-elderly Medicare inpatient discharges (table 5); however, they also report no change in elderly inpatient discharges (table 2).

II. Conceptual Framework

We now turn to economic theories that explain the presence of spillovers across payers. While no single theoretical model fits the specific case of spillovers to Medicare beneficiaries arising from Medicaid expansions, important insights can be drawn from models of physician behavior in two-payer settings or in the presence of heterogeneous payment arrangements. In this section, we review both types of models and develop several testable implications for our empirical work.

A. *Mixed-Economy Models*

The most frequently cited model in the analysis of spillover effects is Sloan, Mitchell, and Cromwell (1978) or SMC, a mixed-economy model in which physicians treat patients covered by two payers. One payer is a government program that pays a fixed reimbursement per service; the other is a private insurer. Notably, the Glied (2014) study of spillovers to Medicare arising from CHIP expansions follows this general approach. Physicians are assumed to be price-takers for the public insurer (Medicare) and to face a downward-sloping demand for healthcare from adults under age 65. Glied (2014) then predicts that increased demand by adults under age 65 will reduce the quantity of care provided to Medicare patients. However, a key assumption is that the marginal revenue from a newly insured adult under age 65 exceeds the marginal revenue from a Medicare patient.[5] This assumption is reasonable for increases in employer-sponsored insurance that are also examined in Glied (2014), and perhaps even for CHIP expansions.[6] However, in the context of the Medicaid expansions we focus on, this assumption is not supported; evidence clearly shows that Medicaid reimburses at lower levels than Medicare in almost all states (Zuckerman et al. 2003, Zuckerman and Goin 2012).

Another application of the SMC model examines physician responses to public insurance expansions specifically when the public insurer reimburses at a lower rate (e.g., Garthwaite 2012, Baker and Royalty 2000). Again, physicians are thought to face downward-sloping demand for private patients and to be price-takers for publicly insured patients. This gives rise to the marginal revenue curve ABCD shown in Figure 1. A key assumption is that because of crowd out, the public insurance expansion decreases privately insured demand and the marginal revenue curve becomes EFCD.[7] Physicians with no pre-expansion contact with publicly insured patients, such as those with labor supply S′, will increase services provided to publicly insured patients; physicians treating some publicly insured patients pre-expansion, such as those with labor supply S″, will not change their share of patients covered by public insurance. However, in the absence of crowd-out, the expansion instead shifts the marginal revenue curve to ABGH;[8] this affects only those physicians who, pre-expansion, were constrained in treating publicly insured

[5] See figure 1 in Glied (2014).

[6] For example, Bronstein, Adams, and Florence (2004) report that CHIP reimbursement rates followed private insurer schedules in Alabama, a state with a standalone CHIP program.

[7] Private demand falls from AB to EF due to crowd out.

[8] Demand among publicly insured individuals rises from BC to BG.

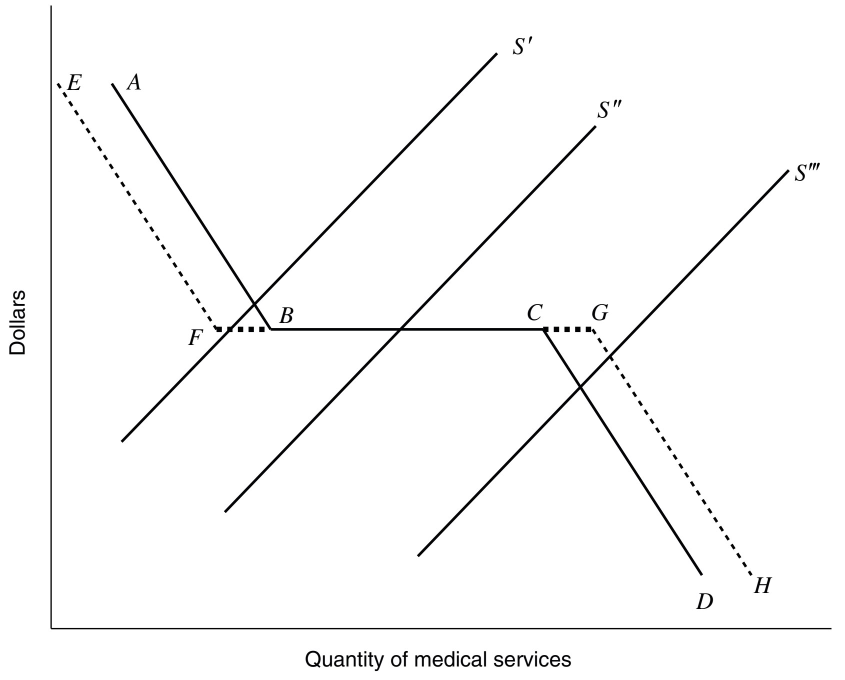

Figure 1. Physician Marginal Revenue and Labor Supply

patients by the relatively low number of publicly insured patients (such as physicians with labor supply S‴). Such physicians will increase services provided to publicly insured patients.

This version of the SMC model can be used to predict how Medicaid expansions affect the quantity of services supplied to Medicare patients. In our context, it is assumed that physicians face downward-sloping demand from patients covered by private insurance, are price-takers for patients covered by Medicare, and Medicaid reimburses at a lower rate than Medicare. The SMC model offers a prediction that varies with crowd out.

PREDICTION 1: *If Medicaid coverage of working-age adults crowds out private insurance coverage, Medicaid expansions to working-age adults will* increase *the supply of services to Medicare patients. However, in the absence of crowd-out, Medicaid expansions would have* no effect *on the supply of services to Medicare patients.*

As we noted earlier, several empirical studies of Medicaid expansions to working-age adults produce much lower estimates of crowd-out than the CHIP expansions to children that Garthwaite (2012) uses this model to study. Absent crowd-out and reduced demand by the privately insured, the SMC model suggests there would be no change in Medicare services. Medicaid expansions simply create a new source of publicly insured patients whose care is reimbursed at rates typically below those of Medicare. Profit-maximizing physicians would have no incentive to treat a Medicaid patient over a Medicare patient, all else equal.

B. *Alternate Models of Physician Behavior*

Many studies show that Medicaid expansions to working-age adults increase healthcare utilization among this group (e.g., DeLeire et al. 2013, Long and Stockley 2011). While this effect could be explained by the SMC model for physicians with supply curves similar to S''' in Figure 1, Baker and Royalty (2000) support an alternative explanation.[9] They posit that the profit-maximizing behavior modeled in SMC may not apply to "public" physicians who work outside of private office-based settings and instead work in hospitals, clinics, medical schools, universities, or state and local government. In such settings, institutional goals or government policies may lead physicians to provide care "to all patients who seek care, generally without regard to levels of Medicaid payment and private fees or to the type of insurance coverage by the patient" (Baker and Royalty 2000, 485). In this framework, public physicians meet the residual demand for care not supplied by private physicians. Unlike the SMC model, Baker and Royalty predict that Medicaid expansions will increase the amount of care supplied to those patients by public physicians even in the absence of crowd-out of private insurance. They offer empirical support for this by showing that expanded Medicaid eligibility increases patients' access to physicians working in public settings, but has no effect on access to private office-based physicians.[10]

While the SMC model does not offer insights into physician behavior for physicians who treat patients regardless of payer (e.g., Medicaid, Medicare, uninsured, or private), a useful approach for considering spillovers in this context is Glied and Zivin (2002). The Glied and Zivin framework examines physician treatment intensity when physicians treat some patients with fee-for-service insurance and others in managed care plans, a context that is applicable to our analysis of Medicare and Medicaid patients. First, most Medicare enrollees are enrolled in traditional fee-for-service Medicare, and most low-income adults in Medicaid are in managed care plans. Specifically, in states that cover low-income adults under Medicaid expansions, the median managed care penetration rate in this population is 90 percent (Kaiser Family Foundation 2015). Second and more importantly, even when Medicaid enrollees are in fee-for-service plans, the fact that Medicaid reimburses providers at lower rates than Medicare creates the type of payment environment heterogeneity that Glied and Zivin (2002) describe. We adopt their framework to consider physicians treating two sets of patients, one with more generous insurance coverage (fee-for-service or FFS in their context, Medicare in ours) and one with more restrictive insurance coverage (managed care in their context, Medicaid in ours).

The three models presented by Glied and Zivin (2002) offer different predictions for Medicaid-Medicare spillovers in our context. In one model, physicians take patients with restrictive coverage only when they have excess capacity. In a second model, physicians respond to increases in the share of patients with restrictive

[9] Baker and Royalty (2000) also argue that it is unlikely that physicians, pre-expansion, are limited by the number of Medicaid patients seeking care.

