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How Welfare Programs Affect Participants' Healthcare and Labor Market Outcomes

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How Welfare Programs Affect Participants' Healthcare and Labor Market Outcomes

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ABSTRACT

This dissertation consists of three essays. The first essay investigates the implications of the Supplemental Nutrition Assistance Program (SNAP) benefit cycle, whereby the benefits are paid once per month, but often do not last for the entire month. We examine whether the benefit cycle affects the timing of medical visits among SNAP recipients. We find that the likelihood of visiting emergency departments, outpatient providers, and dentists are lower in the last week of the benefit month for two-parent, but not single-parent households. This is consistent with the finding that two-parent households have a higher propensity to spend SNAP benefits early in the month than single-parent households, which leads to higher cash spending on food during the last week of the benefit month. These results suggest that two-parent households may need to reduce spending on medical care at the end of the SNAP benefit month in order to reallocate cash income to the purchase of food. Moreover, we show that the reduction in emergency room visits is concentrated among emergent injury-related visits and does not differ by insurance coverage. We thus conclude that the benefit cycle not only constitutes a direct barrier to care but also affects health care utilization indirectly by reducing the need for care.

The second essay examines how the SNAP affects labor force decisions. Labor supply theory predicts that social welfare programs will provide work disincentives to low-paid workers. In response to concerns about welfare dependency, past policy reforms linked work requirements to the SNAP. As a result, for those subject to a work requirement, the labor supply effect of SNAP participation is theoretically ambiguous. This paper empirically examines the impact of SNAP enrollment on labor supply. To account for non-random selection into SNAP, we use quasi-experimental variation in the purchasing power of SNAP benefit amounts as an instrument. For a household of the same composition, SNAP benefit levels are fixed across states, but local food

prices vary widely, leading to substantial variation in the real value of SNAP benefits. Our IV-fixed effect results suggest that SNAP participation increases the likelihood of employment and full-time work among low-income adults. We also find evidence that SNAP participation facilitates employment by increasing the recipients' ability to pay for job-related expenses such as childcare. Moreover, we find that SNAP improves physical and mental health, which could also contribute to higher work effort.

The third essay investigates the effect of the Medicaid fee bump, the largest ever increase in Medicaid primary care reimbursement rates, on the use of medical services. We find that more generous Medicaid payments to primary care providers increase the number of office-based primary care visits. This increase is much larger for mid-level providers than physicians, indicating that the use of nurse practitioners and physician assistants is important to how practice groups respond to payment changes. The largest increases in visits with physicians observed in states with restrictive scope of practice laws governing nurse practitioners. We also find that higher Medicaid fees are associated with improvements in access to timely care and an increase in the utilization of prescription drugs, suggesting that at least some of the additional primary care services by Medicaid enrollees were necessary. Our results also provide suggestive evidence that consumers of medical care with less serious conditions substitute away from services provided in the hospital setting when access to more convenient alternatives improves.

CHAPTER 1

The Within-Month Pattern of Medical Care Utilization among SNAP Households

1.1 Introduction

Although the Supplemental Nutrition Assistance Program (SNAP, formerly the Food Stamp Program) plays a leading role in preventing malnutrition by supplementing food budgets (Hoynes and Schanzenbach 2009), potential negative effects of the monthly nature of benefits provision have been raised. Administrative records reveal that a large proportion of households redeem nearly all of their benefits in the first two weeks of the month (Castner and Henke 2011). This results in what is known as the SNAP benefit cycle, whereby SNAP participants purchase and consume more food at the beginning of the benefit month with SNAP benefits, but less and lower quality food towards the end of the month when their benefits are exhausted (Todd 2015; Wilde and Ranney 2000; Shapiro 2005; Damon et al. 2013; Hamrick and Andrews 2016; Castellari et al. 2017; Kuhn 2018; Gregory and Smith 2019). In this paper, we investigate whether the benefit cycle also affects the timing of medical visits among SNAP-recipients.

Under the permanent income hypothesis framework, these results are puzzling, as consumption should be unrelated to when expected income is received. Shapiro (2005) attributes the end of the month shift in consumption to present-biased time preferences, whereby decision makers over-consume in the present, without internalizing that they will fail to resist the temptation to do so again in the future. He argues that an empirical manifestation of this is downward-sloping intra-month consumption profiles. Smith et al. (2016) find that short-run impatience contributes to the SNAP benefit cycle and that non-fungibility of income can exacerbate the effect of impatience on consumption decisions.

On the other hand, prior studies of SNAP households suggest that intra-household resource allocation decisions may have a significant impact on food purchases and nutrition. For example, Breunig and Dasgupta (2005) construct a noncooperative model of intra-household decision-making and use it to demonstrate that food purchases in multiple-adult SNAP households would decrease if their benefits were replaced with a cash transfer. There is also evidence of such intra-household disagreements over the allocation of resources among welfare recipients enrolled in other public programs (Angelucci 2008; Hidrobo and Fernald 2013; Hsu 2017). For example, Hsu (2017) finds an increase in reports of male-on-female assault shortly after the household receives Temporary Assistance to Needy Families (TANF) payments.

These findings suggest that intra-household resource allocation decisions could be exacerbating the degree of the cycle in a two-parent household. The primary SNAP recipient (usually the mother in two-parent households) has greater control over program benefits than other family members.[1] If SNAP benefits are exhausted at the end of the month, then the primary recipient will need to obtain cash income to purchase the same quantity and quality of food during the last week of the month as during the prior three weeks. Under the assumption that the primary recipient has greater preferences than the non-primary-recipient adult members for food, the benefit owner can pre-commit the household to a monthly consumption plan closer to the individual preference by exhausting SNAP benefits early in the month. This strategic behavior persuades non-primary-recipient member to contribute more cash income to buy food at the end of the month. This pre-commitment device increases the household's total food consumption, but

[1] As part of the 1996 welfare reform act states are required to issue SNAP benefits through the Electronic Benefit Transfer (EBT) system. State EBT instructions explicitly tell primary recipients (usually the mother) not to give their PIN to anyone else; if they want a family member to access the benefits, the primary recipient is instructed to apply for a second EBT card to be used by that family member.

only at the cost of cyclical consumption pattern over the course of the month (although, this does not rule out the existence of cycle independent of the household type driven by other factors such as short-run impatience).

Although SNAP benefits must be spent on food, the benefit cycle may also have implications for medical care consumption. Conceptually, there are a number of reasons why health care utilization may be impacted by the SNAP benefit cycle. First, deductibles, copayments, and other costs of medical visits, such as transportation costs, or the need to reduce paid work time may discourage medical care utilization to free up cash income for food purchases when SNAP benefits are exhausted. One would expect such an effect to be less consequential for the publicly insured who typically face the lowest copayments and deductibles.

Second, SNAP payments may impact health care consumption indirectly, via changes in health. The end of month change in nutrient availability may have adverse effects on participants' health and result in medical care utilization. This is likely to be the case only for participants with diet-sensitive chronic conditions, such as diabetes or gastroesophageal reflux disease. Alternatively, the indirect effect could be negative if households reduce certain types of consumption which affect the need for hospital care when the benefits are exhausted. For instance, if some consumers spend more on recreational drugs on the benefit receipt days, then such a change in consumption patterns might affect health care utilization by leading to adverse events. Such a mechanism would be consistent with the finding that SNAP payments change the pattern of alcohol purchases (Hastings and Washington 2010) and alcohol-related accidents (Cotti et al. 2016).

The literature regarding the affordability of health care supports the view that individuals may not consume the health care they need because they cannot afford it. For example, Moran and

Simon (2006) estimate that an increase in lifetime income (driven by the Social Security benefits "notch") increases the consumption of pharmaceuticals. On the other hand, previous studies have demonstrated that cash welfare programs affect hospital visits by increasing the need for emergency care (Dobkin and Puller 2007; Gross and Tobacman 2014).

Only a few studies consider the within-month patterns of medical care utilization using data on either SNAP participants or low-income households in general, and the findings are contradictory. Heflin et al. (2017) use administrative data from the Missouri SNAP and Medicaid programs to examine whether the timing of the benefits affects the within-month pattern of emergency room visits for hypoglycemia, and find no evidence of a cyclical pattern. Heflin et al. (2019) find a similar result with regards to SNAP timing and childhood asthma. In contrast, Seligman et al. (2014) show that inpatient admissions for hypoglycemia increased by 27 percent in the last week of the month relative to the first week for the low-income population in California. Similarly, Basu et al. (2017) find using data on medical claims that nonelderly adults in the lower half of the income distribution have a higher probability of visiting the emergency room and being hospitalized for the treatment of hypoglycemia at the end of the calendar month.

This paper is the first comprehensive study that investigates whether the SNAP benefit cycle alters consumption patterns for medical care, and specifically, whether benefit-receiving households reduce their medical care utilization towards the end of the benefit month. In order to explore whether intra-household bargaining could be exacerbating the degree of the SNAP benefit cycle, we examine medical care utilization patterns across the benefit month for two-parent and single-parent households, separately.

Our results suggest that there is a reduction in the probability of visiting medical care providers at the end of the benefit month among SNAP-recipient households. However, only two-parent households experience cycles in medical care utilization that are attributed to SNAP payments. We document evidence that two-parent households exhibit a much higher propensity to spend SNAP benefits at the beginning of the month, which leads to higher cash spending on food during the last week of the benefit month. Consequently, two-parent households may need to reduce non-food spending at the end of the SNAP benefit month in order to reallocate cash income to the purchase of food. For example, we find that the likelihood of visiting a dentist and outpatient provider is lower in the last week of the benefit month than the beginning of the month.

Moreover, our results suggest that SNAP benefits also affect medical care use by inducing risky forms of consumption. We find that the decline in emergency room visits is concentrated among injury and accident-related visits, in which the timing of care is not discretionary. Thus, the liquidity constraint at the end of the benefit month not only is a direct barrier to care but also impacts health care utilization indirectly, via changes in health.

1.2 Data

The main data source used in this paper is the 1996-2013 Medical Expenditure Panel Survey (MEPS). The MEPS is a comprehensive, nationally representative survey of the U.S. civilian non-institutionalized population. It contains detailed information for each individual in the household on demographic characteristics, socioeconomic status, health status, health insurance coverage, and whether anyone in the household received SNAP in the past year. Respondents are also interviewed about their medical care use over the course of two years through five survey rounds.

We analyze medical care utilization patterns for outpatient visits (visits to office-based physicians and hospital outpatient departments), emergency room (ER) visits, dental visits, and visits to inpatients facilities (i.e. hospitals). We use information on the date of each medical visit to determine in which week of the month the visit occurred. We define the first seven days of the month as week 1 and the last seven days of the month as week 4. The remaining days of the month are split evenly between weeks 2 and 3, with an extra day added to week 3 as needed.[2]

We consider two sets of analysis. In our first analysis, we subset the sample to SNAP participants if the household received SNAP in any of the twelve months. Our second analysis considers a sample of SNAP-eligible individuals to compare the utilization pattern of SNAP participants with utilization pattern of SNAP-eligible non-participating households. In order to determine whether households were eligible for SNAP benefits we use information in the MEPS to predict whether each household passed the gross income and net income tests. Although households must also pass an asset test to receive SNAP benefits, the MEPS does not contain sufficient information for us to evaluate this eligibility criteria (USDA, SNAP Eligibility 2016; USDA, Income Eligibility Standards 2016). However, only 3% of households who apply for SNAP benefits fail the asset test (Wheaton et al. 2016). The exact method we use to determine SNAP eligibility is contained in the supplementary appendix.

We further restrict our sample to households with at least one child under age 18 and exclude households with a parent younger than age 20 or older than age 50. We disaggregate the sample into single-parent and two-parent households. Parents that were not married, but cohabitated with another adult, are classified as two-parent households.

The primary explanatory variable in our regression model is whether a particular individual in week w is in the last week of the benefit month. Not all states issue SNAP benefits on the same day of the month to all households, nor do they all issue benefits at the very beginning of the calendar month. Some states choose to distribute benefits over several weeks during the month

[2] We drop visits where the date of treatment visit is missing. Since we are concerned that there might be selection into non-reporting, we re-estimate our models using only the subset of individuals with non-missing information on the date of all treatment visits and the results are quantitatively similar to our main results.

(USDA, SNAP Monthly Benefit Issuance Schedule 2016). One limitation of the MEPS is that it does not include information on the date when each household last received SNAP benefits, so it is impossible to determine the last week of benefit month for all SNAP recipients with certainty. Therefore, using state and county codes contained in the restricted-use MEPS, we merge data on the historical monthly SNAP benefit issuance schedule in each state and calculate the probability that each calendar week is the last week of the benefit month.[3,4] In order to check the validity of our conclusions from this approach, we also perform a robustness check on the subsample of households from states that issue benefits on a single day of the month.[5]

Control variables in each model include age (dichotomous indicators for age 7–17, 18–30, 31–45, 31–45, 46–60 , 61–75, age 76 and older with age 0–6 as the omitted category), gender, race and ethnicity (Hispanic, black, and other race with white as the omitted category), region (South, Midwest, and West with Northeast omitted), urban residence, education (high school diploma, any college, with less than a high school degree omitted), number of children in the household, the log of total family income normalized by the square root of household size, and insurance coverage (Medicare, Medicaid, private insurance with uninsured omitted). In order to control for health status, we use self-reported mental and physical health (poor/fair health in all rounds, poor/fair health in some rounds, excellent health in some rounds, excellent health in all rounds, good/very good health in all rounds, and self-reported health is missing, with good/very good health in some rounds serving as the omitted category for both mental and physical health) and a measure of

[3] For example, the benefits are made available over the first 10 days in California every month. We assign probabilities of 3/10 to calendar week 1, and 7/10 to calendar week 4 for being last benefit weeks.
[4] New York follows two different schedules for the Upstate and New York City regions. Therefore, we use county codes to merge the monthly distribution dates in New York.
[5] We drop Alabama, Illinois, Missouri, Mississippi and New Mexico from our sample because their benefit payments are spread over a large number of days. Benefits are made available over 20 days in New Mexico and Alabama, 22 days in Missouri, 18 days in Mississippi, and on the 1st, 3rd, 4th, 5th, 6th, 7th, 8th, 9th, 10th, 13th, 17th, and 20th of every month in Illinois.

disability status. The latter is a binary variable that indicates whether the person had an IADL (Instrumental Activities of Daily Living) or ADL (Activities of Daily Living), functional, activity, or sensory limitation in any interview round.

We also include control variables in our models for a number of state characteristics that were obtained from the U.S. Census Bureau: state-level per-capita income, the poverty rate, unemployment rate and the percentage of persons 25 years of age and older with a bachelor's degree. Table 1-1 contains descriptive statistics for all of the variables we use in the analysis by household type and SNAP participation. These statistics show clear selection into SNAP by individuals with lower incomes, less education, poorer health status, and higher rates of disability, and by Medicaid recipients.

1.3 Empirical Strategy

1.3.1 Analysis of SNAP-Recipient Households

In this section, we analyze within-month medical care utilization patterns for SNAP participating households. This model is estimated on medical care utilization data that are aggregated by week of the month for each individual. In particular, we estimate the following linear regression model that predicts whether the individual visits a medical care provider in a given week:

$$\Pr(m_{iswt} = 1) = \beta wgt_{swt} + X'_{ist}\psi + \tau_w + \tau_t + \tau_s, \tag{1-1}$$

where m_{iswt} is a binary indictor of a visit by individual i to a medical care provider in state s, year t, and week w, where $w = 1, \ldots, 4$. wgt_{swt} is the probability that a particular individual in state s in week w is in the last week of the benefit month (as described in the data section). X_{ist} is a vector of covariates including individual and family-level demographic and socioeconomic variables, health insurance coverage, self-reported health status and a measure of disability.[6] τ_t is a vector of year fixed-effects and τ_s is a vector of state fixed-effects. We also include τ_w, a vector of week fixed effects which account for common payroll trends (i.e. 1st or 15th of a month). Standard errors are clustered by the state to account for serial correlation of the errors within states over time.

Table 1-2 contains the effects of being in the last week of the SNAP benefit month on medical care utilization from equation (1-1) for both two-parent and single-parent SNAP households. Our models are estimated on outpatient visits, ER visits, dental visits, and inpatient admissions. We find that both two-parent and single-parent households are less likely to visit an outpatient provider at the end of the SNAP benefit month. While two-parent SNAP households are 3.1 percentage points (10.5%) less likely to visit an outpatient provider in the last week of the benefit month, the reduction in outpatient visits by single-parent households is 2.8 percentage points (9.1%). We also find that the probability of visiting a dentist is 1.6 percentage points (23.5%) lower in the last week of the SNAP benefit month than at the beginning of the month for two-parent households. However, we do not find any significant change in the pattern of dental visits among single-parent households. Finally, the likelihood of inpatient admissions and ER visits do not vary significantly at the end of the benefit month for both types of households. The results from alternative dependent variables, the log of number of visits are quantitatively similar (Table 1-2, Panel B).[7]

To offer additional clarity on the differential effect of being in the last week of the month for two-parent households, we interact our wgt_{swt} in equation (1-1) with a dummy variable

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[6] In our main specification, we include household income as a control variable. We also estimate our models that do not include household income and the results are quantitatively similar to our main results.

[7] The outcome takes zero if an individual does not have any visit in week w.

indicating whether SNAP-recipient household is a two-parent household. We estimate this model on the sample of SNAP-recipients (both two-parent and single-parent households). As shown in Table 1-3, individuals in a two-parent household experience an additional statistically significant decrease of 1.1 percentage points (21.5%) in the probability of ER visit at the end of the benefit cycle. In Table 1-3 there is an imprecise negative effect of being in the last week of the month on the probability of any dental visit. However, when we use the log of the number of dental visits as an alternative specification of the dependent variable, we find that two-parent households are 0.6 percent less likely to visit a dentist when they are in the last week of the benefit month.

1.3.2 Analysis of SNAP-Eligible Households

The results in Tables 1-2 and 1-3 provide evidence of monthly cycle in medical care utilization. However, a limitation of the preceding analysis is that we may not be able to fully distinguish the effect of the SNAP benefit cycle from other possible cycles. Much like SNAP, evidence suggests that other streams of income are not spent smoothly. For example, Stephens (2006) shows that expenditures increase immediately upon receipt of a paycheck.[8] In order to difference out trends in medical care utilization that are not related to SNAP payments, we compare medical care utilization patterns of both of SNAP households to SNAP-eligible non-participating households. Since enrollment in SNAP is voluntary, we use both panel data models and instrumental variables to account for selection into SNAP by individuals with unobserved attributes that are correlated with medical care use and the SNAP participation decision (Meyerhoefer and Yang 2011).

Fixed-Effects Model

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[8] Other evidence includes analyses of the immediate consumption response to semi-annual bonuses (Browning and Collado 2001), income tax refunds (Souleles 1999), annual payments to Alaskans from the Alaska Permanent Fund (Hsieh 2003), and the final payment of a car loan (Stephens 2008).

Under the assumption that the unobservable characteristics of SNAP participants correlated with medical care utilization are time invariant, we can account for the endogeneity of SNAP participation using individual fixed-effects. We believe this is reasonable because the primary unobserved characteristics of SNAP participants likely to lead to endogeneity bias are preferences for medical care and health status. While the former is likely time invariant, the latter is usually time varying. However, we include controls in our model with self-reported physical and mental health, which should capture changes in health status, leaving as unobserved the component of health status that is time invariant. We therefore use the following specification:

$$\Pr(m_{iswt} = 1) = \beta\ SNAP_{ist} + \gamma\ SNAP_{ist} \times wgt_{swt} + X'_{ist}\psi + \tau_w + \tau_t + \tau_i, \tag{1-2}$$

where $SNAP_{ist}$ is an indicator variable equal to one if individual i is a SNAP recipient, and equal to zero otherwise.[9] τ_i is a stochastic time-invariant individual specific effect that captures the unobserved determinants of medical care utilization. The main effects of interest are β, the effect of SNAP participation on medical care utilization, and γ, the effect of being in the last week of the SNAP benefit cycle on a SNAP recipient's decision to visit a medical provider.

Instrumental Variables Model

Our instrumental variable model is specified as a recursive bivariate probit model. The first equation in the model predicts SNAP participation and the second equation, which is a function of SNAP participation, predicts whether the individual visits a medical care provider in a given week of the month. Therefore, we have:

$$\Pr(SNAP_{iswt} = 1) = \Phi(Z'_{st}\lambda + X'_{ist}\varphi + \tau_t), \tag{1-3}$$

$$\Pr(m_{iswt} = 1) = \Phi(\beta\ SNAP_{ist} + \gamma\ SNAP_{ist} \times wgt_{swt} + X'_{ist}\psi + \tau_w + \tau_t), \tag{1-4}$$

[9] We also estimate models that include wgt as both an individual regressor and interaction term, and find similar results. Importantly, the estimated coefficients on wgt in those models are not statistically different from zero.

where $\Phi(\cdot)$ is the standard normal cumulative distribution function. Z_{st} is our instruments for SNAP participation. The error terms $(\varepsilon_{iswt}, \upsilon_{ist})$ are assumed to be independent of Z_{st}, and distributed as bivariate normal with mean zero and unit variance. In addition, $\rho = corr(\varepsilon_{iswt}, \upsilon_{ist})$ is assumed to be non-zero.

The instruments we use to identify SNAP participation are simplified reporting requirements and whether the SNAP recipient's state of residence operates call centers (USDA, SNAP Policy Database 2016). State-level variables have been widely used in the literature as instruments for SNAP participation (Meyerhoefer and Pylypchuk 2008, Yen et al. 2008, Ratcliffe et al. 2011, Gregory and Deb 2015).[10] The functions of call centers vary widely by state. Most call centers allow clients to report changes in income, assets, or household membership, answer general questions, and provide case information. Based on data from 2011, call centers completed initial application interviews and approved SNAP applications in four states, and some call centers in the state provided these services in eight other states (Rowe et al. 2010; USDA 2011). Under simplified reporting, SNAP households must only report income changes that occur during the reporting period if they result in total countable income rising above 130 percent of the poverty level. The 2002 Farm Bill gave states the discretion to extend simplified reporting requirements to households with non-earned income, referred to as expanded simplified reporting. Many states also lengthened reporting intervals to 4, 5 or 6 months for 12 month certification periods. We only distinguish between states that adopted any form of simplified reporting and those that did not. Call centers and simplified reporting improve program access, as a result, households in states in

[10] Call center is the most powerful predictor of SNAP participation in our sample of eligible households with young children.

which call center services are available or adopted simplified reporting are more likely to participate in SNAP.

We calculate the marginal effect of SNAP participation as:

$$ME_1 = Pr(m = 1|SNAP = 1, SNAP \times wgt = wgt, X) - Pr(m = 1|SNAP = 0\ , SNAP \times wgt = 0, X). \tag{1-5}$$

The estimated treatment effect of the coefficient on the interaction term in nonlinear models such as ours is given by the incremental effect of the coefficient on the interaction term (see Puhani 2012; Mayer et al. 2014). We therefore calculate the marginal effect of a SNAP participant being at the end of the benefit month on medical care utilization as:

$$ME_2 = Pr(m = 1|SNAP = 1, SNAP \times wgt = 1, X) - Pr(m = 1|SNAP = 1\ , SNAP \times wgt = 0, X). \tag{1-6}$$

1.4 Results

1.4.1 Main Results

Tables 1-4 contains estimates of the impact of SNAP participation and being in the last week of the SNAP benefit month on medical care utilization for two-parent and single-parent households. We present estimated effects from both FE model and our IV model that is identified using the presence of state SNAP call centers and simplified reporting requirements as instrumental variables. The first stage F-statistic for the instrument in the sample of two-parent households is 11.5, which exceeds the conventional threshold for sufficiently powerful instruments of F = 10 (Stock et al. 2002). However, in the sample of single-parent households, the F-statistic drops to 8.6.

