The Impact of Provider Payments on Health Care Utilization of Low-Income Individuals: Evidence from Medicare and Medicaid*

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Abstract

Provider payments are the key determinant of insurance generosity within many health insurance programs covering low-income populations. This paper analyzes the effects of a large, federally-mandated provider payment increase for primary care services provided to low-income elderly and disabled individuals. Drawing upon comprehensive administrative payment and utilization data, we leverage variation across beneficiaries and across providers in the policy-induced payment increase in difference-in-differences and triple differences research designs. The estimates indicate that the provider payment reform led to a 6.3% increase in the targeted services provided to eligible beneficiaries, indicating an implied payment elasticity of 1.2. Further, the provider payment reform decreased the fraction of low-income beneficiaries with no primary care visit in a year by 9%, completely closing the gap relative to higher-income beneficiaries with the same observable characteristics. Heterogeneity analysis indicates that the payment increase led to an expansion of utilization for many subgroups, with somewhat larger effects among beneficiaries who are younger, are white, and live in areas with many primary care providers per capita.

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1 Introduction

A commonly stated policy goal is widespread or universal access to health insurance. However, even in health care systems that have achieved this goal, some beneficiaries still struggle to access the care they need, particularly if they are low-income or otherwise disadvantaged. One possible factor contributing to such disparities is the generosity of provider payments. In many settings, providers receive lower payments for treating low-income individuals compared to the same care provided to others.These differences in provider payments could generate disparities in utilization and health outcomes, even when low-income individuals are shielded from out-of-pocket costs for health care. Thus, a pressing policy question is whether targeted increases in payments to providers for treating low-income populations increase access to care for these individuals and narrow health care disparities.

Among elderly and disabled individuals in the United States (US)—for whom there is universal health insurance coverage through the federal Medicare program—there are pronounced income-related disparities in access to care, utilization, and health outcomes. Low-income Medicare beneficiaries are less likely to have any office visit involving evaluation, disease management, or preventive services in a year relative to other beneficiaries, despite higher rates of chronic illnesses (Haber et al., 2014). In addition, low-income Medicare beneficiaries are more likely to have hospitalizations that are considered preventable with regular outpatient care, and they experience rates of hospitalizations for treatable chronic conditions, like diabetes and asthma, at more than double the rates experienced by their higher-income counterparts (Jiang et al., 2010). These disparities have garnered substantial interest among policymakers, because of both the universal nature of the Medicare program and the high health needs of low-income elderly and disabled individuals. However, it is unclear how public health insurance programs—and the generosity of provider payments within these programs—contribute to these disparities.

In this paper, we investigate the impact of a federally-funded provider payment increase on the health care utilization of low-income elderly and disabled individuals in the US. These individuals are dually eligible for health insurance coverage through both the federal Medicare program (which provides universal coverage to elderly and disabled individuals) and their state's Medicaid program (which provides coverage to low-income individuals). These "dual-eligible" beneficiaries—also referred to as "duals"—account for 15% of all Medicaid beneficiaries and 20% of all Medicare beneficiaries, but because of higher health needs, account for about a third of spending in each program (Medicaid and CHIP Payment and Access Commission, 2021). Despite the importance of the dual population in the two largest US public health insurance programs—Medicare and Medicaid—how coverage from these programs interacts, and how this interaction affects provider payments, has attracted little attention as an explanation for the disparities in utilization and outcomes for this population. Our work begins to fill this important gap.

While Medicare provides health insurance coverage for elderly and disabled individuals in

the US, this coverage is incomplete with large patient cost-sharing obligations. The rules and practices for patient cost-sharing, however, differ between dual-eligible beneficiaries and standard, higher-income Medicare beneficiaries ("nonduals"). Providers are not permitted to charge duals for Medicare's cost-sharing, meaning that coverage is more generous for duals than nonduals from a patient's perspective. At the same time, state Medicaid programs pay these cost-sharing amounts either incompletely or not at all. Thus, providers typically receive substantially lower total payments for services provided to duals relative to the same services provided to nonduals. These differences in payments are codified within these programs, leading to effectively two tiers of coverage for Medicare beneficiaries. Because duals are shielded from patient cost-sharing, provider payment is the key policy-relevant dimension of insurance generosity for this population, and our results shed light on how coverage generosity along this margin impacts utilization and access to care.

As part of the Affordable Care Act, the federal government mandated and funded an increase in Medicaid payments for primary care services (specifically, Evaluation and Management or E&M services) provided by qualifying health care providers (providers specializing in primary care). This payment change applied to all Medicaid beneficiaries, including those dually eligible for Medicare, and raised the potential payment for nearly half of office visits among duals. Using linked Medicare-Medicaid administrative data, we demonstrate that this reform sharply increased Medicaid payments of Medicare's cost-sharing, raising the share of Medicare's cost-sharing paid by Medicaid by 14 percentage points. Consequently, the total payments for E&M services increased by 6.5% when summed across Medicare and Medicaid, with this payment increase isolated to duals and qualifying providers.

Given the sharp and targeted response of total payments to the reform, we next investigate whether increased provider payments lead to increases in the provision of primary care services. While the higher payments were intended to increase the supply of visits for Medicaid beneficiaries, that need not happen in practice; for example, capacity-constrained providers may have benefited from the higher payment rates with little or no increase in the number of visits provided. Using comprehensive administrative panel data from Medicare, our baseline difference-in-differences analysis compares the utilization of targeted services for dual and nondual beneficiaries before and after the implementation of the policy. We consider two measures of utilization: a measure of resources expended on E&M services (Relative Value Units or RVUs), which we refer to as E&M services, and the number of visits with E&M services. We find that the reform increased E&M services by 1.211 annually per beneficiary [95% CI: 1.01 to 1.41], which represents a 6.3% increase relative to the baseline mean among duals. The increase in services is largely attributable to an increase in actual visits (not an increase in the services billed per visit). We find the reform caused a 0.62 increase in E&M visits annually per beneficiary [95% CI: 0.53 to 0.71], which is a 5.4% increase relative to the baseline mean. In addition, we find an 8.7% reduction in the percent of beneficiaries who have no E&M visits in a year. These findings are robust across a

number of different specifications, including models with county by year fixed effects, allowing us to flexibly control for geographic trends or place-based policies that may vary over time. Further, monthly difference-in-differences analysis illustrates that the increase in utilization begins in the precise month that the payment increase goes into effect, providing reassurance that the utilization increase is due to increased provider payments rather than differentially-trending factors between high- and low-income individuals. Scaling these utilization impacts by the change in total payments to providers, our results imply a payment elasticity of 1.2 for both E&M services and visits.

The payment increase helps close a gap in access between low-income and higher-income Medicare beneficiaries. To assess how this payment reform impacted this gap, we measure access to care through whether an individual has any E&M visit in a given year. This measure is commonly used to measure access to care in administrative claims data and is correlated with survey-reported measures such as having a usual source of care and being able to obtain care when needed (Institute of Medicine, 1993; Medicaid and CHIP Payment and Access Commission, 2017). Using data from prior to the payment increase, we find duals are 1.1 percentage points more likely to have no E&M visits in a year than nonduals, conditional on measures of observed health status. Comparing this gap to our estimates, we find that the payment reform closed 82% of the gap in access to care between duals and nonduals. The closure of this gap is important, as even a single E&M visit represents an opportunity to manage ongoing chronic diseases and evaluate any new health issues.

To further investigate the robustness of our findings, we relax the difference-in-differences identification assumption by leveraging differences across providers who qualified and who did not qualify for the payment increase. First, we re-estimate the baseline difference-in-differences specification separately for services provided by qualifying and non-qualifying providers. We see that the increase in services is isolated to care provided by qualifying providers. Second, we estimate a complementary difference-in-differences specification comparing changes in services provided by qualifying and non-qualifying providers separately for duals and nonduals. While services provided by qualifying providers relative to those provided by non-qualifying providers trended similarly for duals and nonduals before the payment increase, these trends sharply diverge upon implementation with estimates indicating a relative increase for duals. Finally, we simultaneously leverage all three differences in exposure to the payment change—over time, across dual status, and across provider qualification—to estimate a triple differences specification. The results of the triple differences specification are very similar to the results from the baseline difference-in-differences specification. The fact that all three sources of variation – over time, across beneficiaries, and across providers – indicate similar effects increases the credibility of our findings. In addition, our finding that the policy did not increase E&M for non-targeted beneficiaries (nonduals) or non-qualifying providers indicates that spillover effects were small or nonexistent.

Beyond estimating the mean effect of this reform, we explore heterogeneity and mechanisms.

To explore heterogeneity, we re-estimate our baseline difference-in-differences specification for subgroups defined by demographic and health characteristics. We find the effects of the reform are nearly universal, affecting many subgroups we analyze. However, there is notable heterogeneity in the impacts of the payment reform by beneficiary age, with substantially larger increases in utilization of younger beneficiaries as compared to those experienced by beneficiaries age 75 and older. Further, the estimates suggest increases in utilization are somewhat larger among beneficiaries who are white and those who live in areas with many primary care providers per capita. Finally, while the effects in levels are similar among those with differing baseline health characteristics, the effects are proportionally larger among those in better baseline health and those with no prior avoidable emergency department visits.

To further explore mechanisms behind this expansion of services, we investigate the impact of the payment reform on the types of services provided and billed inputs for these services. We find that the expansion of visits with E&M is isolated to established patient visits—visits where the provider has seen the patient previously—with no increase in new patient visits. These results indicate that the reform increased utilization for duals through increasing the frequency of visits with existing providers rather than increasing the number of visits to new providers. Using information on provider time associated with common E&M codes, we analyze the effect of the reform on billed provider time spent with patients. We find that billed provider time increases by 19 minutes annually per dual-eligible beneficiary [95% CI: 15.71 to 22.49], which represents an increase of 5.7% relative to the baseline mean of about 330 minutes per year. We also explore whether resources per visit change in response to the payment reform. We find that the reform decreased billed provider time per visit by 0.40 minutes (or 1.5% of the baseline mean) and total resources per visit (RVUs per visit) by 0.02 (or 1.3% of the baseline mean).

Our results have implications for the design of public programs that serve low-income individuals more broadly. Policymakers sometimes choose to reduce program costs through cutting payments to suppliers of these programs. These reductions in payment generosity may imperil the quality of the programs and is one way in which "programs for the poor become poor programs." Our results provide direct evidence that providing unequal payments for identical services rendered to low-income versus high-income individuals leads to under-provision of services to low-income individuals. Indeed, we find that unequal payment to providers is responsible for nearly the entire gap in having any visit between low-income and higher-income Medicare beneficiaries. Further, our findings demonstrate that increasing provider payments for care provided to low-income populations can induce large increases in health care utilization and can work to close gaps in access.

This paper contributes to several strands of literature. First, our study contributes to a recent and growing literature on supplier responses to incentives within the Medicare program and health insurance more broadly, adding to a prior literature which has analyzed the generosity of physician fee schedules in Medicare (Clemens and Gottlieb, 2014) and in Norway (Brekke et al., 2017, 2020), the introduction of Medicare (Finkelstein, 2007), the generosity of capitated payments to private

Medicare insurers (Cabral, Geruso and Mahoney, 2018; Duggan, Starc and Vabson, 2016), and the structure of Medicare hospital and facility payments (Dafny, 2005; Einav, Finkelstein and Mahoney, 2018; Eliason et al., 2018). Relative to physician payment variation studied in prior work on Medicare (Clemens and Gottlieb, 2014), the reform we analyze caused a larger and more targeted change in provider payments and acted to close a disparity in payment. The magnitude of the payment reform we analyze allows us to obtain precise estimates of the effect of provider payments on E&M services and to investigate heterogeneity and mechanisms, which was not possible in existing work. Estimates of the effect of a payment change for E&M services are particularly important because E&M services are crucial for managing patient care and are generated via provider time, one of the few inherently scarce inputs in health care production. We find that the increase in payments induced large increases in utilization that were isolated to services provided to targeted beneficiaries (duals). These findings imply that providers respond to payment policy at a granular level and suggest that the two-tiered reimbursement system for coverage provided to Medicare beneficiaries is an important factor in explaining access gaps across lower-income and higher-income Medicare beneficiaries.

Second, our paper contributes to an emerging literature examining the impact of provider payments for care provided to low-income populations. It has been difficult to identify the role of provider payments among the many potential explanations for the utilization patterns of low-income individuals (such as health needs, care seeking behavior, the convenience and location of medical services, and many others). Recent work analyzing the German health care system uses an audit study approach to illustrate that German physicians provide more and more timely appointments to enrollees in private insurance than those enrolled in lower-paying public insurance, with these effects scaling by the magnitude of the difference in provider payments (Werbeck, Wübker and Ziebarth, 2021). In the US context, recent studies leverage the Affordable Care Act Medicaid payment increase to identify the role of provider payments within the general (non-Medicare eligible) Medicaid population, finding improvements in appointment availability[1] (Polsky et al., 2015), increases in self-reported office visits and health (Alexander and Schnell, 2019), and increases in physician earnings (Gottlieb et al., 2020).

Despite the importance of provider payments as a policy tool that may affect care received by low-income elderly and disabled individuals, we are aware of only two recent studies—both in the medical literature—investigating the effects of provider payments on the care received by duals (Fung et al., 2021; Roberts and Desai, 2021). These prior studies have mixed results that are difficult to interpret for two reasons. First, the papers do not present evidence for the first-stage change in provider payments for the variation they exploit. Second, the papers present limited evidence to assess internal validity. Our study moves this nascent literature forward

[1]Polsky et al. (2015) document increases in appointment availability using an audit study. However, the increased availability in appointments does not appear to be generated by increased extensive-margin provider participation in Medicaid (Decker, 2018; Dass, Fung and Price, 2020).

by using linked administrative data from Medicare and Medicaid to directly observe provider payments,—a critical input given the complex interaction of the two programs. This first-stage analysis identifies plausibly exogenous variation that we use to provide robust causal evidence on the impacts of provider payments on care provided to the high-cost, high-need dual-eligible population. Specifically, our use of these linked administrative panel data enables us to identify eligible beneficiaries, qualifying providers, and targeted services; implement multiple identification strategies; obtain precise estimates of the effect of the reform on both actual payments to providers and beneficiary utilization; illustrate robustness to focusing on high-frequency (monthly) data; and investigate heterogeneity in and mechanisms for the treatment effects. Thus, beyond providing new evidence on the importance of provider payments on the utilization of those dually eligible for Medicare and Medicaid, this paper also provides some of the most comprehensive and transparent evidence to date on the impacts of provider payments on health care utilization among low-income populations more generally.