[10] Our data do not permit us to identify physicians working in public settings.

coverage by inducing demand among patients covered by generous insurance. In a third model, Glied and Zivin (2002) allow there to be fixed and variable costs of production of physician services. Fixed costs are such items as durable equipment, office capacity, patient visit duration, weekly office hours, and whether/when to have call-in times available (He and White 2013); these costs are fixed in the sense that they do not vary by the type of insurance a patient has. Glied and Zivin theorize that physicians choose these fixed cost components based on the makeup of their practice. Therefore, if more patients with restrictive coverage enter a practice, the fixed cost components shift as if the patient base were entirely comprised of restrictive coverage. Glied and Zivin (2002) predict that treatment intensity of patients in generous plans will decrease as physicians treat more patients with restrictive coverage.

These models give rise to three additional predictions in our context:

PREDICTION 2: *If physicians accept Medicaid patients because they have excess capacity*, *Medicaid expansions to working-age adults will have* no effect *on healthcare supplied to Medicare enrollees.*

PREDICTION 3: *If physicians respond to the less generous reimbursement of newly insured Medicaid patients by inducing demand among patients with more generous coverage*, *Medicaid expansions will* increase *the supply of services to Medicare patients. Unlike Prediction* 1, *this result arises in the absence of crowd-out.*

PREDICTION 4: *As Medicaid expansions increase the share of Medicaid patients seen by the physician*, *the physician will alter fixed costs to reflect the level that would be optimal for a patient base of Medicaid patients*, *for whom reimbursement is lower. As a result*, *Medicaid expansions will* reduce *the healthcare supplied to Medicare patients.*

The fixed cost hypothesis behind Prediction 4 is supported by empirical evidence from several studies. Using patient-level data, Glied and Zivin (2002) find that increases in the practice's HMO penetration rate decrease treatment intensity. A number of empirical studies on managed care spillover effects find similar patterns. For example, Chernew, DeCicca, and Town (2008) and Baicker, Chernew, and Robbins (2013) find that exogenous changes in Medicare managed care penetration driven by changing Medicare Advantage payment rules reduce costs per hospitalization, length of hospital stay, and average healthcare spending for FFS Medicare beneficiaries. Changing practice pattern norms, similar to those hypothesized in Glied and Zivin (2002) are one proposed mechanism for this type of spillover, and studies that focus on the effects of managed care on the intensity of treatment for AMI patients support this (e.g., Bundorf et al. 2004).[11]

Finally, as extensions to Predictions 3 and 4, we posit that changes in Medicare treatment intensity will vary by the degree to which Medicare providers also treat

[11] See Chernew, Baicker, and Martin (2010) for a detailed review of the spillover literature.

patients covered by Medicaid as well as by the difference between Medicare and Medicaid payment rates, as stated below:

PREDICTION 5: *Medicaid expansions to working-age adults will cause larger changes (increases or decreases) in the supply of services to Medicare patients who seek care from physicians also treating large shares of Medicaid patients.*

PREDICTION 6: *Medicaid expansions to working-age adults will cause larger changes (increases or decreases) in the supply of services to Medicare patients residing in states where Medicaid provider payments are much less generous than Medicare.*

III. Data and Methods

To examine the spillover effects of Medicaid expansions to working-age adults on Medicare utilization, we use data from the Cost and Use files of the MCBS, a nationally representative sample of Medicare beneficiaries conducted by the Centers for Medicare and Medicaid Services (CMS). We use 9 years of data from 2001 through 2009; in each year, approximately 10,000 Medicare beneficiaries are surveyed as part of a rotating panel.[12] We focus on Medicare beneficiaries age 65 and older enrolled in Medicare Parts A and B for the entire year and living in the community all year. Pooling observations from all years for which we have non-missing data on covariates in the models yields a sample of 71,709 individuals.[13]

The MCBS Cost and Use files are well-suited to our research question because they contain the most comprehensive account of the healthcare services received by Medicare beneficiaries as well as the amounts paid for these services and the sources of payment. CMS develops the Cost and Use files by combining and reconciling survey responses from beneficiaries with Medicare administrative files. This reconciliation process improves upon the survey data by validating the accuracy of reported events, and it supplements Medicare payments from administrative records with amounts paid by other payers reported on the survey. As a result the Cost and Use files report services received and amounts paid regardless of whether the payer was Medicare, patient out-of-pocket spending, private insurance, Medicaid, or some other source. This is especially important given that Medicare reimburses about half of the average beneficiary's total healthcare costs (Lind 2012).[14] Additionally, the Cost and Use files report events and spending for events regardless of whether the respondent was in traditional fee-for-service Medicare or a Medicare Advantage plan.

[12] The MCBS captures up to three years of healthcare utilization data for each respondent.

[13] There are 88,521 observations of persons aged 65 and older in the pooled MCBS Cost and Use files from 2001 through 2009. We drop 9,860 observations of persons not enrolled in both Medicare Parts A and B for the full calendar year, another 5,826 observations of persons residing in a facility all or part of the year, and another 1,126 observations lacking data on covariates.

[14] This statistic is not the same as the actuarial value of Medicare, which is 80 percent; the "about half" statistic is calculated using healthcare spending that is not covered by Medicare (such as dental care, some home health care, and some institutional care).

A. *Measures of Healthcare Spending, Utilization, and Health*

We examine several measures of healthcare spending to capture changes in both the frequency and intensity of utilization. We use five measures of all-payer healthcare spending, including total healthcare spending on all service types, and healthcare spending for inpatient care, outpatient hospital care, medical provider events, and prescribed medicine.[15] We then examine Medicare spending measures for the same five types of care, where spending includes both traditional Medicare and Medicare Advantage plan spending.[16] The average MCBS respondent in our sample incurs nearly $11,400 per year in total healthcare spending. Approximately one-quarter is spent on inpatient care, and one-third is spending on medical provider events. The average respondent has close to $7,000 per year in Medicare spending. See Table 1 for additional descriptive statistics on the spending measures.

Our main analysis also includes other measures of healthcare utilization and three measures of self-reported health. We examine several measures of the quantity of healthcare used, including the numbers of inpatient events, medical provider events, outpatient hospital events, and prescribed medicine events.[17] See Table 3 for additional descriptive statistics on the utilization measures. The three measures of self-reported health that we examine are an indicator of poor/fair general health, an indicator that health limits the respondent's social activities, and an indicator that health is worse relative to last year. On average, one in five respondents is in poor or fair health, and one in five reports worsening health. About 12 percent of respondents report that health limits their social activities. See Table 5 for additional summary statistics on self-reported health status.

B. *Measure of Medicaid Expansions to Working-Age Adults*

We measure Medicaid expansions to working-age adults with a state-level measure of the percent of the population aged 20–64 eligible for Medicaid in each year. Since a well-known problem in studies identifying the effects of coverage expansions is that actual program enrollment reflects local economic conditions as well as policy variables, we follow the practice of quantifying simulated eligibility, or the percent of a nationally representative sample of persons who would be eligible using the eligibility rules in place for each state and year (e.g., Currie and Gruber 1996, Gruber and Simon 2008).

We use information on Medicaid income eligibility rules for non-elderly, non-disabled adults from various sources. Reports from the Kaiser Family Foundation provide the main source of information on parental coverage for most years (Kaiser Family Foundation State Health Facts 2014). We supplement this information with other reports, fact sheets, and administrative documents from foundations, states, and the Centers for Medicare and Medicaid Services. Certain gaps in information

[15] These measures are drawn from the Person Summary RIC. Medical provider events include various medical services, such as care provided by physicians and others under the supervision of physicians, including separately billing doctors and separately billing labs, and other medical expenses.

[16] These measures are drawn from the Service Summary RIC.

[17] These measures are defined from Person Summary RIC measures and pertain to all payers.

were addressed through conversations with state officials. The compiled data summarize Medicaid income eligibility thresholds as a percent of the FPL for parents and childless adults in each state-year through multiple pathways, including traditional 1931 parental coverage and 1115 waiver coverage (including through the HIFA waiver program). For the simulated eligibility measure used here, we include eligibility through all Medicaid pathways that provide comprehensive coverage (excluding a small number of waiver programs that primarily provide premium support for employer-sponsored health insurance).

