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 half of disparities in access among adults and would eliminate such disparities among children. We further find that higher physician reimbursement leads to more office visits, better self-reported health, and reduced school absenteeism among the program's beneficiaries.

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I Introduction

Expanding access to health care has long been a goal of health policy in the United States. To this end, substantial political and financial resources have been directed toward increasing access to 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 providers' willingness to accept a given type of coverage (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—remain open questions.

These questions are 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). Because 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; Dunn et al., 2021). Faced with little causal evidence that low reimbursement rates are to blame for disparities in access to care, policy makers often lower or freeze Medicaid payment levels in response to economic downturns and budgetary shortfalls (Smith et al., 2004; MACPAC, 2015).

[1]Using both randomized controlled trials (Finkelstein et al., 2012; Baicker et al., 2013; Goldin et al., 2021) and natural experiments (Currie and Gruber, 1996a,b; Card et al., 2008, 2009; Sommers et al., 2012; Goodman-Bacon, 2018, 2020; Miller et al., 2021), 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).

In this paper, we exploit exogenous variation in provider reimbursement rates to estimate the effects of physician payment levels on patient access, use, and health. To do so, we combine comprehensive, hand-collected data on state-level Medicaid reimbursement rates for new patient evaluation and management (E&M) services from 2009 to 2015 with self-reported measures of access, use, and health for over 70,000 respondents each year from the restricted-access National Health Interview Survey (NHIS). We supplement these data with administrative data on school attendance for nearly 500,000 students biennially from the restricted-access National Assessment of Educational Progress (NAEP). 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, a policy commonly referred to as the ACA "primary care rate increase" or "fee bump."[2] As states were previously able to set their own Medicaid payment levels, reimbursement rates varied widely across states before the primary care rate increase went into effect. While Medicaid payments for select primary care services increased by an average of nearly 60 percent as a result of the mandate, rates more than doubled in ten states and were unchanged in two.

Leveraging within-state changes in provider reimbursement rates driven by the onset of the federal mandate, we find that increasing payments to physicians leads to statistically and economically significant improvements in access to care.[3] Our results indicate that a $10 increase in Medicaid payments for new patient E&M services—a 13.2 percent increase relative to the average baseline Medicaid rate of $76—leads to a 0.71 percentage point reduction in reports of doctors telling adult Medicaid beneficiaries that they are not accepting new patients (p-value$<$0.001; 11.5 percent relative to the baseline mean of 6.2 percent) and a 0.76 percentage point reduction in reports of doctors telling adult Medicaid beneficiaries

[2]Designated in §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 physicians to participate in Medicaid (Blumenthal and Collins, 2014). The primary care services covered by the mandate included E&M services and vaccine administration provided by physicians in family medicine, general internal medicine, and pediatric medicine (CMS, 2012).

[3]Because the federal mandate required states to raise their Medicaid payments to an essentially national Medicare rate, the states that experienced the largest increases in dollars under the mandate also experienced the largest increases in relative terms. We present results throughout the paper as the impacts of a $10 increase in Medicaid payments, although we note that we cannot separately identify the impacts of absolute versus relative payment increases.

that they do not accept their insurance (p-value=0.005; 9.3 percent relative to the baseline mean of 8.2 percent). Among children covered by Medicaid, a \$10 increase in Medicaid payments leads to a 0.54 percentage point reduction in parents reporting having trouble finding a doctor to treat their child (p-value<0.001; 24.5 percent relative to the baseline mean of 2.2 percent) and a 0.28 percentage point reduction in the probability that their child has no usual place of care (p-value=0.068; 8.2 percent relative to the baseline mean of 3.4 percent). We find some evidence that these improvements in access among Medicaid beneficiaries may lead to negative spillovers in access among the privately insured, with a \$10 increase in Medicaid payments leading to a 0.15 percentage point increase in the probability that parents of privately insured children report having trouble finding a doctor to treat their child (p-value=0.009; 18.8 percent increase relative to the baseline mean of 0.8 percent).

These findings have important implications for disparities in access to care. Even absent potential spillovers to the privately insured, our 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 half of the disparities in access among adults and would eliminate such disparities among children. 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 back-of-the-envelope calculation suggests that a \$10 increase in Medicaid payments for office visits increases state-level Medicaid spending by less than 1 percent on average.

If Medicaid beneficiaries eventually receive treatment despite difficulties accessing care, increased payments could reduce search costs but have little impact on the use of services or health among patients. However, we find that higher provider reimbursement under Medicaid leads to greater usage and improved health among the program's beneficiaries. Again using data from the NHIS, we find that a \$10 increase in Medicaid payments leads to a 0.28 percentage point increase in the probability that beneficiaries visited a health care provider in the past two weeks (p-value=0.089; 1.4 percent relative to the baseline mean of 19.7 percent) and a 0.47 percentage point increase in the probability that beneficiaries report being in very good or excellent health (p-value=0.045; 0.8 percent relative to the baseline mean of 56.2 percent).[4] Using self-reported data on school absences from the NHIS and administrative data on school attendance from the NAEP, we further find that a $10 increase in Medicaid payments leads to a 0.51 percentage point decrease in chronic absenteeism due to illness or injury (p-value=0.068; 11.1 percent relative to the baseline mean of 4.6 percent) and a 0.39 percentage point decrease in chronic absenteeism overall among low-income, primary school–aged children (p-value<0.001; 2.95 percent relative to the baseline mean of 13.2 percent).

When the federally mandated rate increase expired at the end of 2014, only 14 states chose to maintain at least 50 percent of the payment increase that they experienced under the mandate. Although the decision not to extend the augmented payments might have depended on a state's experience during the federal mandate, 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. Using data from 2013 to 2015, we further find that many of the improvements that Medicaid beneficiaries experienced when payments increased were lost when payments declined as a result of the mandate's expiration, although the results leveraging payment decreases are generally smaller than our main estimates.

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 changes over our sample period. Most relevant for our analysis, 19 states expanded their Medicaid programs in 2014 to include coverage for low-income, childless adults. 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 sociodemographics. It is therefore not surprising that our results are nearly identical when we control for Medicaid expansions at the state-year level. Second, we find similar effects of changing reimbursement

[4] We further find that a $10 increase in Medicaid payments leads to a statistically insignificant reduction in the probability that privately insured patients had an office visit in the past two weeks of 0.06 percentage points (p-value=0.605; 0.34 percent relative to the baseline mean of 17.5 percent). As outlined in Section III.C, this estimate is sufficiently imprecise that we cannot rule out a one-to-one offset of increased office visits among Medicaid beneficiaries and decreased visits among the privately insured.

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 2014. Finally, as noted above, we find a similar pattern of effects when 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 supply-side health insurance generosity on patient access, use, and health, 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 several decades, and those that had chose to do so voluntarily.[5] Previous research on physician reimbursement under Medicaid 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 analyses in which treatment is potentially endogenous.[6] In contrast, we exploit a federal mandate that induced large, exogenous changes in Medicaid reimbursement rates for physicians 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 under 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 did 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 then pay providers. Although over 60 percent of Medicaid beneficiaries were enrolled in managed care plans by 2015, states—and in turn researchers—know little

[5]Physician reimbursement rates under 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.

[6]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 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).

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.

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 led to increases in appointment availability for Medicaid patients in ten states (Polsky et al., 2015). In contrast, work by Decker (2018) found that Medicaid acceptance rates in an annual survey of physicians did not increase during the primary care rate increase. Using claims data from a convenience sample of primary care physicians, Mulcahy et al. (2018) also found no association between the rate increase and physician participation in Medicaid.[7] In contrast to this previous work, which relied on small, selected samples and included limited information on the size of the rate increase across 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 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 nearly 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 higher physician payments likely lead to increased use of health care and improvements in health.[8]

Our paper also adds to a growing literature documenting the importance of financial

[7]Two recent papers examine the impacts of the primary care rate increase on the use of health care services among the approximately 15 percent of Medicaid beneficiaries that are also covered by Medicare ("dual-eligible" beneficiaries). While Fung et al. (2021) find that the increased payments had no effect on the number of primary care visits made by dual-eligible beneficiaries, Cabral et al. (2022) find an increase in the provision of targeted services and office visits for established, dual-eligible patients. Because payment rates for treating duals were already much closer to Medicare rates due to cost sharing between Medicaid and Medicare, the primary care rate increase lead to substantially smaller payment increases for treating dual-eligible beneficiaries than for treating Medicaid-only beneficiaries.

[8]Two additional differences between our work and Decker (2018) and Mulcahy et al. (2018) are worth noting. First, Decker (2018) and Mulcahy et al. (2018) incorporate implementation delays. Because increased payments were made retroactively in states that experienced such delays, physician behavior should have responded when the augmented payments went into effect at the beginning of 2013. Notably, we show that physician behavior responded equally in 2013 and 2014; incorporating payment delays therefore biases results toward zero because some of the "pre-period" in such specifications was actually treated. Second, Decker (2018) and Mulcahy et al. (2018) use physician-level data. Such data will understate welfare-relevant effects on patients if changes in access are larger among providers who treat a disproportionate share of patients.

incentives in driving physician behavior. Prior work illustrates the impacts of physician payment levels on treatment intensity, showing 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 Gottlieb, 2014; Coey, 2015; Einav et al., 2018). Our work complements these findings by demonstrating that financial incentives further drive extensive margin decisions governing who physicians are willing to see. This counters the previously held notion 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 are now well established, with studies finding that health insurance improves self-reported health and reduces mortality (Card et al., 2009; Finkelstein et al., 2012; Sommers et al., 2012; Goodman-Bacon, 2018; Goldin et al., 2021; Miller et al., 2021). Our findings add to this literature by highlighting that the positive effects of having health insurance will be mediated by supply-side insurance generosity.

Finally, our work contributes to a large literature examining policies to address chronic absenteeism among students. The prevalence of illness-related absences—especially among primary school–aged children—suggests that policies aimed at improving student health might be particularly effective at reducing absenteeism (Kearney, 2008; Balfanz and Byrnes, 2012; Bauer et al., 2018). We add to work showing that school-based health care resources can improve attendance (Allen, 2003; Jacobsen et al., 2016) by showing that increasing access to primary care through increased physician reimbursement reduces chronic absenteeism among young children. With chronic absenteeism linked to lower test scores, graduation rates, and college enrollment (Gottfried, 2009, 2011; Goodman, 2014; Liu et al., 2021), our findings suggest that increased physician reimbursement may have wide-reaching effects on a range of downstream economic outcomes.

The rest of the paper proceeds as follows. We provide an overview of the data in Section II. Section III introduces our empirical strategies 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 provides a discussion and concludes.

II Data

We use three main data sources to document how physician reimbursement rates affect access to care, frequency of office visits, and health among patients. To measure physician reimbursement, we construct a new data set containing Medicaid payments for E&M services for all states from 2009 to 2015. To measure patient access, use, and health, we use the restricted-access NHIS (NCHS, 2009–2015). Finally, to corroborate the NHIS outcomes related to schooling, we use biennial data on school absences from the restricted-access NAEP (NCES, 2009, 2011, 2013, 2015). The features of these data that are most important for our analyses are introduced below; Appendix A provides additional details.

II.A Medicaid reimbursement rates

Our primary explanatory variable is the amount that Medicaid pays physicians for new patient E&M 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). We obtained payment rates for these five codes in each quarter from 2009 to 2015 by contacting the Medicaid offices of all 50 states and the District of Columbia. Our main results use reimbursement rates associated with the most commonly billed new patient E&M code over our sample period: new patient office visits of mid-level complexity (CPT code 99203).[9] Given the strong correlation between Medicaid payments for new and established patient visits of varying complexities within states over time (see Figure A1), our results are robust to using payments for alternative E&M 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. Importantly, the primary care rate increase applied to both fee-for-service and managed care Medicaid programs, and thus both types of programs were required to reimburse providers at the Medi-

[9] 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 Medicare billing frequency–weighted average across the five reimbursement rates for new patient visits. Unfortunately, analogous reports are not available for Medicaid.

care rate (which is observed in the fee-for-service data) in 2013 and 2014.[10] To incorporate managed care before and after the federal mandate, we create 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 E&M services to calculate Medicaid managed care payments from the fee-for-service rates.[11] Using data from the Centers for Medicare and Medicaid Services (CMS) on the percentage of Medicaid beneficiaries enrolled in managed care annually in each state, we then construct expected Medicaid payments at the state-quarter level by taking the enrollment-weighted average of Medicaid fee-for-service and managed care payments.[12]

Both the initial geographic variation in Medicaid reimbursement 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. 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 new patient E&M services. When the primary care rate increase went into effect in 2013, the range tightened, with states paying physicians between $101 (Alabama) and $171 (Alaska).[13] As shown in

[10] MCOs were required to increase payments to qualified physicians for the services covered by the mandate regardless of the payment scheme used for provider reimbursement. Appendix A.1.2 outlines how states adjusted their capitation payments made to MCOs and how MCOs passed these increased payments through to providers to comply with the federal mandate.

[11] The GAO documents the difference between managed care and fee-for-service payments under Medicaid in two de-identified states and eighteen identified states in 2010 (GAO, 2014). 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 report and to imputing missing states with the mean of 14 percent more under managed care.

[12] Data on Medicaid enrollment come from CMS's National Health Expenditure Data and Medicaid Managed Care Enrollment Reports (CMS, 2009–2015). Appendix A.1.3 provides additional details on the construction of the Medicaid payment variable.

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

Figure 2, the primary care rate increase was sufficient to push all states into the top quintile of reimbursement rates as defined in 2009.[14]

Although the federal mandate removed state control over the timing and nature of the payment increases, the magnitude of the payment increase within a given state depended on its baseline payment level. Estimates that leverage within-state variation in payments stemming from the federal mandate will therefore be biased if states with different payment rates at baseline were on systematically different trends. In Section III.B, we estimate event study specifications to demonstrate that states with differing payment increases were on 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 composition, we run balancing regressions in which we use potential confounders as dependent variables (Pei et al., 2019).[15] As shown in Table 1, we find no evidence that our identifying variation is correlated with changes in Medicaid expansions, Medicaid enrollment, or Medicaid managed care penetration at the state level (Panel A) or with changes in the probability of Medicaid coverage or the characteristics of Medicaid respondents in the NHIS (Panel B). Table A2 further shows that state-level sociodemographics and Medicaid enrollment were balanced across payment levels at baseline, supporting our use of a dose-response difference-in-difference strategy.[16]

When the primary care rate increase was initially passed, it was unclear whether the mandate and federal funding for the increased payments would extend beyond 2014. In the end, the mandate and funding were not extended, and in 2015, only 14 states chose to maintain at least 50 percent of the payment increases that they experienced under the mandate (see Figure A3).[17] Among the 34 states that chose not to extend at least 50

[14]Although there is large variation in private insurance rates both within and across states (Cooper et al., 2019), private insurance rates tend to be higher than Medicare rates, which in turn are typically higher than Medicaid rates. Relative to Medicaid payments in the median (average) state at baseline, we estimate that reimbursement rates under private insurance were around \$45 (\$55) higher, whereas Medicare rates were approximately \$35 higher than Medicaid rates in both the median and average state. We recover private insurance rates by combining private insurance to Medicaid payment ratios for office-based E&M services in 2010 from the GAO with our data on Medicaid payments for 2012. The GAO data document the difference between private insurance and Medicaid payments for 32 states (GAO, 2014); we use the recorded ratio for states in the report and the median of 57 percent more under private insurance for missing states.

[15]In particular, we estimate analogues of equation (2) introduced in Section III.C.

[16]Local sociodemographics are taken from the American Community Surveys (ACS, 2009–2015).

[17]We exclude three states from calculations of the share of increased fees that were extended: Alaska,

percent of the increased rates, the median state maintained less than one percent, effectively returning payments to their 2012 levels. Although this reduction in reimbursement rates among states that chose not to extend the increased rates provides another large change in payments, states may have made this decision based on their experience during the primary care rate increase. 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 directly explore the potential endogeneity concerns.

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. Although much of the NHIS is publicly available, our analysis relies on a restricted-access version that contains state and county identifiers for respondents. We use outcomes from three NHIS sample components from 2009 to 2015: 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).

We consider responses to eight questions to measure the impacts of physician reimbursement rates on patient access, use, and health. To measure access to health care services, we consider whether adult respondents report being told that a doctor's office was either not accepting new patients or not accepting their insurance in the past year. For children, we consider whether parents report having trouble finding a doctor to see their child in the past year and whether their child has a usual place of care. To measure use of health care services and patient health, respectively, we consider whether respondents report having seen

Delaware, and North Dakota. Alaska and North Dakota were unaffected by the mandate because their Medicaid payments exceeded the federally mandated Medicare level over the sample period. Although Delaware saw a slight increase in fee-for-service Medicaid payments as a result of the mandate (a 3.0 percent increase over the baseline rate, the lowest percent increase among all states other than Alaska and North Dakota), Medicaid payments in Delaware in 2012 were essentially equivalent to the 2013 Medicare rate when Medicaid managed care is taken into account.

a health care provider in the past two weeks and indicators denoting whether people rate their health as (1) "excellent" or "very good" or (2) "fair" or "poor." Finally, we 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. 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. Appendix A.2.1 provides the exact wording of the survey questions used.