Third, our findings highlight the importance of provider payments as key determinant of insurance generosity, complementing a large literature investigating the impact of patient cost-sharing in settings such as Medicare (e.g., Cabral and Mahoney (2019); Einav, Finkelstein and Schrimpf (2015)), employer sponsored insurance (e.g., Brot-Goldberg et al. (2017); Kowalski (2016)), and private insurance (e.g., Manning et al. (1987); Newhouse (1993)). While much of the prior work on insurance generosity has focused on the impacts of patient cost-sharing, most low-income populations enrolled in public insurance programs face no patient cost-sharing. This paper works to fill an important gap in the literature by providing evidence on the impact of the primary dimension of insurance generosity relevant for low-income populations: provider payments. Our findings demonstrate that provider payment policy has substantial scope to affect utilization in settings where patients face no cost-sharing, and provider payments may be a promising policy tool to work to close socio-economic gaps in access to care.

Finally, this paper also contributes to the broader literature on health care access and insurance among low-income individuals. A number of correlation studies have pointed to disparities in health care access comparing Medicaid beneficiaries to other insured beneficiaries (e.g., Asplin et al. (2005); Bisgaier and Rhodes (2011); Rhodes et al. (2014); Oostrom, Einav and Finkelstein (2017)). In addition, there is a growing literature estimating the causal effect of having Medicaid coverage on short-term (e.g., Baicker et al. (2013); Finkelstein et al. (2012)) and long-term outcomes (e.g., Brown, Kowalski and Lurie (2019); Wherry et al. (2018)). While this prior work has focused on the impacts of Medicaid on the extensive margin (having Medicaid relative to not having Medicaid), we expand upon this literature by quantifying the effects of a reform which increased the generosity of Medicaid coverage on the intensive margin. We find that increasing provider payment generosity has a large impact on beneficiary utilization, and these effects are substantial—with increases large enough to nearly close the cross-sectional observed gap in having any E&M visit in a year between low- and high-income beneficiaries in this setting.

2 Background and Data

2.1 Dual Eligibility for Medicare and Medicaid

Around 20% of Medicare beneficiaries simultaneously qualify for coverage from Medicaid due to income and assets below specified thresholds. The federal Medicare program pays the same for services provided to any Medicare beneficiary, regardless of dual eligibility for Medicaid. While nondual Medicare beneficiaries are responsible for significant cost-sharing (e.g., an annual deductible and 20% coinsurance for physician services), duals are shielded from cost-sharing obligations. Providers are prohibited from billing duals for the cost-sharing. In some cases Medicaid is liable for cost-sharing payments, but state Medicaid programs do not fully reimburse providers for Medicare's cost-sharing for many services, leaving providers with substantially lower total payments for services provided to duals relative to the same services provided to nondual Medicare beneficiaries.

This payment differential for care supplied to dual and nondual beneficiaries has been documented in prior work. For instance, a report prepared for the Medicaid and CHIP Payment and Access Commission (MACPAC) searched for a Medicaid "cost-sharing claim" — a claim to the relevant state Medicaid program to pay for the cost-sharing portion of a Medicare claim — to match to each Medicare claim for duals in 20 states in 2009 (Haber et al., 2014). They found that approximately 44% of Medicare's cost-sharing for E&M claims was paid by state Medicaid programs on average.

There are multiple explanations for why we observe Medicaid programs covering only a fraction of Medicare's cost-sharing for duals. First, state policy in most states dictates that the Medicaid program is not obligated to pay this cost-sharing in many cases. Historically, state Medicaid programs were responsible for the Medicare cost-sharing for duals. However, the enactment of the Balanced Budget Act of 1997 allowed states to limit their responsibility for Medicare's cost-sharing. Over the subsequent years, almost all states have enacted "limited reimbursement" policies rolling back Medicaid's responsibility to reimburse providers for Medicare's cost-sharing (Mitchell and Haber, 2004; Haber et al., 2014). The policies state that Medicaid does not pay cost-sharing for duals if the amount the provider has received from Medicare exceeds the state's Medicaid payment rate for that service. By 2012, the amount that Medicare paid providers for most primary care services exceeded average Medicaid payment rates for the same services by 38%, meaning states adopting these policies were released from significant responsibility for covering Medicare's cost-sharing.[2]

Second, cost-sharing is not always paid out by Medicaid in practice, even when state Medicaid policies allow providers to claim reimbursement for Medicare's cost-sharing (Mitchell and Haber,

[2]Zuckerman and Goin (2012) report that Medicaid payments for primary care averaged only 58% of the total (Medicare-only + cost-sharing) payments in Medicare in 2012. Thus, the ratio of the Medicare-only portion for services after the deductible (80%) to the Medicaid rate is given by 80/58=1.38.

2004; Haber et al., 2014). Incomplete payment of eligible claims means there is limited correlation between state Medicaid statutory rates for Medicare cost-sharing claims and the average fraction of cost-sharing paid, and is consistent with prior studies documenting high administrative burden on physicians seeking payment through Medicaid and frequent claim denials (Kaiser Family Foundation (2011), Gottlieb, Shapiro and Dunn (2018), Dunn et al. (2020)).[3] The value to the provider of payment for cost-sharing amounts should be understood of as net of the hassle costs imposed by this high administrative burden.

Finally, Medicaid reimbursements make up only a small fraction of the total cost-sharing obligations for care provided to duals in part due to low submission rates for cost-sharing claims. For instance, our analysis with linked Medicare-Medicaid data reveals that only about a third of cost-sharing claims for E&M services are submitted by providers to state Medicaid programs prior to the payment reform we analyze. This incomplete claims submission may reflect provider responses to a combination of low statutory payment rates, high administrative burden relative to the dollars at stake, frequent claim denials, and incomplete payments of statutory rates.

2.2 Payment Increase for Medicaid Primary Care Services

The Affordable Care Act (ACA), passed in 2010, mandated a federally-funded temporary increase in Medicaid provider payments for select primary care procedures performed by qualifying providers. This increase temporarily closed the gap in statutory payments for services provided to dual and nondual Medicare beneficiaries. The targeted primary care procedures include all Evaluation and Management (E&M) codes, services during which providers spend time and effort evaluating, diagnosing, and managing patient conditions. These services are the most commonly billed types of procedures at office visits, with E&M billed at more than half of all office visits for Medicare beneficiaries. To be eligible for the rate increase, a health care provider must either: (i) have a board-certified specialty or sub-specialty within general internal medicine, family medicine, or pediatric medicine, or (ii) attest that 60% of his/her prior year's Medicaid claims were for billing codes targeted by the legislation.[4] The ACA payment increase affected provider reimbursement rates for care provided in 2013 and 2014.[5]

[3]Using the Medicaid claims for the years prior to the payment change, we replicate the low correlation between states' statutory rates for cost-sharing payment and the average fraction of cost-sharing paid. In addition, we find that the payment change did not lead to full payment of Medicaid cost-sharing. Because of this, there is no *a priori* reason to expect that the effect of the payment change would vary with a state's pre-period statutory rate. This finding helps rationalize the results reported in Roberts and Desai (2021) and Fung et al. (2021), who find no evidence of differential increases in E&M over this time period in states with lower baseline statutory payments.

[4]States were required to review a sample of physicians who received the payment bump to retrospectively verify eligibility (Medicaid and CHIP Payment and Access Commission, 2015).

[5]While the ACA payment increase and the associated federal funding expired in December 2014, some states changed their state Medicaid fee schedules after this expiration to increase payments for specified services and physicians above baseline 2012 levels (Timbie et al., 2017). Because comparable Medicaid claims data does not extend beyond 2014, we cannot identify whether and how state Medicaid payments for Medicare cost-sharing claims were affected by state policy decisions after the ACA payment increase expired. For this reason, we limit

The ACA increased Medicaid payments for E&M services provided to all Medicaid beneficiaries, including Medicaid beneficiaries dually eligible for Medicare. Increased payments for E&M provided to duals occurred through the interaction of these programs. Specifically, the ACA Medicaid payment increase may have increased provider payments for care provided to duals through at least three mechanisms. First, the ACA payment policy closed the loophole that states with limited reimbursement policies previously leveraged to avoid paying providers for Medicare's cost sharing. This effectively increased statutory rates owed to providers for cost-sharing claims for care provided to duals. Second, the federal funding behind the ACA payment increase may have increased actual payments to providers for any given statutory payment rate. That is, the expanded federal funding may have improved states' compliance with statutory rates for targeted services (through, for example, reduced claim denial rates, reduced administrative hassles, etc). Third, the ACA payment policy may also have indirectly affected participation in the Medicaid program by providers by increasing awareness of state and federal payment policies, which may have encouraged the submission of claims by providers. While our identification strategy will not allow us to distinguish between these mechanisms behind increased physician payments, we use matched Medicare and Medicaid claims data to directly investigate the change in payments to providers. Further, we provide suggestive evidence on the importance of each of these possible mechanisms to interpret the estimated effects. Section 3 provides more detail on this analysis.

Our difference-in-differences analysis uncovers the response of providers to the specific policy we study—the ACA Medicaid payment reform. We note that this reform occurred in the wake of other major changes brought about by the ACA, when salience to policy changes may have been heightened. While the causal impact of the same policy could differ if implemented in a different period, the reform we analyze is relatively recent and many debates occurring during the beginning of the ACA era continue today. As a result, the impact of the provider payment reform we analyze holds continuing relevance for ongoing discussions about payment generosity for care provided to duals and payment generosity in Medicaid and public health insurance programs more broadly.

Before describing the data, we briefly summarize this background and how it influences our approach. Medicaid's payments for Medicare's cost-sharing were incomplete at baseline. The ACA payment change increased Medicaid's payments for Medicare's cost-sharing for eligible services provided by qualifying providers in 2013 and 2014 through a number of channels. Given there are likely multiple mechanisms through which the policy impacted payments, we begin by providing direct evidence on the first-stage of the policy on provider payments leveraging linked Medicaid and Medicare data. We then focus on estimating the reduced-form effect of the ACA payment policy on utilization outcomes for duals. To obtain elasticities, we scale the utilization estimates by estimates of the change in payments using several different measures of payments that may be

our analysis to before and during the implementation of the ACA payment increase, the period in which we can measure the change in payments using our linked Medicare and Medicaid administrative data.

relevant given the complexity of payments in this setting.

2.3 Data

This paper leverages several administrative datasets obtained from the Centers for Medicare and Medicaid Services (CMS). Specifically, we use Medicare health care utilization data for a 20% random sample of Medicare beneficiaries for the period 2010-2014 from the Master Beneficiary Summary File and medical claims files—the Carrier, MedPAR, and Outpatient files. Collectively, these files provide comprehensive administrative panel data on patient demographics and utilization of inpatient and outpatient medical care for Medicare beneficiaries enrolled in traditional (fee-for-service) Medicare.

In addition, we use Medicaid claims data to investigate the impact of the policy on Medicaid's payment of Medicare's cost-sharing to providers. The Medicaid Analytic Extract (MAX) datasets cover the years 2011-2013; however, over this time period states gradually joined another Medicaid claims reporting system. We limit ourselves to 13 states reporting the payment of duals' cost-sharing amounts consistently over the years 2011-2013: Arizona, Connecticut, Iowa, Massachusetts, Michigan, Minnesota, Mississippi, New York, Oregon, Pennsylvania, Vermont, Washington, and West Virginia.[6] Together, these 13 states represent 31% of duals in our sample. In the years 2011-2013, the MAX dataset reports the same beneficiary identifier as Medicare, allowing us to merge these data sources. Claims for E&M services are assembled in the MAX Other Therapies file.[7]

Outcomes, Covariates, and Subsamples To examine how the policy affected payments (our "first stage"), we follow the methods reported by MACPAC to measure duals' cost-sharing claims in the Medicaid data (Haber et al., 2014). Specifically, for each Medicare E&M claim for a dual residing in a Medicaid-reporting state, we look for a Medicaid claim for the same service-date-beneficiary, where services are identified using Healthcare Common Procedure Coding System (HCPCS) codes. Using our claim-level match, we examine three outcomes. First, we measure

[6]There are 26 states reporting claims in the Medicaid MAX data in 2013. We exclude six states (AR, IN, OH, OK, UT, and WY) because they reported in their regulatory documents ("State Plan Amendments") that they implemented the payment increases as lump-sum periodic payments to physicians, rather than an increase in the payment rate per claim. Such lump-sum payments are not reflected in the Medicaid MAX data as payments for the cost-sharing claims. We exclude two states (MO and TN) because CMS's Data Validation Reports suggest that the service codes (a merge variable) are frequently missing. We exclude four states (HI, ID, NJ, and SD) because of extreme changes in the per-capita E&M utilization within the time period, suggesting changes in reporting. California had an administrative issue related to cost-sharing claims that caused them not to be paid during 2013 or 2014 (Schuhmeier, 2018).

[7]States vary in how they report utilization for Medicaid beneficiaries enrolled in managed care organizations for Medicaid medical benefits. In our 13 states, ten report all or nearly all duals are in fee-for-service Medicaid over the sample period. In three others (AZ, MN, and OR), significant shares of duals are enrolled in some form of Medicaid managed care, but the share of E&M claims among fee-for-service Medicare duals that are submitted to Medicaid is similar between enrollees in Medicaid managed care and Medicaid fee-for-service, suggesting that managed care enrollment did not affect reporting of claims.

the share of Medicare E&M claims with a matching Medicaid claim; we refer to these claims as "submitted".[8] Second, we calculate the share of the claim's cost-sharing (according to the Medicare claim) that is paid by Medicaid via a matched Medicaid claim, where this is zero for Medicare claims with no matching submitted Medicaid claim. Finally, we calculate the share of cost-sharing paid conditional on submission, i.e., the share of the cost-sharing paid among Medicare claims with a matching submitted Medicaid claim. Note that these measures allow us to capture both direct effects of the policy (i.e. submitted claims receive higher payments) and indirect effects that might be driven by reduced hassle costs on some dimensions (e.g., claims are denied less frequently).

To examine how the policy affected utilization, we measure E&M services targeted by the payment reform, which we identify using the HCPCS codes in the Medicare Carrier (Part B) claims. We define each claim with an E&M service as an "E&M visit" to capture the number of encounters where patients receive targeted services.[9] In order to measure the quantity of E&M services, we convert each service to its work-related Relative Value Units (RVUs) using Medicare's conversion rates (Centers for Medicare and Medicaid Services, 2016). E&M visits with longer duration and/or greater complexity are assigned higher RVUs, so this measure captures any changes in the intensity of targeted services induced by the payment reform. Our core outcomes are the quantity of E&M services (as measured by work RVUs), the number of E&M visits, and an indicator for having any E&M visit in a given year.