We construct the simulated eligibility measure using all non-elderly adults (age 20–64 years) in the 2000 CPS March Supplement. For each individual, we calculate income as a percent of FPL based on family size. We then determine eligibility for Medicaid for this consistent sample in each state and year based on a comparison of parental status, employment status, and income to the compiled eligibility information from the states. We then aggregate the data, using the appropriate survey weights, to construct a state-year measure of simulated eligibility, defined as the percentage of the national non-elderly adult sample that would be eligible for Medicaid in each state and year.[18]

The simulated eligibility measure has a mean of 4.99 percent (weighting the data by observations in the MCBS) and ranges from 0.7 percent (in Alabama and Arkansas) to 48 percent (in Hawaii). Mean simulated eligibility increases over our sample period from 3.08 percent to 6.74 percent as a consequence of numerous state public insurance expansions to childless adults and parents. Our identifying variation arises from Medicaid expansions occurring in different states over time. For example, simulated eligibility increased by 24 percentage points in Indiana following the implementation of the 2008 Healthy Indiana Plan, a plan that extended Medicaid coverage to adults with household incomes up to 200 percent of the federal poverty level. We document 13 instances of simulated eligibility increasing by more than 5 percentage points in consecutive years, and the mean increase in simulated eligibility corresponding to these expansions is 12.6 percentage points.[19]

C. *Testing for Spillovers*

We first test Predictions 1 through 4 by estimating models of spending and healthcare use. We estimate models of healthcare spending using two-part models,[20] an approach commonly used in the healthcare literature to model both the likelihood of receiving healthcare and the amount of healthcare received, which is appropriate for outcomes with a large number of zeros (Deb and Trivedi 2012).[21] The two parts

[18] California implemented a large Medicaid expansion in a subset of counties in 2007. We calculate separate measures of simulated eligibility for expansion and non-expansion counties and calculate a population weighted average of these measures to represent overall simulated eligibility for the state of California.

[19] Most year-to-year changes are small: the average change in simulated eligibility year-to-year is less than 1 percentage point. This reflects updates to income eligibility thresholds for parental coverage.

[20] Two-part models are estimated using the -tpm- command in Stata (Belotti and Deb 2012). Marginal effects for our key explanatory variable are estimated with the -margins- command in Stata.

[21] It is common to have a large number of zeros in distributions of healthcare spending. For example, in our sample of Medicare beneficiaries, only 19 percent have any inpatient spending.

of the model are estimated independently. For the first part, we estimate the probit model below:

$$(1) \qquad P(y_{ist} > 0 \,|\, SimElig_{s,t-1}, X_{ist}) = \Phi(\beta_1 SimElig_{s,t-1} + \Gamma X_{ist}),$$

where y is a measure of spending. For the second part, we estimate a generalized linear model (GLM), as shown below:

$$(2) \qquad \ln\big(E\big(y_{ist} \,|\, SimElig_{s,t-1}, X_{ist}, y_{ist} > 0\big)\big) = \alpha_1 SimElig_{s,t-1} + \Pi X_{ist}.$$

We choose a log-link function (to account for the skewed nature of our spending data) and we assume a Poisson function for the variance (i.e., variance is proportional to the mean) (Manning and Mullahy 2001).[22] Then the predicted change in spending is given by the product of the probability of having any spending from the first part (probit) and the expected spending levels from the second part (GLM):

$$(3) \quad E(y_{ist} \,|\, SimElig_{s,t-1}, X_{ist}) \\ = P\big(y_{ist} > 0 \,|\, SimElig_{s,t-1}, X_{ist}\big) \times E\big(y_{ist} \,|\, SimElig_{s,t-1}, X_{ist}, y_{ist} > 0\big).$$

We estimate models of the numbers of inpatient, medical provider, outpatient, or prescribed medicine events using Poisson models since the dependent variables are counts, or non-negative integers.

In all models, the key explanatory variable is the percentage of a nationally representative sample of adults aged 20–64 that would be eligible for Medicaid coverage following each state's eligibility rules in the prior year (denoted $SimElig\,(t-1)$ in the tables). We focus on the prior year's eligibility rules since many changes in program eligibility take place mid-year and our simulated eligibility variable is based on the program rules in place for most (but not necessarily all) of a calendar year. Thus, $SimElig\,(t-1)$ consistently captures a full calendar year over which eligibility changes are in place after an expansion. Further, changes in enrollment and utilization among the working-age Medicaid population will not occur immediately after an eligibility change, but will phase in over a number of months. Estimates of a positive relationship between simulated eligibility and spending would be consistent with both profit-maximizing physicians and crowd-out of private insurance (Prediction 1) and demand inducement (Prediction 3). The absence of a relationship between simulated eligibility and spending would support profit-maximization in the absence of crowd-out (Prediction 1) or excess capacity among physicians (Prediction 2). Finally, a negative relationship would be consistent with physicians changing their patterns of practice as more of their patients are covered by a restrictive insurer (Prediction 4).

[22] The log-link function is of course equivalent to

$$E(y_{ist} \,|\, SimElig_{s,t-1}, X_{ist}, y_{ist} > 0) = e^{(\alpha_1 SimElig_{s,t-1} + \Pi X_{ist})}.$$

We include controls for various determinants of spending and utilization. Individual-level controls defined from the MCBS include age and its square, sex, race/ethnicity, household income and its square, household size, educational attainment, marital status, veteran status, and residence in an urban area. We also include controls for the number of chronic conditions, smoking, and body mass index. See online Appendix Table A1 for additional descriptive statistics for the various samples. State-level controls include the annual unemployment rate and the annual percent of Medicaid enrollees in comprehensive managed care plans.[23] Models of total healthcare spending include area-level controls for the hospital wage index and three physician practice costs indices. In models of inpatient and outpatient hospital spending, we control for the hospital wage index, and in models of medical provider spending we control for the physician practice cost indices.[24] All models include state and year fixed effects and a full set of state-specific linear time trends to account for unobservable factors within the state that impact healthcare spending and utilization and vary over time. We adjust the standard errors of the estimated regression coefficients for clustering at the state-level. For the two-part models, we report marginal effects and delta method standard errors.

Prediction 5 suggests that the largest changes in spending and utilization will occur among those Medicare beneficiaries whose physicians treat large shares of working-age Medicaid patients.[25] We test this prediction by estimating our models for separate samples of dual-eligible and non-dual Medicare enrollees. Dual-eligible enrollees are low-income seniors who are eligible for Medicare because of their age and eligible for Medicaid because of their low incomes. Since Medicaid is the payer of last resort, physicians who treat duals are reimbursed by Medicare, while Medicaid covers cost sharing for the patient. Since both working-aged and elderly Medicaid enrollees are low income, we expect that dual eligibles are more likely than non-duals to reside in the same communities and to seek care from the same physicians as working-age Medicaid recipients. To illustrate the ties these patients share, we use Healthcare Cost and Utilization Project (HCUP) data and show that physicians treating dual-eligible beneficiaries are roughly twice as likely to treat working-age Medicaid patients than physicians treating non-duals.[26] The results are shown in online Appendix Table A2. This pattern holds for both ambulatory care

[23] CMS enrollments reports from recent years are available online (http://www.medicaid.gov/medicaid-chip-program-information/by-topics/data-and-systems/medicaid-managed-care/medicaid-managed-care-enrollment-report.html) and data were obtained directly from CMS staff.

[24] We obtained wage index data for 2000–2008 from the NBER website (www.nber.org/data/CBSA-MSA-wage-index.html) and for 2009 from the CMS website (www.cms.gov/Medicare/Medicare-Fee-for-Service-Payment/AcuteInpatientPPS/Wage-Index-Files-Item). We obtained data on physician practice cost indices from the CMS website (https://www.cms.gov/apps/physician-fee-schedule/search/search-criteria.aspx). All data were merged to MCBS respondents by their county of residence, using crosswalks from MSA/CBSA/carrier locality to county as appropriate.

[25] Note that the potential for Medicare patients in general to be treated by physicians also treating Medicaid patients is fairly significant. Using data from the 2000–2001 Community Tracking Study—Physician Survey, we calculate that over 71 percent of physicians report that their practice accepts some patients covered by Medicare and at least some patients covered by Medicaid. Moreover, 42 percent of physicians report that their practice accepts all new Medicare and all new Medicaid patients.

[26] We use one year of the Healthcare Cost and Utilization Project (HCUP) State Inpatient Databases (SID) and State Ambulatory Surgery and Services Databases (SASD) from the three states that report multiple payer fields and a physician identification number on each discharge record (Kentucky, Maryland, and New Jersey). In each state, we examine a sample of physicians identified as the attending physician for patients 18 years and up and for at least

as well as inpatient care, even though inpatient care is likely less common among working-age adults. Thus, Prediction 5 would be supported by evidence that the effects of Medicaid expansions on spending and utilization are larger among dual eligibles.

