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Researching the causes and consequences of unemployment

Medicaid, Family Spending, and the Financial Implications of Crowd-Out

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ABSTRACT

A primary purpose of health insurance is to protect families from medical expenditure risk. Despite this goal and despite the fact that research has found that Medicaid can crowd out private coverage, little is known about the effect of Medicaid on families' spending patterns. This paper implements a simu-lated instrumental variables strategy with data from the Consumer Expendi-ture Survey to estimate the effect of an additional family member becoming eligible for Medicaid on family-level health insurance coverage and spending. The results indicate that an additional family member becoming eligible for Medicaid increases the number of people in the family with Medicaid coverage by about 0.135 to 0.142 and decreases the likelihood that a family has any medical spending in a quarter by 2.7 percentage points. As previous research often finds with different data sets, I find evidence that Medicaid expansions crowd out some private coverage. Unlike most other data sets, the Consumer Expenditure Survey allows for considering the financial implications of crowd-out. The results indicate that families that transition from private coverage to Medicaid are able to spend significantly less on health insurance expenses, meaning Medicaid expansions can be welfare improving for families even when crowd-out occurs.

JEL Codes: D12, I13

Key Words: Medicaid eligibility, crowd-out, family spending

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Medicaid provides free or cheap health insurance to individuals with low incomes and has the potential to transform families' spending patterns. According to the 2014 Consumer Expenditure Survey (CEX), 23 percent of households with at least one person with Medicaid coverage have any quarterly medical spending, while 44 percent of households without Medicaid do. Meanwhile, only 38 percent of households with at least one person with Medicaid coverage have any quarterly health insurance spending, whereas 73 percent without Medicaid do. These numbers are consistent with Medicaid being a financial boon to households. But as Medicaid is a means-tested program, these numbers could also reflect that poorer families that are able to spend less on medical care and health insurance are more likely to be eligible for Medicaid.

Despite a key goal of health insurance being to protect families from medical expenditure risk, little is known about the effect of Medicaid on financial outcomes. As Buchmueller, Ham, and Shore-Sheppard (2015) explain in their recent review of economics research on Medicaid, "Given that a fundamental purpose of health insurance is to protect individuals and families from the financial burden of large medical expenditures, there is surprisingly little research on the effect of Medicaid on financial outcomes." The research on the financial effects of Medicaid that exists often focuses on extreme spending events and finds that Medicaid reduces bankruptcies and the number of bills going to collections (Finkelstein et al. 2012; Gross and Notowidigdo 2011; Hu et al. 2016). In contrast to the literature on the financial effects of Medicaid, the literature that studies Medicaid coverage crowding out private coverage is large,

but a key issue that has received little attention is what the financial implications of this crowd-out are for families.[1]

The goals of this paper are to understand how Medicaid eligibility affects families' spending and to consider the welfare implications of spending effects. To estimate the effect of Medicaid eligibility on families' spending, I implement a simulated instrumental variables (IV) strategy using data from the CEX, which is a data set collected by the Bureau of Labor Statistics (BLS) that tracks the expenditures of U.S. households over the past quarter of the year. I focus on families with incomes less than 200 percent of the federal poverty level (FPL) and use variation in Medicaid eligibility due to legislative changes during the 2000s.[2] During this time period, several states changed their eligibility rules for children, but the majority of changes come from states expanding Medicaid coverage for parents, meaning that the bulk of the identifying variation comes from parents becoming eligible for Medicaid. The CEX provides two main advantages over many other data sets. First, it allows for studying how Medicaid eligibility affects medical spending for a nationally representative sample of the U.S. population. Second, the CEX allows for studying how crowd-out affects families' spending on health insurance.

I first consider the effect of Medicaid eligibility on health insurance coverage using the CEX data. I estimate that an additional person in a family being eligible

[1] A sample of the literature, much of which I discuss later, includes Bronchetti (2014), Buchmeuller et al. (2005), Busch and Duchovny (2005), Cutler and Gruber (1996), Gruber and Simon (2008), Ham and Shore-Sheppard (2005a), Ham, Ozbeklik, and Shore-Sheppard (2014), Hamersma and Kim (2013), Koch (2013), Koch (2015), LoSasso and Buchmueller (2004), and Shore-Sheppard (2008).

[2] I use variation in both Medicaid and the State Children's Health Insurance Program (CHIP) eligibility. As is common in this literature, I refer to both Medicaid and CHIP as Medicaid for ease of discourse, even though the two are distinct programs in many states.

for Medicaid increases the number of people in the family with Medicaid coverage by 0.135 to 0.142 and decreases the number of private health insurance plans paid for by families by an average of 0.053 to 0.055. These estimates are similar to estimates that I find using the March Current Population Surveys (CPS) and the Survey of Income and Program Participation (SIPP) and fall within the range of other estimates of the effect of Medicaid eligibility on health insurance coverage.

I then consider how Medicaid eligibility affects spending. I find that an additional person being eligible for Medicaid reduces the likelihood that a family has any spending on medical care in a quarter by 2.7 percentage points. The bulk of this decrease appears to come from families who had relatively small quarterly medical expenditures prior to Medicaid, as decreases in the likelihood of having positive spending in a quarter that is less than $100 drive the results.

It is not immediately clear that the crowd-out of private health insurance coverage will lead to families spending less on health insurance. According to the 2014 CEX, more than 25 percent of families with private health insurance do not have any quarterly spending on health insurance, while only 30 percent spend more than $1,000 per quarter on health insurance. A cottage industry has developed around helping firms sign up Medicaid-eligible employees for Medicaid to save the firms money (Kim 2016), and some research suggests that firms are successfully able to capitalize on Medicaid expansions to lower their health insurance costs (Buchmeuller et al. 2005). If the crowd-out comes from workers who had low spending on health insurance, crowd-out may not result in cost savings to families.

Taking advantage of the CEX's questions about health insurance spending, I find that an additional person being eligible for Medicaid reduces the likelihood that a family has any health insurance expenditures by 4.3 percentage points. This decrease appears to come from families that were spending more than $100 per quarter on health insurance prior to Medicaid and reduces average spending on health insurance in a quarter by $47. Under the assumption that Medicaid only affects health insurance costs for those who experience crowd-out, the estimates imply that the reductions in private insurance from Medicaid expansions save the switching families an average of $4,124 to $4,284 per year, meaning the families that experience crowd-out were paying significant amounts of the premiums for their private insurance. The results from this study suggest that Medicaid eligibility makes families better off even when crowd-out occurs.

The paper contributes to the small literature on the spending effects of Medicaid in a number of ways. One contribution is that the current study focuses on Medicaid expansions that affect families, whereas other studies have mainly focused on Medicaid coverage for childless adults, who comprise a much smaller share of the Medicaid population. Another contribution of this study is that it uses a nationally representative data set that is designed to measure expenditures and that allows for considering the effect of Medicaid eligibility on a wider variety of financial outcomes than previous research has considered. Importantly, the paper estimates the financial impacts of crowd-out on health insurance spending. Whether families or employers capture monetary savings from crowd-out is an open question that has important welfare implications.

The paper proceeds as follows. The next section provides a brief overview of Medicaid and discusses previous research on crowd-out from Medicaid expansions and on the spending effects of Medicaid. Section 2 discusses a simple conceptual framework for how Medicaid eligibility could affect spending and what the implications of spending effects are for welfare. Section 3 describes the variation in Medicaid eligibility, the CEX data, and the empirical strategy. Section 4 presents the empirical results. Section 5 discusses the results and concludes.

1 Background

1.1 Medicaid

Medicaid is a state-run program that is jointly financed by the federal government and by states; it provides health insurance to people with low incomes, people with disabilities, and the elderly in long-term care with low incomes.[3] In 1997, State Children's Health Insurance Program (CHIP) legislation expanded eligibility of children for public health insurance beyond the existing limits of the Medicaid program. In the years that followed, states have expanded eligibility for both the CHIP program and Medicaid. States typically require family incomes to be lower for parents to be eligible than they do for children to be eligible.

Although federal Medicaid rules require states to cover major services, including physician and hospital care, the rules do not require states to pay for other

[3]Medicaid has traditionally provided coverage for low-income families with children rather than all low-income adults. As part of the Affordable Care Act, Medicaid was expanded in many states to include low-income childless adults. Some states had already expanded Medicaid to low-income childless adults prior to the passage of the Affordable Care Act.

services such as prescription drugs or dental care. Despite flexibility in coverage options, most states cover most basic categories of health spending. For instance, all states cover prescription drugs and optometrist services. While almost all cover dental services for children, not all states cover dental care for adults. Covered services are provided with little or no copayment required (Ross et al. 2009).

1.2 Prior Research on Crowd-Out from Medicaid Expansions

The health insurance effects of Medicaid eligibility are relevant for assessing the magnitude and channels of any potential effects of Medicaid eligibility on spending. In contrast to the literature on the financial impacts of Medicaid, a large literature has examined Medicaid take-up and the effect of Medicaid on private health insurance coverage. Much of this research builds on Cutler and Gruber (1996), who estimate the effect of Medicaid eligibility on Medicaid take-up and on private health insurance coverage using data from the 1988 to 1993 March CPS. To deal with the endogeneity of Medicaid eligibility, they use a simulated IV strategy and find that Medicaid eligibility increases Medicaid coverage by 23.5 percentage points and decreases private insurance coverage by 7.4 percentage points.

Since the seminal work by Cutler and Gruber (1996), other studies using simulated IV strategies with March CPS data have tended to estimate effects of Medicaid eligibility that are smaller in absolute value both on Medicaid take-up and on the decrease in private coverage. For example, Shore-Sheppard (2008) finds that adding controls for age over time causes the estimated effect of Medicaid eligibility on

Medicaid coverage to fall to between 15 and 19 percentage points and the estimated effect on private coverage to be close to zero and statistically insignificant. LoSasso and Buchmueller (2004) estimate that 9 percent of the newly eligible from the CHIP expansions received coverage and that half of this increase came from those with private coverage.

Papers that use simulated IV strategies with the SIPP find similarly mixed estimates. For example, Ham and Shore-Sheppard (2005a) estimate that Medicaid eligibility increases take-up by 11.8 percentage points but does not affect private coverage. Gruber and Simon (2008), on the other hand, estimate that an additional person being eligible for Medicaid increases the number of people in a household with Medicaid coverage by between 0.109 and 0.156 people and reduces the number of people with private coverage by between 0.066 and 0.122. They find that most of the decrease in private coverage comes from employer-sponsored coverage.