We find that SNAP participation is associated with an increase in the probability of an outpatient visit by 2.2 – 4.0 percentage points (8.1% – 14.4%) (Table 1-4, Panel A). We also find that the probability that an individual in a two-parent household visits an outpatient provider is 0.6 – 1.5 percentage points (2.2% – 5.5%) lower at the end of the SNAP benefit month than at the beginning of the month. The marginal effects for emergency room (ER) visits among two-parent households indicate that SNAP participation is associated with an increase in the probability of visiting the ER by 0.7 – 1.1 percentage points (18.9% – 29.7%). As is the case with outpatient visits, the likelihood that a SNAP participant in a two-parent household visits the ER is 0.4 – 1.6 percentage points (10.9% – 43%) lower in the last week of the SNAP benefit month. We also find that SNAP participation is associated with a 0.7 percentage point (6.3%) increase in dental visits among two-parent households, but there is a 1.1 percentage point (15.9%) reduction in the probability of visiting a dentist at the end of the benefit cycle. We also find SNAP participation increases the likelihood of an inpatient admission among two-parent households by 0.4 percentage points (20%), although the marginal effect of SNAP participation in the IV model is not statistically significant.

The analogous effects of SNAP participation and the SNAP benefit cycle on medical care utilization among single-parent households are reported in Table 1-4, Panel B. In this case, we do not find any statistically significant impact of SNAP participation on outpatient visits in the IV model, but we our FE model suggests that SNAP participation is associated with a 2.1 percentage points (7.3%) increase in the probability of visiting an outpatient provider. We also find that SNAP participants in single-parent households are between 0.8 – 1.4 percentage points (16.3% – 28.5%) more likely to visit the ER, 4.0 percentage points (10.3%) more likely to visit a dentist, and 0.6 – 0.9 percentage points (28.5% – 45%) more likely to be admitted to an inpatient facility than eligible non-participants eligible non-participants. However, there is no difference in the likelihood of ER, dental visit, outpatient visits, or inpatient admissions across the SNAP benefit month for single-parent SNAP households.

1.4.2 Heterogeneity by Age

We re-estimate our fixed-effects models for two-parent households and single-parent households separately on the sub-samples of adults and children, and report the estimates in Table 1-5. Splitting the samples in this manner greatly reduces sample size, which results in a loss in statistical precision. Nonetheless, the results suggest there are differences in how resources are allocated between adults and children in SNAP households. In particular, the reduced likelihood of ER, outpatient and dental visits at the end of the SNAP benefit month is concentrated among adults. As before, these probabilities do not vary across the SNAP benefit month in single-parent households.

1.4.3 Misreporting of SNAP Participation in the MEPS

An important identification problem that arises in this study is nonrandom measurement error. This is because a large fraction of recipients fail to report their participation in SNAP, and as a result, the rate of SNAP participation in household surveys is lower than the actual participation rate (see, for example, Bollinger and David 1997; Meyer et al. 2015). Our findings may be biased if underreporting is more prevalent in single-parent households than in the two-parents, or vice versa. Researchers often estimate misreporting with linked administrative data (see, for example, Meyer and George 2011). We do not have access to such data. In order to examine the possibility that our results are confounded by measurement error, we estimate our models after an adjustment for the mis-classification of SNAP enrollment. We use variation in the state-level rates of SNAP participation to predict the likelihood of participation for SNAP-eligible households based on demographic information and socio-economic status. We then reclassify participation status for individuals who did not report participating in SNAP in the MEPS with the highest predicted participation levels until the rate of SNAP participation in the MEPS equals the national rate of SNAP participation in each year. We describe this procedure in greater detail in the supplementary appendix.

Table 1-6 contains estimates from our FE model on the dataset where some SNAP non-participants have been reclassified as participants using the method described above. Overall, the results are very similar to those from the original FE model, suggesting that measurement error bias does not change the qualitative conclusions from our models.

1.4.4 Single-Day Distribution States

Another limitation of our analysis using the MEPS is that we often do not know on which day a household received its SNAP benefits. Rather, we use the probability that a given week is the last week of the benefit month in our empirical model. To check the robustness of our results, we estimate models on the subsample of households in states that issue benefits on a single day of the month. In these states we know with certainty when households receive benefits[11]. When we restrict our dataset to states with a single day distribution schedule, we estimate a univariate probit model on the sample of SNAP recipients (rather than SNAP-eligible households) and report the results in Table 1-7. The findings from these models are qualitatively similar to those from models using data from the larger set of states. Two-parent SNAP households are less likely to visit outpatient provides, dentists or the ER at the end of the benefit month, but this is not the case in single-parent SNAP households.

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[11] In 2004, the following states issued benefits on a single day of the month: Alaska (1st), Nevada (1st), New-Hampshire (5th), Oklahoma (1st), Rhode Island (1st), South Dakota (10th), Vermont (1st), and Virginia (1st).

1.4.5 Effects by Insurance Coverage

In section 1, we discussed two possible mechanisms for the reduction in visits at the end of the benefit month. First, households may delay non-food purchases, such as medical care, at the end of the SNAP benefit month in order to make food purchases after SNAP benefits are exhausted. Alternatively, the change in visits may be driven indirectly, by a change in consumption patterns that may affect health care needs. For instance, if the benefit cycle reduces general activity, then that consumption itself may lead to a reduction in the use of medical services. This section distinguishes between the income and health channel by exploring which patients are responsible for the results above.

In addition to controlling for insurance coverage in our main models, we estimate our models on the sub-sample of beneficiaries of the Medicaid and State Children's Health Insurance Program (SCHIP) who are also eligible for SNAP (Table 1-8).[12] If liquidity constraints constitute direct barriers to care, they would be relatively less consequential for Medicaid beneficiaries who have comprehensive coverage and typically face the lowest copayments and deductibles. We do note that dental coverage is an optional benefit for adults under Medicaid (GAO, 2000a and 2000b). In 2016, 33 states plus the District of Columbia provided dental coverage to adults, 13 states only offered coverage for pain relief or emergency dental services, and 4 states did not provide any coverage (KFF 2016). As a result, in the case of dental visits, we estimate our models on the sub-sample of SNAP-eligible individuals with dental insurance coverage.

The estimates for the population with insurance coverage indicate that the likelihood of visiting the ER at the end of the SNAP benefit month are reduced by roughly the same amount as

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[12] SCHIP is a Medicaid expansion program that provides Medicaid benefits to children and, in some states, parents whose income is too high to quality for traditional Medicaid benefits. Hereafter, we refer to this group as the Medicaid population.

in the full SNAP population in two-parent households. However, in contrast to other SNAP recipients, the population with insurance coverage are not less likely to visit outpatient providers or dentists at the end of the benefit month. While the income channel is not a plausible explanation for the end of the month reduction in ER visits, the changes in dental visits and outpatient visits are more consistent with the income channel as opposed to the health channel.

1.4.6 Effects by ER Visit Category

In order to investigate whether the health channel contributes to the end of the month changes in medical visits, we compare the types of visits that drive our findings for the ER. We classify ER visits using each ER visit's category. The first type isolates visits that are related to accidents or injuries. All other visits are considered as non-injury visits.[13] Our fixed-effects estimates suggest that the reduction in ER visits at the end of the benefit month is driven by injury-related category in which the timing of care is not discretionary (Table 1-9). Taken as a whole, Tables 1-8 and 1-9 are consistent with the hypothesis that the benefit cycle changes households' consumption, which in turn affects health and ER utilization.

1.5 Evidence from Food Expenditures Data

In order to determine whether the changes in medical care utilization that we identify in the previous section are likely to be a result of the SNAP benefit cycle, we investigate how cash and SNAP spending on food change over the course of the month using data from the FoodAPS.

The National Food Acquisition and Purchase Survey (FoodAPS) is a nationally representative survey containing the daily food acquisitions of households over a seven day period between April 2012 and January 2013. Respondents record food acquisitions in two diaries: a food

[13] These include Diagnosis or treatment; Psychotherapy; Follow-up or Post-Operative; Immunization or Shots; Pregnancy-related; or other.

at home (FAH) diary and food away from home (FAFH) diary. In both diaries, households were asked to record the person that acquired the food, as well as the payment type, which indicates whether SNAP benefits or "out-of-pocket" income was used to make the transaction. The initial FoodAPS interview took place prior to the start of the seven-day diary, in most cases the day before the first diary day. During this interview, households were asked the date they last received their SNAP benefits. Using this date and the diary dates, we calculated the number of days since receiving benefits. We aggregate food purchasing events separately for mothers and fathers by weeks of the benefit month and create week of the benefit month indicators corresponding to days 0–5, 6–13, 14–22, and 23–30 (see the supplementary appendix for details on the FoodAPS data and sample construction).[14]

We separately analyze SNAP and cash purchasing patterns for two-parent and single-parent SNAP participating households using the following linear model:

$$\ln(f_{iw}) = \sum_{i=2}^{4} \beta_w D_{iw} + X_i' \psi + \varepsilon_{iw} \qquad (1\text{-}7)$$

where f_{iw} is parent i's food purchase in benefit week w, D_{iw} are binary indicators for the week of the benefit month, and X_i is a vector of covariates including individual and family-level demographic variables, and environmental measures of access to restaurants and grocery markets. We estimate equation (1-7) separately by the type of income used to purchase food (i.e., SNAP benefits vs. cash income).[15,16]

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[14] We test the sensitivity of our estimates to different definitions of benefit weeks, and come to similar conclusions. In particular, we also estimate our models with the following indicators: 0–5, 6–11, 12–20, and 21–30; 0–6, 7–13, 14–20, and 21–30. Likewise, we specify a set of six indicators corresponding to days 0, 1–3, 4–6, 7–14, 15–21, and 22–30.

[15] We obtain identical results when we estimate the model using a second set of dependent variables, constructed by dividing the food expenditures by household size.

[16] Because some households do not purchase food in certain weeks, the dependent variable in equation (7) is sometimes zero. In order to account for the decision to make a purchase food during a given week, we use a two-part

Table 1-10 contains these estimates for mothers and fathers in two-parent households as well as female headed single-parent households with children. First, we find that the propensity of mothers to spend SNAP benefits on food drops significantly from the week of benefit receipt to the last week of the benefit month. This is true for both two-parent and single-parent households, but two-parent households exhibit a more pronounced cyclical pattern in their SNAP spending. While two-parent households reduce their purchases of food with SNAP benefits by 15.9% in the third week of the benefit month and by 53.7% in the fourth week of the benefit month, the reduction in SNAP benefit spending by mothers in single-parent households is only statistically significant in the last week of the benefit month and equal to 16.8%. Although the point estimates suggest a decline in SNAP benefit spending by fathers in two-parent households, none of the effects are precisely estimated. Importantly, both mothers and fathers in two-parent households increase their cash spending on food purchases towards the end of the benefit month, but single-parent households spend cash income smoothly over the month. These results provide evidence that the SNAP benefit cycle is the reason for the change in the use of medical services.

1.6 Discussion and Conclusion

We find that SNAP households are less likely to visit outpatient providers, dentists, and the ER at the end of the benefit month. However, only two-parent households experience cycles in medical care utilization related to SNAP payments. These results are consistent with the hypothesis that mothers in two-parent households will overspend SNAP benefits at the beginning of the month. This strategic behavior persuades fathers to contribute more cash income to buy food at the end of the month. We find empirical evidence of this behavior when we analyze food purchases across

model (Jones 2000). The first part of the two-part model estimates the probability of having positive food expenditure in a given week (the extensive margin), while the second part estimates the level of food expenditure conditional on having positive spending (the intensive margin).

the benefit month. Holding the household income constant, higher cash income spending on food at the end of the month in two-parent households compels them to spend less on non-food goods, such as medical care, at the end of the benefit month. This is presumably so they can reallocate cash income to food at the end of the benefit month.

In order for this behavior to be rational, it must be the case that cash income is spent on medical visits when care is received. Cost-sharing for low-income individuals is often in the form of copayments, which have to be paid at the time of service. Based on data from the 2010 MEPS, we find that approximately 27% of outpatient visits by SNAP participants were subject to copayment requirements.[17] This rate is higher (37%) among SNAP recipients who were not enrolled in Medicaid. The average copayments for these two groups are $37 and $54, respectively, and the median level of copayments is $20 and $25, respectively. While these copayments are significantly less than the total cost of care, a growing body of research has found that even relatively small copayment levels are associated with reduced utilization of services (Newhouse and Rand Corporation Insurance Experiment Group 1993; Chandra et al. 2010, 2014). In addition, there are other costs of visiting medical providers, such as transportation costs, and the need to reduce paid work time, which may be larger than copayment amounts. For example, low-income workers are less likely to have paid sick leave than higher income workers (Bureau of Labor Statistics 2017).

One would expect such an effect to be relevant for medical visits in which the timing of care is discretionary. The fact that individuals in two-parent SNAP households also reduce their ER

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[17] In MEPS, we do not directly observe whether out-of-pocket payments represent copayments or coinsurance, but we are able to identify whether an out-of-pocket payment is a copayment in about 90 percentage of cases by analyzing the payment values. For example, if we observe a nominal amount or a flat fee across the year for different visits we classify these payments as a copayments. If the payment is a conventional percentage of the total cost of the visit cost, we classify it as a coinsurance amount.

visits at the end of the benefit month suggests that the income channel is not the only mechanism for our findings. Moreover, we find evidence that the reduction in ER visits is driven by emergent injury-related visits and does not differ by insurance status. We thus conclude that the SNAP benefit cycle affects ER visits indirectly by reducing certain types of consumption which affect the need for emergency care.

Finally, we find that that the reduction in medical care utilization in two-parent SNAP households at the end of the benefit month is particular to adults. This may reflect a desire by SNAP households to protect children from reductions in consumption at the end of the month. A second possible interpretation is that fewer children are subject to copayment requirements in comparison to adults. Using the 2010 MEPS, we find that 35% of outpatient visits by adults with any type of insurance coverage were subject to upfront copayment requirements, only 12% of visits for children had copayments. In addition, medical visits by children are exempt from Medicaid copayment requirements, although states can set copayment levels to nominal amounts in separate CHIP programs.

Our study has some limitations that must be recognized. First, our bivariate model uses a state-level variable (whether the state operates SNAP call centers) for identification. Although we have included other state-level controls in our model to reduce the potential for policy endogeneity, the validity of the exclusion restriction in the bivariate probit is ultimately untestable. However, we are reassured that estimates from the FE model, which does not require an exclusion restriction for identification, support our findings from the bivariate probit. Finally, prior studies demonstrate that SNAP participation is often under-reported in household surveys. We have conducted a sensitivity analysis to determine whether our results are sensitive to such measurement error. The results of this test suggest that our qualitative findings are not the result of measurement error, but measurement error may still affect the magnitudes of our estimated marginal effects.

Despite these limitations, we believe that our study makes an important contribution to the literature on the SNAP benefit cycle, and has implications for public policy. First, enrollment in SNAP leads to overall greater use of medical services, which suggests that households entering SNAP reallocate some of the cash income previously spent on food to medical care. However, the pattern of spending within the benefit month differs for two-parent and single-parent households. In particular, two-parent households are more likely to delay seeking medical care at the end of the benefit month than single-parent SNAP households or non-SNAP households. This is concerning because the delay of needed medical care can have serious negative health consequences (Begley et al. 1994; Rubin and Mendelson 1995; Zweifel and Manning 2000; Hsu et al. 2006).

One potential solution is to institute mechanisms designed to smooth consumption over the SNAP benefit month, such as the more frequent disbursement of benefit payments, in two-parent households. For example, several states pay out TANF benefits twice each month, and one study finds that this is associated with less domestic violence around the time of TANF benefit receipt in those states (Hsu 2017). Our results suggest that such policies would have little effect in single-parent households, but could improve outcomes in two-parent households. However, the benefits of the more frequent distribution of small SNAP payments would need to be weighed against potential costs, or other mechanisms would need to be put in place to reduce those costs. One concern is that such payments could increase the cost of grocery shopping by making it more difficult for SNAP households to buy in bulk at the beginning of the month. They could also necessitate more shopping trips, which would increase transportation costs.

Finally, our study also adds to the literature on the potential negative consequences of cost sharing for medical services in low-income households. The SNAP benefit cycle represents one type of liquidity constraint that household can face at a certain point in time. Prior research has shown that with individuals face such constraints they may delay needed medical care (Weissman et al. 1991; Wisk and Whitney 2012; KFF 2005). Future studies may wish to consider whether reductions in copayments at the end of the SNAP benefit month might counteract the reduction in medical care utilization associated with the SNAP benefit cycle.

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Table 1-1. Descriptive statistic from the MEPS by SNAP participation status and household composition

	Two-parent				Single-parent			
	SNAP		Non-SNAP		SNAP		Non-SNAP	
	mean	S.D.	mean	S.D.	mean	S.D.	mean	S.D.
Age 0-6	0.277	0.447	0.232	0.422	0.277	0.447	0.223	0.416
Age 7-17	0.266	0.442	0.278	0.448	0.360	0.480	0.357	0.479
Age 18-30	0.198	0.398	0.169	0.374	0.175	0.380	0.173	0.378
Age 31-45	0.227	0.419	0.271	0.444	0.162	0.368	0.206	0.405
Age 46-60	0.027	0.161	0.041	0.199	0.024	0.152	0.039	0.192
Age 61-75	0.004	0.064	0.008	0.087	0.002	0.042	0.001	0.039
Age over 76	0.001	0.036	0.002	0.044	0.001	0.025	0.001	0.029
White	0.423	0.494	0.409	0.492	0.295	0.456	0.356	0.479
Hispanic	0.375	0.484	0.442	0.497	0.257	0.437	0.273	0.446
Black	0.136	0.343	0.087	0.281	0.427	0.495	0.333	0.471
Other race	0.065	0.246	0.062	0.241	0.020	0.142	0.038	0.192
Female	0.490	0.500	0.487	0.500	0.645	0.478	0.629	0.483
No. HH members aged 0-5	1.266	1.002	1.022	0.960	0.891	0.944	0.644	0.789
Midwest census region	0.160	0.367	0.129	0.335	0.202	0.401	0.167	0.373
South census region	0.423	0.494	0.379	0.485	0.381	0.486	0.404	0.491
West census region	0.288	0.453	0.343	0.475	0.204	0.403	0.223	0.416
Urban residence	0.791	0.406	0.838	0.368	0.842	0.365	0.849	0.358
High school degree	0.148	0.355	0.153	0.360	0.128	0.334	0.156	0.363
At least some college	0.076	0.265	0.126	0.331	0.076	0.264	0.125	0.331
Ln(HH income per sqrt(HH size))	8.864	1.306	9.134	1.436	8.182	2.164	8.345	2.491
Uninsured	0.187	0.390	0.267	0.442	0.078	0.268	0.205	0.404
Medicaid	0.713	0.452	0.375	0.484	0.851	0.356	0.516	0.500
Medicare	0.003	0.054	0.005	0.069	0.002	0.041	0.002	0.041

Table 1-1. Continued

	Two-parent				Single-parent			
	SNAP		Non-SNAP		SNAP		Non-SNAP	
	Mean	S.D	Mean	S.D	Mean	S.D	Mean	S.D
Private insurance	0.097	0.296	0.353	0.478	0.070	0.255	0.277	0.447
Good mental health, all rounds	0.354	0.478	0.351	0.477	0.317	0.465	0.321	0.467
Missing mental health status	0.012	0.107	0.011	0.104	0.013	0.112	0.009	0.096
Excellent mental health, all rounds	0.257	0.437	0.295	0.456	0.270	0.444	0.283	0.451
Poor/fair mental health	0.106	0.308	0.060	0.237	0.128	0.334	0.104	0.306
Excellent mental health, some	0.528	0.499	0.577	0.494	0.548	0.498	0.571	0.495
Poor health, all rounds	0.062	0.241	0.041	0.198	0.062	0.241	0.047	0.211
Poor health, some rounds	0.174	0.379	0.122	0.328	0.168	0.374	0.143	0.350
Excellent health, some rounds	0.432	0.495	0.482	0.500	0.470	0.499	0.478	0.500
Excellent health, all rounds	0.186	0.389	0.215	0.411	0.215	0.411	0.209	0.407
Missing health status	0.012	0.107	0.011	0.104	0.012	0.109	0.009	0.096
Good health, all rounds	0.386	0.487	0.387	0.487	0.357	0.479	0.374	0.484
Disability status	0.119	0.323	0.080	0.272	0.124	0.329	0.103	0.303
State-level poverty rate	14.185	2.781	13.637	2.827	13.754	2.938	13.362	2.972
State-level unemp. rate	6.744	2.278	6.242	2.229	6.449	2.200	6.029	2.122
State-level per capita income (thousands)	35.679	7.575	35.542	7.353	35.641	7.627	34.762	7.528
State-level % with BA degree	26.363	4.078	26.881	4.010	26.375	4.264	26.573	4.340
Pr.(calendar week is the last benefit week); wgt	0.250	0.293	0.250	0.298	0.250	0.298	0.250	0.299
Number of individuals	16,831		27,749		12,023		19,587	

Note: Means are weighted to be nationally representative.

Table 1-2. End of the benefit month effect on medical care utilization among SNAP participants, by household composition

	Two-parent				Single-parent			
	Outpatient	Emergency room	Dental	Inpatient	Outpatient	Emergency room	Dental	Inpatient
Any visit								
wgt	-0.031***	0.001	-0.016*	0.000	-0.028***	-0.006	0.000	-0.005
	(0.011)	(0.005)	(0.009)	(0.003)	(0.009)	(0.005)	(0.008)	(0.004)
Average outcome	0.294	0.046	0.068	0.023	0.307	0.056	0.076	0.022
Observations	90,716	90,716	90,716	90,716	89,008	89,008	89,008	89,008
Log of number of visits								
wgt	-0.041***	-0.001	-0.013**	-0.001	-0.030***	-0.005	-0.002	-0.004
	(0.012)	(0.004)	(0.006)	(0.002)	(0.010)	(0.004)	(0.006)	(0.003)
Observations	90,716	90,716	90,716	90,716	89,008	89,008	89,008	89,008

Notes: All regressions include time-varying household demographic controls and state characteristics. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***$p < 0.01$. **$p < 0.05$. *$p < 0.1$.

Table 1-3. End of the benefit month effect on medical care utilization among SNAP participants

	Outpatient	Emergency room	Dental	Inpatient
Any visit				
wgt × two-parent	-0.001	-0.011***	-0.007	0.002
	(0.012)	(0.004)	(0.006)	(0.002)
	0.300	0.051	0.072	0.023
Observations	179,724	179,724	179,724	179,724
Log of number of visits				
wgt × two-parent	-0.008	-0.009***	-0.006*	0.001
	(0.015)	(0.003)	(0.004)	(0.002)
Observations	179,724	179,724	179,724	179,724

Notes: All regressions include time-varying household demographic controls and state characteristics. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 1-4. Effects of SNAP participation and the SNAP benefit cycle on medical care utilization, by household composition

	Outpatient		Emergency room		Dental		Inpatient	
	FE	IV	FE	IV	FE	IV	FE	IV
Two-parent								
SNAP	0.022**	0.040***	0.011***	0.007**	0.002	0.007*	0.004*	0.003
	(0.009)	(0.014)	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)
SNAP×wgt	-0.015*	-0.006**	-0.016***	-0.004**	-0.008	-0.011*	-0.001	0.001
	(0.009)	(0.003)	(0.005)	(0.002)	(0.007)	(0.006)	(0.003)	(0.001)
Average outcome	0.270	0.276	0.035	0.035	0.069	0.069	0.016	0.016
Observations	158,840	178,320	158,840	178,320	158,840	178,320	158,840	178,320
Single-parent								
SNAP	0.021**	-0.005	0.008**	0.014***	0.001	0.008**	0.006**	0.009**
	(0.010)	(0.153)	(0.004)	(0.004)	(0.009)	(0.004)	(0.003)	(0.004)
SNAP×wgt	-0.008	-0.001	0.004	0.001	0.001	0.001	-0.003	0.000
	(0.011)	(0.004)	(0.006)	(0.002)	(0.010)	(0.003)	(0.003)	(0.002)
Average outcome	0.287	0.287	0.049	0.049	0.077	0.077	0.018	0.018
Observations	103,056	126,440	103,056	126,440	103,056	126,440	103,056	126,440

Notes: All regressions include time-varying household demographic controls and state characteristics. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***$p < 0.01$. **$p < 0.05$. *$p < 0.1$.

