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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. The provider payment reform also decreased the fraction of low-income beneficiaries with no primary care visit in a year by 9%. 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

Public health insurance programs covering low-income populations commonly shield individuals from out-of-pocket costs for health care. However, as part of cost-saving measures, health care providers may receive lower payments for delivering services to patients covered by these programs than they receive for care provided to other patients. These differences in provider payments could be a source of disparities in utilization and health outcomes that are frequently seen between low-income and higher-income individuals. 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 what drives these differences. Are these differences driven by differing health needs, income, information, or preferences between higher- and lower-income beneficiaries? Or are these differences driven by the structure of the public health insurance programs that cover these individuals—and the fact that providers are often paid less to treat low-income Medicare beneficiaries?

Leveraging variation arising from a large provider payment increase, this paper investigates the role of provider payments in driving 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.

The structure of the Medicare and Medicaid programs leads to providers being paid less to provide services to dual-eligible beneficiaries compared to the same services provided to 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.

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 cost-sharing, raising the share of Medicare cost-sharing paid by Medicaid by 14 percentage points. Consequently, total payments to providers for E&M services increased by 6.5% when summed across Medicare and Medicaid, with this increase isolated to care provided to duals by 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 1.21 annually per beneficiary [95% CI: 1.01 to 1.42], 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 0.72], which is a 5.5% increase relative to the baseline mean. 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.

We also find that the payment increase results in an 8.7% reduction in the percent of dual beneficiaries who have no E&M visits in a year. This change is notable because prior to the payment policy, duals were less likely to have any E&M visit than non-duals. The payment policy change was successful in completely reversing this gap. Because groups such as nonwhites are disproportionately represented in the dual population, we might have expected the policy to narrow white-nonwhite disparities in access to care. However, the policy actually had negligible impacts in the white-nonwhite gap. Similarly, the policy did not close geographic disparities in access to care between individuals living in Health Professional Shortage Areas (HPSAs).

Leveraging differences across providers who qualified and who did not qualify for the payment increase, we investigate the robustness of our findings and quantify potential spillovers on non-targeted beneficiaries or providers. 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. Services provided by qualifying providers relative to those provided by non-qualifying providers trended similarly for duals and nonduals before the payment increase, but 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. Moreover, these findings indicate that the policy did not increase E&M for non-targeted beneficiaries (nonduals) or non-qualifying providers, suggesting that spillovers from the policy were small or nonexistent.

Beyond estimating the mean effect of this reform, we explore heterogeneity and mechanisms. To explore heterogeneity, we re-estimate our difference-in-differences specification for subgroups defined by demographics and baseline health. We find the effects of the reform are nearly universal, affecting many subgroups we analyze. However, the effects are substantially larger among younger beneficiaries as compared to those aged 75 and older, among white beneficiaries, and among those not residing in HPSAs. In addition, while the effects in levels are similar among those with differing baseline health, 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 through increasing visits with 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, which represents an increase of 5.7% relative to the baseline mean of about 330 minutes per year. We also examine impacts on resources per visit and find the reform decreased billed provider time per visit by 0.39 minutes (or 1.4% 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"[1]. Our results provide direct evidence that providing unequal payments for identical services rendered to low-income versus high-income individuals leads fewer services rendered to low-income individuals. 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 some gaps in access.

This paper contributes to several strands of literature. First, our study contributes to a growing literature on supplier responses to incentives within the Medicare program and health insurance more broadly (Finkelstein, 2007; Cabral, Geruso and Mahoney, 2018; Duggan, Starc and Vabson, 2016; Dafny, 2005; Einav, Finkelstein and Mahoney, 2018; Eliason et al., 2018; Gross et al., 2022).[2] 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. Our study provides novel evidence on the impact of physician payments for E&M services—services that are crucial for managing patient care and are generated via provider time, one of the few inherently scarce inputs in health care production. Further, our estimates indicate that increased provider payments induced large increases in utilization among targeted beneficiaries, with no meaningful spillovers on non-targeted patients or providers. Thus, our findings demonstrate that providers have the capacity to respond to payment policy at a granular level, and that the two-tiered reimbursement system for lower-income and higher-income

[1] Wilbur Cohen, as quoted in Porter (2015).

[2] Our work is also related to prior work on physician payments in other contexts beyond the US (e.g., Brekke et al. (2017, 2020); Werbeck, Wübker and Ziebarth (2021)).

Medicare beneficiaries is an important factor in explaining disparities in access across these groups.

Second, this paper also contributes to a literature on the impact of health insurance program design on the supply of health care services to low-income populations and those covered by Medicaid in particular. This study complements prior work on provider responses to Medicaid coverage expansions (Garthwaite, 2012; Buchmueller, Miller and Vujicic, 2016; Baker and Royalty, 2000). Our paper also 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, preferences, and many others). 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 (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, we are aware of only two recent studies 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, in part because of a lack of evidence on the first-stage effect of the provider payment variation they exploit, as well as limited evidence assessing internal validity. Our study moves this nascent literature forward by using linked administrative data from Medicare and Medicaid to directly observe provider payments and demonstrate that our research design isolates sharp, plausibly exogenous variation in provider payments—a critical fact to establish given the complex interactions of the two programs. Our use of these linked administrative panel data enables us to obtain precise estimates of the effect of the reform, and furthermore to implement multiple identification strategies and demonstrate the sharp timing of effects in the monthly time series. Thus, beyond providing new evidence on the effect of provider payments for duals, this paper also provides some of the most comprehensive and transparent evidence to date on the importance of provider payments on health care utilization among low-income populations more generally.

Third, our findings highlight the importance of provider payments as a 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 correlational 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 demonstrating the effects of increasing the generosity of Medicaid coverage on the intensive margin.

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.[3] 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.

[3]While coverage for duals is less generous in terms of provider payments, it is more generous in terms of patient cost-sharing. Given these opposing forces, it is ex ante ambiguous whether being dually eligible improves or reduces access to care relative to standard Medicare coverage. In recent work, Li (2022) finds that primary care decreases when Medicare beneficiaries become dually eligible for Medicaid, suggesting that on net primary care access may be worse for duals than nonduals.

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.[4]

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, 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)).[5]

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

[4]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.

[5]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.

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.[6] The ACA payment increase affected provider reimbursement rates for care provided in 2013 and 2014.[7] The payment increase applied to care provided to any Medicaid beneficiary, whether the care was covered through fee-for-service Medicaid or Medicaid managed care, though we note that Medicaid managed care played a very limited role in the dual-eligible population during this period.[8]

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).

[6]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).

[7]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 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.

[8]Medicaid managed care is very limited among duals—both in terms of enrollment and in terms of the services it covers. In 2011, only 10% of duals (8% in our Medicaid-claims-reporting states) were enrolled in a "comprehensive" managed care contract that might plausibly have covered E&M services (Centers for Medicare and Medicaid Services, 2011). Thus, for 90% of the duals we consider, claims for Medicare cost-sharing were likely being processed directly by the state fee-for-service Medicaid program. Further, enrollment in Medicaid managed care does not appear to affect our ability to measure the first-stage change in payments. We find comparable levels of E&M claims in the Medicaid claims data we leverage regardless of the state's use of Medicaid managed care contracts for duals. Specifically, when we examine payment for cost-sharing claims in the three Medicaid-claims-reporting states (AZ, MN, and OR) with substantial (more than one-third) managed care enrollment among duals, we found that submission and payment of cost-sharing claims was similar regardless of individual-level managed care enrollment.

Third, the ACA payment policy may also have indirectly affected payments providers receive, as higher payment rates for submitted claims may have encouraged providers to submit claims. 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 and validate the sources of variation we leverage for identification. 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.

We note that this payment change was likely salient to providers. There was extensive outreach to providers to advertise the temporary increase in payments for E&M services provided to Medicaid patients—including outreach from the federal Centers for Medicare and Medicaid Services (CMS) (e.g, Centers for Medicare and Medicaid Services (2012)) and from state Medicaid agencies (e.g, New York State Department of Health (2012), Division of TennCare (2012)). Moreover, our findings in Section 4 suggest providers were informed about the payment change, with our results indicating that providers increase their provision of E&M services and their submission of E&M cost-sharing claims in the first month the payment change went into effect.