In addition to results varying by data set and specification, the effect of Medicaid eligibility on the crowd-out of private coverage differs by group and time period. For instance, Hamersma and Kim (2013) use SIPP data from 1996 to 2007 and find that a parent becoming eligible for Medicaid increases the likelihood that the parent has Medicaid coverage by 14.8 percentage points and has no effect on private coverage. Using CPS data from 1996 to 2002, Busch and Duchovny (2005) find similar take-up rates and only weak, suggestive evidence that parental Medicaid expansions crowd out private coverage. In contrast to these studies, McMorrow et al. (2016) use National Health Interview Survey data from 1998 to 2010 and find evidence that about one-third of people who take up parental Medicaid expansions previously had

private coverage. Wagner (2015) finds that Medicaid eligibility expansions for the disabled crowd out much private coverage, while early results for childless expansions suggest that the degree of crowd-out may vary by state (Sommers, Kenney, and Epstein 2014). As Gruber and Simon (2008) summarize, early results appear to differ by data set and are often sensitive to specification, while the literature on the more recent expansions tends to find more consistent crowd-out effects. Although there is no consensus about the degree of crowd-out, the Congressional Budget Office considers the crowd-out to lie between 25 and 50 percent (Congressional Budget Office 2007).[4] Refer to Bitler and Zavodny (2014); Buchmueller, Ham, and Shore-Sheppard (2015); and Gruber and Simon (2008) for excellent reviews of the literature.[5]

1.3 Prior Research on Financial Impacts of Medicaid

The literature on the financial impacts of Medicaid is smaller than the literature on crowd-out and has generally focused on an inability to pay medical bills. Gross and Notowidigdo (2011) study the effect of Medicaid expansions in the 1990s on bankruptcies. They find that a 10 percent increase in Medicaid eligibility reduces personal bankruptcies by 8 percent. Finkelstein et al. (2012) study Oregon allocat-

[4]While most of the literature is agnostic about reasons that crowd-out might occur, two papers explore endogenous health insurance offerings by firms. Shore-Sheppard, Buchmueller, and Jensen (2000) use firm-level data and find that a firm having more workers eligible for Medicaid is associated with the firm being less likely to offer dependent coverage. However, they do not find that the share of Medicaid-eligible workers in a firm has an effect on the premiums the workers have to pay. Buchmeuller et al. (2005) find some evidence that firms whose workers are likely to have been affected by Medicaid expansions raise employees' contributions for family coverage. Neither paper finds that firms are less likely to offer health insurance after Medicaid expansions.

[5]This literature review focuses largely on studies that use simulated IV strategies. Other papers use different methods, including difference-in-differences and regression discontinuity designs. For examples, refer to Blumberg, Dubay, and Norton (2000); Card and Shore-Sheppard (2004); Dague et al. (2011); De La Mata (2012); Koch (2013, 2015); and Yazici and Kaestner (2000).

ing slots to an oversubscribed Medicaid program for adults using a lottery. Using administrative data from the Consumer Credit Database and from the credit bureau TransUnion, they find that Medicaid coverage is associated with a significant decline in the likelihood that a medical bill is sent to collections but no significant decline in bankruptcies or liens. Hu et al. (2016) study the effect of the Affordable Care Act (ACA) Medicaid expansion to childless adults using data from the Federal Reserve Bank of New York's Consumer Credit Panel. They find that low-income zip codes experienced a reduction in the number of unpaid bills and the amount of debt sent to third-party collection agencies in states that expanded Medicaid.

Although these extreme financial outcomes are important, they do not represent a full picture of the spending effects of Medicaid. Contemporaneous spending has received less attention, though the Finkelstein et al. (2012) Oregon study is an exception. Finkelstein et al. supplement their analysis of administrative data with survey data and find intent-to-treat estimates that suggest that winning the Medicaid lottery decreases the likelihood of having any medical spending in the last six months by 5.8 percentage points and of having an outstanding medical bill by 5.2 percentage points. The current study differs from Finkelstein et al. in three main ways. First, the current study focuses on Medicaid eligibility for families, while Finkelstein et al. study the effect of Medicaid on childless adults, who may respond to Medicaid differently than families. Medicaid eligibility for childless adults is still relatively new, and the vast majority of people covered by Medicaid are in families with children.[6] Second, the CEX contains a much wider set of spending outcomes than is available

[6]According to 2015 March CPS data, less than a quarter of Medicaid recipients had no children in their household.

in the survey used in Finkelstein et al. A particularly important spending outcome available in the CEX that the Oregon health insurance experiment does not address is health insurance spending. Third, the current study uses a simulated IV strategy, while Finkelstein et al. study the effects of an experiment. While experiments are often thought to be the gold standard of research, they are expensive and rarely done with health insurance, which precludes them as an option for most studies of Medicaid. As is the case with the Oregon health insurance experiment, they are also often restricted in their geography.

Another study that considers the financial implications of Medicaid is Sommers and Oellerich (2013), who estimate the impact of Medicaid with the Census Bureau's Supplemental Poverty Measure by stochastically drawing counterfactual medical expenditures from propensity-score-matched individuals without Medicaid. They find that Medicaid reduces out-of-pocket medical spending from $376 to $871 per beneficiary and decreases poverty rates by 1.0 percent among children, 2.2 percent among disabled adults, and 0.7 percent among elderly individuals. My study differs from Sommers and Oellerich in that I estimate the effect of Medicaid eligibility using variation from natural experiments and consider a wider set of outcomes.[7]

[7]Gruber and Yelowitz (1999) produce an early study of the effect of Medicaid eligibility on savings and consumption using CEX data from the 1980s and early 1990s and find evidence that Medicaid eligibility increases non-health-related consumption and decreases savings. As Medicaid typically had asset tests prior to the 1990s, their analysis pertains to a different vintage of the Medicaid program than exists now. In Appendix B, I consider the effect of Medicaid eligibility on non-health-related consumption and discuss Gruber and Yelowitz further.

2 Conceptual Framework

Medicaid eligibility has the potential to affect health insurance coverage in two main ways. First, it can allow uninsured people to become insured. Second, it can allow people who were receiving private coverage to drop that coverage so that they can receive Medicaid.

For the previously uninsured, Medicaid lowers the price of health care. As health care is a normal good, this lower price means people should theoretically consume more of it, a prediction that has found broad empirical support.[8] If all health care expenditures are covered by Medicaid, the effect of Medicaid on spending on health care should be negative. If noncovered care complements covered care, Medicaid eligibility could potentially increase spending on medical care. As lowering the cost of medical care can increase the amount of medical services received, reduce medical expenditures, and lower the risk of catastrophic medical bills, Medicaid likely increases welfare for the previously uninsured.

For those who previously had private coverage, Medicaid may allow families to spend less on health insurance premiums, especially if they were paying a high share of the premiums for the private health insurance. However, if the coverage they had was poor or if employers can successfully capture the cost savings from Medicaid

[8]However, it should be noted that people may ultimately consume less medical care if Medicaid allows them to obtain more preventive care that prevents the need for more health care later. Examples of research that find that Medicaid increases the use of health care services include Aizer (2007); Baicker et al. (2013); Bronchetti (2014); Buchmeuller, Orzol, and Shore-Sheppard (2015); Burns et al. (2014); Currie, Decker, and Lin (2008); Dafny and Gruber (2005); De la Mata (2012); DeLeire et al. (2013); Finkelstein et al. (2012); Lipton and Decker (2015); and Taubman et al. (2014). This finding holds for private health insurance (Anderson, Dobkin, and Gross 2012, 2014), Medicare (Card, Dobkin, and Maestas 2008, 2009), and health insurance expansions coming from broad health insurance reform (Kolstad and Kowalski 2012; Miller 2012) as well.

expansions, households' spending on health insurance may not fall dramatically. If the coverage people lose was poor, then Medicaid would affect medical spending in a way similar to how it does for the previously uninsured.

The welfare implications of crowd-out depend on both the quality of the private coverage that is crowded out and what share the family was paying for the private coverage. Consider the following three pre-Medicaid-eligibility cases:

1. The family was paying very little for good coverage.

2. The family was paying full price for good coverage.

3. The family was paying very little for poor coverage.

Medicaid eligibility crowding out private coverage for case (1) does not necessarily help the family. If an employer was paying large amounts for the worker's health insurance, then the worker leaving private employer-sponsored coverage for Medicaid lowers employers' costs. These lower costs for employers may or may not later translate into higher wages for the worker.[9]

Crowd-out of (2) or (3), on the other hand, will still make the family better off. If the family was paying full price for good coverage (case 2), then Medicaid functions

[9]It is unclear whether workers or firms bear the cost of health insurance benefits. If workers value money spent on health insurance as much as they would value the money itself, if employers can perfectly identify which people are eligible for Medicaid, and if employers can perfectly and instantly adjust compensation, theory predicts that employers will pass the costs of health insurance to employees in the form of lower wages (Gruber 1994; Summers 1989). Therefore, Medicaid expansions have the potential to increase wages if employers no longer have to provide workers with health insurance coverage. However, these assumptions may not hold for a variety of reasons. For example, identifying Medicaid-eligible people is likely difficult since employers would have to know total family income. Furthermore, Finkelstein, Hendren, and Luttmer (2015) find that the Oregon health insurance experiment's Medicaid recipients only value Medicaid at 20 to 40 percent of Medicaid's cost, suggesting that many low-income workers may not fully value health insurance.

as an income transfer to low-income families. As Bitler and Zavodny (2014) explain, these transfers are still welfare enhancing on average if societal preferences put more weight on income at the bottom end of the income distribution than on income at the top end of the income distribution. Under certain assumptions, risk aversion results in crowd-out of (3) being more welfare improving than crowd-out of (2), but in either case, Medicaid still makes these families better off. Refer to Appendix A for a discussion of these assumptions and the model that underlies these predictions.

To summarize the empirical predictions and implications, Medicaid likely lowers medical expenditures, but there are also ways that Medicaid could increase medical expenditures. Medicaid crowding out private coverage would mean that Medicaid eligibility should have a nonpositive effect on health insurance spending. Finding that Medicaid eligibility reduces private health insurance coverage but not health insurance spending means that the family's private insurance likely had very low benefits (and was therefore not costly) or that the cost-savings from Medicaid were passed through to firms.[10] Finding a negative effect of Medicaid eligibility on spending on health insurance suggests that the family was paying the premiums for private insurance prior to switching to Medicaid.