Table 1-5. Fixed-effect estimates of SNAP participation and the SNAP benefit cycle on medical care utilization, separately for adults and children

	Adults				Children			
	Outpatient	Emergency room	Dental	Inpatient	Outpatient	Emergency room	Dental	Inpatient
Two-parent								
SNAP	0.019*	0.013**	-0.001	0.006	0.024**	0.009*	0.006	0.002
	(0.012)	(0.005)	(0.005)	(0.004)	(0.012)	(0.005)	(0.007)	(0.002)
SNAP×wgt	-0.018*	-0.027***	-0.013*	0.002	-0.007	-0.007	0.015	-0.003
	(0.011)	(0.007)	(0.007)	(0.006)	(0.014)	(0.006)	(0.010)	(0.003)
Average outcome	0.246	0.039	0.064	0.022	0.294	0.035	0.071	0.019
Observations	71,088	71,088	71,088	71,088	87,752	87,752	87,752	87,752
Single-parent								
SNAP	0.033**	0.002	0.010	0.015**	0.014	0.011**	-0.001	0.001
	(0.015)	(0.008)	(0.009)	(0.006)	(0.012)	(0.005)	(0.009)	(0.002)
SNAP×wgt	-0.009	0.011	-0.006	-0.011	-0.007	-0.001	0.003	0.001
	(0.018)	(0.011)	(0.012)	(0.008)	(0.013)	(0.007)	(0.012)	(0.003)
Average outcome	0.286	0.050	0.073	0.022	0.304	0.048	0.076	0.020
Observations	35,116	35,116	35,116	35,116	67,940	67,940	67,940	67,940

Notes: All regressions include time-varying household demographic controls and state characteristics. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 1-6. Fixed-effects estimates after adjustment for misclassification of SNAP participation

	Two-parent				Single-parent			
	Outpatient	Emergency Room	Dental	Inpatient	Outpatient	Emergency Room	Dental	Inpatient
SNAP	0.020**	0.011***	0.002	0.001	0.018**	0.010***	0.002	0.011***
	(0.010)	(0.004)	(0.004)	(0.002)	(0.009)	(0.005)	(0.008)	(0.003)
SNAP×wgt	-0.016*	-0.012**	-0.010*	0.001	-0.001	-0.001	-0.002	0.001
	(0.010)	(0.006)	(0.006)	(0.001)	(0.009)	(0.005)	(0.008)	(0.002)
Average outcome	0.270	0.037	0.069	0.020	0.293	0.049	0.075	0.021
Observations	158,840	158,840	158,840	158,840	103,056	103,056	103,056	103,056

Notes: All regressions include time-varying household demographic controls and state characteristics. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***$p < 0.01$. **$p < 0.05$. *$p < 0.1$.

Table 1-7. Marginal effects of being in the last week of benefit month among SNAP participants, states with one distribution day only

	Two-parent			Single-parent		
	Outpatient	Emergency room	Dental	Outpatient	Emergency room	Dental
Full sample						
End of month	-0.045*	-0.030***	-0.018*	-0.028	-0.017	-0.005
	(0.030)	(0.010)	(0.010)	(0.020)	(0.014)	(0.011)
Observations	3496	3496	3496	3724	3724	3724
Medicaid						
End of month	-0.032	-0.027**	-0.021	-0.025	-0.019	0.002
	(0.021)	(0.009)	(0.012)	(0.023)	(0.012)	(0.013)
Observations	2160	2160	2160	2912	2912	2912
Non-Medicaid						
End of month	-0.053*	-0.034**	-0.016	-0.036	-0.011	-0.006
	(0.032)	(0.014)	(0.010)	(0.034)	(0.028)	(0.012)
Observations	1336	1336	1336	812	812	812

Note: Standard errors in parentheses are clustered at the state-level. Significance level: ***p < 0.01, **p < 0.05, *p < 0.1.

Table 1-8. Effects of SNAP participation and the SNAP benefit cycle on medical care utilization, sample of Medicaid beneficiaries

	Outpatient		Emergency room		Dental		Inpatient	
	FE	IV	FE	IV	FE	IV	FE	IV
Two-parent								
SNAP	0.030**	0.094***	0.009*	0.020***	0.019	0.002	0.005*	0.010***
	(0.012)	(0.033)	(0.005)	(0.004)	(0.018)	(0.004)	(0.003)	(0.002)
SNAP×wgt	0.005	-0.002	-0.019***	-0.006*	0.001	-0.002	-0.007	-0.001
	(0.013)	(0.005)	(0.007)	(0.003)	(0.009)	(0.010)	(0.005)	(0.002)
Average outcome	0.320	0.320	0.044	0.044	0.075	0.075	0.025	0.025
Observations	82,412	95,160	82,412	95,160	30,886	35,664	82,412	95,160
Single-parent								
SNAP	0.019*	0.029	0.006	0.028***	0.003	0.000	0.005	0.014***
	(0.012)	(0.152)	(0.005)	(0.010)	(0.010)	(0.008)	(0.003)	(0.003)
SNAP×wgt	0.016	0.004	0.006	0.002	-0.001	0.001	(0.000)	0.001
	(0.013)	(0.007)	(0.008)	(0.004)	(0.010)	(0.012)	(0.005)	(0.003)
Average outcome	0.314	0.313	0.053	0.052	0.078	0.079	0.023	0.023
Observations	77,376	94,116	77,376	94,116	14,554	17,702	77,376	94,116

Notes: All regressions include time-varying household demographic controls and state characteristics. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 1-9. Fixed-effects estimates on ER visits, by visit category

	Two-parent		Single-parent	
	Injury	Non-injury	Injury	Non-injury
Full-sample				
SNAP	0.002	-0.018**	0.001	-0.020*
	(0.002)	(0.009)	(0.002)	(0.010)
SNAP×wgt	-0.005**	0.009	0.001	0.004
	(0.002)	(0.009)	(0.003)	(0.011)
Average outcome	0.028	0.020	0.042	0.028
Observations	158,840	158,840	103,056	103,056
Adults				
SNAP	0.002**	-0.015	0.003	-0.034**
	(0.001)	(0.011)	(0.003)	(0.016)
SNAP×wgt	-0.006**	0.009	0.004	0.008
	(0.003)	(0.011)	(0.006)	(0.019)
Observations	71088	71088	35,116	35,116
Children				
SNAP	0.002	-0.021*	0.001	-0.009
	(0.002)	(0.011)	(0.002)	(0.012)
SNAP×wgt	-0.004	0.006	-0.001	0.000
	(0.004)	(0.014)	(0.003)	(0.013)
Observations	87,752	87,752	67,940	67,940

Notes: All regressions include time-varying household demographic controls and state characteristics. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 1-10. Estimates of food spending patterns over the SNAP benefit month by payment type

		Single-parent Mother	Two-parent Father	Two-parent Mother
Total Spending	week 2	-0.044	0.097	-0.061
		(0.262)	(0.265)	(0.398)
	week 3	-0.120	0.052	-0.608
		(0.365)	(0.291)	(0.394)
	week 4	-0.374	0.386	-0.357
		(0.286)	(0.305)	(0.395)
SNAP spending	week 2	-0.273	-0.194	-0.614
		(0.247)	(0.181)	(0.409)
	week 3	-0.404	-0.209	-0.159 ***
		(0.299)	(0.210)	(0.067)
	week 4	-0.168**	-0.174	-0.537***
		(0.085)	(0.250)	(0.242)
Non –SNAP spending	week 2	0.203	0.373	0.532
		(0.163)	(0.228)	(0.358)
	week 3	0.216	0.405	0.110
		(0.265)	(0.252)	(0.356)
	week 4	0.024	0.675***	0.691*
		(0.230)	(0.228)	(0.355)

Note: Standard errors in parentheses are clustered at the household-level.
Significance level: ***p < 0.01, **p < 0.05, *p < 0.1.

Appendix A. SNAP Eligibility

To be eligible for the SNAP, a household has to pass gross income, net income, and asset tests. Since our data does not contain information on household assets, we simulate the gross income, and net income tests to determine households' eligibility status (USDA, 2016a,c).18 However, only 3% of households who apply for SNAP benefits fail the asset test (Wheaton et al. 2016).

The gross monthly income limits and net monthly income limits are set at 130 percent and 100 percent of the poverty level for the household size, respectively (USDA, 2016b).[19] In accordance with eligibility rules, we exempt households from the gross income test if they are an SSI recipient due to a disability or any household member is 60 years of age or older. To pass the net income test, a number of deductions are allowed. Households are able to deduct dependent care expenses and shelter costs (USDA, 2016a). The MEPS does not contain information on housing costs or child care payments, so we impute this information using state-level average market rate charges for child care at child care centers from the National Women's Law Center (Schulman and Blank 2014) and average monthly shelter expenses from Center on Budget and Policy Priorities (Rosenbaum et al. 2002). Able bodied adults without dependents (ABAWD) are required to work or participate in a work program for at least 20 hours per week in order to receive SNAP benefits for more than 3 months in a 36-month period. States may request to waive the ABAWD time limit in areas with an unemployment rate above 10 percent or a lack of sufficient

[18] When estimating panel data models, we define the eligibility status based on the first-year observation of individuals.
[19] Monthly income eligibility standards for 1996-2003 were obtained from USDA/FNS.

job opportunities. We do not have data on ABAWD waivers, so we exclude ABAWDs who work less than 20 hours per week from our sample.

Appendix B. Measurement Error Adjusted SNAP Measure

In this section, we explore the possibility that our results are confounded by measurement error in self-reported SNAP participation. To do this we use data on state-level rates of SNAP participation from SNAP Data System to construct an error-adjusted measure of SNAP participation.

First, we estimate a state-level regression of the state SNAP participation rate on state-level measures of demographic composition and socio-economic status, the unemployment rate, and the poverty rate from the U.S. Census Bureau, as well as state-level SNAP policies determining eligibility criteria, recertification and reporting requirements, benefit issuance methods, availability of online applications, use of biometric technology (such as fingerprinting), and coordination with other low-income assistance programs from the SNAP Policy Database.[20]

We subsequently use this model to predict SNAP participation for individuals in the MEPS who are eligible for SNAP, but do not report participating in the program. In order to make this prediction, we use the individual's demographic information, but the state-level information from the individual's state of residence for the other state-level measures. We reclassify eligible individuals who did not report participating in SNAP in the MEPS with the highest predicted participation levels as SNAP participants. We do this until the rate of SNAP participation in the MEPS equals the national rate of SNAP participation in each year.[21]

[20] State-level measures of demographic composition and socioeconomic status include age categories (0-5, 6-13, 14-17, 18-24, 25-44, 45-64, 65 and older), educational attainment (college degree or higher, high school diploma, below high school), race and ethnicity (white, black, Hispanic, and other races), and per capita income.
[21] Time-series data on individual level rate of SNAP participation available at: https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap

Appendix C. The National Food Acquisition and Purchase Survey

The National Food Acquisition and Purchase Survey (FoodAPS) is a nationally representative survey containing the daily food acquisitions of households over a seven day period between April 2012 and January 2013. Respondents record food acquisitions in two diaries: a food at home (FAH) diary and food away from home (FAFH) diary. In general, FAH includes food obtained from grocery stores, farmers' markets, food pantries, and home gardens, while FAFH includes food purchased at sit-down restaurants, fast-food establishments and take-away restaurants. For the FAH diary, households were asked to scan UP codes, either on the food package or provided in the diary for loose/bulk items, and to write down the total expenditure for that shopping trip. Similarly, households provided the total expenditure for every FAFH purchase, and were asked to write down each item purchased. In both diaries, households were also asked to provide the receipt if one was given. Importantly, households record the person that acquired the food, as well as the payment type, which indicates whether SNAP benefits or "out-of-pocket" income was used to make the transaction. In all our analyses, we use the sum of the total expenditures for each event for FAH and FAFH by diary day. Of the 4,826 households surveyed, 1,581 households had at least one member currently enrolled in SNAP.

Because we are interested in the spending patterns of SNAP households, we estimate our models on households with at least one member currently enrolled in SNAP. We further restrict our sample to households with at least one child under age 18 and exclude households with a parent younger than age 20 or older than age 50, as our analyses with the MEPS data. Our final sample consists of 535 two-parent, and 908 single-parent SNAP households.[22]

[22] Because there are 84 male-headed households with children, and only 20 households have nonzero weekly food purchase, we subset to female-headed households when analyzing single-parent households.

The initial FoodAPS interview took place prior to the start of the seven-day diary, in most cases the day before the first diary day. During this interview, households were asked the date they last received their SNAP benefits. Using this date and the diary dates, we calculated the number of days since receiving benefits. Day zero indicates the day of benefit arrival and day 30 is the last possible day of the cycle. We aggregate food purchasing events separately for mothers and fathers by weeks of the benefit month and create week of the benefit month indicators corresponding to days 0–5, 6–13, 14–22, and 23–30.[23]

The control variables we include from FoodAPS are age and its square, race and ethnicity (Hispanic, black, and other race with white as the omitted category), region (South, Midwest, and West with Northeast omitted), urban residence, education (high school diploma, any college, with less than a high school degree omitted), number of children in the household, the log of total family income normalized by the square root of household size, and whether the household pays monthly rent for their residential unit. We also include a number of controls to capture the local food environment, such as the number of grocery stores, fast food restaurants, other restaurants within a mile, and distance to nearest SNAP-authorized Walmart. Table 1-E-1 contains descriptive statistics of all the variables we use in our FoodAPS analysis.

[23] We test the sensitivity of our estimates to different definitions of benefit weeks and come to similar conclusions. In particular, we also estimate our models with the following indicators: 0–5, 6–11, 12–20, and 21–30; 0–6, 7–13, 14–20, and 21–30. Likewise, we specify a set of six indicators corresponding to days 0, 1–3, 4–6, 7–14, 15–21, and 22–30.

Table 1-C-1. Descriptive Statistics from the FoodAPS by SNAP Participation Status and Household Composition

	Single-parent Mother			Two-parent Father			Two-parent Mother		
	Observation	Mean	S.D.	Observation	Mean	S.D.	Observation	Mean	S.D.
Any food purchase	908	0.333	0.472	535	0.492	0.500	535	0.758	0.429
Any purchase with SNAP	908	0.189	0.391	535	0.086	0.281	535	0.324	0.468
Any purchase with cash	908	0.298	0.457	535	0.465	0.499	535	0.694	0.461
Total food purchase	310	91.259	108.390	269	45.604	80.827	411	102.495	121.963
Food purchase with SNAP	174	81.776	103.288	49	110.367	141.076	186	99.627	117.578
Food purchase with cash	279	50.418	76.002	253	27.795	37.676	372	65.315	79.594
No. children 0-5	908	0.688	0.819	535	0.872	0.949	535	0.872	0.949
No. children 6-17	908	1.457	1.184	535	1.527	1.270	535	1.527	1.270
HH size	908	3.144	1.081	535	5.037	1.709	535	5.037	1.709
Rural	908	0.163	0.370	535	0.315	0.465	535	0.315	0.465
Midwest census region	908	0.329	0.470	535	0.241	0.428	535	0.241	0.428
South census region	908	0.430	0.495	535	0.379	0.486	535	0.379	0.486
West census region	908	0.166	0.372	535	0.277	0.448	535	0.277	0.448
Age	908	35.686	10.393	535	39.259	9.853	535	38.050	9.994
white	908	0.277	0.448	535	0.417	0.494	535	0.408	0.492
Black	908	0.473	0.500	535	0.116	0.321	535	0.135	0.342
Hispanic	908	0.215	0.411	535	0.395	0.489	535	0.410	0.492
Other race	908	0.035	0.183	535	0.072	0.258	535	0.047	0.212
Missing education	908	0.000	0.000	535	0.008	0.092	535	0.001	0.038
High school diploma	908	0.310	0.463	535	0.337	0.473	535	0.276	0.448
Some college	908	0.354	0.478	535	0.166	0.372	535	0.277	0.448
BA degree or higher	908	0.036	0.186	535	0.090	0.286	535	0.110	0.314
log of family income	908	6.156	1.475	535	6.873	1.546	535	6.873	1.546
Pay rent	908	0.866	0.341	535	0.601	0.490	535	0.601	0.490
No. grocery stores in a mile	908	1.333	2.840	535	1.790	5.355	535	1.856	5.524
Distance to nearest Walmart	908	4.128	4.885	535	4.177	4.187	535	4.141	4.159
No. fast food in a mile	908	1.503	0.948	535	1.410	1.102	535	1.470	1.092

Note: Means are weighted to be nationally representative.

CHAPTER 2

How the Supplemental Nutrition Assistance Program Affects Labor Force Decisions

2.1 Introduction

The Supplemental Nutrition Assistance Program (SNAP, formerly known as the Food Stamp Program) is the largest public assistance program in the United States. While fewer than 10 million low-income individuals participated in the program in the early 1970's, by 2016, more than 44 million individuals were enrolled in SNAP at a total cost of approximately $70 billion (USDA 2018). The program plays a leading role in preventing malnutrition by supplementing food budgets and freeing up income for nonfood expenditures. However, as is well-known, adverse incentives to work generated by welfare programs may partially offset the income enhancing goals of SNAP. As a result, it may cost more than $1 in income support payments to increase a low-income family's available cash and near-cash resources by $1. Given the increasing number of SNAP participants, any work disincentives caused by SNAP could result in large welfare losses.

Safety net programs are designed to ensure a basic level of consumption in low-income families. Consequently, programs such as SNAP feature a guaranteed benefit level if the family has no income. As earnings or income increase, benefits are reduced at the legislated *benefit reduction rate*. Because of the link between labor income and benefit receipt from government assistance programs, standard economic theory suggests that cash and in-kind transfer programs will reduce labor supply (Hoynes and Schanzenbach 2012). The guarantee produces an income effect, and the benefit reduction rate reduces the net wage leading to an income, and substitution effect. For example, a low-income worker may stop working after enrollment in a welfare program.

Likewise, a low-paid worker may have little incentive to work more hours or seek higher wages, because the extra earnings from doing so may be partially offset by a benefit reduction.

A large number of studies examine the impact of transfer programs on labor supply (Danziger, Haveman and Plotnick 1981; Hoynes 1997; Moffitt 1992; Moffitt 2002). For example, Moffitt (1983) finds that Aid to Families with Dependent Children (AFDC) reduces labor supply among program participants. In contrast, Meyer (2002) finds that the Earned Income Tax Credit (EITC), which subsidizes work for low income families encourages work for single mothers on the extensive, but not on the intensive margin. However, Eissa and Hoynes (2004) find evidence of extensive margin work disincentives for married couples. Likewise, Social Security Disability Insurance has generally been found to reduce employment among older men (Bound 1989; Parsons 1991; Gruber and Kubik 1997; Chen and van der Klaauw 2008; Maestas, Mullen and Strand 2013; and French and Song 2014). Given differences across transfer programs in both size and income testing, it is not surprising that the existing literature portrays a mixed picture of the impact of income transfer programs on labor supply.

SNAP participation may impact employment outcomes through three possible channels. First, a direct effect occurs if SNAP generates work disincentives as predicted in the theory of welfare programs. Although SNAP benefits have the structure of a traditional income support program, the reduction rate in SNAP is substantially lower than is specified in other safety net programs (Hoynes and Schanzenbach 2015).[24] SNAP recipients are allotted a benefit amount equal to the difference between the federally defined maximum allotment for a given family size and the

[24] The benefit reduction rate in the AFDC (Aid to Families with Dependent Children) program was 100% by 1967. It was reduced to 67% in 1967, then increased again to 100% in 1981. Since the federal welfare reform in 1996, and the conversion to TANF, there has been substantial variation across states in the program's benefit reduction rate. In contrast, the benefit reduction rate in SNAP is 30% (Ziliak 2016).

amount that the family is deemed to be able to afford to pay for food on its own according to the benefit formula (essentially 30 percent of cash income, minus some deductions). For example, in 2015, a two-member household received a maximum SNAP benefit of $357 per month, and a four-member household could receive at most $649 per month. Based on the SNAP benefit calculation formula, SNAP households are financially better off if they are able to secure employment or increase their earnings. Second, SNAP participation may also increase employment indirectly by allowing recipients to pay for job-related expenses, like childcare or transportation. Finally, if SNAP participation leads to increases in the quality or quantity of food, labor supply and productivity could improve through better nutrition.

SNAP rules make labor supply decisions more complicated than is typically assumed in static labor supply models. Work has been an increasing focus of policy reforms in the United States, culminating with a number of major policy changes in the 1990s intended to increase employment and by welfare recipients. In particular, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 imposed work requirements on Able Bodied Adults Without Dependents (ABAWDs) who receive SNAP benefits.[25] The legislation also requires provisions that encourage work for all participants in SNAP. As a result of the PRWORA legislation, all non-exempt household members participating in SNAP (with or without dependents) must meet general work requirements in order to remain eligible for SNAP.[26] These work requirements include registering for work, not voluntarily quitting a job or reducing work

[25] ABAWDs are defined as those who are between 18 and 50 years of age, not responsible for a child or incapacitated, and medically fit for employment. See the FNS website for details: https://www.fns.usda.gov/snap/able-bodied-adults-without-dependents-abawds
[26] Work requirements apply to those who are mentally and physically fit and over the age of 15 and under the age of 60. See the FNS website for details:
https://www.fns.usda.gov/snap/eligibility

effort below 30 hours a week, taking a job if offered, and participating in employment and training programs assigned by the state. Failure to comply with these requirements can result in disqualification from the program. While SNAP's general work requirements do not restrict the enrollment of unemployed individuals, working participants are prevented from quitting their job if they are to maintain eligibility. As a result, SNAP participation could increase labor supply even though, in theory, there are work disincentive effects from providing unearned income to beneficiaries. For example, Cuffey, Mykerezi and Beatty (2018) find evidence that work requirements affect employment decisions among ABAWDs most likely to participate in SNAP.

While there is a large literature on the work incentive effects of AFDC and the EITC, only a few studies consider the work incentive effects of SNAP, and all of the existing literature uses data prior to the PRWORA legislation. Fraker and Moffitt (1988) use structural models with kinked budget constraints to estimate labor supply and participation in AFDC and SNAP for families headed by a single women. They find that SNAP participation reduces hours of work by 1 hour per week. Hagstrom (1996) estimates the impact of the SNAP on labor supply among married couples, and finds small negative impacts of changes in the benefit amount on labor supply. However, Moffitt (2002) reviews the empirical literature and concludes that SNAP participation results in few work disincentives. Departing from the structural model approach, Hoynes and Schanzenbach (2012) exploit variation in county-level initial program rollout to control for selection into the program, and find that participation in SNAP reduces employment and hours worked among families headed by single woman.

This early research on the effects of enrollment in the SNAP on labor supply concludes that SNAP participation discourages work. However, there is no research on how the program, in its current form, affects work incentives. As Beatty and Tuttle (2015) note, SNAP has evolved considerably since its rollout. Changes in work requirements, eligibility, and program administration may have altered the characteristics of the population served by SNAP, and the work incentives faced by enrollees. Moreover, the role of women in the labor force has changed substantially over this period, which could also result in a different impact on labor supply decisions by participants. For example, Rosenbaum (2013) argues that SNAP participation does not generate work disincentives among recipients, but this study is largely descriptive, and it is unclear whether causal methods would lead to the same conclusion.