Our difference-in-differences analysis uncovers the response of providers to the specific policy we study—the ACA Medicaid payment reform. 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 context being considered. For example, because this was a temporary two-year change in payment policy, our estimates may not reflect the long-run effect of a hypothetical permanent change in provider payments. However, in Section 4 we demonstrate that the impact of the reform on payments and utilization appears immediately and is fairly stable for the two years the payment change was in effect, suggesting that these estimates could have broader relevance for understanding the impacts of longer-term payment policy. Additionally, 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. 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.[9]

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 targeted 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 how the policy impacted 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

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

estimates of the change in payments using a number of different measures of payments that may be 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 (Centers for Medicare and Medicaid Services, 2016*a*,*b*,*c*,*d*). 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 (Centers for Medicare and Medicaid Services, 2019). 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.[10] 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.

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 the share of Medicare E&M claims with a matching Medicaid claim; we refer to these claims

[10]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).

as "submitted".[11] 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 the direct effect of the policy (i.e. submitted claims receive higher payments) and indirect effect of the policy (e.g., increased likelihood of providers submitting claims).

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.[12] 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, 2017). 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.(Cabral, Carey and Miller, 2023) 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.

One-fourth of providers who bill services to Medicare in the years 2010-2014 are in a qualifying

[11]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.

[12]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.

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.[13] 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 (Manitoba Centre for Health Policy, 2014). 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.[14] 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 (New York University Center for Health and Public Service Research, 2018). 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 (HPSAs) for primary care practitioners—counties with relatively few primary care providers per capita (Health Resources and Services Administration, 2017; Carey, Miller and Wherry, 2020*b*).[15]

[13]We construct the time variable by following the methodology in Fang and Gong (2017) (Cabral, Carey and Miller, 2023). 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).

[14]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.

[15]The 2010 HPSA designations for each county are drawn from the 2015-2016 Area Health Resource File.

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.[16] Our core sample reflects a balanced panel of 339,899 duals and 3,201,161 nonduals over the years 2010-2014.

We next provide a description of our sample along the key dimensions of heterogeneity we consider below. Duals are more likely to be female and non-white, and are on average 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 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.[17] Consistent with prior work based on data from 2009 (Haber et al., 2014) and recent work about the administrative burden of Medicaid reimbursement 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

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/`.

[16]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).

[17]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.

both incomplete submission and the fact that only 44-49% 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 controls (X_{it}): age (in five-year bins), sex, and county fixed effects. 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 prominence of local circumstances in affecting medical care (Finkelstein, Gentzkow and Williams, 2016) and the county 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 Figure 5).

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. 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 likely 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:

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

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. Note if the triple difference coefficients δ_t are similar to the coefficients described in the main estimating equation (1), this similarity would build confidence in the research design as well as suggest that spillovers on ineligible beneficiaries and non-qualifying providers are minimal.

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[18], 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-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

[18]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.

B). Appendix Table A.1 demonstrates our inference is unchanged if we rely on wild bootstrap confidence intervals with state-level clustering rather than the baseline 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 (the direct effect of the policy) and the increase in claim submission (the indirect effect of the policy because of changes in provider behavior). If Medicaid had begun paying out cost-sharing claims at the higher rate but provider claim submission behavior did not change, the resulting increase in actual payments would have been about half the observed increase. We can also consider the effect of the observed increase in submissions on actual payments to providers if the payment conditional on submission had not changed; in that case, the change in submission behavior alone would have increased payments by about 40% of the observed increase in actual payments.

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 6.6% increase in total payments based on actual payments (from column 2) or a 5.4% 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 80% increase in actual payments from Medicaid and a 27% 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 (panels a and c) and reported in Table 3 (columns 1 and 2). 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 1.42 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 5.5%) in 2014 [95% CI: 0.53 to 0.73].

To verify that the precise timing of the impacts aligns with the timing of the payment change, we report monthly difference-in-differences event studies in panels (b) and (d) of Figure 2. 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 aware of any other policy change that went into effect the same month (January 2013) that would have differentially affected duals and nonduals.

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.[19] As reported in Figure 2 Panel (e) (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%).

Analysis Comparing Services from Qualifying and non-Qualifying Providers We further investigate the impact of the payment increase by examining differences across providers who did and did not qualify for the payment increase. Leveraging this additional difference allows us to investigate the robustness of our baseline estimates and to quantify potential spillovers of the policy on non-treated providers and beneficiaries. The top row of Figure 3 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.[20] Coefficient estimates corresponding to those in Figure 3 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 provided to their nondual counterparts, but that these outcomes experienced no similar relative change when examining services provided by non-qualifying providers. This evidence suggests there were no meaningful spillovers on care provided by non-qualifying providers.[21]

[19] We do not consider this extensive-margin outcome in our monthly analyses because of the difficulty in interpreting this variable at a monthly level.

[20] 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.

[21] This analysis could potentially be affected by the Primary Care Incentive Program (PCIP), which increased

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 3 (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, Panels (c) and (d) of Figure 3 indicate that care provided to nonduals at qualifying and non-qualifying providers trended similarly before and after the reform is implemented, suggesting there were no obvious spillovers from this reform on standard (nondual) Medicare beneficiaries.

Finally, we can combine all three differences leveraged within Figure 3 to estimate the triple differences model described in Equation (3). The estimates of the three way interaction terms are plotted in Figure 4, 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 0.546 in 2013 [95% CI 0.41 to 0.68] and 0.877 in 2014 [95% CI: 0.68 to 1.07] as a result of the payment change, and E&M visits increased by 0.350 in 2013 [95% CI: 0.29 to 0.41] and 0.394 in 2014 [95% CI: 0.32 to 0.47]. 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/Samples We next return to our baseline dual-nondual comparison and investigate the robustness of these results to alternative specifications and sample restrictions. Figure 5 displays this robustness analysis with the associated coefficient estimates and standard errors reported in Appendix Table A.2. First, we show that inference is similar if we cluster the standard errors at the state rather than county level, confirming that

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 3 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.

our results are not sensitive to this choice. Second, we demonstrate the estimates are similar when including county by year fixed effects instead 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 by both space and time. For instance, this provides reassurance that the results are not influenced by state-level policy changes over this time—e.g., state decisions to expand Medicaid among non-elderly (Medicaid-only) beneficiaries. Moreover, this illustrates the results are not driven by any federal policy changes over this time period that may have differentially impacted different counties—e.g., any changes in federal geographic-based payment policy. Finally, we 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.[22]

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 Implied Elasticities

We calculate implied elasticities based on our estimates and discuss how these compare with elasticities estimated in prior work. 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 Appendix Table A.2), though we obtain similar elasticities when using our baseline (nationwide) utilization estimates.

Table 5 displays elasticities based on our estimates and the associated bootstrapped 95% confidence intervals.[23] Given that claim submission is far from complete, it is natural to calculate

[22]We also considered an alternative comparison group: the subset of Medicare beneficiaries who are "near poor" but still subject to typical Medicare cost-sharing. Following Roberts and Desai (2021) and Fung et al. (2021), we identify "near poor" by eligibility for "partial" Medicaid benefits (excluding cost-sharing) and the Part D Low-Income Subsidy. We report results of this alternative control group in Appendix Table A.3, finding post-period coefficients similar to our baseline results, but some evidence for non-parallel pre-period trends.

[23]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

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 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 6.6% increase in total actual payments and a 5.4% increase in total payments conditional on submission. Based on the utilization estimates described above in Appendix Table A.2 columns 7 and 8, the reform caused a 7.7% increase E&M services and a 7.6% increase 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: 0.90 to 1.55] and E&M visits [95% CI: 0.82 to 1.56]. The elasticity in terms of total payments conditional on submission is 1.4 for both E&M services [95% CI: 0.99 to 1.93] and E&M visits [95% CI: 0.84 to 2.08].