Prediction 6 suggests that negative spillovers will be larger in states where Medicaid is relatively less generous compared to Medicare. To test this, we measure payment generosity with the Medicaid-to-Medicare physician fee ratio for primary care, as reported in Zuckerman et al. (2003). This ratio is constructed for each state by first taking the ratio of the Medicaid fee to the Medicare fee for each service and then using spending weights to combine the service-specific ratios into a single Medicaid-to-Medicare ratio (MMR). We use data from 2003, which is the earliest year for which this measure is available for all states. We then separately estimate spending models for duals residing in states with a relatively low MMR and duals residing in states with a higher MMR. We define "low" MMR values as those below the twenty-fifth, fiftieth, or seventy-fifth percentile of Medicaid generosity. A finding that spending changes are larger among respondents living in low MMR states would be consistent with Prediction 6.

IV. Results

A. *Spillover Effects on Spending and Utilization*

Table 1 reports the key results from two-part models of healthcare spending, namely, the estimated marginal effects of a 1 percentage point increase in the percent of working-age adults eligible for Medicaid. Results from the models estimated with the full sample of Medicare beneficiaries are shown in column 1. The all-payer spending models shown in the top panel suggest that Medicaid expansions have very modest effects on Medicare beneficiaries' total spending. Namely, a 1 percentage point rise in the percent of adults eligible for Medicaid (a 20 percent increase) reduces overall spending by $87 per beneficiary (a 0.7 percent decrease). The decrease reflects small declines in spending on medical providers ($53 per beneficiary), outpatient hospital care ($22), and prescribed medicines ($9). In the Medicare spending models shown in the bottom panel, the marginal effects of simulated eligibility increases are smaller still, and statistically significantly different from zero in only one case. In that case, a 1 percentage point increase in the share of adults eligible for Medicaid corresponds to a $9 decline in Medicare spending for outpatient hospital care. Overall, results from the full sample suggest that Medicaid expansions cause either small decreases in Medicare spending (consistent with Prediction 4, the fixed costs hypothesis) or no change in Medicare spending (consistent with Predictions 1 and 2). We see no support for Prediction 3, that Medicaid expansions will increase supply to Medicare beneficiaries through increased demand inducement.

The remaining columns of Table 1 report marginal effects from two-part models estimated separately for dual eligibles and all other Medicaid beneficiaries

one Medicare patient. We then calculate the fraction of those physicians who are also identified as the attending physician on the discharge records of 18–64-year-old patients with Medicaid coverage.

Table 1—Effects of Eligibility Expansions on Healthcare Spending by Type of Insurance

	Marginal effect of *SimElig* $(t-1)$		
	Full sample (1)	Dual eligibles (2)	Non-duals (3)
Panel A. All payer spending, 2009$			
All payments, all service types	−86.858	−476.602	−38.664
	(34.23)	(82.82)	(40.55)
	$\overline{Y}$ = 11,372	$\overline{Y}$ = 14,652	$\overline{Y}$ = 10,978
	(19,131)	(22,284)	(18,677)
All payments for inpatient events	7.274	−229.681	32.608
	(14.22)	(57.94)	(12.38)
	$\overline{Y}$ = 2,992	$\overline{Y}$ = 4,361	$\overline{Y}$ = 2,828
	(9,868)	(13,128)	(9,388)
All payments for medical provider events	−52.981	−94.239	−47.316
	(25.37)	(35.57)	(29.54)
	$\overline{Y}$ = 3,642	$\overline{Y}$ = 3,913	$\overline{Y}$ = 3,610
	(8,036)	(6,276)	(8,225)
All payments for outpatient hospital events	−21.591	−52.098	−18.060
	(7.98)	(12.93)	(8.54)
	$\overline{Y}$ = 1,278	$\overline{Y}$ = 1,484	$\overline{Y}$ = 1,253
	(5,013)	(4,744)	(5,044)
All payments for prescribed medicine events	−9.058	−2.170	−8.972
	(2.71)	(9.80)	(3.23)
	$\overline{Y}$ = 2,239	$\overline{Y}$ = 3,135	$\overline{Y}$ = 2,131
	(2,578)	(3,357)	(2,448)
Panel B. Medicare spending, 2009$			
Medicare payments, all service types	−29.321	−432.937	21.235
	(23.82)	(75.80)	(19.63)
	$\overline{Y}$ = 6,969	$\overline{Y}$ = 11,055	$\overline{Y}$ = 6,489
	(14,297)	(19,557)	(13,480)
Medicare payments for inpatient events	4.198	−214.431	26.827
	(11.86)	(52.79)	(10.63)
	$\overline{Y}$ = 2,619	$\overline{Y}$ = 3,946	$\overline{Y}$ = 2,460
	(9,052)	(12,179)	(8,587)
Medicare payments for medical provider events	−7.411	−79.890	1.280
	(5.70)	(20.61)	(4.88)
	$\overline{Y}$ = 2,252	$\overline{Y}$ = 2,739	$\overline{Y}$ = 2,193
	(4,182)	(4,399)	(4,151)
Medicare payments for outpatient hospital events	−9.331	−42.393	−5.251
	(4.05)	(12.07)	(4.12)
	$\overline{Y}$ = 821	$\overline{Y}$ = 1,161	$\overline{Y}$ = 780
	(2,943)	(3,914)	(2,801)
Medicare payments for prescribed medicine events	−1.885	8.707	−1.753
	(3.29)	(11.18)	(2.74)
	$\overline{Y}$ = 545	$\overline{Y}$ = 1,606	$\overline{Y}$ = 418
	(1,650)	(3,042)	(1,337)

Notes: Sample sizes range from 71,429 to 71,699 for the full sample; 7,461 to 7,679 observations for dual eligibles; and 63,650 to 64,019 observations for non-duals. All models include controls for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, smoking participation, BMI, number of chronic conditions, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Controls for the hospital wage index and three physician practice costs indices are added to models of all service spending; the hospital wage index is added to models of inpatient and outpatient hospital spending, and physician practice cost indices are added to models of medical provider spending. Robust standard errors clustered by state are reported in parentheses below estimated marginal effects. The mean (standard deviation) of the outcome variable for the regression sample is reported for each model.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data (non-duals). Based on Prediction 5 and the analysis of discharge data reported earlier, we expect Medicaid expansions to working-age adults will have larger (positive or negative) effects on healthcare spending among dual eligibles. The results in column 2 show that duals experience large reductions in healthcare spending as Medicaid coverage to working-age adults expands. A 1 percentage point increase in the Medicaid eligibility of working-age adults reduces duals' all-payer spending by \$477 and reduces their Medicare spending by \$433. The overall reduction comes from reductions in inpatient spending (\$214–\$230), medical provider spending (\$80–\$94), and hospital outpatient spending (\$42–\$52). For the non-duals, changes in simulated eligibility have no effect on total Medicare and all-payer spending. There are some significant marginal effects when we measure spending by type of setting; for example, increases in Medicaid eligibility have small positive effects on inpatient spending and small negative effects on all-payer hospital outpatient spending. Overall, the results in Table 1 support Predictions 4 and 5—that negative spillovers exist for beneficiaries treated by physicians who also treat more working-age Medicaid patients.[27]

Table 2 examines the two-part models more closely to see whether the negative spillovers we document for dual eligibles arise from reductions in the likelihood of receiving any care and/or reductions in the intensity of care (e.g., spending per encounter). For the most part, the marginal effects of Medicaid eligibility expansions are working through reductions in healthcare on the intensive margin (as evident in the size and significance of the GLM marginal effects reported in column 2). Medicaid expansions usually do not have negative significant effects in the probit models of any spending, except for inpatient spending. For example, in the probit model of any all-payer inpatient spending, the marginal effect of a 1 percentage point increase in Medicaid eligibility is -0.004. Since 25.2 percent of the dual eligible population has any inpatient spending, this corresponds to a 1.6 percent decline in the probability of any inpatient use. The evidence in Table 2 provides further support for the fixed cost hypothesis behind negative spillover effects, which suggests that physicians who experience an increase in patients with more restrictive coverage will reduce treatment intensity, or treatment on the intensive margin.