New research is therefore needed to understand the causal relationship between participation in SNAP and labor supply over the past two decades. This paper helps to fill that gap. Identification of causal effects requires a natural experiment that creates exogenous variation in SNAP participation, but does not affect employment decisions. SNAP benefits and eligibility rules are legislated at the federal level, and do not vary across states, leaving few opportunities for quasi-experimental analysis. One set of quasi-experimental studies analyzes the rollout of the SNAP across counties in the 1960s and 1970s to study the effect of SNAP participation on different outcomes (Almond, Hoynes and Schanzenbach 2011; Hoynes and Schanzenbach 2012). A second set of studies uses the policy variation introduced by state level changes in eligibility criteria and policies to reduce administrative burdens for applicants as instruments for SNAP participation (Meyerhoefer and Pylypchuk 2008; Yen et al. 2008; Ratcliffe, McKernan and Zhang 2011; Gregory and Deb 2015; Almada, McCarthy and Tchernis 2016). Similar to Bronchetti, Christensen and Hoynes (2018), our approach leverages plausibly exogenous geographic variation in the real value of maximum allotment of SNAP benefits for identification. Specifically, we use the ratio of maximum SNAP allotment to the food price faced by a household as the instrument for SNAP participation. Annual cost of living adjustments are made to SNAP benefit levels to account for national inflation in the cost of food. However, regional variation in food prices are not part of cost of living adjustment formula, even though regional food prices vary substantially.[27] More importantly, these regional differences change over time, with some areas experiencing larger increases in SNAP purchasing power, and others experiencing smaller increases. In addition, the American Recovery and Reinvestment Act (ARRA) of 2009 led to an unprecedented increase in maximum benefit levels for participant households (Beatty and Tuttle 2015), which creates another source of identifying variation during the sample period. We use USDA's Quarterly Food At Home Price Database (QFAHPD) to measure regional food prices, and assign them to the restricted access Medical Expenditure Panel Survey (MEPS).

Our study contributes to the growing body of research on the SNAP by providing new evidence on the relationship between SNAP participation and labor supply. Our findings suggest that SNAP enrollment increases labor supply, and therefore, that the expansion in SNAP actually helps to boost labor force participation, and hours worked. Furthermore, we find evidence that the increase in employment from SNAP participation is not solely due to work requirements, but results from other mechanisms as well. These results are relevant to recent policy debates discussing changes in SNAP work requirements (CBPP 2018). In particular, given that SNAP participation currently results in higher employment through multiple mechanisms, it is unclear whether imposing stricter work requirements would meaningfully affect the employment rate of participants.

[27] SNAP benefits are higher in Alaska and Hawaii, but the benefit formula is fixed across the other 48 states.

2.2 Empirical Approach

In order to estimate the impact of SNAP participation on employment decisions, we use the following estimating equation:

$$Y_{imt} = f(\alpha + \beta D_{imt} + X_{imt}\varphi + \varepsilon_{imt}), \tag{2-1}$$

where Y_{imt} is the labor market outcome of interest for individual i who resides in market m in year t, and D_{imt} is the measure of SNAP participation. X_{imt} is a vector of covariates including individual and family-level demographic and socioeconomic variables, self-reported health status and a measure of disability.

We estimate both a discrete measure of participation in SNAP and a continuous measure of the SNAP benefit level. The discrete variable equals zero if individuals are non-participants in SNAP and equals one if individuals are participants. The continuous measure is the natural logarithm of SNAP benefits, which we set equal to zero for those who are not enrolled in SNAP. These distinct specifications allow us to distinguish between the average effect of SNAP participation and the marginal effect of an additional dollar of benefit on employment decisions.

For both measures, we must account for the endogeneity of the SNAP variable to get consistent estimates of our outcomes of interest (Meyerhoefer and Yang 2011). Endogeneity of the discrete measure of SNAP participation is caused by adverse selection of individuals with lower SES or health status into SNAP. Along with concerns regarding selection into the program, our estimates for the continuous treatment specification suffer from simultaneity bias. This is because benefits are reduced, when labor market earnings increase. We identify the causal effect of SNAP using instrument variables, exogenous variation in federally determined benefit levels over time to account for both self-selection into SNAP and simultaneity bias.

2.2.1 Identification

Our identification strategy relies on quasi-experimental variation in the purchasing power of the maximum expected SNAP benefit. Though SNAP benefits are implicitly adjusted for the cost of living through allowed deductions, there is some evidence that these adjustments are not sufficient to equalize real benefits, particularly in high cost areas (Leibtag 2007; Todd, Leibtag and Penberthy 2011; Bronchetti, Christensen and Hoynes 2018). As a result, households living in areas of the country with food prices that are higher than the national average must supplement their food purchases with cash to a greater extent.[28] By implication, variation in the purchasing power of SNAP benefits will affect individuals' enrollment decisions. Specifically, all else equal, individuals living in low-cost areas will have higher real benefits, and a greater incentive to enroll in SNAP than those living in high cost areas. While the SNAP benefit level received by a household is endogenous to household members' employment decisions, variation in the real value of the maximum benefit allotment is plausibly exogenous, because it is determined by federal program rules, and regional food prices.

We use the ratio of the maximum allotment of SNAP benefits based on household composition to the regional food price as an instrument for SNAP participation.[29] This ratio will be larger in low cost areas than high cost areas, providing a greater incentive for SNAP participation in the former. However, low cost areas may be different from high cost areas in ways that affect labor force participation rates. In order to distinguish SNAP purchasing power from the broader effects of living in a more or less expensive market, we include market, and year fixed-

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[28] Based on data from the Quarterly Food at Home Price Database, regional food prices vary from 70 to 90 percent of the national average at the low end to 120 to 140 percent at the high end of the distribution.
[29] We use the cost of the Thrifty Food Plan as a standardized index across places and over time that allows us to capture variation in food prices that are relevant for the low-income population.

effects in our models. In addition, we control for regional price parities, the consumer price index, and the price of housing. We also include state-level per-capita income, poverty rate, minimum wage requirements, and educational attainment to control for regional socio-economic characteristics.

The equation for our endogenous variable is non-linear in regional food prices. Without any adjustment to the ratio of maximum SNAP allotment to food price, this non-linearity would largely eliminate the ability of additive fixed-effects to account for unobservable shocks and differential trends in food prices across market regions. We therefore construct our instrument in two steps. First, the regional food price is regressed on market-by-year characteristics. Letting m denote food market groups, F_{mt} denote the market's food basket price in year t, we estimate:

$$F_{mt} = X_{imt}\varphi_0 + \delta_t + \lambda_m + e_{mt}, \quad (2\text{-}2)$$

across food market groups and time using linear regression. λ_m and δ_t are market area and year fixed-effects. Second, the residuals of this regression are retained and used to construct the instrument. We add residuals from this regression to the intercept to calculate the purchasing power of households' maximum allotment across market groups (Chen and Ravallion 1996). Since we include market fixed-effects in our models, the identifying variation comes from differences across the market areas in food price trends. Additionally, in 2009, the American Recovery and Reinvestment Act (ARRA) increased maximum allotment of benefits by 13.6% for a SNAP household, which creates another source of exogenous variation in the instrument. As we discuss in the section on empirical results, the latter source of variation in our instrument is particularly important to account for simultaneity bias in order to identify the effect of changes in the continuous measure of SNAP benefits. The maximum allotment of SNAP benefits also vary according to the size of the household. Since the change in household size might be endogenous to SNAP participation, we control for household composition in all our models.

2.2.2 Econometric Models

Discrete measure of SNAP participation: To identify the causal effect of SNAP participation on employment, we estimate a recursive bivariate probit model. The first equation in the model predicts SNAP participation and the second equation, which is a function of SNAP participation, predicts employment status as follows:

$$\Pr(SNAP_{imt} = 1) = \Phi(\alpha_0 + \gamma Z_{imt} + X_{imt}\varphi_0 + S_{mt}\rho_0 + \delta_t + \lambda_m), \tag{2-3}$$

$$\Pr(E_{imt} = 1) = \Phi(\alpha_1 + \beta SNAP_{imt} + X_{imt}\varphi_1 + S_{mt}\rho_1 + \delta_t + \lambda_m), \tag{2-4}$$

where $\Phi(\cdot)$ is the standard normal cumulative distribution function, E_{imt} is employment status, and Z_{imt} is the real value of the maximum SNAP benefit allotment as discussed in section 2.1, and S_{mt} is a vector of state and market characteristics The main effect of interest in this model is the marginal effect of SNAP participation on employment.

Continuous measure of SNAP benefit level: We estimate the following conditional (recursive) mixed process model that includes a censored regression (Tobit) for our endogenous variable, the logarithm of SNAP benefits (we observe a positive benefit amount only if individuals are SNAP participants), and a second equation for employment status among SNAP-eligible individuals:

$$B_{imt} = \alpha_0 + \gamma Z_{imt} + X_{imt}\varphi_0 + S_{mt}\rho_0 + \delta_t + \lambda_m + \varepsilon_{imt}, \tag{2-5}$$

$$\Pr(E_{imt} = 1) = \Phi(\alpha_1 + \beta B_{imt} + X_{imt}\varphi_1 + S_{mt}\rho_1 + \delta_t + \lambda_m), \tag{2-6}$$

where B_{imt} is the log of SNAP benefits.

In addition, we modify both the model indicated in equations (2-3), (2-4), and the model indicated in equations (2-5), (2-6) to investigate whether SNAP causes transitions between full-time and part-time employment. First, we create three categories of weekly hours worked: less than 30 hours a week, between 30 and 40 hours a week, and at least 40 hours a week (full-time). When the log of SNAP benefits is the regressor of interest, we estimate a conditional mixed process model that uses a tobit model to predict the log of SNAP benefits, and an ordered probit model to predict full-time versus part-time work status among SNAP-eligible individuals. Likewise, we estimate a conditional mixed process model that uses a probit regression for SNAP participation, and an ordered probit to determine full-time versus part-time work status. In both cases, the error terms of the recursive model are assumed to be correlated (Roodman 2018).

2.3 Data

We use two data sources to determine how participation in SNAP affects labor force decisions. The main source of variation in our instrument, regional food prices, comes from USDA's Quarterly Food At Home Price Database (QFAHPD). Our outcome variables and many of our control variables come from the 1999-2010 Medical Expenditure Panel Survey (MEPS).

2.3.1 The Medical Expenditure Panel Survey

The MEPS is a nationally representative household survey of the US civilian noninstitutionalized population. Each panel of respondents was interviewed in five rounds covering two calendar years. MEPS contains detailed information on household and individual demographic characteristics, socioeconomic status, health status, and labor force participation. The MEPS contains several key variables that are useful for our analysis. MEPS respondents are asked whether anyone in the household received some amount of SNAP benefits in the past year, for how many months, and the monthly value of the benefit. We use the data in MEPS to construct a group of SNAP-eligible households. To be eligible for the SNAP, a household has to pass gross income, net income, and asset tests. The net income calculation requires subtracting certain deductions from a household's basic (or gross) monthly income. Since our data do not contain information on household assets and allowed deductions, we simulate gross income to determine households' eligibility status (we define the eligibility status based on the first-year observation of individuals). SNAP eligibility is formally restricted to those with gross household incomes at or below 130% of the federal poverty level based on household size (USDA 2016a).[30] We subset our sample to individuals aged 18-64 years.

2.3.2 The Quarterly Food-at-Home Price Database

In order to construct a regional food price to calculate the real value of maximum SNAP benefits, we use the 1999-2010 Quarterly Food-at-Home Price Database (QFAHPD). To construct this database, ERS researchers aggregated food purchases for food-at-home from the Nielsen Homescan database to estimate household-level quarterly prices for over 50 food groups. The household-level prices were then aggregated to estimate quarterly market-level prices. Quarterly prices for these goods are available for 35 market groups: 26 metropolitan areas, and 9 nonmetropolitan areas, though for 1999-2001 only 4 nonmetropolitan areas are captured.[31]

We construct a regional food price from the QFAHPD using information from the Thrifty Food Plan (TFP). The USDA's TFP defines a representative healthful and minimal cost diet with limited resources. Maximum allotments are set at the monthly cost of the TFP for a four person

[30] Monthly income eligibility standards for 1996-2003 were obtained from USDA/FNS.
[31] In 1999-2001, the QFAHPD identified one nonmetropolitan area for each of the 4 census regions (east, central, south and west). In 2002 and later, they expanded to include nonmetropolitan areas in each of the 9 census divisions: New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain and Pacific (USDA 2017). For consistency, we use the 4 nonmetro areas throughout.

family consisting of a couple between ages 20 and 50 and two school-age children, adjusted for family size. We follow Gregory and Coleman-Jensen (2013) and create a food basket price for each market and year during 1999-2010 in two steps. First, we map the QFAHPD food categories to the 29 TFP food group prices in the market basket using an expenditure-weighted average of the prices for the QFAHPD foods, where the weights are the fraction of yearly national expenditures in the TFP category for the QFAHPD good (most TFP food items consist of multiple QFAHPD food groups). Once we have constructed the region-by-year price for 29 TFP food groups, we calculate our basket (TFP) price using the amounts recommended for a family of four comprised of two adults and two children. Figure 2-B-1 illustrates the variation across the QFAHPD market areas and over time in TFP price (in 2013 dollars).

We assign the market region-by-year TFP prices to households in the MEPS based on the household's county of residence (which we map into the QFAHPD market area) and the year of interview. We then measure the purchasing power of SNAP benefits using the ratio of the maximum SNAP benefit to the TFP price faced by the household.[32] Figure 2-1 shows the quarterly-level mean of variation in SNAP purchasing power for a four member family across the U.S. from 1999 to 2010 as well as the minimum and maximum levels of purchasing power. SNAP purchasing power varies significantly across areas rising sharply in all areas with the ARRA. In the first quarter of 1999, at the beginning of the sample period, the ratio varies from 0.62 to 0.75, but gap in real SNAP benefits between high cost and low cost areas increases over time. After the sharp increase in SNAP purchasing power in the second quarter of 2009 due to the implementation of

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[32] Allotments are adjusted for food price inflation annually, each October, to reflect the cost of the TFP in the immediately previous June. We use the weighted average of monthly amounts to obtain the allotment for each calendar year. We obtained maximum allotment amounts for 1999-2004 from USDA/FNS.

the ARRA, SNAP benefits cover 84 percent of the TFP in the lowest cost area, west south central, while in the highest cost area, Metropolitan New York, benefits only cover 64 percent of the TFP. Our purchasing power measure is less than 1 for all market groups, but this is because our constructed regional TFP prices from the QFAHPD are based on average prices paid by all consumers, whereas the USDA's TFP is based on prices paid by low-income individuals (Ziliak 2016). Another reason is that the annual cost of living adjustment to maximum SNAP benefits that occurs in October is based on price levels from the previous June. Because of this lag, even at the start of a fiscal year, the maximum benefit is unlikely to cover the full cost of the TFP (Todd 2015).

2.3.3 Control Variables

We control for a full set of socio-demographic characteristics and health status variables in our models. Our main control variables include dichotomous indicators for age (30–39, 40–50, 51–64, with age 18–29 as the omitted category), gender, race and ethnicity (Hispanic, black, and other race, with white as the omitted category), region (South, Midwest, and West with Northeast omitted), urban residence, education (high school diploma, any college, with less than a high school degree omitted), family size, number of children in the household (under age 5, 5-18), whether the household has a disabled member, whether the household has an elderly member[33], and the log of income earned by other family members normalized by the square root of household size. In order to control for health status, we use self-reported mental and physical health (poor/fair health in all rounds, poor/fair health in some rounds, excellent health in some rounds, excellent health in all rounds, good/very good health in all rounds, and self-reported health is missing, with good/very good health in some rounds serving as the omitted category for both mental and physical

[33] Households are exempt from the gross income test if they are an SSI recipient or if any household member is 60 years of age or older.

health) and a measure of disability status. The latter is a binary variable that indicates whether the person had an IADL (Instrumental Activities of Daily Living) or ADL (Activities of Daily Living), functional, activity, or sensory limitation in any interview round.

In addition, we include state-level per-capita income, poverty rate, minimum wage requirements, and educational attainment (percentage of bachelor's degree for persons 25 years of age and older) to control for socio-economic characteristics; state-level housing cost, and market-level price parities to control for the differences in overall price levels, and the CPI for the four census regions to control for changes in price over time. Per capita income, poverty rate, and educational attainment were obtained from the U.S. Census Bureau, state minimum wages and the census CPI comes from the Bureau of Labor Statistics, the housing cost measure was obtained from Freddie Mac[34], and state price parities are calculated by the Bureau of Economic Analysis.

In our conditional sample of working adults, we also control for employment characteristics. These include union status, employer size (between 100-500 employees, more than 500 employees, with less than 100 employees as the omitted category), benefits provided by the employer (retirement plan, and paid vacations), a white collar occupation indicator, and industry indicators.[35]

Table 2-C-1 lists summary statistics for the main variables used in the analysis. These statistics show clear selection into SNAP by individuals with lower incomes, less education, poorer

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[34] The annual home price index is the average of the monthly home price indices, by state, published by Freddie Mac as the Freddie Mac Home Price Index (FMHPI), found at http://www.freddiemac.com/finance/ fmhpi/.

[35] The industry indicators include: 1. natural resources/mining/construction/manufacturing; 2. wholesale and retail trade/transportation and utilities; 3. professional and business services/education, health, and social services; 4. other services/public administration/military/unclassifiable industry.

health status, and higher rates of disability. Importantly, our summary measures indicate that labor force participation rates among SNAP recipients are lower than those who do not receive benefits.

2.4 Main Results

We begin by estimating alternative first stage models in order to demonstrate how our models are identified, and highlight the importance of controlling for differences in overall price levels and other time invariant factors across market areas. First, we estimate first stage models for SNAP participation and the log of SNAP benefits on our sample of SNAP eligible individuals with and without market fixed effects.[36] These models contain all of the control variables listed in Table 2-C-1, which include three variables to control for changes in non-food prices over time (regional CPI, state price parities, and state housing price index). The coefficient on our instrument, the maximum SNAP allotment divided by the TFP-derived price, and the F-statistic of the instrument are reported in Table 2-1. In both the models for SNAP participation and SNAP benefits there is a clear downward bias in the coefficient estimates on MAX allotment/TFP price in the models without market fixed effects. This is because overall market-area-specific price levels, and possibly other area-level time-invariant unobservable factors, are negatively correlated with the instrument and positively correlated with SNAP participation and benefits.[37]

Next, we estimate models with an alternative instrument, the maximum SNAP allotment not adjustment for the TFP price. The power of this instrument is much lower than the price-adjustment instrument in the first-stage model of SNAP participation, with the F-statistic dropping from 25.2 to 9.5. Because increases in the maximum SNAP allotment over time lead to increases

[36] We set the SNAP benefit level equal to $1 for individuals who are eligible for, but do not participate in SNAP.
[37] SNAP benefits increase with overall price levels because the value of applicable deductions (compensating for expenses such as dependent care, excess shelter costs, and out-of-pocket medical expenses) increase.

in SNAP participation, this instrument nearly meets the conventional standard for adequate power for a continuous instrument (F = 10; Stock, wright, and Yogo 2002). However, the price-adjusted instrument is a much stronger predictor of SNAP participation because households make SNAP participation decisions based on the real value of benefits. The F-statistic in the first stage model of SNAP benefits also drops when the unadjusted maximum allotment is used as an instrument, but to a lesser extent. This is because the correlation between the two instruments and SNAP benefits for the sample of participating households is due solely to a correlation between benefits and the maximum SNAP allotment over time.

In Table 2-2, we report the first stage estimates separately for women and men for our main specification with market fixed effects, and the real value of the maximum SNAP allotment as the instrument. The subgroup analysis by gender indicates that the purchasing power of the maximum SNAP allotment is statistically significant for both men and women, but the point estimate of the coefficient is twice as large for women. Moreover, the F-statistic associated with the excluded instrument after controlling for market group and year fixed effects is 37.7 for SNAP participation and 46.6 for the SNAP benefit level in the sample of women, but falls to 7 and 6.8, respectively, in the sample of men.

In Table 2-3, we report the marginal effects from our IV model (second column), as well as results from a univariate probit model that does not account for the endogeneity of SNAP participation (first column). The first row in each panel presents results from the discrete measure of SNAP participation, and the second row contains marginal effect estimates for the continuous measure of SNAP benefits. We present results for the pooled sample, as well as the samples of men and women. The marginal effect from the non-IV model indicates that SNAP participation is associated with 10.6 percentage points (17.9%) lower likelihood of employment. We find similar results when we estimate the effects of the SNAP benefit level; doubling SNAP benefit (a 100% increase) is associated with a 1.7 percentage points (2.9%) reduction in the probability of employment. However, after controlling for the endogeneity of SNAP participation, we find that SNAP participation increases the likelihood of employment by 3.8 percentage points (6.4%) among low-income adults. Likewise, doubling the benefit results in a 2.7 percentage points (4.5%) increase in the probability of employment in the pooled sample. The downward bias on the effect of SNAP in the non-IV model of employment is consistent with a negative correlation between lower unobserved SES and employment, and a positive correlation between the lower SES and SNAP participation.

There is a noticeable difference between the labor market effects of SNAP for men and women. However, this may be due to the low power of the IV in the sample of men. After controlling for endogeneity, our results suggest that SNAP participation results in 5.5 percentage points (9.7%) increase in the probability of employment for women, but the effect is not statistically different from zero in the sample of men. Likewise, doubling the SNAP benefit increases the probability of employment by 3.3 percentage points (5.8%) for women in the IV model, but not for men.

In Table 2-4, we investigate whether SNAP causes transitions between full-time and part-time employment using the sample of working adults.[38] The non-IV results imply that SNAP participation is associated with 6.4 percentage points (10.1%) lower likelihood of full-time work. Likewise, a 100% increase in SNAP benefits reduces the probability of working full time by 0.5 percentage points (0.8%). These results are completely reversed in sign when the endogeneity of

[38] Employed individuals who had missing hours were dropped from the conditional sample of working adults. Those who reported working more than 120 hours per week were also excluded due to concerns over reporting error.

SNAP is taken into account. The marginal effects from IV models for part-time versus full-time employment imply that SNAP participation results in a 22 percentage point (35%) increase in full-time rather than part-time work, and doubling the SNAP benefit increases the incentive to work full time by 2.9 percentage points (5%). As in the pooled sample, we find that an increase in SNAP benefit dollars increases the likelihood of full-time employment when we estimate separate models for men and women. These results suggest that SNAP appears to move people from part-time to full-time work.

2.5 Robustness Checks and Falsification Tests

We conduct several robustness checks of our main results. First, we re-estimate all of our models after excluding ABAWDs. These individuals face stricter work requirements and time limitations as a qualification for receiving assistance. When we exclude ABAWDs from the sample, we find our marginal effect estimates are very similar. This suggests that our results are not driven by this sub-population.[39]

2.5.1 Measurement Error

An important identification problem that arises in this study is nonrandom measurement error. This is because a large fraction of recipients mis-report their participation in SNAP, and as previous research suggests, these mis-reports mostly "false negative" reports by households that do not report participation, but are in fact enrolled in SNAP (Bollinger and David 1997; Meyer, Mok, and Sullivan 2009). In the presence of substantial reporting error in participation, drawing definitive conclusions about the effects of SNAP can be challenging. (Kreider et al. 2012;

[39] In addition to controlling for disability status in our main models, we re-estimate our models after dropping individuals with a disability, and find similar results.

Alamada, McCarthy, and Tchernis 2016). In order to examine the possibility that our results are confounded by measurement error, we estimate our models after an adjustment for the mis-classification of SNAP enrollment. We use variation in the state-level rates of SNAP participation to predict the likelihood of participation for SNAP-eligible households based on demographic information and socio-economic status. We then reclassify participation status for individuals who did not report participating in SNAP in the MEPS with the highest predicted participation levels until the rate of SNAP participation in the MEPS equals the national rate of SNAP participation in each year. We describe this procedure in greater detail in the supplementary appendix. Table 2-C-2 contains estimates from our IV models for the discrete measure of SNAP where some SNAP non-participants have been reclassified as participants using the method described above. The marginal effect of SNAP participation on employment in this adjusted model is 3.8 percentage points in the sample of women, which is similar to the 5.5 percent point effect from our main model. Although we lose power to detect effects in the full-sample after re-classifying individuals as SNAP participants, we are reassured that measurement error bias does not change the qualitative conclusions from our models.

2.5.2 Exclusion Restriction

In order for our models to generate consistent estimates of the impact of SNAP participation on employment outcomes, the instrument must be excludable from the outcome equation. A natural validity check of our identification strategy results is to estimate the reduced form model of employment on the real value of SNAP benefits among SNAP participants, and compare these results to those from a "placebo" sample of adults whose employment decisions should not be affected by the purchasing power of SNAP benefits. We use non-participating SNAP eligible adults as the placebo sample because they are comparable (after regression adjustment) to SNAP participants. If we find a significant effect of the instrument on non-participating SNAP eligible adults, it would suggest a violation of the exclusion restriction. Table 2-C-3 contains the result of this test. As with the main results, we find that an increase in the purchasing power of SNAP benefits significantly raises the likelihood of employment (panel 1). However, we find no effect of the instrument on the probability of employment for SNAP-eligible adults who do not report participating in SNAP (panel 2).