For comparison, Table 5 also presents elasticities with respect to Medicaid payments, rather than total payments. The payment reform caused an 80% increase in actual Medicaid payments and a 27% 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: 0.06 to 0.13] with respect to actual Medicaid payments and 0.28 for E&M services [95% CI: 0.17 to 0.43] and and E&M visits [95% CI: 0.14 to 0.48] with respect to Medicaid payments conditional on submission.

Finally, Table 5 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 4.8 percentage points based on actual payments and 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 0.21] and in E&M visits [95% CI: 0.11 to 0.22] based on actual payments and would also lead to a 16% increase in E&M services [95% CI: 0.12 to 0.22] and E&M visits [95% CI: 0.10 to 0.24] 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

form an elasticity estimate. We generate 95% confidence intervals by taking the estimates at the 2.5th and 97.5th percentiles of this distribution.

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.[24] Thus, a 1% increase 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).

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). 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.[25] Importantly, our findings suggest that payment policy targeting a subgroup of Medicare beneficiaries—duals who comprise 20% of all Medicare beneficiaries—can have a large impact on services provided, even among services that are tightly linked to physician labor supply and are generally thought to 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.[26] 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. Note first that the vast majority (93%) of E&M visits at baseline for duals were

[24] We represent Medicare's estimates of the share of inputs due to provider time as the share of total RVUs (relative value units) for services that are due to the provider's own work.

[25] 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: 0.90 to 1.55] 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).

[26] While the discussion below focuses on analysis of annual outcomes, monthly analysis reported in Figure A.2 reveals the same patterns and illustrates that the impacts appear in the first month the policy is implemented.

established patient visits—where the beneficiary has some existing relationship with the provider. Comparing Panels (a) and (b) in Figure 6, and the corresponding columns 1 and 2 in Appendix Table A.4, 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 with existing providers to obtain care more often from these providers—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. 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 Appendix Table A.4 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 23 minutes or smaller than 16 minutes.

A natural question to consider is whether this additional time spent with dual patients resulted in a reduction of time spent with nondual patients (i.e., negative spillover effects). For instance, we might expect to see such negative spillovers if providers are capacity constrained. However, as we demonstrated in Figure 3, 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 Appendix Table A.4 columns 4 and 5 display these results. Both of 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*a*; Neprash et al., 2021).

measures slightly declined when payments increased: billed provider time per visit declined by 0.39 minutes (1.5% of the baseline mean) and RVUs per visit declined by 0.02 (1.3% of the baseline mean), suggesting the marginal visit was less resource-intensive. 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, geographic characteristics, and pre-policy measures of health, with estimates summarizing these results reported in Appendix Table A.5.[29] Panel (a) 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 panel 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 18.72 for these 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. Panel (c) reports the impacts of the payment increase separately for white beneficiaries and non-white beneficiaries. The effects are somewhat larger among white beneficiaries. The payment change increases utilization among white beneficiaries by 1.298 RVUs per person annually, or 6.8% of the baseline mean. The estimated increase for non-white beneficiaries is smaller—0.958 RVUs (or 4.9% of the baseline mean).

Panel (d) 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

[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] Results for E&M visits and any E&M claim are reported in Appendix Tables A.6 and A.7 respectively.

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 in panels (e) and (f). 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. In panel (e) of Figure 7, 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 somewhat larger among patients in worse baseline health. However, because baseline utilization is much higher among individuals in worse health, the effects are smaller on a percent basis within this subgroup: patients in worse baseline health (with a pre-period Charlson Index ≥ 2) experience a 5.7% increase in utilization, while patients in better baseline health (with a lower pre-period Charlson Index) experience a 11.3% increase in utilization.

Panel (f) of Figure 7 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 estimates are larger among those with a prior avoidable ED visit, although we observe some non-parallel pre-policy trends for this group. However, baseline utilization is very different for those with and without a preventable ED visit in the pre-period. Thus, the estimates are larger relative to the baseline mean among those with no preventable ED visit (7.9% vs. 6.9%).

These heterogeneity analyses reveal the extent to which payment policy can and cannot alleviate observed disparities in access to care. As seen in Table 1, prior to the payment increase, dual eligible beneficiaries were somewhat less likely to have any E&M related visit with a provider despite their observably worse health. By increasing provider fees, the policy was able to completely close and indeed reverse this gap in utilization. At the same time, the policy was less effective at closing other policy-relevant disparities, such as those observed between white and non-white beneficiaries or between beneficiaries living in or not living in HPSAs. Prior to the payment reform, in our sample, white beneficiaries were 6.1 percentage points more likely to have any E&M visit than non-white beneficiaries and non-HPSA residents were 2.0 percentage points more likely to have an E&M visit than HPSA residents. Both non-white beneficiaries and HPSA-residing beneficiaries are over represented among the dual-eligible, so it therefore may be natural to expect that a policy targeting duals could help meaningfully address these disparities. However, we calculate that the payment policy closed the white/non-white gap by only about 0.8 percent and actually slightly *increased* the gap between HPSA and non-HPSA residents.[31] The ineffectiveness of this

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

[31] In our sample, 29% of non-white beneficiaries are duals and 6.9% of white beneficiaries are duals. The policy

policy at addressing these other important disparities reflects the fact that the payment increase had a larger effect on white and non-HPSA resident dual beneficiaries (see Appendix Table A.7). Furthermore, dual beneficiaries make up a relatively small share these groups and disparities by race and geographic location are present even among the non-dual population, limiting the ability of a policy targeting duals alone to ameliorate these gaps.

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. We illustrate that our 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 the increase in utilization induced by the payment 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. As a result of these heterogeneous impacts, the payment policy did little to address gaps in having any primary care utilization between white and non-white beneficiaries and indeed worsened gaps between beneficiaries living in provider-shortage areas compared to those living in other areas, even though it was successful at closing and indeed *reversing* this gap between dual and non-dual beneficiaries.

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

increased the fraction of non-white duals with any E&M visit by about 0.4 percentage points and of white duals by about 1 percentage point (see Appendix Table A.7). Applying our result that there were minimal spillovers to non-duals, the policy therefore increased the fraction of non-white beneficiaries with any visit by about 0.11pp and white beneficiaries by 0.07pp, decreasing the gap by about 0.05pp, about a 0.8% change compared to the 6pp gap. Analogous calculations were conducted for the HPSA and non-HPSA residing beneficiaries.

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 the same services provided to higher-income beneficiaries. Our research suggests that reducing gaps in provider payments may work to close gaps in health care access between low- and higher-income Medicare beneficiaries. More broadly, our findings suggest targeted increases in provider payments may be a promising policy tool to address socio-economic disparities in health care access and health in other settings.

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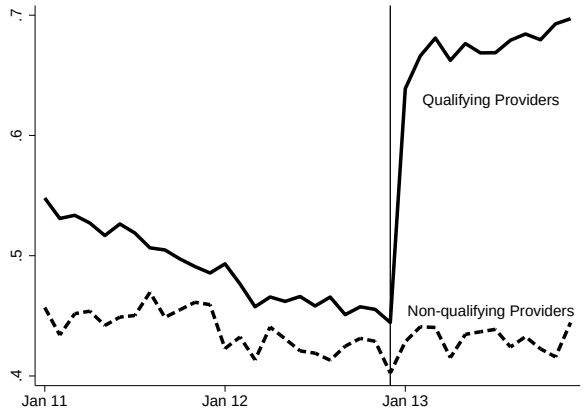
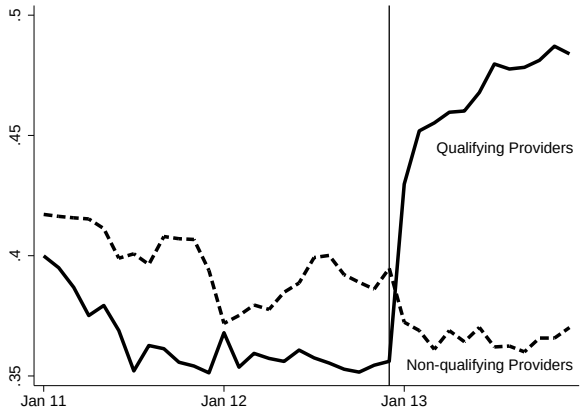
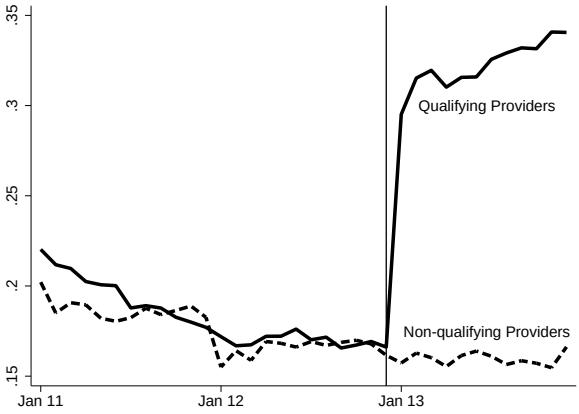
Figure 1: First Stage: Submission and Payment of Cost-Sharing Claims



