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THE IMPACTS OF PHYSICIAN PAYMENTS ON PATIENT ACCESS, USE, AND HEALTH

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ABSTRACT

We examine how supply-side health insurance generosity affects patient access, use, and health. Exploiting large, exogenous changes in Medicaid reimbursement rates for physicians, we find that increasing payments for new patient office visits reduces reports of providers turning away beneficiaries: closing the gap in payments between Medicaid and private insurers would reduce more than two-thirds of disparities in access among adults and would eliminate such disparities entirely among children. These improvements in access lead to more office visits, better self-reported health, and reduced school absenteeism. While attention is often focused on the role of demand-side insurance generosity, such as program eligibility and patient cost-sharing, our results demonstrate that financial incentives for physicians drive access to care and have important implications for patient health.

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I Introduction

Expanding access to health care has long been a primary goal of health policy in the United States. To this end, substantial political and financial resources have been directed toward increasing affordable health insurance coverage, including the recent Medicaid expansions and the formation of new health insurance exchanges under the Affordable Care Act (ACA). This focus has both led to and is motivated by a comprehensive literature documenting the important role of demand-side insurance generosity—including program eligibility and plan characteristics such as copayments—on patient access, use, and health.[1] But in a system with many health insurance providers, the benefits of having health insurance should be mediated by the willingness of providers to accept a given type of insurance (McGuire and Pauly, 1991). To what extent supply-side insurance generosity affects who physicians are willing to see—and whether these decisions affect the health of patients—remains an open question.

This question is particularly important in light of significant disparities in access to care between the publicly and privately insured: in 2009, office-based physicians were 35 percent less likely to accept new patients covered by Medicaid than those covered by private insurance (MACPAC, 2011; Decker, 2012, 2013). Since Medicaid historically pays physicians less than two-thirds of what Medicare and private insurers pay for the same services, these disparities in access could be driven by differences in payment generosity (Zuckerman and Goin, 2012). Alternatively, this preference for the privately insured could be driven by complex patient needs, payment delays, and high denial rates that are known to plague the Medicaid system (Sloan et al., 1978; Cunningham and O'Malley, 2009; Long, 2013; Gottlieb et al., 2018; Niess et al., 2018). Faced with little causal evidence that low payment levels are to blame for

[1] Using both randomized controlled trials (Finkelstein et al., 2012; Baicker et al., 2013; Goldin et al., 2019) and natural experiments (Currie and Gruber, 1996b,a; Card et al., 2008, 2009; Sommers et al., 2012; Goodman-Bacon, 2018; Miller et al., 2019; Goodman-Bacon, 2020), researchers have documented that having health insurance increases the use of health care services and can improve health. Studies further indicate that demand for health care is sensitive to price, making patient cost-sharing an appealing tool to steer the level and type of service use among those with health insurance (Manning et al., 1987; Baicker et al., 2015; Brot-Goldberg et al., 2017; Han et al., 2020).

disparities in access to care, policy makers often lower Medicaid payments in response to economic downturns and budgetary shortfalls (Smith et al., 2004; MACPAC, 2015).

In this paper, we exploit exogenous variation in reimbursement rates to estimate the effects of physician payment levels on patient access, use, and health. Most of our identifying variation comes from a federal mandate that required states to increase their Medicaid payments to match federally regulated Medicare levels for select primary care services in 2013 and 2014.[2] As states traditionally had wide latitude in setting their Medicaid payments, reimbursement rates varied dramatically across states before the primary care rate increase went into effect. While Medicaid payments for select primary care services increased by an average of 60 percent as a result of the mandate, rates more than doubled in eleven states and were unchanged in two.[3]

We find that increased physician reimbursement causes statistically and economically significant improvements in access to care. Combining a new database of state-level Medicaid reimbursement rates for new patient evaluation and management services from 2009 to 2014 with measures of access from the restricted-access National Health Interview Survey (NHIS) files, we estimate that a $10 increase in Medicaid payments reduces reports of doctors telling adult Medicaid beneficiaries that they are not accepting new patients or their insurance by 13 and 11 percent, respectively.[4,5] Among children covered by Medicaid, a $10 increase in Medicaid payments leads to a 25 percent decrease in parents reporting having trouble finding a doctor for their children. Notably, we find little evidence that these improvements in access among Medicaid beneficiaries are offset by negative spillovers to the privately insured. Our

[2] Designated in Section 1202 of the ACA, the rate increase was federally funded and was intended to ease the absorption of new Medicaid enrollees entering through the ACA's Medicaid expansions by encouraging primary care physicians to participate in Medicaid (Blumenthal and Collins, 2014). The primary care services covered by the mandate included evaluation and management services and vaccine administration provided by physicians in family medicine, general internal medicine, and pediatric medicine.

[3] Balancing regressions demonstrate that these changes in Medicaid payments are orthogonal to changes in Medicaid enrollment, Medicaid managed care penetration, and local economic and demographic conditions.

[4] Depending on the organization of a given practice, physicians themselves may not have full control over the types of patients that they see. Our results should therefore be interpreted as the joint responses of physicians and the organizations in which they work.

[5] Compared to the average baseline payment of $76, these improvements in access imply payment elasticities of physician willingness to accept new adult Medicaid beneficiaries of 0.83 to 1.01.

results indicate that closing the gap in payments between private insurance and Medicaid—a $45 increase in Medicaid payments for the median state—would close over two-thirds of disparities in access for adults and would eliminate such disparities entirely among children.

If Medicaid beneficiaries eventually receive treatment despite difficulties accessing care, increased payments could reduce search costs but have no impact on the use of services or health among patients. However, we find that increased reimbursement rates lead to greater usage and improved health among beneficiaries. Again using data from the NHIS, we find that a $10 increase in Medicaid payments leads to a 1.4 percent increase in the probability that beneficiaries visited a doctor in the past two weeks and a 1.1 percent increase in the probability that beneficiaries report being in very good or excellent health. Using self-reported data on school absences from the NHIS and administrative data on school attendance from the restricted-access National Assessment of Educational Progress (NAEP) files, we further find that a $10 increase in Medicaid payments leads to a 14 percent decrease in chronic absenteeism due to illness or injury and a 2.6 percent decrease in chronic absenteeism overall. These improvements come at the cost of only moderate increases in Medicaid budgets: taking into account increases in physician reimbursement for both marginal and inframarginal visits, a $10 increase in Medicaid payments for office visits increases state-level Medicaid spending by less than 1 percent on average.

When the federally mandated rate increase expired at the end of 2014, 34 states chose to return to their previous payment levels (MACPAC, 2015). This provides us with a second round of changes in reimbursement to exploit.[6] Using data from 2013 to 2015, we find that the reduction in reimbursement rates following the expiration of the federal mandate had effects of similar magnitudes—but opposite signs—as the primary care rate increase itself. This suggests that many of the improvements in access, use, and health that Medicaid beneficiaries experienced when payments increased were lost when payments returned to

[6]The decision not to extend the augmented payments may have depended on a state's experience during the federal mandate. However, we find that states that ultimately did and did not extend the higher payments experienced similar improvements in outcomes as a result of the primary care rate increase.

their previous levels.

Of course, changes in Medicaid payments stemming from the primary care rate increase did not occur in isolation. The U.S. health care system in general, and Medicaid in particular, experienced many other changes over our sample period. Most relevant for our analysis, 27 states and the District of Columbia expanded their Medicaid programs in 2014 to include coverage for low-income, childless adults. While we control for Medicaid expansions in all analyses, four additional sets of results confirm that our findings are not confounded by the 2014 Medicaid expansions. First, balancing regressions demonstrate that our identifying variation neither predicts state-level Medicaid expansions nor is associated with changes in Medicaid enrollment, Medicaid managed care penetration, or patient socio-demographics. Second, we find similar effects of changing reimbursement rates in states that did and did not expand their Medicaid programs under the ACA. Third, we estimate similar effects when we truncate the sample period to exclude the 2014 Medicaid expansions. Finally, as noted above, we estimate similar effects of reimbursement rates using variation in payments stemming from the expiration of the federal mandate in 2015, a year after the majority of Medicaid expansions had gone into effect.

While economists, public health researchers, and policy makers have long been interested in the effects of program generosity on access, use, and health among beneficiaries, causal analyses have been hampered by two important data limitations. First, before the primary care rate increase, most states had not made large changes to their Medicaid reimbursement rates in the last decades, and those that had chose to do so voluntarily.[7] Previous research on program generosity has therefore had to rely on cross-sectional associations that likely suffer from omitted variable bias, case studies of single fee changes that may be confounded by time trends, and difference-in-difference models in which treatment is potentially endogenous.[8] In

[7]Physician reimbursement rates in Medicare offer even less variation, as changes are made to a single, nationwide fee schedule. Furthermore, Medicare reimbursement rates for physicians have remained essentially the same for the past decade and will remain largely unchanged until at least 2025 under the Medicare Access and CHIP Reauthorization Act of 2015.

[8]Cross-sectional studies: Sloan et al. (1978); Hadley (1979); Long et al. (1986); Mitchell (1991); Cohen (1993); Cohen and Cunningham (1995); Showalter (1997). Case studies: Fox et al. (1992); Fanning and

contrast, we exploit a federal mandate that induced large, exogenous changes in Medicaid reimbursement rates across the United States.

Second, the rise of Medicaid managed care that began in the early 1990s has made it difficult to know how much physicians are actually reimbursed through Medicaid. In a fee-for-service system, state Medicaid programs pay providers a fixed amount for each service they provide. Although time consuming, these payment rates can be hand-collected by contacting each state (as we do in this study). Under managed care, in contrast, states typically pay managed care organizations (MCOs) a fixed amount per beneficiary to provide all covered services, and MCOs pay providers. While over 60 percent of Medicaid beneficiaries are enrolled in comprehensive risk-based managed care plans, states—and in turn researchers—know little about how or how much MCOs actually pay physicians for the services that they provide. As the primary care rate increase required states to raise their Medicaid payments to achieve parity with Medicare levels for *both* their fee-for-service and managed care programs, we are able to examine the effects of changing physician payments on the entire Medicaid system.[9]

Our work contributes to an ongoing debate on the effects of the Medicaid primary care rate increase on access to care. An early audit study found that the federal mandate was associated with increases in appointment availability among Medicaid patients in ten states (Polsky et al., 2015).[10] In contrast, recent work by Decker (2018) found that Medicaid acceptance rates in an annual survey of 1,500 physicians did not increase during the primary care rate increase. Using claims data on office visits among a convenience sample of 11 percent of primary care physicians, Mulcahy et al. (2018) also found no association between the rate increase and office visits among Medicaid beneficiaries. In contrast to this previous work, which relied on small, selected samples and included limited—if any—information on

de Alteriis (1993); Adams (1994); Gruber et al. (1997); Coburn et al. (1999). Difference-in-difference models: Baker and Royalty (2000); Shen and Zuckerman (2005); Decker (2007, 2009); Atherly and Mortensen (2014); Chen (2014); Buchmueller et al. (2015); Callison and Nguyen (2017).

[9]Section II.A and Appendix A.3 outline how the primary care rate increase was applied to managed care.

[10]Candon et al. (2018) replicate the analysis following the end of the mandate in 2015 and find that appointment availability declined in the sampled states that did not extend the increased payments.

the size of the rate increase in given states, we use comprehensive data covering every state and exploit continuous variation in the magnitude of the payment increases. Notably, we demonstrate that the effects of the federal mandate scale linearly with the size of the payment increase. This highlights that simple before-after designs—which average treatment effects across states that experienced payment increases of 0 to over 200 percent as a result of the mandate—lead to estimates that mask the true relationship between reimbursement rates and access to care. Additionally, we look beyond access alone and find that improvements in access resulting from increased payments lead to increased use and improvements in health.[11]

Our paper also adds to a growing literature documenting the importance of financial incentives in driving physician behavior. Prior work illustrates the impact of physician payment levels on treatment choices, decisions over treatment intensity, and adoption of new technologies, suggesting that higher fees lead providers to do more once a patient is through their door (Rice, 1983; Yip, 1998; Gruber et al., 1999; Clemens and Joshua, 2014; Coey, 2015). Our work complements these findings by demonstrating that financial incentives further drive extensive margin decisions governing who physicians are willing to see. This calls into question the common belief that demand-side incentives, via their influence on the initiation of visits, are the predominant dimension of insurance generosity that affects access to care (Cutler and Zeckhauser, 2000).

More broadly, our work relates to the literature that studies the effects of insurance coverage itself on the use of medical services and health outcomes. The health effects of health insurance have long been debated in the literature, with large-scale randomized controlled trials finding that health insurance improves self-reported health (Finkelstein et al., 2012) but has no widespread effects on clinical measures (Brook et al., 1984; Baicker et al., 2013).

[11] The studies by Decker (2018) and Mulcahy et al. (2018) further incorporate implementation delays that were associated with the rate increase. Since increased payments were made retroactively in states that experienced such delays, we would expect the behavior of physicians to respond when the augmented payments went into effect on January 1, 2013. In fact, even though some states took until May 2013 to release the increased payments (MACPAC, 2015), our event studies demonstrate that physician behavior responded equally in 2013 and 2014. Incorporating payment delays therefore biases the results towards zero because some of the "pre-period" in such specifications was actually treated.

However, mounting evidence using both randomized controlled trials (Goldin et al., 2019) and natural experiments (Card et al., 2009; Sommers et al., 2012; Goodman-Bacon, 2018; Miller et al., 2019) documents that having health insurance reduces mortality. We add to this literature by documenting that improvements in health care access resulting from increased payments for physicians lead to improvements in both self-reported health and reductions in school absenteeism due to illness and injury.[12] This highlights that any positive health effects of health insurance will be mediated by supply-side insurance generosity.

The rest of the paper proceeds as follows. We provide an overview of our data in Section II. Section III introduces our empirical strategy and examines the impacts of increased payments on access to care, use of services, and health. Section IV probes the robustness of these findings. Section V examines the effects of the reduction in reimbursement rates resulting from the end of the federal mandate. Section VI concludes by discussing mechanisms and implications for state Medicaid budgets.

II Data

We use three main data sources to document how physician reimbursement rates affect access to primary care, frequency of office visits, and health among patients. To measure physician reimbursement, we construct a new dataset containing Medicaid payments for new patient evaluation and management services for all states from 2009 to 2015. To measure patient access, use, and health, we use the NHIS. Finally, to corroborate the NHIS outcomes related to schooling, we use data on school absences and test scores from the NAEP. These datasets are supplemented with (1) information from the Health Resources and Services Administration's (HRSA) Area Resource Files (ARF) to control for spatial and temporal differences in socio-demographics and health care resources and (2) information from the Centers for Medicare and Medicaid Services's (CMS) Medicaid Managed Care Enrollment

[12]The reductions in illness-related school absenteeism that we document provide a plausible mechanism for work showing that public health insurance expansions among children have long-run effects on educational attainment (Brown et al., 2015; Cohodes et al., 2016).

Reports and National Health Expenditure Data (NHED) to examine changes in Medicaid enrollment and spending.[13]

II.A Medicaid Reimbursement Rates

Our primary explanatory variable is the amount that Medicaid pays physicians for new patient evaluation and management services across states and over time. Under a fee-for-service system, there are five Medicaid reimbursement rates for these services, each corresponding to a specific length and complexity of visit (current procedural terminology (CPT) codes 99201–99205).[14] We obtained historical payment data for these five codes by contacting the Medicaid offices of all 50 states and the District of Columbia.[15] Our main results use reimbursement rates associated with the most commonly billed new patient evaluation and management code over our sample period: new patient office visits of mid-level complexity (CPT code 99203).[16] Given the strong correlation between Medicaid payments for CPT codes 99201–99205 within states over time (see Figures A1 and A2), all of our results are robust to using payments for these alternative CPT codes.

The amount physicians are paid under fee-for-service Medicaid does not tell the full story, however, as over half of Medicaid beneficiaries are enrolled in managed care. While the primary care rate increase applied to both fee-for-service and managed care Medicaid programs, it was not immediately clear how states would adjust their capitation payments made to MCOs—or how MCOs would pass these increased payments through to providers—

[13] State-level information on the fraction of Medicaid beneficiaries enrolled in managed care is missing from the 2012 Medicaid Managed Care Enrollment Reports. We therefore use data from a CMS report titled "Medicaid Managed Care Trends and Snapshots 2000–2013" for 2012.

[14] Some states have modifier codes that allow for different payment rates depending on patient characteristics, such as age. Ideally we would incorporate modifier codes for children, although in practice this is difficult due to varying age group carve-outs across states (e.g., "pediatric only" or under age 15, 20, or 21) and restrictions on the level of aggregation allowed for merging variables within the NHIS. We therefore use base rates in all states but note that Medicaid payments for children will on average be slightly higher.

[15] We have complete payment data for 44 states and the District of Columbia. Appendix A.2 outlines the methodologies used to impute payment rates for the six states with partial payment histories.

[16] Of new patient visits billed to Medicare in 2009, the relative billing frequencies across CPT codes 99201–99205 were 3 percent, 19 percent, 43 percent, 27 percent, and 8 percent, respectively (Levinson, 2012). Our results are robust to using a billing frequency–weighted average across the five reimbursement rates for new patient visits. Unfortunately, analogous reports are not available for Medicaid.

to comply with the federal mandate. In light of these complications, CMS required states to submit information on the fraction of capitation payments made to MCOs that were used to reimburse qualified providers for the primary care services targeted by the primary care rate increase. Combined with data on utilization, these cost estimates were then used to generate implied fee-for-service rates to which the payment increases could be applied.[17] According to the federal mandate, these additional payments were required to be passed through to qualified physicians regardless of the payment scheme used by MCOs for provider reimbursement.[18] Combining our payment variation with administrative tax records, Gottlieb et al. (2020) demonstrate that the primary care rate increase indeed led to increases in take-home pay for primary care physicians.

We take managed care into account by creating an expected Medicaid payment measure that combines the state-level fee-for-service data with (1) state-level managed care to fee-for-service payment ratios and (2) state-level Medicaid managed care enrollment shares. In particular, we first use Medicaid managed care to fee-for-service payment ratios from the Government Accountability Office (GAO) for office-based evaluation and management services to calculate Medicaid managed care payments from the fee-for-service rates.[19] Using data from CMS on the percentage of Medicaid beneficiaries enrolled in managed care annually in each state (shown in Figure A3), we then define expected Medicaid payments at the state-quarter level as the enrollment-weighted average of Medicaid fee-for-service and managed care

[17] As outlined in Appendix A.3, states either used the utilization and implied rate information from 2009 to adjust their 2013 and 2014 capitation payments or maintained their original capitation rates and made supplemental payments following a pre-specified period based on realized utilization and cost data.

[18] If MCOs did not pass-through the increased payments to providers due to limited scope for enforcement, then the primary care rate increase would have created incentives for MCOs to attract additional enrollees. However, as shown in Table 1, we find no evidence that our identifying variation is correlated with Medicaid managed care enrollment.