[10]Alternatively, finding an effect of Medicaid eligibility on private health insurance coverage but not on health insurance spending could also mean that firms distribute the cost savings from Medicaid expansions to employees in other ways, such as through higher wages or better working conditions. In the empirical section, I find that families experience a reduction in the amount that they pay for health insurance, but I cannot rule out adjustments along other margins.

3 Medicaid Variation, Data, and Estimation

3.1 Variation in Medicaid Eligibility

This paper uses variation in Medicaid eligibility that arises due to changes to the income thresholds for parents and children.[11] As most of the CHIP expansions for children occurred in the late 1990s, the majority of the variation in this paper comes from expansions for parents. In 2000, the mean income threshold for parental Medicaid eligibility for states in the CEX was 76.7 percent of the FPL. By 2014, the mean income threshold was 100.7 percent of the FPL. From 2000 to 2014, 14 states more than doubled their income thresholds. Many states, such as Arizona, Connecticut, and Illinois, increased their threshold dramatically in the early to mid-2000s. Other states, such as Arkansas and Kentucky, did not experience large increases until they adopted the ACA's Medicaid expansion in 2014. A third set of states, including Missouri and Washington, lowered their eligibility requirements from 2000. Despite the increases in parental Medicaid eligibility since 2000, all states limit coverage to parents in families with incomes under 250 percent of the FPL, and all states except 3 limit coverage to parents in families with incomes under 200 percent of the FPL.

While most of the expansions for children occurred in the late 1990s, several states changed their income thresholds for children during the time period studied. For instance, Arkansas and Tennessee both lowered their income thresholds in the early 2000s to under 200 percent of the FPL before they increased the thresholds a few years later. Both Hawaii and Texas expanded CHIP in 2000, while other states

[11] Data on Medicaid's income thresholds come from Hamersma and Kim (2013), the Kaiser Family Foundation (2016a), and LoSasso and Buchmueller (2004).

altered their eligibility thresholds in the mid-2000s. Table 1 shows the states with the five largest increases and decreases to adult income thresholds during the time period studied as well as the states with the five largest increases to the income thresholds for younger and older children. For each of these states, Table 1 displays the thresholds in 2000, 2013, and 2014.[12]

3.2 Data

The main data used in the analysis come from the 2000-2014 CEX Interview Surveys. The CEX is a rotating panel that interviews roughly 5,000 consumer units about their spending in the previous quarter every three months over five calendar quarters. Every quarter, 20 percent of the sample is replaced by new consumer units; these are not necessarily households but instead include household members who share expenditures. I use the terms household and family to mean consumer unit.

I first use the CEX to estimate the effect of Medicaid eligibility on measures of health insurance coverage. The CEX asks about the number of people in a family covered by Medicaid as well as each plan that a family has. Unlike most other data sets that ask about health insurance, the CEX also asks about spending on each private plan. I focus on the number of family members covered by Medicaid and on the number of various types of private health insurance plans that the family spends money on in a quarter. I then study four main categories of quarterly health-related spending: medical, prescription drugs, dental, and health insurance. Next,

[12]I display the thresholds in 2013 in Table 1 in addition to the thresholds at the end of the study period because many states adjusted their adult eligibility in 2014 as they adopted the Medicaid expansions of the ACA.

I draw on the CEX's limited information on medical debt to estimate the effect of Medicaid eligibility on medical debt. In Appendix B, I use the CEX's information on labor market outcomes and other sources of income to consider possible effects on nonhealth spending and on program participation.

As the focus of this study is on low-income families, I restrict the sample to families who make less than 200 percent of the FPL. I choose 200 percent since most Medicaid expansions for parents are no less than 200 percent of the FPL, but I evaluate the sensitivity of the results to the cutoff in Section 4.3. For the primary analysis, I use income data as reported by families but drop families with people who work but have no income since they likely did not fully report income.[13] I calculate all expenditures in 2012 dollars and exclude families whose state identifiers are suppressed in the CEX due to confidentiality reasons.[14] The final sample contains 30,752 observations from 14,024 families. Summary statistics for the sample are shown in Table 2.[15] As would be expected with a low-income sample, these households have lower average education levels than the rest of the nation in addition to lower incomes. Slightly more than one-third of the sample has someone who receives Medicaid.

[13]Prior to 2004, the CEX did not impute income, so families who declined to report income or did not know their income show up in the microdata as having no income. Beginning in 2004, the CEX began imputing the income of nonresponders. I use reported income data and drop families with people who work but have no income to have the most consistent sample across time that is possible. When the CEX begins imputing income in 2004, almost all families with earners report positive income or the BLS imputes income for them. I consider the robustness of the results to a variety of ways of handling income in Section 4.3.

[14]Families in the following states are not interviewed or their states are not identified for confidentiality reasons: Arkansas, Iowa, Mississippi, Montana, New Mexico, North Dakota, South Dakota, West Virginia, and Wyoming.

[15]I do not show summary statistics for the ethnicity of Medicaid recipients because the CEX does not begin asking about Hispanic status until 2003.

Table 3 summarizes spending patterns for families with incomes less than 200 percent of the FPL as well as for families above 200 percent of the FPL. All of the means are statistically different from each other at the 1 percent level except for the mean percent of household spending on medical care.[16]

3.3 Estimation

The primary goal of this paper is to estimate the effect of an additional person in a family becoming eligible for Medicaid on health insurance and spending outcomes. I estimate models of the following form:

$$y_{ifst} = \alpha + \gamma_{fs} + \delta_{ft} + \beta X_{ifst} + unemp_{st} + \lambda ELIG_{ifst} + \epsilon_{ifst}, \tag{1}$$

where i indexes the family, f indexes the family type, s indexes the state, t indexes the year, y is a measure of health insurance or spending, X is a set of individual covariates, *unemp* is the state-year unemployment rate from the BLS's Local Area Unemployment Statistics, γ are state fixed effects, δ are year fixed effects, $ELIG$ is the number of people in the family eligible for Medicaid, and ϵ is the error term. The X vector includes family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race of the head of the household, the age of the head of the household, the number of working adults in the household, and the number of people

[16]Refer to Levy and DeLeire (2008) for a full consideration of spending patterns by income with CEX data.

in the household with high school degrees, with some college, and with bachelors' degrees.

I impute $ELIG$ using family structure, family income, the FPL, and the state's income eligibility rules. The λ coefficient is the effect of an additional family member becoming eligible for Medicaid on the dependent variable. Because I focus on family measures and the data are at the family level, this specification is slightly different than much of the related literature that conducts analysis at the individual level. This model is similar to Gruber and Simon (2008), who consider family-level eligibility in addition to individual-level eligibility and argue that family-level analysis is important since health insurance decisions are made at the family level. I define family type using the number of parents, the number of school-aged children (aged 6–18), and the number of young children (under age 6) in a family. With this set-up, a family with two parents and a young child, a family with two parents and a teenager, and a family with one parent, one young child, and one teenager are all different family types. This model accommodates the fact that multiple people in a family can become eligible for Medicaid at the same time. For instance, all two-parent families will have two people become eligible for Medicaid at the same time if parental Medicaid is expanded, and the family's $ELIG$ increases by two.[17]

[17]Differential effects based on the number of people becoming eligible at the same time is potentially an interesting source of heterogeneity. Unfortunately, results from analysis that examines this heterogeneity are too imprecise to draw meaningful conclusions about differences. Another potentially interesting source of heterogeneity is the effect of an additional parent becoming eligible for Medicaid versus the effect of an additional child becoming eligible for Medicaid. However, there is generally not enough variation in child Medicaid eligibility for precise estimates of the effect of children separately.

Estimating Equation (1) with OLS would yield biased estimates of λ because Medicaid eligibility is endogenous as families with lower incomes are more likely to be eligible for Medicaid. Since health insurance and health care are normal goods, families with lower incomes will likely consume less of them and therefore spend less on them. Estimating Equation (1) with OLS would falsely attribute this lower ability to spend on health insurance and health care to cost savings from Medicaid eligibility.

To overcome the endogeneity of Medicaid eligibility, I use a simulated IV strategy to estimate the effect of an additional person in a family becoming eligible for Medicaid. This approach was pioneered by Cutler and Gruber (1996) and involves creating a measure that represents a state's Medicaid generosity that is correlated with Medicaid eligibility and that is correlated with the outcome of interest only through its correlation with Medicaid eligibility. To compute the instrument, I apply the income eligibility criteria in each state-year to the full sample for that year and calculate the average number of people in each family type that would be eligible under that state's Medicaid rules. Conditional on family type, states with more generous Medicaid eligibility rules will have a higher value of the instrument than states with less generous Medicaid rules. Since I use the same sample to compute each state's instrument in a given year, variation in the instrument across states comes solely from differences in Medicaid eligibility rules. I estimate Equation (1) with two-stage least squares (2SLS) and use the simulated measure as an instrument for $ELIG$. The instrument is highly correlated with the number of people in a family

eligible for Medicaid. The first-stage coefficients on simulated eligibility are close to 1 (0.95–0.99 dependent on specification), and the F-statistics are over 1,000.

While the simulated IV approach provides a useful way to summarize complicated variation in Medicaid eligibility rules, the exogeneity of the instrument relies on the exogeneity of the Medicaid eligibility rules. If states that expand Medicaid are changing differently from states that do not in unobservable ways, then the estimates from the simulated IV strategy may be biased. For instance, if firms increase employees' share of premiums for spousal coverage over time, then states with more two-parent households would see average health insurance spending increase. If states with more two-parent households were also more likely to expand Medicaid eligibility, then a failure to account for these unobserved trends would incorrectly attribute the increase in health insurance spending from rising employee premiums to Medicaid eligibility.

I take several steps to mitigate concerns about unobserved trends. First, I allow each family type to have different state fixed effects (γ coefficients) and different year fixed effects (δ coefficients). Thus, identification of λ comes from how an outcome changes after a change to eligibility relative to the initial differences for that family type in that state and relative to how outcomes change for that family type over time. This approach addresses the criticism of early use of the simulated IV strategy from Shore-Sheppard (2008), who shows that the implicit assumption that everyone has the same year effect and that all people in a state have the same baseline state effects can lead to biased estimates.