(a) Cost-Sharing Payment Rate | Submission



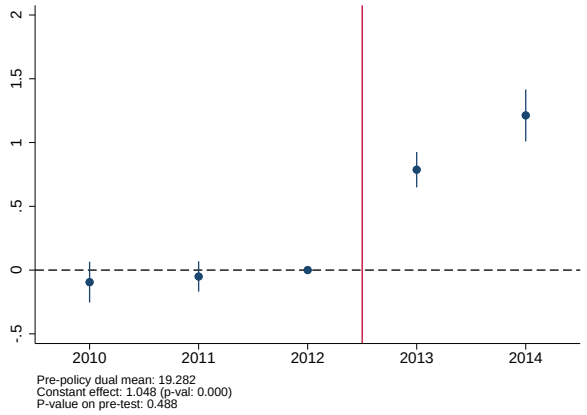
(b) Submissions



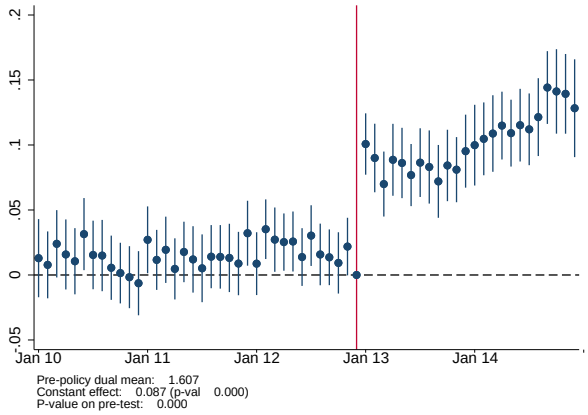
(c) Cost-Sharing Payment Rate

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.

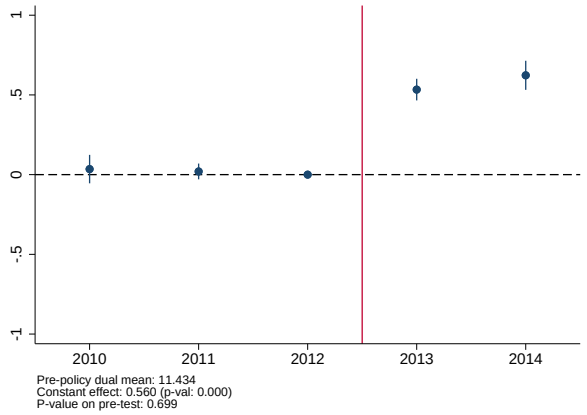
Figure 2: Impact on E&M Services and Visits: Baseline Difference-in-Differences (dual vs. nondual)



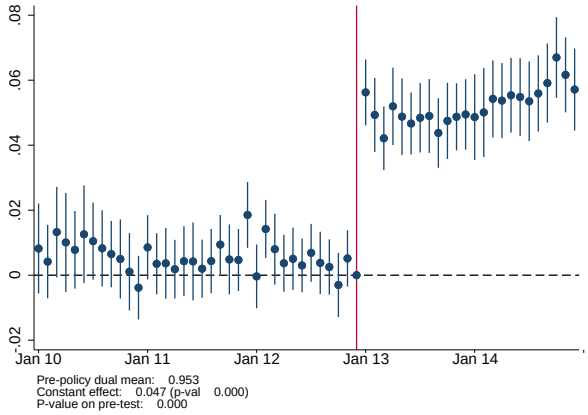
(a) E&M Services, annual



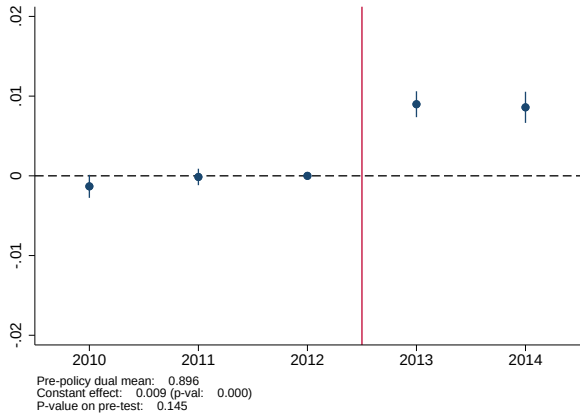
(b) E&M Services, monthly



(c) E&M Visits, annual



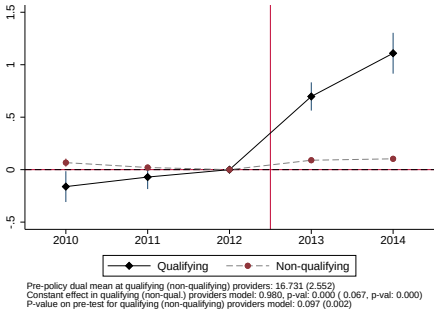
(d) E&M Visits, monthly



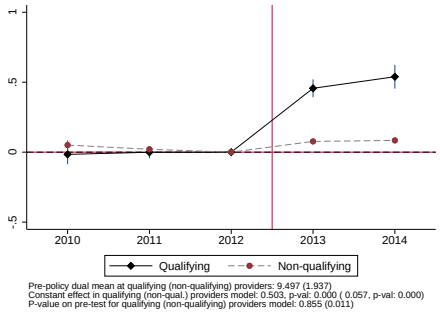
(e) Any E&M Visit, annual

Notes: These figures report coefficient estimates and 95 percent confidence intervals from Equation (1) of the interaction between the indicator $Dual_i$ and year fixed effects (panels (a), (c), and (e)) or month fixed effects (panels (b) and (d)). Panels (b) and (d) reflect seasonal adjustment with month-of-year fixed effects separately for duals and nonduals. "P-value for pre-test" refers to the p-value for an F-test that all pre-policy event study coefficients are jointly equal to zero. See text for further details.

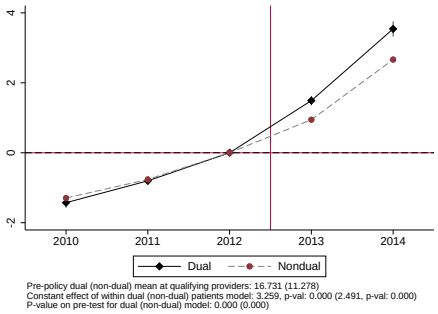
Figure 3: Impact on E&M Services and Visits: Additional Difference-in-Differences Specifications



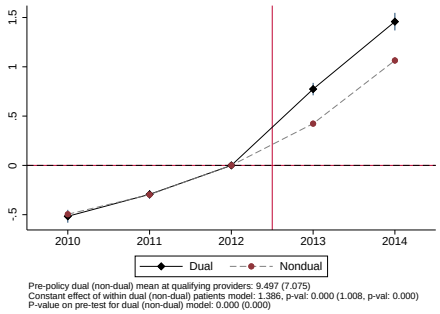
(a) E&M Services: DD dual vs. nondual (by $Qualifying_p$)