[19] These payment ratios come from a GAO report documenting the difference between managed care and fee-for-service payments under Medicaid at the state level in 2010 (GAO, 2014). The report provides payment ratios for two de-identified states and eighteen identified states (Arizona, California, Connecticut, Florida, Georgia, Indiana, Michigan, New Jersey, New Mexico, New York, Ohio, Pennsylvania, Rhode Island, South Carolina, Texas, Virginia, Washington, Wisconsin). We use the recorded ratio for states in the report and the median of 5 percent more under managed care for missing states. As shown in Section IV, our results are robust to only using states in the GAO report and to imputing missing states with the mean (14 percent more under managed care).

payments.[20]

Both the initial geographic variation in Medicaid payment rates and the changes over our sample period are substantial. Figure 1 plots our constructed measure of Medicaid payments at the state-quarter level from 2009 to 2015.[21] In the first quarter of 2009, the expected Medicaid payment for a new patient office visit of mid-level complexity ranged from $37 in Minnesota to $160 in Alaska. Few states made meaningful changes to their reimbursement rates in the next three years: between 2009 and 2012, Medicaid payments for new patient office visits increased by an average of only $4.27 across states, with more than half of states making no changes to their payment schedules for evaluation and management services. When the primary care rate increase went into effect in 2013, the range tightened, with states paying physicians between $101 (Alabama) to $171 (Alaska).[22] As shown in Figure 2, the primary care rate increase was sufficient to push all states into the top quintile of reimbursement rates as defined in 2009.

While the federal mandate removed state control over the timing and nature of the payment increases, the magnitudes of the payment increases within states depended on their baseline level of payments. Estimates that leverage within-state variation in payments stemming from the federal mandate will therefore be biased if states with different payment rates in the pre-period were on systematically different trends. In Section III.B, we estimate event study specifications to demonstrate that states with differing payment increases were on

[20] That is, letting R_{sqy}^{FFS} denote the Medicaid fee-for-service reimbursement rate in state s in quarter q of year y, $\left(\frac{R^{MC}}{R^{FFS}}\right)_{s,2010}$ denote the managed care to fee-for-service payment ratio under Medicaid in state s in 2010, and $\%B_{sy}^{MC}$ denote the fraction of Medicaid beneficiaries enrolled in a managed care plan in state s in year y, the expected Medicaid reimbursement rate in each state-quarter before and after the primary care rate increase is approximated by

$$\tilde{R_{sqy}} = (1 - \%B_{sy}^{MC}) \cdot R_{sqy}^{FFS} + \%B_{sy}^{MC} \cdot R_{sqy}^{FFS} \cdot \left(\frac{R^{MC}}{R^{FFS}}\right)_{s,2010}$$

[21] Figure A2 plots quarterly fee-for-service Medicaid payment rates for CPT codes 99201–99205 in each state from 2009 to 2015.

[22] The remaining variation across states comes from two sources. First, Medicare payment levels vary across locations due to adjustments for geographic and market area differences. Second, Alaska and North Dakota maintained Medicaid payment rates that exceeded federally mandated Medicare levels over the sample period.

similar trends in terms of access, use, and health before the federal mandate. To further examine whether within-state variation in Medicaid payments is orthogonal to changes in Medicaid enrollment and local socio-demographics, we run balancing regressions in which we use potential confounders as dependent variables (Pei et al., 2019).[23] As shown in Table 1, we find no evidence that our identifying variation is correlated with changes in Medicaid enrollment, Medicaid managed care penetration, or local economic and demographic conditions.

When the primary care rate increase was initially passed, it was unclear whether federal funding for the increased payments would extend beyond 2014. In the end, the funding was not extended, and in 2015, 34 states chose to return to their previous payment levels (see Figure A4). While this provides another large change in payment rates, states may have made this decision based on their experience during the primary care rate increase (MACPAC, 2015). Thus, in our main analysis we do not use variation in Medicaid payments stemming from the expiration of the federal mandate. Instead, we examine the effects of this reverse experiment on outcomes separately and explore the potential endogeneity concerns.

Although the federal government mandated that states increase select Medicaid payments to primary care providers starting on January 1, 2013, many states experienced implementation delays (MACPAC, 2015). We do not incorporate state-level variation in the implementation of the primary care rate increase into our Medicaid payment variable; that is, we use the payment rates reported by the state as effective in each month and year. Because states with implementation delays were required to retroactively pay physicians the difference between the amount paid and the enhanced Medicaid rate, the behavior of physicians—who are largely not credit constrained—should respond at the start of the rate increase rather than when the higher payments were actually released. This is confirmed in our event study designs, which show that physician behavior responded equally in 2013 and 2014.

Finally, we can only expect physician behavior to respond to increased payments if

[23] In particular, we estimate analogs of Equation (2) introduced in Section III.C.

providers are aware of changes in reimbursement. We note that the primary care rate increase was covered widely by news outlets: for example, *The Washington Post* published an article on December 21, 2012—before the federal mandate went into effect—titled "Obamacare is about to give Medicaid docs a 73 percent raise" (The Washington Post, 2012). As Medicaid payments for select primary care services more than doubled in some states, it is reasonable that physicians would take notice of the change, and thus there is scope for physician behavior to respond.

II.B National Health Interview Survey

The NHIS is the largest in-person household survey that tracks health care access, health care utilization, and health outcomes across the United States. While many data sets measure health patterns, the NHIS is well-suited for our study for a number of reasons. First, while health insurance claims data sets provide information on the use of health care services, they provide no information on the difficulties that patients face accessing care. Furthermore, as the United States does not have a national all-payer claims database, nearly all claims data cover only a subset of patients with a specific insurance type in often limited geographic areas.[24] Finally, most other surveys only collect information on insurance status, not insurance provider, and are not large enough to be used for state-level estimates.[25] In contrast, the NHIS allows us to exploit state-level variation in Medicaid reimbursement rates over time to measure the effects of changing payments on access, use, and health separately

[24]We applied for the Medicaid Analytic eXtract (MAX) data to corroborate our findings surrounding use of health care services and to provide additional information on potential mechanisms. While having these additional outcomes will help expand the scope of our analysis in future work, we stress that the MAX data is not a substitute for the NHIS. In addition to providing no information on access or outcomes for patients with private insurance, the MAX data does not cover the entire United States. According to CMS, only 28 (17) states submitted sufficient information to be included in data extracts for 2013 (2014), which will substantially limit our identifying variation.

[25]The NHIS is very thorough with eliciting and coding insurance type. Rather than relying solely on patient reports of insurance type, which would lead to misclassification if Medicaid beneficiaries with private, managed care plans do not recognize that they are covered by Medicaid, the NHIS asks patients to report the actual name of their health insurance plan (e.g., Aetna Better Health of Illinois). The NHIS then uses this information to code insurance type based on their own categorization of over 4,000 plans. Additional details are provided on the NHIS website: https://www.cdc.gov/nchs/nhis/health_insurance/hi_eval.htm.

among patients with private insurance and Medicaid beneficiaries.

We use outcomes from three NHIS sample components in our analysis: the family file, the sample child file, and the sample adult file. The family component collects demographic information and answers to basic questions (e.g., health status) for all members of a family. The sample child and sample adult components each sample one child and one adult in the family and ask a longer list of more detailed questions (e.g., days of school or work missed in the past year). Sample sizes are thus more limited when working with questions asked in the sample child or sample adult files relative to the full family sample.

To measure access to health care services, we consider whether respondents report difficulty with doctors either not accepting new patients or not accepting their insurance.[26] For children, we further consider indicators denoting whether parents report having difficulty finding a doctor to see their child and whether their child has a usual place of care. To measure use of health care services, we consider whether respondents report having had an office visit in the past two weeks.

Policies targeting health care access do so with the hope that improving access will improve health. To examine whether higher reimbursement rates lead to better health, we consider indicators denoting whether people rate their health as excellent/very good or fair/poor. We further consider the number of work days adults report having missed and the number of school days parents report their child having missed in the past year. Notably, the NHIS asks specifically about school absences due to illness or injury, allowing us to focus on the category of absences most likely to be sensitive to changes in access to primary care. We focus on chronic absenteeism when considering school absences, which we define as missing fourteen or more days of school in the past year.[27]

[26] The exact survey questions used are outlined in Appendix A.1. All questions were asked throughout our full sample period except those asking whether children and adults had trouble finding a doctor, which started in 2011.

[27] Chronic absenteeism is linked to low academic achievement, including test scores, test score growth, and on-time graduation rates (Buehler et al., 2012; Connolly and Olson, 2012; Spradlin et al., 2012; Utah Education Policy Center, 2012; Schanzenbach et al., 2016). Unlike average absenteeism, rates of chronic absenteeism vary widely across schools, and all but thirteen states use chronic absenteeism in their accountability system under the Every Student Succeeds Act (Bauer et al., 2018).

As shown in Table 2, Medicaid beneficiaries and the privately insured have a similar likelihood of visiting a doctor in the past two weeks. However, those covered by Medicaid are more than twice as likely to report difficulties finding physicians who are willing to accept them as new patients. Baseline differences in health between Medicaid beneficiaries and the privately insured are also large: compared to respondents with private insurance, Medicaid beneficiaries are almost three times more likely to report being in fair or poor health, and children covered by Medicaid are twice as likely to be chronically absent.

To account for differences in demographics and the availability of medical resources across locations and over time, we control for individual demographics from the NHIS and county-level characteristics from the ARF. Table 3 reports summary statistics for individual and county-level characteristics by insurer. Relative to the privately insured, Medicaid beneficiaries have lower income and education levels, live in larger families, are less likely to be married, and are more likely to be black or Hispanic. Respondents covered by Medicaid also live in poorer, more densely populated areas.

Although much of the NHIS data is publicly available, geographic identifiers for areas smaller than Census regions are restricted. In order to link our outcome measures to state-level variation in Medicaid reimbursement rates and county-level health resources, we obtained access to confidential state and county identifiers. All of our analyses using the NHIS are therefore conducted in a Census Research Data Center.

II.C National Assessment of Educational Progress

To examine whether increased payments to physicians lead to better educational outcomes among low-income children, we supplement self-reported days of missed school from the NHIS with administrative data from the NAEP. The NAEP is a congressionally mandated assessment that provides information on reading and mathematics performance in grades 4 and 8 every other year in all states. Not all schools are tested in each wave, although the schools and students participating in NAEP are selected to be representative of all schools nationally and of public schools at the district level. We use data from both the publicly available, state-level files and the restricted-access, individual-level files for 2009, 2011, and 2013.

Importantly for our work, the NAEP reports whether a child missed 0, 1–2, 3–4, 5–10, and 11 or more days of school (for any reason) in the month preceding their national assessment exam. While the NAEP data does not include information on absences due specifically to illness or injury (as in the NHIS), most school absences—particularly among young children—are attributable to either acute illnesses such as respiratory infections and gastroenteritis or chronic childhood diseases such as asthma (Neuzil et al., 2002; Ehrlich et al., 2014; Wiseman and Dawson, 2015). As these conditions are commonly treated in a primary care setting, overall school absenteeism may be responsive to changes in access to primary care.

As in the NHIS, we focus on the fraction of chronically absent children. When considering absences over the period of a month (rather than over the course of a year, as in the NHIS), chronic absenteeism is commonly defined as three or more days of missed school (KewalRamani et al., 2007; Ginsburg et al., 2014; Schanzenbach et al., 2016). Figure A5 shows the distribution of absences averaged over math and reading assessments in grades 4 and 8. There are large differences by grade, with a larger fraction of students reporting zero absences in the past month in grade 4 than in grade 8.

As was seen in the NHIS data, there are large differences in the number of absences by socioeconomic status. Although we do not observe whether children are covered by Medicaid in the NAEP data, we can identify children that are eligible to receive free school meals. Like Medicaid, free school lunch is a means-tested program; according to income eligibility limits for each program, all children who are eligible for free school meals are also eligible for Medicaid (but not vice versa).[28] In grade 4, 54 percent of children ineligible for free lunch

[28] Across the United States, children in households with income at or below 130 percent of the federal poverty level are eligible to receive free meals at school (FRAC, 2018). Medicaid eligibility requirements vary by state, although the federal minimum income limit for children's health coverage is 138 percent of the federal poverty level with a median income limit of 149 percent across states (KFF, 2018).

missed zero days in the past month compared to 44 percent among students eligible for free lunch (Figure A5). The discrepancy in school absences by free lunch eligibility is similar in grade 8, though fewer children report zero absences in both groups.

In all grades and subjects, average test scores are monotonically decreasing in the number of school days missed in the past month (see Figure A6). Given the negative correlation between absences and test scores, it is possible that test scores could be affected by changes in access to primary care. We therefore also look at the effects of physician reimbursement on average state-level performance on national math and reading assessments.

III Physician Payments and Access, Use, and Health

The summary statistics in Table 2 demonstrate that those covered by Medicaid face greater difficulty accessing health care services and have worse health than the privately insured. To investigate whether differences in physician reimbursement contribute to these differences in outcomes, we examine the effects of changes in physician payments under Medicaid on patient access, use, and health. Sections III.A through III.C consider a range of outcomes from the NHIS, while Section III.D turns to educational outcomes from the NAEP. We focus on the impacts of the increase in Medicaid payments stemming from the onset of the primary care rate increase in 2013 throughout Section III; Section V considers the effects of the reduction in Medicaid payments following the expiration of the federal mandate in 2015.

III.A Raw Data

We begin by examining patterns in the raw data. To do so, we divide states into deciles based on the size of the payment increase that they experienced under the Medicaid primary care rate increase. Figure 3 plots the average change in various outcomes in the two years after the payment increase (2013–2014) versus the two years before (2011–2012) against the average payment increase in each decile. We plot two lines for each outcome—one for Medicaid beneficiaries and one for privately insured patients—that depict the best fit line through these points. We adjust the outcomes such that higher values denote better outcomes; an increasing slope therefore indicates that larger payment increases are associated with larger improvements in a given outcome.

Across a range of measures, we see that Medicaid beneficiaries in states with larger increases in Medicaid payments saw greater improvements in access, frequency of office visits, and health.[29] For example, in the upper left subplot of Figure 3 we see that Medicaid beneficiaries in states in the lowest decile of payment increases (average increase of $17.43) experienced little change in the probability of having an office visit in the past two weeks following the payment increase, whereas Medicaid beneficiaries in states in the highest decile of payment increases (average of $88.41) experienced an average increase of nearly 6 percent. Notably, across most outcomes there is no association between changes in Medicaid payments and changes in outcomes among privately insured patients; that is, the line is flat.

III.B Event Studies

To examine the timing of the effects and to control for differences across individuals and locations, we estimate event study specifications. In particular, letting $\Delta Payment_s = Payment_{s,2013Q1} - Payment_{s,2012Q4}$ denote the change in Medicaid payments resulting from the primary care rate increase in state s, we estimate the following equation:

$$Outcome_{icsy} = \beta_0 + \beta_y \Delta Payment_s * \lambda_y + \gamma X_i + \delta Z_{cy} + \lambda_s + \epsilon_{icsy} \quad (1)$$

where $Outcome_{icsy}$ denotes an outcome for Medicaid beneficiary i living in county c in state s in year y; X_i and Z_{cy} are vectors of individual and county characteristics (listed in Table 3), respectively; and λ_s and λ_y are state and year fixed effects, respectively. By scaling the

[29]Many of the subfigures in Figure 3 show a slight worsening of outcomes over time among Medicaid beneficiaries in states whose reimbursement rates were largely unaffected by the federal mandate. This highlights the importance of an empirical design that controls for Medicaid-specific time trends.

association between time and the outcome by the extent of the treatment, this specification exploits the full variation in Medicaid payments induced by the primary care rate increase. As in the raw data analysis, we adjust the outcomes such that higher values are indicative of better outcomes. We use the sample weights provided in the NHIS and cluster standard errors by state.

Figure 4 plots the β_ys from Equation (1). The coefficients before the primary care rate increase—$\hat{\beta}_{2009}$ through $\hat{\beta}_{2012}$—are statistically indistinguishable from zero, indicating that the outcomes were stable before the federal mandate. Following the rate increase, however, there are persistent, significant increases in many of the outcomes. For example, the bottom left subplot indicates that Medicaid beneficiaries saw improvements in physicians' willingness to accept new patients when Medicaid reimbursement rates increased in 2013 and 2014. The effects are immediate for most outcomes, although there is some evidence that health effects—such as patients reporting their health as excellent or very good—accrue over time.[30] As shown in Figure A7, we observe no effects among patients with private insurance.

III.C Regression Analysis

Figure 4 demonstrates that increased Medicaid payments lead to improved outcomes among Medicaid beneficiaries. To quantify the effects of physician reimbursement on access, use, and health, we estimate the following specification:

$$Outcome_{icsqy} = \beta_0 + \beta_1 Payment_{sqy} + \gamma X_i + \delta Z_{cy} + \lambda_s + \lambda_{qy} + \epsilon_{icsqy} \tag{2}$$

where $Outcome_{icsqy}$ denotes an outcome for respondent i living in county c in state s in quarter q of year y, $Payment_{sqy}$ denotes Medicaid payments in state s in quarter q of year

[30] Coefficients for 2013 may also be smaller because the reference periods for some questions include part of the pre-period. For example, when respondents are asked about difficulty finding a doctor over the past twelve months, respondents who were interviewed before the end of 2013 will include some of their experience from before the rate increase. Table A1 lists the reference window for each outcome.

y, λ_{qy} are quarter-year fixed effects, and all other variables are defined as in Equation (1).[31] We divide payments by $10 such that β_1 represents the effect of a $10 increase in Medicaid payments.[32] For the outcomes covering a retrospective time period of twelve months, the payment variable is the average Medicaid payment over the past four quarters; for all other outcomes we use the average payment in the quarter of the interview. Since we include state and quarter-year fixed effects, the coefficient of interest, β_1, is identified by changes in Medicaid payments within states over time. As before, all regressions use the sampling weights provided in the NHIS, and standard errors are clustered by state.

Results from estimation of Equation (2) are presented in Table 4. The first three columns of each panel show the effects of changes in Medicaid payments on survey respondents covered by Medicaid. Looking first to columns (1) and (2) of Panel B, we see that a $10 increase in Medicaid payments leads to a 0.54 percentage point decrease in the probability that parents report difficulty finding a doctor to see their child covered by Medicaid and a 0.36 percentage point decrease in the likelihood that the child has no usual place of care (reflecting decreases relative to the mean of 25 and 11 percent, respectively). Among adult Medicaid beneficiaries, a $10 increase in Medicaid payments causes both a 0.82 percentage point reduction in the probability of being told that a physician is not accepting new patients and a 0.89 percentage point reduction in the probability of being told that one's insurance is not accepted (decreases of 13 and 11 percent of the mean, respectively; see columns (1) and (2) of Panel C). Notably, these improvements in access lead to more use: in the full sample, a $10 increase in Medicaid payments increases the probability that respondents covered by Medicaid had an office visit in the past two weeks by 0.28 percentage points (1.4 percent relative to the mean; column

[31] Recall from Figure 1 that some states made minor adjustments to their Medicaid payments between 2009 and 2012 (over our sample window but before the federally mandated primary care rate increase). Most of our estimates of Equation (2) include these changes, although we confirm in Section IV that our results are robust to excluding variation in payments from before the federal mandate.