The payment increase was limited to "primary care" providers, defined broadly. Physicians qualify for increased payments based on their specialty (general practitioners as well as internal medicine subspecialists such as cardiologists). Providers also qualify if they self-attest that 60% of their Medicaid claims in the prior year were for E&M services. Mid-level providers such as nurse practitioners qualify if practicing under the supervision of a qualifying physician. We implement this definition of qualifying providers as closely as possible. We follow the CMS guidance in coding providers as qualifying through specialty. Because Medicaid claims are only available for a subset of states, we proxy for qualifying through the claims-based threshold using information on the fraction of a physician's *Medicare* claims billed for E&M services. We classify mid-level providers as qualifying if they work within a tax-unit with a qualifying physician. While our main difference-in-differences analysis relies on variation over time and across beneficiaries, some of our robustness analysis exploits variation across qualifying and non-qualifying providers. To the extent that we have mis-classified some providers' qualifying status, we expect that robustness analysis exploiting across-provider variation will result in estimates that understate the impact of the ACA payment reform on the provision of E&M services by qualifying providers.

[8]It is unclear whether we observe claims that are denied at an early part of the process. States vary in the share of claims with a zero payout. Overall, 4% of the matched claims are paid out at zero by Medicaid.

[9]Most E&M visits (73%) occur in a physician's office but some represent services rendered during a hospital stay, ED visit, or other encounter.

One-fourth of providers who bill services to Medicare in the years 2010-2014 are in a qualifying specialty. An additional 12% of providers are not in a qualifying specialty but met the 60% claims threshold in Medicare in at least one year in the sample period. Another 15% are mid-level providers who practiced in a tax-unit with a qualifying physician. Overall, these three categories imply that 52% of Medicare providers qualify for the increased payments; these qualifying providers supply 85% of E&M services in Medicare. In order to receive higher payments, qualifying providers were required to demonstrate their qualifying status to the relevant state Medicaid program. Note we are unable to ascertain whether a qualifying provider has actually demonstrated her qualifications to the relevant state Medicaid programs, and thus our qualifying designation is likely an upper bound on registered administrative qualification.

To determine mechanisms underlying utilization changes induced by the payment increase, we examine patterns in new vs. established patient visits and the time duration and service intensity of visits. A patient is defined as a "new patient" of a provider if the provider ever bills the HCPCS code for a "new patient" visit for that patient in the year; all other patients are "established patients" even if the patient visits the provider infrequently. We follow the procedures of Fang and Gong (2017) to determine the provider time associated with each E&M code.[10] To measure the service intensity of visits, we examine the RVUs supplied at each visit.

We explore heterogeneity by patient characteristics such as age, sex, race, baseline health, and prior service use. To explore heterogeneity by baseline health, we measure health status using the Charlson Comorbidity Index, a well-studied and validated summary measure of 19 chronic conditions. The Charlson Index has been shown to predict mortality, disability, hospital readmissions, and hospital length of stay (Charlson et al., 1987; de Groot et al., 2003). We measure the Charlson Index for each patient-year and split individuals on the basis of whether they ever have an Index of two or greater in the pre-period.[11] Using the taxonomy of emergency department (ED) visits developed by Billings, Parikh and Mijanovich (2000), we also flag individuals who have a "preventable" ED visit in the pre-period. The presence of such ED visits is commonly interpreted as a signal of inadequate access to primary care (Ballard et al., 2010; Baicker et al., 2013). Finally, we flag beneficiaries living in counties designated in 2010 as Health Professional Shortage Areas for primary care practitioners by the U.S. Health Resources and Services Administration—counties with relatively few primary care providers per capita. [12]

[10]We construct the time variable by following the methodology in Fang and Gong (2017). Fang and Gong (2017) map procedure CPT codes into either the lower end of the recommended time range provided by Medicare or reported time spent providing services according to a survey conducted by CMS (Zuckerman et al., 2014).

[11]The Charlson Index is not an ideal measure of health because it is only observed for beneficiaries who generate claims listing the included conditions. If care to duals is under-provided in the pre-period, we may be more likely to misclassify this group. As a validation, we verify that outcomes observed in duals and nonduals classified as having different baseline health with the Charlson Index trended similarly in the pre-reform period.

[12]The 2010 HPSA designations for each county are drawn from the 2015-2016 Area Health Resource File. We consider a county to have HPSA designation if the entire county was indicated to be a geographically defined HPSA. For information on the prevalence of HPSAs, see `https://www.kff.org/other/state-indicator/primary-care-health-professional-shortage-areas-hpsas/`.

Descriptive Statistics In our main specification, we consider a balanced sample. Our analysis is limited to Medicare beneficiaries who are enrolled in fee-for-service Medicare hospital (Part A) and physician (Part B) coverage for all twelve months for the years 2010-2014, because medical service utilization is not observed for individuals outside of Medicare or enrolled in Medicare Advantage. We further limit the sample to individuals who are either dual-eligible in every month or not dual-eligible in any month. Among those who are dual-eligible, we limit our attention to duals who are Qualified Medicaid Beneficiaries (QMBs), as these individuals qualify for the standard dual coverage that includes exemptions from cost-sharing requirements as described above.[13] Our core sample reflects a balanced panel of 339,689 duals and 3,199,990 nonduals over the years 2010-2014.

Table 1 presents descriptive statistics for the baseline Medicare sample in Panel A, split by dual-eligibility and time period. Over the years 2010-2012, duals average 11 visits with E&M services each year, while nonduals average two fewer visits. Consistent with aging of our balanced sample, both groups increase the number of visits in the years 2013-2014, although the increase is larger among duals. The next two rows break down the visits into those supplied by qualifying and non-qualifying providers. The increase in E&M visits among duals compared to nonduals is driven by qualifying providers; both duals and nonduals have a similar (slight) increase in E&M visits provided by non-qualifying providers. The next row shows the percent of beneficiaries with any E&M visit and E&M services (as measured using work RVUs), with the same patterns as above. Next, we show that approximately 6% of E&M visits occur between patients and providers where the provider bills a "new patient" code over the year; thus, most E&M for this population is occurring in the context of ongoing patient-provider relationships. Finally, we report the total number of Medicare work RVUs for this population. We note that E&M work RVUS make up about a third of all Medicare work RVUs.

We next provide a description of our sample along the key dimensions of heterogeneity we consider below. Duals are somewhat more likely to be female and somewhat younger than nonduals (dual eligibility is common among individuals entitled to Medicare via participation in Social Security Disability Insurance). They are more likely to be in poor health and have a preventable ED visit in the pre-period and are also more likely to live in a primary care shortage area.

In Panel B, we describe the subset of Medicare E&M claims that we match to our Medicaid

[13]While over our time period more than two-thirds of duals are QMBs, some Medicare beneficiaries dually qualify for Medicaid through other pathways and have different coverage details. For instance, individuals who meet their state's Medicaid eligibility thresholds but are not QMBs are known as "full benefit" duals. Medicaid acts as a "secondary" payer for cost-sharing for those individuals. However, those individuals are subject to the state's typical cost-sharing for Medicaid beneficiaries for the services (CMS Medicare-Medicaid Coordination Office, 2021). In fiscal year 2013, more than 40 states had cost-sharing requirements that could apply to non-QMB duals for physician services (Kaiser Family Foundation, 2013). We exclude "full benefit" duals from our treatment group because we don't have data on applicable cost-sharing for the services we consider. We also exclude other non-QMB duals from our analysis, as they are not eligible for the exemption from Medicare's cost-sharing (and therefore the increase in provider payments we analyze does not apply to care provided to them).

data, namely E&M claims for duals over the years 2011-2013 in the 13 Medicaid-reporting states. The first row demonstrates that these cost-sharing claims have low dollar amounts, averaging between $21 and $31. The total money at stake with cost-sharing claims is roughly a third of the full Medicare payment rate for E&M claims at qualifying providers.[14] Consistent with prior work based on data from 2009 (Haber et al., 2014) and recent work about the administrative burden of Medicaid participation for providers (Gottlieb, Shapiro and Dunn, 2018; Dunn et al., 2020), we find that many cost-sharing claims are not submitted to Medicaid. In the pre-period, 36–40% of E&M claims among duals in the Medicaid-reporting states are actually submitted to Medicaid, with the amount increasing in 2013 among qualifying providers. Only about 18% of the total cost-sharing for E&M claims for duals is paid to providers in the pre-period, which reflects both incomplete submission and the fact that only 43-49% of cost-sharing is actually paid out for submitted claims. However, in 2013, we see increases in overall payment and payment conditional on submission for E&M services provided by qualifying providers. We present more comprehensive evidence on the first-stage effect of the reform on provider payments through regression analysis in Section 3.2.

3 Empirical Strategy

We use a difference-in-differences research design to estimate the effect of the payment increase on the care provided to duals. Our analysis leverages variation across time (before vs. during the payment increase), across beneficiaries (dual vs. nondual beneficiaries), and across providers (qualifying vs. non-qualifying providers). Below, we outline our econometric model. We then present first-stage estimates illustrating the effect of the ACA policy on the payment of cost-sharing for E&M services provided to duals.

3.1 Econometric Model

Let i index beneficiaries and t index year. Our baseline difference-in-differences specification flexibly compares how outcomes evolve for duals relative to nondual beneficiaries upon the implementation of the payment increase. Specifically, we estimate:

$$y_{it} = \sum_{t \neq 2012} \beta_t \times I_t \times Dual_i \; + \alpha_t + \lambda Dual_i + \gamma X_{it} + \epsilon_{it}, \tag{1}$$

where I_t indicates year t and $Dual_i$ indicates beneficiary i is dual-eligible. This specification includes year fixed effects (α_t) and a control for dual status ($Dual_i$). The baseline specification includes other time-varying controls (X_{it}): age (in five-year bins), sex, and county fixed effects.

[14] Recall that cost-sharing for physician services includes both an annual deductible, in which cost-sharing can equal 100% of the total payment, and 20% coinsurance on all claims beyond the deductible.

Robust standard errors are clustered at the county level in all models to adjust for both correlation in the error over time within the same county and correlation in the error within a county in any year. Such within-county correlations may be especially relevant in our setting given the county-level structure of the Medicare Advantage market. However, the results remain essentially unchanged in alternative specifications in which we cluster standard errors at the state level (see Appendix Table A.2).

We normalize β_{2012} to zero. The coefficients of interest are β_t's, which capture the mean difference across dual and nondual beneficiaries in the change in the outcome variable in year t relative to 2012, the year just prior to the policy implementation.

The key identification assumption is that, in the absence of the reform, the outcomes of interest (e.g., E&M services, E&M visits) would have evolved in parallel for dual and nondual beneficiaries differentially exposed to the reform. While we cannot test this assumption directly, we can assess its validity in three ways. First, we plot the year-specific β_t coefficients, which allows the reader to visually examine the evolution of outcomes across dual and nondual beneficiaries prior to the reform. Second, we plot more granular monthly event study estimates, which allow us to verify that the precise timing of the impacts on outcomes lines up with the implementation of the payment increase. Third, we estimate alternative specifications that allow us to relax the identification assumption along different dimensions.

Specifically, we estimate three alternative specifications. First, we re-estimate our baseline difference-in-differences specification in Equation (1) separately for care provided by qualifying and non-qualifying providers. This specification allows us to assess whether the baseline difference-in-differences findings are driven by changes in the care provided by qualifying providers, who directly benefited from the payment policy, rather than non-qualifying providers, who should be largely unaffected in the absence of spillovers. Second, we estimate a complementary alternative difference-in-differences specification that directly leverages differences across providers, comparing care provided by qualifying versus non-qualifying providers:

$$y_{ipt} = \sum_{t \neq 2012} \theta_t \times I_t \times Qualifying_p + \eta_t + \kappa Qualifying_p + \mu X_{it} + e_{ipt}, \tag{2}$$

where p indexes the provider type (either qualifying or non-qualifying) and $Qualifying_p$ indicates care provided by a qualifying provider. We estimate this specification separately by beneficiary dual status. The coefficients of interest in these specifications are the θ_t's, which capture the mean difference across care provided by qualifying and non-qualifying providers in year t relative to 2012. These specifications allow us to assess the robustness of the main findings when employing an alternative identification assumption: the utilization of services would have evolved in parallel for care provided by qualifying and non-qualifying providers if not for the reform. We also estimate Equation (2) for nonduals, which allows us to investigate any potential spillover effects on nondual

patients.

Finally, we leverage all these differences in a triple differences specification:

$$\begin{aligned} y_{ipt} = \quad & \textstyle\sum_{t \neq 2012} \delta_t \times I_t \times Qualifying_p \times Dual_i + \sum_t \pi_t Qualifying_p \times I_t \\ & + \textstyle\sum_t \nu_t Dual_i \times I_t + \phi Qualifying_p \times Dual_i + \tau_t + \xi X_{it} + u_{ipt}. \end{aligned} \tag{3}$$

The δ_t's are the coefficients of interest. The triple differences specification relaxes the identification assumption behind the baseline difference-in-differences analysis, only requiring parallel trends in the difference in care performed by qualifying and non-qualifying providers across dual and nondual beneficiaries in the absence of the reform.

Under the identifying assumptions described above, our parameters of interest provide an estimate of the causal impact of the ACA payment policy on the use of E&M visits and services among duals. However, it is important to note that these estimates capture only the effect of this specific policy and the extent to which they generalize to other settings may depend on the specific context being considered. Nevertheless, we believe this policy is particularly relevant because it occurred relatively recently and because the policy itself continues to be relevant in discussions about E&M reimbursement in the Medicare program.[15] In addition, these results could have implications for other recent changes to E&M reimbursement, such as the incorporation of social risk factors into fee schedules for these types of visits.[16]

3.2 Identifying Variation and First Stage

To motivate our empirical strategy, we first show that the payment reform sharply increased Medicaid's payment of Medicare's cost-sharing for duals. By definition Medicaid does not cover cost-sharing for nonduals[17], so rather than comparing duals to nonduals, we compare Medicaid cost-sharing payments for E&M services supplied to duals by qualifying and non-qualifying providers over time.

Figure 1 reports how (seasonally-adjusted) payments evolve in the months surrounding the policy implementation for E&M services provided by qualifying and non-qualifying providers. Panel (a) displays the share of Medicare's cost-sharing that was paid for claims submitted to Medicaid. In the pre-period, Medicaid paid out less than half of Medicare's cost-sharing for claims submitted by qualifying providers. Beginning in the month the policy is implemented (January 2013), Medicaid payments sharply increased to more than two-thirds of Medicare's cost-sharing for claims submitted by qualifying providers. In contrast, payments for claims submitted by non-

[15] See, for example, pages 119-121 of MedPAC (2022).

[16] See, e.g., `https://www.healthaffairs.org/do/10.1377/forefront.20201201.150443/`.