Importantly, the NHIS asks specifically about school absences due to illness or injury. Among young children, acute illnesses such as respiratory infections and gastroenteritis and chronic childhood diseases such as asthma are among the most common reasons for school absenteeism (Neuzil et al., 2002; Moonie et al., 2006; Ehrlich et al., 2014; Wiseman and Dawson, 2015). Improved access to timely primary care could therefore lead to improvements in school attendance by allowing children to access antibiotics for bacterial infections, by increasing vaccination rates, or by improving the management of chronic diseases. Because absenteeism is most closely tied to health for primary school–aged children—whereas absences for older children are more likely to be for reasons unrelated to health care access, such as truancy—we look separately at younger and older children when considering school absences (Balfanz and Byrnes, 2012). Finally, given the negative impacts of chronic absenteeism on both contemporaneous and long-term educational outcomes (Gottfried, 2009, 2011; Goodman, 2014; Liu et al., 2021), we focus predominately on whether parents report their child having missed fourteen or more days of school in the past year.

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.[18] Baseline differences in health between Medicaid beneficiaries and

[18]These statistics are in line with those found in other surveys. For example, according to data from the 2016 National Survey of Children's Health, 3.6 percent of parents with children covered by Medicaid or other government assistance plans reported being usually or always frustrated in efforts to get services for their children in the past year compared to only 1.7 percent among parents of children with insurance of any type. Moreover, parents with children covered by Medicaid were twice as likely to report that there was a time in the past year that their child needed care but did not receive it relative to parents of children with insurance of any type (4.2 percent versus 2.1 percent, respectively). Approximately half of the parents who reported that their child did not receive necessary care attributed this to difficulties getting an appointment.

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. Medicaid beneficiaries also differ from the privately insured in terms of their socio-demographics: as shown in Table A1, 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.

II.C National Assessment of Educational Progress

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 schools and students are selected to be representative of all schools nationally and of public schools at the district level. We use data from the restricted-access, individual-level files for 2009, 2011, 2013, and 2015 in our primary analysis.

The NAEP reports whether a child missed 0, 1–2, 3–4, 5–10, or 11 or more days of school in the month preceding their national assessment exams. Although the NAEP data does not include information on absences due specifically to illness or injury (as in the NHIS), recall that most school absences—particularly among young children—are attributable either to acute illnesses or chronic childhood diseases (Ehrlich et al., 2014; Wiseman and Dawson, 2015). We again focus on chronic absenteeism, which is commonly defined as three or more days of missed school when using monthly data. 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 lunch. Like Medicaid, free school lunch is a means-tested program; according to income-eligibility limits for each program, many children who are individually eligible for free school meals are also eligible for Medicaid (although not all, depending on the state and year).

Figure A4 shows the distribution of absences averaged over math and reading assessments by grade for students that do and do not qualify for free school lunch. As was seen in the NHIS, children from low-income families have higher rates of chronic absenteeism in the

NAEP data than children from less disadvantaged backgrounds. In grade 4, 24.3 percent of children eligible for free lunch missed three or more days in the past month compared to 16.2 percent among students ineligible for free lunch. The disparity in school absences by free lunch eligibility is similar in grade 8, though more students are chronically absent in both groups relative to grade 4.

III Physician payments and access, use, and health

The summary statistics in Table 2 demonstrate that those covered by Medicaid face greater difficulties 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. We focus on the impacts of the increase in Medicaid payments stemming from the onset of the primary care rate increase in 2013 in Sections III.A through III.C; Section III.D considers the effects of the reduction in Medicaid payments following the expiration of the federal mandate in 2015. Sections III.A through III.D consider a range of outcomes from the NHIS, while Section III.E turns to educational outcomes from the NAEP.

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 our outcome measures 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 (solid line) and one for privately insured patients (dashed line)—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 were associated with larger improvements in a given outcome.

The results in Figure 3 show that Medicaid beneficiaries in states with larger increases in Medicaid payments saw greater improvements in access, frequency of office visits, and health.[19] For example, the bottom-left subplot shows that Medicaid beneficiaries in states in the lowest decile of payment increases (average increase of $5.98) experienced little change in the probability of being told by a doctor's office that they were not accepting new patients following the onset of the federal mandate, whereas Medicaid beneficiaries in states in the highest decile of payment increases (average of $71.46) experienced an average improvement of over 2 percentage points. 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 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 onset of the primary care rate increase in state s, we estimate the following specification using data from 2009–2014:

$$Outcome_{isy} = \beta_y \cdot \Delta Payment_s + \gamma X_i + \lambda_s + \lambda_y + \epsilon_{isy} \quad (1)$$

where $Outcome_{isy}$ denotes an outcome for Medicaid beneficiary i living in state s in year y; X_i is a vector of individual characteristics (listed in Table A1); and λ_s and λ_y are state and year fixed effects, respectively.[20] By scaling the 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.

[19] 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.

[20] Our primary specification controls for individual characteristics to account for any changes in sample composition over our sample period and to increase precision. Nevertheless, we verify in Table 1 that our identifying variation is uncorrelated with changes in observable characteristics of respondents such as race and gender. Moreover, as shown in Figure 6, both the magnitudes and precision of the estimates are very similar if we exclude all individual-level controls. We further show in Figure 6 that our results are not sensitive to the inclusion of a time-varying, state-level control for Medicaid expansions or time-varying, county-level controls such as total population, population density, and the unemployment rate.

Figure 4 plots the $\hat{\beta}_y$s from estimation of equation (1). The coefficients before the primary care rate increase—$\hat{\beta}_{2009}$ through $\hat{\beta}_{2012}$—are statistically indistinguishable from zero, indicating that outcomes were on similar trends before the federal mandate across states that would soon experience differing payment increases. Following the onset of the federal mandate, however, there are persistent increases in many of the outcomes in states that experienced larger payment increases. For example, the bottom left subplot indicates that Medicaid beneficiaries saw significant 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—may accrue over time. As shown in Figure A5, we do not observe such effects among patients with private insurance.

III.C Primary estimates

Figure 4 demonstrates that increased Medicaid payments to physicians lead to improved outcomes among Medicaid beneficiaries. To quantify the effects of physician reimbursement on access, use, and health, we estimate the following specification using data from 2009–2014:

$$Outcome_{isqy} = \beta \cdot Payment_{sqy} + \gamma X_i + \lambda_s + \lambda_{qy} + \epsilon_{isqy} \tag{2}$$

where $Outcome_{isqy}$ denotes an outcome for respondent i living in state s in quarter q of year y, $Payment_{sqy}$ denotes the relevant Medicaid payment rate in state s in quarter q of year y, λ_{qy} are quarter-year fixed effects, and all other variables are defined as in equation (1). For 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 payment rate in the quarter of the interview. Moreover, to isolate variation in payments stemming from the onset of the primary care rate increase, we use state-level reimbursement rates from the fourth quarter of 2012 (final quarter before the rate increase) for 2009 through the third quarter of 2012, thereby excluding all non-policy variation from the analysis.[21,22] Finally, we divide payments by $10 such that β represents the effect of a $10 increase in Medicaid payments induced by the federal mandate. As before, all regressions use the sampling weights provided in the NHIS, and standard errors are clustered by state.

Table 3 presents results from estimation of equation (2). The left half of each panel shows the effects of changes in Medicaid payments on survey respondents covered by Medicaid. Looking first to outcomes measuring access among children in columns (1) and (2) of Panel B, we see that a $10 increase in Medicaid payments leads to a 0.54 percentage point decrease (p-value<0.001) in the probability that parents report difficulty finding a doctor to see their child covered by Medicaid and a 0.28 percentage point decrease (p-value=0.068) in the likelihood that their child has no usual place of care (24.5 and 8.2 percent decreases relative to the respective baseline means of 2.2 and 3.4 percent). Among adult Medicaid beneficiaries, a $10 increase in Medicaid payments causes both a 0.71 percentage point reduction (p-value<0.001) in the probability of being told that a physician is not accepting new patients and a 0.76 percentage point reduction (p-value=0.005) in the probability of being told that one's insurance is not accepted (11.5 and 9.3 percent decreases relative to the respective baseline means of 6.2 and 8.2 percent; see columns (1) and (2) of Panel C). Notably, these improvements in access lead to more use: as shown in column (1) of Panel A, a $10 increase in Medicaid payments increases the probability that respondents covered by Medicaid visited a health care provider in the past two weeks by 0.28 percentage points (1.4 percent relative to the baseline mean of 19.7 percent), although we note that this effect is only significant at the 10 percent level (p-value=0.089).

In addition to improved access and increased use, increases in Medicaid payments lead to better health among the program's beneficiaries. As shown in column (3) of Panel A,

[21] 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 mandated rate increase). Because states that adjusted their reimbursement rates before the federal mandate chose to do so voluntarily, these payment changes may be endogenous, and thus we exclude these changes from our primary specification.

[22] An alternative way to exclude non-policy variation is to estimate a version of equation (1) that pools the post-period coefficients. Although we confirm in Figure 6 that our results are very similar in this alternative specification, we prefer equation (2) because it allows us to easily account for the relevant look-back period for survey responses and to exploit variation in payments stemming from both the mandate's onset and expiration (which often led to payment changes of different sizes in the same state) in a single specification in analyses below.

a $10 increase in physician reimbursement under Medicaid increases the probability that Medicaid beneficiaries report being in excellent or very good health by 0.47 percentage points (p-value=0.045; 0.8 percent relative to the baseline mean of 56.2 percent). Among young children covered by Medicaid, we see in column (3) of Panel B that a $10 increase in physician reimbursement reduces the probability of being chronically absent due to illness or injury by 0.51 percentage points (p-value=0.068; 11.1 percent relative to the baseline mean of 4.6 percent).[23] There is no reduction in illness-related chronic absenteeism among older children covered by Medicaid (column (4) of Panel B). 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 a $35 increase in Medicaid payments—the median increase in Medicaid payments across states stemming from the onset of the federal mandate. Multiplying the point estimates in Table 3 by 3.5, we see that an increase of $35 in physician reimbursement under Medicaid leads to a 5.0 percent increase in the probability of having visited a health care provider in the past two weeks and a 2.9 percent increase in the probability of being in very good or excellent health among the program's beneficiaries. Applying the same calculations to the access measures further indicates that the Medicaid primary care rate increase reduced the probability that parents had trouble finding doctors for their Medicaid-covered children by nearly 90 percent and reduced these difficulties by over one-third for adult beneficiaries in the median state.

We can compute elasticities by comparing the effects of a $10 increase in Medicaid payments in percent terms to the corresponding percent change in Medicaid payments implied by a $10 increase.[24] As reported in column (5) of Table 4, our results imply elasticities with respect to Medicaid payments of physician willingness to accept new adult Medicaid patients of 0.71, office visits among beneficiaries of 0.11, and self-reported good health among beneficiaries of 0.06. The implied elasticity for ease of finding a doctor willing to treat children

[23]We find similar results when we consider a continuous measure of school absences rather than an indicator denoting chronic absenteeism. As shown in Table A3, a $10 increase in Medicaid payments leads to an average reduction of 0.20 days of school missed due to illness or injury per year among young children covered by Medicaid (p-value=0.015), a 5.7 percent reduction relative to the baseline mean of 3.5 days.

[24]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.

covered by Medicaid is 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). While our outcomes for children and adults are somewhat different, thereby complicating direct comparisons of the access elasticities, it is nevertheless reasonable that physician behavior would be more responsive to Medicaid payments for children.

Spillovers to the privately insured The right half of Table 3 presents analogous estimates for privately insured respondents, who may be indirectly affected by Medicaid patients becoming relatively more attractive to physicians. For most outcomes, we find no significant effects of increases in Medicaid payments on the privately insured despite large sample sizes.

One notable exception, however, is for access among children: as shown in column (5) of Panel B, we estimate that a $10 increase in physician reimbursement under Medicaid leads to a 0.15 percentage point increase (p-value=0.009) in the probability that parents of children covered by private insurance report difficulty finding a doctor to treat their child, an increase of 18.8 percent relative to the (low) baseline mean of 0.8 percentage points. Moreover, although statistically insignificant, we note that the point estimate for the effect of increased Medicaid reimbursement rates on office visits among the privately insured (p-value=0.605) does not rule out a one-to-one offset of office visits by Medicaid patients and those with private insurance.[25]

To further probe these potential access spillovers, we examine how our results differ across

[25] Column (1) of Panel A in Table 3 shows that a $10 increase in Medicaid payments leads to a statistically significant 0.28 percentage point increase (p-value=0.089) in the probability that a Medicaid beneficiary had an office visit in the past two weeks. Assuming that this increase is driven by a single additional visit among marginal patients, 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 a recent office visit. This is within the confidence interval for the estimate in column (4) of Panel A in Table 3, which suggests that a $10 increase in Medicaid payments leads to a statistically insignificant 0.06 percentage point reduction (p-value=0.605) in the probability that a patient with private insurance had an office visit in the past two weeks.

areas in which providers have more or less capacity to absorb additional patients. If there are spillovers to the privately insured, we would expect them to be more pronounced in areas in which providers are capacity constrained. Although we do not have measures of physician capacity in our data, we can divide counties by whether they have a shortage of primary care providers as defined by the Health Resources and Services Administration. We then estimate analogues of equation (2) that include a main effect of being designated a shortage area and an interaction between the payment variable and this shortage indicator. As shown in Table A4, we find no evidence that spillovers to the privately insured are more pronounced in areas in which providers have less scope to take on new patients.[26] Further examinations into how Medicaid reimbursement policies affect the care provided to the privately insured is an important area for future work.

Implications for disparities Our findings indicate that the Medicaid primary care rate increase had large implications for disparities in access to care between the publicly and privately insured. Column (3) of Table 4 reports baseline disparities in our outcome measures between Medicaid beneficiaries and patients with private insurance. Columns (6) through (8) show the share of these disparities that are reduced by increasing Medicaid payments by \$10, \$35, and \$45, respectively. These calculations ignore any potential spillovers to the privately insured discussed above and only consider the amount of each disparity closed by improvements among Medicaid beneficiaries. Since negative spillovers on the privately insured necessarily serve to decrease disparities, the reductions in disparities resulting from increased physician reimbursements under Medicaid presented in Table 4 can be interpreted as lower bounds.

Looking first to column (7), we see that increasing Medicaid payments for physicians by \$35—the median increase under the primary care rate increase—reduces disparities in reports of doctors telling adults that they are not taking new patients or their insurance by

[26]If physicians are capacity constrained, then we might also expect to see smaller effects of increased Medicaid payments on outcomes among the program's beneficiaries. However, Table A4 further shows that there were no differential effects of the primary care rate increase on patients covered by Medicaid in counties that were and were not primary care shortage areas. This suggests that even in areas with relatively few primary care providers, some providers have scope to increase the number of patients that they see. This would be possible, if, for example, providers decrease their appointment length per patient (Garthwaite, 2012).

55 and 47 percent, respectively. Closing the gap in payments between private insurance and Medicaid—a $45 increase in Medicaid payments for the median state at baseline—closes over two-thirds of the gap in reports of doctors not taking new adult patients and 60 percent of the gap in reports of doctors not taking an adult patient's insurance. Because providers are more elastic to payments for children, and because baseline disparities in childhood access are smaller, it is easier to close gaps in access among children: as shown in column (9) of Table 4, it would take an increase in Medicaid payments of about $26 on average to eliminate disparities in parental reports of having difficulty finding a doctor to treat their child covered by Medicaid versus private insurance.[27]

III.D 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 A3, the median state extended less than 4 percent of the increased payments. Only 14 states maintained at least 50 percent of the higher rates, with 29 states returning their payments to no more than 15 percent above their December 2012 levels in January 2015.

Although the reduction in Medicaid reimbursement rates resulting from the end of the federal mandate provides a second round of changes in physician reimbursement, the decision not to maintain the higher payments could be endogenous. In particular, states that experienced greater success under the federal mandate—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. To examine whether the primary care

[27] As shown in column (7) of Table 4, the median increase in Medicaid payments of $35 under the federal mandate was sufficient to close *more* than the disparity in reports of difficulty finding a doctor among children with private insurance and children with Medicaid; this suggests that children on Medicaid were more attractive to physicians than children with private insurance in some states after the rate increase. As the use of pediatric modifier codes often results in state Medicaid programs paying slightly more for children than for adults, the median payment increase of $35 will close more of—or may even go beyond—the gap in payments between Medicaid and private insurance for children. Moreover, as discussed above, we find some evidence of negative spillovers to children with private insurance as a result of the Medicaid primary care rate increase.

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 extended less than 50 percent of the increases that they experienced under the mandate beyond 2014. Among the 34 states included in this analysis, no state maintained more than one-third of the increased payments, and the median state maintained less than one percent. 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 turning on in states that ultimately decided to turn off the majority of the increased payments.

As shown in the top two rows of each subfigure in Figure 5, states that chose to extend less than half of 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 majority of the increased payments experienced slightly smaller improvements in access, the effects on use and health among this subsample of states are very similar to our main results. This suggests that states chose to lower their reimbursement rates toward previous levels despite significant improvements in outcomes resulting from the increased payments.[28]

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 equation (2) including data for all states but only from 2013 to 2015 to exploit variation in payments stemming from the federal mandate expiring at the end of 2014.[29] Because states that

[28]There are a number of reasons a state's decision over whether to extend the higher 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).