[32] As shown in Figure 3, the relationship between increases in Medicaid payments and changes in our outcome variables is approximately linear. We therefore prefer a linear specification both because it is suggested by the data and because it allows for the coefficients to be easily interpreted as the effect of a $10 increase in payments. We can, however, use a specification in which we consider $log(Payment_{sqy})$ on the right-hand side. The elasticities implied from both specifications are quantitatively similar.

(1) of Panel A).[33]

In addition to improved access and use, increases in Medicaid payments lead to better health among the program's beneficiaries. A $10 increase in physician reimbursement reduces the probability that beneficiaries report being in fair or poor health by 0.31 percentage points (1.8 percent of the mean; column (2) of Panel A) and increases the probability that beneficiaries report being in excellent or very good health by 0.62 percentage points (1.1 percent of the mean; column (3) of Panel A). Among young children covered by Medicaid, a $10 increase in Medicaid payments reduces the probability of being chronically absent due to illness or injury by 0.65 percentage points (a decrease of 14 percent of the mean; column (3) of Panel B).[34] There is no reduction in illness-related chronic absenteeism among older children covered by Medicaid (column (4) of Panel B); this is likely due to the fact that absences among older children are less closely tied to health (Neuzil et al., 2002; Ehrlich et al., 2014; Wiseman and Dawson, 2015).[35] We further find no reduction in days of work missed among adult Medicaid beneficiaries (column (3) of Panel C).

To get a sense of what these effects imply for the typical state under the primary care rate increase, we consider the effects of an increase in Medicaid payments of $35—the median increase in Medicaid payments across states from the fourth quarter of 2012 to the first quarter of 2013. Multiplying the point estimates in Table 4 by 3.5, we see that an increase of $35 in physician reimbursement under Medicaid leads to a 5.0 percent increase in the

[33] Using information on respondent age, we can examine effects of increased Medicaid reimbursement rates on office visits among children and adults separately. Although imprecise, the effects are very similar for children and adults: we find that a $10 increase in Medicaid reimbursement leads to a 1.0 (1.5) percent increase in the probability that a child (adult) had an office visit in the past two weeks.

[34] We find similar results when we consider a continuous measure of school absences rather than an indicator for chronic absenteeism. As shown in Table A2, a $10 increase in Medicaid payments leads to an average reduction of 0.23 days of school missed due to illness or injury per year among young children covered by Medicaid, a 6.4 percent reduction relative to the mean. We find no effects of Medicaid payments on illness-related school absences for older children covered by Medicaid or for children with private insurance.

[35] Acute illnesses such as respiratory infections and gastroenteritis and chronic childhood diseases such as asthma are among the most common reasons for school absenteeism among young children (Neuzil et al., 2002; Ehrlich et al., 2014; Wiseman and Dawson, 2015). Improved access to timely primary care could therefore lead to contemporaneous improvements in school attendance by allowing children to access antibiotics for bacterial infections, increasing the prevalence of influenza vaccinations, or improving the management of chronic diseases. We plan to explore the importance of these mechanisms using the Medicaid MAX data in future work (see Footnote 24).

probability of having visited a doctor's office in the past two weeks, a 6.2 percent decrease in the probability of being in fair or poor health, and a 3.9 percent increase in the probability of being in very good or excellent health among beneficiaries. Applying the same calculations to the access measures further indicates that the Medicaid primary care rate increase nearly eliminated parents having trouble finding doctors for their Medicaid-covered children and approximately halved these difficulties for adult beneficiaries in the median state.

We can compute elasticities by comparing the effects of a $10 increase in Medicaid payments in percentage terms to the corresponding percent change in Medicaid payments implied by a $10 increase.[36],[37] As reported in column (4) of Table 5, our results imply elasticities with respect to Medicaid payments of physician willingness to accept new adult Medicaid patients of 0.83, office visits among beneficiaries of 0.11, and self-reported good health among beneficiaries of 0.08. The implied elasticities for access among children are even more pronounced, suggesting that physicians are more responsive to payments for children. Although billing difficulties known to plague the Medicaid system should not depend on beneficiary age (Cunningham and O'Malley, 2009; Gottlieb et al., 2018), providers report that adult Medicaid beneficiaries have a wider breadth of needs, which makes managing their cases more difficult than those of children or patients with private insurance (Long, 2013; Niess et al., 2018).[38] It is therefore reasonable that physician behavior would be more responsive to Medicaid payments for children.

While we find strong evidence that increasing physician reimbursement under Medicaid improves access and health among the program's beneficiaries, there is little evidence of spillovers to the privately insured. The last three columns of Table 4 present analogous estimates for privately insured respondents, who may be indirectly affected by Medicaid

[36]Compared to the average baseline Medicaid payment of $76 for a new patient office visit of mid-level complexity, a $10 increase in payments corresponds to a 13.2 percent increase.

[37]Alternatively, we can calculate elasticities by including payments in logs instead of in levels in Equation (2). The elasticities from this alternative specification are very similar to those reported in Table 5.

[38]Niess et al. (2018) surveyed 806 physicians in Colorado to assess their beliefs and attitudes about adult Medicaid patients. Eighty-six percent of respondents had an unfavorable attitude toward adult Medicaid beneficiaries, with respondents most likely to agree that "socially complicated" and "medically complicated" described a typical adult Medicaid patient.

patients becoming relatively more attractive to physicians. However, we find no change in access, use, or health among the privately insured when Medicaid payments increase, with the exception of parents having slightly more trouble finding a doctor for their children (an increase of 0.13 percentage points, significant at the 10 percent level). Not only are the coefficients nearly all statistically insignificant despite large sample sizes, but across all outcomes the point estimates are much smaller than those observed among respondents covered by Medicaid.[39]

These effects have large implications for disparities in access to care between the publicly and privately insured. Column (3) of Table 5 reports baseline disparities in our outcome measures between Medicaid beneficiaries and patients with private insurance. Columns (6) through (8) show how much of this disparity is reduced by increasing Medicaid payments by $10, $35, and $45, respectively. As shown in column (7), increasing Medicaid payments by $35—the median increase under the primary care rate increase—reduces disparities in reports of doctors telling adult patients that they are not taking new patients or their insurance by 64 and 55 percent, respectively. Closing the gap in payments between private insurance and Medicaid—a $45 increase in Medicaid payments for the median state at baseline[40]—closes over 80 percent of the gap in reports of doctors not taking new adult patients and over two-thirds of the gap in reports of doctors not taking an adult patient's insurance. Because providers are more elastic to payments for children, it is easier to close gaps in access: as shown in column (9) of Table 5, it would take an increase in Medicaid payments of about

[39] Although statistically insignificant, the point estimate for the effect of increased Medicaid reimbursement rates on office visits among the privately insured could be consistent with some spillovers. Column (1) of Panel A in Table 4 shows that a $10 increase in Medicaid payments leads to a 0.28 percentage point increase in the probability that a Medicaid beneficiary had an office visit in the past two weeks. Assuming that this increase amounts to a single visit per marginal patient, this estimate translates to about 150,000 additional visits by Medicaid beneficiaries every two weeks (0.0028 times the approximately 54 million Medicaid beneficiaries in 2012). Among the 165 million Americans with private insurance in 2012, a reduction of 150,000 visits among 150,000 unique patients in a two-week period would lead to a 0.09 percentage point reduction in the probability that a privately insured patient had an office visit in the past two weeks. This is well within the confidence interval for the estimate in column (4) of Panel A in Table 4.

[40] We calculate the median difference in reimbursement rates between private insurance and Medicaid at baseline by combining private insurance to Medicaid payment ratios for office-based evaluation and management services from the GAO with our data on Medicaid payments. The GAO data documents the difference between private insurance and Medicaid payments for 32 states in 2010 (GAO, 2014).

\$26 on average to eliminate disparities in access between children with private insurance and children with Medicaid.[41]

III.D Educational Outcomes in the NAEP

All of the measures in the NHIS, including days of school missed, are self reported. To corroborate our finding that increased reimbursement rates for physicians reduce school absenteeism among children—and to examine whether these reductions in absenteeism lead to improvements in test scores—we use administrative data from the NAEP. We begin by estimating a specification similar to Equation (2) using individual-level data:

$$Outcome_{isy} = \beta_0 + \beta_1 Payment_{sy} + \gamma X_i + \lambda_s + \lambda_y + \epsilon_{isy} \quad (3)$$

where $Outcome_{isy}$ denotes an attendance outcome for student i in state s in year y; X_i is a vector of individual-level demographics included in the NAEP (indicators denoting age, sex, race, and ethnicity); and λ_s and λ_y are state and year fixed effects, respectively. As all state assessments take place between January and March, $Payment_{sy}$ is the expected Medicaid payment in state s in the first quarter of year y. We use the sample weights provided by the NAEP and cluster standard errors by state.

Results from estimation of Equation (3) are presented in Table 6. Panel A shows the effects of changes in Medicaid payments on outcomes among students who qualify for free lunch, our proxy for Medicaid eligibility in the NAEP. For low-income children in the 4th

[41] As shown in column (7) of Table 5, the median increase in Medicaid payments of \$35 under the federal mandate was sufficient to close *more* than the disparity in outcomes between children with private insurance and children with Medicaid; this suggests that children on Medicaid were more attractive to physicians than children with private insurance after the rate increase. As noted in footnote 14, state Medicaid programs often pay slightly more for children than for adults. The median payment increase of \$35 will therefore close more of—or may even go beyond—the gap in payments between Medicaid and private insurance for children. Furthermore, we note that we do find some, albeit weak, evidence of negative spillovers to children with private insurance as a result of the Medicaid primary care rate increase: as shown in Table 4, a \$10 increase in Medicaid payments increases reports of parents having trouble finding a doctor to see their child with private insurance by 0.13 percentage points, or 16 percent relative to the mean (significant at the 10 percent level).

grade, a \$10 increase in Medicaid payments reduces the fraction of students who missed three or more days in the past month for any reason by 0.34 percentage points (2.6 percent relative to the mean; column (3)) and increases the fraction of students with zero absences by 0.28 percentage points (0.6 percent relative to the mean; column (1)).[42] Columns (2) and (4) in Panel A show a similar pattern for children in grade 8, although the point estimates are much smaller and less precise. The larger effects in grade 4 relative to grade 8 likely reflect the fact that absences for younger children are closely tied to health status whereas absences for older children are more likely to be for reasons unrelated to health care access, such as truancy (Neuzil et al., 2002; Ehrlich et al., 2014; Wiseman and Dawson, 2015). As shown in Panel B, we find no effects among children who do not qualify for free lunch.

As school absenteeism is closely linked to test scores, it is possible that these reductions in absenteeism among disadvantaged children lead to improvements in their performance. To examine the effects of increased Medicaid payments on test scores, we estimate an analog of Equation (2) at the state-year level:

$$Outcome_{sy} = \beta_0 + \beta_1 Payment_{sy} + \gamma X_{sy} + \lambda_s + \lambda_y + \epsilon_{sy} \tag{4}$$

where $Outcome_{sy}$ denotes an average schooling outcome in state s in year y; X_{sy} is a vector of state-level analogs of the controls listed in Table 3; and λ_s and λ_y are state and year fixed effects, respectively. As in Equation (3), $Payment_{sy}$ is the expected Medicaid payment in state s in the first quarter of year y. We weight the regressions by state population and cluster standard errors by state.

As shown in Panel B of Table A3, we find no effects of increased physician reimbursement under Medicaid on average state-level scores on national math and reading assessments.[43]

[42] Recall that the NAEP covers absences for any reason whereas the NHIS asks specifically about school absences due to illness or injury. It is therefore not surprising that we find larger effects in percentage terms when considering school absenteeism in the NHIS than in the NAEP.

[43] Panel A of Table A3 shows effects of Medicaid payments on average state-level absences. As in Table 6, we find significant reductions in absenteeism only for children who qualify for free school lunch. When aggregating to the state level, however, we find evidence of significant improvements in attendance among disadvantaged children in both grades 4 and 8.

It is possible that more prolonged improvements in attendance are necessary to cause improvements in student performance.

IV Robustness

IV.A 2014 Medicaid Expansions

In 2014, 27 states and the District of Columbia expanded their Medicaid programs to extend coverage to low-income, childless adults (see Figure A8). If states that saw larger payment increases under the primary care rate increase were also more likely to expand their Medicaid programs, then our results could be confounded by changes in program eligibility. Although the timing of the Medicaid expansions and the Medicaid primary care rate increase were similar, we are confident that our results are not confounded by the Medicaid expansions for several reasons. First, we control for state-year level Medicaid expansions in all of our analyses, thereby absorbing any direct effects of the expansions. Second, recall that states were required to raise their Medicaid payments to match Medicare levels for select primary care services beginning in January 2013, a year before most of the expansions. As shown in Figure 4, most of the effects of the rate increase were realized before 2014. Third, we find the largest effects on access and health among children, whose eligibility was largely unaffected by the expansions.[44] Finally, as shown in Section V below, we estimate similar effects of physician reimbursement rates on patient outcomes using variation in payments stemming only from the federal mandate expiring in 2015, a year after the majority of Medicaid expansions had gone into effect.

Nevertheless, we conduct four additional analyses to further verify that our results are not confounded by the Medicaid expansions. As first introduced in Section II.A, we run

[44]Recent work demonstrates that children's use of preventive services increases when Medicaid eligibility is extended among adults (Venkataramani et al., 2017). While such spillovers could influence our estimates of child health, it is unlikely that an improvement in coverage among adults would make it easier for parents to find physicians willing to see their children.

balancing regressions to directly examine whether our identifying variation is correlated with changes in Medicaid enrollment and composition. As shown in Table 1, we find no evidence that within-state changes in Medicaid payments predict Medicaid expansions or are associated with changes in Medicaid enrollment or managed care penetration. We also re-estimate Equation (2) including only: (1) the years before the 2014 Medicaid expansions (2009–2013), (2) states that did not expand their Medicaid programs in 2014, and (3) families with children. The top rows of each subfigure in Figure 5 compare the estimates from our main sample with the results from these subsample analyses. Looking first to the results using data from 2009–2013 only, we see that our estimates are remarkably consistent when we exclude 2014. While some of our estimates lose precision when we only consider states that did not expand Medicaid, the general pattern of results is consistent with our main findings. Finally, we see that—if anything—our results are often stronger among households with children.

Taken together, these results provide strong evidence that we are identifying changes in access, use, and health driven by changes in supply-side program generosity, not demand-side program eligibility.

IV.B Medicaid Payment Variable

As outlined in Section II.A, we create expected Medicaid payment rates by combining: (1) state-level reimbursement rates under fee-for-service Medicaid collected directly from state Medicaid offices, (2) state-level Medicaid fee-for-service to managed care payment ratios from the GAO, and (3) state-level Medicaid managed care enrollment shares from CMS. While we have Medicaid fee-for-service rates and Medicaid managed care enrollment shares for all states, the GAO report only provides payment ratios for 20 states.[45]

In our main analysis, we use the median payment ratio among states in the GAO report

[45] As previously noted, we have complete payment information for 44 states and the District of Columbia. Appendix A.2 outlines the methodologies used to impute payment rates for the six states with incomplete payment histories. Given that only a few imputations are required, our results are robust to only using non-imputed data and to using alternative imputation strategies.

(5 percent more under Medicaid managed care) for states that are not in the GAO data. To probe the robustness of our results to this imputation, we replicate our main results: (1) imputing states that are not in the GAO report with the mean payment ratio of 14 percent more under Medicaid managed care, (2) only using states in the GAO report, and (3) only using variation stemming from Medicaid fee-for-service payment rates.

Results from these additional analyses are presented in Figure 5. Across all outcomes, imputing missing states with the mean payment ratio instead of the median has very little impact on the results. Narrowing the sample to only the 20 states in the GAO report tends to decrease the precision of our estimates, but the magnitudes of the effects are very similar to our primary specification. Finally, despite the fact that nearly 60 percent of Medicaid beneficiaries are enrolled in managed care, our results are very similar if we only consider fee-for-service reimbursement rates. This is not surprising given that most of the residual variation in our measure of Medicaid payments when controlling for state and time fixed effects comes from changes in fee-for-service payments within states over time.[46]

Recall that our primary payment variable includes all variation in state-level Medicaid payments for new patient office visits of mid-level complexity between 2009 and 2014. As previously noted, states that adjusted their reimbursement rates before the federal mandate chose to do so voluntarily, and thus the payment changes may be endogenous. The final row in each subfigure in Figure 5 replicates our main results using variation in Medicaid reimbursement rates stemming only from the federally mandated primary care rate increase. To do so, we impute state-level reimbursement rates from 2009 through the third quarter of 2012 with the relevant payment rate from the fourth quarter of 2012. As the overwhelming majority of variation in Medicaid reimbursement rates between 2009 and 2014 was driven

[46]Recall that variation in our Medicaid payment variable comes from three sources: (1) time-series variation in state-level fee-for-service payments, (2) cross-sectional variation in state-level Medicaid managed care to fee-for-service payment ratios, and (3) time-series variation in the fraction of Medicaid beneficiaries enrolled in managed care in each state. With the inclusion of state fixed effects, residual variation in payments comes only from (1) and (3). While the fraction of Medicaid beneficiaries enrolled in managed care varies within a state over time (Figure A3), the vast majority of our identifying variation comes from changes in fee-for-service payments (Figure 1).

by the federal mandate in 2013 (see Figure 1), our results are nearly identical if we ignore variation in payments between 2009 and 2012.

IV.C Triple Difference Model

In our preferred empirical specification, we conduct analyses separately among Medicaid beneficiaries and patients covered by private insurance. We look separately at these two groups, rather than using the privately insured as a control group, as changes in relative reimbursement rates could influence the treatment of individuals with private insurance. If, for example, increases in Medicaid payments lead physicians to see fewer patients with private insurance, then a triple difference strategy using patients with private insurance as a control group would overestimate improvements among Medicaid beneficiaries.

However, as this strategy has been used previously when examining the impacts of changing reimbursement rates (see, for example, Shen and Zuckerman, 2005; Atherly and Mortensen, 2014; and Callison and Nguyen, 2017), we provide estimates from triple difference models for comparison. In particular, we estimate analogs of Equation (2) that include main effects for all independent variables in addition to interactions between each independent variable and an indicator denoting whether respondent i is a Medicaid beneficiary. We only include patients with private insurance and Medicaid beneficiaries; that is, we exclude those without insurance and those covered by Medicare.

Table 7 presents results from this fully interacted specification.[47] The pattern of results is very similar to that found using only Medicaid beneficiaries in a difference-in-difference framework. The similarity is not surprising given the minimal evidence of spillovers to the privately insured that we saw in Table 4.

[47] Table A4 shows results from an alternative specification in which we allow the constant and the effect of changing Medicaid reimbursement rates to differ for Medicaid beneficiaries but restrict the time trends and the associations between individual-level demographics and the outcome to be the same across insurance types. Results from this alternative specification are similar but generally smaller. We prefer the fully interacted specification since it controls for differential time trends, age profiles, and impacts of other demographics between patients with private insurance and Medicaid beneficiaries.