[17] In our sample of nonduals never enrolled in Medicaid over a five-year period, only a trivial number of E&M claims appear to have a matching Medicaid claim, on the order of five per million. These claims are most likely submitted to Medicaid due to administrative error.

qualifying providers continue a slow downward trend over this time period. Given the documented increase in payments, qualifying providers had increased incentive to submit cost-sharing claims to Medicaid after the payment reform was implemented. Consistent with these changing incentives, Panel (b) reports a sharp increase in claim submission among qualifying providers in the month the policy was implemented. In 2013, the share of Medicare claims from qualifying providers that were submitted to Medicaid increased by about twelve percentage points, from 35% to 47%. There is no analogous increase in submissions among non-qualifying providers. Finally, Panel (c) shows how "actual payments" to providers—Medicaid payments as a share of Medicare's cost-sharing regardless of whether the claim was submitted to Medicaid—evolve over this time period. Changes in actual payments are driven by changes in both payments conditional on submission and claim submission. This figure shows that the share of Medicare's cost-sharing that was actually paid out to qualifying providers doubles in 2013, from about 16% to 32%. Actual payments to non-qualifying providers show no such increase. Collectively, these findings establish a strong first stage: the payment reform sharply increased Medicaid payments for Medicare's cost-sharing for E&M care provided to duals by qualifying providers, and this increase begins in the first month the policy is implemented.

Table 2 reports first-stage estimates of the effect of the reform on payments comparing changes in payments over time for care provided by qualifying and non-qualifying providers (as in Equation (2)). Paralleling the graphical evidence discussed above, the regression estimates indicate that payments and claim submission sharply increase for care provided by qualifying providers after the reform is implemented. The results are similar whether we exclude any additional controls (Panel A) or include the controls from our baseline utilization analysis— age, sex, and county (Panel B). Confidence intervals are similar but slightly larger if we derive them from a wild bootstrap with state clusters (Appendix Table A.1) rather than the baseline analysis using county-level clustering.

The estimates in the table allow us to decompose how much the increase in cost-sharing payments is attributable to its two components: the increase in cost-sharing payments for submitted claims and the increase in claim submission. If Medicaid had begun paying out cost-sharing claims at the higher rate but there had been no increase in claim submission, the resulting increase in actual payments would have been about half the observed increase. Similarly, the increase in claim submission behavior accounts for about half of the observed increase.

While these estimates capture the mean change in Medicaid payments as a fraction of Medicare's cost-sharing, we can scale these effects to recover the implied change in total payments providers receive, summing across Medicare and Medicaid payments. We characterize the change in total payments to providers using the fact that Medicare's cost-sharing on average accounts for 33% of the Medicare-defined full payment rate for targeted services provided to duals by qualifying providers (see Table 1). Combining this with the results from Table 2 Panel B, we obtain that this reform caused a 6.5% increase in total payments based on actual payments (from column 2) or a 5.5% increase in total payments based on payments conditional on submission (from column

3). Relative to mean Medicaid payments in the pre-period, the reform caused an 86% increase in actual payments from Medicaid and a 28% increase in payments from Medicaid conditional on submission. We combine the estimated change in total payments and our reduced form estimates of the impact of the reform on utilization to calculate an implied payment elasticity in Section 4.2.

4 Results

4.1 Main Results

Next, we turn to our main difference-in-differences event study examining the impact of the payment change on the amount of E&M services received by duals. Our first specification compares E&M utilization between dual and nondual beneficiaries. The coefficients capturing the relative change in E&M utilization by year (denoted β_t in Equation (1)) are presented in Figure 2 and reported in Table 3. We find that in the years prior to the payment increase, duals and nonduals experienced similar trends in E&M services, but diverged sharply following the payment increase, with a relative increase in annual E&M services among duals of 0.79 (about 4.1% relative to the dual pre-policy mean) in 2013 and 1.21 (6.3%) in 2014 relative to their nondual counterparts. These effects are precisely estimated, with 95% confidence intervals (CIs) allowing us to rule out effects outside the range of 0.65 to 0.93 in 2013 and outside the range of 1.01 to 1.41 in 2014. Similarly, we see an increase in E&M visits, of 0.53 visits (about 4.7%) in 2013 [95% CI: 0.46 to 0.60] and 0.62 visits (about 5.4%) in 2014 [95% CI: 0.53 to 0.71].

In addition to exploring the effect on total E&M services and visits, we also examine how the payment change affected the probability a beneficiary had *any* E&M visit in a given year. As reported in Figure 2 Panel (c) (with estimates reported in Table 3 column 3), we see that the payment change increased the probability of having an E&M visit within a year by 0.9 percentage points. This effect is large, as it represents an 8.7% decrease in the mean share of individuals with no visits in a given year (10.4%).

To verify that the precise timing of the impacts aligns with the timing of the payment change, we estimate monthly difference-in-differences event studies. Figure 3 reports the coefficients from a monthly version of Equation (1) for E&M services and E&M visits.[18] The divergence between the utilization of duals and nonduals began sharply in January 2013, exactly coincident with the increase in payments from (and submissions to) state Medicaid programs (see Figure 1). The precise alignment of the timing of the effects on utilization and payments strongly suggests that the estimated change in utilization is due to the change in physician payments, rather than any continuously trending factor that may be correlated with dual-eligibility. Further, we are not

[18] We exclude the extensive-margin "any E&M visit" outcome from this analysis because of the difficulty in interpreting this variable at a monthly level.

aware of any other policy change that went into effect the same month (January 2013) that would have differentially affected duals and nonduals.

Robustness to Alternative Sources of Variation We further investigate the impact of the payment increase by relaxing the difference-in-differences identification assumption behind the baseline dual-nondual specification. We do so by leveraging differences across providers who did and did not qualify for the payment increase. The top row of Figure 4 displays the results from re-estimating the baseline difference-in-differences specification for E&M services and visits separately for care provided by qualifying and non-qualifying providers.[19] Coefficient estimates corresponding to those in Figure 4 are reported in Table 4. Estimates for qualifying providers, who would have benefited from the payment increase, are represented by the solid black line, while estimates for non-qualifying providers, who would not have seen higher payments, are denoted in the dashed line. We see that E&M services and visits provided by qualifying providers to duals increased sharply relative to those among their nondual counterparts, but that such outcomes experienced no similar relative change when examining services provided by non-qualifying providers.[20]

Just as we can examine how care changed within each qualifying status across duals relative to nonduals, we can examine how care changed within each dual status across qualifying and non-qualifying providers through estimating Equation (2). These results are presented in the bottom row of Figure 4 (with coefficient estimates in Table 4). Prior to the payment change, there was an upward trend in E&M services and visits at qualifying providers relative to non-qualifying providers that was nearly identical across duals (solid black line) and nonduals (dashed line). However, beginning in 2013, these trends diverged, with dual patients experiencing larger increases in services (Panel (c)) and visits (Panel (d)) provided by qualifying relative to non-qualifying providers. The fact that trends in the use of care at qualifying relative to non-qualifying providers tracked so closely prior to the payment increase, but diverged sharply in 2013, provides reassurance that the effects we documented are due to the policy itself, rather than concurrent shocks around the time of the policy's implementation. Further, the fact that care provided to nonduals at qualifying and non-qualifying providers trended similarly before and after the reform is implemented suggests there were no obvious spillovers from this reform on standard (nondual)

[19] As above, we drop the extensive-margin "any E&M visit" outcome from this analysis since it is unclear how to define it when splitting by provider type.

[20] This analysis could potentially be affected by the Primary Care Incentive Program (PCIP), which increased the Medicare component of payments (but not cost-sharing) for most E&M services for primary care providers, a subset of qualifying providers. The PCIP affected payments for all beneficiaries (not just duals) and was in effect for the years 2011–2015. If the PCIP caused an increase in the supply of primary care among qualifying relative to non-qualifying providers, this would appear in Panels (c) and (d) of Figure 4 as an increase in 2011 relative to 2010 that exceeds the annual trend. Instead, we see a linear annual trend in E&M from qualifying versus non-qualifying providers for nonduals between 2010 and 2013, with the trend for duals the same until 2012, deviating in 2013 when the payment increase we analyze for duals takes effect. The finding of a smooth trend between 2010 and 2011 for both duals and nonduals is consistent with the null results reported in Chen et al. (2018), who find that the PCIP did not increase the supply of targeted services for Medicare beneficiaries from primary care providers versus all other providers.

Medicare beneficiaries.

Finally, we can combine all three differences leveraged within Figure 4 to estimate the triple differences model described in Equation (3). The estimates of the three way interaction terms are plotted in Figure 5, with coefficient estimates reported in the bottom panel of Table 4. We find similarly-sized effects in the triple differences model. These triple differences estimates indicate that E&M services increased by 0.610 in 2013 [95% CI 0.47 to 0.75] and 0.977 in 2014 [95% CI 0.78 to 1.17] as a result of the payment change, and E&M visits increased by 0.399 in 2013 [95% CI: 0.33 to 0.46] and 0.462 in 2014 [95% CI: 0.38 to 0.55]. Appendix Figure A.1 repeats our triple differences specification at the monthly level, confirming that the relative shift in the utilization targeted by the reform happens sharply during the first month the payment change was implemented—January 2013.

Robustness to Alternative Specifications We next return to our baseline dual-nondual comparison and investigate the robustness of these results to alternative specifications. As shown in the first three columns of Table 5, our estimates are similar, although slightly larger, if we include county by year fixed effects (in lieu of separate county and year fixed effects). The similarity of the results when including county by year fixed effects allows us to rule out that the results are driven by other factors that vary across space and time. For instance, this provides reassurance that the results are not influenced by state decisions to expand Medicaid among non-elderly (Medicaid-only) beneficiaries or any changes in federal geographic-based payment policy.[21]

If we use an unbalanced panel and include individual fixed effects, rather than restricting our sample to be enrolled as a dual or nondual beneficiary in fee-for-service Medicare over the entire five-year period, we see that the estimates are notably larger, with an increase in E&M services of just over 2 (8.2% relative to the pre-policy mean among duals) and in E&M visits of 1.14 (8.5%). However, within the unbalanced sample, we observe some statistically significant association between E&M utilization and the identifying variation in the period before the payment change is adopted. These patterns in the unbalanced sample indicate there may be differential selection into the unbalanced sample over time (e.g., into fee-for-service Medicare) across dual and nondual beneficiaries or differential selection into the Medicaid program over time among fee-for-service Medicare beneficiaries. This makes it difficult to interpret the unbalanced sample estimates and leads us to prefer the balanced sample as our main estimate of the causal impact of the payment increase.

The last three columns of Table 5 show that our core findings are very similar if we restrict attention to the 13 states with available Medicaid data for which we provide evidence on the first-stage effect of the policy on provider payments.

Appendix Table A.2 presents the results of two additional alternative specifications. First, we

[21]The stability of our estimates when controlling for county by year effects is consistent with prior work documenting that there were no spillovers on Medicare beneficiaries (including duals) from state Medicaid expansions (Carey, Miller and Wherry, 2020; Neprash et al., 2021).

report results from a specification where standard errors are clustered at the state rather than county level. Our conclusions are unchanged when varying the level of clustering. Second, we report results from a specification using an alternative control group—Medicare beneficiaries who are "near poor" but are still subject to typical Medicare cost-sharing (and are thus ineligible for the payment increase). Following Roberts and Desai (2021) and Fung et al. (2021), we identify "near poor" beneficiaries as Medicare beneficiaries with income/assets that are too high to be eligible for standard Medicaid coverage but who are eligible for means-tested "partial" Medicaid benefits (excluding the exemption from cost-sharing) or the Medicare Part D low-income subsidy. A drawback of this alternative control group is that "partial" Medicaid beneficiaries could experience spillovers if providers are unaware of the exact type of Medicaid beneficiary and do not recognize the policy was specific to the subset of Medicaid beneficiaries with cost-sharing exemptions.[22] In practice, the results with this alternative control group are challenging to interpret because there is evidence of non-parallel pre-trends. Nevertheless, the post-period coefficients with this alternative control group are similar to our baseline results for E&M services and visits.

Together, these robustness checks provide reassurance that the increased utilization we document is indeed due to the change in provider payments rather than another policy or omitted factors. We find an increase in E&M services for duals as compared to nonduals, as well as an increase in E&M services supplied by qualifying providers as compared to non-qualifying that is much larger among duals than it is among nonduals. The alignment in the precise timing of the changes in utilization and payments measured at a monthly level provides further reassurance that the patterns are unlikely to be driven by unobserved factors. Finally, we confirm that our findings are similar when including finer controls or limiting to the subsample of states reporting Medicaid payment data.

4.2 Interpretation

To contextualize our findings, we benchmark the magnitude of the impacts we estimate against observed gaps in access conditional on observable health status. As described earlier, the disparity in provider payments between duals and nonduals is one possible explanation for the disparity in access to care between these groups. While we find both intensive- and extensive-margin effects of the payment increase, we focus here on the extensive margin (any E&M visit) because it is a focus of the literature that measures access to care among underserved populations. For this exercise, we measure the gap in access through an OLS regression using data from the years prior to the payment reform. We regress an indicator for receiving any E&M on a dual indicator, our baseline fixed effects (year, county, age group), and a set of binary variables indicating the presence of the 19 different chronic conditions that form the Charlson Comorbidity Index. We interpret the coefficient on the dual indicator as a measure of the gap in access prior to the payment reform

[22] As described in Section 2.3, our baseline analysis sample drops these "partial" Medicaid beneficiaries.

conditional on observable measures of health. We find that, prior to the payment change, duals are 1.1 percentage points less likely than nonduals to have any E&M visit in a given year, about 1.2% less when compared to the mean among the nonduals (Appendix Table A.3). Comparing the observed gap to our estimates of the effect of the reform on the likelihood of having any E&M visit, we see that the reform closed 82% of the observed access gap between dual and nondual beneficiaries. This finding highlights the potential for payment policy to reduce socio-economic gaps in access to health care.[23]

Next, we calculate implied elasticities based on our estimates and discuss how these compare with elasticities estimated in prior work. These elasticity calculations have some limitations; for example, non-pecuniary factors such as hassle costs may have changed at the same time as the payment policy, which may have altered the perceived reimbursement for a service beyond the actual money received. Since such changes in hassle costs are unobservable (to us), we cannot incorporate them into our elasticity calculation, although the fact that we observe the amount received by a physician does allow us to account for dimensions of hassle such as claim denial rates. Despite these limitations, we believe an elasticity calculation is still valuable in that it allows us to benchmark our estimates to those published in previous work (which also did not incorporate hassle costs). Furthermore, if any changes in unobserved factors related to reimbursement were small, our estimate may be a close approximation of the actual elasticity of interest.

We calculate elasticities with respect to provider payments, combining the evidence of the reduced form effects of the payment reform on utilization with the effect of the reform on payments themselves. We focus on medium-run elasticities, drawing on the utilization estimates from the second year of the payment increase. For consistency, the elasticities are calculated using estimated effects on utilization and payments within the subset of states for which we have linked Medicaid data (as reported in Table 5), though we obtain similar elasticities when using our baseline (nationwide) utilization estimates.