2.5.3 Alternative Instrument

Since the exogeneity of instruments is difficult to validate, we use another source of identification to see whether our estimates are robust to different plausible instruments. In particular, we use simplified reporting requirements as the excluded instrument in equations (2-3), and (2-4) (USDA 2016b).[40] Under simplified reporting, SNAP households must only report income changes that occur during the reporting period if they result in total countable income rising above 130 percent of the poverty level. The 2002 Farm Bill gave states the discretion to extend simplified reporting requirements to households with non-earned income, referred to as expanded simplified reporting. Many states also lengthened reporting intervals to 4, 5 or 6 months for 12 month certification periods. We only distinguish between states that adopted any form of simplified reporting and those that did not. The F-statistic of the IV (F=21.34) indicate that simple reporting requirements are predictive of SNAP participation. Table 2-C-4 shows the marginal effects of SNAP on the probability of employment for the discrete SNAP participation specification. Marginal effect estimates for the full-sample and the sample of women are qualitatively similar to those from our main model that uses the purchasing power of SNAP benefit as an instrument. However, we also

[40] Simplified reporting requirements is the most powerful state-level instrument in the data sample. When we use state-level instrument, we use the MEPS data from 1996-2013.

find that SNAP participation increases labor supply in men only sample as well. This may be because the new instrument has sufficient power in the sample of men to identify an effect, which is not the case with our previous instrument. Interestingly, the magnitude of the effect of SNAP participation on employment is twice as large for women as compared to men.

2.6 Potential Mechanisms

Our results suggest that participation in SNAP leads to higher rates of employment and more working hours. In order to investigate the mechanism behind these findings, we first consider the effect of changes in SNAP benefits on employment decisions by those enrolled in SNAP.

Since SNAP's general work requirements do not restrict the enrollment of unemployed individuals (just separation from employment for those receiving benefits), finding an effect of SNAP benefit amounts on those enrolled in SNAP would suggest that mechanisms other than work requirements contribute to the positive effect of SNAP benefits on employment. To investigate this issue, we use two-stage least squares where the endogenous variables is the logarithm of SNAP benefits with the same regressors as specified in equations (2-5) and (2-6). We also estimate a conditional mixed process model to identify the marginal effect of SNAP benefits on working hours. In both cases, we estimate the model on the sample of individuals enrolled in SNAP. Tables 2-5 (employment) and 2-6 (work hours) contain the marginal effects of SNAP benefits on employment outcomes for SNAP participants. We find that doubling SNAP benefits increases the likelihood of employment by 15.3 percentage points (35.6%) and also increases the likelihood of full-time work. When we stratify the sample by gender, we find that doubling SNAP benefit amounts increases the probability of employment for women by 13.7 percentage points (32.6%), but we lose power to identify effects on part-time versus full-time work. Overall, these results suggest that SNAP's general work requirement is not the only reason why increases in SNAP benefit amounts lead to higher rates of employment and full time work. Therefore, we consider other possible mechanisms that may explain our results.

One possibility is that SNAP improves recipients' ability to pay for job-related expenses. Major categories of such expenses include transportation and child care (see Figure 2-B-2). The high cost of child care can be an impediment to taking a job among low income households with children. Since SNAP participation frees up income for nonfood expenditures, participant households may face fewer challenges in terms of arranging safe and reliable child care. The ability to pay childcare may induce them to work more by reducing their own time spent providing child care.

According to the U.S. Census Bureau, the percentage of income that poor families with employed mothers spend on child care is four times more than that of other working parents. Families with incomes below 100 percent of the FPL spend 30.1 percent of their income on care and families with incomes from 100 to 200 percent of the FPL spend 17.9 percent of their income on care, compared to 6.9 percent of income for families with incomes at or above 200 percent of the FPL. While child care subsidy programs help defray these costs for some low-income families, only a small proportion of eligible families receive them. To help buffer the impact that out-of-pocket child care expenses can have on family food budgets, Congress in 1980 created a separate SNAP deduction for dependent care expenses. This allows SNAP recipients to deduct dependent care expenses required for work from income when calculating SNAP benefits. The deduction allows for both licensed child care as well as informal or alternative types of care as long as another member of the food stamp household does not provide it. Similarly, household members caring for elderly or disabled adults who are financially dependent upon the household member may also be eligible for the dependent care deduction even if they live in the same household. While any household with out-of-pocket dependent care expenses is eligible for this deduction, the group most likely to claim it is single-parent households with children where the parent is employed. The deduction provides SNAP recipients with children an additional incentive to work.

We use data from Early Childhood Longitudinal Study, Kindergarten Class (ECLS-K) to empirically examine whether children in SNAP households are more likely to receive care from non-parental sources. The ECLS-K is a nationally representative survey of children entering kindergarten in the 1998–1999 school year conducted by the National Center for Educational Statistics of the U.S. Department of Education (Institute of Education Sciences 2009). The ECLS-K collected information from children, their parents, teachers and their schools, using a variety of methods. Parents were surveyed by a trained interviewer over the phone, and teachers and school administrators completed paper and pencil surveys.

Data were collected during the fall and spring of kindergarten (1998–1999), fall and spring of first grade (1999–2000), the spring of third grade (2002), the spring of fifth grade (2004), and the spring of eighth grade (2007), but not all of those waves are useful for this analysis. We include the fall kindergarten, the spring 1st, 3rd, and 5th grades, because information on child care arrangements are recorded only for these four waves. The spring kindergarten wave does not include information on child care, so we use that wave only to extract certain time-invariant characteristics of children, such as their race and ethnicity.

Parents in the ECLS-K are interviewed about their participation in SNAP, and the data also contain various measures of child care. We create binary measures of whether the child receives care from a child care center or from a non-parental arrangement, current relative, or non-relative. We then estimate a recursive bivariate probit model to determine whether SNAP households are more likely to utilize non-parental child care services. The first equation in the model predicts

SNAP participation and the second equation predicts whether the child receives any non-parental care. The instrument we use to identify SNAP participation is a variable that indicates whether the SNAP recipient's state of residence expands categorical eligibility rules, also known as broad-based categorical eligibility (BBCE).[41] Under BBCE, states can opt to set a gross income limit higher than the SNAP Federal limit and waive, or relax, the SNAP Federal asset test. The F-statistic of this instrument is 8.9. The set of control variables includes: child age, gender, race/ethnicity (White, Black, Hispanic, and Other), population density of residence (urban, suburban, and rural), number of household members under age 18, the age of parents (if they live in the household), the log of total family income normalized by the square root of household size, and the years of education of the most educated parent.

Results for this model are reported in Table 2-7. We do not find any statistically significant impact of SNAP participation on relative care or care from a child care center, but we do find that children in SNAP households are 3.9 percentage points more likely to receive care from informal arrangements. Informal care refers to minimally regulated care provided by a neighbor or extended family member looking after a child outside school hours. Importantly, this type of child care qualifies for the SNAP dependent care deduction.

Another potential mechanism for our findings is that SNAP participants may be able to consume more or higher quality food, which increases their productivity. This could both increase their incentives to seek paid employment, and also increase the effectiveness of their job search. We are limited in our ability to investigate such a nutrition effect directly because the MEPS does

[41] When we use ECLS-K data, we are not able to use the purchasing power of maximum allotment as an instrument for SNAP enrollment, since the ECLS-K data does not contain information on the county of residence. BBCE is the most powerful state-level instrument in the ECLS-K data sample.

not contain data on food intake. Instead, we estimate the impact of SNAP participation on self-reported physical and mental health status, both of which are correlated with nutrition (Evans and Garthwaite 2014; Kreider et al. 2012; Miller and Morrissey 2017).[42] Results from our IV model indicate that SNAP participation decreases the probability of reporting poor or fair health by 5.9 percentage points (9.3%) and increases the probability of reporting very good or excellent health by 3.4 percentage points (11.0%; Table 2-8). These results are qualitatively similar to those in Gregory and Deb (2015), who use fewer years of MEPS data, and a different estimation strategy. Likewise, we find that SNAP participation is associated with improvements in self-assessed mental health. Prior research has shown that a better physical and mental health is associated with a higher likelihood of employment (Dooley, Fielding and Levi 1996; Currie and Madrian 1999; Peng, Meyerhoefer and Zuvekas 2016).

2.7 Conclusion

In this article, we present evidence on the work incentive effects of SNAP participation and the level of SNAP benefits. Our finding that SNAP participation increases employment is contrary to earlier studies based on data collected before important welfare reforms. SNAP has undergone substantial changes over the past two decades. These changes affected both the number and characteristics of those enrolled in SNAP, as well as the work incentives that they face. Our results suggest that the combined effect of these incentives is to increase both employment and hours worked. We also find that increases in SNAP benefits lead to higher employment and work hours among adult SNAP participants, which suggests that the positive effect of SNAP on employment is not purely due to SNAP's general work requirement. In support of this finding, we find evidence that

[42] In addition to the set of controls in the labor supply models, we control for health insurance coverage when we estimate the effects on self-reported health.

SNAP households are more likely to use informal child care than non-participating SNAP eligible households, suggesting that the ability to afford job-related expenses may also contribute to higher rates of employment. Moreover, our subgroup analyses indicate that the effects of SNAP on employment are concentrated among women, which is consistent with the importance of SNAP to child care affordability. We also find that SNAP increases levels of both physical and mental health, which could also lead to higher rates of employment.

Our study has some limitations that must be recognized. First, we use variation in the purchasing power of the maximum SNAP allotment (determined by federal program rules and regional food prices) for identification. Although we include controls for time-varying state and regional characteristics in our models as well as market area fixed effects, it is still possible that our results could be confounded by the selection of SNAP-eligible individuals into low-cost areas. We do note, however, that the large size of market regions may mitigate the effects of consumer selection into specific locations. We are also reassured that our models fail to identify effects in a placebo sample of non-SNAP adults. Another limitation of our study is that we are not able to disentangle work incentives effect of work requirements from the effects of SNAP participation on disposable income or nutrition and health. However, we find evidence that work requirements are not the only mechanism for our findings. Finally, prior studies demonstrate that SNAP participation is often under-reported in household surveys. We conduct a sensitivity analysis which suggests that our qualitative findings are not the result of measurement error, but measurement error may still affect the magnitudes of our estimated marginal effects.

Despite these limitations, we believe that our study has important implications for public policy. First, we provide the foundational analysis necessary to understand how low paid workers react to SNAP participation, and insight into how states can structure the design of policies to facilitate employment among SNAP recipients. For example, our study suggests that states may wish to examine how SNAP allowances for dependent care are utilized. Although households have been able to deduct the full amount of their eligible dependent care costs from their gross income since 2008, few households take advantage of this deductions (CBPP 2017). Great use of the SNAP dependent care deduction could boost labor force participation and hours worked among low-paid workers. Second, these findings are relevant to recent policy debates on strengthening work requirements in the SNAP. If SNAP participation already increases labor force participation and hours worked, it is unclear whether stronger work requirements would have a meaningful impact on these outcomes. Our results suggest that increasing SNAP benefit levels could lead to greater labor force participation in recipient households without any change in work requirements.

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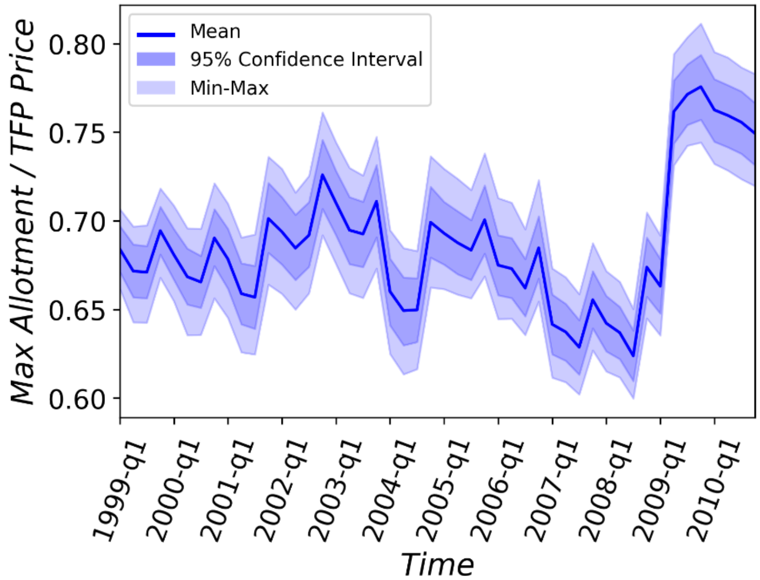
Figure 2-1. Variation in SNAP Purchasing Power over Time



Notes: The above figure depicts quarterly-level mean of the ratio of maximum SNAP allotment to TFP price (with 95% confidence intervals, minimum and maximum) constructed from the Quarterly Food-at-home Price Database (QFAHPD) across market group areas from 1999 to 2010.

Table 2-1. Linear Relationship between Endogenous Variables and Maximum SNAP Allotment

	SNAP participation				Log (SNAP benefit)			
	(1)		(2)		(3)		(4)	
	Coefficient	F-statistic	Coefficient	F-statistic	Coefficient	F-statistic	Coefficient	F-statistic
SNAP eligible								
MAX allotment/TFP price	0. 172***	17.06	0.211***	25.20	1.081***	18.92	1.376***	27.77
	(0.042)		(0.042)		(0.248)		(0.261)	
MAX allotment	0.001***	9.45	0.001***	9.46	0.004***	17.89	0.004***	17.81
	(0.000)		(0.000)		(0.001)		(0.001)	
State and market controls	X		X		X		X	
Year fixed effect	X		X		X		X	
Market fixed effect			X				X	

Notes: Both specifications include time-varying household and individual demographic controls, state and market characteristics and year fixed effects. In columns 2, 4 we add market fixed effects. Standard errors in parentheses are adjusted for the complex design of the MEPS in the second row. Bootstrap standard errors in the first row based on 300 iterations are corrected for clustering at the market group level.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-2. First Stage of IV Model

	(1) SNAP participation		(2) Log(SNAP benefit)		
	Coefficient	F-statistic	Coefficient	F-statistic	observations
Panel 1: Full-sample					
SNAP purchasing power	0.211***	25.20	1.376***	27.77	62,065
	(0.042)		(0.261)		
Panel 2: Women only					
SNAP purchasing power	0.242***	37.70	1.630***	46.65	36,180
	(0.039)		(0.239)		
Panel 3: Men only					
SNAP purchasing power	0.141**	6.81	0.901***	7.02	25,885
	(0.054)		(0.340)		

Notes: Both specifications include time-varying household demographic controls, state and market characteristics, market and year fixed effects. Bootstrap standard errors in parentheses based on 300 iterations are corrected for clustering at the market group level.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-3. Marginal Effects of SNAP on Employment among SNAP-Eligible Adults

	(1) Non-IV	(2) IV	Mean of dep. var.
Panel 1: Full-sample			
SNAP participation	-0.106***	0.038***	0.589
	(0.008)	(0.007)	
Log (SNAP benefit)	-0.017***	0.027***	0.589
	(0.001)	(0.004)	
Panel 2: Women only			
SNAP participation	-0.116***	0.055***	0.565
	(0.009)	(0.007)	
Log (SNAP benefit)	-0.018***	0.033***	0.565
	(0.001)	(0.005)	
Panel 3: Men only			
SNAP participation	-0.107***	-0.003	0.618
	(0.013)	(0.012)	
Log (SNAP benefit)	-0.017***	0.010	0.618
	(0.002)	(0.009)	

Notes: Standard errors in parentheses are adjusted for the complex design of the MEPS for the non-IV model. For IV model, bootstrap standard errors based on 300 iterations are corrected for clustering at the market group level. Both specifications include time-varying household demographic controls, state and market characteristics, market and year fixed effects.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-4. Marginal Effects of SNAP on Working Hours among Employed SNAP-Eligible Adults

	(1) Non-IV			(2) IV		
	hour<30	(hour<40 & hour>=30)	hour>=40	hour<30	(hour<40 & hour>=30)	hour>=40
Panel 1: Full-sample						
SNAP participation	0.051***	0.013***	-0.064***	-0.171***	-0.049***	0.220***
	(0.008)	(0.002)	(0.009)	(0.022)	(0.006)	(0.028)
Log (SNAP benefit)	0.004***	0.001***	-0.005***	-0.022***	-0.007***	0.029***
	(0.001)	(0.000)	(0.001)	(0.003)	(0.001)	(0.004)
Mean of dep. var.	0.194	0.163	0.632	0.194	0.163	0.632
Panel 2: Women only						
SNAP participation	0.067***	0.010***	-0.077***	-0.186***	-0.036***	0.223***
	(0.011)	(0.001)	(0.012)	(0.025)	(0.004)	(0.029)
Log (SNAP benefit)	0.006***	0.001	-0.007***	-0.025***	-0.005***	0.030***
	(0.001)	(0.000)	(0.002)	(0.004)	(0.001)	(0.005)
Mean of dep. var.	0.244	0.205	0.539	0.244	0.205	0.539
Panel 3: Men only						
SNAP participation	0.050***	0.017***	-0.067***	- 0.116***	-0.046***	0.161***
	(0.010)	(0.003)	(0.013)	(0.034)	(0.011)	(0.045)
Log (SNAP benefit)	0.004***	0.002***	-0.006***	- 0.014***	-0.006***	0.020***
	(0.001)	(0.000)	(0.002)	(0.004)	(0.002)	(0.005)
Mean of dep. var.	0.139	0.117	0.734	0.139	0.117	0.734

Notes: Standard errors in parentheses are adjusted for the complex design of the MEPS for the non-IV model. For IV model, bootstrap standard errors based on 300 iterations are corrected for clustering at the market group level. Both regression models include time-varying household demographic controls, state and market characteristics, market and year fixed effects.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-5. Marginal Effect of SNAP Benefits on Employment among Adult SNAP Participants

	Non-IV	IV	Mean	Observations
Panel 1: Full-sample				
Log (SNAP benefit)	-0.002	0.153***	0.43	22,859
	(0.006)	(0.044)		
Panel 2: Women only				
Log (SNAP benefit)	-0.006	0.137***	0.42	14,839
	(0.007)	(0.053)		
Panel 3 :Men only				
Log (SNAP benefit)	0.001	0.049	0.45	8,020
	(0.008)	(0.178)		

Notes: Standard errors in parentheses are adjusted for the complex design of the MEPS for the non-IV models. For IV models, bootstrap standard errors based on 300 iterations are corrected for clustering at the market group level. Both regression models include time-varying household demographic controls, state and market characteristics, market and year fixed effects.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-6. Marginal Effect of SNAP Benefits on Work Hours among Employed SNAP Adults

	hour<30	(hour<40 & hour>=30)	hour>=40	Observations
Panel 1: Full-sample				
Log (SNAP benefit)	-0.147*	-0.031**	0.178**	11,448
	(0.081)	(0.012)	(0.093)	
Panel 2: Women only				
Log (SNAP benefit)	-0.162	-0.015	0.177	6,877
	(0.115)	(0.011)	(0.126)	
Panel 3: Men only				
Log (SNAP benefit)	-0.080	-0.036	0.117	4,571
	(0.191)	(0.069)	(0.261)	

Notes: Bootstrap standard errors in parentheses based on 300 iterations are corrected for clustering at the market group level. The regression model includes time-varying household demographic controls, state and market characteristics, market and year fixed effects.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-7. Marginal Effects of SNAP Participation on the Use of Non-Parental Child Care

	Non-parental care	Formal care	Informal care	Relatives
Panel A: non-IV	-0.041*	-0.005	0.012	-0.042*
	(0.023)	(0.015)	(0.011)	(0.023)
Panel B: IV	-0.082	-0.043	0.039*	-0.016
	(0.122)	(0.042)	(0.020)	(0.073)
Observations	6450	6450	6450	6450

Notes: Standard errors for IV models are corrected for clustering at the state level. Sample sizes are rounded to the nearest 50 in order to comply with Department of Education non-disclosure requirements for ECLS-K, 1998. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-8. Marginal Effects of SNAP Participation on Self-Reported Health

	Physical health			Mental health		
	Poor	Fair/Good	Excellent	Poor	Fair/Good	Excellent
SNAP	-0.059***	0.001	0.034***	-0.035***	-0.023***	0.044***
	(0.005)	(0.015)	(0.003)	(0.009)	(0.009)	(0.005)
Log(SNAP benefit)	-0.031***	0.013**	0.032***	-0.016***	-0.006	0.031***
	(0.003)	(0.006)	(0.004)	(0.003)	(0.006)	(0.006)

Notes: bootstrap standard errors in parentheses based on 300 iterations are corrected for clustering at the market group level. Regression models include time-varying household demographic controls, state and market characteristics, market and year fixed effects.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Appendix A. Measurement Error Adjusted SNAP Measure

In this section, we explore the possibility that our results are confounded by measurement error in self-reported SNAP participation. To do this we use data on state-level rates of SNAP participation from SNAP Data System to construct an error-adjusted measure of SNAP participation.