V Expiration of the Primary Care Rate Increase

Both the federally mandated Medicaid primary care rate increase and the accompanying federal funding expired at the end of 2014. Beginning in 2015, states could therefore choose either to maintain the payments at higher levels—and pay for the higher payments themselves—or revert to their original payments. As shown in Figure A4, 34 states chose not to extend the increased payments; in these states, Medicaid reimbursement rates returned to their December 2012 levels in January 2015.

While the reduction in Medicaid reimbursement rates resulting from the end of the federal mandate provides a second round of changes in reimbursement, the decision not to maintain the higher payments could be endogenous. Notably, states that experienced greater success under the federally mandated rate increase—that is, states in which the rate increase led to larger improvements in access, use, and health among Medicaid beneficiaries—may have been more likely to extend the increased rates.[48] To examine whether the primary care rate increase had smaller impacts in states that chose not to maintain the higher payments, we replicate our analysis using only the subset of states that did not extend the increased rates beyond 2014.[49] Although we divide the sample by a decision made at the end of 2014, we only use variation in Medicaid payments stemming from the onset of the primary care rate increase in 2013; that is, we consider the effects of the primary care rate increase switching on in states that ultimately decided to switch it off.

As shown in the top two rows of each subfigure in Figure 6, states that chose not to extend

[48] Alternatively, states that did not extend the increased payments could have had less pronounced effects because providers in those states knew that the payments would be temporary (and physician-patient relationships tend to last for many years). However, it was difficult to forecast whether the rates would ultimately be extended. Notably, it was not announced until late 2014 that the increased payments would not receive federal funding in 2015; if the federal funding were to have persisted, it seems likely that all states would have maintained the higher rates. Furthermore, the states that chose to extend the increased payments show diversity in geography, demographics, and political affiliations, so it is unlikely that providers could have predicted whether the payments would be extended based on state-level characteristics alone.

[49] We further estimate an augmented version of Equation (2) that includes a state-level indicator denoting whether a state extended the increased payments beyond 2014 and an interaction between the payment variable and this indicator. As shown in Table A5, the interaction is not significant for any outcome other than days of work missed, demonstrating that states that did and did not ultimately extend the increased payments experienced similar effects from the mandated rate increase.

the higher payments saw improvements in outcomes during the federal mandate that were similar in magnitude to those experienced by the average state. While there is some evidence that states that did not extend the increased payments experienced smaller improvements in access, the point estimates for effects on use and health suggest that these states experienced similar—and if anything larger—improvements in downstream outcomes during the primary care rate increase. This suggests that states chose to return their reimbursement rates to previous levels despite significant improvements in outcomes resulting from the increased payments.

There are a number of reasons a state's decision over whether to extend the payments may have been unrelated to its experience during the federally mandated rate increase. First, federal funding for the increased payments expired with the mandate. Budgetary considerations could therefore have led states to lower payments even if they were aware of the implications for the health care of Medicaid beneficiaries. Second, until this point, little comprehensive evidence has existed to demonstrate that the primary care rate increase had significant impacts on access, use, and health among Medicaid beneficiaries. Notably, a small survey of Medicaid officials, plan administrators, and provider organizations conducted by the Medicaid and CHIP Payment and Access Commission in the summer of 2014 suggests that states believed that the primary care rate increase had little impact on access to primary care (MACPAC, 2015).

We therefore consider the effects of the primary care rate increase expiring in 2015 on access, use, and health among Medicaid beneficiaries. To do so, we estimate a specification analogous to Equation (2) that instead exploits variation in payments stemming only from the federal mandate expiring in 2015. Although the variation comes from payment decreases, the estimated coefficients again represent the effects of a $10 *increase* in Medicaid payments. As shown in the bottom row of each subfigure in Figure 6, the effects of reimbursement rates using variation stemming from the expiration of the primary care rate increase look similar, although slightly smaller, than the effects of reimbursement rates using variation stemming from the onset of the primary care rate increase. While we lose precision when considering a subset of states and years, comparing the point estimates between the final two rows in each subfigure—which hold the sample of states constant—suggests that the rate increase switching off had effects of similar magnitudes on access and use but smaller effects on health than the rate increase switching on. This could be because there is more persistence in health than physician and patient behavior.

It is notable that we find similar effects of physician payments using variation stemming from a payment increase and a payment decrease. One could imagine that providers would adjust their practice in response to a payment increase in ways that would persist in the face of subsequent payment decreases. For example, providers might pay the fixed cost to enroll as a Medicaid provider or invest in learning to deal with the complexities of Medicaid billing. Additionally, providers might establish relationships with Medicaid patients that would be difficult to abandon if payments were to subsequently decline. Although limited in precision, our results instead suggest that many of the improvements in access, use, and health that Medicaid beneficiaries experienced when payments increased were lost when payments returned to their previous levels.

VI Discussion and Conclusion

While it is known that financial incentives matter in health care, increasing reimbursement rates may not make physicians more willing to accept new patients for at least two reasons. First, factors other than low payments may lead providers to restrict access for certain patients. In the case of Medicaid, for example, payment delays, high denial rates, and complex patient needs may make treating beneficiaries unattractive regardless of relative payment levels (Sloan et al., 1978; Cunningham and O'Malley, 2009; Long, 2013; Gottlieb et al., 2018; Niess et al., 2018). Second, capacity constraints limit the number of patients that providers can see. With a fixed number of hours in the day, access to health care will necessarily be rationed when the supply of providers has not kept pace with growing demand.

In contrast, we find that changes in physician reimbursement have meaningful effects on access to care for patients. Exploiting large, exogenous changes in physician reimbursement rates for primary care visits under Medicaid, we estimate that an increase in Medicaid payments of $35—the median increase across states over the federally mandated primary care rate increase—reduced the probability that adult Medicaid beneficiaries were told that a physician was not accepting their insurance by 3.1 percentage points, or 38 percent of the mean. Compared to the average Medicaid payment of $76 for a new patient office visit of mid-level complexity before the rate increase, our estimates imply an elasticity of physician willingness to accept adult Medicaid patients with respect to reimbursement of 0.83.

These improvements in access among Medicaid beneficiaries have large implications for disparities in access to care. Before the primary care rate increase, 8.2 percent of adult Medicaid beneficiaries reported being told that a provider was not accepting their insurance compared to only 2.5 percent among adults with private insurance. Our results demonstrate that increasing Medicaid reimbursement rates by $45—enough to close the median gap in payments between Medicaid and private insurers—would reduce disparities in access to care by at least 70 percent. Our results are even more pronounced among children, for whom we find that closing the gap in physician payment rates between Medicaid and private insurance would eliminate disparities in access entirely.

We further find that improving access leads to increased use and better health among Medicaid beneficiaries. Increasing Medicaid payments by $35 increases the probability that program beneficiaries had an office visit in the past two weeks by 5.0 percent and increases the probability that they report being in excellent or very good health by 3.9 percent. The implied elasticity of self-reported health with respect to outpatient care is consistent with the literature using exogenous variation in health insurance coverage itself: when Medicaid was extended to low-income adults using a lottery in Oregon, those who gained insurance saw a 50 percent increase in office visits and were 25 percent more likely to report being in excellent or very good health (Finkelstein et al., 2012; Baicker et al., 2013). We further find that increased access to primary care reduces school absenteeism among young children: a $35 increase in Medicaid payments leads to an average reduction of 0.79 days missed per year due to illness or injury, or 22 percent of the mean, and reduces illness-related chronic absenteeism by nearly 50 percent.

The improvements in access, use, and health that we document come at the cost of increased Medicaid spending. Taking into account increases in physician reimbursement for both marginal and inframarginal visits, back-of-the-envelope calculations suggest that a $10 increase in Medicaid payments for office visits increases average state-level Medicaid spending by approximately $150 million annually, or less than 1 percent of average state-year Medicaid spending of over $16 billion.[50] In line with this calculation, we replicate our analysis using data on annual state-level Medicaid spending from the NHED and find that a $10 increase in Medicaid payments for office visits leads to a statistically insignificant increase of $141 million in Medicaid spending per state-year (Table A6). Although the monetary value of the sizable increases in access, use, and health among Medicaid beneficiaries that we document are difficult to quantify, the limited impacts of changes in Medicaid reimbursement for office visits on government budgets suggest that the policy may have been cost effective.

An outstanding question is how physicians are able to absorb new patients when reimbursement rates increase. If physicians are capacity constrained, they could increase the number of Medicaid patients they see either by substituting away from patients with private

[50]Table 2 shows that Medicaid beneficiaries had an average of 0.197 office visits over a two-week period, or 5.12 visits per year, before the primary care rate increase. Multiplied by the average number of Medicaid beneficiaries per state in the pre-period (2.66 million; Table 1), there were an average of 13.62 million office visits among Medicaid beneficiaries per state-year before the primary care rate increase. Increasing payments per visit by $10 should therefore lead to $136.19 million in additional spending on inframarginal visits, or 0.008 percent of average annual state-level Medicaid spending ($16.68 billion; Table A6). In terms of marginal visits, Table 4 shows that a $10 increase in Medicaid payments leads to an increase of 0.0028 office visits per Medicaid beneficiary in a two-week period, or 0.0728 visits per year. Again multiplied by the average number of Medicaid beneficiaries, and taking into account the total physician payment for an office visit in the average state ($76 in the pre-period plus a $10 increase), increasing payments per visit by $10 should lead to $16.65 million in additional spending on marginal visits, or less than 0.001 percent. Combined, a $10 increase in Medicaid payments for office visits should therefore increase state-level Medicaid spending by approximately $150 million annually, or less than 1 percent.

insurance or by decreasing their appointment length per patient. We find little evidence that increasing Medicaid payments negatively impacts access among patients with private insurance, suggesting that physicians do not respond to increased Medicaid payments by substituting away from the privately insured on the extensive margin. Furthermore, we find no health effects among the privately insured, whereas decreased appointment length may result in worse provision of care.

If physicians are not capacity constrained, they could increase the number of Medicaid patients they see by increasing their total hours worked. Although we cannot look at physician labor supply directly in our data, we can divide counties by whether they have a shortage of primary care providers as defined by the HRSA. If some physicians are capacity constrained, we would expect increased Medicaid payments to have smaller effects in areas where providers have little scope to take on new patients. However, as shown in Table A7, we find no evidence of differential effects between counties that are and are not primary care shortage areas. This suggests that some providers in areas with and without an adequate supply of providers have scope to increase the number of patients they see. Understanding how physicians accommodate more patients when payments increase is an important area for future work.

The difficulties that Medicaid patients face accessing care is commonly attributed to a combination of complex patient needs, billing complications, and low reimbursement rates. This has led policy makers, practitioners, and researchers alike to argue that increasing reimbursement rates alone will not be enough to improve the provision of care to Medicaid beneficiaries (Goroll, 2018). In turn, efforts to promote health care access and use have largely focussed on dimensions of demand-side insurance generosity, such as program eligibility and patient cost-sharing. In contrast, we find that the majority of differences in access between Medicaid beneficiaries and privately insured patients are driven by differences in physician reimbursement. Not only does increasing Medicaid reimbursement rates improve access, but these improvements in access lead to meaningful improvements in self-reported health and school absenteeism among the program's beneficiaries. While it is well-known that financial incentives matter in health care, they appear to matter even more than previously thought.

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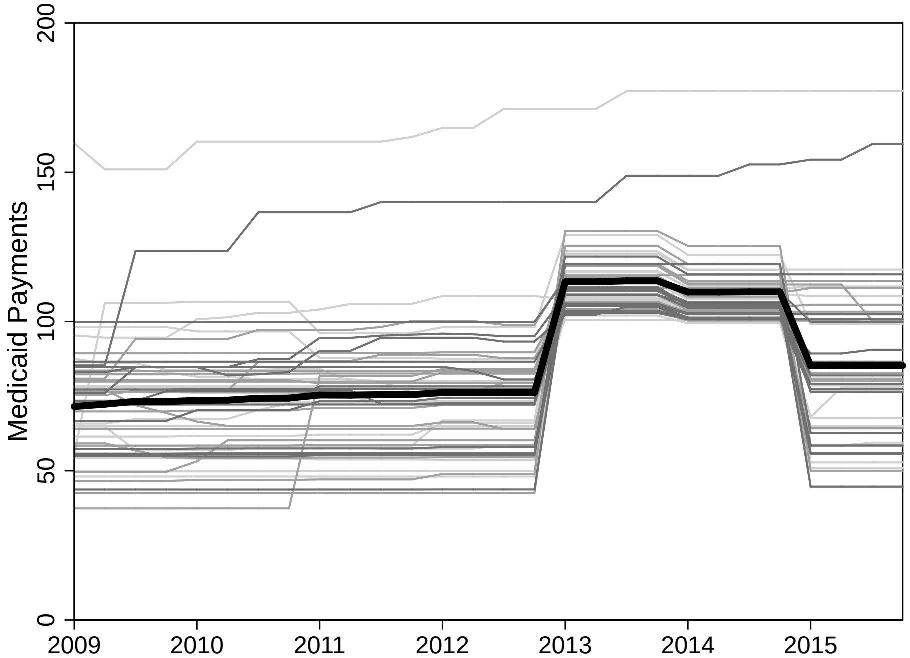
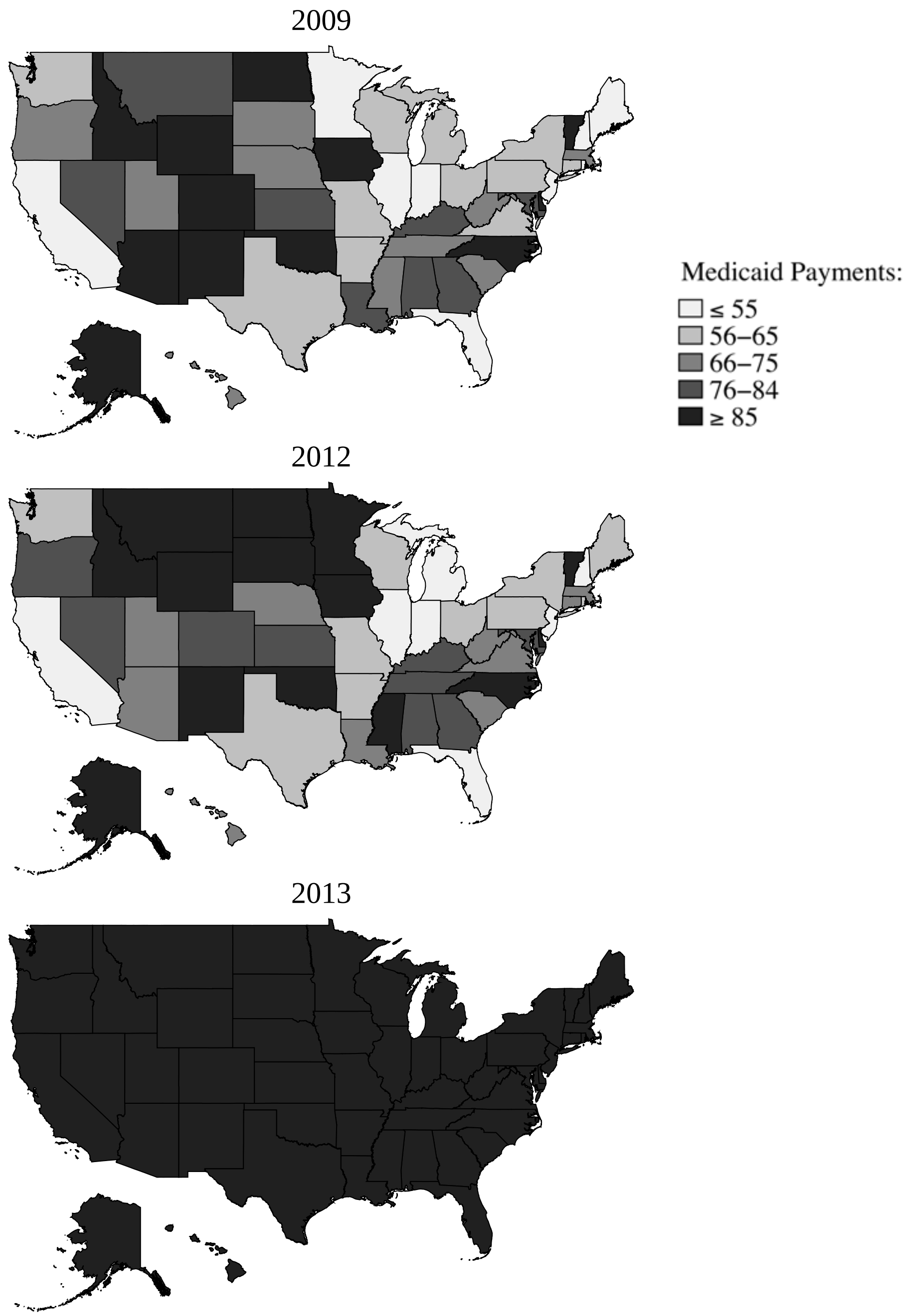
VII Figures

Figure 1: State-Level Medicaid Payments over Time



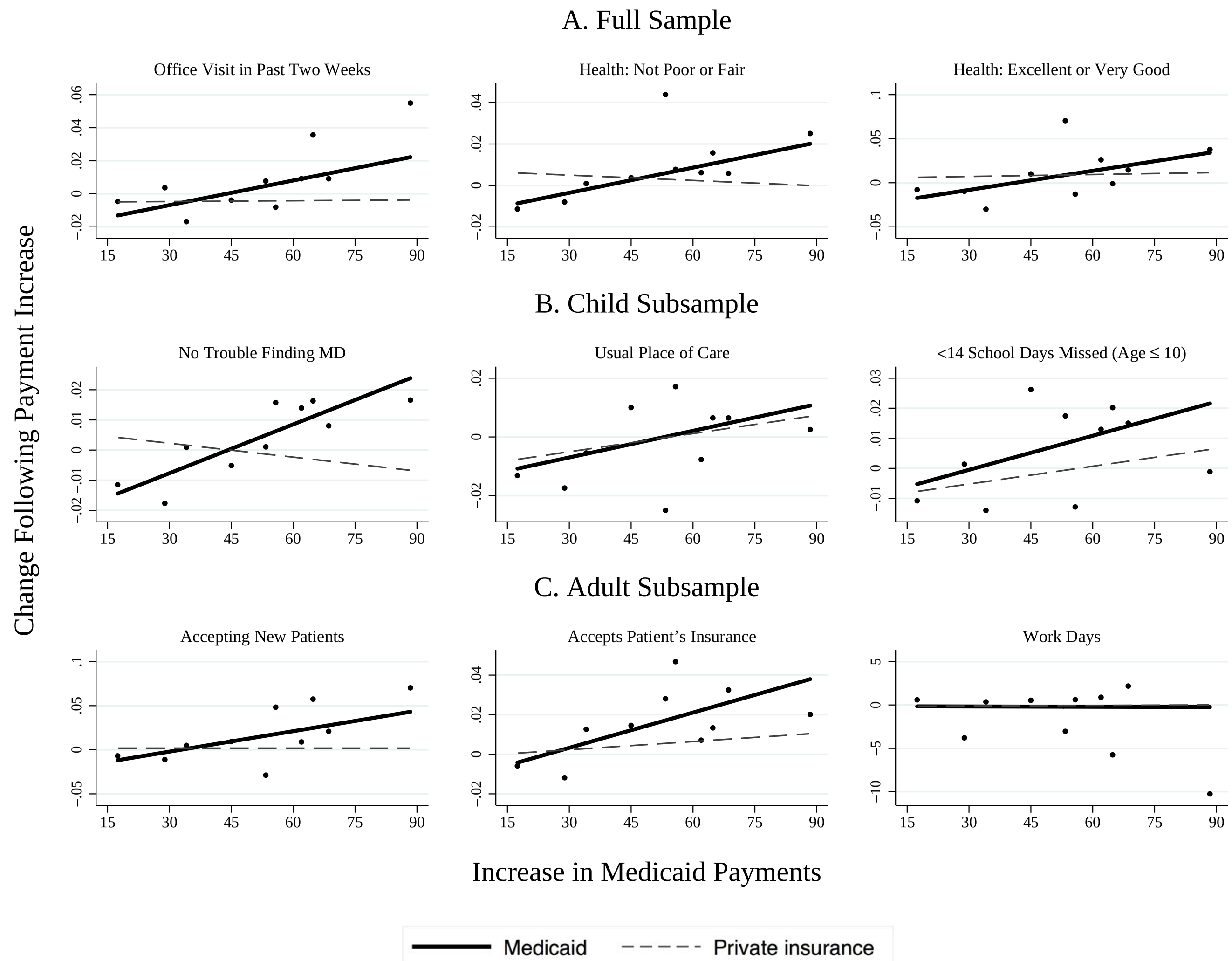
Notes: The above figure depicts Medicaid payments at the state-quarter level from 2009 to 2015. As defined in footnote 20, the payments are beneficiary-weighted averages of Medicaid fee-for-service and managed care payments for new patient office visits of mid-level complexity; patterns are qualitatively robust to using Medicaid payment rates for alternative billing codes. The top two lines are Alaska (1) and North Dakota (2); the bottom two lines in 2009 are New Hampshire (50) and Minnesota (51).