[29]We further estimate event study specifications that are analogous to equation (1) but that include data from 2009–2015 rather than only 2009–2014. The results from this analysis are presented in Figure A6. As expected, the results following the expiration of the federal mandate are not always sharp, as not all states returned their payments to their baseline levels. Nevertheless, for variables for which we observe a clear increase at the onset of the rate increase in 2013, such as whether parents report difficulty finding a doctor willing to treat their child, we also observe a clear decrease when the rate increase expired in 2015.

chose to extend some portion of the increased payments experienced a smaller change in payments resulting from the federal mandate's expiration than they experienced at its onset, the payment changes that we exploit in this analysis are generally less pronounced than in our main specification.

Results from this analysis are presented in the bottom row of each subfigure in Figure 5. Although the variation comes from payment decreases, the estimated coefficients again represent the effects of a $10 *increase* in Medicaid payments. Comparing the point estimates between the top and bottom rows in each subfigure, which both consider the effects across all states, suggests that the rate increase partially turning off had effects that were similar, although slightly smaller, than the effects of the federate mandate turning on. Moreover, many of the access results are significant at the 10 percent level, indicating that Medicaid beneficiaries experienced significant reductions in access when the mandate expired that undid many of the improvements that they had experienced while the mandate was in effect.[30]

We further estimate specifications that jointly exploit variation in physician reimbursement rates stemming from the onset and expiration of the Medicaid primary care rate increase. In particular, we estimate equation (2) using data from 2009–2015 rather than data from 2009–2014 as in Table 3 or 2013–2015 as in Figure 5. As shown in Table A5, results from this analysis are similar, although slightly smaller, than our main findings discussed in Section III.C. Given that the effects of the mandate's expiration were somewhat less pronounced than the effects of the mandate's onset (Figure 5), the results are unsurprisingly attenuated slightly when we pool these two sources of variation.

III.E School absenteeism in the NAEP

All of the measures in the NHIS, including days of school missed, are self reported. To further examine the finding that increased reimbursement rates for physicians may reduce school absenteeism among young children covered by Medicaid, we use administrative data from the NAEP. Although comprehensive, the NAEP is only available every two years, and

[30]These findings are consistent with Candon et al. (2018), who replicate the analysis of Polsky et al. (2015) 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.

thus we use data from 2009, 2011, 2013, and 2015 and exploit variation stemming from both the onset and expiration of the federal mandate in these analyses. We estimate a specification similar to equation (2) in these data:

$$Outcome_{isy} = \beta \cdot Payment_{sy} + \gamma X_i + \eta X_{sy} + \lambda_s + \lambda_y + \epsilon_{isy} \quad (3)$$

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); X_{sy} is a vector of state-level analogues of all individual-level controls included in equations (1) and (2) (denoted with an asterisk in Table A2); 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 Medicaid payment rate (in \$10s) in state s in the first quarter of year y. As in Section III.C, we exclude all non-policy variation from this analysis by using state-level reimbursement rates from 2011 (the last year in the NAEP before the rate increase) for 2009. We use the sample weights provided in the NAEP and cluster standard errors by state.

Table 5 presents results from estimation of equation (3).[31] Panel A shows the effects of changes in physician reimbursement under Medicaid on outcomes among students who qualify for free lunch, our proxy for Medicaid eligibility in the NAEP. As shown in columns (1) and (3), respectively, a \$10 increase in Medicaid payments increases the share of low-income students in grade 4 with zero absences by 0.42 percentage points (p-value=0.008; 0.9 percent relative to the baseline mean of 44.8 percent) and reduces the share who missed three or more days in the past month by 0.39 percentage points (p-value<0.001; 3.0 percent relative to the baseline mean of 13.2 percent). As the NAEP covers absences for any reason, whereas the NHIS asks specifically about school absences due to illness or injury, it is not surprising that we find smaller effects in percent terms when considering school absenteeism in the NAEP. Columns (2) and (4) in Panel A show a similar pattern for children in grade 8, although the point estimates are smaller and less precise. The larger effects in grade 4 relative to grade 8 again likely reflect the fact that absences for younger children are more

[31] Figure A7 presents event study estimates from analogues of equation (1). Moreover, using data from the publicly available state-level files, Appendix D considers impacts on state-level absenteeism and test scores. We do not find significant effects on average test scores; this is to be expected given the small magnitude of anticipated effects.

closely tied to health (Balfanz and Byrnes, 2012; Wiseman and Dawson, 2015).

As shown in Panel B, we find no significant effects on chronic absenteeism among children who do not qualify for free lunch (columns (3) and (4)). However, as shown in column (1), we find that a $10 increase in Medicaid payments increases the share of students in grade 4 with perfect attendance by 0.30 percentage points (p-value=0.033; 0.6 percent relative to the baseline mean of 53.8 percent). This improvement in attendance among young children who are ineligible for free lunch might reflect the fact that some of these students are covered by Medicaid due to stricter income thresholds for the receipt of free meals than for childhood Medicaid coverage in many states.[32] Alternatively, reduced disease burden among students who qualify for free lunch could lead to improved health among their classroom peers by reducing disease transmission at school.

IV Robustness

IV.A Medicaid expansions and FQHCs

In 2014, 19 states expanded their Medicaid programs to extend coverage to low-income, childless adults (Leung and Mas, 2018). If states that saw larger payment increases under the Medicaid 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 ACA Medicaid expansions and the Medicaid primary care rate increase were similar, our results are unlikely to be confounded by Medicaid expansions for several reasons. First, 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 federally mandated rate increase were realized before 2014. Second, we find some of the largest effects among children, whose eligibility was largely unaffected by the expansions.[33] Third, as

[32] Children in households with incomes at or below 130 percent of the federal poverty level (FPL) are eligible to receive free meals at school across the United States (FRAC, 2018). According to the Kaiser Family Foundation, the median state's eligibility limit for childhood Medicaid coverage was 133 percent of the FPL in 2013, with the five most generous states having an eligibility limit of 300 percent and the 18 most restrictive states having an eligibility limit of 100 percent.

[33] Work by Venkataramani et al. (2017) demonstrates that children's use of preventive services increases

shown in Section III.D, we estimate a similar pattern of effects when leveraging payment decreases stemming from the federal mandate expiring in 2015, a year after the majority of Medicaid expansions had gone into effect. Finally, as discussed in Section II.A, we find no evidence that our identifying variation is correlated with Medicaid expansions or changes in Medicaid enrollment or managed care penetration at the state level (Table 1, Panel A). Using individual-level data from the NHIS, we further find no evidence that our identifying variation is associated with changes in Medicaid enrollment or composition in our analysis sample (Table 1, Panel B).

Nevertheless, we conduct four additional analyses to further verify that our results are not confounded by the ACA Medicaid expansions. In particular, we re-estimate equation (2) either (1) controlling for Medicaid expansions at the state-year level, (2) only including the years before the 2014 Medicaid expansions (2009–2013), (3) only including states that did not expand their Medicaid programs in 2014, or (4) limiting the analysis sample to families with children. The top rows of each subfigure in Figure 6 compare our primary estimates with the results from these analyses. Looking first to the results that control for Medicaid expansions, we see that our results are nearly identical—and in some cases even stronger—when we absorb any direct effects of the expansions. Moreover, looking 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 in 2014, the general pattern of results is consistent with our main findings. Finally, we see that our results are very similar among households with children. Taken together, these results demonstrate that our effects are driven by changes in supply-side program generosity rather than demand-side program eligibility.

A related concern is that our results could be confounded by the recent growth in federally qualified health centers (FQHCs), community-based health centers that predominately serve low-income populations. To expand the capacities and operations of such centers, the ACA allocated $11 billion to be spent over five years. With over $1.7 billion in grants awarded in 2011 alone, the timing of increased funding for FQHCs again does not coincide

when Medicaid eligibility is extended among adults. Such spillovers could influence our estimates of child health, yet it is unlikely that an improvement in coverage among adults would make it easier for parents to find physicians willing to see their children.

directly with the onset of the Medicaid primary care rate increase in 2013. Furthermore, because payments to FQHCs are made on a facility basis and are not specific to an individual physician's services, the primary care rate increase did not apply to services provided at FQHCs. Nevertheless, we estimate state-level analogues of equation (2) using measures of FQHC presence and use as outcomes to examine whether our identifying variation predicts changes in these potential confounders.[34] As shown in Table A6, changes in Medicaid payments stemming from the onset of the federal mandate are not correlated with changes in the number of FQHC grantees, sites, or patient encounters.

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. In our main analysis, we use the median payment ratio among states in the GAO report (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 findings: (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. As shown in Figure 6, these additional analyses yield very similar results.

Recall that our primary payment variable is the level of Medicaid payments measured in dollars. Since the impacts of a $10 increase in Medicaid payments should vary depending on how baseline levels of Medicaid payments compare to the payment levels of other payers in the same market, we further estimates analogues of equation (2) that use the Medicaid-to-Medicare payment ratio as the independent variable of interest. As shown in Figure 6, the

[34] Information on the presence and use of FQHCs comes from the National Association of Community Health Centers (NACHC, 2009–2014).

impacts of an increase in the Medicaid-to-Medicare payment ratio of 0.10 (an increase of 14.9 percent relative to the average baseline payment ratio of 0.67) are very similar to the effects of an increase in Medicaid payments of $10 (an increase of 13.2 percent relative to the average baseline payment of $76). Since the primary care rate increase raised state-level Medicaid rates to an essentially national Medicare rate, the states with the largest increases in Medicaid payments were also the states with the lowest payments—both relative to Medicare and in absolute terms—at baseline. The states that experienced the largest increases in dollars therefore also experienced the largest increases in relative terms, and thus the similarly between these results is not surprising.

IV.C Triple difference model

We conduct analyses separately among Medicaid beneficiaries and patients covered by private insurance in our preferred specification. 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. However, as triple difference models have 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 triple difference estimates for comparison.[35] As shown in Tables A7 and A8, the pattern of results from triple difference specifications is very similar to that found using only Medicaid beneficiaries in a difference-in-difference framework.

V 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

[35] In particular, we estimate analogues 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 further consider specifications 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.

patients. In the case of Medicaid, 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; Dunn et al., 2021). 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 2.7 percentage points, or 32 percent relative to the baseline mean. We further find large improvements in access among children, with a $35 increase in Medicaid payments reducing the probability that parents report difficulty finding a doctor willing to treat their child covered by Medicaid by 1.9 percentage points, a reduction of over 85 percent relative to the baseline mean.

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 gap in payments between Medicaid and private insurers in the median state—would reduce disparities in access to care by over 50 percent. The effects on disparities are even more pronounced among children, for whom we find that closing the gap in physician payment rates between Medicaid and private insurance has the potential to eliminate disparities in access entirely.[36]

[36] An outstanding question is whether these improvements in access are driven by additional physicians starting to accept Medicaid or by physicians who already accepted Medicaid taking additional Medicaid beneficiaries. Although our current data are not well suited to separately identify the importance of extensive versus intensive margin adjustments, the contrast between our results—in which we find large improvements in access resulting from the primary care rate increase using patient-level data—and those of Decker (2018) and Mulcahy et al. (2018)—who find no evidence that the primary care rate increase led to increased participation in Medicaid using physician-level data—suggests that intensive margin adjustments may play an important role.

These improvements in access lead to increased use and better health among Medicaid beneficiaries. Increasing Medicaid payments by $35 increases the probability that program beneficiaries visited a health care provider in the past two weeks by 5.0 percent and increases the probability that they report being in excellent or very good health by 2.9 percent. The implied elasticity of self-reported health with respect to outpatient care is consistent with the literature that uses 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).

Our results further demonstrate that increases in access to primary care have the potential to reduce school absenteeism: among young children from low-income families, a $35 increase in Medicaid payments reduces chronic absenteeism by 10.3 percent. While sizable, this effect is comparable in magnitude to light-touch policies aimed at reducing absenteeism, such as interventions informing parents about their child's absences (Rogers and Feller, 2018). Moreover, several additional findings demonstrate that this result is robust and is in line with what is known about school absences. First, we find reductions in school absenteeism in both the NHIS and the NAEP, two data sets with different sampling and reporting methodologies. Additionally, the effects are more pronounced when we focus specifically on illness-related absences in the NHIS. The reductions in absenteeism are also larger among younger children, for whom absences are most closely tied to communicable diseases and chronic childhood illness (Balfanz and Byrnes, 2012; Ehrlich et al., 2014; Wiseman and Dawson, 2015). Finally, the effects are concentrated among children whose access to health care was directly affected by the primary care rate increase; that is, children from low-income families.

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 $60 million annually, or less than 1 percent of average state-year Medicaid spending of over $7 billion.[37] This implies that the median payment increase of $35

[37]Table 2 shows that Medicaid beneficiaries had an average of 0.197 office visits over a two-week period,

increased annual state-level Medicaid spending by about 3 percent. This estimate is larger, although of the same order of magnitude, as CMS's own projections of the mandate's costs: the proposed rule published in advance of the primary care rate increase estimated that the mandate would cost around $6 billion in each year, an increase of 1.5 percent relative to total Medicaid spending of nearly $400 billion annually at baseline (CMS, 2012). Although the full monetary value of the sizable increases in access, use, and health among Medicaid beneficiaries that we document are difficult to quantify, it is possible that these benefits may have exceeded the mandate's cost.[38]

The difficulties that Medicaid patients face accessing care is commonly attributed to a combination of complex patient needs, billing complications, and low provider reimbursement rates. The multifaceted nature of the problem 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 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.

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 as reported by CMS (1.07 million), there were an average of 5.48 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 $54.8 million in additional spending on inframarginal visits, or 0.76 percent of average annual state-level Medicaid spending ($7.23 billion). In terms of marginal visits, Table 3 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 $6.70 million in additional spending on marginal visits, or less than 0.10 percent. Combined, a $10 increase in Medicaid payments for office visits should therefore increase state-level Medicaid spending by approximately $62 million annually, or less than 1 percent.

[38] Reduced school absenteeism has been linked to higher rates of educational attainment (Liu et al., 2021), which in turn is linked to improved labor market performance (Card, 1999). While health has likewise been shown to causally affect labor market performance (e.g., Stephens and Toohey, 2022), the Oregon Health Insurance Experiment had sizable impacts on self-reported health and use of health care services (Finkelstein et al., 2012) but limited short-run impacts on labor market performance and reliance on other forms of government assistance (Baicker et al., 2014).