Second, for each outcome, I estimate two specifications: one set with minimal controls and another set with extensive controls. The rationale for this approach is that many potential unobserved trends would likely be correlated with observable characteristics like unemployment and income levels. For example, suppose that a state increases Medicaid eligibility because one of its main industries experienced increased competition from abroad, which led to many people losing their jobs and health insurance over time. Since the important, unobserved trend occurs at the state level, it would not be captured by the year fixed effects. While this trend is unobservable to the researcher, it would likely be correlated with income and unemployment, which means including income and unemployment rates as controls would alter the estimates because they would be correlated with the error term. However, if the instrument really is exogenous, including the controls described above should not alter the estimates.

I return to the issues of unobserved trends and of policy endogeneity in Section 4.4 to consider other ways to address the possibility that unobserved trends confound the simulated IV strategy.

4 Results

4.1 The Effect of Medicaid Eligibility on Health Insurance Coverage

Estimates from the CEX

I begin by showing estimates of the effect of an additional family member becoming eligible for Medicaid on various health insurance measures from the CEX. Table 4 displays the results. Each cell under the *2SLS Estimates* subheading is the effect of an additional person becoming eligible for Medicaid from separate regressions of Equation (1) estimated with 2SLS. In the first column of estimates, the regressions control for year fully interacted with family type and for state fully interacted with family type. In the second column of estimates, I supplement the regressions with the various demographic and labor force controls discussed earlier.

The first row of Table 4 considers the effect of an additional person in a family becoming eligible for Medicaid on the number of people in the family with Medicaid. The estimated effect is 0.135 when only state-by-family-type and year-by-family-type fixed effects are included. When demographic and labor market controls are included, the estimate is 0.142.

The remainder of Table 4 considers changes in private health insurance coverage. In row 2, the dependent variable is an indicator variable equal to one if the family pays for any private health insurance. The share of families with expenditures on private health insurance falls by 4.4 to 4.6 percentage points when an additional person becomes eligible for Medicaid. In row 3, the dependent variable is the number

of private health insurance plans paid for by the family. The results indicate that an additional person in the family becoming eligible for Medicaid decreases the average number of health insurance plans paid for by the family by 0.053 to 0.055. The bulk of this decrease appears to come from a drop in the number of employer-sponsored plans paid for by the family, which falls by 0.038 to 0.040. The estimated coefficients are statistically indistinguishable from zero when the dependent variable is the number of privately purchased plans paid for by the family.

The last two rows of Table 4 consider the effect of an additional person becoming eligible for Medicaid on the number of special purpose health insurance plans paid for by the family. Special purpose health insurance plans include dental, vision, and prescription drug coverage. The results provide no evidence that Medicaid eligibility affects the number of special purpose plans paid for by the family. The last row of Table 4 focuses on dental coverage, which is the most common special purpose plan, and shows no evidence that an additional person becoming eligible for Medicaid affects the number of dental plans paid for by the family.

Estimates from the March CPS and SIPP

The most commonly used data sets to estimate health insurance changes from Medicaid expansions are the March CPS and SIPP. To consider how estimates from the CEX compare to estimates from the March CPS and the SIPP over this time period and with the states available in the CEX, I next replicate the health insurance analysis with both March CPS and SIPP data.

For the March CPS analysis, I use data from the 2001–2015 March CPS. As the March CPS asks about health insurance coverage in the previous year, these data provide information on health insurance coverage from 2000 to 2014. I impute eligibility based on the household's income in the prior year and the state's eligibility rules in the prior year. Although it is a common data set for studying the health insurance effects of Medicaid, the March CPS is not without problems. Partly because people may not be able to remember their insurance from the previous year, as many as 10 percent of Medicaid recipients in the previous year may not report that they received Medicaid (DeNavas-Walt, Proctor, and Smith 2013; Klerman et al. 2009; Lewis, Ellwood, and Czajka 1998). This Medicaid undercount can bias estimates of the effect of Medicaid eligibility on take-up toward zero. A related issue is that some people report having had multiple sources of health insurance in the previous year. I code people based on their responses, meaning that some people may be counted as having had both private health insurance and Medicaid in the previous year.

For the SIPP analysis, I use data from the 2001, 2004, and 2008 waves of the SIPP, which provide information from 2001 to 2013. The SIPP interviews people every four months and asks about each month since the previous interview. As Chetty (2008) finds that people report the same responses during one interview for all four months associated with the interview period, I restrict the sample to observations from the interview month.

I aggregate information in both data sets to the family level so that the three analysis data sets have similar structures. This aggregation involves calculating the number of people in each family with various sources of health insurance as well as

the number of workers in the household and the number of people with high school degrees, with some college, and with at least a bachelor's degree. I also control for the age, race, and gender of the head of the household. As with the CEX analysis, I restrict the sample to families making less than 200 percent of the FPL in the past year and to the 42 states that are publicly identifiable in the CEX. I create the instruments as described earlier and then instrument for the number of people in the household eligible for Medicaid using 2SLS.

Table 5 displays estimates of Equation (1) using these two data sets. Panel A displays the March CPS results, while Panel B displays the SIPP results. The estimates suggest that an additional person in a family becoming eligible for Medicaid increases the average number of people in the family with Medicaid by 0.126 to 0.151. The point estimates for the fall in private coverage range from 0.028 to 0.096. Only one is statistically significant.

Although the evidence for a fall in overall private coverage is weak, the results show a clear reshuffling of insurance. The point estimates for the change in employer-sponsored coverage range from -0.095 to -0.053 and are statistically significant at at least the 10 percent level in three specifications. This decrease appears to come from both employer-sponsored health insurance as a dependent and as a policyholder. Neither data set provides evidence that Medicaid eligibility affects the number of people in the family with privately purchased health insurance.

Comparison of Estimates

All three sets of estimates are generally consistent with each other and with the previous literature. The point estimates of the increase in Medicaid coverage from an additional person becoming eligible for Medicaid in the CEX fall within the bounds of the estimates from the March CPS and SIPP, and none are statistically different from each other. As these estimates are at the family level, they are comparable to Gruber and Simon (2008). Their estimates of the effect of an additional person in the family becoming eligible for Medicaid on the number of people with Medicaid coverage range from 0.109 to 0.156, which encompass the estimates presented in this paper.

The point estimates from my study for the fall in employer-sponsored insurance from the March CPS and SIPP range from 0.053 to 0.096, while the point estimates in Gruber and Simon (2008) range from 0.066 to 0.121. Although these results for employer-sponsored health insurance are not directly comparable to the CEX results since the CEX results are about plans that may cover multiple people, they suggest similar patterns among the three data sets.

The results support the idea that there is more crowd-out from more recent expansions for parents than from earlier expansions as suggested by the results of Busch and Duchovny (2005), Hamersma and Kim (2013), and McMorrow et al. (2016).[18] One possible reason that crowd-out could be higher in more recent years is

[18]In results available upon request, I have redone the SIPP and CPS analysis focusing exclusively on the parental expansions and using the same years of data as Busch and Duchovny (2005) and Hamersma and Kim (2013) and find similar results, which suggests that the different time periods and expansions are why we find different crowd-out results rather than differences in our approaches.

that firms have pushed more of the cost of health insurance onto employees as health insurance costs have risen (Kaiser Family Foundation 2016b). In short, the results about the effect of Medicaid expansions on health insurance coverage found in the CEX are largely corroborated by the March CPS and the SIPP and are generally consistent with other studies.

4.2 The Effect of Medicaid Eligibility on Spending on Health Insurance and Medical Care

I next consider the implications of these health insurance changes for household spending on medical care and on health insurance. Table 6 displays 2SLS estimates of the effect of an additional person becoming eligible for Medicaid from Equation (1) on various spending outcomes. .

Row 1 displays estimates of the effect on medical spending variables. The estimates imply that an additional person in a family becoming eligible for Medicaid reduces the likelihood that a family has any medical spending by 2.7 percentage points. Much of this decrease appears to come from families with positive quarterly spending that is less than $100, which falls by about 2 percentage points. I do not find evidence that Medicaid reduces the likelihood that families have medical spending over $100 per quarter, nor do I find any evidence that Medicaid has impacts on

average medical spending, though I should note that I cannot rule out meaningful effects for either outcome.[19]

Row 2 of Table 6 considers the effect of an additional person becoming eligible for Medicaid on prescription drug spending and displays some evidence that Medicaid reduces the likelihood that families have positive but small expenditures on prescription drugs. Row 3 considers the effect of Medicaid on dental spending. I find no evidence that an additional family member being eligible for Medicaid affects dental spending. This null result may arise with dental spending because dental spending is more discretionary and families might not visit the dentist without dental insurance.[20]

Row 4 of Table 6 considers how spending on health insurance changes as a result of an additional person in a family becoming eligible for Medicaid. The estimates suggest that the likelihood that families have any spending on health insurance falls by 4.3 percentage points when an additional family member becomes eligible for Medicaid. This decrease in the number of families with any health insurance spending does not appear to come from families that were spending small amounts on health insurance coverage as the percentage of families with positive spending less

[19]Estimating the effect on average medical spending is complicated by medical spending's severe right skew. Likely for this reason, other research (e.g., Finkelstein et al. 2012) often does not consider average effects and instead considers indicators for any medical spending or for any medical bills. I have also considered other methods, such as estimating quantile IV regression models, but these models do not converge, likely due to the large number of fixed effects. Therefore, I report the estimated effects on average spending but focus the discussion on indicators for categories of medical spending because of the issues with studying average medical spending.

[20]Although all states provide preventive dental coverage for children, many states only provide emergency dental services for adults. When I restrict the sample to states that provide dental coverage to adults during this time period (as defined in Decker and Lipton 2015), I still find no evidence of an effect of an additional family member becoming eligible for Medicaid on dental spending.

than $100 does not change. Instead, all of the increase in families with no spending appears to come from families that were spending over $100 per quarter. Average per quarter spending on health insurance coverage falls by about $47.

Medicaid allowing families to spend less on private health insurance coverage is important. A concern with Medicaid crowding out private coverage is that Medicaid could potentially not be helping low-income people as was originally intended. Instead, Medicaid could mean employers have to pay less in fringe benefits. However, these results suggest that low-income families that were relying on employer-sponsored coverage were doing so at a large cost.[21] Since these estimates imply that the coverage that was crowded was high quality, they also suggest that the reduced medical spending likely comes from the previously uninsured.