First, we estimate a state-level regression of the state SNAP participation rate on state-level measures of demographic composition and socio-economic status, the unemployment rate, and the poverty rate from the U.S. Census Bureau, as well as state-level SNAP policies determining eligibility criteria, recertification and reporting requirements, benefit issuance methods, availability of online applications, use of biometric technology (such as fingerprinting), and coordination with other low-income assistance programs from the SNAP Policy Database.[43]

We subsequently use this model to predict SNAP participation for individuals in the MEPS who are eligible for SNAP, but do not report participating in the program. In order to make this prediction, we use the individual's demographic information, but the state-level information from the individual's state of residence for the other state-level measures. We reclassify eligible individuals who did not report participating in SNAP in the MEPS with the highest predicted participation levels as SNAP participants. We do this until the rate of SNAP participation in the MEPS equals the national rate of SNAP participation in each year.[44]

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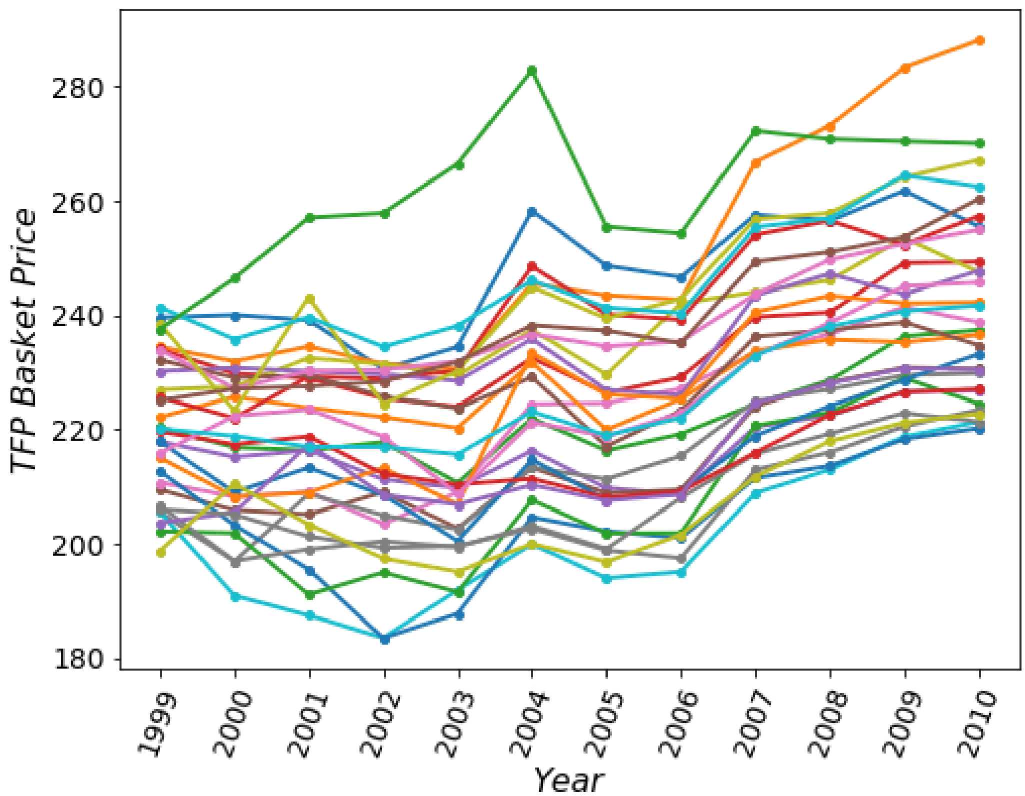
[43] State-level measures of demographic composition and socioeconomic status include age categories (0-5, 6-13, 14-17, 18-24, 25-44, 45-64, 65 and older), educational attainment (college degree or higher, high school diploma, below high school), race and ethnicity (white, black, Hispanic, and other races), and per capita income.
[44] Time-series data on individual level rate of SNAP participation available at: https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap

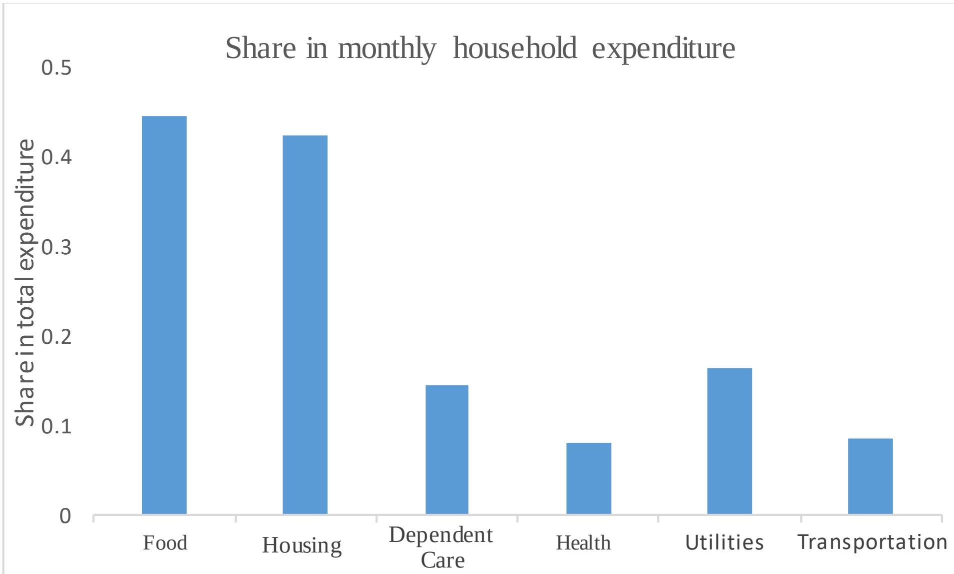
Appendix B. Additional Figures

Figure 2-B-1. Variation in TFP Basket Price across Market Group Areas over Time



Notes: The above figure depicts average TFP basket prices constructed from the Quarterly Food-at-home Price Database (QFAHPD) for each market from 1999 to 2010.

Figure 2-B-2. Fraction of Monthly Household Expenditures, By Expenditure Category



Notes: Authors' calculations from the FoodAPS. Sample includes SNAP recipients.

Appendix C. Additional Tables

Table 2-C-1. Descriptive Statistics for SNAP-Eligible Adults by SNAP Participation and Employment Status

	SNAP recipients				Non-SNAP			
	All adults		Employed only		All adults		Employed only	
	Mean	S. D.	Mean	S. D.	Mean	S. D.	Mean	S. D.
Individual level controls								
Age 30-39	0.251	0.433	0.303	0.460	0.175	0.380	0.198	0.398
Age 40-50	0.220	0.414	0.188	0.391	0.213	0.409	0.213	0.409
Age 51-64	0.173	0.378	0.066	0.248	0.329	0.470	0.262	0.440
Hispanic	0.214	0.410	0.224	0.417	0.185	0.389	0.185	0.388
Black	0.304	0.460	0.304	0.460	0.139	0.346	0.133	0.340
Other race	0.042	0.201	0.044	0.206	0.051	0.221	0.049	0.215
Married	0.288	0.453	0.321	0.467	0.519	0.500	0.518	0.500
NO. children under 5	0.623	0.888	0.801	0.918	0.212	0.562	0.212	0.555
NO. children 6-17	1.084	1.357	1.249	1.354	0.482	0.998	0.467	0.979
HH size	3.494	2.080	3.853	1.983	2.586	1.635	2.557	1.576
Any senior member	0.104	0.306	0.049	0.216	0.197	0.398	0.137	0.344
Any disabled member	0.201	0.400	0.099	0.298	0.102	0.302	0.058	0.234
Urban	0.767	0.423	0.773	0.419	0.804	0.397	0.823	0.381
High School	0.354	0.478	0.397	0.489	0.321	0.467	0.320	0.467
Some college or above	0.166	0.372	0.188	0.391	0.430	0.495	0.470	0.499
Missing education	0.011	0.104	0.012	0.107	0.008	0.089	0.007	0.082
Ln(income earned by other members/ sqrt(size))	4.431	4.035	4.282	3.914	6.650	4.546	6.763	4.542
Good MH all rounds	0.378	0.485	0.421	0.494	0.385	0.487	0.395	0.489
Excellent MH all rounds	0.145	0.352	0.195	0.396	0.235	0.424	0.256	0.436
Poor/ fair MH	0.277	0.448	0.143	0.350	0.127	0.333	0.080	0.272
Excellent MH all rounds	0.358	0.479	0.450	0.498	0.488	0.500	0.528	0.499
Poor health all rounds	0.221	0.415	0.085	0.279	0.097	0.296	0.044	0.205

Table 2-C-1. Continued.

	SNAP recipients				Non SNAP			
	All adults		Employed		All adults		Employed	
	Mean	S. D.	Mean	S. D.	Mean	S. D.	Mean	S. D.
Poor health some rounds	0.407	0.491	0.247	0.431	0.214	0.410	0.145	0.352
Excellent health some rounds	0.227	0.419	0.300	0.458	0.342	0.474	0.377	0.485
Excellent health all rounds	0.081	0.273	0.109	0.312	0.140	0.347	0.154	0.361
Good health all rounds	0.374	0.484	0.466	0.499	0.444	0.497	0.483	0.500
Any disability	0.125	0.331	0.031	0.173	0.055	0.228	0.018	0.133
Region and state level controls								
State level per capita income	35.433	6.181	35.281	6.005	36.294	6.133	36.415	6.049
State level poverty rate	13.514	2.838	13.444	2.842	12.950	2.814	12.837	2.810
State level bachelor attainment	26.050	4.263	26.002	4.044	26.656	4.290	26.685	4.296
State level housing price	1.534	0.291	1.542	0.300	1.647	0.346	1.659	0.348
State price parity index	98.604	8.968	98.217	8.613	99.681	8.979	99.458	8.835
Regional consumer price index	196.523	17.983	195.940	17.514	196.274	15.744	196.472	15.130
Instrument								
Max allotment/TFP price	0.609	0.317	0.666	0.298	0.460	0.246	0.456	0.237
Outcome variables								
Employed	0.431	0.495	-	-	0.630	0.483	-	-
Weekly hours worked								
Hours<30	-	-	0.273	0.446	-	-	0.264	0.441
30<Hours<40	-	-	0.246	0.431	-	-	0.188	0.391
Hours>40	-	-	0.468	0.499	-	-	0.535	0.499

Table 2-C-1. Continued.

	SNAP recipients				Non SNAP			
	All adults		Employed		All adults		Employed	
	Mean	S. D.	Mean	S. D.	Mean	S. D.	Mean	S. D.
Individual controls for working adults								
Employer size < 100	-	-	0.255	0.436	-	-	0.256	0.437
Employer size 100-500	-	-	0.164	0.370	-	-	0.199	0.400
Employer size > 100	-	-	0.084	0.278	-	-	0.152	0.359
Employer size missing	-	-	0.126	0.332	-	-	0.096	0.294
Union	-	-	0.046	0.209	-	-	0.099	0.299
Retirement plan	-	-	0.133	0.339	-	-	0.404	0.491
Paid vacation	-	-	0.359	0.479	-	-	0.595	0.490
Industry-transportation and utility	-	-	0.132	0.339	-	-	0.148	0.355
Industry-professional and education	-	-	0.342	0.474	-	-	0.403	0.491
Industry-other services	-	-	0.048	0.215	-	-	0.074	0.261
White collar occupation	-	-	0.440	0.496	-	-	0.579	0.494
Observations	17,097		7,247		44,968		26,912	

Note: Means are weighted to be nationally representative.

Table 2-C-2. Marginal Effect of SNAP on Employment Outcomes after Adjustment for Misclassification of SNAP Participation

	Employment	Hours		
		hour<30	(hour<40 & hour>=30)	hour>=40
Panel 1: Full sample				
SNAP	0.021	-0.217***	-0.060***	0.276***
	(0.015)	(0.028)	(0.005)	(0.033)
Panel 2: Women only				
SNAP	0.038**	-0.262***	-0.049***	0.312***
	(0.016)	(0.026)	(0.003)	(0.029)
Panel 3: Men only				
SNAP	-0.023	-0.095**	-0.040**	0.135**
	(0.016)	(0.048)	(0.018)	(0.066)

Notes: Bootstrap standard errors in parentheses based on 300 iterations are corrected for clustering at the market group level. Both regression model includes time-varying household demographic controls, state and market characteristics, market and year fixed effects.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-C-3. Marginal Effect of Variation in SNAP Purchasing Power on Employment among SNAP Eligible Adults, by Participation Status

	Maximum SNAP allotment/TFP
Panel 1: SNAP	
Full-sample	0.098**
	(0.036)
Women only	0.112***
	(0.039)
Men only	0.020
	(0.066)
Panel 2: Non-SNAP	
Full-sample	0.049
	(0.032)
Women only	0.076
	(0.059)
Men only	-0.022
	(0.030)

Notes: Bootstrap standard errors in parentheses based on 300 iterations are corrected for clustering at the market group level. The regression model includes time-varying household demographic controls, state and market characteristics, market and year fixed effects.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 2-C-4. Marginal Effect of SNAP Participation on Employment when Simplified Reporting is Used as the Instrument

	IV	F-statistic	Observations
Panel 1: Full-sample			
SNAP participation	0.034***	21.34	88,214
	(0.004)		
Panel 2: Women only			
SNAP participation	0.040***	19.3	51,614
	(0.003)		
Panel 3: Men only			
SNAP participation	0.019**	25.70	36,600
	(0.008)		

Notes: Standard errors in parentheses are corrected for clustering at the state level. The regression model includes time-varying household demographic controls, state characteristics, state and year fixed effects.
Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

CHAPTER 3

How Was Medical Care Utilization Affected by the Medicaid Primary Care Fee Bump?

3.1 Introduction

The fraction of Medicaid enrollees has almost tripled since the 1970s, from approximately 8.4 percent in 1972 to 23.6 percent in 2017 (Gruber 2003; Sommers and Grabowski 2017). However, providing coverage is not equivalent to providing access to medical care. Coverage expansions will only translate into better access to health care if the supply of providers is adequate and providers are willing to treat publicly insured patients. Historically, Medicaid offered lower reimbursement rates to providers for the same services relative to other payers (Zuckerman and Goin 2012; Zuckerman, Skopec and Epstein 2017; Zuckerman, Skopec and Mccormack 2014; Berman et al. 2002). For example, in 2012, the average state-level ratio of Medicaid-to-Medicare physician fees was 0.59 for primary care providers. This disparity in reimbursement discourages providers from participating in Medicaid, and many primary care physicians either do not accept Medicaid, or are not currently accepting new Medicaid patients (Medicaid and CHIP Payment and Access Commission 2011).

This preference for privately insured patients concerns policymakers for two reasons. First, limited access to physicians leads to less utilization of health care, and poor health outcomes by Medicaid beneficiaries (Dafny and Gruber 2005). Recent evidence further suggests that there are significant differences in treatment for the same conditions between privately insured and publicly insured patients (Alexander and Currie 2017). Given the large and increasing population of Medicaid enrollees, the costs of adverse health resulting from poorly treated or under-treated ailments can be substantial. In addition to direct medical costs, poor health is associated with lower human capital accumulation, and more limited labor market participation (Ettner, Frank and Kessler 1997; Currie and Stabile 2006; Peng, Meyerhoefer and Zuvekas 2016).

Second, a shortage of participating physicians could reduce the efficiency of health care use. In particular, if Medicaid recipients cannot gain access to physician offices, they will seek care in the emergency room (ER). This is one of the reasons why Medicaid patients rely more heavily on hospital-based sites as a "usual source of care" than do privately insured patients (Cohen and Cunningham 1995; Long, Settle and Stuart 1986; Decker 2009). Treating non-urgent and primary care preventable illnesses in the ER is more expensive than treating these illnesses in the physician's office (Baker and Baker 1994; Bamezai, Melnick and Nawathe 2005; Weinick, Burns and Mehrotra 2010).

Understanding how health care utilization responds to changes in Medicaid payments is critical for evaluating the costs, benefits, and incidence of payment changes. We aid in this understanding by exploring the impact of large increases and decreases in Medicaid provider reimbursement rates for primary care services. Much of the variation in Medicaid reimbursement rates used in the study is driven by the Medicaid fee bump, a provision of the Affordable Care Act (ACA) mandating that states raise Medicaid payments to match Medicare rates for primary care visits in 2013 and 2014. The fee bump was not re-authorized by the federal government and ended on December 31st, 2014, although some states continued to fund the enhanced rates using state revenues (Tollen 2015). Thus, the fee bump policy resulted in substantial variation in Medicaid reimbursement rates; a large increase in 2013 followed by a steep decline for most states in 2015.

Using variation in the generosity of Medicaid reimbursement, we investigate the effect of changes in Medicaid payments on the type, place, and composition of care received by Medicaid patients. We employ two complementary identification strategies to address the potential endogeneity of states' provider payment policies.[45] Our first analysis includes a regression discontinuity (RD) design that uses the Medicaid fee bump to identify the break in the utilization trend of medical services. Our second strategy is a fixed-effects (FE) model that exploits within-state variation in Medicaid payments to primary care physicians over the 2008-2015 time period for identification. In the latter, we incorporate hand-collected state-level data on fee schedules for five primary care services in order to trace out variation in state-level Medicaid payment policies before and during the Medicaid fee bump.

This paper makes several contributions to the literature. First, our estimates improve upon earlier studies that rely on limited within-state variation in payment rates over time, which may not be fully exogenous. Second, we consider the mechanisms through which higher payments to providers improve access to care. We find that physicians likely increase the supply of services through the use of substitutes such as nurse practitioners and physician assistants. No past study provides information on how changes in Medicaid payments affect provider mix. This extension sheds light on previous inconsistencies identified in the literature. In particular, several studies find that enhanced provider reimbursement rates improve access to care and health outcomes (Alexander and Schnell 2017; Polsky et al. 2015; Candon et al. 2018), but two studies find limited effects of the fee bump on physician participation in Medicaid (Decker 2018; Mulcahy, Gracner and Finegold 2018). Our results suggest that increasing the number of physicians participating in

[45] While the variation driven by the Medicaid fee bump was exogenous to states, the size of fee bump in each state reflects its pre-2013 policy in the payments to physicians. For example, states may have increased Medicaid reimbursement rates in response to serious access problems prior to the fee bump. If so, the change in Medicaid reimbursement rates might be correlated with unobserved factors that influence healthcare outcomes.

Medicaid is not necessary to enhance access to care among Medicaid patients. This mechanism is more pronounced in states with more liberal scope of practice laws governing nurse practitioners.

Thirty one states and the District of Columbia expanded their Medicaid programs by the end of our study period to include coverage for low-income adults without children. Since the expansion could also affect the supply of primary care providers, we conduct several tests to confirm that our results represent the causal effect of the payment policy changes as opposed to the ACA Medicaid expansions. First, we estimate our models using the time period before the ACA Medicaid expansions, and find similar results. This is also true when we estimate our models using the sample of children, who are relatively unaffected by the ACA expansions. Finally, we conduct a falsification test using non-primary care physicians, who should not be affected by the Medicaid fee bump.

3.2 Background

3.2.1 Medicaid Primary Care Fee Bump and Provider Behavior

The Medicaid program is jointly financed by the federal government and the states, and is administered by state agencies. Although states must follow several guidelines in order to receive federal matching funds, they have always had substantial discretion in determining Medicaid reimbursement policy, and many state Medicaid programs reimburse providers at a lower rate for the same services relative to other payers.

The ACA required states in 2013 and 2014 to raise Medicaid payment rates for primary care services to match Medicare rates. The federal government financed the full amount of the Medicaid reimbursement rate increase over this period.[46] The fee bump was intended to facilitate absorption

[46] The amount provided was equal to the difference between a state's Medicaid fees in effect in 2009 and Medicare fees in 2013 and 2014.

of new enrollees entering Medicaid through the ACA's expansion (Blumenthal and Collins 2014). Because Medicaid payment generosity varied considerably across states before the mandated fee increase, the policy had a heterogeneous impact across states (Figure 3-C-1). The federal government did not re-authorize the fee bump beyond December 31st, 2014. In January 2015, 16 states and District of Columbia used their own funds to continue to finance the enhanced reimbursement rates and 34 states reduced payment rates.

The ACA specified 146 health care services eligible for the fee bump that include primary care office visits, outpatient visits, and vaccine administration codes that are used by physicians in family medicine, general internal medicine, and pediatrics. The enhanced Medicaid rates were also available for services delivered by nurse practitioners and physician assistants under the personal supervision of a qualified physician. The Medicaid fee increase applied in managed care organizations as well as fee-for-service Medicaid programs.

A useful theoretical framework for analyzing provider responses to changes in Medicaid reimbursement rates is a model by Sloan, Mitchell, and Cromwell (1978), which allocates provider supply between private and public patients. This model predicts that higher Medicaid rates relative to private market fees will increase the number of visits supplied to Medicaid patients. In addition, the response of physicians to a Medicaid fee boost will depend on the shape of their marginal cost curves, which may depend on the availability of ancillary personnel such as nurses and physician assistants. The use of substitutes will increase the productivity of the practice and result in a more elastic supply curve (see the appendix section A1).

3.2.2 Literature Review

Several studies leveraging pre-fee bump data empirically examine the relationship between Medicaid reimbursement and physician participation in the program, and find that higher Medicaid fees are associated with a higher probability of seeing any Medicaid patients (Sloan, Mitchell, and Cromwell 1978; Decker 2007).

Studies that investigate whether this supply response leads to better access to treatment suggests that higher payment rates are indeed associated with improved access (Shen and Zuckerman 2005; Decker 2007, 2009, 2011; Buchmueller, Orzol and Shore-Sheppard 2015; Baker and Royalty 2000; Gruber, Adams, and Newhouse 1997; Cohen and Cunningham 1995; Chen 2014; Gray 2001, Sonchak 2015). A much smaller literature investigates the impact of Medicaid payments on the utilization of medical services. Decker (2009) finds that reductions in Medicaid physician fees are associated with fewer physician visits for Medicaid enrollees, and a shift from Medicaid patient encounters in physicians' offices toward hospital outpatient and emergency departments. However, Atherly and Mortensen (2014) find that increases in the Medicaid primary care fee rate have no effect on the probability of receiving a test for cancer, hypertension, or high cholesterol. Callison and Nguyen (2018) find that an increase in Medicaid payments for primary care services results in an increase in physician visits, emergency department utilization, and prescription fills.

Work specifically on the effects of the Medicaid fee bump has also been inconclusive. In an audit study, Polsky et al. (2015) find evidence that payment increases between 2012 and 2014 in 10 states were associated with increases in appointment availability for Medicaid patients, with the largest effects occurring in states with the largest payment increases. Likewise, Candon et al. (2018) find that Medicaid appointment availability declined in states that did not maintain the previously mandated higher reimbursement rates. Alexander and Schnell (2017) find that the fee bump was associated with improvements in access, better self-reported health, and fewer school days missed among beneficiaries. However, other studies have documented more limited effects of the payment increase. Decker (2018) and Mulcahy, Gracner, and Finegold (2018) find no overall increase in primary care physicians' acceptance of new Medicaid patients following the Medicaid fee bump. Similarly, Maclean et al. (2018) find no significant spillover effects on behavioral healthcare outcomes, substance use disorders, and tobacco product use.

3.3 Data

Our primary data source is the Medical Expenditure Panel Survey (MEPS). While our regression discontinuity (RD) design relies on the 2011-2014 MEPS, the fixed-effects (FE) model uses the 2008-2015 sample.

3.3.1 Medical Expenditure Panel Survey

The MEPS is a comprehensive, nationally representative survey of the U.S. civilian non-institutionalized population. It contains detailed information for each individual in the household on demographic characteristics, socioeconomic status, health status, and health insurance coverage. Respondents are interviewed about their medical care use and expenditures over the course of two years through five survey rounds. In addition, information from the household is supplemented by expenditure data collected directly from participants' medical service providers and pharmacies through a Medical Provider Component. We subset the sample to Medicaid beneficiaries, and exclude those age 65 and older, and adults under-65 who are Medicare beneficiaries.[47,48] We also analyze the privately insured population to examine whether changes in Medicaid reimbursement rates spill over into the care of the privately insured. In particular, if

[47] It is not possible in the MEPS to distinguish individuals who are enrolled in Medicaid and those who are in separate stand-alone CHIP programs.
[48] For the dual-eligible population, the physician fee received for treating patients is the Medicare-allowed charge. Medicaid covers only the deductibles and cost sharing for this amount.

primary care doctors are capacity constrained, incentivizing doctors to see more Medicaid patients could lead to worse healthcare outcomes for the privately insured.

3.3.2 Medicaid Reimbursement Rates

The primary explanatory variable in our FE model is the amount Medicaid pays doctors for new patient evaluation and management services across states and over time. Under fee-for-service, there are five Medicaid reimbursement rates for these services, each corresponding to a specific length and complexity of the visit. By contacting the Medicaid offices of all 50 states and the District of Columbia, we obtained historical fee-for-service payment data for five office visit codes (CPT: 99201-99205) that accounted for around half of fee bump-eligible claims. Our main results use reimbursement rates associated with a visit lasting approximately 30 minutes with a patient of intermediate complexity (CPT 99203).[49,50] We exclude Tennessee from our FE analysis as this state did not a have fee-for-service Medicaid program during our study period.

Figure 3-1 illustrates the variation across the U.S. and over time in primary care fees for five Medicaid reimbursement rates for new patient evaluation and management services. In 2008, the Medicaid payment for treating a new patient (CPT 99203) varied from $29 in the least generous state (New York) to $133 in the most generous state (Alaska). The range tightened considerably in 2013 (in the first year of the primary care rate increase), varied from $98 to $174. Since we include state and time fixed-effects in our models, the identifying variation comes from within-state changes in Medicaid payments over time. While there were some changes in Medicaid

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[49] We test the sensitivity of our estimates to different definitions of Medicaid reimbursement rates and come to similar conclusions. In particular, we also estimate our models using the rates associated with other CPT codes as well as the average of the five reimbursement rates.

[50] Many existing studies rely on the Medicaid-Medicare fee index (see, for example, Decker 2007, 2009; Zuckerman and Goin 2012; Callison and Nguyen 2018; Maclean et al. 2018). We do not use the Medicaid-Medicare fee index in our study for two reasons. First, the data are only available on 1998, 2003, 2008, and 2012. Second, using a Medicaid-Medicare ratio may measure changes driven by fluctuations in Medicare prices instead of Medicaid prices.

payment rates prior to 2013, most of the variation comes from the primary care rate increase mandated by the ACA, followed by the large decline in 2015.[51]

Around 70 percent of Medicaid beneficiaries were enrolled in managed care plans over our analysis period. Although we know how much doctors were reimbursed under both fee-for-service and Medicaid managed care plans in 2013 and 2014, we do not know the level of managed care payments of physicians prior to 2013 or after 2014. Therefore, we impute Medicaid managed care plan payments before and after the fee bump, using a similar approach to Alexander and Schnell (2017). We describe this procedure in greater detail in the appendix section A2.