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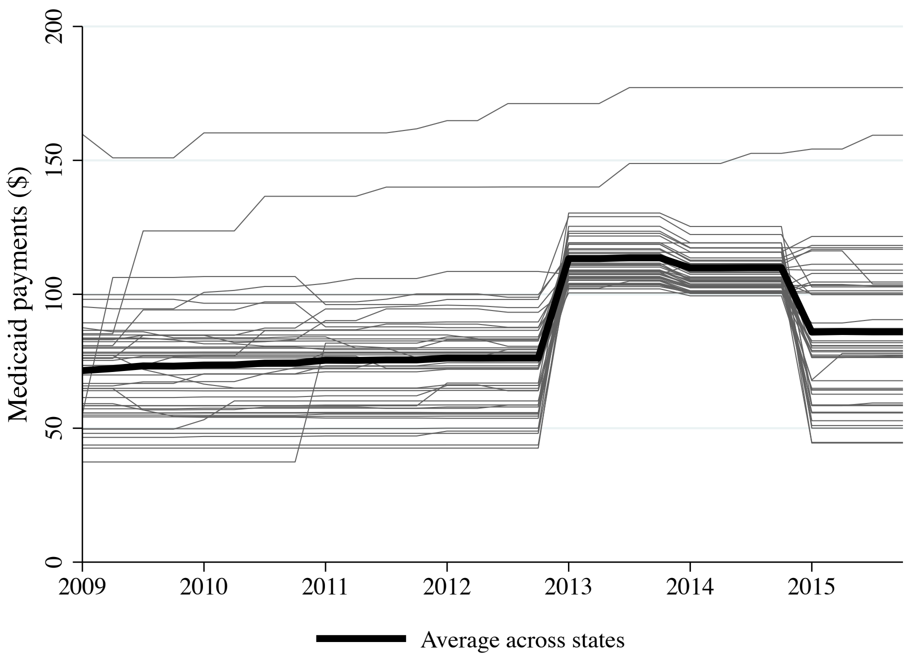
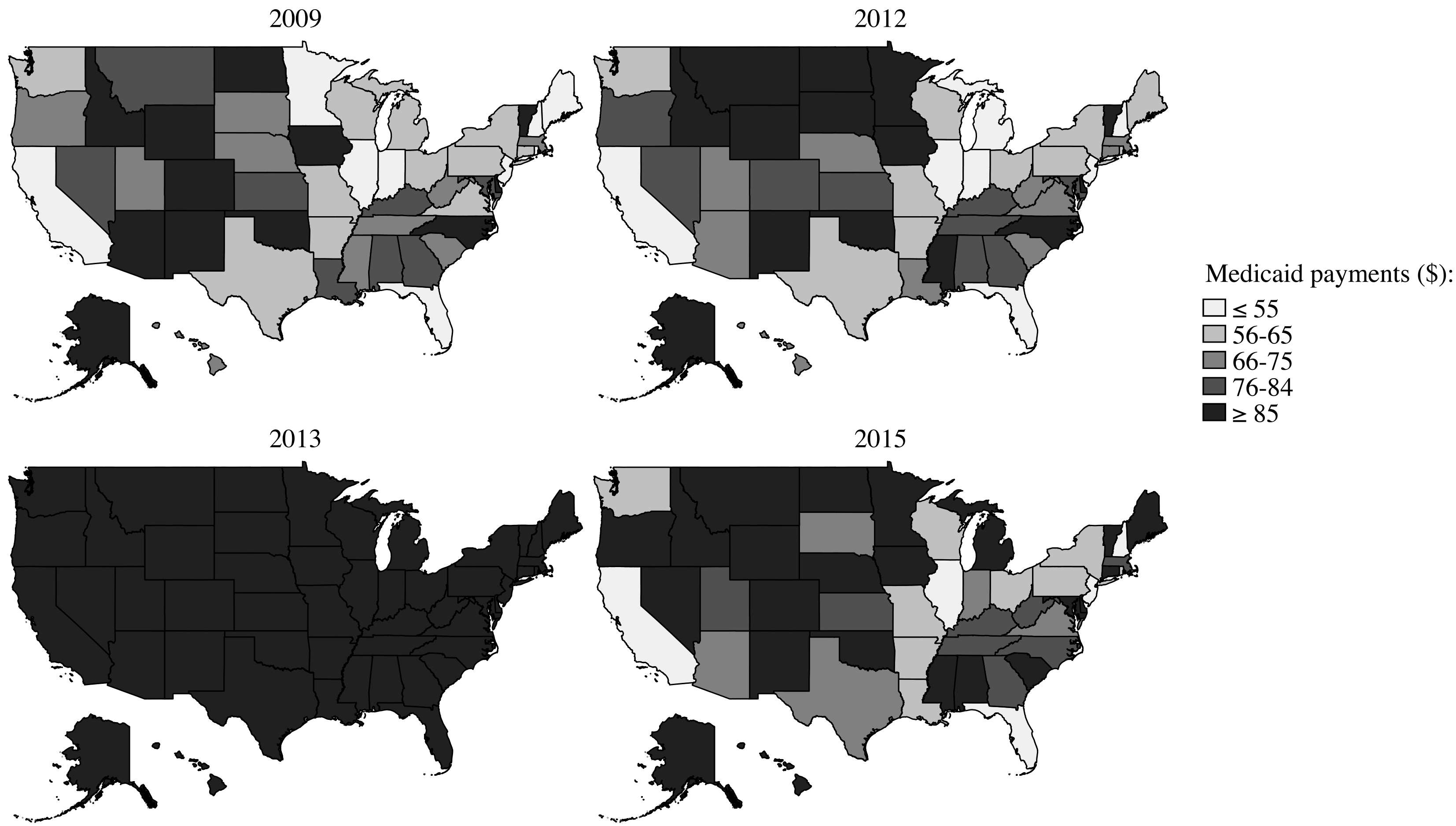
VI Figures

Figure 1: State-level Medicaid payments from 2009 to 2015



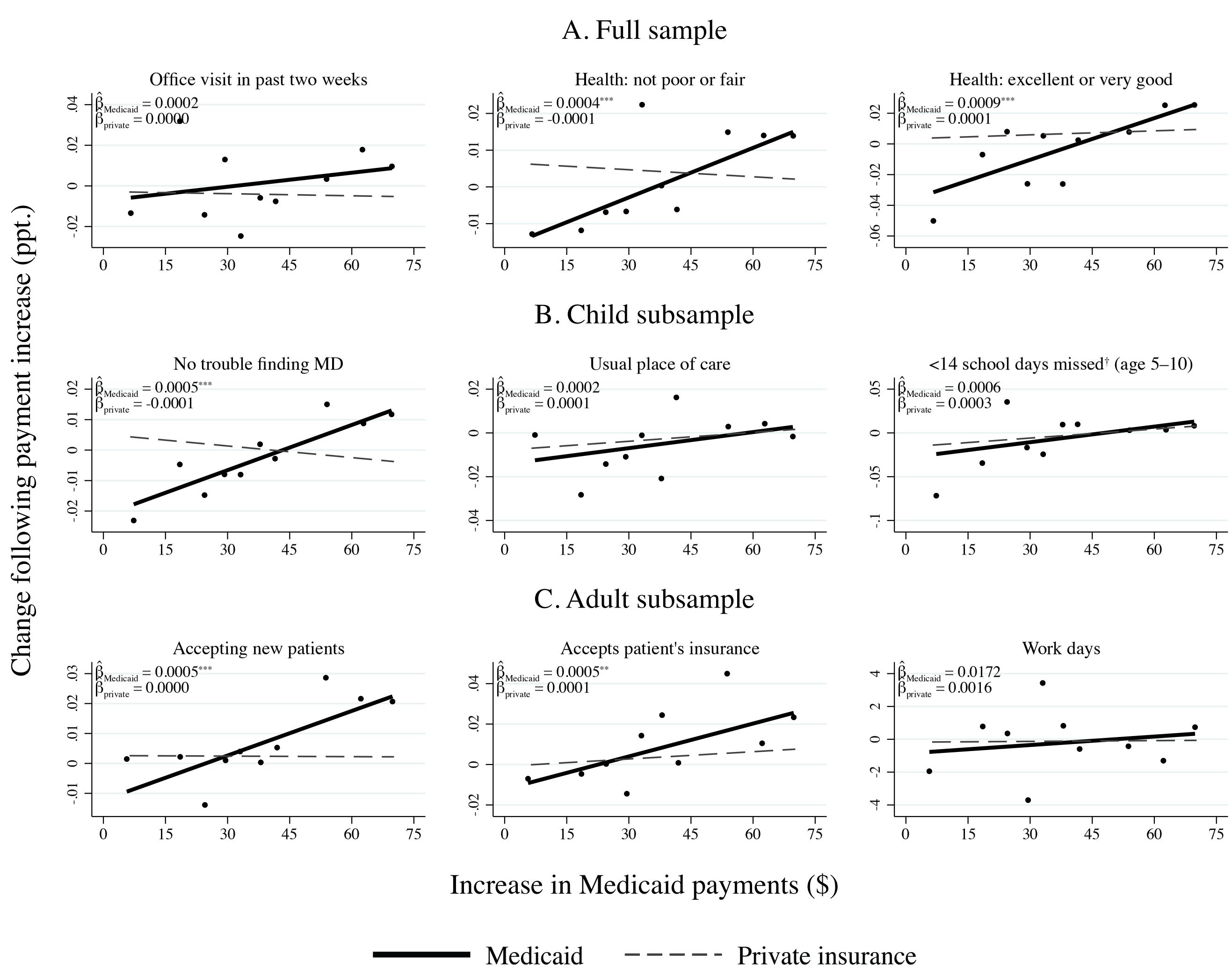
Notes: The above figure depicts Medicaid payments at the state-quarter level from 2009 to 2015. As defined in Section II.A, the payments are beneficiary-weighted averages of Medicaid fee-for-service and managed care payments for new patient office visits of mid-level complexity (CPT 99203); similar patterns are observed for other E&M 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 A2 provides analogous figures for each state individually.

Figure 2: Maps of state-level Medicaid payments



Notes: The above maps depict Medicaid payments for each state in 2009 (the first year in the sample period), 2012 (the year before the rate increase), 2013 (the first year of the rate increase), and 2015 (the first year after the federal mandate expired). As defined in Section II.A, the payments are beneficiary-weighted averages of Medicaid fee-for-service and managed care payments for new patient office visits of mid-level complexity (CPT 99203); similar patterns are observed for other E&M codes. The colors reflect quintiles of reimbursement levels in 2009; following the primary care rate increase in 2013, all states had Medicaid payments that were in the highest 2009 quintile.

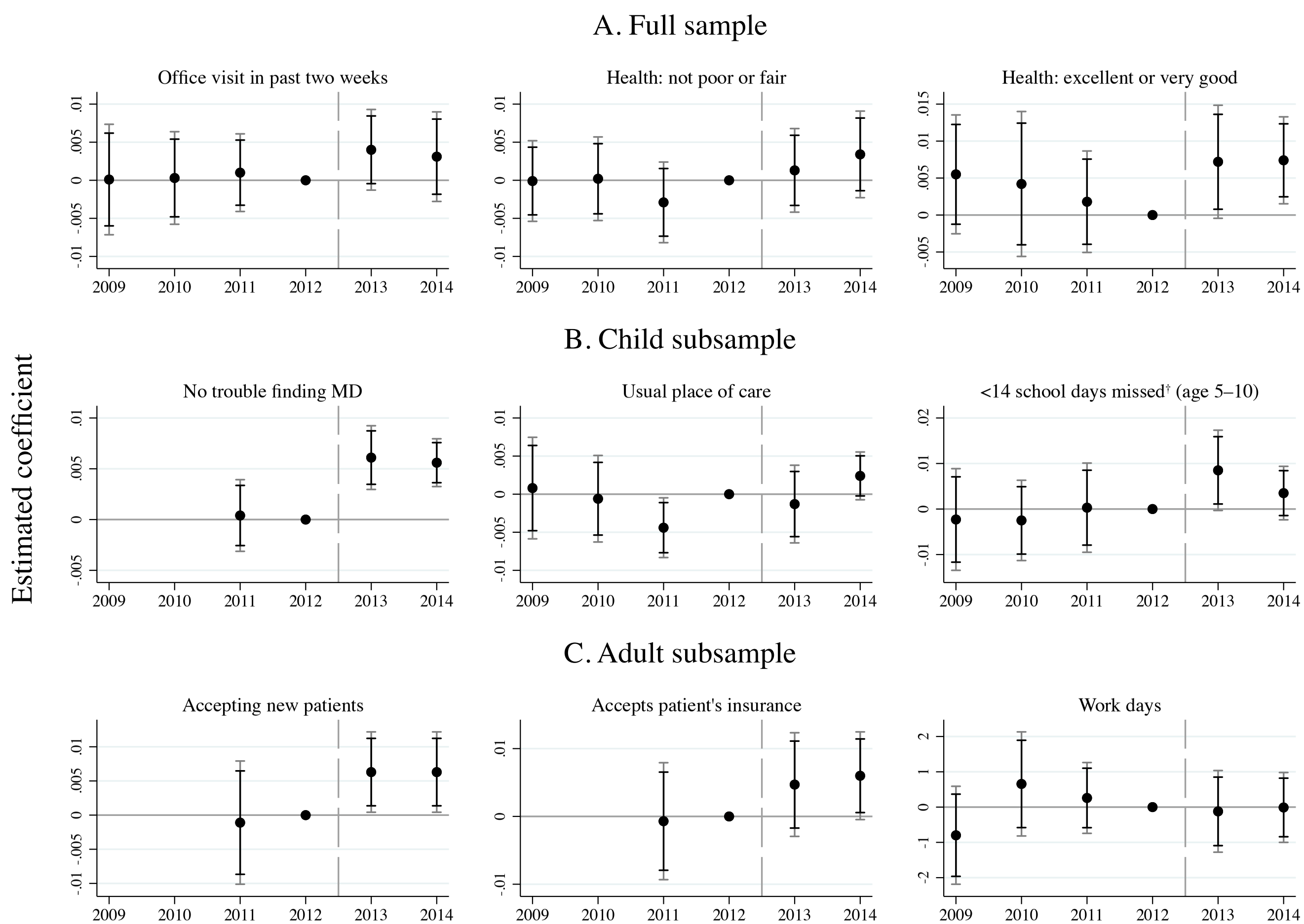
Figure 3: Changes in access, use, and health by size of payment increase



† Reflects absenteeism due to illness or injury over the previous year.

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 in dollars. The dots represent changes among Medicaid beneficiaries; the solid black line is the best fit line through these points. The dashed line depicts the best fit line for changes among respondents with private insurance. Outcomes come from the NHIS and are adjusted such that higher values denote better outcomes; Appendix A.2.1 outlines the exact survey questions and corresponding reference windows.

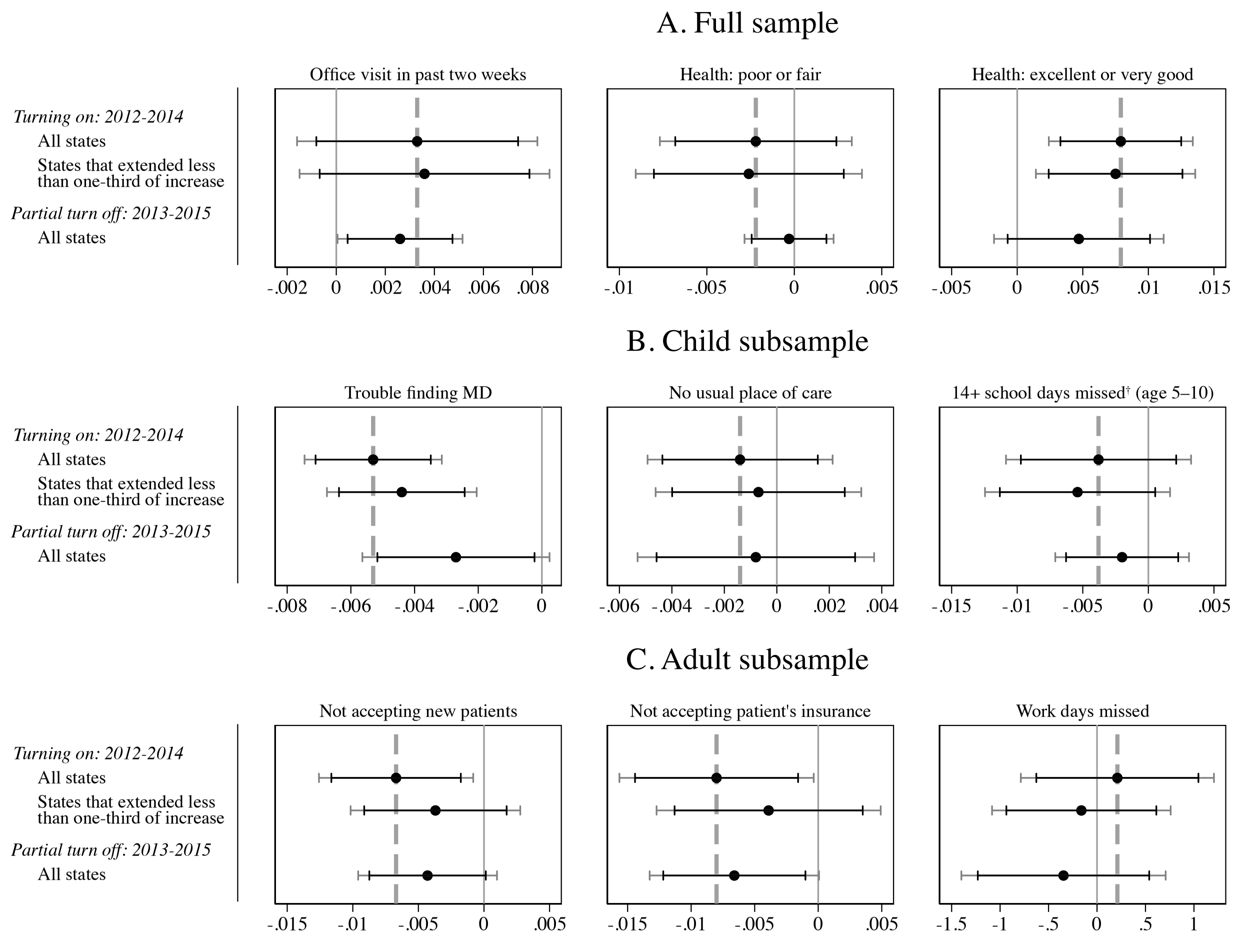
Figure 4: Event study: Effects of Medicaid payments on access, use, and health among Medicaid beneficiaries



† Reflects absenteeism due to illness or injury over the previous year.

Notes: The above figures plot the coefficients, 90% confidence intervals (dark bars), and 95% confidence intervals (light bars) on year indicators interacted with state-level changes in Medicaid payments in dollars induced by the primary care rate increase from estimation of equation (1). Results for Medicaid beneficiaries are shown; refer to Figure A5 for analogous results for individuals with private insurance. Outcomes come from the NHIS and are adjusted such that higher values denote better outcomes; Appendix A.2.1 outlines the exact survey questions and corresponding reference windows. Outcomes with missing coefficients in 2009 and 2010 were added to the NHIS in 2011.