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



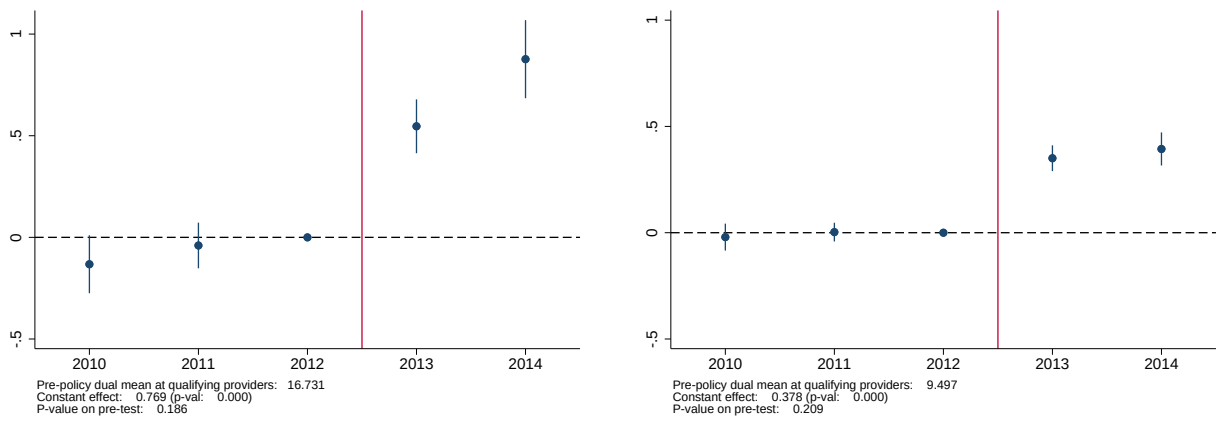
(c) E&M Services: DD qualifying vs. non-qualifying (by $Dual_i$)



(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. "P-value for pre-test" refers to the p-value for an F-test that all pre-policy event study coefficients are jointly equal to zero. See text for further details.

Figure 4: Impact on E&M Services and Visits: Triple Differences specification

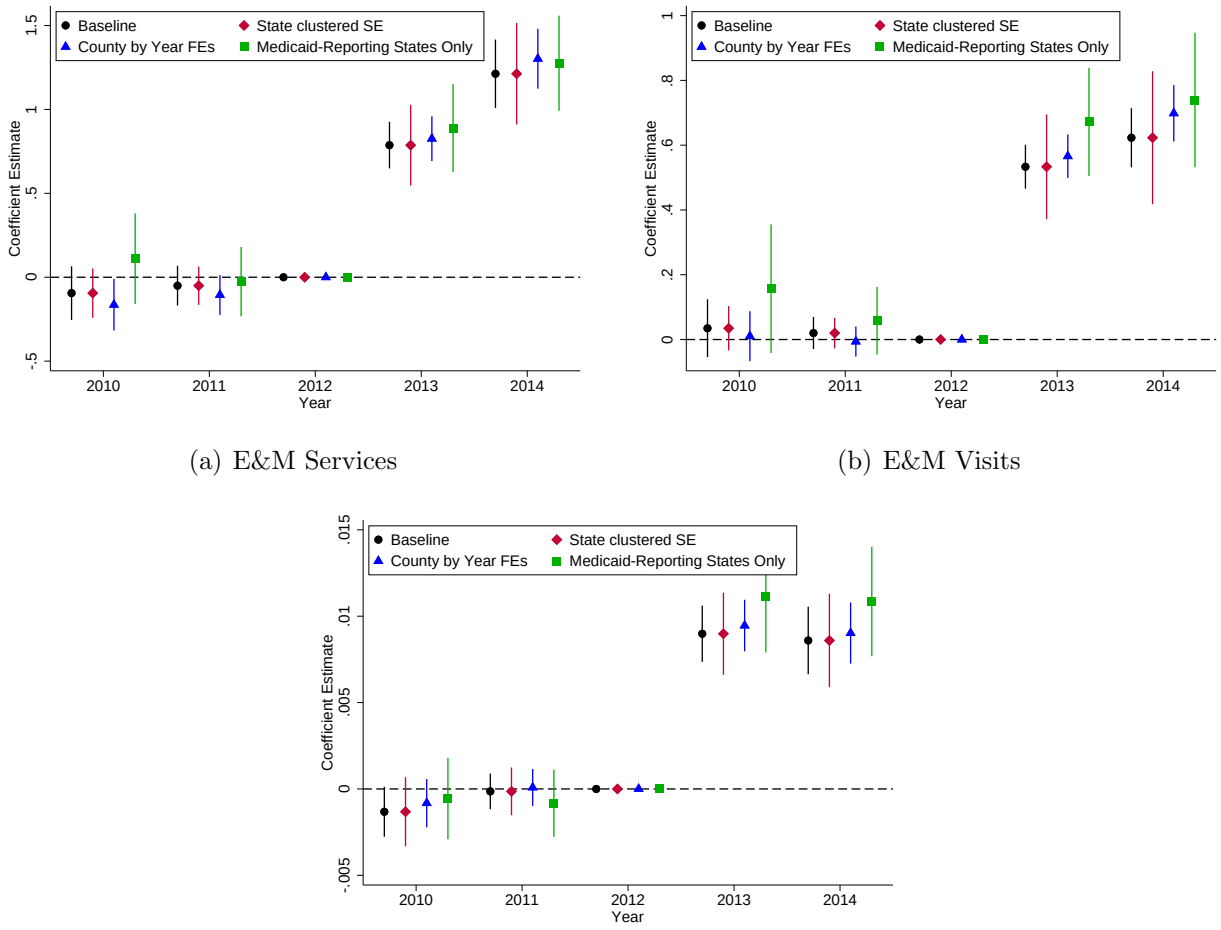


(a) E&M Services

(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. "P-value for pre-test" refers to the p-value for an F-test that all pre-policy event study coefficients are jointly equal to zero. See text for further details.

Figure 5: Impact on E&M Services and Visits: Robustness to Alternative Specifications and Samples



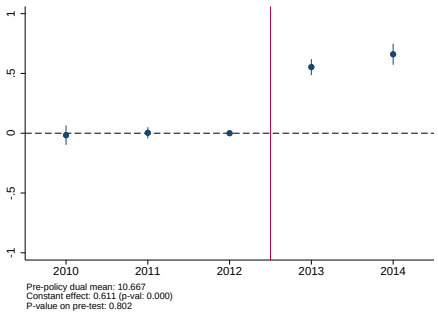
(a) E&M Services

(b) E&M Visits

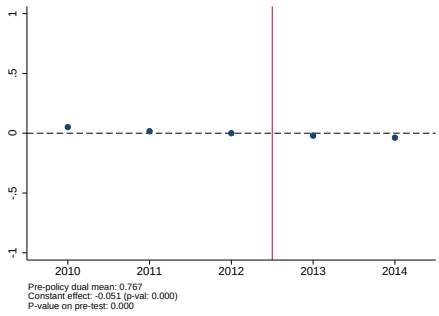
(c) Any E&M Visit

Notes: These figures report coefficient estimates and 95 percent confidence intervals of our baseline estimate (circle) and three alternative estimates that cluster by state (diamond), include county by year fixed effects (triangle), and restrict the sample to only states who report data in the Medicaid claims sample (squares). See text for further details.