Figure 2: Maps of State-Level Medicaid Payments



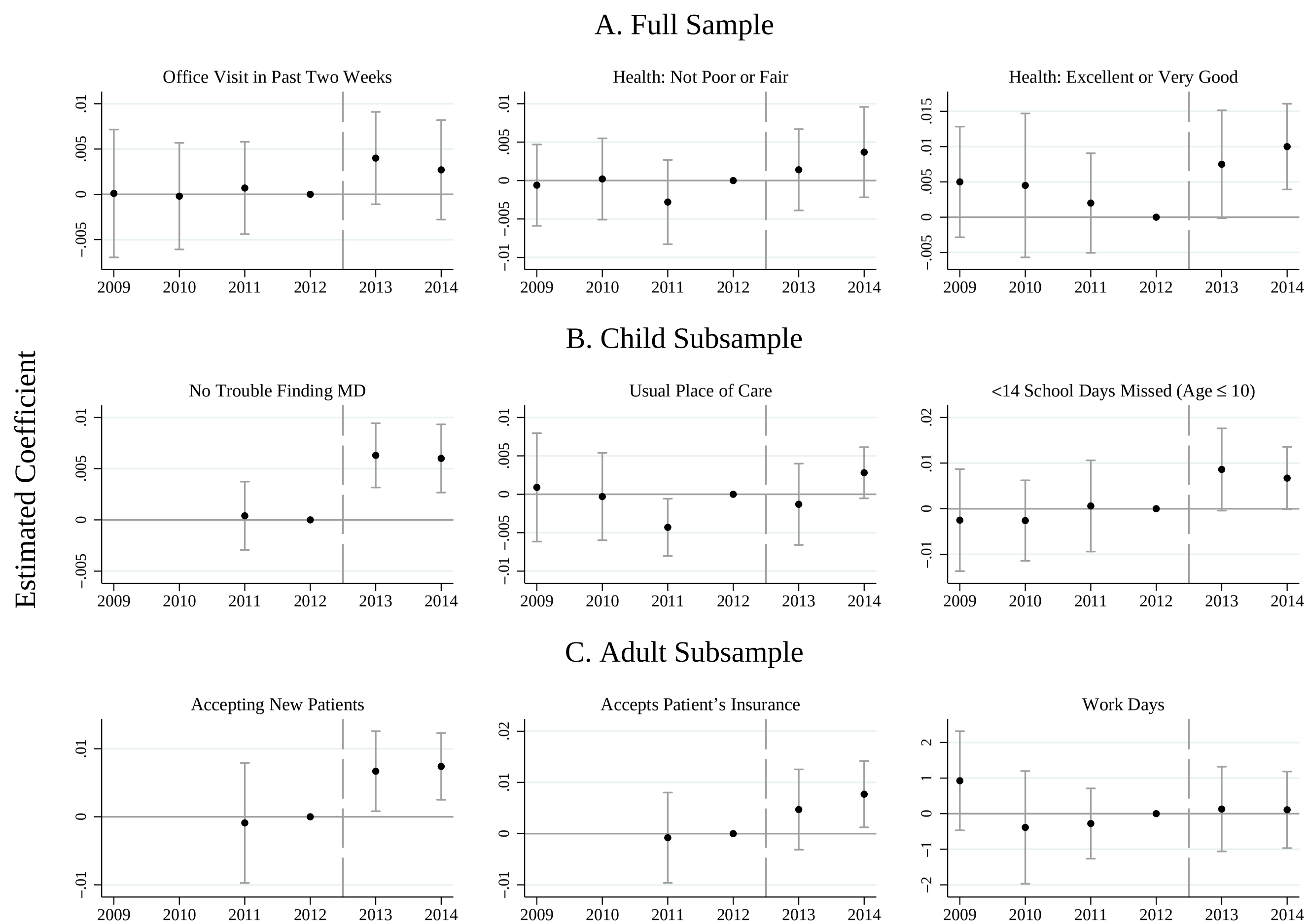
Notes: The above maps depict Medicaid payments for each state in 2009 (first year in sample period), 2012 (year before rate increase), and 2013 (first year of rate increase). As defined in footnote 20, the payments are beneficiary-weighted averages of Medicaid fee-for-service and managed care payments for new patient office visits of mid-level complexity; patterns are qualitatively robust to using Medicaid payment rates for alternative billing codes. The colors reflect quintiles of reimbursement levels in 2009; following the primary care rate increase, all states had Medicaid payments that were in the highest 2009 quintile.

Figure 3: Changes in Access, Use, and Health by Size of Payment Increase



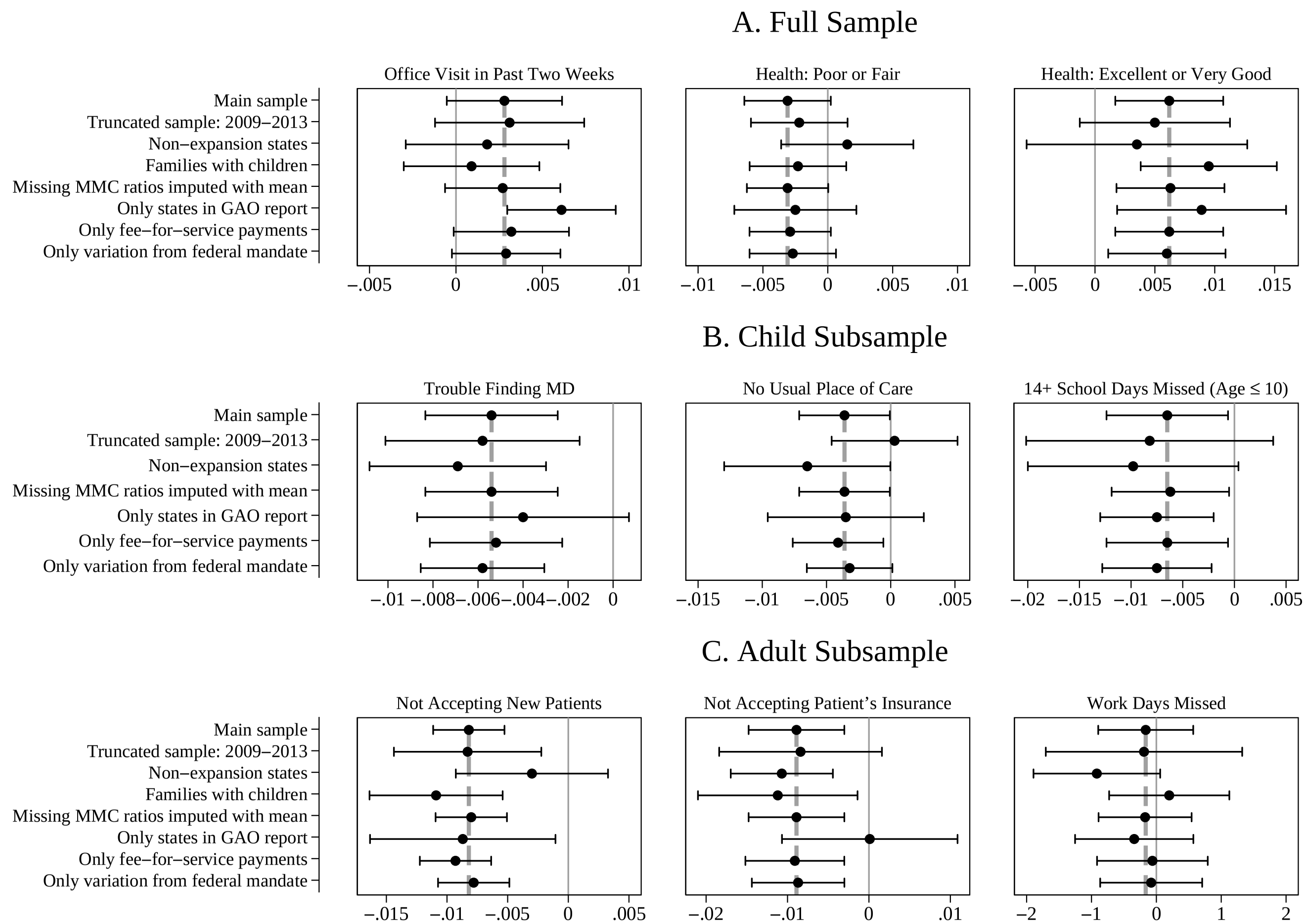
Notes: The above figures plot the average change in each outcome between 2011–2012 and 2013–2014 across deciles of state-level Medicaid payment increases. The dots represent changes among Medicaid patients; the solid black line is the best fit line through these points. The dashed line depicts the best fit line for changes among patients with private insurance. Outcomes are adjusted such that higher values denote better outcomes. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1.

Figure 4: Event Study: Effects of Medicaid Payments on Access, Use, and Health Among Medicaid Beneficiaries



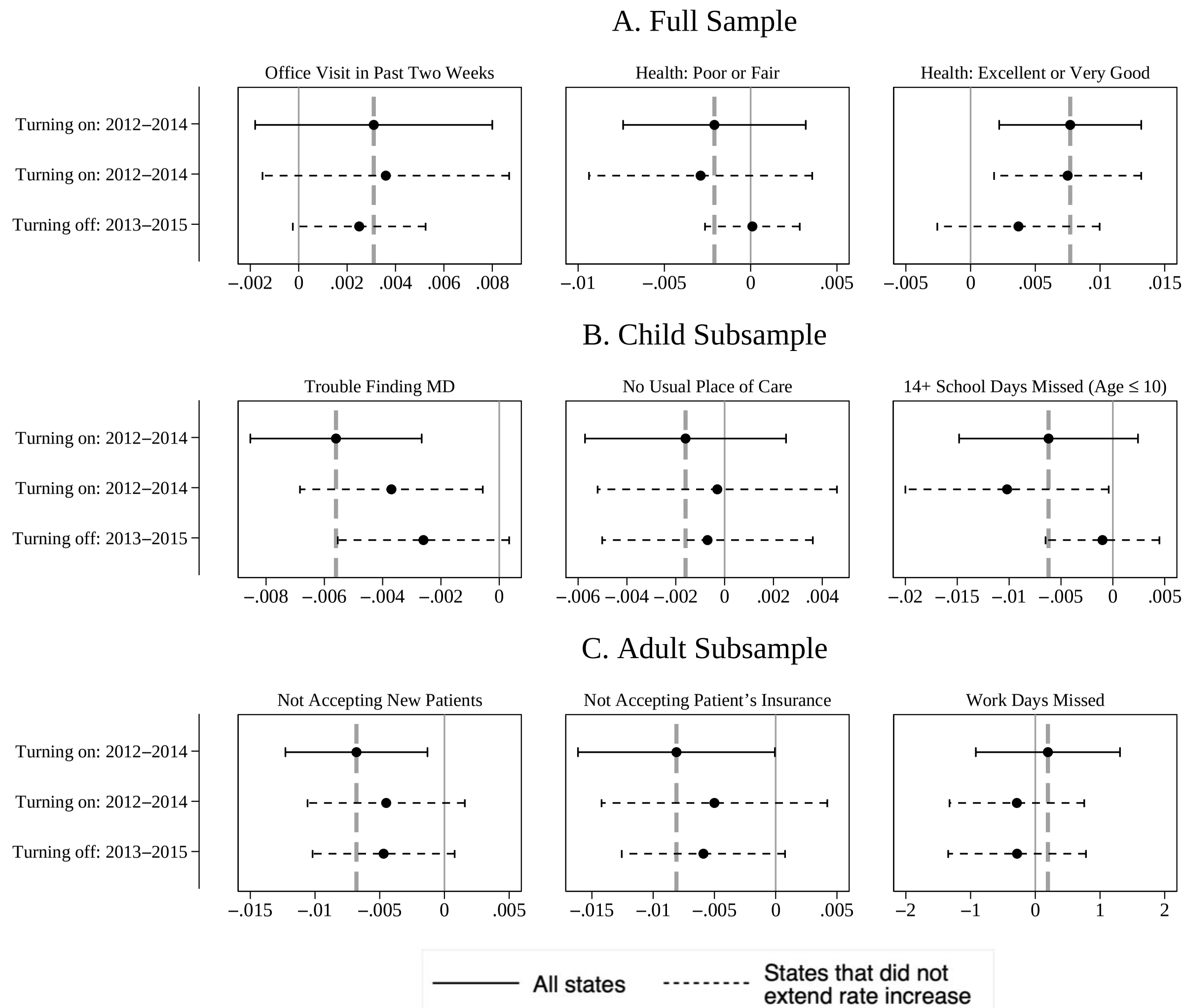
Notes: The above figures plot the coefficients and 95% confidence intervals on year indicators interacted with state-level changes in Medicaid payments induced by the primary care rate increase from estimation of Equation (1). The above figures show results for Medicaid beneficiaries; refer to Figure A7 for analogous results for individuals with private insurance. Outcomes are adjusted such that higher values denote better outcomes. Outcomes with missing coefficients in 2009 and 2010 were added to the NHIS in 2011. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1.

Figure 5: Robustness: Effects of Medicaid Payments on Access, Use, and Health Among Medicaid Beneficiaries



Notes: Each dot in the above figures depicts the estimated effect of a $10 increase in Medicaid payments for the subsample listed on the y-axis from estimation of Equation (2). Each coefficient comes from a separate regression. The horizontal bars depict 95% confidence intervals for each coefficient. The dashed vertical line in each subplot displays the coefficient estimate from the main sample (as reported in Table 4), which includes years 2009–2014 and imputes missing payment ratios with the median across all states in the GAO report. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1.

Figure 6: Effects of Payment Increases versus Payment Decreases



Notes: Each dot in the above figures depicts the estimated effect of a $10 increase in Medicaid payments for the subsample listed on the y-axis from estimation of Equation (2). Although the third row in each subfigure exploits payment decreases, the estimated coefficients still represent the effects of a $10 *increase* in Medicaid payments. Each coefficient comes from a separate regression. The horizontal bars depict 95% confidence intervals for each coefficient. The dashed vertical line in each subplot displays the coefficient estimate from the main sample (as reported in Table 4), which includes years 2009–2014 and imputes missing payment ratios with the median across all states in the GAO report. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1.

VIII Tables

Table 1: Balancing Regressions of Medicaid Enrollment and Socio-demographics on Medicaid Payments

A. Medicaid Enrollment		Medicaid Enrollment			Medicaid Managed Care Enrollment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\mathbb{1}\{\text{Expansion}\}$	Millions	Logs	Per Capita	Millions	Per Capita	Share of Medicaid
Medicaid payments ($10s)	0.027	-0.0056	-0.0040	-0.00093	-0.0021	-0.0021	-0.0092
	(0.019)	(0.018)	(0.0062)	(0.0012)	(0.014)	(0.0022)	(0.012)
Observations	306	306	306	306	306	306	306
R^2	0.639	0.997	0.998	0.974	0.993	0.937	0.914
Baseline mean	0.48	2.66	14.3	0.18	1.45	0.089	0.50

B. Socio-demographics		Share of Population					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Median Income (10,000s)	Impoverished	Under 19	Over 65	White	Black	Hispanic
Medicaid payments ($10s)	0.0081	0.00012	-0.00017	-0.00018	-0.00019	-0.000040	0.00032
	(0.0055)	(0.00037)	(0.00011)	(0.00015)	(0.00022)	(0.00027)	(0.00030)
Observations	306	306	306	306	306	306	306
R^2	0.997	0.992	0.997	0.998	0.999	1.000	1.000
Baseline mean	5.25	0.15	0.25	0.13	0.79	0.13	0.16

Notes: Observations are at the state level-year; standard errors are clustered by state. All regressions include state and year fixed effects and state-level analogs of the controls listed in Table 3. Regressions are weighted by state population. Total Medicaid enrollment comes from CMS's National Health Expenditure Data, Medicaid managed care enrollment comes from CMS's Medicaid Managed Care Enrollment Reports, and socio-demographics come from the HRSA's Area Resource Files. Results using individual-level data from the NHIS (our primary sample) reveal no changes in Medicaid enrollment or composition; release of results delayed due to COVID-19 disruptions.

Table 2: Summary Statistics: NHIS Outcome Measures

	Entire Sample (2009–2014)		Baseline (2009–2012)		Rate Increase (2013–2014)	
	Medicaid (1)	Private (2)	Medicaid (3)	Private (4)	Medicaid (5)	Private (6)
A. Full Sample						
Office visit in past two weeks	0.196	0.174	0.197	0.175	0.194	0.172
Health: poor or fair	0.174	0.061	0.176	0.062	0.169	0.059
Health: excellent or very good	0.566	0.729	0.562	0.726	0.573	0.734
B. Child Subsample						
Trouble finding a doctor	0.021	0.008	0.022	0.008	0.020	0.008
No usual place of care	0.031	0.021	0.034	0.022	0.026	0.019
School days missed (age $\leq$ 10)	3.433	2.887	3.516	2.933	3.292	2.792
School days missed (age $>$ 10)	4.428	3.221	4.745	3.302	3.895	3.058
14+ school days missed (age $\leq$ 10)	0.044	0.022	0.046	0.023	0.042	0.021
14+ school days missed (age $>$ 10)	0.064	0.032	0.070	0.034	0.053	0.028
C. Adult Subsample						
Not accepting new patients	0.055	0.016	0.062	0.017	0.049	0.015
Not accepting patient's insurance	0.075	0.022	0.082	0.025	0.069	0.020
Work days missed	4.929	3.730	5.010	3.711	4.798	3.767

Notes: The reported statistics are weighted using the sample weights provided in the NHIS. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1.