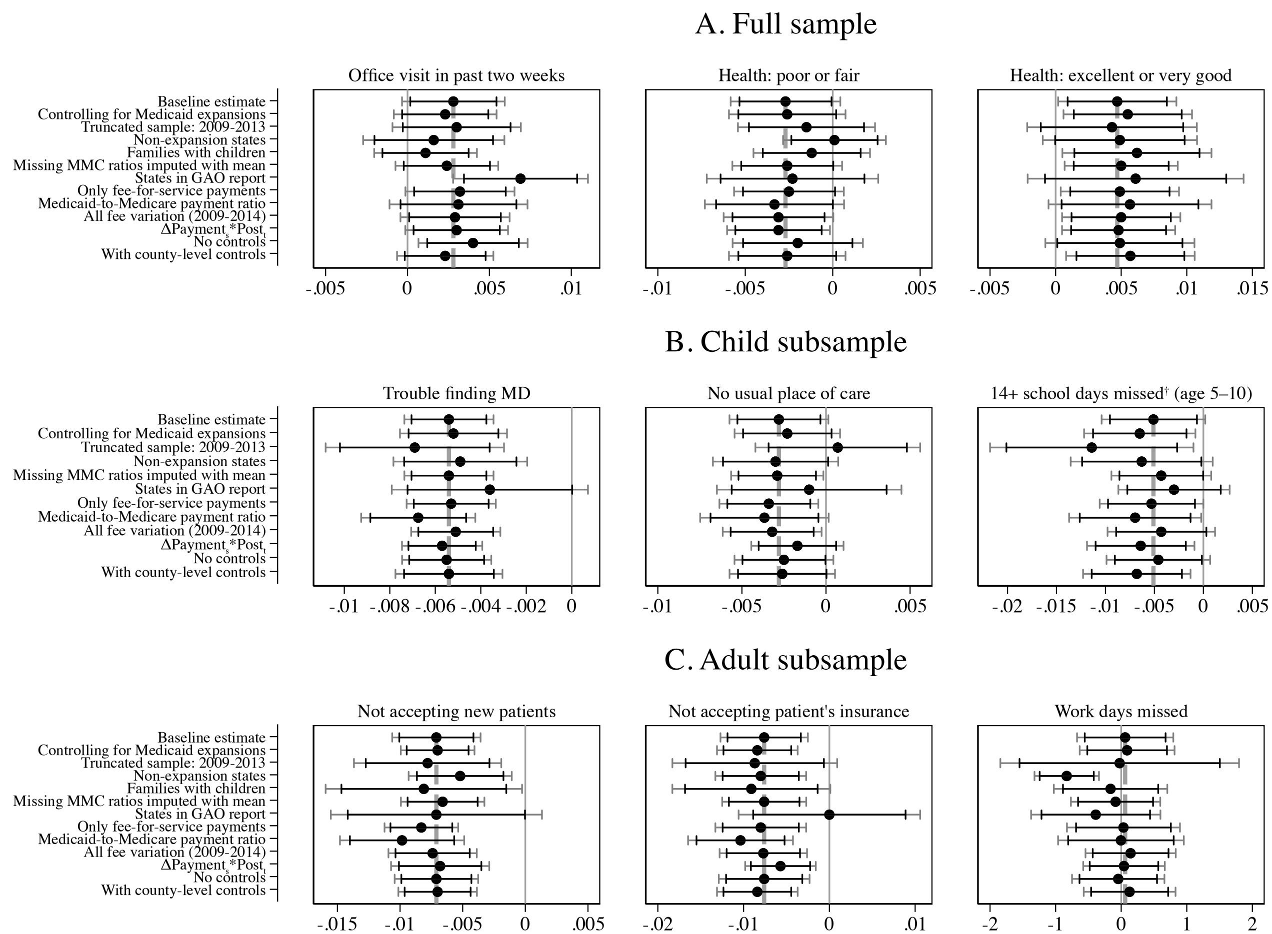
Figure 5: Effects of federal mandate turning on versus (partially) turning off



† Reflects absenteeism due to illness or injury over the previous year.

Notes: Each dot in the above figures depicts the estimated effect of a $10 increase in Medicaid payments from estimation of equation (2) using data from the time period and sample of states listed on the y-axis. The dark (light) horizontal bars depict 90% (95%) confidence intervals for each coefficient. Each coefficient comes from a separate regression and displays results for Medicaid beneficiaries. Although the third row in each subfigure exploits payment decreases, the estimated coefficients represent the effects of a $10 *increase* in Medicaid payments. As outlined in the text, many states maintained some of the increased payments into 2015, and thus the analysis leveraging data from 2013–2015 is only a "partial" turn off. The dashed vertical line in each subplot displays the coefficient estimate from the full sample (all states) using only data from 2012–2014 (rather than 2009–2014 as in our primary specification). Outcomes come from the NHIS; Appendix A.2.1 outlines the exact survey questions and corresponding reference windows.

Figure 6: Robustness: Effects of Medicaid payments on access, use, and health among Medicaid beneficiaries



† Reflects absenteeism due to illness or injury over the previous year.

Notes: Most dots in the above figures depict the estimated effects of a $10 increase in Medicaid payments from estimation of equation (2) for the subsample, alternative construction of the payment variable, or alternative set of controls listed on the y-axis. The two exceptions are the results for "Medicaid-to-Medicare payment ratio," which reflect the estimated effects of an increase in the payment ratio of 0.10, and the results for "$\Delta Payment_s * Post_t$," which report estimates from an analogue of equation (1) that pools the post-period coefficients. The dark (light) horizontal bars depict 90% (95%) confidence intervals for each coefficient. Each coefficient comes from a separate regression and displays results for Medicaid beneficiaries. The dashed vertical line in each subplot displays our baseline estimate from Table 3; these estimates come from estimation of equation (2), include data from 2009–2014 and all individual-level controls listed in Table A1, and exclude all non-policy variation and impute missing payment ratios with the median across all states in the GAO report when constructing the payment variable. Outcomes come from the 2009–2014 NHIS; Appendix A.2.1 outlines the exact survey questions and corresponding reference windows.

VII Tables

Table 1: Balancing regressions of Medicaid enrollment and sample composition on Medicaid payments

A. State-level data (CMS)		Medicaid enrollment		Medicaid managed care enrollment	
	$\mathbb{1}$\{Expansion\} (1)	Per capita (2)	Logs (3)	Per capita (4)	Share of Medicaid (5)
Medicaid payments ($10s)	0.0173	-0.0004	-0.0084	-0.0001	-0.0054
	(0.0250)	(0.0013)	(0.0065)	(0.0018)	(0.0110)
	[0.4839]	[0.7238]	[0.2053]	[0.9756]	[0.6233]
Observations	306	306	306	306	306
R^2	0.819	0.961	0.998	0.925	0.906
Baseline mean	0.140	0.177	14.31	0.089	0.496
B. Individual-level data (NHIS)		Medicaid beneficiaries			
	$\mathbb{1}$\{Medicaid\} (1)	≤FPL (2)	Black (3)	Hispanic (4)	Male (5)
Medicaid payments ($10s)	0.0024	-0.0008	-0.0036	-0.0048	-0.0017
	(0.0017)	(0.0031)	(0.0054)	(0.0048)	(0.0021)
	[0.1513]	[0.7861]	[0.5014]	[0.3156]	[0.4370]
Observations	596,474	96,080	96,080	96,080	96,080
R^2	0.332	0.220	0.236	0.338	0.044
Baseline mean	0.136	0.475	0.253	0.296	0.439

Notes: The above table shows associations between a $10 increase in Medicaid payments and potential confounders from estimation of equation (2) either at the state-year level (Panel A) or the individual-quarter level (Panel B) from 2009–2014. All regressions include state and time fixed effects. Regressions in Panel A additionally include all state-year controls denoted with an asterisk in Table A2 and are weighted by state population. Regressions in Panel B control for all individual-level controls listed in Table A1 and are weighted using the sample weights provided in the NHIS. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets. In Panel A, dates of Medicaid expansions come from Leung and Mas (2018), total Medicaid enrollment comes from CMS's National Health Expenditure Data, and Medicaid managed care enrollment comes from CMS's Medicaid Managed Care Enrollment Reports; all outcomes in Panel B come from the NHIS.

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 5–10)	3.433	2.887	3.516	2.933	3.292	2.792
School days missed† (age 11–17)	4.428	3.221	4.745	3.302	3.895	3.058
14+ school days missed† (age 5–10)	0.044	0.022	0.046	0.023	0.042	0.021
14+ school days missed† (age 11–17)	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

† Reflects absenteeism due to illness or injury over the previous year.

Notes: The reported statistics are weighted using the sample weights provided in the NHIS. Respondents are asked to report their health on a five-point scale as poor, fair, good, very good, or excellent; "good" is the omitted category above. Appendix A.2.1 outlines the exact survey questions and corresponding reference windows for all outcomes.

Table 3: Effects of Medicaid payments on access, use, and health

A. Full sample	Medicaid			Private		
	Office visit (2 weeks) (1)	Health $\leq$ fair (2)	Health $\geq$ very good (3)	Office visit (2 weeks) (4)	Health $\leq$ fair (5)	Health $\geq$ very good (6)
Medicaid payments ($10s)	0.0028 (0.0016) [0.0887]	-0.0027 (0.0016) [0.1067]	0.0047 (0.0023) [0.0451]	-0.0006 (0.0011) [0.6049]	0.0004 (0.0007) [0.5662]	0.0015 (0.0019) [0.4271]
Observations	95,969	96,019	96,019	336,644	337,041	337,041
R^2	0.071	0.295	0.231	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			
	Trouble finding MD (1)	No usual place of care (2)	14+ school absences[†] (age 5–10) (3)	14+ school absences[†] (age 11–17) (4)	Trouble finding MD (5)	No usual place of care (6)	14+ school absences[†] (age 5–10) (7)	14+ school absences[†] (age 11–17) (8)
Medicaid payments ($10s)	-0.0054 (0.0010) [$<$0.0001]	-0.0028 (0.0015) [0.0679]	-0.0051 (0.0027) [0.0676]	0.0043 (0.0040) [0.2837]	0.0015 (0.0005) [0.0086]	0.0002 (0.0010) [0.8750]	0.0029 (0.0043) [0.5061]	-0.0003 (0.0021) [0.8708]
Observations	16,745	21,211	6,662	6,762	26,229	33,911	10,049	14,905
R^2	0.015	0.021	0.034	0.045	0.006	0.029	0.024	0.020
Baseline mean	0.022	0.034	0.046	0.070	0.008	0.022	0.023	0.034

C. Adult subsample	Medicaid			Private		
	Not accepting new patients (1)	Not accepting patient's insurance (2)	Work days missed (3)	Not accepting new patients (4)	Not accepting patient's insurance (5)	Work days missed (6)
Medicaid payments ($10s)	-0.0071 (0.0018) [0.0002]	-0.0076 (0.0026) [0.0052]	0.0606 (0.3744) [0.8721]	0.0003 (0.0006) [0.6301]	-0.0007 (0.0006) [0.2847]	-0.0145 (0.1012) [0.8866]
Observations	14,800	14,799	6,293	79,692	79,682	76,792
R^2	0.036	0.037	0.074	0.006	0.008	0.009
Baseline mean	0.062	0.082	5.010	0.017	0.025	3.711

† Reflects absenteeism due to illness or injury over the previous year.

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of equation (2). Observations are at the individual level and cover 2009–2014. All regressions include state and quarter-year fixed effects and all individual-level controls listed in Table A1 (with age in five-year bins). Regressions are weighted using the sample weights provided in the NHIS. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets. Appendix A.2.1 outlines the exact survey questions and corresponding reference windows for all outcomes. Only adults with employment histories are asked to report days of missed work in the past year.

Table 4: Effects of Medicaid payments on disparities between publicly and privately insured

	Baseline mean					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: excellent or very good	0.562	0.726	0.164	0.0047	0.06	0.03	0.10	0.13	$348.94
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	-0.0028	-0.63	0.23	0.82	1.05	$42.86
14+ school days missed†	0.046	0.023	-0.023	-0.0051	-0.84	0.22	0.78	1.00	$45.10
C. Adult subsample									
Not accepting new patients	0.062	0.017	-0.045	-0.0071	-0.87	0.16	0.55	0.71	$63.38
Not acc. patient's insurance	0.082	0.025	-0.057	-0.0076	-0.71	0.13	0.47	0.60	$75.00

† Reflects absenteeism due to illness or injury over the previous year among children aged 5–10.

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 3. Column (5) is the implied elasticity of each outcome with respect to physician payments; this is computed using an average 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 (the median payment increase from the federal mandate), and $45 (the median payment difference between Medicaid and private insurance at baseline), 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 5: Effects of Medicaid payments on school absences (NAEP)

A. Free lunch eligible	0 days missed		3+ days missed	
	Grade 4	Grade 8	Grade 4	Grade 8
	(1)	(2)	(3)	(4)
Medicaid payments ($10s)	0.0042	0.0025	-0.0039	-0.0018
	(0.0015)	(0.0011)	(0.0008)	(0.0012)
	[0.0080]	[0.0352]	[<0.0001]	[0.1198]
Observations	487,863	394,788	487,863	394,788
R^2	0.010	0.018	0.005	0.012
Baseline mean	0.448	0.391	0.132	0.149

B. Free lunch ineligible	0 days missed		3+ days missed	
	Grade 4	Grade 8	Grade 4	Grade 8
	(1)	(2)	(3)	(4)
Medicaid payments ($10s)	0.0030	0.0002	-0.0008	-0.0010
	(0.0014)	(0.0014)	(0.0008)	(0.0009)
	[0.0330]	[0.8905]	[0.3601]	[0.2582]
Observations	553,310	531,958	553,310	531,958
R^2	0.009	0.014	0.004	0.007
Baseline mean	0.538	0.466	0.099	0.110

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of equation (3). Observations are at the individual level and cover 2009, 2011, 2013, and 2015. All regressions include state and year fixed effects, individual demographic controls available in the individual-level NAEP (sex, age, race, ethnicity), and all state-year controls denoted with an asterisk in Table A2. Regressions are weighted using the sample weights provided in the NAEP. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets. School days missed reflect absenteeism for any reason in the month preceding national math and reading assessments.

For Online Publication

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

Alexander and Schnell (2023)

A Data appendix

A.1 Medicaid reimbursement rates

We collected data on fee-for-service reimbursement rates for E&M services directly from state Medicaid offices. The raw 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.

A.1.1 Data completeness

We obtained complete rate information used to construct this panel from 44 states and the District of Columbia. 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 2012. 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, and WY) over the same period.

- Tennessee: We have no micro-data on reimbursement rates, as the state only uses Medicaid managed care. However, the state told us 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, and 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.

Given that only a few imputations are required, our results are robust to only using non-imputed data and to using alternative imputation strategies.

A.1.2 Medicaid managed care

The primary care rate increase applied to both Medicaid fee-for-service and Medicaid managed care programs. While states could simply 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 implement 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.

Additional payments were required to be passed through to qualified physicians regardless of the payment scheme used by MCOs for provider reimbursement. If MCOs did not pass through the increased payments to providers due to limited scope for enforcement, the rate increase would have created incentives for MCOs to attract additional enrollees. As shown in Table 1, we find no evidence that the rate increase led to increases in Medicaid managed care enrollment. Moreover, 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.

A.1.3 Primary payment variable

As outlined in Section II.A, we take managed care into account by creating an expected Medicaid payment measure. This measure combines state-level fee-for-service data with: (1) state-level managed care to fee-for-service payment ratios from the GAO, and (2) state-level Medicaid managed care enrollment shares from CMS. 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

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

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 quarter. 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 should have responded at the start of the rate increase rather than when the higher payments were actually released. This assumption is confirmed in our event study designs, which show that physicians responded similarly to the increased payments in 2013 and 2014. Incorporating implementation delays, as in Decker (2018) and Mulcahy et al. (2018), therefore biases results toward zero because some of the "pre-period" in such specifications was actually treated.

A.2 National Health Interview Survey

Our primary outcome measures come from the NHIS. Although much of the NHIS data is publicly available, geographic identifiers for areas smaller than census regions are restricted. To link our outcome measures to state-level variation in Medicaid reimbursement rates, we obtained access to confidential state identifiers. We further use confidential county identifiers to control for county characteristics in some analyses. All of our analyses using the NHIS data were therefore conducted in a Census Research Data Center.

Although 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 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.[39] 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.[40] 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 among patients with private insurance and Medicaid beneficiaries.

A.2.1 Survey questions

We use responses to the following eight questions in our analysis:

- 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?

[39] While using the Medicaid Analytic eXtract (MAX) data to corroborate our findings surrounding use of health care services would be a fruitful area for future research, we stress that the MAX data are not a substitute for the NHIS. In addition to providing no information on access or outcomes for patients with private insurance, the MAX data do not cover the entire United States. According to CMS, over 20 states have not submitted sufficient information to be included in data extracts for the entirety of our sample period, which limits the variation in payments that can be exploited. Although additional state-years are continuously being added, the transition from MAX to T-MSIS Analytic Files further complicates efforts to use comprehensive Medicaid claims data over this time period.

[40] 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 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.