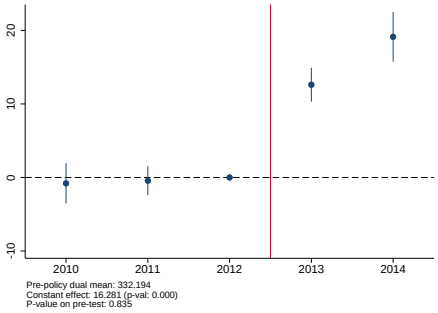
Figure 6: Mechanisms: Impact on Types of Services Provided



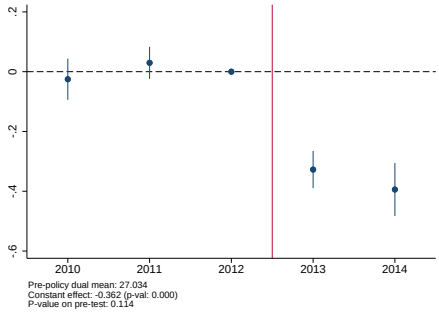
(a) Established Patient E&M Visits



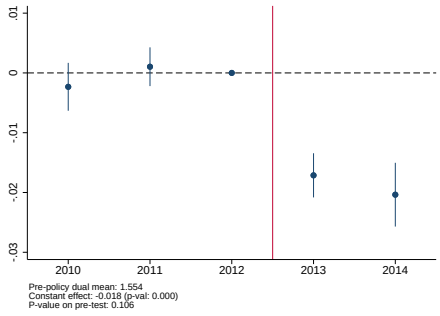
(b) New Patient E&M Visits



(c) Billed E&M Time



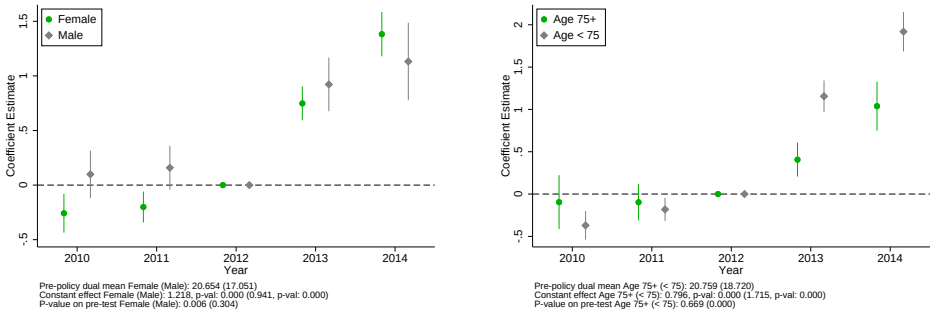
(d) Billed Time per E&M Visit



(e) RVUs per E&M Visit

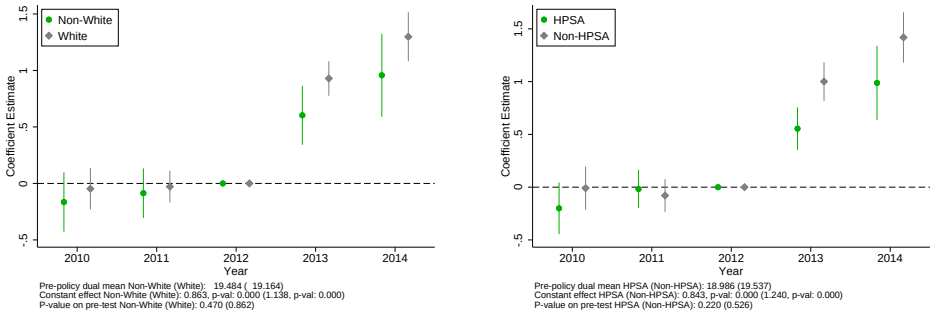
Notes: These figures report coefficient estimates and 95 percent confidence intervals from Equation (1) of the interaction between year fixed effects and the indicator $Dual_i$ equals 1. Each panel is a different dependent variable. "P-value for pre-test" refers to the p-value for an F-test that all pre-policy event study coefficients are jointly equal to zero. See text for further details.

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



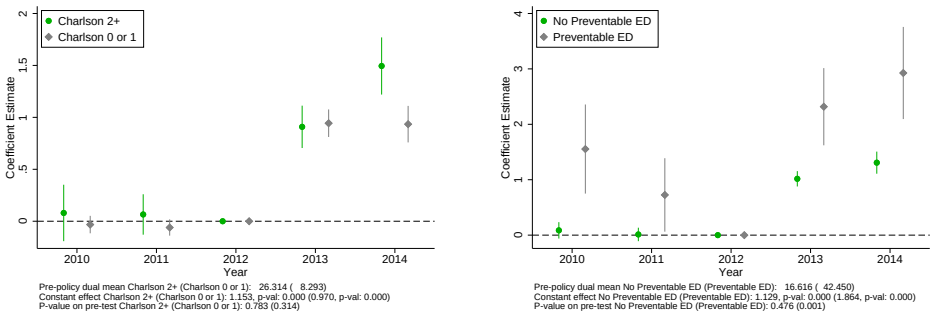
(a) Sex

(b) Age Group



(c) Race

(d) HPSA Status



(e) Charlson Score

(f) Preventable 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 two subgroup. The following post treatment coefficients differ significantly at the 5% level: age (2013, 2014); race (2013); HPSA status (2013 2014); Charlson score (2014); Preventable ED visit (2013, 2014). 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	9.50 (11.79)	11.09 (14.05)	7.07 (8.56)	8.25 (10.58)
Non-qualifying Providers	1.94 (3.39)	2.14 (3.65)	2.29 (3.2)	2.46 (3.39)
% With Any E&M Visit	89.63 (30.48)	91.27 (28.22)	89.99 (30.02)	90.99 (28.63)
E&M Services (Work RVUs)	19.28 (30.93)	23.22 (38.63)	14.01 (20.63)	17.09 (26.91)
Qualifying Providers	16.73 (28.89)	20.33 (36.43)	11.28 (18.88)	14.06 (25)
Non-qualifying Providers	2.55 (4.7)	2.89 (5.24)	2.73 (3.99)	3.03 (4.43)
New Patient Visits	.77 (1.21)	.75 (1.19)	.82 (1.18)	.85 (1.22)
Total Work RVUs	38.41 (55.12)	41.91 (61.16)	33.05 (44.72)	36.80 (49.69)
Age	62.44 (16.78)	64.94 (16.77)	73.92 (9.12)	76.42 (9.09)
% Female	61.92 (48.56)		55.14 (49.74)	
% White	62.92 (48.30)		90.45 (29.4)	
% Poor health in pre-period	60.98 (48.78)		53.93 (49.85)	
% Preventable ED visit in pre-period	10.32 (30.42)		4.35 (20.40)	
% in Primary Care Shortage Area	46.19 (49.85)		38.73 (48.71)	
Number of Beneficiary-years	1,019,697	679,798	9,603,483	6,402,322
Number of Beneficiaries	339,899		3,201,161	

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 ($)	28.93 (29.66)	31.22 (30.65)	21.74 (19.58)	24.41 (20.77)
Cost-Sharing as % of Total Payment	33.20 (34.42)	35.81 (35.34)	26.58 (21.20)	29.37 (22.85)
% of E&M Claims Submitted	36.33 (48.08)	46.78 (49.86)	39.66 (48.83)	36.63 (48.02)
% of E&M Cost-Sharing Paid	18.24 (50.93)	32.27 (61.13)	17.59 (52.09)	15.99 (47.75)
% of E&M Cost-Sharing Paid | Submission	49.21 (74.54)	67.20 (74.79)	43.77 (75.00)	43.22 (70.72)
Number of E&M Services	2,986,404	1,718,120	420,185	239,054

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

Panel A: no controls	(1)	(2)	(3)
	Cost-Sharing Payment Rate | Submission	Submissions	Cost-Sharing Payment Rate
2011*Qualifying Provider	0.0249	-0.00863	0.0014
	(0.0102)	(0.0046)	(0.0040)
	[0.015]	[0.060]	[0.717]
2012*Qualifying Provider			
	(omitted)	(omitted)	(omitted)
2013*Qualifying Provider	0.1980	0.1260	0.1460
	(0.0137)	(0.0123)	(0.0093)
	[0.000]	[0.000]	[0.000]
Panel B: baseline controls			
2011*Qualifying Provider	0.0251	-0.0029	0.0041
	(0.0078)	(0.0032)	(0.0033)
	[0.001]	[0.354]	[0.212]
2012*Qualifying Provider			
	(omitted)	(omitted)	(omitted)
2013*Qualifying Provider	0.1350	0.1180	0.1460
	(0.0123)	(0.0118)	(0.0090)
	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean at Qualifying Providers	0.4921	0.3633	0.1824

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	-0.003
	(0.081)	(0.046)	(0.001)
	[0.246]	[0.445]	[0.001]
2011*Dual	-0.050	0.020	0.000
	(0.061)	(0.025)	(0.001)
	[0.406]	[0.429]	[0.981]
2012*Dual			
	(omitted)	(omitted)	(omitted)
2013*Dual	0.787	0.533	0.009
	(0.071)	(0.035)	(0.001)
	[0.000]	[0.000]	[0.000]
2014*Dual	1.213	0.623	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 Mean at Qualifying Providers	16.78	9.50	
N	17,705,282	17,705,282	17,705,282