Table 3: Summary Statistics: Individual and County Controls

	All	Medicaid	Private
Individual-level controls			
Demographics			
Male	0.489	0.439	0.489
Average age	0.373	0.242	0.384
Black	0.132	0.252	0.097
Hispanic	0.167	0.296	0.101
U.S. citizen	0.927	0.936	0.959
Education			
< High school	0.135	0.307	0.058
High school or GED	0.255	0.307	0.218
Some college	0.190	0.179	0.194
Associate's degree	0.107	0.079	0.120
Bachelor's degree	0.181	0.049	0.246
Master's, professional, or Ph.D.	0.097	0.013	0.139
Family structure			
Married	0.582	0.400	0.666
Live with partner	0.055	0.049	0.045
No children	0.479	0.229	0.503
1 child	0.176	0.193	0.179
2 children	0.191	0.243	0.197
3 children	0.099	0.185	0.086
4 children	0.036	0.090	0.025
5+ children	0.019	0.059	0.010
Income and wealth			
Welfare	0.127	0.483	0.035
Homeowner	0.334	0.645	0.223
Income to poverty line: <1	0.138	0.475	0.036
Income to poverty line: 1-1.99	0.166	0.285	0.097
Income to poverty line: 2-3.99	0.250	0.109	0.286
Income to poverty line: 4+	0.299	0.025	0.436
County-level controls			
Population	1,126,919	1,284,943	1,050,948
Population density	2,010	3,087	1,834
Median income	53,749	50,031	55,408
Unemployment rate	0.083	0.087	0.081
Medicaid eligibles	286,546	362,920	255,757
Expansion state (2014)	0.093	0.118	0.096
Number of pediatricians	234	265	222
Number of primary care doctors	876	969	838
Number of nurse practitioners	401	447	386
Number of hospital beds	3,254	3,750	3,037
Observations	603,074	96,128	338,174

Notes: All statistics are in percentages unless otherwise specified; some categories do not sum to one due to missing responses. Individual-level statistics are weighted using the sample weights provided in the NHIS. County-level controls come from the HRSA's Area Resource Files.

Table 4: Effects of Medicaid Payments on Access, Use, and Health

A. Full Sample	Medicaid			Private		
	(1) Office Visit (2 Weeks)	(2) Health ≤ Fair	(3) Health ≥ Very Good	(4) Office Visit (2 Weeks)	(5) Health ≤ Fair	(6) Health ≥ Very Good
Medicaid payments ($10s)	0.0028* (0.0017)	-0.0031* (0.0017)	0.0062*** (0.0023)	-0.0011 (0.0010)	0.0002 (0.0006)	0.0020 (0.0017)
Observations	96,017	96,067	96,067	337,506	337,903	337,903
R^2	0.071	0.296	0.232	0.036	0.079	0.138
Baseline mean	0.197	0.176	0.562	0.175	0.062	0.726

B. Child Subsample	Medicaid				Private			
	(1) Trouble Finding MD	(2) No Usual Place of Care	(3) 14+ School Absences (Age≤10)	(4) 14+ School Absences (Age≥11)	(5) Trouble Finding MD	(6) No Usual Place of Care	(7) 14+ School Absences (Age≤10)	(8) 14+ School Absences (Age≥11)
Medicaid payments ($10s)	-0.0054*** (0.0015)	-0.0036* (0.0018)	-0.0065** (0.0030)	0.0019 (0.0038)	0.0013* (0.0007)	0.0000 (0.0010)	0.0012 (0.0028)	0.0003 (0.0026)
Observations	16,752	21,221	6,665	6,766	26,277	33,994	10,079	14,337
R^2	0.016	0.022	0.036	0.047	0.007	0.029	0.025	0.021
Baseline mean	0.022	0.034	0.046	0.070	0.008	0.022	0.023	0.034

C. Adult Subsample	Medicaid			Private		
	(1) Not Accepting New Patients	(2) Not Accepting Patient's Insurance	(3) Work Days Missed	(4) Not Accepting New Patients	(5) Not Accepting Patient's Insurance	(6) Work Days Missed
Medicaid payments ($10s)	-0.0082*** (0.0015)	-0.0089*** (0.0030)	-0.1638 (0.3737)	0.0005 (0.0006)	-0.0007 (0.0007)	0.1047 (0.1147)
Observations	14,806	14,805	6,298	79,812	79,802	76,971
R^2	0.037	0.039	0.076	0.006	0.009	0.009
Baseline mean	0.062	0.082	5.010	0.017	0.025	3.711

Notes: Observations are at the individual level; standard errors are clustered by state. All regressions include state and quarter-year fixed effects and all individual and county-level controls listed in Table 3 (with age in five-year bins). Regressions are weighted using the sample weights provided in the NHIS. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1. Fewer children report days of missed school in the past year relative to other child outcomes because a child must be at least five years old to be asked this question. Similarly, only adults with employment histories are asked how many days of work they missed in the past year.

Table 5: Effects of Medicaid Payments on Disparities between Publicly and Privately Insured

	Baseline Means					Share of Disparity Closed by			
	Medicaid	Private	Disparity: (2) - (1)	Effect of $10 Increase	Implied Elasticity	$10	$35	$45	Increase to Erase Disparity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Full Sample									
Office visit (2 weeks)	0.197	0.175	-0.022	0.0028	0.11	NA	NA	NA	NA
Health: poor or fair	0.176	0.062	-0.114	-0.0031	-0.13	0.03	0.10	0.12	$367.74
Health: excellent or very good	0.562	0.726	0.164	0.0062	0.08	0.04	0.13	0.17	$264.52
B. Child Subsample									
Trouble finding MD	0.022	0.008	-0.014	-0.0054	-1.87	0.39	1.35	1.74	$25.93
No usual place of care	0.034	0.022	-0.012	-0.0036	-0.81	0.30	1.05	1.35	$33.33
14+ school days missed	0.046	0.023	-0.023	-0.0065	-1.08	0.28	0.99	1.27	$35.38
C. Adult Subsample									
Not accepting new patients	0.062	0.017	-0.045	-0.0082	-1.01	0.18	0.64	0.82	$54.88
Not acc. patient's insurance	0.082	0.025	-0.057	-0.0089	-0.83	0.16	0.55	0.70	$64.04

Notes: The above table displays the effects of changes in Medicaid payments on disparities in outcomes between Medicaid beneficiaries and privately insured patients. Columns (1) and (2) reproduce the averages from Table 2; column (3) is the difference in these means. Column (4) reproduces our main estimates from Table 4. Column (5) is the implied elasticity of each outcome with respect to physician payments; this is computed using a median baseline payment of $76 under Medicaid for an office visit of mid-level complexity ((5) = [(4)/(1)]/(10/76)]). Columns (6)–(8) show how much of the disparity from column (3) would be reduced by increasing Medicaid payments by $10, $35 (median payment increase from federal mandate), and $45 (median payment difference between Medicaid and private insurance), respectively. Column (9) shows the implied increase in Medicaid payments that would be required to eliminate the disparity in each outcome between Medicaid patients and patients with private insurance ((9) = [(3)/(4)]*10). Office visits are excluded from columns (6)–(9) because Medicaid beneficiaries have more office visits than patients with private insurance.

Table 6: Effects of Medicaid Payments on School Absences: NAEP

A. Free Lunch Eligible	0 days missed (%)		3+ days missed (%)	
	(1) Grade 4	(2) Grade 8	(3) Grade 4	(4) Grade 8
Medicaid payments ($10s)	0.0028*	0.0013	-0.0034***	-0.0002
	(0.0016)	(0.0011)	(0.0006)	(0.0007)
Observations	750,170	686,070	750,170	686,070
R^2	0.020	0.026	0.006	0.013
Baseline mean	0.457	0.401	0.130	0.145

B. Free Lunch Ineligible	0 days missed (%)		3+ days missed (%)	
	(1) Grade 4	(2) Grade 8	(3) Grade 4	(4) Grade 8
Medicaid payments ($10s)	0.0016	-0.0002	0.0003	-0.0007
	(0.0013)	(0.0014)	(0.0008)	(0.0008)
Observations	622,070	665,060	622,070	665,060
R^2	0.015	0.019	0.004	0.008
Baseline mean	0.538	0.467	0.099	0.109

Notes: Observations are at the individual level; standard errors are clustered by state. All regressions include state and year fixed effects and individual demographic controls (sex, age, race, ethnicity). Regressions are weighted using the sample weights provided in the NAEP. School days missed reflect absenteeism (for any reason) in the month preceding national math and reading assessments. Data are from the 2009, 2011, and 2013 National Assessment of Educational Progress.

Table 7: Effects of Medicaid Payments: Triple Difference Model

A. Full Sample	(1) Office Visit (2 Weeks)	(2) Health: Poor or Fair	(3) Health: Excellent or Very Good
Medicaid payments ($10s)	-0.0012	0.0002	0.0022
	(0.0010)	(0.0006)	(0.0017)
𝟙{Medicaid}	-0.0857**	0.1299***	-0.2784***
	(0.0377)	(0.0411)	(0.0553)
Medicaid payments * 𝟙{Medicaid}	0.0040**	-0.0033*	0.0040
	(0.0018)	(0.0019)	(0.0031)
Observations	430,800	431,244	431,244
R^2	0.044	0.178	0.175
Baseline mean	0.179	0.082	0.698

B. Child Subsample	(1) Trouble Finding MD	(2) No Usual Place of Care	(3) 14+ School Days Missed (Age≤10)	(4) 14+ School Days Missed (Age≥11)
Medicaid payments ($10s)	0.0014*	-0.0001	0.0018	0.0001
	(0.0007)	(0.0010)	(0.0022)	(0.0024)
𝟙{Medicaid}	0.1337***	-0.0491	0.0646	-0.0361
	(0.0297)	(0.0455)	(0.0683)	(0.0860)
Medicaid payments * 𝟙{Medicaid}	-0.0068***	-0.0035	-0.0083***	0.0018
	(0.0016)	(0.0024)	(0.0029)	(0.0037)
Observations	42,541	54,602	16,544	21,473
R^2	0.016	0.027	0.036	0.037
Baseline mean	0.013	0.026	0.029	0.042

C. Adult Subsample	(1) Not Accepting New Patients	(2) Not Accepting Patient's Insurance	(3) Work Days Missed
Medicaid payments ($10s)	0.0007	-0.0006	0.0998
	(0.0006)	(0.0006)	(0.1158)
𝟙{Medicaid}	0.1082**	0.0505	-1.2433
	(0.0471)	(0.0728)	(6.5189)
Medicaid payments * 𝟙{Medicaid}	-0.0088***	-0.0082***	-0.2636
	(0.0016)	(0.0029)	(0.4207)
Observations	94,162	94,150	83,054
R^2	0.025	0.029	0.018
Baseline mean	0.022	0.031	3.785

Notes: Observations are at the individual level; standard errors are clustered by state. All regressions include state and quarter-year fixed effects and all individual and county-level controls listed in Table 3 (with age in five-year bins). We allow the associations between the controls and each outcome to differ for Medicaid beneficiaries and patients with private insurance; refer to Table A4 for results from specifications without interacted controls. Regressions are weighted using the sample weights provided in the NHIS. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1. Fewer children report days of missed school in the past year relative to other child outcomes because a child must be at least five years old to be asked this question. Similarly, only adults with employment histories are asked how many days of work they missed in the past year.

Online Appendix

The Impacts of Physician Payments on Patient Access, Use, and Health

Alexander and Schnell (2020)

A Data Appendix

A.1 Outcomes

Table A1: Overview of Data Sources used for Outcome Measures

Outcome	Data Source	Years Available	Look Back Period	Payment Variable	Sample
Office visit	NHIS	2009-2015	Past 2 weeks	Avg. rate in interview quarter	Full sample
Health: excellent or very good	NHIS	2009-2015	Not specified	Avg. rate in interview quarter	Full sample
Health: poor or fair	NHIS	2009-2015	Not specified	Avg. rate in interview quarter	Full sample
Trouble finding MD	NHIS	2011-2015	Past 12 months	Avg. rate over past 12 months	Child subsample
No usual place of care	NHIS	2009-2015	Not specified	Avg. rate over past 12 months	Child subsample
School days missed	NHIS	2009-2015	Past 12 months	Avg. rate over past 12 months	Child subsample
Not accepting new patients	NHIS	2011-2015	Past 12 months	Avg. rate over past 12 months	Adult subsample
Not acc. patient's insurance	NHIS	2011-2015	Past 12 months	Avg. rate over past 12 months	Adult subsample
Work days missed	NHIS	2009-2015	Past 12 months	Avg. rate over past 12 months	Adult subsample
School absences	NAEP	2009, 2011, 2013	30 days before test	Avg. rate in first quarter	4th and 8th grade math and reading
Test scores	NAEP	2009, 2011, 2013	Testing occurs in Q1	Avg. rate in first quarter	4th and 8th grade math and reading

National Health Interview Survey questions

- **Full Sample (from Family File)**
  - During the last two weeks, did {person} see a doctor or other health care professional at a doctor's office, a clinic, an emergency room, or some other place? (Do not include times during an overnight hospital stay.)
  - Would you say {person's} health in general is excellent, very good, good, fair, or poor?
- **Child Subsample**
  - During the past 12 months, did you have any trouble finding a general doctor or provider who would see {sample child}?
  - Is there a place that {sample child} usually goes when {he/she} is sick or you need advice about {his/her} health?
  - During the past 12 months, that is, since {12-month ref. date}, about how many days did {sample child} miss school because of illness or injury?
- **Adult Subsample**
  - During the past 12 months, were you told by a doctor's office or clinic that they would not accept {sample adult} as a new patient?
  - During the past 12 months, were you told by a doctor's office or clinic that they would not accept {sample adult}'s health care coverage?
  - During the past 12 months, about how many days did {sample adult} miss work?

A.2 Medicaid Payments

We collected data on fee-for-service reimbursement rates directly from state Medicaid offices. The data have two components: (1) standard fee-for-service rates applicable in 2009–2015 for all providers and (2) augmented fee-for-service rates applicable in 2013–2014 (and 2015, depending on the state) for qualifying physicians in family medicine, general internal medicine, and pediatric medicine. In constructing our state-quarter panel of payments, we use standard rates in 2009–2012, augmented rates in 2013–2014, and either the standard or augmented rates in 2015 depending on whether a given state extended the primary care rate increase. 44 states and the District of Columbia provided us with complete, quarterly rate information used to construct this panel. For the remaining six states, we use the following procedures to impute missing rate information:

- California: We only have the standard rates for 2009 and 2015. As the standard rates were the same in 2009 and 2015, we assume that they did not change over this period and pull forward the standard rates to 2012.

- Hawaii: We only have the standard rates for 2009, 2012, and 2015. As the standard rates were the same in 2009 and 2012, we assume that they did not change over this period and pull forward the standard rates to 2011.

- New Mexico: We are missing standard rates for January–November 2009. The rates changed over this period; we impute the missing months with the rate in the nearest month with non-missing rate information.

- Utah: We are missing standard rates for January–May 2009 and July–December 12. We impute the missing months with the rate in the nearest month with non-missing rate information.

- South Dakota: Standard rates are not archived, so we only have standard rates for 2015. We impute standard rates from 2009–2012 such that the change in reimbursement rates between each quarter and 2015 reflects the average change in reimbursement rates for neighboring states (MT, ND, MN, IA, NE, WY) over the same period.

- Tennessee: We have no micro-data on reimbursement rates, as the state only uses

Medicaid managed care. However, we were told by the state that average reimbursements increased by 44 percent as a result of the primary care rate increase. We impute reimbursement rates for Tennessee in 2013 and 2014 by averaging the 2013 and 2014 augmented rates for neighboring states (MO, KY, VA, NC, GA, AL, MS, AR). We then apply the 44 percent increase from 2012 to 2013 to impute the rates for 2012. For 2009–2012 and 2015, we calculate the average change in physician payments across neighboring states in the relevant period and apply this rate change to Tennessee over the same window.

A.3 Medicaid Managed Care

The primary care rate increase applied to both Medicaid fee-for-service and Medicaid managed care programs. While states could just increase fee-for-service reimbursement rates for the covered services to comply with the mandated higher rates, determining how to increase reimbursement rates for physicians treating patients enrolled in Medicaid managed care was more complicated. To ensure that Medicaid managed care programs complied with the rate increase, each state's Medicaid program was required to submit proposals to CMS that outlined methodologies for:

1. Identifying the proportion of the capitation payments made by the state to its contracted MCOs in 2009 that was spent on each of the applicable primary care services, as well as the per-unit cost of each of these services. These baseline costs were used to calculate the refunds that each state's Medicaid program was eligible to receive from the federal government in 2013 and 2014.

2. Developing a "model" that incorporated the increased fees for primary care services into the state's 2013 and 2014 capitation payments to MCOs. It was recommended that states implemented one of three types of models:

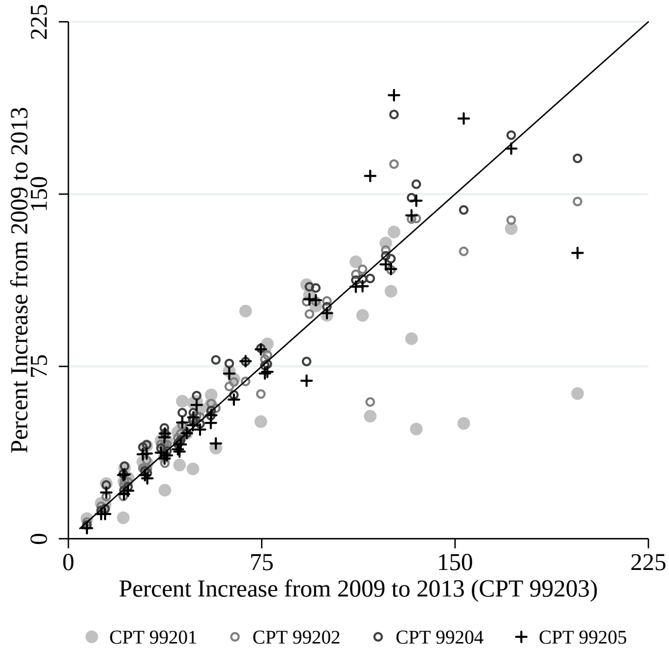
    - Model 1: "Full-risk prospective capitation" in which states incorporated increased fees directly into their capitation payments to MCOs for 2013 and 2014.

- Model 2: "Prospective capitation with risk-sharing that incorporates retrospective reconciliation" in which increased fees were built into states' capitation payments for 2013 and 2014 (similar to Model 1), but capitation payments were to be adjusted at the end of an agreed-upon time period to reflect actual utilization and costs (states and MCOs engage in "retrospective reconciliation").
- Model 3: "Non-risk reconciled payments for enhanced rates" in which states' initial capitation payments to MCOs for 2013 and 2014 did not incorporate increased fees. Instead, MCOs submitted encounter data to the state at the end of the quarter, year, etc., and the state sent an additional payment to the MCOs to cover the costs of the increased fees.