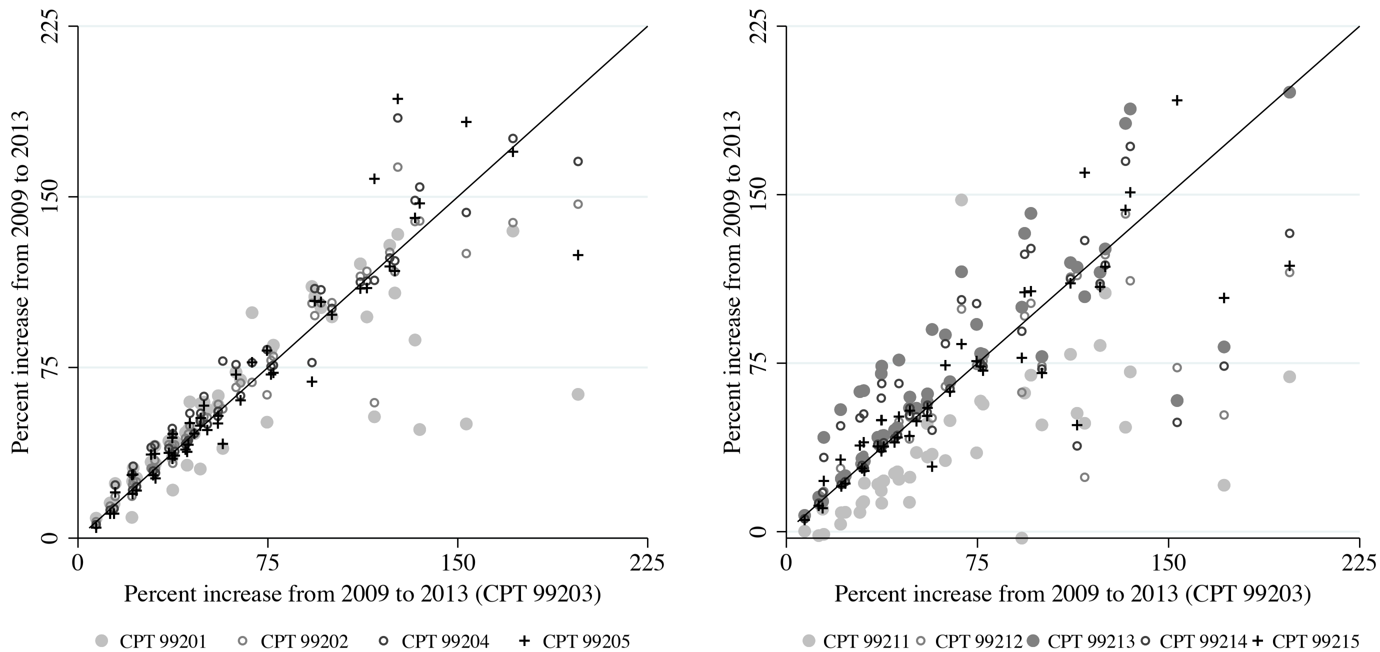
- Child subsample
  - During the past 12 months, did you have any trouble finding a general doctor or provider who would see {sample child}?
  - Is there a place that {sample child} usually goes when {he/she} is sick or you need advice about {his/her} health?
  - During the past 12 months, that is, since {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?

B Supplementary figures

Figure A1: Increases in Medicaid payments for E&M codes from 2009 to 2013

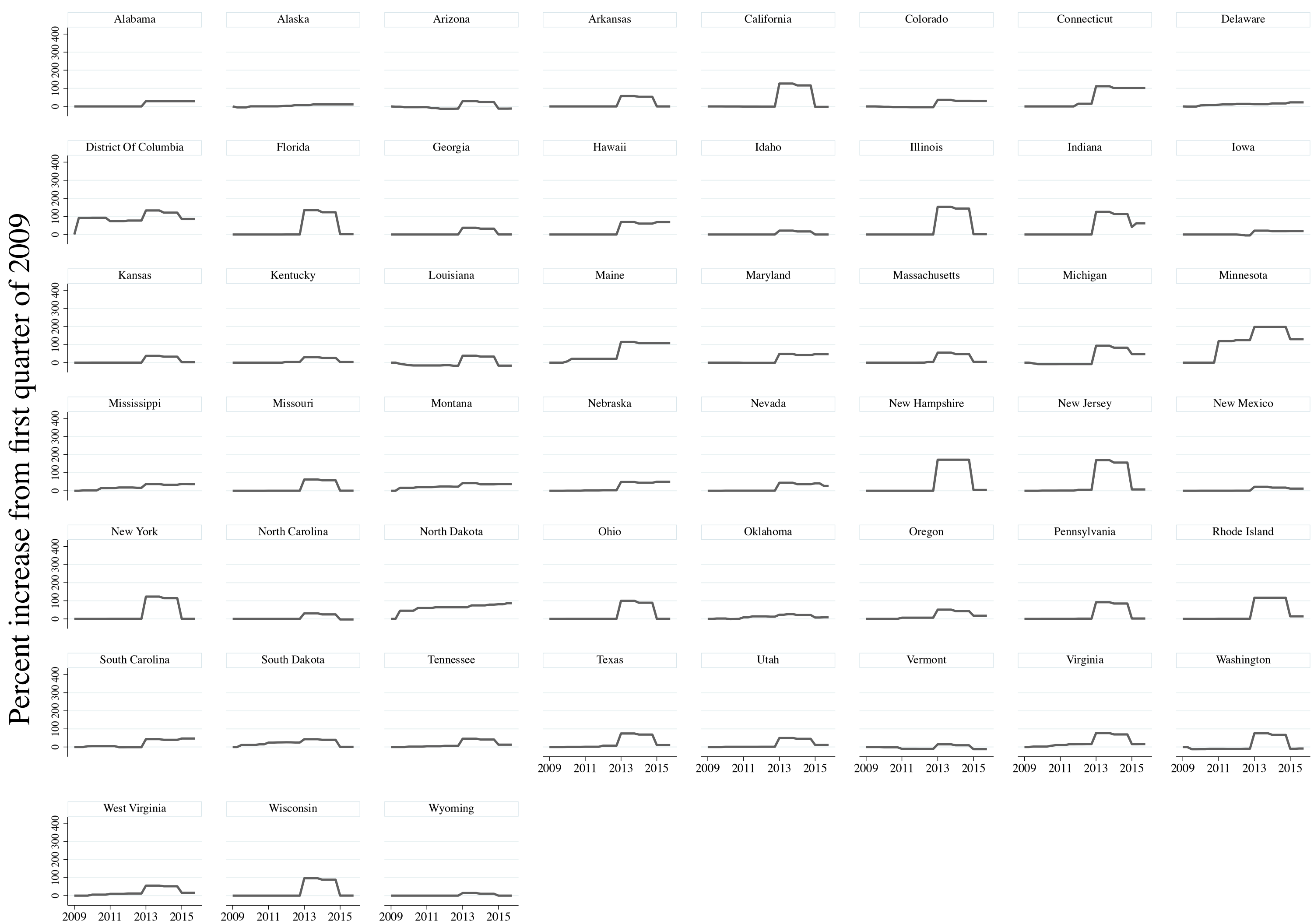
(a) New patient codes

(b) Established patient codes



Notes: The above figures display state-level percent changes from 2009 to 2013 in Medicaid payments for CPT code 99203 (the CPT code used in the majority of our analyses) versus analogous changes in other new patient codes (subfigure (a)) and established patient codes (subfigure (b)). As defined in Section II.A, the payments are beneficiary-weighted averages of Medicaid fee-for-service and managed care payments for each payment code. The black line in each subfigure is the 45-degree line. Subfigure (a) excludes CPT codes 99204 and 99205 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. Subfigure (b) excludes CPT codes 99214 and 99215 for New Jersey and CPT code 99214 for California; payments for these codes increased by 265 percent, 388 percent, and 241 percent, respectively. CPT code 99203 increased by 126 percent in California.

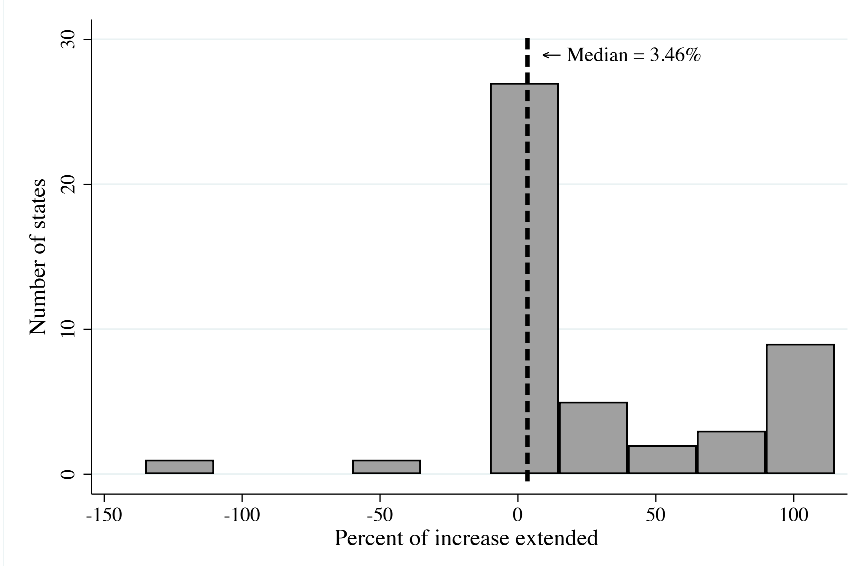
Figure A2: Primary Medicaid payment variable from 2009 to 2015 by state



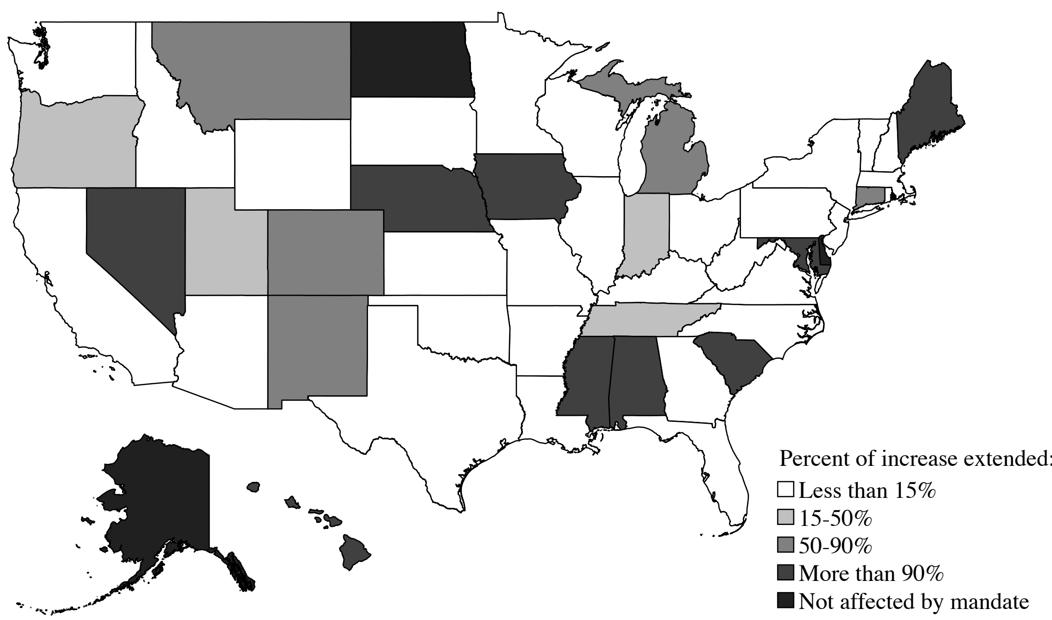
Notes: The above figure depicts the Medicaid payment variable used in the majority of our analyses at the state-quarter level from 2009 to 2015 relative to the relevant state-level rate in the first quarter of 2009. As defined in Section II.A, the payments are beneficiary-weighted averages of Medicaid fee-for-service and managed care payments for new patient office visits of mid-level complexity (CPT 99203); similar patterns are observed for other E&M codes.

Figure A3: Percent of payment increases maintained following mandate expiration

(a) Histogram of percent maintained



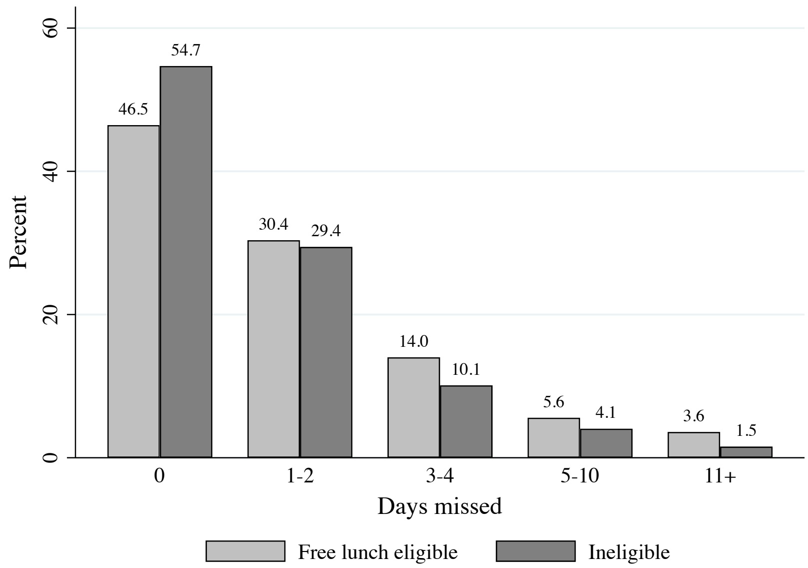
(b) Map of percent maintained



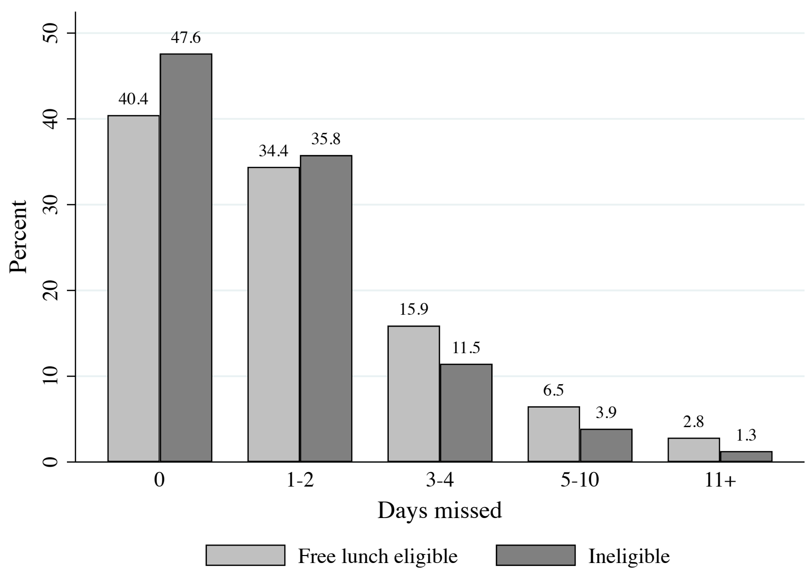
Notes: The above figures show the percent of payment increases from 2012 to 2013 that were maintained after the federal mandate expired at the end of 2014. Subfigure (a) presents a histogram of the percent maintained across states, and subfigure (b) presents a map showing which states chose to maintain payment increases into 2015. Alaska, Delaware, and North Dakota are excluded from subfigure (a). Alaska and North Dakota were unaffected by the mandate because their Medicaid payments exceeded the federally mandated Medicare level over the sample period. Although Delaware saw a slight increase in fee-for-service Medicaid payments as a result of the mandate (a 3.0 percent increase over the baseline rate, the lowest percent increase among all states other than Alaska and North Dakota), Medicaid payments in Delaware in 2012 were essentially equivalent to the 2013 Medicare rate when Medicaid managed care is taken into account.