Table 6 displays elasticities based on our estimates and the associated bootstrapped 95% confidence intervals.[24] Given that claim submission is far from complete, it is natural to calculate elasticities with respect to two measures of payments—"actual payments" accounting for incomplete submission (column 1) and payments conditional on submission (column 2). We calculate elasticities with respect to total payments to providers—i.e., payments from both Medicare and

[23]We note that the fact that similar shares of duals and nonduals obtained E&M services after the payment change does not mean the reform removed all barriers to access that differentially affect duals. For example, to the extent that the Charlson chronic condition indicators are insufficient controls for health status, our analysis may have understated the true gap in access prior to the payment change. Moreover, even if all else were equal, nonduals may use less care than duals because many nonduals face substantial cost-sharing for these visits; alternatively, duals and nonduals may value medical services differently. Thus, there may be persistent socio-economic gaps in access to health care, despite the observed similarity of utilization rates across duals and nonduals after the reform.

[24]We conduct a clustered bootstrap at the county level. For each of 1,000 bootstrap samples, we recompute estimates of the change in Medicaid payments and the change in utilization and take the ratio of these estimates to form an elasticity estimate. We generate 95% confidence intervals by taking the estimates at the 2.5th and 97.5th percentiles of this distribution.

Medicaid— and elasticities with respect to payments from Medicaid alone. To estimate the effect of the reform on total payments, we use the fact that Medicare's cost-sharing accounts for 33% of the full Medicare payment rate, on average, for targeted services provided to duals by qualifying providers (see Table 1).

The reform caused a 6.5% increase in total actual payments and a 5.5% increase in total payments conditional on submission. Based on the utilization estimates described above in Table 5 columns 7 and 8, the reform caused a 7.8% increase in both E&M services and in E&M visits provided to duals by qualifying providers. Combining these estimates, we obtain an elasticity with respect to total actual payments of 1.2 for both E&M services [95% CI: 0.94 to 1.47] and E&M visits [95% CI: 0.89 to 1.50]. The elasticity in terms of total payments conditional on submission is 1.4 for both E&M services [95% CI: 1.06 to 1.86] and E&M visits [95% CI: 0.92 to 1.95].

For comparison, Table 6 also presents elasticities with respect to Medicaid payments, rather than total payments. The payment reform caused an 86% increase in actual Medicaid payments and a 28% increase in Medicaid payments conditional on submission. These payment changes imply an elasticity of 0.09 for E&M services [95% CI: 0.07 to 0.12] and E&M visits [95% CI: 0.07 to 0.12] with respect to actual Medicaid payments and 0.28 for E&M services [95% CI: 0.19 to 0.40] and 0.29 for E&M visits [95% CI: 0.16 to 0.44] with respect to Medicaid payments conditional on submission.

Finally, Table 6 also presents semi-elasticities illustrating the percent change in services for a 10 percentage point increase in the payment rate relative to the full Medicare payment rate. The payment reform caused an increase in payments relative to the full Medicare payment rate of 4.7 percentage points based on actual payments and 4.6 percentage points based on payments conditional on submission. Combining this evidence with the estimated changes in utilization, we obtain semi-elasticities indicating that a 10 percentage point increase in payments relative to the full Medicare payment rate would lead to an 16% increase in E&M services [95% CI: 0.13 to 0.20] and in E&M visits [95% CI: 0.12 to 0.21] based on actual payments and a 17% increase in E&M services [95% CI: 0.13 to 0.22] and E&M visits [95% CI: 0.12 to 0.23] based on payments conditional on submission.

We note that the payment elasticity we estimate is not comparable to traditional labor supply elasticities estimated in other settings. A key difference is that the provision of E&M services—and physician services more generally—involves substantial variable costs, meaning the net per-service revenue (or the net wage rate) received by physicians is smaller than the overall payment rate for these services. For example, variable costs arise when physicians need to pay office staff, nurses, and/or physician extenders (e.g., nurse practitioners or physician assistants) for their role in scheduling, assisting with, and providing E&M services. Medicare estimates that a provider's own work represents roughly 40% of the resources used in the provision of physician services overall and 55% of the resources used in the provision of E&M services in particular.[25] Thus, a 1% increase

[25]We represent Medicare's estimates of the share of inputs due to provider time as the share of total RVUs

in payments for E&M services translates to roughly a 1.8% increase in physician earnings for these services, and our estimated payment elasticity would imply a labor supply elasticity around 0.7. We note that an implied labor supply elasticity of 0.7 is in line with some prior estimates of labor supply elasticities among the self-employed (Saez, 2010) and physicians (Clemens and Gottlieb, 2014).

Further, it is important to emphasize that the payment change we analyze is a targeted change that only applied to a subset of procedures (E&M services provided by primary care providers) and a subset of patients (the one-fifth of Medicare patients who dually qualify for Medicaid). Physicians may have more ability to adjust their hours and variable cost inputs (e.g., support staff and capital inputs) in response to a targeted payment change than would be proportionally feasible in response to an across-the-board payment change affecting all procedures or all patients. Thus, we might expect a larger estimated payment elasticity (and implied labor supply elasticity) from this payment change targeting primary care E&M services provided to low-income beneficiaries than would be expected for the same services from a hypothetical, broader-based change in payments. While there is little known about the effect of broader-based changes in payments on the provision of E&M services in particular, our payment elasticity estimates are comparable to those estimated for physician services overall in Clemens and Gottlieb (2014) using broader-based payment changes.

In comparison to the payment area consolidation analyzed in Clemens and Gottlieb (2014), the reform we analyze causes a larger and more targeted change in payments, both in terms of beneficiaries targeted by the reform (duals rather than all Medicare beneficiaries) and services targeted by the reform (E&M services rather than all physician services). It is difficult to compare our estimates of the elasticity of E&M services to those reported in Clemens and Gottlieb (2014) because their setting has insufficient precision to look at these types of services separately. Our estimated elasticity of 1.2 for E&M services [95% CI: 0.94 to 1.47] lies within the confidence interval of Clemens and Gottlieb (2014) for that subcategory of services in the medium-run: -0.19 to 2.13. However, we can compare our estimates to the main estimates in Clemens and Gottlieb (2014), which imply a payment elasticity of around 1.5 for overall physician services (as summarized by total RVUs). We obtain a similar, albeit slightly smaller, elasticity for E&M services—the services most closely linked to provider time—as Clemens and Gottlieb (2014) find with a broader-based payment change for overall physician services, which are typically more capital-intensive and require less physician time. Furthermore, we note that our payment elasticity estimates imply similar wage elasticities when scaled by the relative importance of provider time inputs in the provision of overall physician services and E&M services.[26] Importantly, our findings suggest that payment policy targeting a subgroup of Medicare beneficiaries—duals who comprise 20% of all

(relative value units) for services that are due to the provider's own work.

[26]Based on Medicare's estimates that roughly 40% of overall physician services are related to physician time, Clemens and Gottlieb (2014) argue that their estimated elasticity of 1.5 for all physician services is equivalent to a wage elasticity of around 0.6. Given that Medicare estimates that provider time represents roughly 55% of resources used in the provision of E&M services, our estimates would imply a wage elasticity around 0.7.

Medicare beneficiaries—can have a large impact on services provided, even among services that are tightly linked to physician labor supply and generally may be more inelastically provided than physician services overall. More generally, this suggests that targeted payment increases for care provided to low-income individuals may be a powerful policy tool to close disparities in health care access and utilization between low- and high-income individuals.

4.3 Mechanisms

To better understand how the payment increase encouraged additional consumption of care, we next explore the response of provider inputs. First, we examine whether the effect of the payment increase varied across patients who had an existing relationship with the provider or who were new patients. Comparing Panels (a) and (b) in Figure 6, and the corresponding columns 1 and 2 in Table 7, we see that overwhelmingly the change in E&M visits is driven by an increase in established patient visits rather than new patient visits. The payment increase caused an increase of 0.66 [95% CI: 0.57 to 0.75] in established patient E&M visits, or 6.2% of the baseline mean. In contrast, the payment increase caused a small but statistically significant *decrease* in new patient E&M visits of 0.037, where the 95% confidence interval allows us to rule out a decrease outside of 0.027 to 0.047. This represents roughly a 4.8% decline in new patient visits relative to the baseline mean, but is economically trivial, at less than a twentieth of a visit. Because we also observe a significant increase in the likelihood that a beneficiary has any visit in a year, these patterns suggest that the reform induced beneficiaries with infrequent visits to obtain care more often—moving, for example, from a visit every other year to a visit every year. More generally, these results suggest that the payment increase resulted in increased frequency of interactions within existing provider-patient relationships, rather than the establishment of new relationships. And, the small decline in new patient visits may reflect less churn across providers as established provider-patient relationships solidified.

Next, we characterize the amount of additional time, in minutes, that providers spent treating dual-eligible beneficiaries. We map E&M services to provider time associated with these services, following the methodology in Fang and Gong (2017). These results—presented in Figure 6 Panel (c) and in Table 7 column 3—demonstrate that each dual-eligible beneficiary received an additional 19 minutes of provider time on average per year as a result of the payment increase, with a 95 percent confidence interval that allows us to rule out an effect greater than 22 minutes or smaller than 16 minutes.

A natural question to consider is whether this additional time spent with dual patients resulted in reduction of time spent with nondual patients (i.e., negative spillover effects). However, as we demonstrated in Figure 4, E&M services performed by—and thus time spent with—qualifying providers (relative to non-qualifying) trended similarly for nonduals before and after the payment increase was implemented. It therefore does not appear to be the case that nonduals experienced

a negative spillover in the form of a decrease in provider time as a result of the payment increase, although we naturally cannot rule out that there were such spillovers onto other non-Medicare patients or those in Medicare Advantage.[27]

Finally, we look at whether resources supplied per visit change with the payment increase, through estimating the impact on billed provider time per visit and RVUs per visit. Figure 6 panels (d) and (e) and Table 7 columns 4 and 5 display these results. Both of these measures slightly declined when payments increased: billed provider time per visit declined by 0.40 minutes (1.5% of the baseline mean) and RVUs per visit declined by 0.02 (1.3% of the baseline mean). These results are consistent with multiple possible responses on the part of providers. For instance, it is possible that the marginal visits induced by the payment increase involved less effort and fewer resources than inframarginal visits that would have occurred absent the payment change. Alternatively, it could be that the payment increase affected time allocated to inframarginal visits with duals, as providers may have reduced time and resources spent on inframarginal visits in order to accommodate more visits.[28]

4.4 Heterogeneity

An increase in provider payments may affect patients differently based on their demographic characteristics, location, or underlying health status. Our setting is well-suited to examine such heterogeneity given that our sample size and the payment increase we study are both large. Identifying whether some subgroups benefit disproportionately from the payment increase can help policymakers target future policies to those who most stand to gain. In this section, we examine whether the impact of the payment increase varied across patients with different characteristics.

Figure 7 displays heterogeneity in the effect on E&M services by patient demographic and geographic characteristics, with estimates summarizing these results reported in Table 8.[29] The top row of Figure 7 displays the estimates by patient sex. We see that the estimated effects are broadly similar among male and female beneficiaries, and these estimates are statistically indistinguishable from one another. The second row displays the results by age. The estimated effects of the payment change are larger among younger beneficiaries relative to older beneficiaries, where these estimated effects are statistically distinguishable from one another at the 0.01 percent level. Among beneficiaries younger than 75 years, the payment change increases E&M services provided by 1.92 RVUs per person annually, or 10.3% on a baseline mean of 16.80 for these

[27] We note that our finding of no spillovers on non-targeted (i.e., nondual) Medicare beneficiaries is consistent with prior work illustrating no spillovers from other recent health insurance expansions (Carey, Miller and Wherry, 2020; Neprash et al., 2021).

[28] If changes in provider time among inframarginal visits are a contributing factor behind this finding, providers must have differentially reduced time spent with dual patients relative to nondual patients, rather than an across-the-board reduction in time spent on inframarginal visits.

[29] While Figure 7 and Table 8 report estimates for E&M services, we obtain very similar patterns for analgous regressions investigating E&M visits.

beneficiaries. In contrast, we find that beneficiaries age 75 and older increase utilization by 1.04 RVUs, or 5.0% on a baseline mean of 20.76 for this subgroup.

We also investigate the impacts of the reform by patient race. The third row reports the impacts of the payment increase separately for white beneficiaries and non-white beneficiaries. The effects appear more concentrated among white beneficiaries. The payment change increases utilization among white beneficiaries by 0.96 RVUs per person annually, or 5.2% of the baseline mean. The estimated increase for non-white beneficiaries is substantially smaller—0.24 RVUs (or 1.5% of the baseline mean) —and statistically indistinguishable from zero.

The fourth row of Figure 7 displays estimates by whether the patient resides within a primary care Health Professional Shortage Area (HPSA)—counties with few primary care providers per capita. The impacts of the payment increase are larger for individuals residing within non-HPSA counties, with the effect statistically distinguishable from the effect among those residing in HPSA counties. The estimates indicate that individuals outside of HPSA counties increase utilization by 1.42 RVUs per person annually, or 7.3% of the baseline mean. In contrast, we see individuals in HPSA counties increase utilization by 0.99 RVUs per person annually, or 5.2% of the baseline mean.

Next, we consider heterogeneity by baseline patient health and prior medical utilization. We define these measures using data from 2010-2012—prior to the payment increase—to avoid defining measures that may reflect endogenous responses to the policy. Figure 8 provides this evidence. In the top row, we present evidence by baseline patient health, where patients are classified based on their pre-period Charlson Index.[30] The effects on the level of utilization are economically and statistically similar across patients in worse or better baseline health. However, because baseline utilization is much lower among individuals with fewer health issues, the effects are larger on a percent basis within this subgroup: patients in better baseline health (with a pre-period Charlson Index <2) experience a 17.0% increase in utilization while patients in worse baseline health (with a higher pre-period Charlson Index) experience a 6.9% increase in utilization.

The second row of Figure 8 presents estimates of the payment change's impact for subgroups of patients who did or did not have a preventable Emergency Department (ED) visit in the pre-period. The effects are similar and statistically indistinguishable across these subgroups. However, as with the health measure above, baseline utilization is very different for those with and without a preventable ED visit in the pre-period. Thus, the estimated effects are larger relative to the baseline mean among those with no preventable ED visit (11.0% vs. 5.5%). Collectively, this evidence implies that the payment increase had the effect of increasing the share of E&M services provided to patients in better health and patients without a preventable ED visit.

[30]See Section 2 for more discussion of the Charlson Index.