Table 7 considers the effect of Medicaid eligibility on medical debt. Medicaid has the potential to affect medical debt since families may not pay medical bills immediately. The CEX only asks about debt in certain interviews, so the sample sizes are much smaller, which results in noise in the estimates. I do not find strong evidence that Medicaid eligibility affects the likelihood of having any debt. However, it is important to note that I cannot rule out sizable impacts on many of the medical debt outcomes.

[21]Note that the size of the estimates do not rule out that employers could have reduced health insurance expenses because of crowd-out. Any reduced health insurance expenditures for firms arising from crowd-out may be profit for firms or may be passed onto workers in a variety of ways.

4.3 Robustness

I next verify the robustness of the key health insurance and spending results to various data and estimation choices. Column 1 of Table 8 displays the original estimates with state-by-family-type indicators, year-by-family-type indicators, and demographic and labor market controls.[22]

The main results use reported income and drop families with people who worked but had no income from the sample. I now consider the sensitivity of the results to three alternatives. Column 2 of Table 8 displays results that no longer drop families with workers but no income. Column 3 displays results that use imputed income rather than reported income when imputed income is available beginning in 2004. Column 4 displays results that drop workers who had their incomes imputed once the CEX began imputing data as well as families with workers but no incomes. In all cases, the results are qualitatively similar and generally maintain statistical significance.

For the main analysis, I generate the simulated measure of eligibility by applying the income eligibility criteria in each state-year to the CEX sample for that year. I now consider two alternative methods of constructing the instrument. As the March CPS income measure has advantages over the CEX measure, such as a larger sample size and more comparability over time (BLS 2016), I next construct the instrument and eligibility measure using March CPS data rather than CEX data. Specifically, I use the March CPS to compute the average number of family members

[22]Although Tables 8 and 9 focus on select results for conciseness, the omitted results are qualitatively similar to the main results presented in the paper as well and are available from the author upon request.

eligible for Medicaid as well as the simulated instrument for each family type, year, and state combination. I then merge these measures to the CEX on family type, year, and state and replicate the CEX analysis using average eligibility and the simulated instrument from the March CPS.[23] The results are shown in Column 5 of Table 8. Another alternative method of constructing the instrument is to apply the income eligibility criteria in each state-year to the entire sample for all years.[24] The results are shown in column 6 of Table 8. With both alternative instrument definitions, the results are similar to the original estimates.

The main analysis did not weight the estimation using the CEX weights, which are designed to make the CEX representative of regions of the United States. In column 7, I weight the regressions using the CEX weights. Again, the results are similar regardless of whether or not weights are used.

The CEX contains multiple observations per family. In column 8, I consider the robustness of the results to collapsing the data to one observation per family by producing quarterly means of the outcomes. I use eligibility and characteristics in the most recent interview, meaning eligibility and the controls are imperfectly measured. Despite the measurement error, the results remain similar.

[23]This strategy follows Bronchetti (2014), who studies how the effect of Medicaid eligibility on health care access differs for immigrants and natives using outcomes and demographic characteristics from the National Health Interview Surveys but the instrument and eligibility measure from the March CPS.

[24]Using a fixed sample each year to compute the instrument is valid since the year coefficients control for national changes in income over time, but the results not being robust to using a fixed sample across years would still be troubling. Examples of studies that use a fixed sample across years include Gross and Notowidigdo (2011) and Shore-Sheppard (2008), while examples of studies that use different samples for each year include Bronchetti (2014) and Gruber and Simon (2008)

In the main analysis, I restricted the sample to families with incomes under 200 percent of the FPL because most of the Medicaid expansions during the 2000s were for parents making under 200 percent of the FPL. Since the CEX does not contain nearly as many observations as the CPS and SIPP, focusing narrowly on the affected population makes the estimates more precise. Nevertheless, we would be concerned if the results change drastically with slight sample alternations. In column 9 of Table 8, I broaden the sample to include families under 250 percent of the FPL. Although a few of the coefficients are no longer statistically significant at conventional levels, the point estimates are similar and the results are qualitatively the same.

An alternative to using a simulated IV strategy is to include the eligibility threshold as an independent variable in OLS regressions. Instead of providing an estimate of the effect of a marginal person becoming eligible for Medicaid, this approach provides an estimate of the effect of a change in the income threshold. As Hamersma and Kim (2013) explain, the estimate of the effect of the income threshold is appealing since the threshold is a major policy instrument that legislators can control. Since most of the identifying variation comes from parental eligibility, I focus on the effects of the parental eligibility threshold. The results are shown in column 9 and tell a story similar to the original estimates. An estimate with this approach can be interpreted as the effect of increasing the eligibility threshold by 100 percentage points. This approach implies that increasing the threshold by 100 percentage points increases the number of people receiving Medicaid in an average family by 0.175 and lowers the average number of private plans that a family has by 0.057.

4.4 Revisiting Assumption of Parallel Trends

An important assumption of the analysis is that states that alter their Medicaid eligibility rules would have trended similarly to states that did not if not for the changes to the eligibility rules. One way to relax this assumption that has often been used in individual-level analysis has been to take advantage of the fact that Medicaid expansions do not always apply to all ages. For instance, many CHIP expansions commonly set different eligibility rules for young children and older children. Researchers can include state-year fixed effects and identify the effects of Medicaid eligibility as to how outcomes of children at the affected ages change after Medicaid expansions relative to how outcomes change for children at other ages within the state. Studies of parental eligibility do not typically include state-year fixed effects, presumably because the expansions do not vary by age for adults. With family-level analysis, including state-year fixed effects is further complicated by the fact that parents and children of all ages share a household budget, so an increase to parental eligibility increases the value of the instrument for all observations in a state. In principle, this study could take advantage of the fact that differences in family structure mean that raising the parental eligibility threshold increases the number of people eligible for Medicaid in a newly eligible two-parent household by two and the number of people eligible for Medicaid in a newly eligible one-parent household by one. In practice, there is not enough family-type variation within state-year cells in the CEX to identify the effect of an additional person becoming eligible for Medicaid in this way.

To adapt the strategy to identify the effect of Medicaid eligibility using within-state variation, I include wealthier families in the sample since they would presumably not be directly affected by Medicaid expansions. Specifically, for each state, I calculate the highest Medicaid threshold during the time period and include all families with incomes above that amount in the sample. Since Medicaid expansions should not affect wealthier families, I include the simulated eligibility measure as a control and use the simulated eligibility measure interacted with an indicator for being in the original, low-income sample as the instrument. I allow the state and year effects to vary for low-income and high-income families. With this broadened sample, I can now supplement Equation (1) with separate year-by-state fixed effects for each family type, meaning that identification of the effect of an additional person becoming eligible for Medicaid comes from how outcomes change for low-income families after a state expands Medicaid relative to how they change for high-income families of the same type in the state. This approach is similar in spirit to Hamersma and Kim (2013), who perform a placebo test using a sample of high-income individuals.

The results are shown in column 1 of Table 9. Overall, the results are less precise when identifying the effect on an additional person becoming eligible based on within-state changes. The point estimate for the effect on Medicaid coverage is smaller, but the estimate is statistically significantly different from zero and not statistically different from the original estimate. Some of the other estimates are no longer statistically significant from zero, but they are qualitatively similar to the original estimates and never statistically different from them.

The results from using the within-state control group do not provide evidence against the main empirical strategy but are not precise enough to completely allay concerns about unobserved trends. Another issue with this approach is that it can only account for state-level trends that affect all families in a state. This strategy would not account for trends that only low-income families experience. Given that Medicaid is targeted at low-income people, it is not unreasonable to worry that different trends in health insurance for low-income people may cause states to change their Medicaid eligibility rules.

Another way to relax the parallel trends assumption is to supplement Equation (1) with state-specific time trends for each state. If the estimates change dramatically, we would be concerned that differential trends prior to changes in eligibility rules drive the results. Estimates of Equation (1) supplemented with linear state-specific time trends are shown in the second column of Table 9. The estimates are similar to the original estimates, which is reassuring that state trends do not drive the results.

A more direct test of preexisting trends is to examine if and how the outcome variables change immediately prior to changes in Medicaid eligibility. To do this, I supplement Equation (1) with an indicator variable that is equal to one in the year before a state changes its Medicaid thresholds. Significant coefficients on this indicator variable would raise concerns about preexisting trends. The coefficients on the indicator variable are shown in column 3 of Table 9 and are statistically indistinguishable from zero in all cases. To restrict the focus to larger changes, I next create an indicator equal to one in the year before eligibility increases by more than

10 percentage points of the FPL. Again, I find no evidence of preexisting trends that are not accounted for by the empirical strategy.

4.5 Local Average Treatment Effect Estimates and Estimated Effects of Treatment on the Treated

The main analysis focuses on estimating the effect of an additional person in a family being eligible for Medicaid rather than on the effect of an additional person signing up for Medicaid coverage for two main reasons. First, Medicaid take-up is endogenous. Sicker, more risk-averse, and more financially savvy families may be more likely to take up Medicaid conditional on being eligible. Eligibility rules, on the other hand, are decided by policy. The focus on the effect of eligibility means that the main estimates are of the average intent-to-treat effect, which is a policy-relevant parameter since policymakers can influence eligibility more easily than they can influence take-up. Second, it is possible that additional people receiving Medicaid may have effects on the non-Medicaid population. For instance, if Medicaid expansions crowd out private health insurance for sick people, then the Medicaid expansions would lower health risks in the private market, which could lower private health insurance premiums. Alternatively, if Medicaid increases access to care, waiting times for non-Medicaid patients may increase, which could lower spending on health care by reducing access to the health care system. Medicaid expansions having broader effects than just on those who sign up would bias estimates that assume Medicaid only affects those who sign up for Medicaid.

Despite the appeal of the intent-to-treat estimates, comparing intent-to-treat estimates across studies can be difficult. To facilitate comparisons of my estimates to other estimates and to provide an alternative way to assess the plausibility of the estimates, I compute the average effect of the treatment on the treated, which is equivalent to the local average treatment effect (LATE). As these estimates require the critical assumption that Medicaid eligibility only impacts those who take up Medicaid, which may be untenable, they should be taken with caution.