3.3.3 Control Variables

Control variables in each model include age (dichotomous indicators for age 6–17, 18–24, 25–34, 35–44, 45–54 , 55–64 with age 0–5 as the omitted category), gender, marital status, race and ethnicity (Hispanic, black, and other race with white as the omitted category), urban residence, education (high school diploma, some college, college degree, with less than a high school degree omitted), number of children in the household (under age 5, 5–18), the log of total family income normalized by the square root of household size, and whether the individual has a paid sick leave benefit. In order to control for health status, we use self-reported mental and physical health (poor/fair health in all rounds, poor/fair health in some rounds, excellent health in some rounds, excellent health in all rounds, good/very good health in all rounds, and self-reported health is missing, with good/very good health in some rounds serving as the omitted category for both mental and physical health), and a measure of disability status. The latter is a binary variable that

[51] Although the federal government mandated that states increase their Medicaid payments to primary care providers starting on January 1st, 2013, many states experienced implementation delays. We follow Alexander and Schnell (2017) and do not incorporate such delays into our Medicaid payment variable. This decision is based on the assumption that affected physicians expected to receive ACA adjusted payments in the future from Medicaid.

indicates whether the person had an IADL (Instrumental Activities of Daily Living) or ADL (Activities of Daily Living), functional, activity, or sensory limitation in any interview round.

To account for area-level attributes, we control for several variables at either the county or state level that could be correlated with both healthcare outcomes and Medicaid fees. These include the county-level per capita supply of general practitioner, nurse practitioners, pediatricians, hospital beds, as well as median household income, unemployment rate, percentage of bachelor's degree for persons 25 years of age and older, state-level managed care penetration rate, and whether area is underserved for primary care services (Mathematica Policy Research 2017; Health Resources and Services Administration 2017a,b). Finally, we include an indicator for the ACA Medicaid expansion following Maclean, Pesko and Hill (2017). The inclusion of a Medicaid expansion control is particularly important as this policy may have altered the composition of enrollees.

Table 3-1 reports summary statistics for our main explanatory variables, as well as individual and county-level controls by patient insurance type. Relative to the privately insured, Medicaid beneficiaries have lower income and education levels, live in larger families, are less likely to be married, and are more likely to be black and Hispanic. Furthermore, respondents covered by Medicaid live in areas that are underserved by primary care providers and have fewer health care providers per capita.

3.3.4 Outcome Variables

Our main outcomes are derived from the MEPS medical event files. We classify medical visits into whether the individual had any visits to a primary care physician, physician specialist, or non-physician provider. Primary care physicians are general practitioners, family practitioners, internists, and pediatricians. Physicians in any other specialty are considered specialists. Non-physician providers are separated into midlevel primary care providers, which for our study are comprised of nurses, nurse practitioners, and physician assistants, and all other providers (chiropractors, or physical and occupational therapists). For each of these types of visits, we study the number of visits, and expenditures.

Figure 3-C-2 shows the utilization rates for office-based visits with these four provider types by the type of insurance coverage. Medicaid beneficiaries have a slightly higher likelihood of visiting a primary care provider in the past year compared to the privately insured. Likewise, among those with some utilization, Medicaid respondents report having 0.5 more primary care physician visits in the last year compared to individuals with private insurance.

3.4 Econometric Models

3.4.1 Regression Discontinuity Design

Our first estimation approach exploits unexpected, exogenous variation in the generosity of Medicaid reimbursement rates for primary care providers driven by the Medicaid fee bump. In essence, this approach involves comparing the healthcare outcomes of Medicaid beneficiaries just prior to January 1, 2013 (immediately before the Medicaid fee bump was implemented) with healthcare outcomes just after January 1, 2013 (immediately after the Medicaid fee bump was enacted). This strategy is motivated by the idea that characteristics related to outcomes of interest vary smoothly across this treatment threshold; therefore, any discontinuity in medical care utilization can be reasonably attributed to the sharp change in Medicaid reimbursement rates.

Our RD design takes the form of an interrupted time-series model. In particular, we estimate the following model:

$$Y_{ist} = f(\Delta) + \beta Post_{2013} + X'_{ist}\gamma + \delta_m + \delta_s + \varepsilon_{ist}, \tag{3-7}$$

where Y_{ist} is a healthcare outcome for individual i living in state s in month t[52], X_{ist} is a vector of socio-demographic and health status variables, $Post_{2013}$ is a dummy that takes the value one for fee bump months, δ_m is a vector of month fixed effects in a year, and δ_s is a vector of state fixed effects. $f(\Delta)$ is a smooth function of time in months from the cutoff, which represents the trend in Medicaid payments. We use a linear function of time, fully interacted with the post dummies as a baseline specification. As a robustness check, we add quadratic terms. Because the fee schedule changed again in many states in January 2015, we do not use any observations after December 2014, and, for symmetry, we do not use any data from before January 2011. Results from narrower bandwidths yield nearly identical results, and are discussed in the results section. The Standard errors are clustered on the months from the cutoff (Carr and Packham 2019).

When the outcome variable is the number of visits, or medical spending, we use a two-part model (Jones 2000). To account for a mass point in the utilization distribution at zero, the first part estimates the probability of having any medical visits (the extensive margin). The second part estimates the number of visits or amount of medical expenditures among medical care users (the intensive margin). The first part of the two part model is specified as:

$$Pr(I_{ist}=1) = f(\Delta) + \beta_1 Post_{2013} + X'_{ist}\gamma_1 + \tau_m + \delta_s, \tag{3-8}$$

Likewise, the second part of the two part model is the log of medical visits or expenditures for the sample of individuals with medical care use, specified as:

$$\log(Y_{ist}|I_{ist}=1) = f(\Delta) + \beta_2 Post_{2013} + X'_{ist}\gamma_2 + \tau_m + \delta_s + \varepsilon_{ist}. \tag{3-9}$$

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[52] We use information on the date of each medical visit to construct monthly-level data on medical care utilization.

Equations (3-2) and (3-3) can be combined to derive the unconditional mean of medical visits or expenditures as follows:[53]

$$\mathrm{E}[\log(Y_{ist})] = Pr(I_{ist} = 1) \times \mathrm{E}[\log(Y_{ist}) | I_{ist} = 1]. \tag{3-10}$$

The unconditional marginal effect of interest is derived as follows:

$$\begin{aligned} ME_1 = {} & Pr(I_{ist} = 1 | \, Post_{2013} = 1) \times \mathrm{E}[\log(Y_{ist}) | I_{ist} = 1, Post_{2013} = 1] - \\ & Pr(I_{ist} = 1 | \, Post_{2013} = 0) \times \mathrm{E}[\log(Y_{ist}) | I_{ist} = 1, Post_{2013} = 0]. \end{aligned} \tag{3-11}$$

The standard error of overall marginal effect is calculated using 300 bootstrap iterations.

3.4.2 Fixed-Effects Specification

Our second specification is a reduced-form FE model of the effect of Medicaid payment rates for primary care providers on healthcare outcomes. In this case, the estimates are identified by within-state variation in Medicaid payment rates over time that deviates from a linear trend. In particular, we estimate the following model:

$$Y_{ist} = \beta Fee_{st} + X'_{ist}\gamma + Z'_{st}\theta + \delta_t + \delta_{0s} + \delta_{1s}t + \varepsilon_{ist}, \tag{3-12}$$

where Fee_{st} is the 12-month average Medicaid fee schedule for primary care services in state s in year t, Z_{st} is a vector of area-level controls, δ_t is a vector of year fixed effects, δ_{0s} is a vector of state fixed effects, and $\delta_{1s}t$ is a vector of state-specific linear time trends. Standard errors are clustered by the state to account for serial correlation of the errors within states over time (Bertrand, Duflo and Mullainathan 2004). In this equation, our coefficient of interest, β represents the effect of a $10 fee increase on outcome variable Y_{ist}.

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[53] To avoid the re-transformation problem, we use the log form of the dependent variable, and interpret the marginal effects as percentage changes.

Similar to our RD design, when the outcome variable is the number of visits, or medical spending, we use a two-part model. In this case, the unconditional marginal effect of interest is derived as follows:

$$ME_2 = Pr(I_{ist} = 1) \times \beta_2 + \beta_1 \times \mathrm{E}[\log(Y_{ist})|I_{ist} = 1]. \tag{3-13}$$

3.5 Results

3.5.1 Regression Discontinuity Results

We first examine the effect of the Medicaid fee bump on monthly-level utilization of office-based primary care physician visits using the RD design. Figure 3-2 presents graphical evidence that the fee bump affected visits to different provider types. Each figure plots the monthly mean of the log of visits (after differencing out state and month fixed effects).[54] The months to the left of the vertical line are before the policy change, and the months to the right of the vertical line are after the policy change when enhanced rates were in effect. Visits to primary care physicians and mid-level primary care providers both exhibit a jump after the Medicaid fee bump.

Table 3-2 contains RD estimates of the Medicaid fee bump on medical care utilization based on regression models described in Equations (3-2) and (3-3). We measure utilization in three ways. First, we analyze the extensive margin of utilization, using an indicator variable equal to one if the individual visits any provider in a month and zero otherwise. Second, we analyze the log of number of visits conditional on use. Third, we present overall marginal effects from combing the first part and the second part of the model as indicated in Equation (3-5). Our main specification uses a 24 month bandwidth on either side of the Medicaid payment policy change.

[54] These fixed-effect account for time-series seasonality in visits that are not due to the Medicaid fee bump.

Increased physician reimbursement under Medicaid increases the number of visits to primary care physicians by 1.3 percent at the intensive margin. The number of visits to mid-level primary care providers increases substantially, by 15.8 percent. Likewise, we find that the Medicaid fee bump increased total Medicaid spending on visits to primary care physicians by 16.7 percent, and spending on visits to mid-level providers by 44.6 percent, both at the intensive margin. The observed increase in mid-level provider visits is larger in percentage terms than the increase in visits to physicians, indicating that making greater use of mid-level primary care providers is an important part of how practices met the new demand created by the Medicaid policy change. As shown in Column (3), on average there is no statistically significant effect of the Medicaid fee bump on the number of primary care physician visits at the extensive margin.

The extent to which higher Medicaid payments translate into better access to care among Medicaid patients will depend to some extent on the availability of other resources. Increasing Medicaid payments to primary care physicians should impact healthcare access most in areas with an insufficient supply of primary care providers, where there may be a pent up demand for primary care services. We investigate whether the Medicaid fee bump had a greater influence on access to care among Medicaid enrollees who live in areas designated as primary care Health Professional Shortage Areas (HPSAs) (HRSA 2017b).[55]

To conduct this analysis, we interact our post-policy indicator in the RD design with a dummy variable indicating that county c in year t is underserved for primary care services. As shown in column (4), individuals in counties that are underserved by primary care physicians experienced a statistically significant increase of 1.5 percentage points in the probability of visiting

[55] Primary care HPSAs are defined by the Department of Health and Human Services as geographic areas where the ratio of full-time primary care physician to population is less than one primary care physician per 2,000 individuals. For details of shortage designation criteria, see https://bhw.hrsa.gov/shortage-designation/hpsas.

a primary care physician at least once in a month. The only significant change in the number of visits to other non-physician providers is at the extensive margin in shortage areas.

We also investigate the impact of Medicaid fee bump on visits to non-primary care physicians, who should not be affected by the Medicaid fee bump. This is because the Medicaid fee bump only increased rates for primary care services. The last panel of Table 3-2 displays marginal effects on the number of visits to other specialists. We find no change in the number of visits by Medicaid patients. This falsification test suggests that the effects of the fee bump on primary care visits resulted from the Medicaid payment policy, and not from contemporaneous changes in the health care system.

We also test for spillovers to individuals with private insurance and incomes above 400% of FPL, who may be indirectly affected by Medicaid payment increases (Table 3-C-1). There are no significant effects of the Medicaid fee bump on visits by privately insured patients, suggesting that physicians were able to increase care to the new patients without reducing the amount of care they provided to other patients.

We explore the robustness of several aspects of our RD results. To test bandwidth sensitivity, we replicate the models under a range of bandwidths. We test bandwidths from 12 months on either side (i.e. prior to the ACA Medicaid expansion) to the full 24 months used in the main specification in increments of one month at a time. Figure 3-3 presents the coefficients and standard errors from models using each of these alternative bandwidths. For both visits to primary care physician and visits to mid-level providers, the estimated coefficient on the policy change is stable across the different bandwidths and is nearly always statistically significant at the 5% level.

When we use alternative specifications for the running variable, including linear and quadratic, results are nearly identical (Table 3-C-2).[56]

We also investigate whether covariates varied significantly at the time of the policy change in order to validate the basic assumption of the RD specification. If the sample before and after the cut-off was different, then our RD estimates may reflect the change in the composition of the sample, not the effect of the Medicaid fee bump policy itself. All but one variable show no differential change at the January 2013 policy implementation date (Table 3-C-3).[57]

Finally, we check whether the running variable was being manipulated across the cutoff. Our RD design will over-estimate the treatment effect if providers were systematically re-scheduling visits until after the fee bump took effect. We present a histogram of visits to primary care providers to check whether abnormal heaps occur to the left- or right-hand side of the cutoff. As can be seen in Figure 3-C-3 this does not appear to be the case.

3.5.2 Fixed-Effects Results

Figure 3-C-4 shows positive associations in the raw data between increases in state-level Medicaid primary care rates and changes in the office-based primary care physician visits among Medicaid beneficiaries from before the rate increase (2011-2012) to after the rate increase (2013-2014). We use variation in Medicaid payment rates over an 8-year period to identify our FE models.

Table 3-3 presents these results in regression form using the FE specification.[58] The estimates are scaled to measure the effect of a $10 increase in Medicaid payments. We find that

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[56] We find similar results for polynomials of order 3. These results are available upon request.
[57] Since we do not have monthly demographic data, we are not able to examine whether the covariates vary smoothly at the cutoff in the regression framework of equation (1).
[58] We note that we are not able to estimate our FE model to investigate the impact of higher fees on the utilization of visits with mid-level providers. This is because we do not have data on payments to mid-level providers. Close to half of state Medicaid programs pay nurse practitioners the same rate that they pay physicians for the same services (Kaiser Family Foundation 2011). Other states pay 85% or a different share of the physician payments. Further, some states differentiate based on the service.

this increase in payments is associated with an increase in the total number of visits to primary care physicians by 1.7 percent. On the extensive margin, a $10 increase in payments is associated with an increase in the fraction of individuals having at least one office-based primary care physician visit of 0.6 percentage points (1 %). On the intensive margin, the point estimates imply that a $10 increase in Medicaid payments increases the number of visits by 1.1 percent. This increase is more pronounced in HPSAs. In particular, visits to office-based physicians increase by an additional 0.7 percent in HPSAs relative to non-HPSAs (Column (2)). Table 3-3 also examines the impact of a $10 increase in physician reimbursement on health care expenditures for primary care visits. The overall marginal effects imply that a $10 increase in physician reimbursement is associated with an increase in total medical spending of 5 percent. Similar to findings from the RD design, we find no statistically significant evidence that visits by non-Medicaid enrollees were affected by the fee bump.

To make these estimates more comparable to our RD results, we consider the implied change associated with an increase in payments of $40, which corresponds to the average fee increase in payments under the Medicaid fee bump across states from the third quarter of 2012 to the first quarter of 2013. Multiplying the point estimates in Table 3-3 by four, reveals that an increase of $40 in physician reimbursement is associated with an increase of 6.8 percent in the number of visits to a primary care physician's office in a year and a 20 percent increase in medical spending.

One concern about the validity of our FE model is that states may have increased Medicaid reimbursement rates in response to serious access problems prior to the fee bump. If so, the magnitude of fee bump in each state might be correlated with unobserved factors that influence healthcare outcomes. While the inclusion of state fixed effects can account for unobservable factors that are state-specific and time invariant, there is still the concern that (nonlinear) time-varying unobserved factors could result in biased estimates. One way to test for endogeneity bias in the FE specification is to investigate whether the strict exogeneity assumption of the model is valid. To do so, we re-estimate our FE model including the first period lead of state's Medicaid fees in effect in $t+1$. Finding a significant effect of future Medicaid payment rates (while controlling for current payment policy), could suggest we might be capturing the effects of some other state-level trend. Table 3-C-4 suggests that we cannot reject the null hypothesis that the coefficient of the lead variable is zero. Importantly, the magnitudes of the estimated coefficients for the contemporaneous effects are quite similar to those in Table 3-3.

As another robustness check, we investigate whether the monotonicity assumption is valid. Under monotonicity assumption, the effects must be larger among states that saw a bigger payment increase in 2013 than in states that saw a smaller payment increase. To do so, we classify states as "high" and "low" fee bump states depending on if they have an above or below the median fee increase between 2012 and 2013. Figure 3-C-5 presents trends in primary care physician visits (after differencing out individual and state characteristics)[59], which shows that the trends in outcomes were similar between two groups before 2013, and after the fee bump took affect the increase in the outcomes was larger in the higher fee bump states, and the decline in the outcomes was also larger after the higher fees were phased out.

Next, we investigate whether improvements in access to primary care services that we observe being driven by people newly eligible for Medicaid. In order to determine whether this is the case, we analyze our results across the following subsamples: children only, adults only, and families with children. The sample of childless adults is subject to increased Medicaid enrollment

[59] In particular, we estimate the following specification $Y_{ist} = X'_{ist}\gamma + Z'_{st}\theta + \delta_{0s} + \delta_{1s}t + \varepsilon_{ist}$ for the two groups of states. The residuals of this regression are retained and used to calculate the residualized mean of primary care physician visits for the two groups of states over time.

and a compositional shift due to the ACA Medicaid expansion, while the samples of children and adults with children are less likely to be affected by the ACA. Estimated effects in the sample of children and adults with children who are always eligible for Medicaid are broadly comparable to the main results (Table 3-C-5).

Finally, we explore the robustness of our findings across other time horizons. In particular, we estimate our main specifications after dropping (i) the 2008-2010 time period prior to the ACA rate increase, and (ii) the post-fee bump period of 2015, when the rate increase was phased out and some states went back to their pre-ACA fee levels. The results are reported in the last two panels of Table 3-C-5. While we lose precision in some specifications, the direction and magnitude of the relationship between the Medicaid fee and outcomes are generally consistent.

3.6 Potential Mechanisms

3.6.1 Provider Supply

The results in Table 3-2 imply that an important way that health care providers may respond to changes in payment levels is to make greater use of other health professionals, in this case physician assistants and nurse practitioners. We further investigate this mechanism using data on county-level per capita supply of primary care physicians, physician assistants, and nurse practitioners from the Area Health Resource File.[60] The results in Table 3-C-6 show that a $10 increase in Medicaid payments is associated with an increase in the supply of physician assistants and nurse practitioners. However, there is no significant effect of higher Medicaid payments on the per capita supply of primary care physicians.

[60] Unfortunately, the AHRF does not contain information on the number of providers participating in Medicaid.

3.6.2 Heterogeneous Effects by Scope of Practice Laws

These results suggest that one way to supply more Medicaid visits is to make more effective use of mid-level providers, or effectively expanding the scope of practice of physician assistants and nurse practitioners. However, the degree to which a nurse practitioner can practice without direct supervision of a physician varies across states. For example, in the state of Washington, nurse practitioners can provide services independently without the oversight of a physician, making them "perfect substitutes" for physicians in the production of primary care services. In contrast, California requires that nurse practitioners perform their duties under the direct supervision of a physician. One policy that is particularly relevant to our analysis is the legal permission for nurse practitioners to practice independently. We use this measure provided by state Boards of Nursing, to see if the effects of Medicaid fee changes differ across states with more liberal scope of practice environments.

We present RD results in Table 3-4, which show that the Medicaid fee bump has no significant effect on visits with primary care physicians in states where nurse practitioners have greater autonomy. We find similar results from the FE regression model (Table 3-C-7). We also find the increase in visits with nurse practitioners is larger in states where nurse practitioners can practice independently, although the difference in the effect is not statistically significant.

3.6.3 Place of Visit

Our finding that enhanced payment rates increase office-based primary care physician visits could reflect better access to physician offices as a substitute for care received in hospital settings. In this section, we analyze whether higher physician payments affect the composition of ER visits, in particular. Any decrease in ER visits should be concentrated among those conditions that are not urgent and can most easily be treated by primary care providers.

The New York University Emergency Department (Billings, Parikh and Mijanovich 2000b) visit severity algorithm classifies all ER visits into the following general categories based on the patient's diagnostic code:

(1) Non-emergent[61]

(2) Emergent/Primary Care Treatable[62]

(3) Emergent/Preventable[63]

(4) Emergent/Not Preventable.[64]

High levels of emergency visits in categories (1) through (3) suggest that an individual has limited access to other sources of regular care besides the ER (Billings, Parikh and Mijanovich 2000a).

Because it is not possible to ascertain with certainty the degree to which an ER visit was emergent and/or preventable, we assign to each visit a probability of being in each of the categories based on the first diagnosis code. We then estimate a RD regression similar to Equation (1), but we replace the dependent variable with the probability the visit falls into a given category. We find no statistically significant evidence that the primary care fee bump changed utilization of either type of ER visit (Table 3-5).

Our estimates from the FE regression model are reported in Table 3-6. Our results suggest that higher physician reimbursement under Medicaid is associated with a 0.3 percentage point (4.7%) reduction in ER visits classified as non-urgent. However, we find no statistically significant evidence that increasing payments to primary care physicians changes utilization of either primary

[61] Medical care not needed within 12 h (e.g., sore throats).
[62] Medical care needed within 12 h but safely treatable in a primary care setting (e.g., an ear infection).
[63] ER care needed but the patient could have avoided the medical issue if they had received timely and effective outpatient care (e.g., an asthma attack).
[64] ER care needed, not preventable (e.g., a cardiac dysrhythmia).

care treatable, preventable or non-preventable ER visits. Furthermore, increases in reimbursement rates in HPSAs lead to a statistically significant (0.4 percentage point, 4.3%) reduction in emergent, primary care treatable visits in the ER. We next consider visits to hospital outpatient departments, which could also be affected by the fee bump (Row (1) of Table 3-6). Higher reimbursement in HPSAs seem to cause a shift in primary care visits from outpatient departments to physician offices.

3.6.4 Qualitative Measures of Access

In order to determine whether higher Medicaid payment rates changed the quality or appropriateness of health care services, we analyze several qualitative measures of access to care, including whether the respondent has a usual source of care, past year abilities to receive medical treatment and receive it without delay, and measures of patient-perceived quality of care.[65]

Using the FE model, we do not find any evidence that higher Medicaid fees improve access to a usual source of care (Table 3-7). However, a $10 increase in Medicaid payments is associated with 0.2 percentage point (8.3%) reduction in the probability of being unable to receive treatment and a 0.2 percentage point (5.9%) reduction in the probability that the person was delayed in receiving treatment. In addition, there is a reduction of 0.2 percentage points (7.9%) in the probability of being unable to receive treatment, when physician reimbursement rates are raised by $10 in HPSAs.

We also find that individuals are 0.8 percentage points (1.1%) more likely to report their usual source of care helped in making decisions, and 0.4 percentage points (0.4%) more likely to report the provider explained all options, when physician reimbursement increased by $10.

[65] Unlike medical utilization data, other measures of access are not asked immediately before and after the fee bump took effect. Thus, we are unable to use RD design for those outcomes.

However, we do not find any significant change in the probability that the usual source of care asked about the medication other doctors may give or respected alternative treatments. Overall, these results suggest that higher Medicaid payments are associated with improvements in access measures among Medicaid beneficiaries, and that providers do not see more patients when payments increase by making appointments shorter.

We also check whether higher payments are associated with better levels of self-reported health among Medicaid beneficiaries. As shown in the third panel of Table 3-7, a $10 increase in physician reimbursement is associated with a 0.3 percentage point (2.3%) reduction in the probability of reporting fair or poor health and a 0.7 (1.4%) percentage point increase in the probability of reporting excellent health. The relationship between measures of mental health and provider reimbursement is not statistically significant.