CMS had to sign off on each state's methodology for determining the 2009 rates and on its plan for implementing the rate increase for eligible physicians treating managed care enrollees. According to CMS, at least 21 states opted to receive the increased funding in lump-sum payments based on encounter data (Model 3). The rest of the states incorporated the increased fees directly into their capitation payments (Models 1 and 2); most of these states did not engage in any retrospective reconciliation based on actual utilization data.

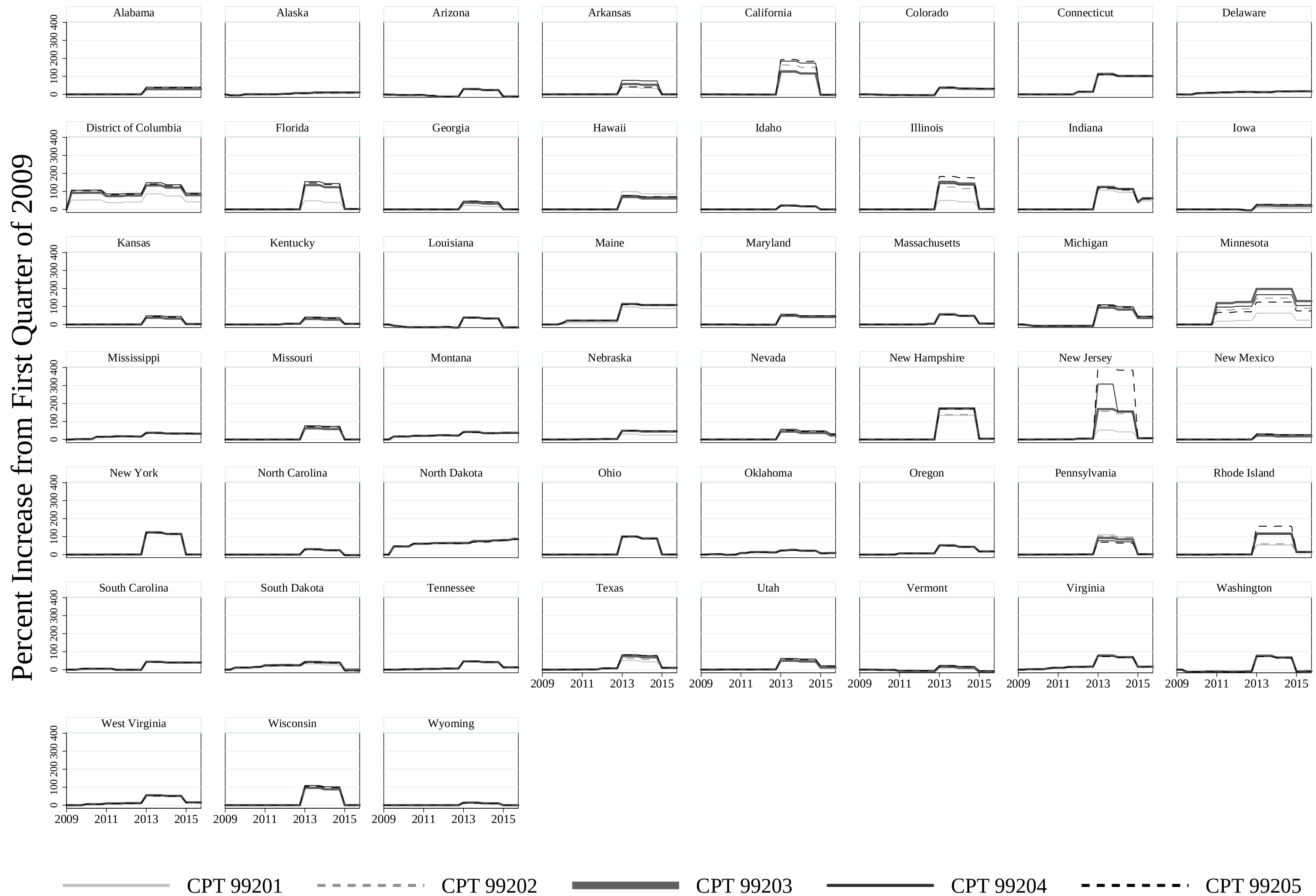
B Supplementary Figures

Figure A1: Increases in Medicaid Payments from 2009 to 2013 Across New Patient Codes



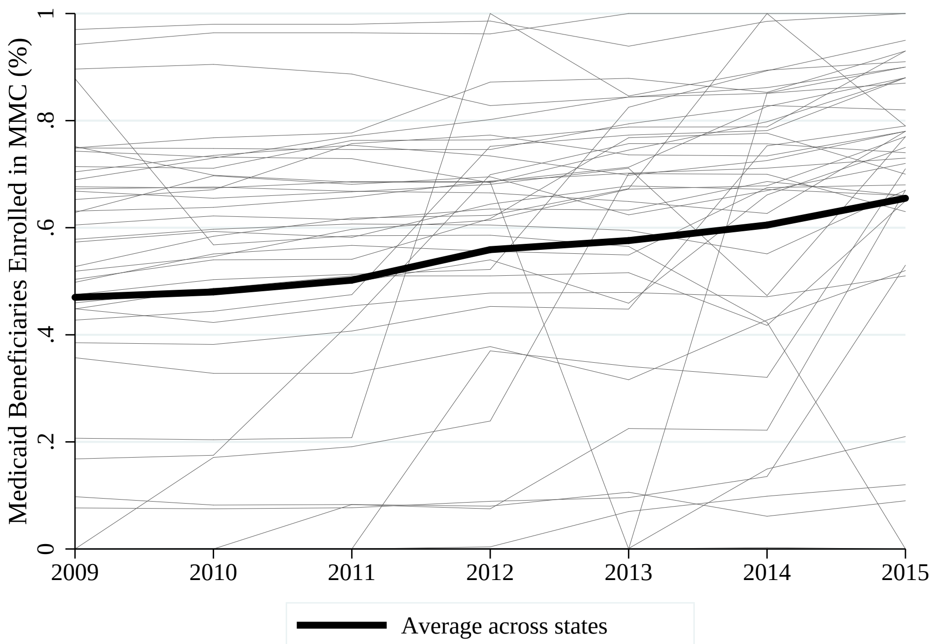
Notes: The above figure displays state-level changes in Medicaid payments for CPT codes 99201, 99202, 99204, and 99205 from 2009 to 2013 versus state-level changes in Medicaid payments for CPT code 99203 (the CPT code used in the majority of our analyses) over the same period. The black line is the 45-degree line. The figure excludes CPT codes 99204 and 99204 for New Jersey; payments for these codes increased by 308 percent and 404 percent, respectively, while payment for CPT code 99203 increased by 169 percent.

Figure A2: Medicaid Payments Across New Patient Codes by State



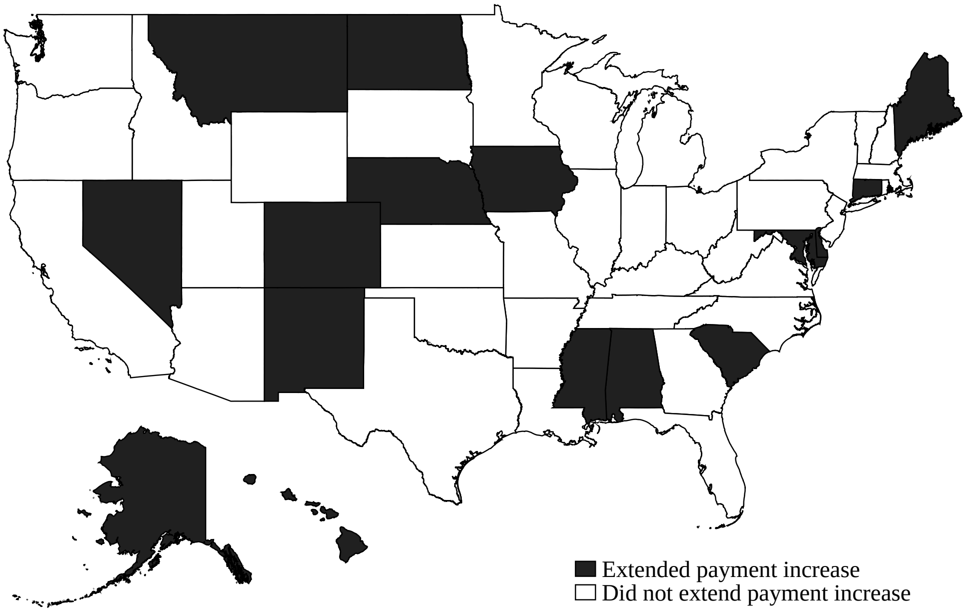
Notes: The above figure displays Medicaid payments by states for CPT codes 99201, 99202, 99203, 99204, and 99205 from 2009 to 2015. The payments for each CPT code in each state are normalized to their level in the first quarter of 2009.

Figure A3: State-Level Medicaid Managed Care Penetration over Time



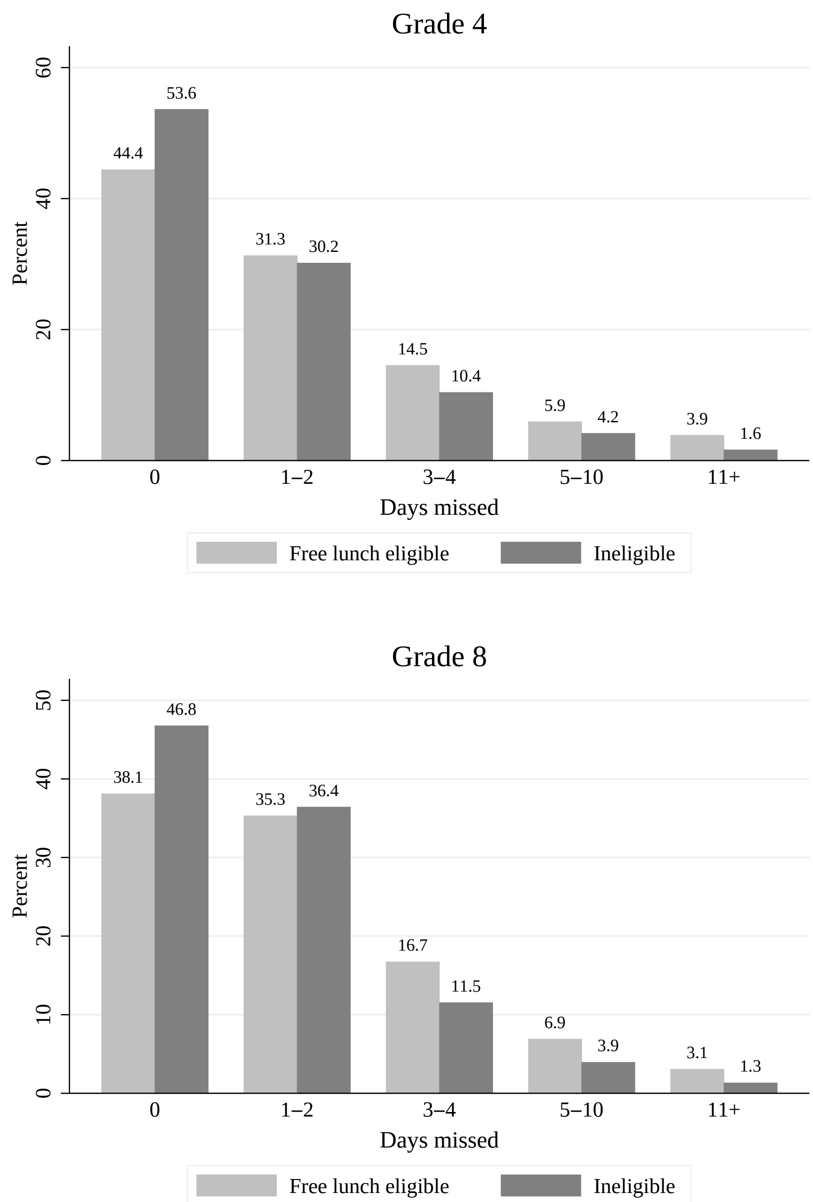
Notes: The above figure depicts the fraction of Medicaid beneficiaries enrolled in comprehensive risk-based managed care in each state from 2009 to 2015. The black line depicts the national average. Data for 2009 through 2014 come from CMS; data for 2015 comes from the Henry J. Kaiser Family Foundation. In 2014, eleven states had less than one percent of their Medicaid beneficiaries enrolled in managed care plans: Alabama, Alaska, Arkansas, Connecticut, Idaho, Maine, Montana, North Carolina, Oklahoma, South Dakota, and Wyoming. In the same year, nine states had more than 85 percent of their Medicaid beneficiaries enrolled in such plans: Arizona, Delaware, Hawaii, Kansas, Kentucky, New Hampshire, New Jersey, Tennessee, and Washington.

Figure A4: States that Extended the Medicaid Primary Care Rate Increase Past 2014



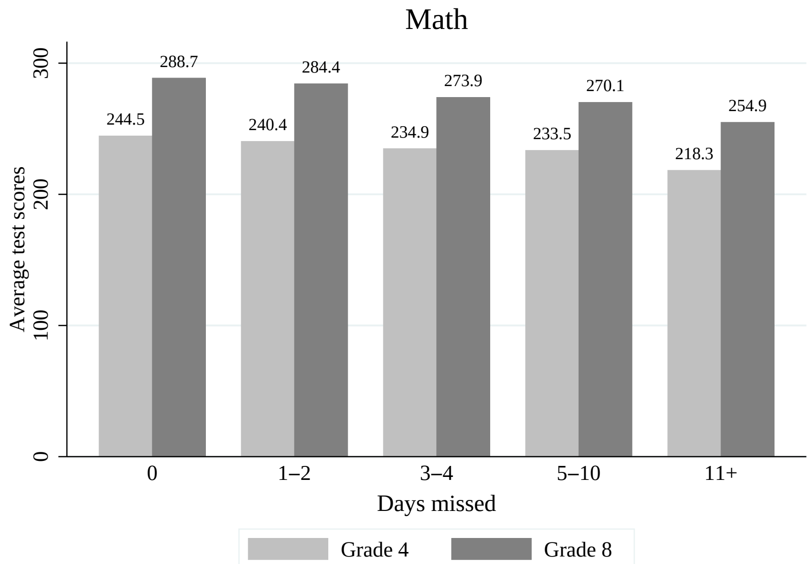
Notes: The above map depicts whether states chose to maintain the primary care rate increase after the federal mandate expired in 2014: shaded states extended higher Medicaid payment rates into 2015.

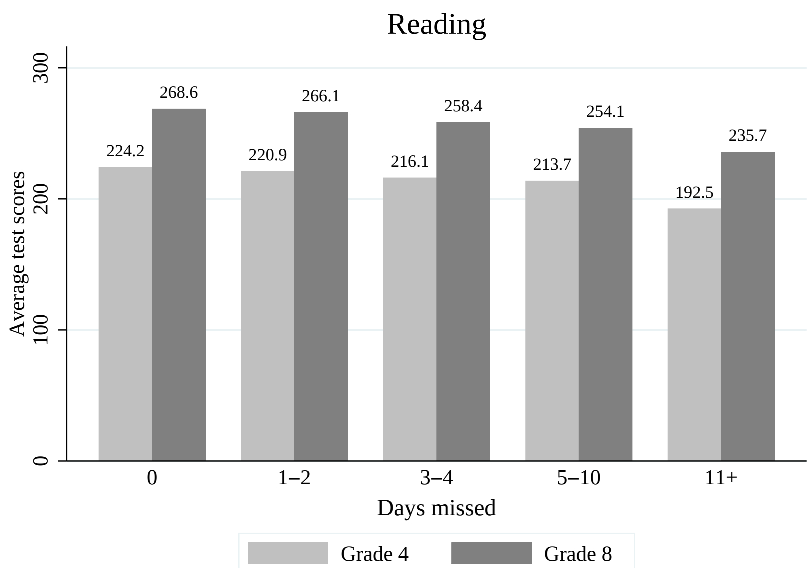
Figure A5: Distribution of School Absences by Free Lunch Eligibility: NAEP



Notes: The above figures display the average percentage of students who missed 0, 1–2, 3–4, 5–10, or 11+ days in the month preceding their national math and reading assessments in 2009, 2011, and 2013. Data come from the National Assessment of Educational Progress.