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: Dual vs. Non-Dual Beneficiaries				
	At Qualifying Providers		At Ineligible Providers	
	E&M Services	E&M Visits	E&M Services	E&M Visits
	(1)	(2)	(3)	(4)
2013*Dual	0.697	0.456	0.090	0.077
	(0.069)	(0.032)	(0.010)	(0.007)
	[0.000]	[0.000]	[0.000]	[0.000]
2014*Dual	1.109	0.539	0.103	0.084
	(0.099)	(0.043)	(0.014)	(0.009)
	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: Difference-in-Differences: Services Performed by Qualifying vs. Non-qualifying Providers				
	Dual Eligible Beneficiaries		Not Dual Eligible Beneficiaries	
	E&M Services	E&M Visits	E&M Services	E&M Visits
	(5)	(6)	(7)	(8)
2013*Qualifying Provider	1.491	0.774	0.945	0.424
	(0.069)	(0.032)	(0.019)	(0.008)
	[0.000]	[0.000]	[0.000]	[0.000]
2014*Qualifying Provider	3.541	1.458	2.665	1.064
	(0.110)	(0.045)	(0.041)	(0.016)
	[0.000]	[0.000]	[0.000]	[0.000]
Panel C: Triple Difference				
	E&M Services	E&M Visits		
	(9)	(10)		
2013*Dual*Qualifying Provider	0.546	0.350		
	(0.068)	(0.031)		
	[0.000]	[0.000]		
2014*Dual*Qualifying Provider	0.877	0.394		
	(0.098)	(0.040)		
	[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: Implied Elasticities

	Based on Estimated Change in			
	Actual Payment Rate (unconditional)		Payment Rate | Submission	
	Estimate	[95% CI]	Estimate	[95% CI]
		(1)		(2)
E&M services elasticity with respect to...				
% Change in Total Payments (Medicare + Medicaid)	1.18	[0.90, 1.55]	1.38	[0.99, 1.93]
% Change in Medicaid Payments	0.09	[0.07, 0.12]	0.28	[0.17, 0.43]
10pp Change Relative to Full Payment Rate	0.16	[0.13, 0.21]	0.16	[0.12, 0.22]
E&M visits elasticity with respect to...				
% Change in Total Payments (Medicare + Medicaid)	1.17	[0.82, 1.56]	1.38	[0.84, 2.08]
% Change in Medicaid Payments	0.09	[0.06, 0.13]	0.28	[0.14, 0.48]
10pp Change Relative to Full Payment Rate	0.16	[0.11, 0.22]	0.16	[0.10, 0.24]

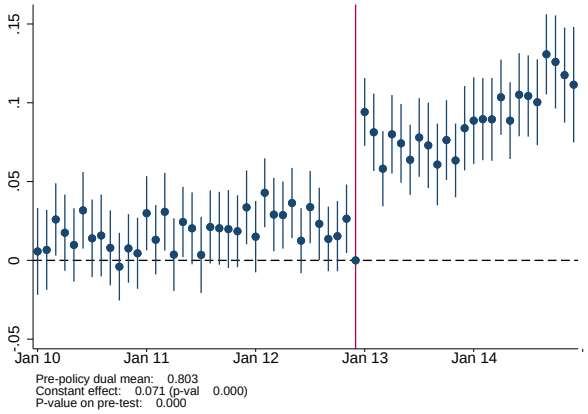
Notes: This table presents implied elasticities and the associated bootstrapped 95 percent confidence intervals for the change in payments and elasticities as indicated in the table above. These calculations restrict attention to the subset of states for which we have linked Medicaid data. We conduct a clustered bootstrap at the county level. For each of 1,000 bootstrap samples, we recompute estimates of the reduced form change in Medicaid payments and the reduced form change in utilization (based on the second year of the payment increase); we then 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. 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.

Appendix

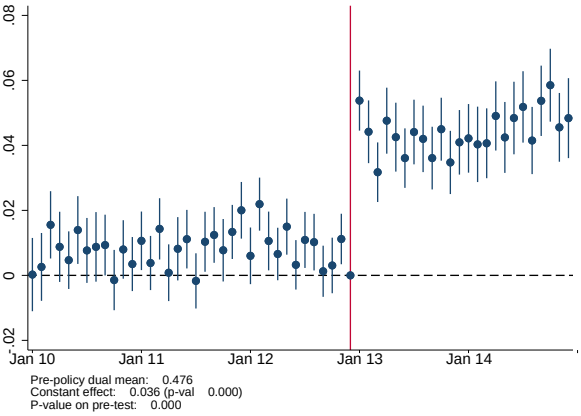
The Impact of Provider Payments on Health Care Utilization of Low-Income Individuals: Evidence from Medicare and Medicaid

Marika Cabral, Colleen Carey, & Sarah Miller

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



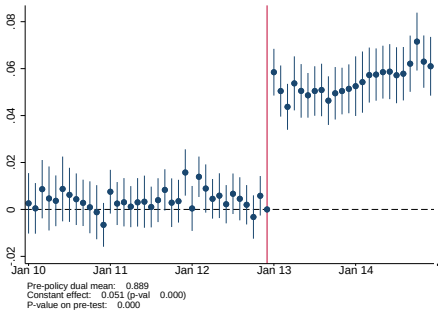
(a) E&M Services



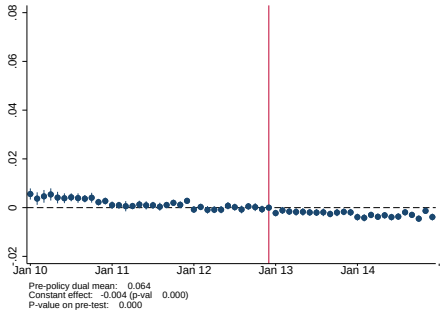
(b) E&M Visits

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.

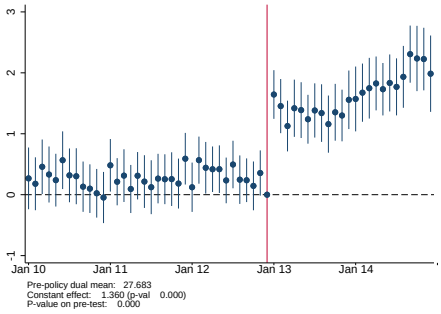
Figure A.2: Mechanisms: Impact on Types of Services Provided, Monthly Level



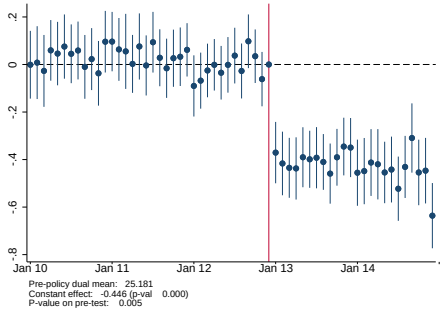
(a) Established Patient E&M Visits



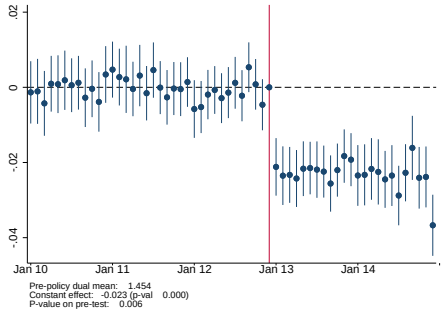
(b) New Patient E&M Visits



(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 month fixed effects and the indicator $Dual_i$ equals 1. Each panel is a different dependent variable. Note the time per E&M visit and RVUs per E&M visit exist only in months when the individual has a visit. 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	0.0249	-0.0086	0.0014
	(-0.137, 0.234)	(-0.015, -0.003)	(-0.028, 0.024)
	[0.654]	[0.026]	[0.960]
2013*Qualifying Provider	0.1982	0.1256	0.1463
	(0.012, 0.362)	(0.039, 0.203)	(0.077, 0.224)
	[0.035]	[0.005]	[0.007]
Panel B: baseline controls			
2011*Qualifying Provider	0.0251	-0.0029	0.0041
	(-0.093, 0.237)	(-0.008, 0.001)	(-0.019, 0.026)
	[0.830]	[0.129]	[0.915]
2013*Qualifying Provider	0.1351	0.1176	0.1458
	(0.028, 0.296)	(0.041, .0.200)	(0.079, 0.228)
	[0.016]	[0.004]	[0.008]
Dual Pre-Policy Mean at Qualifying Providers	0.4921	0.3633	0.1824