In Table 3, we examine the effects of Medicaid expansions on measures of healthcare utilization among Medicare beneficiaries. We present results from Poisson models of the counts of inpatient events, outpatient hospital visits, medical provider events, and prescribed medical events. For the average Medicare beneficiary, increased Medicaid eligibility has no effect on inpatient events and causes a small significant decline in the number of medical provider events. For non-duals, increased Medicaid eligibility has no effect on any measure of healthcare utilization. In contrast, Medicaid expansions have large negative effects on dual-eligible beneficiaries' inpatient admissions and medical provider events. Specifically, a 1 percentage

[27] Other dimensions of heterogeneity between duals and non-duals could contribute to the results we observe. For example, we considered whether the spending differences between duals and non-duals are driven by the greater illness burden among duals and examined differences in spending effects between healthy and sick non-duals. We did not see a pattern of larger negative spending reductions among unhealthy non-duals, suggesting that the differences we observe between duals and non-duals are not driven by their greater illness burden (results available upon request). Nonetheless, other differences, such as differences in residential location, may still play some role.

Table 2—Effects of Eligibility Expansions on Healthcare Spending, 2001–2009, Dual Eligibles

	Coefficient on *SimElig* $(t-1)$			
	Probit (marginal effect) (1)	GLM (marginal effect) (2)	Marginal effect of *SimElig* $(t-1)$ (3)	Observations (4)
Panel A. All payer spending, 2009$				
All payments, all service types	0.001	−496.409	−476.602	7,525
	(0.001)	(84.227)	(82.825)	
All payments for inpatient events	−0.004	−620.501	−229.681	7,679
	(0.002)	(197.772)	(57.944)	
All payments for medical provider events	0.002	−102.769	−94.239	7,461
	(0.001)	(36.809)	(35.572)	
All payments for outpatient hospital events	−0.0004	−68.856	−52.098	7,679
	(0.002)	(16.499)	(12.929)	
All payments for prescribed medicine events	0.001	−4.254	−2.170	7,507
	(0.001)	(10.146)	(9.802)	
Panel B. Medicare spending, 2009$				
Medicare payments, all service types	0.00001	−447.849	−432.937	7,466
	(0.001)	(77.958)	(75.799)	
Medicare payments for inpatient events	−0.004	−592.013	−214.431	7,679
	(0.002)	(180.688)	(52.788)	
Medicare payments for medical provider events	−0.0003	−84.936	−79.890	7,660
	(0.0014)	(21.836)	(20.607)	
Medicare payments for outpatient hospital events	−0.002	−56.068	−42.393	7,679
	(0.003)	(15.539)	(12.068)	
Medicare payments for prescribed medicine events	0.001	12.234	8.707	7,672
	(0.001)	(23.476)	(11.179)	

Notes: All models control for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, enrollment in FFS Medicare, smoking participation, BMI, the number of chronic conditions, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Controls for the hospital wage index and three physician practice costs indices are added to models of all service spending; the hospital wage index is added to models of inpatient and outpatient hospital spending and physician practice cost indices are added to models of medical provider spending. Robust standard errors clustered by state are reported in parentheses.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

point increase in Medicaid eligibility reduces the number of inpatient admissions by 5.6 percent and the number of medical provider events by 3.1 percent among dual eligibles. These empirical results are again consistent with negative spillovers explained by changes in fixed practice costs, and consistent with Predictions 4 and 5.

We next examine support for Prediction 6, which suggests that negative spillovers will be larger in states where Medicaid is relatively less generous compared to Medicare. Results from this exercise are shown in Table 4, which reports the combined marginal effects from two-part models. Under Prediction 6, we would expect to see that Medicaid expansions cause larger spending reductions for individuals residing in less generous states (i.e., with low ratios of Medicaid to Medicare fees for primary care). We focus on the results presented in column 3 for medical provider spending because the measure of payment generosity pertains to relative physician fees for primary care. We note that in every case, there is a statistically significant reduction in spending for medical provider events in the

Table 3—Effects of Eligibility Expansions of Healthcare Utilization by Type of Insurance

	Poisson coefficient on *SimElig* $(t-1)$		
	Full sample (1)	Dual eligibles (2)	Non-duals (3)
Number of inpatient events	−0.005	−0.056	0.005
	(0.005)	(0.008)	(0.004)
	$\overline{Y} = 0.31$	$\overline{Y} = 0.47$	$\overline{Y} = 0.29$
	(0.82)	(1.08)	(0.78)
	$N = 71{,}695$	$N = 7{,}679$	$N = 64{,}015$
Number of medical provider events	−0.004	−0.031	−0.0005
	(0.002)	(0.007)	(0.002)
	$\overline{Y} = 26.06$	$\overline{Y} = 30.89$	$\overline{Y} = 25.48$
	(30.28)	(36.96)	(29.33)
	$N = 71{,}707$	$N = 7{,}679$	$N = 64{,}027$
Number of outpatient hospital events	−0.004	−0.013	−0.002
	(0.003)	(0.010)	(0.003)
	$\overline{Y} = 4.43$	$\overline{Y} = 5.62$	$\overline{Y} = 4.29$
	(8.80)	(11.50)	(8.40)
	$N = 71{,}707$	$N = 7{,}679$	$N = 64{,}027$
Number of prescribed medicine events	−0.002	−0.002	−0.001
	(0.001)	(0.003)	(0.001)
	$\overline{Y} = 33.68$	$\overline{Y} = 54.49$	$\overline{Y} = 31.18$
	(31.95)	(46.74)	(28.69)
	$N = 71{,}707$	$N = 7{,}679$	$N = 64{,}027$

Notes: All models include controls for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, smoking participation, BMI, number of chronic conditions, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Robust standard errors clustered by state are reported in parentheses.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

less generous states and no statistically significant change in spending for medical provider events in the more generous states. Further, the differential spending reductions become smaller in magnitude as we examine results for cutoffs going up the distribution of state generosity (i.e., as the "less generous states" include states with higher MMRs). Together, these patterns in the results for medical spending events provide some evidence in support of Prediction 6—that negative spillovers will be larger in states where Medicaid reimburses at a less generous level relative to Medicare.

B. *Effects on Health*

We next turn to the question of whether Medicaid expansions to working-age adults impact the health of seniors in Medicare. Given the pattern of results thus far, we should be most concerned about potential adverse effects on duals. We present results from linear probability models for several measures of poor health in

Table 4—Effects of Eligibility Expansions on Duals' Healthcare Spending by Medicaid Payment Generosity

	All payer healthcare spending measures, 2009$ ($Marginal\ effect\ of\ SimElig(t-1)$)				
	Total payments (1)	Inpatient events (2)	Medical provider events (3)	Outpatient hospital events (4)	Prescribed medicine events (5)
States with generosity below twenty-fifth percentile	−422.9 (268.4)	−187.3 (134.8)	−190.8 (65.0)	−96.0 (29.3)	41.3 (23.3)
States with generosity at or above twenty-fifth percentile	−392.9 (134.5)	−277.7 (75.1)	−28.1 (41.0)	−28.0 (17.5)	−8.3 (9.5)
States with generosity below fiftieth percentile	−659.8 (122.3)	−328.7 (95.4)	−117.8 (56.6)	−67.2 (26.8)	−25.6 (10.3)
States with generosity at or above fiftieth percentile	−256.8 (212.2)	−265.4 (101.1)	17.3 (63.9)	−43.8 (19.2)	−5.7 (17.3)
States with generosity below seventy-fifth percentile	−491.6 (173.8)	−224.0 (115.3)	−79.4 (44.9)	−45.1 (25.4)	−15.1 (11.2)
States with generosity at or above seventy-fifth percentile	−469.4 (180.7)	−344.4 (114.3)	−15.3 (77.3)	−36.3 (28.8)	−0.94 (22.2)

Notes: Sample sizes range from 966 to 6,146. State generosity is measured by the distribution of the Medicaid/Medicare fee ratio for primary care in the year 2003 (Zuckerman et al. 2003). All models include controls for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, smoking participation, BMI, number of chronic conditions, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Controls for the hospital wage index and three physician practice costs indices are added to models of all service spending; the hospital wage index is added to models of inpatient and outpatient hospital spending and physician practice cost indices are added to models of medical provider spending. Robust standard errors clustered by state are reported in parentheses below estimated marginal effects.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

Table 5, and find no evidence of adverse health effects. In the full sample, increases in simulated Medicaid eligibility are associated with a significant *decrease* in the likelihood that the respondent says health limits his/her social activities.[28] Even among the dual eligible enrollees whose healthcare use is reduced by eligibility expansions, the coefficients of simulated Medicaid eligibility changes are never statistically significant.

We further investigate the potential for adverse health effects with additional measures from the MCBS—mortality, preventive care use, and potentially avoidable hospitalizations. For this additional analysis, we focus on the dual eligibles since their healthcare spending and utilization is most affected by Medicaid expansions.[29] We first look at one-year mortality, which can be defined for some Medicare beneficiaries in each survey year using the rotating panel design of the survey.