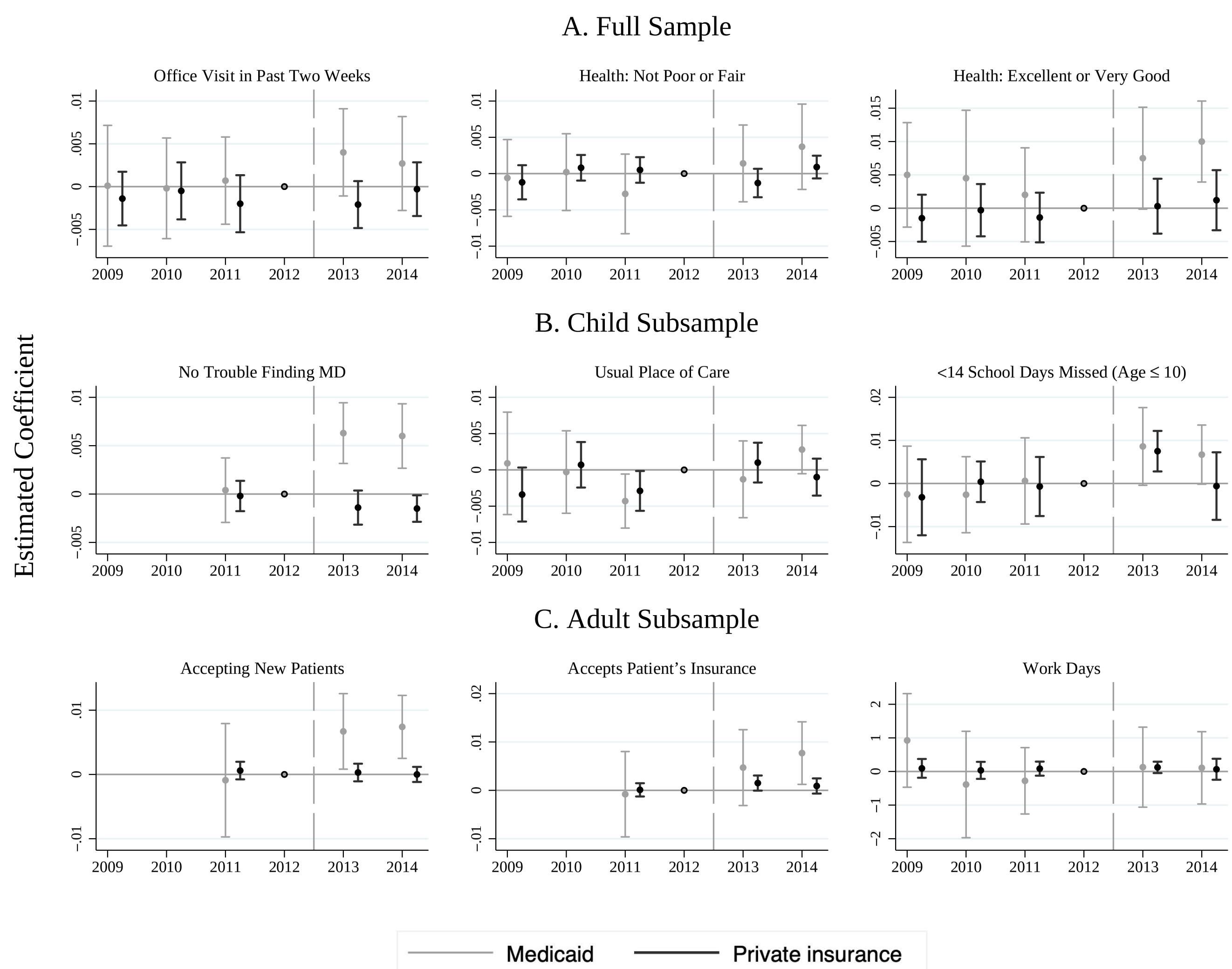
Figure A6: Average Test Scores by School Absences: NAEP





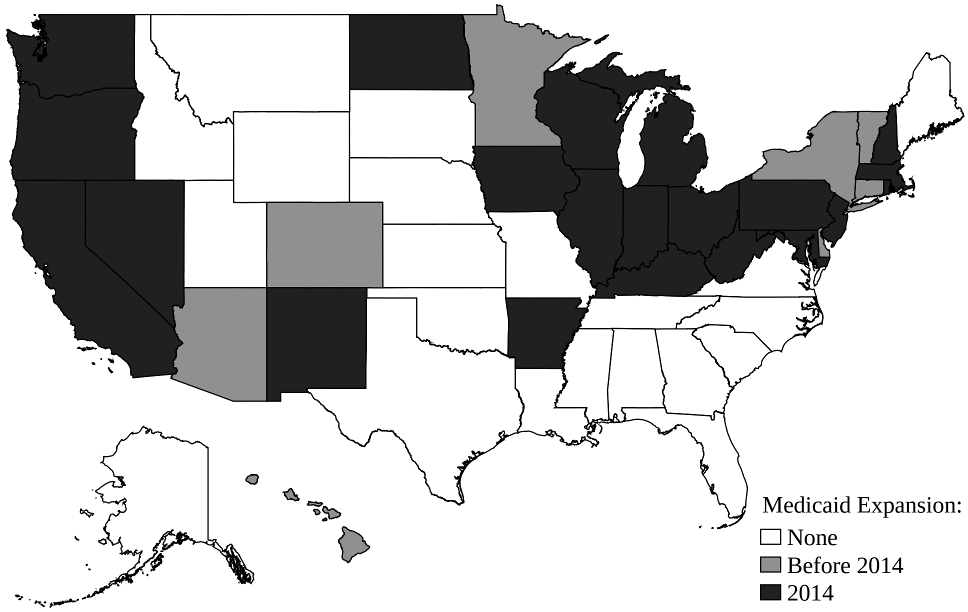
Notes: The above figures display the average test scores of students who missed 0, 1–2, 3–4, 5–10, or 11+ days in the month preceding their national assessment in 2009, 2011, and 2013. Data come from the National Assessment of Educational Progress.

Figure A7: Event Study: Effects of Medicaid Payments on Access, Use, and Health



Notes: The above figures plot the coefficients and 95% confidence intervals on year indicators interacted with state-level changes in Medicaid payments induced by the primary care rate increase from estimation of Equation (1). We run separate regressions for patients with Medicaid and patients with private insurance. Outcomes are adjusted such that higher values denote better outcomes. Outcomes with missing coefficients in 2009 and 2010 were added to the NHIS in 2011. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1.

Figure A8: States that Expanded Medicaid



Notes: The above map depicts whether states expanded their Medicaid programs: the dark-shaded states participated in the ACA Medicaid expansion in 2014, the light-shaded states expanded their Medicaid program prior to the ACA, and the remaining states did not participate in any type of Medicaid expansion by 2014. Data are from Leung and Mas (2018).

C Supplementary Tables

Table A2: Effects of Medicaid Payments on School Days Missed (Continuous Measure)

Child Subsample	Medicaid		Private	
	(1) School Days Missed (Age≤10)	(2) School Days Missed (Age≥11)	(3) School Days Missed (Age≤10)	(4) School Days Missed (Age≥11)
Medicaid payments ($10s)	-0.2256**	-0.0702	0.0014	0.0649
	(0.1022)	(0.2251)	(0.0463)	(0.1018)
Observations	6,687	6,794	10,168	15,046
R^2	0.043	0.062	0.033	0.029
Baseline mean	3.516	4.745	2.933	3.302

Notes: Observations are at the individual level; standard errors are clustered by state. All regressions include state and quarter-year fixed effects and all individual and county-level controls listed in Table 3 (with age in five-year bins). Regressions are weighted using the sample weights provided in the NHIS. School days missed reflect absenteeism due to illness or injury over the previous year.

Table A3: Effects of Medicaid Payments on School Absences and Test Scores: State-level NAEP

A. School Absences	Free Lunch Eligible				Free Lunch Ineligible			
	0 days missed (%)		3+ days missed (%)		0 days missed (%)		3+ days missed (%)	
	(1) Grade 4	(2) Grade 8	(3) Grade 4	(4) Grade 8	(5) Grade 4	(6) Grade 8	(7) Grade 4	(8) Grade 8
Medicaid payments ($10s)	0.00315*	0.00593**	-0.00420**	-0.00249	0.00180	0.00156	0.00112	-0.000562
	(0.00174)	(0.00232)	(0.00157)	(0.00183)	(0.00210)	(0.00222)	(0.00163)	(0.00129)
Observations	150	150	150	150	150	150	150	150
R^2	0.952	0.952	0.913	0.933	0.852	0.907	0.788	0.897
Baseline mean	0.444	0.381	0.243	0.267	0.536	0.468	0.162	0.168

B. Test Scores	Free Lunch Eligible				Free Lunch Ineligible			
	0 days missed (%)		3+ days missed		0 days missed		3+ days missed (%)	
	(1) Grade 4	(2) Grade 8	(3) Grade 4	(4) Grade 8	(5) Grade 4	(6) Grade 8	(7) Grade 4	(8) Grade 8
Medicaid payments ($10s)	-0.0551	0.0367	-0.0373	0.0150	-0.0803	0.0291	-0.0649*	-0.00418
	(0.0361)	(0.0491)	(0.0551)	(0.0558)	(0.0543)	(0.0423)	(0.0373)	(0.0545)
Observations	150	150	150	150	150	150	150	150
R^2	0.959	0.943	0.927	0.886	0.925	0.950	0.950	0.939

Notes: Observations are at the state-year level; standard errors are clustered by state. All regressions include state and year fixed effects and state-level analogs of the controls listed in Table 3. Regressions are weighted by state population. In Panel A, outcomes are the fraction of students missing a given amount of school (for any reason) in the month preceding their national math and reading assessments; in Panel B, outcomes are average test scores standardized across the population included in a given regression. Data are from the 2009, 2011, and 2013 National Assessment of Educational Progress.

Table A4: Effects of Medicaid Payments: Triple Difference without Interacted Controls

A. Full Sample	(1) Office Visit (2 Weeks)	(2) Health: Poor or Fair	(3) Health: Excellent or Very Good
Medicaid payments ($10s)	-0.0003	0.0001	0.0022
	(0.0009)	(0.0005)	(0.0015)
𝟙{Medicaid}	0.0690***	0.1137***	-0.1359***
	(0.0107)	(0.0073)	(0.0136)
Medicaid payments * 𝟙{Medicaid}	-0.0007	-0.0024***	0.0035**
	(0.0008)	(0.0007)	(0.0015)
Observations	430,800	431,244	431,244
R^2	0.040	0.128	0.163
Baseline mean	0.179	0.082	0.698

B. Child Subsample	(1) Trouble Finding MD	(2) No Usual Place of Care	(3) 14+ School Days Missed (Age≤10)	(4) 14+ School Days Missed (Age≥11)
Medicaid payments ($10s)	-0.0006	-0.0011	-0.0008	0.0018
	(0.0008)	(0.0008)	(0.0023)	(0.0022)
𝟙{Medicaid}	0.0267***	0.0043	0.0376***	0.0541***
	(0.0054)	(0.0065)	(0.0109)	(0.0160)
Medicaid payments * 𝟙{Medicaid}	-0.0018***	-0.0009	-0.0023**	-0.0032*
	(0.0005)	(0.0007)	(0.0012)	(0.0016)
Observations	42,541	54,602	16,544	21,473
R^2	0.010	0.021	0.023	0.026
Baseline mean	0.013	0.026	0.029	0.042

C. Adult Subsample	(1) Not Accepting New Patients	(2) Not Accepting Patient's Insurance	(3) Work Days Missed
Medicaid payments ($10s)	0.0002	-0.0010	0.0964
	(0.0007)	(0.0008)	(0.1094)
𝟙{Medicaid}	0.0691***	0.0887***	1.9949*
	(0.0123)	(0.0118)	(1.1576)
Medicaid payments * 𝟙{Medicaid}	-0.0042***	-0.0046***	-0.1241
	(0.0011)	(0.0011)	(0.1108)
Observations	94,162	94,150	83,054
R^2	0.016	0.020	0.009
Baseline mean	0.022	0.031	3.785

Notes: Observations are at the individual level; standard errors are clustered by state. All regressions include state and quarter-year fixed effects and all individual and county-level controls listed in Table 3 (with age in five-year bins). In contrast to the specification used in Table 7, we do not interact the time fixed effects or the demographic controls with insurance type in these regressions; that is, we assume that the associations between these variables and each outcome are the same for Medicaid beneficiaries and patients with private insurance. Regressions are weighted using the sample weights provided in the NHIS. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1. Fewer children report days of missed school in the past year relative to other child outcomes because a child must be at least five years old to be asked this question. Similarly, only adults with employment histories are asked how many days of work they missed in the past year.

Table A5: Effects of Medicaid Payments: States that Extended the Rate Increase

A. Full Sample	Medicaid			Private		
	(1) Office Visit (2 Weeks)	(2) Health ≤ Fair	(3) Health ≥ Very Good	(4) Office Visit (2 Weeks)	(5) Health ≤ Fair	(6) Health ≥ Very Good
MC payments ($10s)	0.0026	-0.0028*	0.0063***	-0.0009	0.0001	0.0021
	(0.0017)	(0.0017)	(0.0022)	(0.0010)	(0.0006)	(0.0017)
𝟙{Extended}	-0.0991**	-0.0254	-0.2376***	-0.0487***	0.0118	-0.0587**
	(0.0374)	(0.0395)	(0.0538)	(0.0138)	(0.0083)	(0.0236)
Payments * 𝟙{Extended}	-0.0018	0.0027	0.0008	0.0019**	-0.0003	0.0010
	(0.0020)	(0.0023)	(0.0033)	(0.0008)	(0.0005)	(0.0014)
Observations	96,017	96,067	96,067	337,506	337,903	337,903
R^2	0.071	00.296	0.232	0.036	0.079	0.138
Baseline mean	0.197	0.176	0.562	0.175	0.062	0.726

B. Child Subsample	Medicaid				Private			
	(1) Trouble Finding MD	(2) No Usual Place of Care	(3) 14+ School Absences (Age≤10)	(4) 14+ School Absences (Age≥11)	(5) Trouble Finding MD	(6) No Usual Place of Care	(7) 14+ School Absences (Age≤10)	(8) 14+ School Absences (Age≥11)
MC payments ($10s)	-0.0054***	-0.0034*	-0.0074**	0.0010	0.0012*	0.0000	0.0010	0.0000
	(0.0015)	(0.0019)	(0.0032)	(0.0040)	(0.0007)	(0.0010)	(0.0028)	(0.0027)
𝟙{Extended}	0.0324	-0.0199	0.1609	-0.0234	-0.0259***	0.0666***	0.0581	-0.0393
	(0.0209)	(0.0327)	(0.0974)	(0.0802)	(0.0080)	(0.0236)	(0.0371)	(0.0769)
Payments * 𝟙{Extended}	0.0002	0.0016	-0.0074	-0.0082*	-0.0002	-0.0001	-0.0017	-0.0024
	(0.0011)	(0.0017)	(0.0053)	(0.0045)	(0.0004)	(0.0012)	(0.0019)	(0.0041)
Observations	16,752	21,221	6,665	6,766	26,277	33,994	10,079	14,337
R^2	0.016	0.022	0.036	0.047	0.007	0.029	0.025	0.021
Baseline mean	0.022	0.034	0.046	0.070	0.008	0.022	0.023	0.034

C. Adult Subsample	Medicaid			Private		
	(1) Not Accepting New Patients	(2) Not Accepting Patient's Insurance	(3) Work Days Missed	(4) Not Accepting New Patients	(5) Not Accepting Patient's Insurance	(6) Work Days Missed
MC payments ($10s)	-0.0084***	-0.0088***	-0.2808	0.0005	-0.0006	0.1255
	(0.0014)	(0.0030)	(0.3704)	(0.0006)	(0.0007)	(0.1158)
𝟙{Extended}	0.1212***	0.0322	16.5395**	0.001	-0.0093	-4.4484**
	(0.0410)	(0.0699)	(6.0360)	(0.0133)	(0.0150)	(1.8844)
Payments * 𝟙{Extended}	-0.0022	0.0008	-0.7897**	-0.0003	0.0007	0.1825*
	(0.0024)	(0.0037)	(0.2961)	(0.0008)	(0.0008)	(0.0972)
Observations	14,806	14,805	6,298	79,812	79,802	76,971
R^2	0.037	0.039	0.076	0.006	0.009	0.009
Baseline mean	0.062	0.082	5.010	0.017	0.025	3.711

Notes: Observations are at the individual level; standard errors are clustered by state. All regressions include state and quarter-year fixed effects and all individual and county-level controls listed in Table 3 (with age in five-year bins). Regressions are weighted using the sample weights provided in the NHIS. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1. Fewer children report days of missed school in the past year relative to other child outcomes because a child must be at least five years old to be asked this question. Similarly, only adults with employment histories are asked how many days of work they missed in the past year.

Table A6: Effects of Medicaid Payments on Medicaid Spending

	Spending		Spending per Beneficiary	
	(1) Millions	(2) Logs	(3) Levels	(4) Logs
Medicaid payments ($10s)	141.3	0.00082	14.1	0.0049
	(95.1)	(0.0055)	(55.0)	(0.0074)
Observations	306	306	306	306
R^2	0.998	0.998	0.958	0.957
Baseline mean	16,677.5	23.1	6,811.3	8.80

Notes: Observations are at the state-year level; standard errors are clustered by state. All regressions include state and year fixed effects and state-level analogs of the controls listed in Table 3. Regressions are weighted by state population and use CMS's National Health Expenditure Data.

Table A7: Effects of Medicaid Payments: Primary Care Shortage Areas

A. Full Sample	Medicaid			Private		
	(1) Office Visit (2 Weeks)	(2) Health ≤ Fair	(3) Health ≥ Very Good	(4) Office Visit (2 Weeks)	(5) Health ≤ Fair	(6) Health ≥ Very Good
MC payments ($10)	0.0025	-0.0030*	0.0060**	-0.0008	0.0006	0.0016
	(0.0018)	(0.0017)	(0.0027)	(0.0010)	(0.0006)	(0.0018)
$\mathbb{1}${Shortage}	-0.0056	0.0148	-0.0242	0.0028	0.0087***	-0.0110*
	(0.0127)	(0.0113)	(0.0183)	(0.0046)	(0.0029)	(0.0063)
Payments * $\mathbb{1}${Shortage}	0.0008	-0.0007	0.0011	-0.0005	-0.0009**	0.0008
	(0.0017)	(0.0014)	(0.0022)	(0.0006)	(0.0004)	(0.0008)
Observations	96,017	96,067	96,067	337,506	337,903	337,903
R^2	0.071	00.296	0.232	0.036	0.079	0.138
Baseline mean	0.197	0.176	0.562	0.175	0.062	0.726

B. Child Subsample	Medicaid				Private			
	(1) Trouble Finding MD	(2) No Usual Place of Care	(3) 14+ School Absences (Age≤10)	(4) 14+ School Absences (Age≥11)	(5) Trouble Finding MD	(6) No Usual Place of Care	(7) 14+ School Absences (Age≤10)	(8) 14+ School Absences (Age≥11)
MC payments ($10)	-0.0056***	-0.0036*	-0.0052	0.0005	0.0012	0.0002	0.0015	0.0000
	(0.0017)	(0.0020)	(0.0034)	(0.0047)	(0.0008)	(0.0012)	(0.0027)	(0.0028)
$\mathbb{1}${Shortage}	-0.0081	-0.0001	0.0065	-0.0187	0.0010	0.0028	0.0095	-0.0086
	(0.0141)	(0.0115)	(0.0266)	(0.0309)	(0.0052)	(0.0059)	(0.0118)	(0.0110)
Payments * $\mathbb{1}${Shortage}	0.0006	0.0000	-0.0024	0.0026	0.0002	-0.0003	-0.0005	0.0007
	(0.0015)	(0.0012)	(0.0028)	(0.0037)	(0.0006)	(0.0007)	(0.0014)	(0.0013)
Observations	16,752	21,221	6,665	6,766	26,277	33,994	10,079	14,337
R^2	0.016	0.022	0.036	0.047	0.007	0.029	0.026	0.021
Baseline mean	0.022	0.034	0.046	0.070	0.008	0.022	0.023	0.034

C. Adult Subsample	Medicaid			Private		
	(1) Not Accepting New Patients	(2) Not Accepting Patient's Insurance	(3) Work Days Missed	(4) Not Accepting New Patients	(5) Not Accepting Patient's Insurance	(6) Work Days Missed
MC payments ($10)	-0.0084***	-0.0077**	-0.2740	0.0004	-0.0005	0.1710
	(0.0017)	(0.0030)	(0.3170)	(0.0007)	(0.0008)	(0.1273)
$\mathbb{1}${Shortage}	-0.0212	-0.0002	-2.3331	-0.0026	0.0048	0.7920
	(0.0128)	(0.0168)	(2.8151)	(0.0051)	(0.0075)	(0.7211)
Payments * $\mathbb{1}${Shortage}	0.0008	-0.0020	0.2592	0.0002	-0.0003	-0.1233
	(0.0012)	(0.0016)	(0.3144)	(0.0006)	(0.0008)	(0.0841)
Observations	14,806	14,805	6,298	79,812	79,802	76,971
R^2	0.038	0.040	0.076	0.006	0.009	0.010
Baseline mean	0.062	0.082	5.010	0.017	0.025	3.711

Notes: Observations are at the individual level; standard errors are clustered by state. All regressions include state and quarter-year fixed effects and all individual and county-level controls listed in Table 3 (with age in five-year bins). Regressions are weighted using the sample weights provided in the NHIS. School days missed reflect absenteeism due to illness or injury over the previous year; the exact survey questions and the corresponding reference windows for all outcomes are outlined in Appendix A.1. Fewer children report days of missed school in the past year relative to other child outcomes because a child must be at least five years old to be asked this question. Similarly, only adults with employment histories are asked how many days of work they missed in the past year.