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: Alternative Specifications and Samples

	State-level Clustering			County x Year Fixed Effects			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
		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2010*Dual	-0.095	0.035	-0.003	-0.164	0.010	-0.002	0.110	0.157	-0.002
	(0.073)	(0.034)	(0.001)	(0.078)	(0.039)	(0.001)	(0.138)	(0.101)	(0.001)
	[0.203]	[0.313]	[0.029]	[0.036]	[0.794]	[0.015]	[0.422]	[0.121]	[0.115]
2011* Dual	-0.050	0.020	0.000	-0.106	-0.006	0.000	-0.026	0.058	-0.000
	(0.057)	(0.023)	(0.001)	(0.061)	(0.024)	(0.001)	(0.105)	(0.053)	(0.001)
	[0.382]	[0.399]	[0.988]	[0.082]	[0.795]	[0.847]	[0.803]	[0.275]	[0.651]
2012*Dual									
	(omitted)	(omitted)	(omitted)	(omitted)	(omitted)	(omitted)	(omitted)	(omitted)	(omitted)
2013* Dual	0.787	0.533	0.009	0.826	0.566	0.009	0.889	0.672	0.011
	(0.120)	(0.080)	(0.001)	(0.068)	(0.034)	(0.001)	(0.133)	(0.085)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
2014* Dual	1.213	0.623	0.009	1.302	0.698	0.009	1.275	0.739	0.011
	(0.150)	(0.102)	(0.001)	(0.091)	(0.044)	(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	19.28	11.43	0.896	19.28	11.43	0.896	18.90	11.67	0.885
Dual Mean at Qualifying Providers	16.78	9.50		16.78	9.50		16.46	9.68	
N	17,705,282	17,705,282	17,705,282	17,704,175	17,704,175	17,704,175	4,811,629	4,811,629	4,811,629

Note: This table displays estimates of regression coefficients from Equation (1) across alternative specifications and samples. Standard errors (clustered at the state level in columns (1)-(3) and at the county level for other columns) 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 A.3: Impact of the Payment Increase on E&M, Using "Near Poor" Beneficiaries as Comparison Group

	E&M Services	E&M Visits	Any E& M
	(1)	(2)	(3)
2010*Dual	-0.421	-0.203	-0.004
	(0.115)	(0.054)	(0.001)
	[0.000]	[0.000]	[0.004]
2011*Dual	-0.084	-0.020	-0.003
	(0.107)	(0.042)	(0.001)
	[0.433]	[0.636]	[0.025]
2012*Dual			
	(omitted)	(omitted)	(omitted)
2013*Dual	0.784	0.473	0.003
	(0.119)	(0.047)	(0.001)
	[0.000]	[0.000]	[0.016]
2014*Dual	1.085	0.547	0.001
	(0.169)	(0.063)	(0.002)
	[0.000]	[0.000]	[0.405]
Dual Pre-Policy Mean	19.28	11.43	0.896
Dual Mean at Qualifying Providers	16.78	9.50	
N	2,186,806	2,186,806	2,186,806

Notes: This table displays estimates of regression coefficients from Equation (1), where the control group is defined 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.4: 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	-0.802	-0.025	-0.002
	(0.041)	(0.007)	(1.393)	(0.035)	(0.002)
	[0.695]	[0.000]	[0.565]	[0.469]	[0.256]
2011* Dual	0.003	0.017	-0.434	0.030	0.001
	(0.024)	(0.003)	(1.009)	(0.027)	(0.002)
	[0.887]	[0.000]	[0.667]	[0.279]	[0.530]
2012*Dual					
	(omitted)	(omitted)	(omitted)	(omitted)	(omitted)
2013* Dual	0.553	-0.020	12.616	-0.327	-0.017
	(0.034)	(0.003)	(1.177)	(0.032)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
2014* Dual	0.660	-0.037	19.130	-0.394	-0.020
	(0.045)	(0.005)	(1.726)	(0.045)	(0.003)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Mean	10.67	0.767	332.2	27.03	1.554

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 A.5: Heterogeneity: E&M Services

	Female	Male	Age >= 75	Age < 75	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)
2013* Dual	0.748	0.922	0.407	1.155 †††	0.929	0.603 ††
	(0.079)	(0.125)	(0.102)	(0.095)	(0.078)	(0.133)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
2014* Dual	1.382	1.132	1.039	1.919 †††	1.298	0.958
	(0.104)	(0.181)	(0.148)	(0.118)	(0.111)	(0.187)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean	20.65	17.05	20.76	18.72	19.16	19.48

	HPSA	Non-HPSA	Worse Health	Better Health	Preventable ED	No Preventable ED
	(7)	(8)	(9)	(10)	(11)	(12)
2013* Dual	0.554	1.001 †††	0.909	0.944	2.318	1.017 †††
	(0.103)	(0.094)	(0.104)	(0.068)	(0.355)	(0.071)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
2014* Dual	0.988	1.419 ††	1.495	0.935 †††	2.926	1.308 †††
	(0.179)	(0.122)	(0.140)	(0.090)	(0.424)	(0.102)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean	18.99	19.54	26.31	8.293	42.45	16.62

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.

Table A.6: Heterogeneity: E&M Visits

	Female	Male	Age >= 75	Age < 75	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)
2013* Dual	0.004	0.018 †††	0.000	0.017 †††	0.010	0.005 †††
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.936]	[0.000]	[0.000]	[0.000]
2014* Dual	0.004	0.018	-0.002	0.025 †††	0.010	0.004 †††
	(0.001)	(0.002) †††	(0.001)	(0.001)	(0.001)	(0.002)
	[0.000]	[0.000]	[0.015]	[0.000]	[0.000]	[0.025]
Dual Pre-Policy Mean	0.923	0.853	0.927	0.885	0.899	0.891

	HPSA	Non-HPSA	Worse Health	Better Health	Preventable ED	No Preventable ED
	(7)	(8)	(9)	(10)	(11)	(12)
2013* Dual	0.007	0.010 †	0.003	0.022 †††	0.005	0.010 †††
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
2014* Dual	0.007	0.010	0.002	0.024 †††	0.006	0.010 †††
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean	0.887	0.904	0.964	0.791	0.975	0.887

Notes: This table displays estimates of regression coefficients from Equation (1) for subgroups defined in the top row. The outcome variable is E&M visits. 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.

Table A.7: Heterogeneity: Any E&M

	Female	Male	Age >= 75	Age < 75	White	Non-white
	(1)	(2)	(3)	(4)	(5)	(6)
2013* Dual	0.004	0.018 †††	-0.001	0.018 †††	0.010	0.005 †††
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.253]	[0.000]	[0.000]	[0.000]
2014* Dual	0.004	0.018	-0.003	0.024 †††	0.010	0.004 †††
	(0.001)	(0.002) †††	(0.001)	(0.001)	(0.001)	(0.002)
	[0.000]	[0.000]	[0.018]	[0.000]	[0.000]	[0.025]
Dual Pre-Policy Mean	0.910	0.838	0.915	0.870	0.899	0.891

	HPSA	Non-HPSA	Worse Health	Better Health	Preventable ED	No Preventable ED
	(7)	(8)	(9)	(10)	(11)	(12)
2013* Dual	0.007	0.010 †	0.003	0.022 †††	0.005	0.010 †††
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
2014* Dual	0.007	0.010	0.002	0.024 †††	0.006	0.010 †††
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Dual Pre-Policy Mean	0.871	0.892	0.964	0.791	0.975	0.887

Notes: This table displays estimates of regression coefficients from Equation (1) for subgroups defined in the top row. The outcome variable is the fraction of beneficiaries with any E&M claim. 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.