To calculate the LATE estimates, I replace $ELIG$ in Equation (1) with the number of people in a family receiving Medicaid and estimate the model using 2SLS. As with the main analysis, the instrument is the simulated measure of eligibility. Table 10 displays the LATE estimates for selected outcomes. The LATE estimates imply that an additional person taking up Medicaid lowers the number of private health insurance plans that a family pays for by 39 percent and decreases the likelihood that a family has any medical spending in a quarter by 19.3 to 20.4 percentage points. An additional person taking up Medicaid reduces the likelihood that a family has any spending on health insurance by 30.6 to 31.8 percentage points and saves families $333 to $347 per quarter on health insurance.

The LATE can also be formulated as the effect of switching from private coverage to Medicaid for the health insurance spending variables. For this calculation, I replace $ELIG$ in Equation (1) with an indicator for whether or not the family pays for any private coverage. These estimates require the even stronger assumption that Medicaid eligibility affects health insurance spending only for families who experience

crowd-out. The estimates suggest that Medicaid saves families who experience crowd-out $1,031 to $1,071 on quarterly health insurance spending.[25]

5 Discussion and Conclusions

Medicaid has goals of expanding health care access and of reducing out-of-pocket expenditure risk for low-income families. While many studies have explored the effect of Medicaid on health care access, much less research has considered the effects on household expenditures. In this paper, I use CEX data to examine how Medicaid eligibility affects families' health insurance coverage and spending patterns.

My estimates suggest that an additional family member being eligible for Medicaid reduces the likelihood that a family has any medical spending by 2.7 percentage points. Although much of this decrease appears to come from families who had small medical expenditures, I cannot rule out meaningful decreases in large expenditure risks. These results are most comparable to Finkelstein et al. (2012), who study the effect of the Oregon lottery on the likelihood that nonparents have any medical spending. In their study, they calculate a LATE estimate for Medicaid coverage on the likelihood of having any medical spending in the past six months of -0.200. The estimates from this paper suggest a LATE estimate for an additional family member having Medicaid coverage on the likelihood of having any medical spending in the past three months of -0.204 to -0.193. Our estimates are not directly comparable because my estimates describe quarterly spending for a family, while theirs describe

[25]These results are not shown in Table 10. The first estimate ($1,031) includes demographic and labor market controls, while the second ($1,071) does not. They are statistically significant at the 1 percent level with standard errors of 290 and 294.

six-month spending for adults without children. However, despite the different samples and methodologies, the LATE estimates are similar, suggesting the results from the Oregon health insurance experiment may generalize to the larger population.

This study is one of the first to consider the financial implications of the crowd-out of private coverage. The analysis provides evidence that crowd-out occurs, which is a common but not ubiquitous finding in this literature. An important and unique finding from this paper is that crowd-out saves families money on health insurance spending. Assuming that only families who switch from private coverage experience reduced health insurance expenditures implies that these families save \$4,124–\$4,284 on health insurance per year on average, which is similar to the average family contribution for employer-sponsored health insurance coverage of \$4,316 in 2012 (Claxton et al. 2012). This crowded-out insurance being high value suggests that the medical spending effects are likely driven by the previously uninsured.

Some have suggested that crowd-out is a reason to limit Medicaid eligibility (Cannon 2005; U.S. Senate Republican Policy Committee 2015). But the results from this study highlight two important aspects about crowd-out arising from Medicaid and about the spending effects of Medicaid that policymakers should keep in mind. First, even though crowd-out occurs, Medicaid expansions still appear to lower medical spending for a sizable portion of recipients, suggesting that the previously uninsured or underinsured experience welfare increases from Medicaid. Second, the crowd-out from Medicaid allows families to spend less on health insurance. As low-income families likely consume where the marginal utility of consumption is high, this transfer can result in overall welfare improvements. In short, Medicaid realizes

its intended effect of reducing medical expenditures for low-income families, but focusing solely on Medicaid's effect on medical expenditures ignores a major part of Medicaid's contribution to low-income families' financial health.

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Table 1: Largest Changes in Medicaid Eligibility

State	2000	2013	2014
Panel A: Parents			
Largest increases			
Arkansas	19	16	138
Arizona	32	106	138
Nevada	32	84	138
Maryland	34	122	138
Illinois	35	139	138
Largest decreases			
New Jersey	200	200	138
Washington	200	71	138
Maine	157	200	105
Wisconsin	185	200	100
Missouri	107	35	24
Panel B: Children Aged 1-5			
Largest increases			
Oregon	170	300	305
Illinois	185	200	318
Indiana	150	250	255
Louisiana	150	250	255
Hawaii	185	300	313
Panel C: Children Aged 6-18			
Largest increases			
Hawaii	100	300	313
Texas	100	200	206
Oregon	170	300	305
Louisiana	150	250	255
Wisconsin	185	300	306

SOURCE: Data on Medicaid's income thresholds come from Hamersma and Kim (2013), Kaiser Family Foundation (2016a), and LoSasso and Buchmueller (2004).

Table 2: Summary Statistics

	Mean	St. dev.
Male head of household	0.340	0.474
Age of head of household	36.940	8.733
White head of household	0.723	0.447
Black head of household	0.207	0.405
Asian head of household	0.045	0.207
Two parents present	0.586	0.492
Number of high school graduates in family	1.115	0.898
Number in family with hs some college	0.618	0.792
Number of college graduates in family	0.190	0.501
Family income as a fraction of FPL	1.006	0.637
Anyone in family with Medicaid	0.355	0.479
Number of people in family receiving Medicaid	0.992	1.602
Any private health insurance plan	0.316	0.465
Number of private health insurance plans	0.374	0.614

NOTE: The data come from the 2000 to 2014 CEX. The sample contains 30,752 observations from 14,024 families.

Table 3: Quarterly Spending Patterns in the CEX

	Incomes 200% of FPL or less			Incomes over 200% of FPL		
Spending categories	Means	St. devs.	Mean % of spending	Means	St. devs.	Mean % of spending
Health insurance	223	536	1.8	544	749	2.7
Medical care	118	493	0.8	297	792	1.0
Prescription drugs	39	157	0.3	81	207	0.4
Dental care	40	238	0.3	118	419	0.5
Food	1,793	1,074	19.4	2,653	1,524	12.9
Clothes	349	514	3.4	630	1,142	2.6
Housing	3,603	2,659	35.5	6,801	5,011	30.5
Transportation	1,831	4,362	12.6	3,697	6,923	13.1
Entertainment	419	893	3.6	1,138	2,163	4.6
Retirement	172	679	1.3	578	2,456	2.1
Other	2,480	2,912	21.1	7,170	6,358	29.6
Total	11,069	8,813		23,708	16,127	

NOTE: The data come from the 2000 to 2014 CEX. There are 30,752 observations with family incomes at 200 percent or less of the FPL and 62,029 observations with incomes above 200 percent of the FPL. All expenditures are in 2012 dollars. The mean family size for families with incomes less than or equal to 200 percent of the FPL is 3.9, while the mean family size for families with incomes greater than 200 percent of the FPL is 3.8. All of the means are statistically different from each other at the 1percent level except for the mean percent of household spending on medical care.

Table 4: The Effect of Medicaid Eligibility on Health Insurance Coverage: Estimates from the CEX

Dependent variables	Means	2SLS estimates	
Number of people receiving Medicaid	0.992	0.135**	0.142***
		(0.055)	(0.050)
Any private health insurance plans	0.316	-0.044***	-0.046***
		(0.014)	(0.013)
Number of private health insurance plans	0.374	-0.053**	-0.055***
		(0.023)	(0.020)
Number of employer-sponsored plans	0.300	-0.038**	-0.040**
		(0.018)	(0.015)
Number of privately purchased plans	0.055	-0.012	-0.012
		(0.011)	(0.011)
Number of non-employer-group plans	0.019	-0.003	-0.003
		(0.003)	(0.003)
Number of special purpose plans	0.049	-0.001	-0.001
		(0.010)	(0.009)
Number of private dental insurance plans	0.031	0.006	0.006
		(0.005)	(0.005)
Additional controls			x

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The data come from the 2000 to 2014 CEX. The sample contains 30,752 observations from 14,024 families. Each cell is the coefficient on the number of people in a family eligible for Medicaid from Equation (1). All regressions control for family-type-year and family-type-state fixed effects. The second column of estimates also includes controls for family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race/ethnicity of the head of the household, the number of working adults in the family, the age of the head of the household, the annual state unemployment rate, and the number of family members with high school degrees, with some college, and with bachelors' degrees. Standard errors are clustered at the state level and are shown in parentheses below the estimates.

Table 5: The Effect of Medicaid Eligibility on Health Insurance Coverage: Estimates from the March CPS and SIPP

	Panel A: March CPS			Panel B: SIPP		
Dependent variables	Means	2SLS estimates		Means	2SLS estimates	
Number of people with Medicaid	1.62	0.126**	0.133***	1.45	0.151***	0.142***
		(0.051)	(0.048)		(0.045)	(0.042)
Number of people with private coverage	1.37	-0.028	-0.035	1.47	-0.096*	-0.071
		(0.036)	(0.032)		(0.055)	(0.051)
Number of people with ESHI	1.03	-0.053	-0.060*	1.30	-0.095**	-0.076*
		(0.035)	(0.030)		(0.041)	(0.040)
Number of people with ESHI through their own employers	0.34	-0.015*	-0.018***	0.46	-0.034***	-0.030**
		(0.008)	(0.006)		(0.012)	(0.011)
Number of people with ESHI as dependents	0.70	-0.041	-0.046*	0.86	-0.068**	-0.052
		(0.029)	(0.026)		(0.033)	(0.033)
Number of people with privately purchased coverage	0.17	0.004	0.005	0.17	-0.001	0.005
		(0.014)	(0.015)		(0.026)	(0.024)
Additional controls			x			x

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The data for the estimation in Panel A come from the 2001 to 2015 March CPS. The sample contains 128,385 observations. The data for the estimation in Panel B come from the 2001, 2004, and 2008 SIPP. The sample contains 77,388 observations from 18,949 families. Each cell is the coefficient on the number of people in a family eligible for Medicaid from Equation (1). All regressions control for family-type-year and family-type-state fixed effects. The second column of estimates also includes controls for family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race/ethnicity of the head of the household, the number of working adults in the household, the age of the head of the household, the annual state unemployment rate, and the number of family members with high school degrees, with some college, and with bachelors' degrees. Standard errors are clustered at the state level and are shown in parentheses below the estimates.