5 Conclusion

This paper analyzes the effect of a large payment increase for primary care services provided to low-income elderly and disabled individuals in the US—individuals dually eligible for Medicare and Medicaid. This payment increase worked toward reducing disparities in payments providers receive for services provided to low-income elderly and disabled Medicare beneficiaries relative to services provided to their higher income counterparts. By combining administrative data from Medicare and Medicaid, we demonstrate the reform we analyze induced a sharp increase in payments for primary care services provided to duals. Leveraging a difference-in-differences research design, we show that this increase in payments leads to a substantial increase in primary care services and associated visits supplied by qualifying providers to targeted beneficiaries. Before the payment increase, duals were less likely to have at least one primary care visit each year than nonduals with similar observable characteristics, but the payment increase closes this gap. We illustrate that the findings are robust when analyzing utilization patterns at the monthly level, varying included controls, and leveraging additional policy variation in alternative difference-in-differences or triple differences specifications. Supplemental analysis suggests this increase represents an intensifying of existing provider-patient relationships due to a greater investment of provider time, as opposed to the formation of new provider-patient relationships. Heterogeneity analysis suggests that the payment increase had near-universal impacts on the utilization of all beneficiaries, though the effects appear somewhat larger for beneficiaries who are younger, are white, and live in areas with many primary care providers per capita.

Recent public policy has made tremendous efforts to expand access to health care, often through public health insurance programs. Nevertheless, disparities in health and access to care remain persistent. While much of the recent policy and research efforts have focused on demand-side policies such as cost sharing or premiums, the role of provider payments in affecting access for low-income patients has received less attention among policymakers. This lack of attention to the provider side has led to a two-tiered system in the US's largest health insurance program—Medicare—with providers receiving reduced payments for services provided to low-income beneficiaries relative to same services provided to higher-income beneficiaries. Our research suggests that lower provider payments are a key determinant of disparities in utilization between low- and high-income Medicare beneficiaries and demonstrates that reducing gaps in provider payments may work to close gaps in health care access among Medicare beneficiaries. More broadly, our findings suggest targeted increases in provider payments may be a promising policy tool to address socio-economic gaps health care access and health in other settings.

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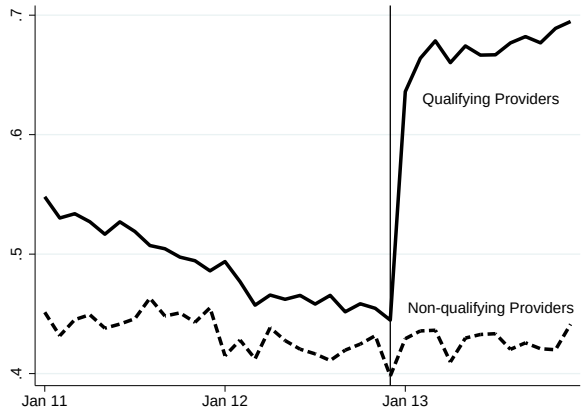
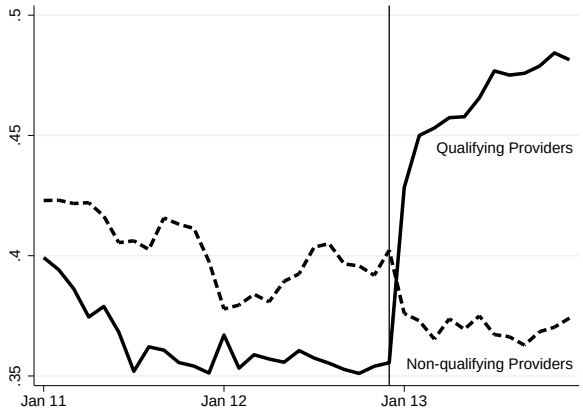
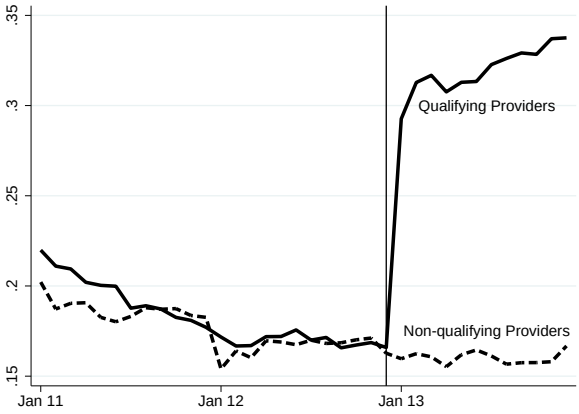
Figure 1: First Stage: Submission and Payment of Cost-Sharing Claims



(a) Cost-Sharing Payment Rate | Submission



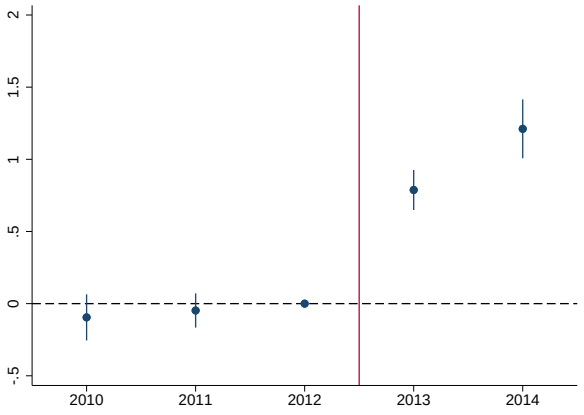
(b) Submissions



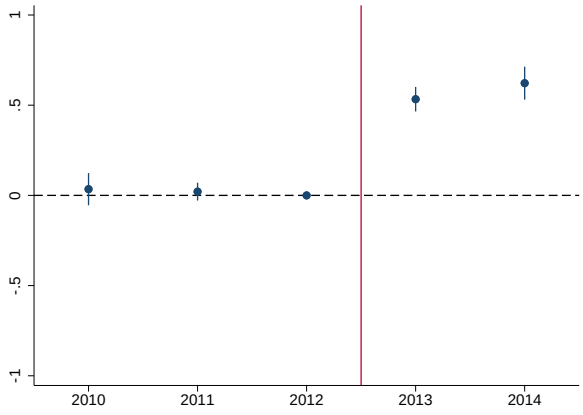
(c) Cost-Sharing Payment Rate

Notes: These figures report monthly outcomes for E&M claims from qualifying and non-qualifying providers among dual-eligible beneficiaries in 13 states with available Medicaid claims. Panel (a) reports the share of cost-sharing paid for submitted claims. Panel (b) reports the share of claims submitted to Medicaid. Panel (c) reports the overall share of cost-sharing paid by Medicaid (set to zero for unsubmitted claims). Outcomes are net of month-of-year fixed effects.

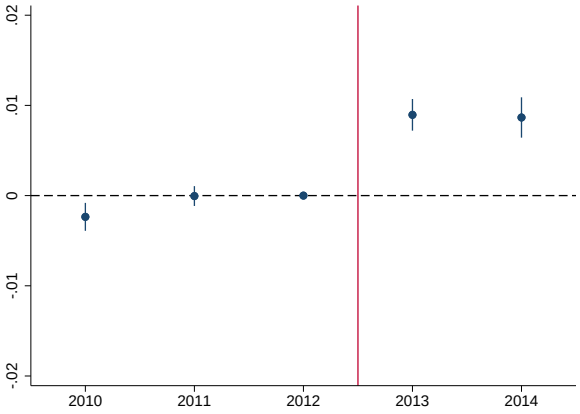
Figure 2: Impact on E&M Services and Visits: Baseline Difference-in-Differences (dual vs. nondual)



(a) E&M Services



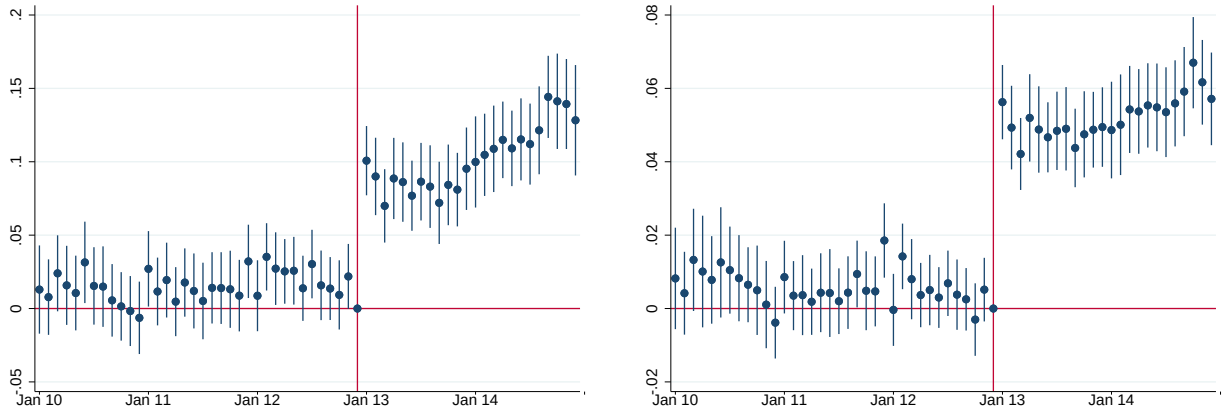
(b) E&M Visits



(c) Any E&M Visit

Notes: These figures report coefficient estimates and 95 percent confidence intervals from Equation (1) of the interaction between year fixed effects and the indicator $Dual_i$ equals 1. See text for further details.

Figure 3: Impact on Monthly E&M Services and Visits: Baseline Difference-in-Differences (dual vs. nondual)

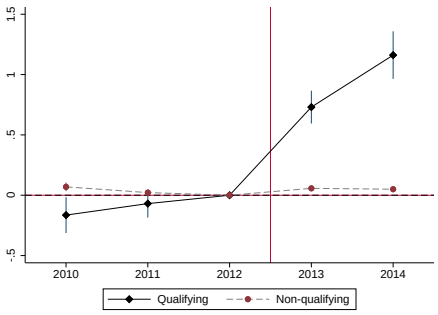


(a) E&M Services

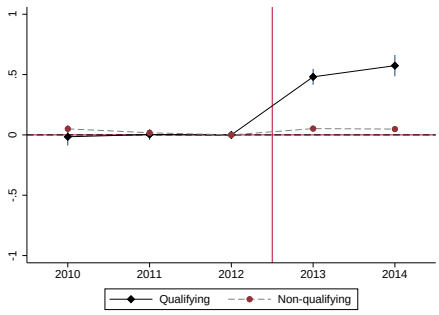
(b) E&M Visits

Notes: These figures report coefficient estimates and 95 percent confidence intervals from the monthly analog of Equation (1) of the interaction between month fixed effects and the indicator $Dual_i$ equals 1. Each series is seasonally adjusted with month-of-year fixed effects. See text for further details.

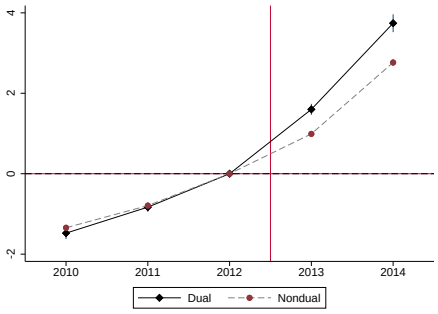
Figure 4: Impact on E&M Services and Visits: Additional Difference-in-Differences Specifications



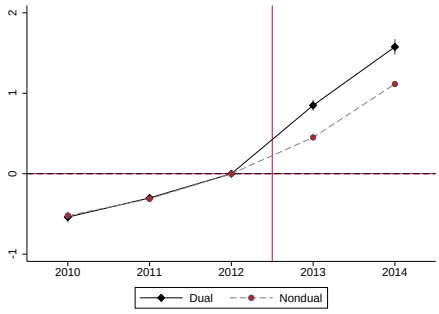
(a) E&M Services: DD dual vs. nondual (by $Qualifying_p$)



(b) E&M Visits: DD dual vs. nondual (by $Qualifying_p$)



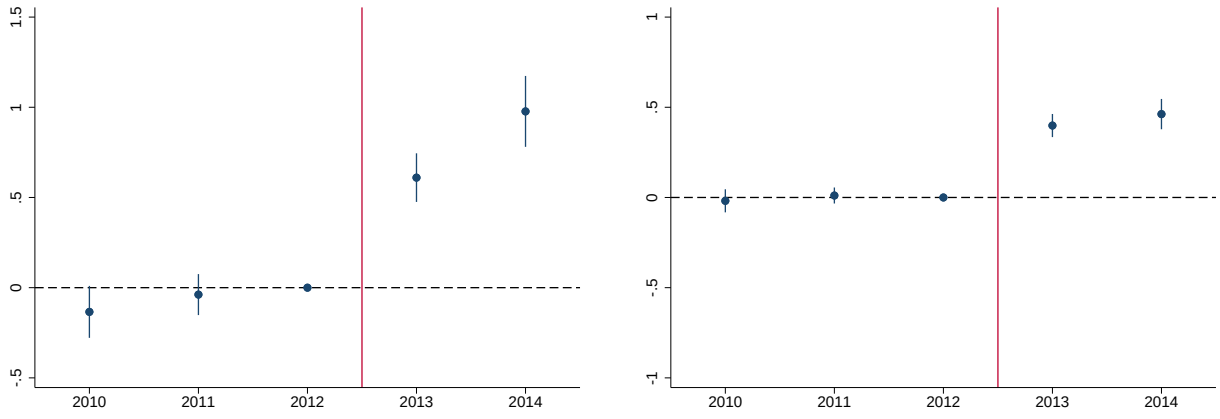
(c) E&M Services: DD qualifying vs. non-qualifying (by $Dual_i$)



(d) E&M Visits: DD qualifying vs. non-qualifying (by $Dual_i$)

Notes: These figures report coefficient estimates and 95 percent confidence intervals from Equation (1) (Panels (a) and (b)) and Equation (2) (Panels (c) and (d)). In Panels (a) and (b), solid lines plot coefficients for services from qualifying providers and dashed lines plot coefficients for services from non-qualifying providers. In Panels (c) and (d), solid lines plot coefficients among duals and dashed lines plot coefficients among nonduals. See text for further details.

Figure 5: Impact on E&M Services and Visits: Triple Differences specification

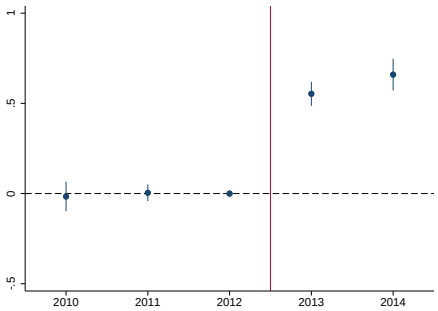


(a) E&M Services

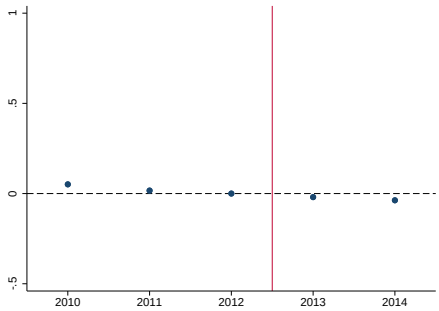
(b) E&M Visits

Notes: These figures report coefficient estimates and 95 percent confidence intervals from Equation (3) of the interaction between year fixed effects, the indicator $Dual_i$ equals 1, and the indicator that the provider is qualifying. See text for further details.