[28] It is possible that part of the health improvements observed in this model could be due to increases in insurance coverage that occur when Medicaid expansions increase Medicaid participation by adults *prior* to their Medicare enrollment.

[29] For completeness, we also estimated the mortality, preventive care, and avoidable hospitalization models for non-duals (although since an avoidable hospitalization is a claims-based measure, it can be defined only for non-duals in FFS Medicare). The results are available upon request. We find little evidence that Medicaid eligibility expansions have adverse effects on these alternate measures of health among non-duals.

Table 5—Effects of Eligibility Expansions on Self-Reported Health Status by Type of Insurance

	OLS coefficient of *SimElig* $(t-1)$		
	Full sample (1)	Dual eligibles (2)	Non-duals (3)
General health is fair or poor	−0.0001	−0.0005	−0.00005
	(0.0005)	(0.0014)	(0.0006)
	$\bar{Y} = 0.209$	$\bar{Y} = 0.405$	$\bar{Y} = 0.185$
	$N = 71{,}435$	$N = 7{,}640$	$N = 63{,}795$
Health limits social activities	−0.0007	−0.0020	−0.0005
	(0.0004)	(0.0023)	(0.0005)
	$\bar{Y} = 0.123$	$\bar{Y} = 0.238$	$\bar{Y} = 0.109$
	$N = 71{,}553$	$N = 7{,}646$	$N = 63{,}907$
Health is worse compared to one year ago	−0.0001	0.0010	−0.0002
	(0.0006)	(0.0024)	(0.0007)
	$\bar{Y} = 0.214$	$\bar{Y} = 0.305$	$\bar{Y} = 0.204$
	$N = 71{,}581$	$N = 7{,}661$	$N = 63{,}920$

Notes: All models include controls for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, smoking participation, BMI, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Robust standard errors clustered by state are reported in parentheses.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

Table 6 column 1 presents the results of a linear probability model of mortality for the roughly 4,400 dual eligibles (57 percent) for whom we are able to measure it.[30] About 7.2 percent of these duals die in the year after the survey year (compared to 4.5 percent of the full sample respondents for whom we can observe mortality). We find no evidence that increases in Medicaid eligibility to working-age adults increase mortality among duals. In columns 3 and 5, we include additional lags of the simulated eligibility variable to assess longer term effects of eligibility changes on health. We discuss these results in more detail below; overall we do not find strong evidence of mortality effects in any of the models.

We next look more closely at specific types of healthcare that may be linked to health in ways not reflected by self-reported health assessments. Online Appendix Table A3 reports models of whether duals receive preventive healthcare, such as cancer screenings, blood pressure tests, and flu shots. These indicators are defined from self-reported responses to questions on the survey portion of the MCBS. The results provide no evidence that Medicaid expansions alter dual eligibles' utilization of preventive care.

[30] We examine mortality in year $t+1$, two years following expansions that occur in year $t-1$. We only observe mortality for individuals who are not rotating out of the MCBS.

TABLE 6—EFFECTS OF ELIGIBILITY EXPANSIONS ON MORTALITY ($t+1$) FOR DUALS

	Coefficient from LPM of mortality in year $t+1$				
	(1)	(2)	(3)	(4)	(5)
SimElig ($t-1$)	−0.0001	−0.0014	−0.0010	−0.0024	−0.0019
	(0.0023)	(0.0027)	(0.0029)	(0.0026)	(0.0026)
SimElig ($t-2$)			−0.0019		−0.0012
			(0.0012)		(0.0012)
SimElig ($t-3$)					0.0017
					(0.0009)
	$\overline{Y}=0.072$	$\overline{Y}=0.072$	$\overline{Y}=0.072$	$\overline{Y}=0.072$	$\overline{Y}=0.072$
	(0.258)	(0.258)	(0.258)	(0.258)	(0.258)
Observations	4,397	3,877	3,877	3,346	3,346

Notes: All models control for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, enrollment in FFS Medicare, smoking participation, BMI, the number of chronic conditions, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Column 1 uses the relevant sample for 2001–2009 (years for which available data allow consideration of a one-year lag of *SimElig*); columns 2–3 use the relevant sample for 2002–2009 (years for which available data allow consideration of a two-year lag); columns 4–5 use the relevant sample for 2003–2009 (years for which available data allow consideration of a three-year lag). Robust standard errors clustered by state are reported in parentheses.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

Finally, we look at potentially avoidable hospitalizations.[31] These include hospitalizations for dehydration, hypertension, or diabetes complications, among other conditions, which if appropriately treated in an outpatient setting are unlikely to require an inpatient admission. We use claims data to define an indicator for any preventable hospitalization and indicators for five specific types of hospitalizations that we can model for beneficiaries at risk for that condition. For example, we examine hospitalizations for diabetes-related complications among respondents with a prior diabetes diagnosis. Since claims data are required to identify these hospitalizations, these models are estimated only for dual eligibles in FFS plans. Online Appendix Table A4 reports the results. Overall, there is no evidence that increases in Medicaid eligibility increase the likelihood that a dual eligible has an avoidable hospitalization. In contrast, we find that increases in Medicaid eligibility have a significant negative effect on hospitalizations for COPD or asthma, as well as marginally significant negative effects on hospitalization for diabetes-related complications among diabetics and on hospitalization for angina among respondents at risk for angina (i.e., coronary heart disease, high blood pressure, diabetes, and past myocardial infarction).

[31] The 15 types of preventable admissions we can identify in the MCBS claims are: diabetes short-term complications; perforated appendix; diabetes long-term complications; chronic obstructive pulmonary disease (COPD) or asthma in older adults; hypertension; heart failure; dehydration; bacterial pneumonia; urinary tract infection; angina without procedure; uncontrolled diabetes; asthma in younger adults; and lower-extremity amputation among patients with diabetes. We follow the definitions of Prevention Quality Indicators by the Agency for Healthcare Research and Quality in constructing these indicators.

Table 7—Short- and Long-Term Effects of Eligibility Expansions on Spending and Self-Reported Health Status for Duals

	All payer healthcare spending measures, 2009$					General health measures		
	Total payments (1)	Inpatient events (2)	Medical provider events (3)	Outpatient hospital events (4)	Prescribed medicine events (5)	General health is fair or poor (6)	Health limits social activities (7)	Health is worse than one year ago (8)
SimElig ($t-1$), original sample	−476.602	−229.681	−94.239	−52.098	−2.170	−0.0005	−0.0020	0.0010
	(82.82)	(57.94)	(35.57)	(12.93)	(9.80)	(0.0014)	(0.0023)	(0.0024)
Models with two lags								
SimElig ($t-1$)	−384.185	−195.536	−71.848	−32.191	3.756	0.0019	0.0003	0.0031
	(81.60)	(63.77)	(42.11)	(14.82)	(6.81)	(0.0018)	(0.0017)	(0.0025)
SimElig ($t-2$)	−160.388	−69.757	−41.897	−52.863	−7.981	0.0008	−0.0023	0.0002
	(106.71)	(42.10)	(35.85)	(13.74)	(13.58)	(0.0028)	(0.0023)	(0.0025)
Models with three lags								
SimElig ($t-1$)	−449.889	−261.482	−66.981	−31.233	2.435	0.0028	−0.00008	0.0013
	(96.98)	(90.28)	(55.00)	(24.04)	(9.85)	(0.0016)	(0.0016)	(0.0026)
SimElig ($t-2$)	−115.342	−97.255	−33.877	−20.869	−9.858	0.0042	0.0004	−0.0004
	(115.65)	(51.04)	(37.71)	(18.89)	(13.65)	(0.0028)	(0.0026)	(0.0029)
SimElig ($t-3$)	−183.020	−126.356	−40.555	−35.490	15.363	−0.0028	−0.0018	0.0013
	(95.36)	(78.93)	(22.58)	(23.88)	(13.47)	(0.0019)	(0.0021)	(0.0020)

Notes: Sample sizes range from 7,170 to 7,679 for the original sample; 6,444 to 6,847 for the two-year lag sample; and 5,561 to 6,014 for the three-year lag sample. All models control for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, enrollment in FFS Medicare, smoking participation, BMI, the number of chronic conditions (not included in columns 6–8), the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Controls for the hospital wage index and three physician practice costs indices are added to models of all service spending; the hospital wage index is added to models of inpatient and outpatient hospital spending and physician practice cost indices are added to models of medical provider spending. Row 1 uses the relevant sample for 2001–2009 (years for which available data allow consideration of a one-year lag of *SimElig*); rows 2 and 4 for 2002–2009 (allowing consideration of a two-year lag); rows 3 and 5 for 2003–2009 (allowing consideration of a three-year lag).