Figure A4: Distribution of school absences by free lunch eligibility (NAEP)

(a) Grade 4

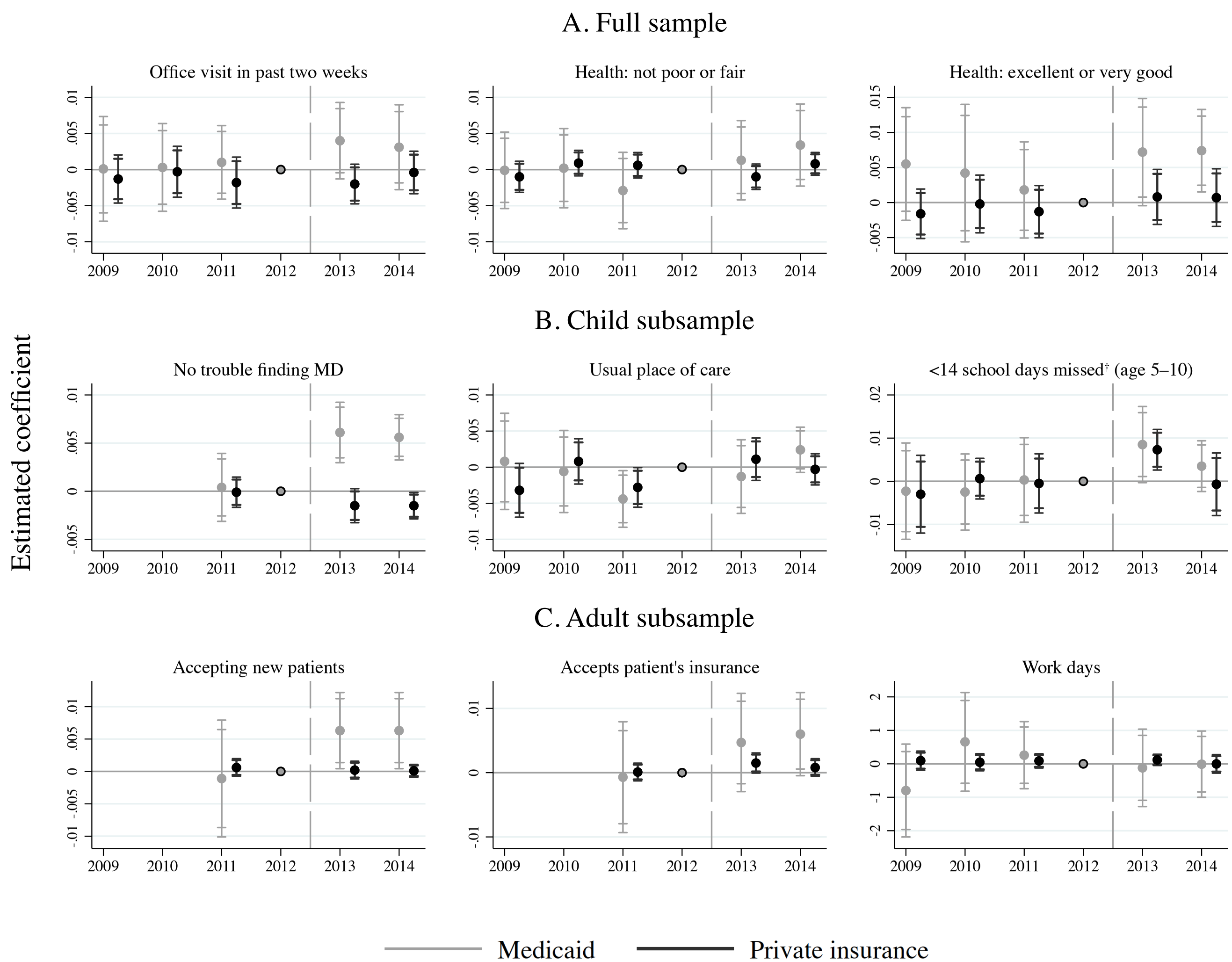


(b) Grade 8



Notes: The above figures display the average percent of students in grade 4 (subfigure (a)) and grade 8 (subfigure (b)) who missed 0, 1–2, 3–4, 5–10, or 11+ days in the month preceding their national math and reading assessments in 2009 and 2011. Observations are at the state-year level and are weighted by population. Data come from the NAEP.

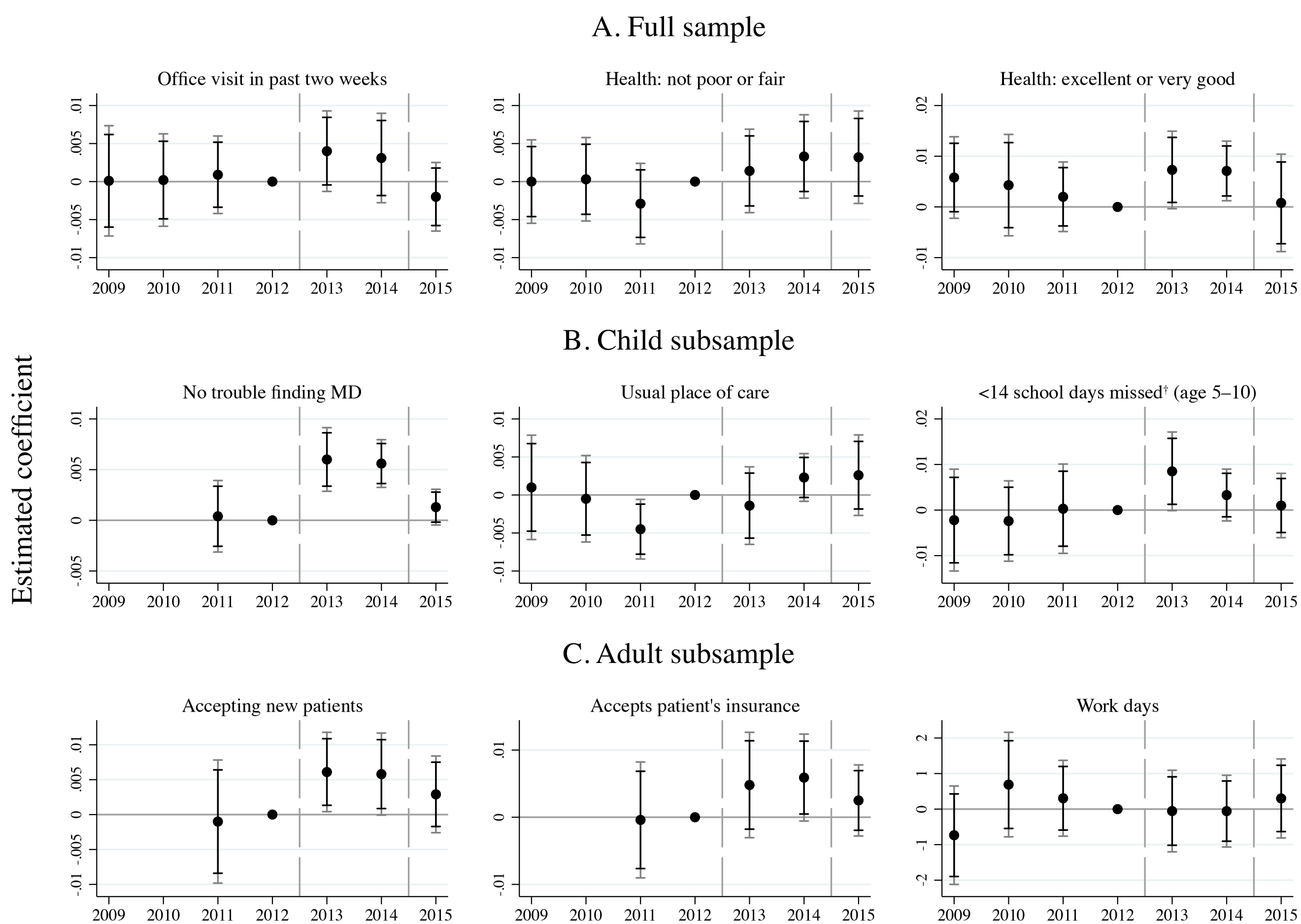
Figure A5: Event study: Effects among Medicaid beneficiaries versus the privately insured



† Reflects absenteeism due to illness or injury over the previous year.

Notes: The above figures plot the coefficients, 90% confidence intervals (short bars), and 95% confidence intervals (wide bars) on year indicators interacted with state-level changes in Medicaid payments in dollars 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 come from the NHIS and are adjusted such that higher values denote better outcomes; Appendix A.2.1 outlines the exact survey questions and corresponding reference windows. Outcomes with missing coefficients in 2009 and 2010 were added to the NHIS in 2011.

Figure A6: Event study: Effects following mandate expiration

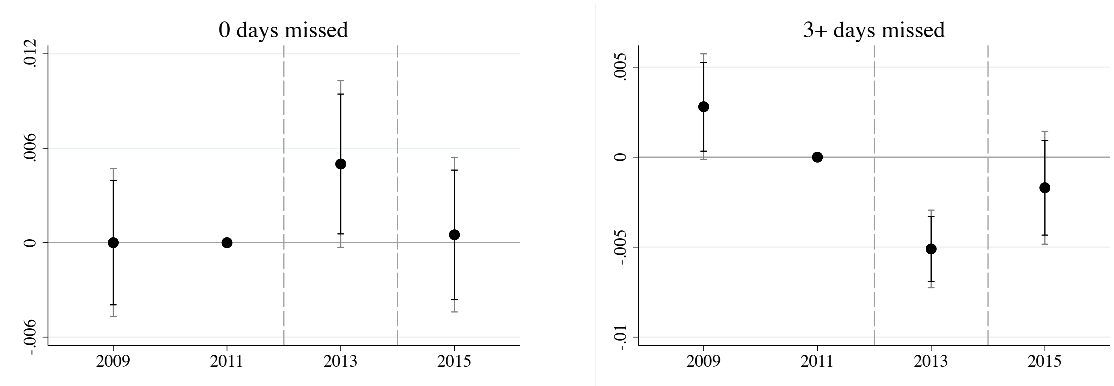


† Reflects absenteeism due to illness or injury over the previous year.

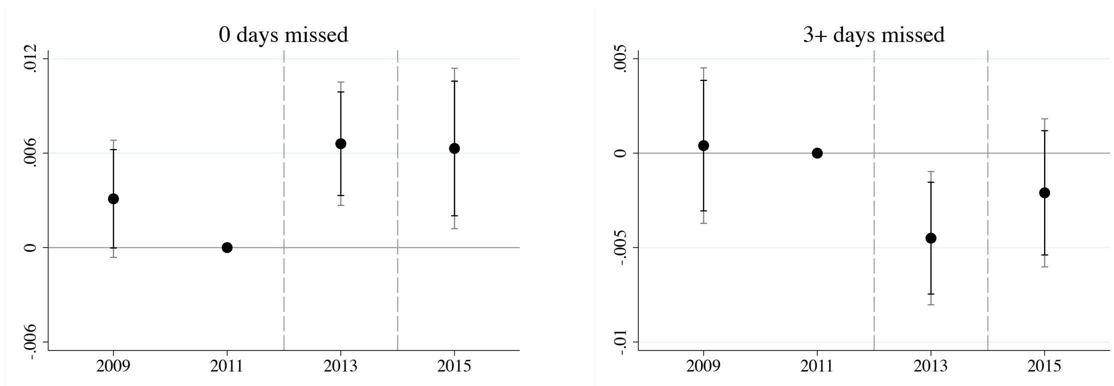
Notes: The above figures plot the coefficients, 90% confidence intervals (dark bars), and 95% confidence intervals (light bars) on year indicators interacted with state-level changes in Medicaid payments in dollars induced by the onset of the primary care rate increase from estimation of equation (1). Results for Medicaid beneficiaries are shown. The first vertical line in each subfigure marks the onset of the primary care rate increase in 2013; the second vertical line marks its expiration at the end of 2014. Because many states extended at least part of the increased payments through 2015 (see Figure A3), state-level changes in Medicaid payments stemming from the mandate's onset do not perfectly capture changes stemming from the mandate's expiration. Outcomes come from the NHIS and are adjusted such that higher values denote better outcomes; Appendix A.2.1 outlines the exact survey questions and corresponding reference windows. Outcomes with missing coefficients in 2009 and 2010 were added to the NHIS in 2011.

Figure A7: Event study: Effects on school absences (NAEP)

(a) Grade 4



(b) Grade 8



Notes: The above figures plot the coefficients, 90% confidence intervals (dark bars), and 95% confidence intervals (light bars) on year indicators interacted with state-level changes in Medicaid payments in dollars induced by the onset of the primary care rate increase from estimation of analogues of equation (1). Results for students eligible for free lunch are shown. The first vertical line in each subfigure marks the onset of the primary care rate increase in 2013; the second vertical line marks its expiration at the end of 2014. Because many states extended at least part of the increased payments through 2015 (see Figure A3), state-level changes in Medicaid payments stemming from the mandate's onset do not perfectly capture changes stemming from the mandate's expiration. Outcomes come from the NAEP.

C Supplementary tables

Table A1: Individual characteristics by insurance status

	All	Medicaid	Private
Demographics			
Male	0.489	0.439	0.489
Average age	37.3	24.3	38.4
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			
Other public assistance	0.127	0.483	0.035
Homeowner	0.660	0.346	0.773
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
Observations	603,074	96,128	338,174

Notes: Observations are at the individual level and cover 2009–2014; reported statistics reflect weighted averages using the NHIS sample weights. Some categories do not sum to one due to missing responses. "Other public assistance" is an indicator denoting whether the individual being interviewed received food stamp benefits or assistance (cash or otherwise) from a welfare program in the previous year. All data come from the NHIS.

Table A2: State characteristics by payments at baseline

Payment quintiles:	Q1	Q2	Q3	Q4	Q5
Average baseline payment	51	65	76	83	108
Medicaid enrollment					
Per capita	0.165	0.185	0.156	0.153	0.175
Managed care per capita	0.087	0.077	0.084	0.060	0.055
*Demographics**					
Male	0.491	0.492	0.492	0.494	0.497
Under 18	0.229	0.235	0.240	0.240	0.234
Aged 18–64	0.630	0.628	0.625	0.631	0.635
Over 65	0.141	0.137	0.135	0.129	0.131
White	0.795	0.731	0.798	0.800	0.739
Black	0.099	0.108	0.113	0.111	0.126
Hispanic	0.122	0.114	0.097	0.098	0.098
U.S. citizen	0.941	0.944	0.953	0.950	0.966
*Education**					
< High school	0.127	0.131	0.134	0.122	0.118
High school graduate	0.306	0.297	0.289	0.287	0.287
Some college	0.283	0.293	0.300	0.302	0.314
College+	0.285	0.279	0.277	0.288	0.281
*Family structure**					
Married	0.487	0.494	0.501	0.505	0.477
Never married	0.323	0.311	0.302	0.303	0.330
Separated	0.190	0.194	0.197	0.191	0.193
Avg. household size	2.58	2.59	2.61	2.56	2.51
*Household income**					
Income to poverty line: <1	0.140	0.148	0.156	0.144	0.152
Income to poverty line: 1-1.99	0.177	0.185	0.195	0.188	0.189
Income to poverty line: 2+	0.683	0.667	0.649	0.668	0.658
Number of states	11	10	10	10	10

Notes: Observations are at the state-year level and cover 2009–2012; reported statistics reflect averages taken over this baseline period. 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 sociodemographics come from the one-year ACS. Variable categories denoted with an asterisk are included as controls in the state-level regressions.

Table A3: Effects of Medicaid payments on school days missed (continuous measure)

School days missed†	Medicaid		Private	
	Age 5–10 (1)	Age 11–17 (2)	Age 5–10 (3)	Age 11–17 (4)
Medicaid payments ($10s)	-0.2021 (0.0802) [0.0150]	-0.0047 (0.2335) [0.9839]	0.0514 (0.0704) [0.4683]	0.0362 (0.0890) [0.6862]
Observations	6,662	6,762	10,049	14,905
R^2	0.040	0.056	0.031	0.028
Baseline mean	3.516	4.745	2.933	3.302

† Reflects absenteeism due to illness or injury over the previous year.

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of equation (2). Observations are at the individual level and cover 2009–2014. All regressions include state and quarter-year fixed effects and all individual-level controls listed in Table A1 (with age in five-year bins). Regressions are weighted using the sample weights provided in the NHIS. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets. Appendix A.2.1 outlines the exact survey question and corresponding reference window.

Table A4: Effects of Medicaid payments: Primary care shortage areas

A. Full sample	Medicaid			Private		
	Office visit (2 weeks) (1)	Health $\leq$ fair (2)	Health $\geq$ very good (3)	Office visit (2 weeks) (4)	Health $\leq$ fair (5)	Health $\geq$ very good (6)
MC payments ($10)	0.0025	-0.0024	0.0044	-0.0002	0.0008	0.0010
	(0.0016)	(0.0018)	(0.0025)	(0.0010)	(0.0006)	(0.0019)
	[0.1310]	[0.1808]	[0.0871]	[0.8135]	[0.2022]	[0.5975]
$\mathbb{1}\{\text{Shortage}\}$	-0.0074	0.0152	-0.0224	0.0040	0.0072	-0.0101
	(0.0115)	(0.0131)	(0.0192)	(0.0055)	(0.0040)	(0.0062)
	[0.5203]	[0.2513]	[0.2493]	[0.4658]	[0.0787]	[0.1085]
Payments * $\mathbb{1}\{\text{Shortage}\}$	0.0009	-0.0011	0.0014	-0.0006	-0.0009	0.0011
	(0.0015)	(0.0015)	(0.0022)	(0.0007)	(0.0005)	(0.0009)
	[0.5495]	[0.4642]	[0.5077]	[0.3802]	[0.0709]	[0.2449]
Observations	95,969	96,019	96,019	336,644	337,041	337,041
R^2	0.071	0.295	0.231	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			
	Trouble finding MD (1)	No usual place of care (2)	14+ school absences† (age 5–10) (3)	14+ school absences† (age 11–17) (4)	Trouble finding MD (5)	No usual place of care (6)	14+ school absences† (age 5–10) (7)	14+ school absences† (age 11–17) (8)
MC payments ($10)	-0.0055	-0.0027	-0.0037	0.0032	0.0014	0.0002	0.0036	-0.0004
	(0.0011)	(0.0015)	(0.0030)	(0.0049)	(0.0006)	(0.0012)	(0.0044)	(0.0022)
	[<0.0001]	[0.0777]	[0.2223]	[0.5140]	[0.0274]	[0.8819]	[0.4224]	[0.8686]
$\mathbb{1}\{\text{Shortage}\}$	-0.0042	0.0031	0.0157	-0.0084	0.0017	0.0006	0.0160	-0.0034
	(0.0125)	(0.0111)	(0.0279)	(0.0307)	(0.0051)	(0.0066)	(0.0114)	(0.0110)
	[0.7394]	[0.7775]	[0.5755]	[0.7863]	[0.7434]	[0.9235]	[0.1653]	[0.7593]
Payments * $\mathbb{1}\{\text{Shortage}\}$	0.0003	-0.0002	-0.0032	0.0020	0.0002	-0.0000	-0.0015	0.0001
	(0.0014)	(0.0012)	(0.0029)	(0.0037)	(0.0006)	(0.0008)	(0.0015)	(0.0014)
	[0.8041]	[0.8366]	[0.2698]	[0.5823]	[0.7958]	[0.9641]	[0.2988]	[0.9280]
Observations	16,745	21,211	6,662	6,762	26,229	33,911	10,049	14,905
R^2	0.015	0.021	0.034	0.045	0.006	0.029	0.024	0.020
Baseline mean	0.022	0.034	0.046	0.070	0.008	0.022	0.023	0.034

C. Adult subsample	Medicaid			Private		
	Not accepting new patients (1)	Not acc. patient's insurance (2)	Work days missed (3)	Not accepting new patients (4)	Not acc. patient's insurance (5)	Work days missed (6)
MC payments ($10)	-0.0069	-0.0064	0.1327	0.0002	-0.0006	-0.0725
	(0.0021)	(0.0024)	(0.3257)	(0.0007)	(0.0007)	(0.1151)
	[0.0017]	[0.0101]	[0.6855]	[0.7940]	[0.3680]	[0.5316]
$\mathbb{1}\{\text{Shortage}\}$	-0.0116	0.0078	1.8325	-0.0021	0.0020	-0.6126
	(0.0131)	(0.0157)	(2.9538)	(0.0055)	(0.0068)	(0.7489)
	[0.3805]	[0.6220]	[0.5378]	[0.6956]	[0.7674]	[0.4173]
Payments * $\mathbb{1}\{\text{Shortage}\}$	0.0002	-0.0022	-0.2167	0.0002	-0.0001	0.1120
	(0.0014)	(0.0016)	(0.3242)	(0.0007)	(0.0008)	(0.0851)
	[0.8925]	[0.1830]	[0.5069]	[0.7764]	[0.9457]	[0.1945]
Observations	14,800	14,799	6,293	79,692	79,682	76,792
R^2	0.036	0.037	0.074	0.006	0.008	0.009
Baseline mean	0.062	0.082	5.010	0.017	0.025	3.711

† Reflects absenteeism due to illness or injury over the previous year.