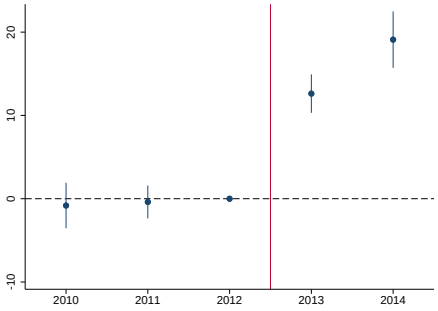
Figure 6: Mechanisms: Impact on Types of Services Provided



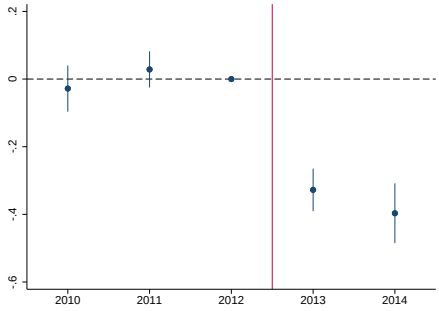
(a) Established Patient E&M Visits



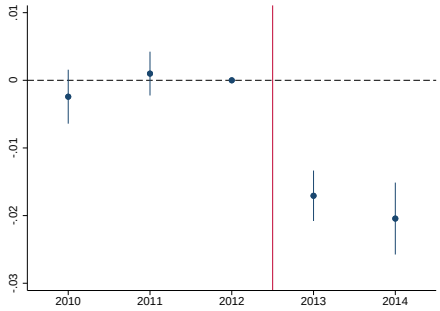
(b) New Patient E&M Visits



(c) Billed E&M Time



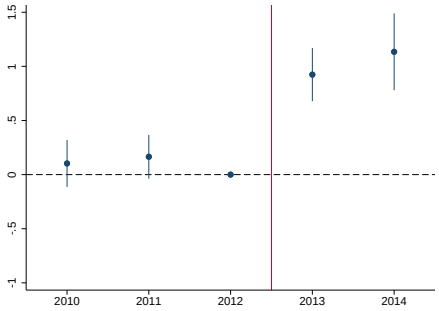
(d) Billed Time per E&M Visit



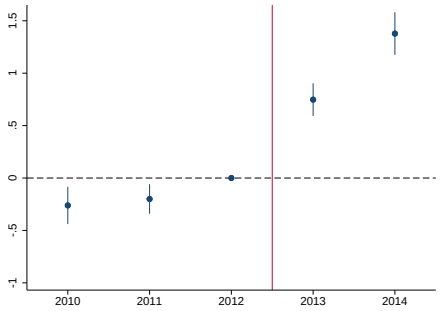
(e) RVUs per E&M Visit

Notes: These figures report coefficient estimates and 95 percent confidence intervals from Equation (1) of the interaction between year fixed effects and the indicator $Dual_i$ equals 1. Each panel is a different dependent variable. See text for further details.

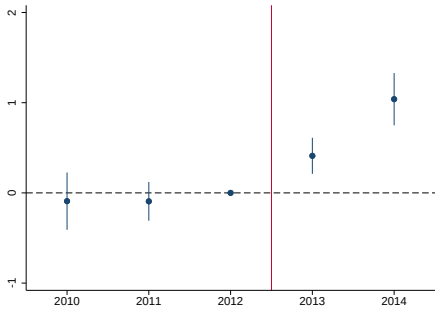
Figure 7: Heterogeneity in Impact on E&M Services by Patient Demographics



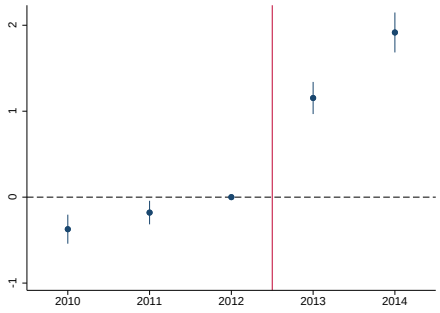
(a) Male



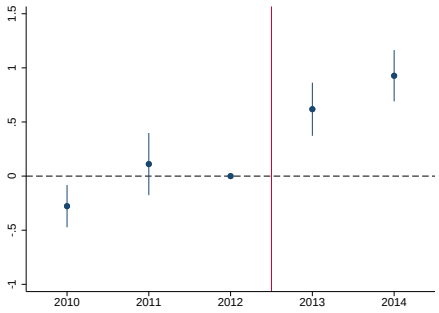
(b) Female



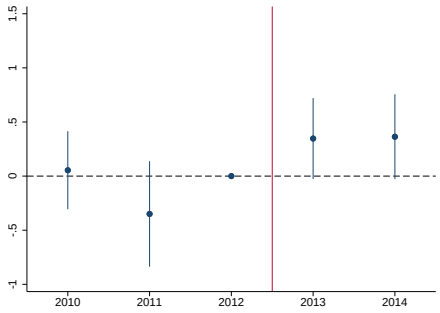
(c) Age >= 75



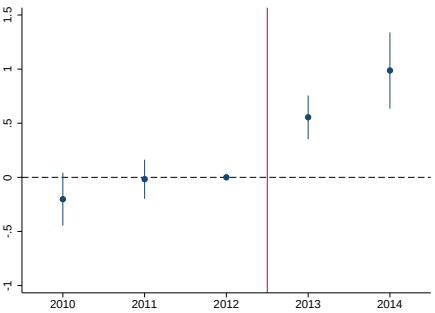
(d) Age < 75



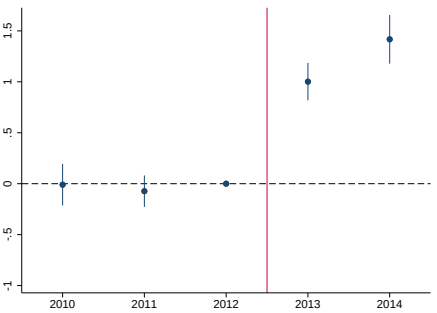
(e) White



(f) Non-White



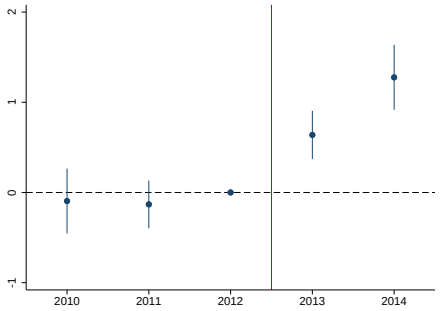
(g) HPSA County



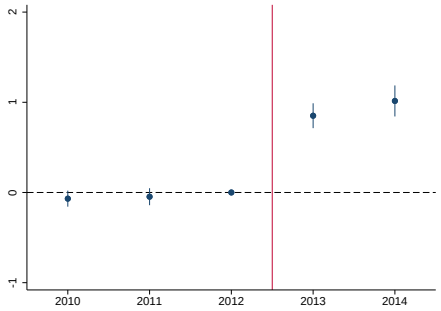
(h) Non-HPSA County

Notes: These figures report coefficient estimates and 95 percent confidence intervals from Equation (1) of the interaction between year fixed effects and the indicator $Dual_i$ equals 1 for the dependent variable E&M services. Each panel plots estimates from regressions estimated on the indicated subgroup. See text for further details.

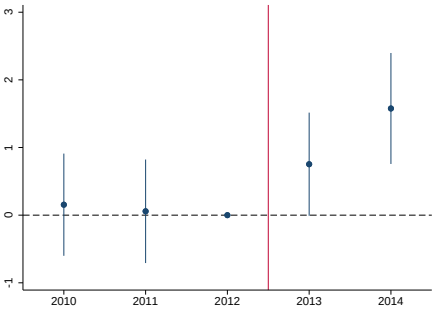
Figure 8: Heterogeneity in Impact on E&M Services by Patient Health and Prior Medical Utilization



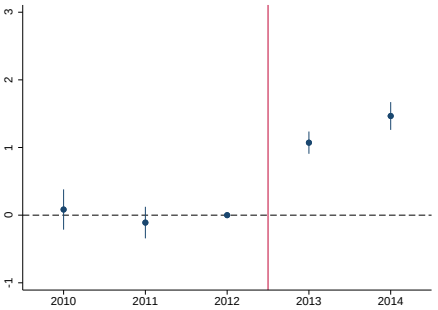
(a) Worse Baseline Health (pre-period Charlson Index ≥ 2)



(b) Better Baseline Health (pre-period Charlson Index <2)



(c) Prior Avoidable ED Visit



(d) No Prior Avoidable ED Visit

Notes: These figures report coefficient estimates and 95 percent confidence intervals from Equation (1) of the interaction between year fixed effects and the indicator $Dual_i$ equals 1 for the dependent variable E&M services. Each panel plots estimates from regressions estimated on the indicated subgroup. Above, ED represents Emergency Department. See text for further details.

Table 1: Descriptive Statistics

Panel A: Medicare Sample	Dual-Eligible		Not Dual-Eligible	
	2010-2012	2013-2014	2010-2012	2013-2014
E&M Visits	11.43 (13.28)	13.22 (15.54)	9.37 (10.06)	10.71 (12.08)
Qualifying Providers	9.64 (11.92)	11.28 (14.21)	7.18 (8.65)	8.39 (10.69)
Non-qualifying Providers	1.79 (3.22)	1.94 (3.42)	2.18 (3.09)	2.32 (3.26)
% With Any E&M Visit	89.63 (30.49)	91.27 (28.22)	89.99 (30.01)	91 (28.62)
E&M Services (Work RVUs)	19.28 (30.93)	23.22 (38.63)	14.01 (20.63)	17.09 (26.91)
Qualifying Providers	16.97 (29.16)	20.66 (36.76)	11.44 (19.04)	14.28 (25.22)
Non-qualifying Providers	2.31 (4.33)	2.56 (4.78)	2.57 (3.8)	2.82 (4.17)
New Patient Visits	.77 (1.21)	.75 (1.19)	.82 (1.18)	.85 (1.22)
Total Work RVUs	38.41 (55.13)	41.91 (61.17)	33.05 (44.72)	36.8 (49.69)
Age	62.44 (16.78)	64.94 (16.76)	73.91 (9.11)	76.41 (9.08)
% Female	61.92 (48.56)		55.14 (49.74)	
% White	62.91 (48.30)		90.45 (29.40)	
% Poor health in pre-period	61.12 (48.75)		54.09 (49.83)	
% Preventable ED visit in pre-period	10.32 (30.42)		4.35 (20.4)	
% in Primary Care Shortage Area	46.2 (49.86)		38.72 (48.71)	
Number of Beneficiary-years	1,019,067	679,378	9,599,970	6,399,980
Number of Beneficiaries	339,689		3,199,990	

Panel B: Medicare E&M Claims For Duals in Medicaid-Reporting States	At Qualifying Providers		At Non-Qualifying Providers	
	2011-2012	2013	2011-2012	2013
Average Cost-Sharing Amount ($)	28.87 (29.56)	31.16 (30.53)	21.53 (19.46)	24.09 (20.61)
Cost-Sharing as % of Total Payment	33.12 (34.28)	35.75 (35.21)	26.54 (21.22)	29.11 (22.46)
% of E&M Claims Submitted	36.29 (48.07)	46.55 (49.85)	40.21 (48.94)	37.05 (48.13)
% of E&M Cost-Sharing Paid	18.22 (51.27)	31.99 (61.03)	17.64 (50.07)	16.05 (47.27)
% of E&M Cost-Sharing Paid | Submission	49.23 (75.22)	66.95 (74.92)	43.31 (71.18)	42.92 (69.56)
Number of E&M Services	3,018,703	1,741,192	386,737	215,186

Notes: This table reports means and standard deviations (in parentheses) of key outcomes and covariates. Panel A reports on our baseline balanced person-year sample of Medicare beneficiaries by dual-eligibility and time period. Panel B reports on the subset of E&M claims from Panel A duals that are in the 13 states with available Medicaid claims, by qualifying provider and time period.

Table 2: First Stage: Submissions and Payment of Cost-Sharing Claims

	(1)	(2)	(3)
Panel A: no controls	Cost-Sharing Payment Rate | Submission	Submissions	Cost-Sharing Payment Rate
2011*Qualifying Provider	0.0277	-0.00931	0.0021
	(0.0094)	(0.0051)	(0.0036)
	[0.003]	[0.067]	[0.553]
2013*Qualifying Provider	0.1960	0.1240	0.1440
	(0.0125)	(0.0125)	(0.0095)
	[0.000]	[0.000]	[0.000]
Panel B: baseline controls			
2011*Qualifying Provider	0.0264	-0.0043	0.0046
	(0.0075)	(0.0034)	(0.0030)
	[0.000]	[0.207]	[0.13]
2013*Qualifying Provider	0.1290	0.1180	0.1430
	(0.0111)	(0.0117)	(0.0093)
	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean at Qualifying Providers	0.4923	0.3629	0.1822

Notes: This table displays estimates of regression coefficients from Equation (2) for the sample of Medicare E&M claims for dual-eligible beneficiaries in Medicaid-reporting states, with each person-year observation weighted by the number of E&M claims represented. Standard errors (clustered at the county level) are reported in parentheses and p-values are reported in brackets. Outcome variables are listed in the top row. Panel A includes no controls, while Panel B includes the "baseline" controls as in Table 3: age bin, sex, and county.

Table 3: Impact of the Payment Increase on E&M

	E&M Services	E&M Visits	Any E&M
	(1)	(2)	(3)
2010*Dual	-0.095	0.035	-0.002
	(0.081)	(0.046)	(0.001)
	[0.242]	[0.447]	[0.003]
2011*Dual	-0.048	0.021	-0.000
	(0.060)	(0.025)	(0.001)
	[0.432]	[0.405]	[0.928]
2013*Dual	0.788	0.533	0.009
	(0.071)	(0.035)	(0.001)
	[0.000]	[0.000]	[0.000]
2014*Dual	1.211	0.622	0.009
	(0.104)	(0.047)	(0.001)
	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean	19.28	11.43	0.896
Dual Pre-Policy Mean at Qualifying Providers	16.97	9.64	
N	17,698,395	17,698,395	17,698,395

Notes: This table displays estimates of regression coefficients from Equation (1). Standard errors (clustered at the county level) are reported in parentheses and p-values are reported in brackets. Outcome variables are listed in the top row. See text for further details.