Table 6: The Effect of Medicaid Eligibility on Health-Related Spending

	Any spending			Spending positive but less than $100			Spending at least $100			Average spending		
Spending categories	Means	2SLS estimates		Means	2SLS estimates		Means	2SLS estimates		Means	2SLS estimates	
Medical	0.312	-0.027**	-0.027**	0.131	-0.020**	-0.019**	0.181	-0.008	-0.008	118	11	11
		(0.013)	(0.011)		(0.009)	(0.008)		(0.010)	(0.010)		(9)	(10)
Prescription	0.261	-0.018	-0.016	0.160	-0.019**	-0.019**	0.101	0.001	0.002	39	-6	-5
		(0.014)	(0.013)		(0.009)	(0.009)		(0.012)	(0.011)		(6)	(5)
Dental	0.099	-0.011	-0.011	0.031	0.000	-0.000	0.069	-0.012	-0.011	40	-2	-2
		(0.008)	(0.008)		(0.005)	(0.005)		(0.007)	(0.007)		(5)	(5)
Health insurance	0.355	-0.043***	-0.043***	0.038	-0.000	-0.000	0.317	-0.043***	-0.043***	223	-47**	-47***
		(0.014)	(0.012)		(0.004)	(0.005)		(0.014)	(0.011)		(19)	(17)
Additional controls			x			x			x			x

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The data come from the 2000 to 2014 CEX. The sample contains 30,752 observations from 14,024 families. Each cell is the coefficient on the number of people in a family eligible for Medicaid from Equation (1). All regressions control for family-type-year and family-type-state fixed effects. The second column of estimates under each subheading also includes controls for family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race/ethnicity of the head of the household, the number of working adults in the family, the age of the head of the household, the annual state unemployment rate, and the number of family members with high school degrees, with some college, and with bachelors' degrees. Standard errors are clustered at the state level and are shown in parentheses below the estimates.

Table 7: The Effect of Medicaid Eligibility on Medical Debt

Dependent variables	Mean	2SLS estimates	
Has medical debt	0.064	0.011	0.010
		(0.014)	(0.014)
Number of medical debts	0.072	0.003	0.002
		(0.013)	(0.013)
Amount of medical debt	379	-119	-122
		(425)	(414)
Medical debt more than $100	0.057	0.015	0.014
		(0.013)	(0.012)
Medical debt more than $1,000	0.030	0.014	0.013
		(0.011)	(0.011)
Medical debt more than $10,000	0.005	0.003	0.003
		(0.005)	(0.004)
Additional controls			x

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The data come from the 2000 to 2014 CEX. The sample contains 6,049 families/observations. Each cell is the coefficient on the number of people in a family eligible for Medicaid from Equation (1). All regressions control for family-type-year and family-type-state fixed effects. The second column of estimates also includes controls for family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race/ethnicity of the head of the household, the number of working adults in the family, the age of the head of the household, the annual state unemployment rate, and the number of family members with high school degrees, with some college, and with bachelors' degrees. Standard errors are clustered at the state level and are shown in parentheses below the estimates.

Table 8: Robustness of Select CEX Results

	(1) Original estimates	(2) Keeping families with workers but no income	(3) Using imputed income	(4) Dropping families with imputed incomes	(5) Using CPS for instrument	(6) Using fixed sample for instrument	(7) Using CEX weights	(8) Collapsing to one observation per family	(9) Broaden sample to 250 percent FPL	(10) Income threshold as independent variable
Number of people receiving Medicaid	0.142***	0.169***	0.137***	0.165***	0.146***	0.169***	0.155***	0.138***	0.159***	0.175***
	(0.050)	(0.058)	(0.048)	(0.059)	(0.043)	(0.049)	(0.050)	(0.050)	(0.046)	(0.045)
Number of private health insurance plans	-0.055***	-0.057***	-0.050**	-0.046***	-0.045**	-0.056**	-0.051*	-0.038*	-0.027	-0.057***
	(0.020)	(0.020)	(0.019)	(0.015)	(0.018)	(0.022)	(0.026)	(0.021)	(0.023)	(0.016)
Any medical spending	-0.027**	-0.026*	-0.023**	-0.027**	-0.017	-0.027**	-0.031**	-0.025	-0.025	-0.025*
	(0.011)	(0.015)	(0.011)	(0.013)	(0.011)	(0.012)	(0.015)	(0.017)	(0.015)	(0.014)
Medical spending positive but less than $100	-0.019**	-0.011	-0.020**	-0.022**	-0.016**	-0.021**	-0.016**	-0.023*	-0.011	-0.017
	(0.008)	(0.012)	(0.008)	(0.008)	(0.008)	(0.009)	(0.007)	(0.013)	(0.010)	(0.012)
Medical spending on at least $100	-0.008	-0.015	-0.003	-0.005	-0.001	-0.006	-0.015	-0.002	-0.014	-0.009
	(0.010)	(0.013)	(0.009)	(0.010)	(0.009)	(0.010)	(0.012)	(0.013)	(0.013)	(0.009)
Average medical spending	11	16	5	-1	17*	12	2	-4	4	6
	(10)	(16)	(10)	(10)	(9)	(10)	(11)	(14)	(12)	(11)
Any health insurance spending	-0.043***	-0.044***	-0.037***	-0.043***	-0.037***	-0.047***	-0.045***	-0.039***	-0.031**	-0.049***
	(0.012)	(0.015)	(0.011)	(0.013)	(0.009)	(0.012)	(0.015)	(0.014)	(0.014)	(0.010)
Health insurance spending positive	-0.000	-0.006	-0.001	-0.003	0.001	0.002	-0.001	0.006	0.006	0.003
but less than $100	(0.005)	(0.006)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Health insurance spending on at least $100	-0.043***	-0.039**	-0.036***	-0.040***	-0.038***	-0.048***	-0.044***	-0.045***	-0.038***	-0.052***
	(0.011)	(0.015)	(0.010)	(0.012)	(0.009)	(0.012)	(0.014)	(0.016)	(0.014)	(0.009)
Average health insurance spending	-47***	-41**	-32***	-28**	-41***	-50***	-52***	-51***	-38*	-53***
	(17)	(19)	(12)	(11)	(14)	(17)	(19)	(18)	(19)	(17)

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The data come from the 2000-2014 CEX. In columns 1, 5, 6, 7, and 10 the sample contains 30,752 observations from 14,024 families. In column 2, the sample contains 41,509 observations from 18,448 families. In column 3, the sample contains 28,722 observations from 13,179 families. In column 4, the sample contains 22,170 observations from 10,526 families. In column 8, the sample contains one observation for each of the 14,024 families in the main sample. In column 9, the sample contains 38,322 observations from 16,904 families. Each cell in columns 1-9 is the coefficient on the number of people in a family eligible for Medicaid from Equation (1). Each cell in column 10 is the coefficient on the parental Medicaid eligibility threshold as a fraction of the FPL. All regressions control for family-type-year fixed effects, family-type-state fixed effects, family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race/ethnicity of the head of the household, the number of working adults in the family, the age of the head of the household, the annual state unemployment rate, and the number of family members with high school degrees, with some college, and with bachelors' degrees. Standard errors are clustered at the state level and are shown in parentheses below the estimates.

Table 9: Considering the Possibility of Preexisting Trends

	Including within state control group	State-specific time trends	Year before eligibility change	Year before eligibility increase > 10 percentage points
Number of people receiving Medicaid	0.101**	0.091*	0.013	-0.016
	(0.041)	(0.052)	(0.021)	(0.028)
Number of private health insurance plans	-0.065	-0.062***	0.004	0.014
	(0.039)	(0.016)	(0.009)	(0.015)
Any medical spending	-0.010	-0.028**	0.007	-0.018
	(0.018)	(0.011)	(0.007)	(0.012)
Medical spending positive but less than $100	-0.015	-0.023**	-0.001	-0.009
	(0.013)	(0.009)	(0.005)	(0.007)
Medical spending on at least $100	0.005	-0.005	0.008	-0.009
	(0.013)	(0.009)	(0.007)	(0.009)
Average medical spending	1	12	10	-5
	(25)	(11)	(11)	(13)
Any health insurance spending	-0.035*	-0.039***	0.006	0.006
	(0.018)	(0.012)	(0.009)	(0.010)
Health insurance spending positive but less than $100	0.007	-0.003	0.000	-0.004
	(0.009)	(0.006)	(0.004)	(0.004)
Health insurance spending on at least $100	-0.042*	-0.036***	0.006	0.010
	(0.021)	(0.012)	(0.007)	(0.009)
Average health insurance spending	-66**	-45***	11	1
	(31)	(16)	(11)	(14)

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The data come from the 2000 to 2014 CEX. In column 1, the sample contains 75,549 observations from 29,875 families. In columns 2, 3, and 4, the sample contains 30,752 observations from 14,024 families. Each cell in columns 1 and 2 is the coefficient on the number of people in a family eligible for Medicaid from Equation (1). Each cell in column 3 is the coefficient on an indicator variable equal to one the year before Medicaid eligibility increases. Each cell in column 4 is the coefficient on an indicator variable equal to one the year before Medicaid eligibility changes by more than 10 percentage points. All regressions in columns 2, 3 and 4 control for family-type-year fixed effects, family-type-state fixed effects, family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race/ethnicity of the head of the household, the number of working adults in the family, the age of the head of the household, the annual state unemployment rate, and the number of family members with high school degrees, with some college, and with bachelors' degrees. All regressions in column 1 include family-type-income-group-year, family-type-income-group-state, and family-type-year-state fixed effects rather than family-type-year and family-type-state fixed effects, while all regressions in column 2 also control for state-specific linear time trends. Standard errors are clustered at the state level and are shown in parentheses below the estimates.