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

C. *Longer Term Effects on Spending and Health*

We next consider whether the spending effects we observe are short-term transitory effects or persistent effects. In Table 7, we report the results of two-part models of spending in which we add lagged values of the key explanatory variable. In the first row of the top panel, we report the main results shown earlier, for ease of comparison.[32] The results from allowing Medicaid eligibility to have longer lagged effects are reported in the remainder of the table. Models with two lags show that Medicaid expansions continue to reduce inpatient and outpatient hospital healthcare in the second year following the expansion. Models with three lags show that even in the third year after the expansion, there is still some reduction in dually enrolled seniors' healthcare spending, albeit to a lesser extent. Overall, the results

[32] We also ran these models with the somewhat smaller samples that result when we allow Medicaid expansions to enter with additional lags, so as to correspond with the samples used in the bottom two panels of the table. We found similar effect sizes when we drop 2001 data and 2001–2002 data from the samples.

from Table 7 suggest that Medicaid expansions have more pronounced short-run effect on duals' spending, but that reductions can persist for several years.

We next examine the long-term effects of Medicaid expansions on health. Although results presented earlier showed no immediate adverse health effects, we are interested in whether expansions have health effects that manifest over time. The first row of Table 7, columns 6–8, reproduces our earlier results from models of self-reported health, for ease of comparison. The panels below report models with additional lags of simulated eligibility; the results provide no evidence that increases in Medicaid expansion have stronger negative health effects over time.[33] Finally, we look at whether there are mortality effects when we allow for longer lags of the Medicaid expansions variable. These models are reported in columns 3 and 5 of Table 6. In column 5, we find that increases in simulated eligibility have a positive and marginally significant effect on duals' mortality within three years. Specifically, a 1 percentage point increase in simulated eligibility increases the probability of dying in 3 years by 0.17 percentage points, or 2.4 percent. Though statistically significant, this is a small effect; in addition, it is not robust to the inclusion of additional lags of simulated eligibility (e.g., over 4 or 5 periods; results available upon request).

D. *Sample Selection*, *Care Coordination*, *or Preexisting Trends?*

Thus far we have documented reductions in healthcare spending and utilization among dual eligible enrollees that are consistent with negative spillovers from Medicaid to Medicare. We next explore potential threats to our identification strategy.[34] First, we examine whether the findings could be explained by sample selection. We consider two types of selection into the sample of dual eligibles. One type of selection could occur through mortality. For example, if increased mortality among the dual population is tied to Medicaid expansions, reductions in healthcare spending could reflect an improvement in the average health of those duals who remain in our sample, as opposed to reduced access to care. We test for this type of selection into the dual eligible sample in Table 8, column 1. The dependent variable is equal to one if the respondent died in year t, thus making the individual ineligible for inclusion in analysis of our main results on healthcare spending, utilization, and health; note that this model differs from the Table 6 models, where the dependent variable is equal to one if the respondent died in year $t + 1$. We find that the coefficient on the Medicaid expansion variable is very small and insignificant, and thus we rule out this type of selection.

[33] When we estimate our original model with restricted samples used for the models with additional lags, there is one health measure (general health is fair or poor) for which restricting the sample to 2003 to 2009 results in a positive and significant effect of Medicaid expansions (result not shown), but the bulk of the evidence suggests no self-reported health effects.

[34] In addition to the explanations we examine here, another possibility is that eligibility changes occur at the same time as other policy changes related to Medicaid programs. We look at the relationship between changes over time in simulated eligibility and changes in Medicaid physician payment and managed care enrollment during years for which these data are available, and find no evidence that changes in eligibility are correlated with changes in these policy variables (results available upon request).

Table 8—Tests for Selection: Effects of Eligibility Expansions on Mortality (*year t*) and Medicaid Receipt of Senior Medicare Beneficiaries

	Mortality in year t	Medicaid receipt in year t			
	Sample of dual eligibles	Sample of individuals in households with income <100 percent FPL			
	(1)	(2)	(3)	(4)	(5)
SimElig $(t-1)$	−0.0003	0.0019	0.0017	0.0029	0.0020
	(0.0003)	(0.0013)	(0.0013)	(0.0012)	(0.0016)
SimElig $(t-1)$ × no chronic conditions		—	0.0013	—	—
			(0.0032)		
SimElig $(t-1)$ × no ADLs		—	—	−0.0018	—
				(0.0015)	
SimElig $(t-1)$ × no IADLs		—	—	—	−0.0002
					(0.0014)

Notes: The mean of the mortality dependent variable is 0.0057; there are 7,684 observations in the mortality model. The mean of the Medicaid receipt dependent variable is 0.48; there are 9,190 observations in the Medicaid receipt models. All models include controls for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, smoking participation, BMI, number of chronic conditions, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate; the Medicaid receipt models also include an indicator for incontinence, controls for the numbers of limitations in activities of daily living and independent activities of daily living. All models also include year and state fixed effects as well as state-specific linear time trends. Robust standard errors clustered by state are reported in parentheses.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

Another type of selection that may explain our results is the so-called "welcome mat effect," whereby persons who were previously eligible for Medicaid enroll when a state expands Medicaid coverage to another population. The welcome mat effect could explain our results if the duals who newly enroll in Medicaid are healthier, on average; as a result, the changing composition of the dual population drives down average spending. We also test for this type of selection in Table 8. In columns 2 through 5, we estimate models of Medicaid participation among a sample of MCBS respondents who are likely to be eligible for full Medicaid coverage—those whose household income is below 100 percent of the federal poverty level defined for their household type, age, and year in the sample. We follow Pezzin and Kasper (2002) and Ungaro and Federman (2009) in the construction of the sample and the inclusion of specific controls. We first examine the effect of the simulated Medicaid eligibility measure; in column 2, its estimated coefficient is positive but statistically insignificant ($p = 0.15$). In the remaining columns, we test whether Medicaid expansions have welcome mat effects for those in good health. We interact the simulated eligibility measure with different indicators for good health: reporting no chronic conditions, reporting no difficulties with activities of daily living (ADLs), and reporting no difficulties with instrumental activities of daily living (IADLs). In each case, the interaction term is insignificant, suggesting that healthier low-income seniors are no more likely to take-up Medicaid during state Medicaid expansions to the working-age population.

We next look at whether the reductions in healthcare utilization among duals reflect state efforts to coordinate duals' care coinciding with Medicaid expansions.

Data reported by Gold, Jacobson, and Garfield (2012) show a nationwide increase in the share of duals in Medicare Advantage (MA) plans, including Dual Special Needs Plans or D-SNPs over the 2000s. Specifically, the share of duals in MA plans increased from 10 percent in 2000 to nearly 20 percent in 2008. If MA plans lower spending, and their enrollment is increasing in the same states and years in which working-aged eligibility expansions occur, our results may reflect this shift and not spillover effects. Annual state-level estimates of Medicare Advantage enrollment among duals are not available so we cannot control for this change directly, but we explore this issue indirectly. We exclude duals residing in the states where more than 25 percent of the duals are enrolled in Medicare Advantage in 2010, as reported by Gold, Jacobson, and Garfield (2012). These states are Arizona, Hawaii, Minnesota, New Mexico, Oregon, Tennessee, and Vermont. The results are shown in Table 9, column 1. Outside of these states, we continue to see that increases in Medicaid eligibility for working-age adults reduces duals' healthcare spending. Column 2 shows that results are similar when we also exclude duals in California (where 20.1 percent of duals are enrolled in MA). Thus, these results suggest that rising enrollment in MA plans is not responsible for the decline in average healthcare spending among duals.

Finally, we examine whether the healthcare spending declines we observe could be the result of a preexisting trend, even though all of our models already include controls for state-specific time trends. In Table 10, we conduct a falsification test in which we replace the simulated eligibility measure by a one-period lead and then a two-period lead. Since we have the simulated eligibility measure through 2014, we are able to include the leads without reducing the sample size. The coefficients on the one-period lead variable are usually negative, but they are a small fraction of their size in the main results table (Table 1) and they are never statistically significant. The coefficients on the two-period lead variable are also not statistically significant. Overall, the falsification test results rule out a preexisting trend of declining healthcare spending for duals in states that expanded Medicaid eligibility to working-age adults.