Table 4: Alternative Sources of Variation

Panel A: Difference-in-Differences Across Dual vs. Nondual Beneficiaries				
	Qualifying Providers		Non-qualifying Providers	
	E&M Services	E&M Visits	E&M Services	E&M Visits
	(1)	(2)	(3)	(4)
2013*Dual	0.731	0.481	0.053	0.048
	(0.069)	(0.033)	(0.010)	(0.007)
	[0.000]	[0.000]	[0.000]	[0.000]
2014*Dual	1.161	0.574	0.052	0.047
	(0.101)	(0.045)	(0.012)	(0.008)
	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: Difference-in-Differences Across Services from Qualifying vs. Non-qualifying Providers				
	Duals		Nonduals	
	E&M Services	E&M Visits	E&M Services	E&M Visits
	(5)	(6)	(7)	(8)
2013*Qualifying Provider	1.600	0.850	0.990	0.451
	(0.070)	(0.034)	(0.019)	(0.008)
	[0.000]	[0.000]	[0.000]	[0.000]
2014*Qualifying Provider	3.742	1.577	2.765	1.115
	(0.113)	(0.049)	(0.042)	(0.016)
	[0.000]	[0.000]	[0.000]	[0.000]
Panel C: Triple Differences				
	E&M Services	E&M Visits		
	(9)	(10)		
2013*Dual*Qualifying Provider	0.610	0.399		
	(0.069)	(0.033)		
	[0.000]	[0.000]		
2014*Dual*Qualifying Provider	0.977	0.462		
	(0.100)	(0.043)		
	[0.000]	[0.000]		

Note: This table displays estimates of regression coefficients from Equations (1)-(3) as noted in the panel heading. Standard errors (clustered at the county level) are reported in parentheses and p-values are reported in brackets. Outcome variables are listed at the top of each panel. See text for further details.

Table 5: Alternative Specifications and Samples

	Alternative Control Variables			Unbalanced Panel			Medicaid-Reporting States		
	E&M Services	E&M Visits	Any E&M	E&M Services	E&M Visits	Any E&M	E&M Services	E&M Visits	Any E&M
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2010*Dual	-0.165	0.010	-0.002	-0.629	0.088	0.009	0.107	0.156	-0.002
	(0.078)	(0.039)	(0.001)	(0.114)	(0.043)	(0.001)	(0.138)	(0.101)	(0.001)
	[0.035]	[0.800]	[0.034]	[0.000]	[0.038]	[0.000]	[0.437]	[0.125]	[0.087]
2011* Dual	-0.103	-0.005	0.000	0.097	0.276	0.008	-0.028	0.057	-0.001
	(0.061)	(0.024)	(0.001)	(0.064)	(0.028)	(0.001)	(0.105)	(0.053)	(0.001)
	[0.089]	[0.824]	[0.861]	[0.132]	[0.000]	[0.000]	[0.789]	[0.283]	[0.488]
2013* Dual	0.826	0.566	0.009	1.450	0.940	0.010	0.887	0.671	0.011
	(0.068)	(0.034)	(0.001)	(0.088)	(0.041)	(0.001)	(0.134)	(0.085)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
2014* Dual	1.301	0.698	0.009	2.048	1.143	0.009	1.271	0.738	0.011
	(0.091)	(0.044)	(0.001)	(0.113)	(0.055)	(0.001)	(0.145)	(0.106)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean	17.45	10.1	0.896	24.84	13.4	0.897	18.90	11.67	0.886
Dual Mean at Qualifying Providers	16.97	9.64		22.37	11.59		16.70	9.83	
Baseline Controls							X	X	X
County x Year Interaction	X	X	X						
Individual FE				X	X	X			
Sample	Balanced	Balanced	Balanced	Unbalanced	Unbalanced	Unbalanced	Balanced	Balanced	Balanced

Note: This table displays estimates of regression coefficients from Equation (1) across alternative specifications and samples. Standard errors (clustered at the county level) are reported in parentheses and p-values are reported in brackets. Outcome variables are listed in the top row. See text for further details.

Table 6: Implied Elasticities

	Based on Estimated Change in...			
	Payment Rate		Payment Rate | Submission	
	Estimate	[95% CI]	Estimate	[95% CI]
Payment Change	(1)		(2)	
PP Change Relative to Full Payment Rate	4.71	[4.17, 5.27]	4.59	[3.89, 5.21]
% Change in...				
Total Payments (Medicare + Medicaid)	6.51	[5.76, 7.29]	5.50	[4.58, 6.32]
Medicaid Payments	86.06	[71.98, 102.32]	28.11	[20.55, 35.91]
E&M services elasticity with respect to...				
% Change in Total Payments (Medicare + Medicaid)	1.18	[0.94, 1.47]	1.41	[1.06, 1.86]
% Change in Medicaid Payments	0.09	[0.07, 0.12]	0.28	[0.19, 0.40]
10pp Change Relative to Full Payment Rate	0.16	[0.13, 0.20]	0.17	[0.13, 0.22]
E&M visits elasticity with respect to...				
% Change in Total Payments (Medicare + Medicaid)	1.18	[0.89, 1.50]	1.41	[0.92, 1.95]
% Change in Medicaid Payments	0.09	[0.07, 0.12]	0.29	[0.16, 0.44]
10pp Change Relative to Full Payment Rate	0.16	[0.12, 0.21]	0.17	[0.12, 0.23]

Notes: This table presents implied elasticities and the associated bootstrapped 95 percent confidence intervals as described in the text. Changes in the total payments are calculated using the fact that Medicare's cost-sharing accounts for 33% of the full payment rate, according to estimates in Table 1. Column 1 presents estimates and confidence intervals based on changes in actual payments to providers, accounting for incomplete claim submission. Column 2 presents estimates and confidence intervals based on payments conditional on submission. See text for further details.

Table 7: Mechanisms

	Established E&M Visits	New Patient E&M Visits	Billed Provider Time	Billed Provider Time per Visit	RVUs per Visit
	(1)	(2)	(3)	(4)	(5)
2010*Dual	-0.016	0.051	-0.817	-0.028	-0.002
	(0.041)	(0.007)	(1.393)	(0.035)	(0.002)
	[0.692]	[0.000]	[0.421]	[0.234]	[0.013]
2011*Dual	0.004	0.017	0.028	0.143	0.007
	(0.024)	(0.003)	(1.007)	(0.027)	(0.002)
	[0.855]	[0.000]	[0.699]	[0.298]	[0.548]
2013*Dual	0.553	-0.020	12.620	-0.328	-0.017
	(0.034)	(0.003)	(1.178)	(0.032)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
2014*Dual	0.659	-0.037	19.100	-0.397	-0.020
	(0.045)	(0.005)	(1.728)	(0.045)	(0.003)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean	10.67	0.768	332.2	27.01	1.55

Notes: This table displays estimates of regression coefficients from Equation (1) for outcomes listed in the first row. Standard errors (clustered at the county level) are reported in parentheses and p-values are reported in brackets. See text for further details.

Table 8: Heterogeneity

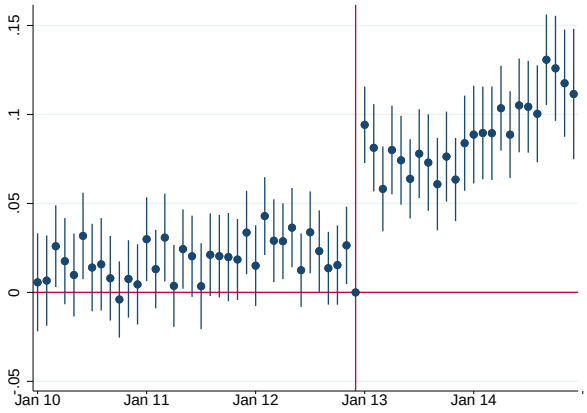
	Female	Male	Age >= 75	Age < 75	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)
2013*Dual	0.748	0.924	0.411	1.154 †††	0.731	0.033 †††
	(0.079)	(0.125)	(0.102)	(0.095)	(0.123)	(0.263)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.900]
2014 * Dual	1.378	1.135	1.039	1.916 †††	0.963	0.240
	(0.104)	(0.181)	(0.148)	(0.118)	(0.144)	(0.241)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.318]
Dual Pre-Policy Mean	20.65	17.05	20.76	18.72	18.38	15.78

	HPSA	Non-HPSA	Worse Health	Better Health	Preventable ED	No Preventable ED
	(7)	(8)	(9)	(10)	(11)	(12)
2013*Dual	0.555	1.002 †††	0.527	0.927	0.658	0.901
	(0.103)	(0.094)	(0.226)	(0.069)	(0.361)	(0.095)
	[0.000]	[0.000]	[0.020]	[0.000]	0.069	0.000
2014*Dual	0.987	1.417 ††	1.337	1.065	1.541	1.490
	(0.179)	(0.122)	(0.181)	(0.077)	(0.467)	(0.162)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]
Dual Pre-Policy Mean	18.99	19.54	19.40	6.276	28.07	13.75

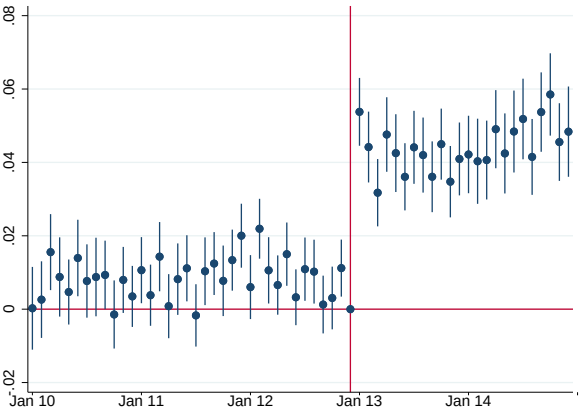
Notes: This table displays estimates of regression coefficients from Equation (1) for subgroups defined in the top row. The outcome variable is E&M services. Standard errors (clustered at the county level) are reported in parentheses and p-values are reported in brackets. The symbol † indicates the significance level of a test that the coefficients are the same across each paired group. The significance levels denoted are: †=0.10, ††=0.05, † † †=0.01. See text for further details.

Appendix

Figure A.1: Impact on Monthly E&M Services and Visits: Triple Differences Specification



(a) E&M Services



(b) E&M Visits

Notes: These figures report coefficient estimates and 95 percent confidence intervals from the monthly analog to Equation (3) of the interaction between month fixed effects, the indicator $Dual_i$ equals 1, and the indicator that the provider is qualifying. The dependent variable is E&M services in Panel (a) and E&M visits in panel (b). See text for further details.

Table A.1: First Stage: Submissions and Payment of Cost-Sharing Claims, Confidence Intervals from Wild Bootstrap with State Clusters

	(1)	(2)	(3)
Panel A: no controls	Cost-Sharing Payment Rate | Submission	Submissions	Cost-Sharing Payment Rate
2011*Qualifying Provider	0.0277	-0.0093	0.0021
	(-0.115, 0.222)	(-0.018, 0.002)	(-0.023, 0.023)
	[0.615]	[0.083]	[0.955]
2013*Qualifying Provider	0.196	0.124	0.144
	(0.020, .428)	(0.040, 0.209)	(0.071, 0.224)
	[0.023]	[0.003]	[0.009]
Panel B: baseline controls			
2011*Qualifying Provider	0.0264	-0.0043	0.0046
	(-0.072, 0.230)	(-0.008, 0.001)	(-0.017, 0.024)
	[0.842]	[0.086]	[0.905]
2013*Qualifying Provider	0.129	0.118	0.143
	(0.019, 0.362)	(0.041, 0.205)	(0.085, 0.222)
	[0.019]	[0.007]	[0.009]
Dual Pre-Policy Mean at Qualifying Providers	0.4923	0.3629	0.1822

Notes: This table displays estimates of regression coefficients from Equation (2) for the sample of Medicare E&M claims for dual-eligible beneficiaries in Medicaid-reporting states, with each person-year observation weighted by the number of E&M claims represented. Confidence intervals (parentheses) and p-values (brackets) are derived from a wild bootstrap with state clusters. Outcome variables are listed in the top row. Panel A includes no controls, while Panel B includes the "baseline" controls as in Table 3: age bin, sex, and county.

Table A.2: Impact of the Payment Increase on E&M, State Level Clustering and Alternative Group Definitions

	Baseline with State Clusters			"Near Poor" Control Group		
	E&M Services	E&M Visits	Any E&M	E&M Services	E&M Visits	Any E&M
	(1)	(2)	(3)	(4)	(5)	(6)
2010*Dual	-0.095	0.035	-0.002	-0.384	-0.201	-0.004
	(0.073)	(0.034)	(0.001)	(0.105)	(0.051)	(0.001)
	[0.199]	[0.315]	[0.035]	[0.000]	[0.000]	[0.001]
2011*Dual	-0.048	0.021	-0.000	-0.109	-0.044	-0.003
	(0.057)	(0.023)	(0.001)	(0.092)	(0.036)	(0.001)
	[0.410]	[0.375]	[0.950]	[0.239]	[0.230]	[0.016]
2013*Dual	0.788	0.533	0.009	0.744	0.463	0.003
	(0.120)	(0.080)	(0.001)	(0.102)	(0.043)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.007]
2014*Dual	1.211	0.622	0.009	0.831	0.438	0.001
	(0.151)	(0.102)	(0.001)	(0.149)	(0.060)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.521]
Dual Pre-Policy Mean	17.45	10.1	0.896	17.45	10.1	0.896
N	17,698,378	17,698,378	17,698,378	2,190,093	2,190,093	2,190,093

Notes: This table displays estimates of regression coefficients from Equation (1). In columns (1)–(3), we use the baseline model but cluster standard errors at the state rather than county level. In columns (4)–(6), we define our control group to be "near poor" Medicare beneficiaries (Low-Income Subsidy recipients and partial duals) instead of all nonduals. Standard errors are reported in parentheses and p-values are reported in brackets. See text for further details.

Table A.3: Gap in Fraction of Duals vs. Nonduals with Any E&M Visit, 2010-2012

	Unconditional	Chronic Illness and County/Age Bin FE
	(1)	(2)
Dual-Nondual Difference	-0.004	-0.011
	(0.003)	(0.001)
	[0.263]	[0.000]

Note: This table displays regression-adjusted estimates of the difference in the fraction of duals vs. nonduals reporting any E&M visits during 2010-2012. The first column presents unconditional differences across duals and non-duals in the probability of having at least one E&M visit. The second column presents regression-adjusted differences across these groups that control for the 18 chronic disease indicators included in the Charlson Index, county fixed effects, and 5-year age bin fixed effects. Standard errors (clustered at the county level) are reported in parentheses and p-values are reported in brackets. See text for further details.