Table 10: LATE Estimates of the Effect of Medicaid

Number of private health insurance plans	-0.394**	-0.387**
	(0.184)	(0.160)
Any medical spending	-0.204*	-0.193**
	(0.105)	(0.090)
Medical spending positive but less than $100	-0.146	-0.136*
	(0.088)	(0.079)
Medical spending of at least $100	-0.058	-0.058
	(0.068)	(0.064)
Average medical spending	79	75
	(81)	(79)
Any health insurance spending	-0.318**	-0.306***
	(0.127)	(0.113)
Health insurance spending positive but less than $100	-0.000	-0.002
	(0.033)	(0.032)
Health insurance spending of at least $100	-0.318**	-0.304**
	(0.128)	(0.113)
Average health insurance spending	-347**	-333**
	(145)	(134)
Additional controls		x

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The data come from the 2000 to 2014 CEX. The sample contains 30,752 observations from 14,024 families. Each cell is the LATE effect of an additional person in the family enrolling in Medicaid. All regressions control for family-type-year and family-type-state fixed effects. The second column of estimates also includes controls for family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race/ethnicity of the head of the household, the number of working adults in the family, the age of the head of the household, the annual state unemployment rate, and the number of family members with high school degrees, with some college, and with bachelors' degrees. Standard errors are clustered at the state level and are shown in parentheses below the estimates.

Appendices

A The Model for the Conceptual Framework

In this appendix, I provide a fuller discussion of the welfare implications for the family from crowd-out. As described in Section 2, the welfare implications of crowd-out that arises from Medicaid expansions depend on the quality of private coverage that is crowded out and the share the family was paying for the private coverage. The following simple framework illustrates the possible welfare changes that may be associated with crowd-out.

Suppose that a family has utility $u(c)$ that is an increasing function of non-health consumption c. The family's spending must satisfy the budget constraint $c = y - m - P$, where y is income, m is out-of-pocket medical expenditures, and P is the premium spent on private health insurance. Both P and y are known in advance, while m is a random variable with probability density function $f(m)$ and support $[0, \overline{m}]$.[26] Suppose that private insurance is actuarially fair and covers all expenditure risk, meaning that

$$P = \int_0^{\overline{m}} f(m)dm.$$

If the family purchases private insurance, its utility equals

$$u(y - \delta P),$$

where $\delta \in [0,1]$ is the family's portion of the premium.[27]

This simple framework allows for describing the utility available to the family in each of the three pre-Medicaid eligibility cases from Section 2. For simplicity, I

[26] This simple set up draws on Finkelstein and McKnight (2008), who consider the welfare effects of Medicare.

[27] The determinants of δ are beyond the scope of this paper. As described in Section 2, if workers value money spent on health insurance as much as they would value the money itself, if employers can perfectly identify which people are eligible for Medicaid, and if employers can perfectly and instantly adjust compensation, theory predicts that employers will pass the costs of health insurance to employees in the form of lower wages (Gruber 1994; Summers 1989). Therefore, Medicaid expansions have the potential to increase wages if employers no longer have to provide workers with health insurance coverage. However, these assumptions may not hold for a variety of reasons. For example, identifying Medicaid-eligible people is likely difficult since employers would have to know total family income. Furthermore, Finkelstein, Hendren, and Luttmer (2015) find that the Oregon health insurance experiment's Medicaid recipients only value Medicaid at 20 to 40 percent of Medicaid's cost, suggesting that many low-income workers may not fully value health insurance.

assume the price and coverage for poor health insurance are approximately zero, meaning that the family would essentially be uninsured even though it shows up in survey data sets as having private insurance. The utility for each original case from Section 2 can be summarized as follows:

1. The family was paying very little for good coverage (δ=0): $u(y)$
2. The family was paying full price for good coverage (δ=1): $u(y-P)$
3. The family was paying very little for poor coverage (equivalent to not purchasing insurance): $\int_0^{\overline{m}} u(y-m)f(m)dm$

Assuming that Medicaid covers all expenditure risks, then the family's utility is $u(y)$ with Medicaid. Therefore, the utility gains from switching to Medicaid are

1. The family was paying very little for good coverage: $u(y)-u(y)=0$
2. The family was paying full price for good coverage: $u(y)-u(y-P)$
3. The family was paying very little for poor coverage: $u(y)-\int_0^{\overline{m}} u(y-m)f(m)dm$

Utility being an increasing function of consumption means both $u(y)-u(y-P)$ and $u(y)-\int_0^{\overline{m}} u(y-m)f(m)dm$ are positive. Thus, the crowd-out of (2) and (3) make the family better off, while crowd-out of (1) does not.

The availability of actuarially fair insurance and the family having a concave utility function mean that $u(y)-\int_0^{\overline{m}} u(y-m)f(m)dm > u(y)-u(y-P)$, which would imply that crowd-out of (3) is more welfare improving than the crowd-out of (2). Of course, these assumptions would also imply that low-income families always purchase insurance, which is clearly not the case. Families likely do not purchase health insurance because insurance is not actuarially fair in reality, especially since some low-income families may not pay emergency room medical bills, meaning they do not bear all of the costs of their negative health shocks if they are uninsured. The empirical result of Medicaid eligibility causing both private coverage and health insurance spending to fall would imply that crowd-out of (2) has occurred. The empirical result of Medicaid eligibility causing private coverage but not health insurance spending to decrease, means crowd-out of (1) or (3) occurred.

Another point to note is that concavity means that $u(y)-u(y-P) > u(y')-u(y'-P)$ and $u(y)-\int_0^{\overline{m}} u(y-m)f(m)dm > u(y')-\int_0^{\overline{m}} u(y'-m)f(m)dm$ for $y'>y$. In other words, the less income a family has, the more welfare improving crowd-out of (2) or (3) is for the family.

B The Effect of Medicaid Eligibility on Nonhealth spending, Participation in Other Programs, and Labor Market Outcomes

The main manuscript focused on exploring the first-order effects of Medicaid on household spending, but Medicaid has the potential to affect household budgets in a variety of ways. I now consider second-order effects of Medicaid eligibility on household budgets.

Through cost savings on medical care and health insurance, Medicaid eligibility has the potential to lower total expenditures or to increase non-health-related spending. As improved medical care access can lead to conversations with doctors that lead families to consume less tobacco or alcohol and because smoking cessation programs are part of Medicaid in many states, Medicaid can also influence spending on categories that have health-related consequences.

Panel A of Table B.1 displays estimates of the effect of an additional person being eligible for Medicaid on various types of expenditures and shows no evidence that Medicaid eligibility affects other forms of spending. These results could indicate that Medicaid eligibility has no impact on other forms of spending, but they may also reflect that effects on other forms of spending are too subtle to detect with these data and with this identification strategy.

These results differ from Gruber and Yelowitz (1999), who find a positive effect of Medicaid eligibility on non-medical consumption using 1983 to 1993 data. The fact that I do not find evidence of large consumption changes could occur because of a lack of power. However, it could also reflect the fact that Medicaid programs no longer have asset tests, which Gruber and Yelowitz find interact with eligibility to make the effect of eligibility on consumption even larger.

Medicaid eligibility has the potential to affect labor market outcomes and program participation in a variety of ways. For example, the income ceiling for Medicaid eligibility may discourage employment and earnings so families can maintain eligibility. The insurance coverage from Medicaid may lessen the advantages of entering into the labor market for employer-sponsored coverage. On the other hand, Medicaid improving health could increase labor force participation or productivity. Furthermore, any changes in earnings could affect eligibility for other means-tested programs, while participation in Medicaid may increase awareness of other programs.

Panel B of Table B.1 considers the effects of an additional person being eligible for Medicaid on the likelihood that families participate in other social programs, while Panel C considers the impact on the number of earners and on family income as a

percent of the FPL. I find no evidence that Medicaid eligibility affects receipt of benefits from Social Security Disability Insurance, Supplemental Security Insurance, unemployment insurance, workers' compensation insurance, food stamps, or welfare. Similarly, I find no evidence of effects on earnings or family income as a percent of the FPL.[28]

[28]Other research typically finds no effect of family-level Medicaid expansions on labor force participation (e.g., Ham and Shore-Sheppard 2005b; Meyer and Rosenbaum 2001), though there are exceptions (e.g., Dave et al. 2015; Yelowitz 1995). Hamersma and Kim (2009) find that Medicaid eligibility results in unmarried mothers being more likely to change jobs. Burns and Dague (2016) find that Medicaid eligibility for childless adults lowers the likelihood that adults receive Supplemental Security Income.

Table B.1: The Effect of Medicaid Eligibility on Non-health Spending, Participation in Other Programs, and Labor Market Outcomes

Dependent variables	Means	2SLS estimates	
Panel A: Other Spending			
Spending on food	1,793	-16	-20
		(25)	(27)
Spending on clothes	349	17	14
		(13)	(14)
Spending on housing	3,603	82	57
		(68)	(87)
Spending on transportation	1,831	-107	-115
		(118)	(109)
Spending on entertainment	419	13	7
		(23)	(24)
Spending on education	156	11	12
		(33)	(31)
Contributions to retirement	172	-2	-4
		(23)	(26)
Spending on cigarettes	88	-6	-6
		(11)	(12)
Spending on alcohol	41	0	-1
		(4)	(4)
Total spending	11,069	7	-57
		(282)	(298)
Panel B: Other Social Programs			
Receives SSDI	0.057	-0.005	-0.002
		(0.009)	(0.009)
Receives SSI	0.058	-0.008	-0.005
		(0.007)	(0.007)
Receives unemployment	0.056	-0.010	-0.009
		(0.010)	(0.010)
Receives workers' compensation	0.011	-0.003	-0.003
		(0.005)	(0.005)
Receives food stamps	0.267	-0.004	-0.000
		(0.011)	(0.009)
Receives welfare	0.057	0.001	0.003
		(0.007)	(0.007)
Panel C: Income and Employment			
Percent of federal poverty level	1.006	0.001	0.002
		(0.019)	(0.019)
Number of people working	1.294	0.012	0.019
		(0.021)	(0.017)
Additional controls			x

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The data come from the 2000 to 2014 CEX. The sample contains 30,752 observations from 14,024 families. Each cell is the coefficient on the number of people in a family eligible for Medicaid from Equation (1). All regressions control for family-type-year and family-type-state fixed effects. The second column of estimates for Panels A and B also includes controls for family income as a percent of the FPL, the square of family income as a percent of the FPL, an indicator for the head of the household being male, indicators for the race/ethnicity of the head of the household, the number of working adults in the family, the age of the head of the household, the annual state unemployment rate, and the number of family members with high school degrees, with some college, and with bachelors' degrees. In Panel C, the additional controls are the same but do not include income as percent of the FPL or the number of people in the family working. Standard errors are clustered at the state level and are shown in parentheses below the estimates.