V. Discussion

In this study, we find new evidence that public insurance expansions have negative spillover effects on spending and utilization among the already insured population. Specifically, dual eligibles experience reductions in healthcare spending and utilization when states expand Medicaid eligibility to working-age adults. We show that this occurs through reductions in both inpatient hospital care and outpatient care (both medical provider events and outpatient hospital care). The leading explanation for our results is that they stem from physician responses to changes in fixed practice components in response to an increase in the share of patients covered by more restrictive insurance.[35]

Our work has important policy implications related to Medicaid expansions. First, we see no evidence that the majority of Medicare beneficiaries are made worse off in

[35] Note that the presence of the expected spillover effects and the mechanisms underlying them may differ in other contexts, such as dental care, where mid-level practitioners play a central role in providing care (Buchmueller, Miller, and Vujicic 2014).

Table 9—Effects of Eligibility Expansions for Duals, Excluding States with Large Share of Duals in Medicare Advantage

	Marginal effect of *SimElig* $(t-1)$	
	Including California (1)	Excluding California (2)
Panel A. All payer spending, 2009$		
Total payments, all sources	−517.325	−558.006
	(120.26)	(202.54)
All payments for inpatient events	−251.649	−263.839
	(78.30)	(115.82)
All payments for medical provider events	−104.243	−129.861
	(37.01)	(55.35)
All payments for outpatient hospital events	−51.508	−51.190
	(19.20)	(19.97)
All payments for prescribed medicine events	−9.455	6.848
	(10.96)	(17.17)
Panel B. Healthcare utilization	Poisson coefficient on *SimElig* $(t-1)$	
Number of inpatient events	−0.062	−0.069
	(0.012)	(0.017)
Number of medical providers	−0.028	−0.025
	(0.008)	(0.012)
Number of outpatient hospital events	−0.013	−0.026
	(0.013)	(0.014)
Number of prescribed medicine events	−0.003	−0.004
	(0.003)	(0.005)

Notes: The sample size in column 1 ranges from 6,703 to 7,156. Sample sizes for regressions in column 2 ranges from 5,907 to 6,145. Sample excludes duals residing in states where more than 25 percent of duals are enrolled in MA plans (i.e., Arizona, Hawaii, Minnesota, New Mexico, Oregon, Tennessee, and Vermont. All models include controls for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, smoking participation, BMI, number of chronic conditions, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Controls for the hospital wage index and three physician practice costs indices are added to models of all service spending; the hospital wage index is added to models of inpatient and outpatient hospital spending and physician practice cost indices are added to models of medical provider spending. Robust standard errors clustered by state are reported in parentheses.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

terms of their access to care. Second, even though dual eligibles experience reductions in care, we see little evidence that these spending reductions are negative from a welfare perspective. Duals are no more likely to report poor/fair health when Medicaid expansions occur, there is no change in their receipt of preventive care, and there is little evidence of increases in their mortality rates following Medicaid expansions. Duals in FFS Medicare are less likely to be hospitalized for conditions like COPD, asthma, diabetes, and angina. In light of our results for self-reported health, preventive care, and mortality, we interpret the declines in preventable hospitalizations as improvements in ambulatory care (as Joynt et al. 2013 interpret their findings), and not as delays in hospital admission resulting from possible congestion.

Using microdata from the MCBS yields some results that are similar to Glied (2014), and others that differ. The spending reductions we estimate for the entire

Table 10—Falsification Test: Effect of Lead Simulated Eligibility on Healthcare Spending among Duals

	All payer healthcare spending measures, 2009$				
	Total payments (1)	Inpatient events (2)	Medical provider events (3)	Outpatient hospital events (4)	Prescribed medicine events (5)
SimElig $(t+1)$	−62.163	−49.211	−3.215	33.358	−19.149
	(145.44)	(64.25)	(37.82)	(21.41)	(20.88)
SimElig $(t+2)$	61.237	101.071	−4.402	19.499	−8.338
	(113.02)	(72.29)	(40.70)	(44.45)	(15.70)
Observations	7,170	7,679	7,461	7,679	7,507

Notes: All models control for age and age squared, highest level of educational attainment, sex, race or ethnicity, veteran status, marital status, urban residence, household income and its square, Medicaid coverage, enrollment in FFS Medicare, smoking participation, BMI, the number of chronic conditions, the percent of state Medicaid enrollees in comprehensive MCO plans, and the state unemployment rate. All models also include year and state fixed effects and a full set of state-specific linear time trends. Controls for the hospital wage index and three physician practice costs indices are added to models of all service spending; the hospital wage index is added to models of inpatient and outpatient hospital spending and physician practice cost indices are added to models of medical provider spending. Robust standard errors clustered by state are reported in parentheses.

Source: Authors' calculations based on Medicare Current Beneficiary Survey data

Medicare population are similar in magnitude to Glied (2014), who found that states enacting substantial expansions in parental CHIP eligibility (i.e., a 5 percentage point increase in simulated eligibility or more) experience declines in aggregate per capita Medicare Part B spending of 3.2 percent. Similarly, our estimates suggest that a 5 percentage point increase in Medicaid eligibility leads to a $47 or 5.7 percent reduction in Medicare outpatient spending (and no statistically significant difference in Medicare spending to medical providers). In contrast to that prior study, we examine and find heterogeneous effects by type of beneficiary. Specifically, the reductions in healthcare spending we observe are concentrated among dual eligibles, for whom these reductions are sizeable. A 5 percentage point increase in the share of working-age adults eligible for Medicaid in the state reduces the average duals' Medicare outpatient spending by 18.3 percent (or $212) and payments for medical provider events by 14.6 percent (or $399).

We estimate that the Medicaid expansions we study are comparable in magnitude to expansions under the ACA. Among states with a large expansion over our study period, the mean increase in simulated eligibility is 12.6 percentage points. To illustrate how this compares to ACA expansions, we use our simulated eligibility measure for 2013 to 2014 and calculate the average change in simulated eligibility in ten states that implemented the ACA Medicaid expansion in 2014, but did not previously have a large expansion to non-elderly adults.[36] Among these states, the average change in simulated eligibility is 11.5 percentage points and state-specific increases range from 5 to 15 percentage points.

[36] This group of states includes Arkansas, Colorado, Illinois, Kentucky, Michigan, Nevada, North Dakota, Ohio, Rhode Island, and West Virginia.

The ACA also provides a context for comparing the size of the spillover reductions in Medicare spending to the size of the increases in Medicaid spending resulting from Medicaid expansions. To do this, we calculate the change in simulated eligibility associated with the ACA and draw on estimates of the ACA's impact on Medicaid coverage and Medicaid's impact on spending. Our back-of-the-envelope calculation suggests that Medicaid eligibility expansions increase healthcare spending overall, but the negative spillovers we estimate offset a portion of the increased healthcare spending driven by state-level Medicaid expansions.[37] This finding also suggests that supply-side capacity is not entirely fixed; healthcare suppliers have some capacity to expand their services.

Although our study has a number of strengths, it also has some limitations. Relative to the prior literature on this topic, the strengths include its national focus, our use of microdata on healthcare spending, use, and health to analyze heterogeneous effects by type of Medicare beneficiary and state Medicaid payment generosity. Further, we look very closely at whether changes in the composition of the dual population are driving the results and at alternate explanations like care coordination and preexisting trends. An important weakness is the limited detail on health. We observe mortality, but there are small numbers of deaths in the samples, and we have no detail on specific causes of death. We also rely on somewhat crude measures of self-reported health, as opposed to more detailed instruments like the SF-20 or SF-12. Nonetheless, taken as a whole, our results indicate negative spillovers in terms of spending and utilization, but no deleterious effects on health. This suggests that the spillovers from Medicaid expansions may reduce unnecessary or inefficient care and the observed reductions in spending are unlikely to have negative welfare effects.

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[37] Specifically, we estimate that the national change in simulated eligibility in the year 2014 is between 1.3 and 2.9 percentage points; applying our estimates from Table 1 to 10 million dual eligibles suggests a resulting decrease in Medicare spending of \$6.0–\$14.4 billion. We then compare this to the expected increase in Medicaid spending. We derive this by multiplying estimates of increases in Medicaid enrollees (4.3 million, from the Centers for Medicare and Medicaid Services, to 6 million, from the Congressional Budget Office) by average Medicaid enrollee spending (\$3,247) and the 25 percent increase in utilization after gaining Medicaid coverage estimated in the Oregon Health Insurance Experiment. This suggests that overall healthcare spending directly linked to the 1.3 to 2.9 percentage point increase in simulated Medicaid eligibility due to expansion can be expected to increase by \$17.4–\$24.4 billion. Although this exercise requires many assumptions, it suggests that the overall Medicaid spending increase is larger than the overall Medicare spending decrease.

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