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of an augmented version of equation (2). Observations are at the individual level and cover 2009–2014. All regressions include state and quarter-year fixed effects and all individual-level controls listed in Table A1 (with age in five-year bins). Regressions are weighted using the sample weights provided in the NHIS. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets.

Table A5: Effects of Medicaid payments: 2009–2015

A. Full sample	Medicaid			Private		
	Office visit (2 weeks) (1)	Health ≤ fair (2)	Health ≥ very good (3)	Office visit (2 weeks) (4)	Health ≤ fair (5)	Health ≥ very good (6)
Medicaid payments ($10s)	0.0017 (0.0013) [0.2210]	-0.0011 (0.0010) [0.2536]	0.0039 (0.0023) [0.0920]	-0.0002 (0.0009) [0.8395]	0.0000 (0.0005) [0.9793]	0.0009 (0.0014) [0.5213]
Observations	115,663	115,720	115,720	397,479	397,909	397,909
R^2	0.070	0.287	0.229	0.036	0.077	0.137
Baseline mean	0.197	0.176	0.562	0.175	0.062	0.726

B. Child subsample	Medicaid				Private			
	Trouble finding MD (1)	No usual place of care (2)	14+ school absences† (age 5–10) (3)	14+ school absences† (age 11–17) (4)	Trouble finding MD (5)	No usual place of care (6)	14+ school absences† (age 5–10) (7)	14+ school absences† (age 11–17) (8)
Medicaid payments ($10s)	-0.0040 (0.0010) [0.0003]	-0.0029 (0.0018) [0.1137]	-0.0041 (0.0021) [0.0553]	0.0054 (0.0037) [0.1565]	0.0006 (0.0005) [0.2836]	0.0004 (0.0008) [0.6192]	0.002 (0.0032) [0.5346]	-0.0014 (0.0016) [0.3589]
Observations	21,044	25,515	8,049	8,276	32,669	40,353	11,961	17,694
R^2	0.013	0.020	0.032	0.041	0.005	0.028	0.022	0.020
Baseline mean	0.022	0.034	0.046	0.070	0.008	0.022	0.023	0.034

C. Adult subsample	Medicaid			Private		
	Not accepting new patients (1)	Not accepting patient's insurance (2)	Work days missed (3)	Not accepting new patients (4)	Not accepting patient's insurance (5)	Work days missed (6)
Medicaid payments ($10s)	-0.0057 (0.0016) [0.0008]	-0.0064 (0.0022) [0.0063]	-0.3187 (0.3266) [0.3338]	-0.0004 (0.0005) [0.4142]	-0.0008 (0.0006) [0.1920]	0.0632 (0.0687) [0.3624]
Observations	19,230	19,226	8,065	99,976	99,963	92,205
R^2	0.030	0.034	0.057	0.006	0.008	0.009
Baseline mean	0.062	0.082	5.010	0.017	0.025	3.711

† Reflects absenteeism due to illness or injury over the previous year.

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of equation (2). Observations are at the individual level and cover 2009–2015. All regressions include state and quarter-year fixed effects and all individual-level controls listed in Table A1 (with age in five-year bins). Regressions are weighted using the sample weights provided in the NHIS. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets. Appendix A.2.1 outlines the exact survey questions and corresponding reference windows. Only adults with employment histories are asked to report days of missed work in the past year.

Table A6: Balancing regressions of FQHC presence and use on Medicaid payments

	Grantees		Sites		Encounters	
	Per million	Logs	Per 100,000	Logs	Per capita	Logs
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid payments ($10s)	0.0073	-0.0058	0.0397	0.0121	0.0018	0.0024
	(0.0272)	(0.0086)	(0.0239)	(0.0107)	(0.0016)	(0.0069)
	[0.7889]	[0.5043]	[0.1027]	[0.2643]	[0.2677]	[0.7310]
Observations	303	303	303	303	300	300
R^2	0.962	0.991	0.982	0.989	0.992	0.998
Baseline mean	3.604	3.452	2.626	5.412	0.249	14.60

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of a state-level analogue of equation (2). Observations are at the state-year level and cover 2009–2014. All regressions include state and year fixed effects and all state-level controls denoted with an asterisk in Table A2. Regressions are weighted by state population. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets. Outcomes were provided by the NACHC and are based on information from the HRSA's Uniform Data System; data are missing for New Mexico in 2011 and Nevada and Virginia in 2012.

Table A7: Effects of Medicaid payments: Triple difference model with interacted controls

A. Full sample	Office visit (2 weeks) (1)	Health: poor or fair (2)	Health: excellent or very good (3)
Medicaid payments ($10s)	-0.0006	0.0004	0.0015
	(0.0011)	(0.0007)	(0.0019)
	[0.5853]	[0.5275]	[0.4327]
$\mathbb{1}$\{Medicaid\}	-0.0642	0.0841	-0.2551
	(0.0361)	(0.0365)	(0.0599)
	[0.0811]	[0.0254]	[0.0001]
Medicaid payments * $\mathbb{1}$\{Medicaid\}	0.0034	-0.0031	0.0032
	(0.0020)	(0.0018)	(0.0032)
	[0.0953]	[0.0932]	[0.3245]
Observations	429,894	430,338	430,338
R^2	0.044	0.178	0.174
Baseline mean	0.179	0.082	0.698

B. Child subsample	Trouble finding MD (1)	No usual place of care (2)	14+ school days missed† (age 5–10) (3)	14+ school days missed† (age 11–17) (4)
Medicaid payments ($10s)	0.0016	0.0001	0.0036	-0.0002
	(0.0006)	(0.0011)	(0.0042)	(0.0020)
	[0.0093]	[0.9452]	[0.3956]	[0.9306]
$\mathbb{1}$\{Medicaid\}	0.1492	-0.0692	0.0547	-0.0894
	(0.0215)	(0.0371)	(0.0928)	(0.0566)
	[<0.0001]	[0.0679]	[0.5586]	[0.1206]
Medicaid payments * $\mathbb{1}$\{Medicaid\}	-0.0070	-0.0028	-0.0087	0.0045
	(0.0012)	(0.0021)	(0.0052)	(0.0036)
	[<0.0001]	[0.1749]	[0.1042]	[0.2140]
Observations	42,486	54,509	16,511	21,437
R^2	0.015	0.027	0.034	0.036
Baseline mean	0.013	0.026	0.029	0.042

C. Adult subsample	Not accepting new patients (1)	Not accepting patient's insurance (2)	Work days missed (3)
Medicaid payments ($10s)	0.0004	-0.0006	-0.0067
	(0.0006)	(0.0006)	(0.1008)
	[0.5028]	[0.3229]	[0.9469]
$\mathbb{1}$\{Medicaid\}	0.1264	0.0922	6.5047
	(0.0344)	(0.0448)	(7.3522)
	[0.0006]	[0.0448]	[0.3805]
Medicaid payments * $\mathbb{1}$\{Medicaid\}	-0.0075	-0.0070	0.0674
	(0.0019)	(0.0026)	(0.4158)
	[0.0002]	[0.0092]	[0.8720]
Observations	94,037	94,025	82,870
R^2	0.024	0.028	0.017
Baseline mean	0.022	0.031	3.785

† Reflects absenteeism due to illness or injury over the previous year.

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of an augmented version of equation (2). Observations are at the individual level and cover 2009–2014. All regressions include state and quarter-year fixed effects and all individual-level controls listed in Table A1 (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 A8 for results from specifications without interacted controls. Regressions are weighted using the sample weights provided in the NHIS. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets.

Table A8: Effects of Medicaid payments: Triple difference without interacted controls

A. Full sample	Office visit (2 weeks) (1)	Health: poor or fair (2)	Health: excellent or very good (3)
Medicaid payments ($10s)	0.0002	0.0004	0.0014
	(0.0009)	(0.0006)	(0.0017)
	[0.8705]	[0.5712]	[0.4159]
𝟙{Medicaid}	0.0669	0.1141	-0.1357
	(0.0108)	(0.0076)	(0.0140)
	[<0.0001]	[<0.0001]	[<0.0001]
Medicaid payments * 𝟙{Medicaid}	-0.0005	-0.0024	0.0034
	(0.0008)	(0.0007)	(0.0015)
	[0.5032]	[0.0017]	[0.0273]
Observations	429,894	430,338	430,338
R^2	0.040	0.128	0.163
Baseline mean	0.179	0.082	0.698

B. Child subsample	Trouble finding MD (1)	No usual place of care (2)	14+ school days missed† (age 5–10) (3)	14+ school days missed† (age 11–17) (4)
Medicaid payments ($10s)	-0.0004	-0.0009	0.0009	0.0022
	(0.0005)	(0.0008)	(0.0028)	(0.0021)
	[0.4276]	[0.2953]	[0.7484]	[0.2932]
𝟙{Medicaid}	0.0262	0.0026	0.0386	0.0520
	(0.0055)	(0.0065)	(0.0117)	(0.0160)
	[<0.0001]	[0.6920]	[0.0017]	[0.0020]
Medicaid payments * 𝟙{Medicaid}	-0.0017	-0.0007	-0.0024	-0.0030
	(0.0005)	(0.0007)	(0.0012)	(0.0016)
	[0.0020]	[0.3179]	[0.0528]	[0.0703]
Observations	42,486	54,509	16,511	21,437
R^2	0.009	0.021	0.021	0.025
Baseline mean	0.013	0.026	0.029	0.042

C. Adult subsample	Not accepting new patients (1)	Not accepting patient's insurance (2)	Work days missed (3)
Medicaid payments ($10s)	0.0001	-0.0008	-0.0191
	(0.0006)	(0.0007)	(0.0943)
	[0.8506]	[0.2811]	[0.8399]
𝟙{Medicaid}	0.0676	0.0870	-1.9492
	(0.0123)	(0.0119)	(1.1443)
	[<0.0001]	[<0.0001]	[0.0947]
Medicaid payments * 𝟙{Medicaid}	-0.0041	-0.0044	0.1242
	(0.0011)	(0.0011)	(0.1117)
	[0.0006]	[0.0002]	[0.2718]
Observations	94,037	94,025	82,870
R^2	0.016	0.020	0.009
Baseline mean	0.022	0.031	3.785

† Reflects absenteeism due to illness or injury over the previous year.

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of an augmented version of equation (2). Observations are at the individual level and cover 2009–2014. All regressions include state and quarter-year fixed effects and all individual-level controls listed in Table A1 (with age in five-year bins). In contrast to the specification used in Table A7, 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. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets.

D Supplementary outcomes

As school absenteeism is closely linked to test scores, it is possible that the reductions in absenteeism that we document could lead to improvements in academic performance (Gottfried, 2011; Goodman, 2014; Liu et al., 2021). We note, however, that finding effects on test scores in our setting is unlikely. In 2011, for example, the average score on the national math assessment among the 45.6 percent of free lunch–eligible fourth graders who missed 0 days in the month preceding the exam was 231.6, compared to 228.4 among the 30.6 percent who missed 1–2 days and 222.8 among the 23.8 percent who missed 3+ days. If a $10 increase in Medicaid payments shifts approximately 0.4 percentage points of students from missing 3+ days to missing 0 days, as is suggested by the results in Table 5, then this would change the average test score among free lunch–eligible fourth graders from 228.51 to only 228.55. This increase is less than one percent of the standard deviation in average state-level math scores among free lunch–eligible fourth graders.

Nevertheless, to examine the effects of increased Medicaid payments on test scores, we use information from the publicly available, state-level files and estimate an analogue of equation (3) using data from 2009, 2011, 2013, and 2015:

$$Outcome_{sy} = \beta \cdot Payment_{sy} + \gamma X_{sy} + \lambda_s + \lambda_y + \epsilon_{sy} \tag{A1}$$

where $Outcome_{sy}$ denotes an average schooling outcome in state s in year y, and all other variables are defined as in equation (3). We weight the regressions by state population and cluster standard errors by state.

Results from this analysis are shown in Table A9. Panel A begins by showing effects of Medicaid payments on average state-level absences. When aggregating to the state level, we find evidence of improvements in attendance that are generally slightly smaller and less precise than in the individual-level data. This is to be expected for two reasons. First, we are able to include individual-level controls for key demographics (i.e., age, sex, race, and ethnicity) in the individual-level regressions, whereas we can only include state-level aggregates of these controls from the ACS in the state-level analogues. Second, in the individual-level data we can observe students who are individually eligible for free lunch whereas the flag for free lunch in the state-level aggregates includes students whose eligibility was determined at the school level (that is, children whose household income is above the threshold for individual eligibility but who attend schools in which everyone qualifies for free lunch due to a high number of individually eligible students). Because students whose eligibility is determined at the school-level are less likely to qualify for Medicaid than those who are individually eligible, our proxy for Medicaid coverage in the state-level NAEP is less precise than in the individual-level NAEP.

Panel B of Table A9 provides results for test scores. As expected, we find no effects of increased physician reimbursement under Medicaid on average state-level scores on national math and reading assessments.

Table A9: 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 (%)	
	Grade 4 (1)	Grade 8 (2)	Grade 4 (3)	Grade 8 (4)	Grade 4 (5)	Grade 8 (6)	Grade 4 (7)	Grade 8 (8)
Medicaid payments ($10s)	0.0036	0.0013	-0.0042	-0.0013	0.0027	0.0000	-0.0005	-0.0002
	(0.0016)	(0.0014)	(0.0009)	(0.0012)	(0.0020)	(0.0018)	(0.0012)	(0.0009)
	[0.0250]	[0.3837]	[0.0001]	[0.2608]	[0.1909]	[0.9977]	[0.6979]	[0.8268]
Observations	199	199	199	199	199	199	199	199
R^2	0.938	0.930	0.893	0.922	0.825	0.892	0.788	0.882
Baseline mean	0.465	0.404	0.232	0.252	0.547	0.476	0.157	0.166

B. Test scores	Free lunch eligible				Free lunch ineligible			
	Math		Reading		Math		Reading	
	Grade 4 (1)	Grade 8 (2)	Grade 4 (3)	Grade 8 (4)	Grade 4 (5)	Grade 8 (6)	Grade 4 (7)	Grade 8 (8)
Medicaid payments ($10s)	-0.0087	0.0003	-0.0057	0.0208	-0.0602	0.0073	-0.0413	0.0390
	(0.0269)	(0.0444)	(0.0401)	(0.0514)	(0.0363)	(0.0236)	(0.0396)	(0.0409)
	[0.7491]	[0.9946]	[0.8868]	[0.6873]	[0.1040]	[0.7584]	[0.3016]	[0.3445]
Observations	199	199	199	199	199	199	199	199
R^2	0.935	0.922	0.916	0.869	0.901	0.930	0.906	0.916

Notes: The above table shows the estimated effects of a $10 increase in Medicaid payments from estimation of equation (A1). Observations are at the state-year level and cover 2009, 2011, 2013, and 2015. 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. All regressions include state and year fixed effects and all state-level controls denoted with an asterisk in Table A2. Regressions are weighted by state population. Standard errors are clustered by state and are reported in parentheses; p-values are reported in brackets.