Next, we examine the relationship between Medicaid primary care physician rates and several important preventive care outcomes. Estimates suggest that higher reimbursement rates are associated with a greater likelihood of getting a flu shot (1.2 percentage points). However, we find no statistically significant effects on the likelihood of checking blood pressure or cholesterol in the past 12 months. We do note that higher rates of flu shot might be an indication that vaccine administration codes were also eligible for the enhanced rates rather than better access to a usual source of care.

Finally, we examine whether higher provider payments are associated with an increase in the utilization of diagnostic tests.[66] As shown in the bottom panel of Table 3-7, a $10 increase in physician reimbursement results in a significant reduction in the use of lab tests during a primary

[66] Unlike other measures of access to care, analyses of utilization of diagnostic tests are at the visit level.

care visit, which is consistent with improved access to usual source of care. However, we find no significant change in the use of x-rays.

3.6.5 Prescription Drugs

In this section, we study potential spillover effects on prescription drug use. Table 3-8 displays results from this analysis using the FE model. We find that a $10 in Medicaid fees is associated with a 0.8 percentage point (1.6%) increase in prescription drug fills. In addition, there are statistically significant increases in utilization for certain drug classes, particularly those relevant to shorter-term acute conditions. A $10 increase in Medicaid payments results in a 0.6 percentage point (3.7%) increase in the use of antibiotics, while the use of respiratory/allergy medications increases by 0.5 percentage points (2.8%), and the use of psychotherapeutic prescription drugs increases by 0.5 percentage points (5%). The increase in the use of medications for mental health conditions that can be prescribed by primary care physicians is consistent with the finding that behavioral healthcare provider participation in Medicaid is particularly scarce (Buck 2011; Bishop et al. 2014). Meanwhile, there is no significant change in the use of cardiovascular medications (those for high blood pressure, high cholesterol, or heart disease) and medications used for treating diabetes.

3.7 Discussion and Conclusion

In this study, we examine the effects of the federal mandate that substantially increased Medicaid reimbursement for a range of common primary care services. The rate boost was significant and increased Medicaid rates by 73% (Zuckerman and Goin 2012). To the best of our knowledge, no study has investigated the effect of the ACA Medicaid fee bump on the use and composition of medical care services.

Our results suggest that higher Medicaid fees increase the number of primary care visits among Medicaid patients without decreasing the amount of care provided to the privately insured. We find evidence that medical practices accomplish this by using substitutes for physicians such as nurse practitioners. This mechanism is consistent with the finding that dentists supply more visits by making greater use of dental hygienists when states expand coverage of dental services to adult Medicaid beneficiaries (Buchmueller, Miller and Vujicic 2016). Given established workforce shortages within the Medicaid healthcare delivery system, the use of mid-level providers to meet the greater demand for care among Medicaid enrollees is not surprising. For example, it has been estimated that nurse practitioners can safely provide 70-80% of the care provided by physicians (Scheffler, Waitzman and Hillman 1996).

In addition to being able to access services, Medicaid patients may receive higher quality care when payment rates increase. Given the wide range of services covered by the fee bump, patients may have access to a broader set of services that allows for better and more comprehensive care. We explore this possibility through the use of indicators for unmet need and patient perceived quality of care. We find evidence that higher reimbursement rates improve patient-provider interactions. For example, physicians spend more time with Medicaid patients and provide more counselling on treatment options.

While the costs of the federal Medicaid fee bump were non-trivial over a two year period (Medicaid and CHIP Payment and Access Commission 2015), we find some evidence that higher reimbursement rates may result in more efficient allocation of health care services. For example, our estimates indicate that a $10 increase in Medicaid reimbursement to primary care providers is associated with a 4.7% reduction in ER usage for non-urgent care. In 2008, there were 25.1 million Medicaid ER visits, and 4.5 per 100 visits were classified as non-urgent (Sommers, Boukus and

Carrier 2012). Using Baker and Baker's (1994) overall excess charge estimate of $93.85 for use of the ER in a non-urgent situation, we conclude that a 4.7% reduction in ER use results in savings of over $10 million per year.[67] We also find that more generous Medicaid payments are associated with greater use of prescription drugs, but fewer ancillary services such as laboratory tests. The greater use of prescription drugs provides some indication that additional primary care services were necessary and led to actionable treatment plans. In addition, a reduction in lab tests could indicate that patients were better able to schedule appointments with the same provider or practice group.

Our study has limitations that must be recognized. The use of a longer time horizon in the FE model provides a larger sample and more identifying variation in Medicaid payment rates, but the decision to change Medicaid reimbursement in these models may not be exogenous. In addition, the rise of Medicaid managed care has made it difficult to know how much doctors are actually reimbursed through Medicaid. Although we address this problem by imputing payments for Medicaid managed care plans, measurement error may affect the magnitudes of our estimated marginal effects. Although the RD design addresses both of these concerns simultaneously by restricting our analysis to the exogenous change in the generosity of Medicaid reimbursement driven by the ACA fee bump, the temporary nature of the policy may have muted provider responses. Another limitation of our RD design is that we are averaging a policy effect over the states with a large Medicaid fee bump with a null effect over several states with small/no change in the fee, which might explain why our RD estimates are broadly smaller in magnitude than results from the FE model. Finally, the fee bump was not applied to all primary care services, but instead

[67] Calculated from: 25.1 million * 0.045* 93.85 * 2.1*0.047= $10.5 million. The 2.1 is the CPI index converts the Baker and Baker (1994) estimate from 1987 dollars to 2013 dollars.

only to payments associated with certain procedure codes that were eligible for a fee increase. However, the MEPS does not contain information to identify which events were eligible for the fee increase.

Several policies have focused on creating stronger financial incentives to address concerns about a growing physician shortage (Petterson et al. 2012; Hofer, Abraham and Moscovice 2011). By analyzing the impact of the Medicaid fee bump on utilization, this research contributes to the ongoing debate about the role of Medicaid provider payments in access to care. Our results suggest that providers change the way they practice on several margins when faced with a large change in payment levels. At least in the market for primary care, we find that enough flexibility exists to adjust capacity in order to treat higher levels of Medicaid patient demand. Our results further suggest that individuals who live in areas that are underserved by primary care physicians benefit more from higher Medicaid payments. Thus, policymakers may wish to consider prioritizing areas with an under-provision of primary care physicians for payment increases to help mitigate the negative consequences of physician shortages.

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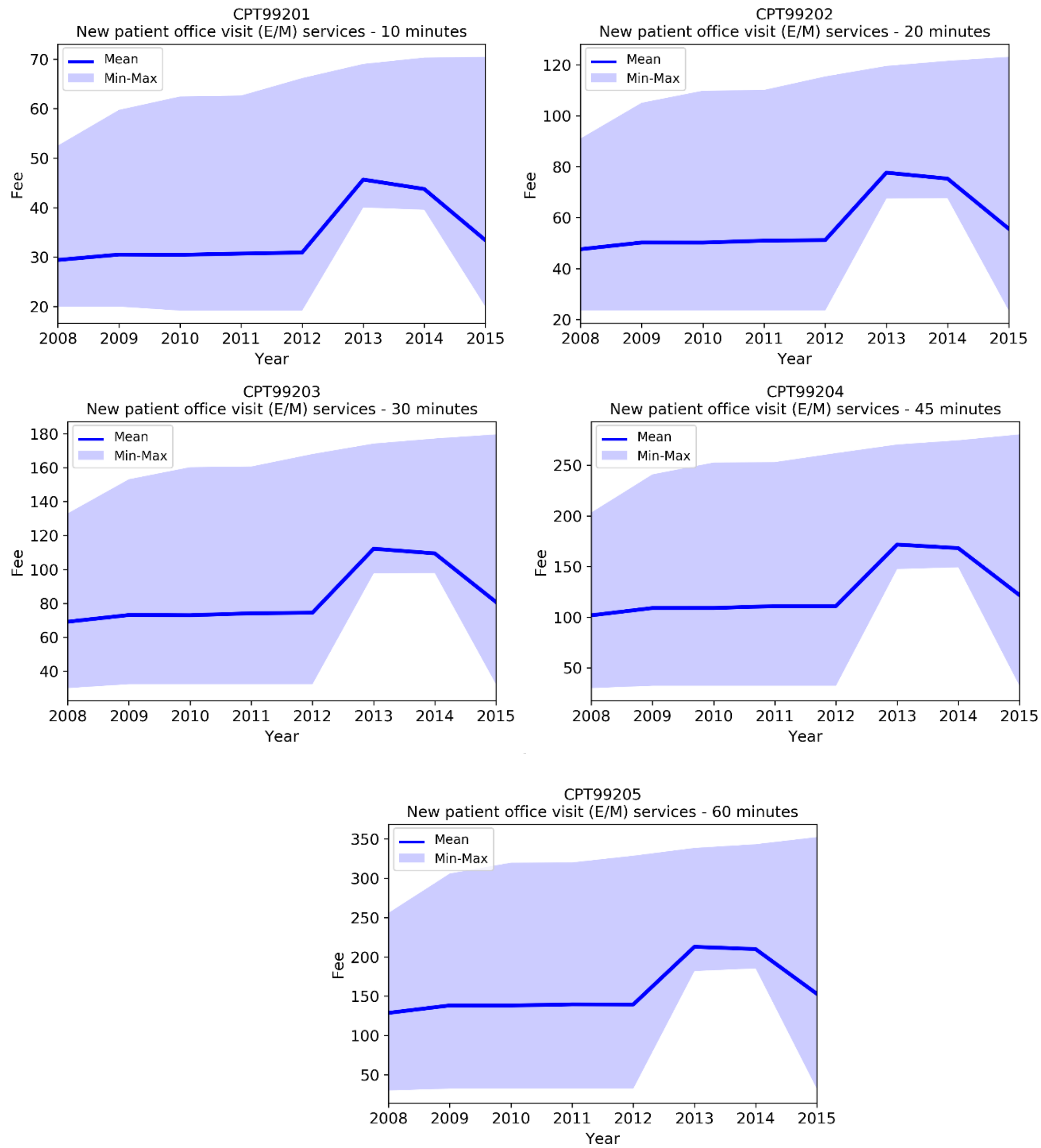
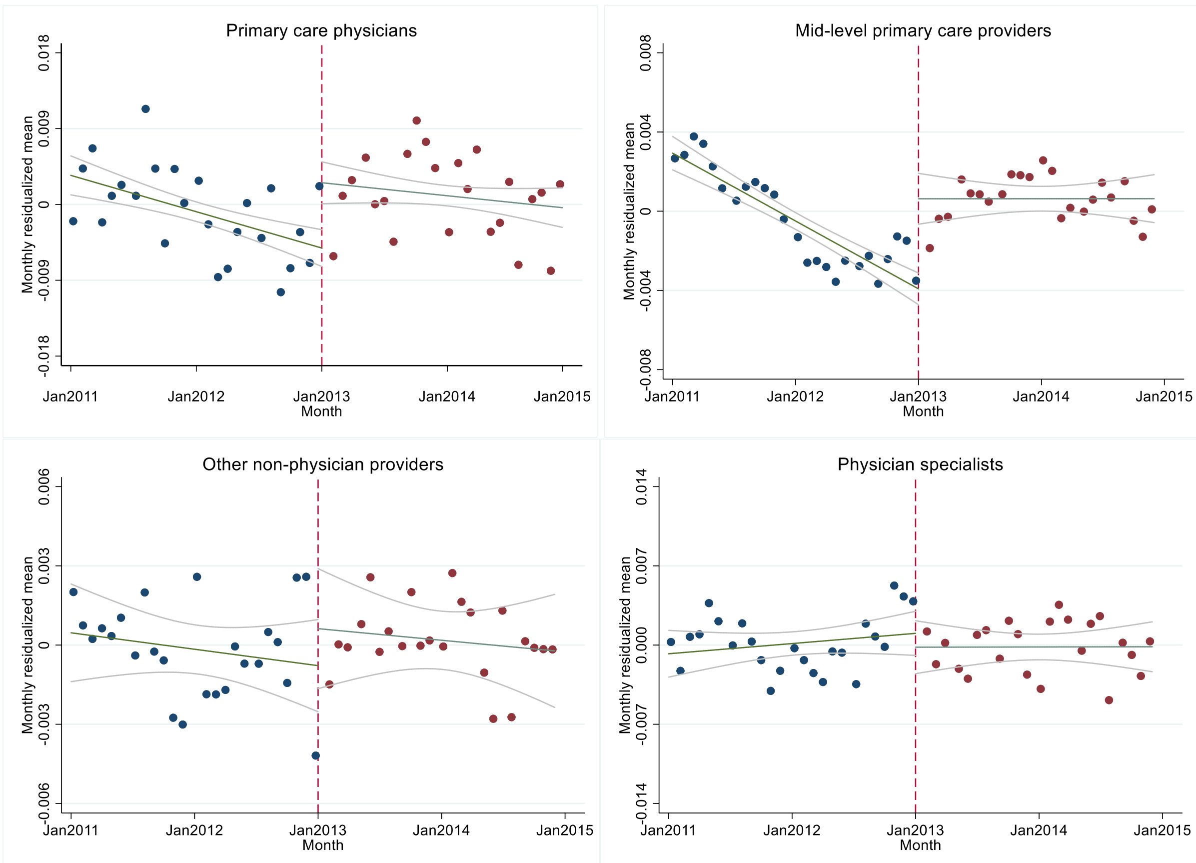
Figure 3-1. State-level Medicaid fees for new patient primary care services over time



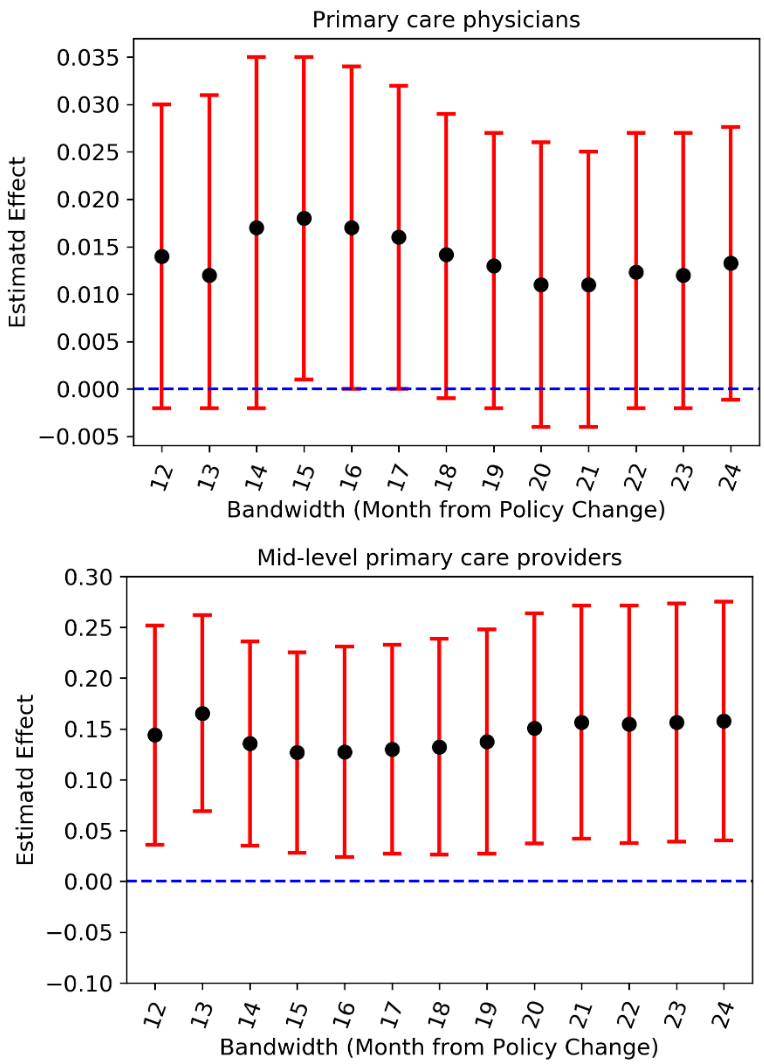
Notes: The above figure depicts averages of Medicaid payments with minimum and maximum for new patient evaluation and management services (CPT 99201-99205) across states from 2008 to 2015.

Figure 3-2. Effect of the Medicaid fee bump on the number of office-based visits, by provider type



Notes: The monthly residualized mean (accounting for state and month fixed effects) of the log of visits to different providers. Primary care physician visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Physicians in other specialty are considered physician specialists. Mid-level primary care providers are visits with nurses, nurse practitioners, and physician assistants. All other non-physician providers are considered other providers.

Figure 3-3. Robustness check, RD estimates for different bandwidths



Notes: Each dot represents the coefficient of interest generated by a separate regression. The various bandwidths on which these regressions were performed are represented on the x-axis. We also report the 95% confidence interval of the coefficient. Primary care visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Mid-level primary care providers are visits with nurses, nurse practitioners, and physician assistants.

Table 3-1. Descriptive statistics by insurance coverage, MEPS 2008-2015

	Medicaid		Private	
	Mean	Std. Dev.	Mean	Std. Dev.
Medicaid fee (CPT 99203)	76.955	26.281	74.500	26.068
Individual-level controls				
Age 0-5	0.232	0.422	0.066	0.249
Age 6-17	0.429	0.495	0.170	0.376
Age 18-24	0.083	0.276	0.095	0.293
Age 25-34	0.082	0.275	0.148	0.355
Age 35-44	0.066	0.249	0.167	0.373
Age 45-54	0.060	0.238	0.189	0.391
Age 55-64	0.040	0.196	0.163	0.370
Female	0.541	0.498	0.506	0.500
Hispanic	0.330	0.470	0.111	0.314
Black	0.250	0.433	0.098	0.298
Other race	0.036	0.186	0.052	0.222
Married	0.093	0.290	0.478	0.500
No. of children 0-5	0.722	0.896	0.306	0.631
No. of children 6-17	1.481	1.312	0.813	1.057
Urban	0.828	0.377	0.875	0.331
High school/GED	0.089	0.285	0.154	0.361
Some college	0.066	0.248	0.209	0.406
College degree	0.019	0.136	0.284	0.451
Missing education	0.210	0.407	0.113	0.317
Log(income/sqrt(HH size))	9.115	2.055	10.783	0.902
Paid sick leave	0.030	0.169	0.451	0.498
Good MH all rounds	0.306	0.461	0.328	0.469
Excellent MH all rounds	0.308	0.462	0.356	0.479
Poor/ fair MH	0.132	0.338	0.049	0.215
Excellent MH all rounds	0.571	0.495	0.627	0.483
Poor health all rounds	0.072	0.258	0.027	0.163
Poor health some rounds	0.162	0.368	0.085	0.280
Excellent health some rounds	0.508	0.500	0.496	0.500
Excellent health all rounds	0.247	0.431	0.260	0.438
Good health all rounds	0.342	0.474	0.423	0.494
Any disability	0.057	0.232	0.012	0.108
County and state controls				
County log(median income)	10.820	0.248	10.911	0.250
County unemployment rate (16+)	8.103	2.731	7.345	2.515
State pct. BA degree	0.289	0.046	0.289	0.046
Medicaid expansion state	0.670	0.470	0.629	0.483
State managed-care penetration	0.723	0.147	0.730	0.145

Table 3-1. Continued

	Medicaid		Private	
	Mean	Std. Dev.	Mean	Std. Dev.
County-level medical resources				
HPSA primary care shortage	0.331	0.470	0.290	0.454
Per capita nurse practitioners	0.043	0.030	0.043	0.029
Per capita general practitioners	0.072	0.027	0.077	0.030
Per capita hospital beds	0.309	0.199	0.300	0.192
Per capita pediatricians	0.018	0.012	0.019	0.012
Observations	45,768	45,768	96,675	96,675

Notes: Means are weighted using the sample weights provided in the MEPS to be nationally representative.

Table 3-2. RD estimates of Medicaid fee bump on office-based visits by provider type, MEPS 2011-2014

	Log(Visits)						Log(Expenditures)					
	Overall effect		Extensive margin		Intensive margin		Overall effect		Extensive margin		Intensive margin	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Primary care physicians												
Post	0.004	0.002	0.004	0.002	0.013*	0.015**	0.040*	0.027	0.004	0.002	0.167***	0.135***
	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.024)	(0.030)	(0.005)	(0.007)	(0.040)	(0.045)
Post×shortage		0.008		0.015*		0.002		0.042		0.015*		0.026
		(0.008)		(0.010)		(0.014)		(0.045)		(0.010)		(0.071)
Observations	[338,256]		[338,256]		[37,543]		[338,256]		[338,256]		[37,543]	
Mid-level primary care providers												
Post	0.002	0.004	0.000	0.001	0.158***	0.220***	0.005	0.006	0.000	0.001	0.446***	0.378**
	(0.003)	(0.003)	(0.002)	(0.002)	(0.059)	(0.075)	(0.010)	(0.013)	(0.002)	(0.002)	(0.139)	(0.163)
Post×shortage		-0.003		-0.002		-0.140*		-0.008		-0.002		0.035
		(0.004)		(0.005)		(0.073)		(0.024)		(0.005)		(0.261)
Observations	[338,256]		[338,256]		[4,270]		[338,256]		[338,256]		[4,270]	
Other non-physician providers												
Post	0.005	0.000	0.005	0.002	-0.042	-0.071	0.032	0.017	0.005	0.002	0.017	0.009
	(0.007)	(0.008)	(0.005)	(0.007)	(0.055)	(0.059)	(0.027)	(0.035)	(0.005)	(0.007)	(0.130)	(0.156)
Post×shortage		0.019*		0.016*		0.111		0.051		0.016*		0.058
		(0.011)		(0.009)		(0.101)		(0.049)		(0.009)		(0.230)
Observations	[338,256]		[338,256]		[10,097]		[338,256]		[338,256]		[10,097]	
Physician specialists												
Post	0.002	0.000	0.003	0.000	-0.013	-0.003	0.022	0.005	0.003	0.000	0.107	0.128
	(0.006)	(0.008)	(0.006)	(0.007)	(0.040)	(0.054)	(0.027)	(0.034)	(0.006)	(0.007)	(0.086)	(0.126)
Post×shortage		0.006		0.006		-0.007		0.041		0.006		0.154
		(0.009)		(0.009)		(0.061)		(0.046)		(0.009)		(0.204)
Observations	[338,256]		[338,256]		[17,595]		[338,256]		[338,256]		[17,595]	

Notes: RD model specified as linear function of time, fully interacted with dummy for post fee bump years. All regressions include time-varying household demographic controls, state and month fixed effects. Primary care physician visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Physicians in other specialty are considered physician specialists. Mid-level primary care providers are visits with nurses, nurse practitioners, and physician assistants. All other non-physician providers are considered other providers. Standard errors in parentheses are corrected for clustering on the distance from the cutoff. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-3. Fixed effects estimates of a $10 Medicaid fee increase on office-based primary care physician visits, MEPS 2008-2015

	Log(Visits)						Log(Expenditures)					
	Overall effect		Extensive margin		Intensive margin		Overall effect		Extensive margin		Intensive margin	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Covered by Medicaid												
Fee	0.017***	0.012**	0.006**	0.007*	0.011***	0.009**	0.050**	0.037*	0.006**	0.004	0.026**	0.025**
	(0.005)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)	(0.019)	(0.020)	(0.003)	(0.004)	(0.010)	(0.012)
Fee×shortage		0.007*		0.002		0.006*		0.018		0.003		0.006
		(0.004)		(0.003)		(0.003)		(0.015)		(0.003)		(0.010)
Observations	[45,768]		[45,768]		[27,290]		[45,768]		[45,768]		[27,290]	
Covered by Private												
Fee	-0.001	-0.001	-0.001	0.000	-0.001	0.000	-0.003	0.000	-0.001	0.000	-0.007	-0.007
	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)	(0.000)	0.018	(0.003)	(0.002)	(0.003)	(0.007)	(0.007)
Fee×shortage		-0.001		-0.002		0.001		-0.001		-0.002		0.000
		(0.003)		(0.002)		(0.002)		(0.002)		(0.002)		(0.005)
Observations	[96,675]		[96,675]		[55,028]		[96,675]		[96,675]		[55,028]	

Notes: All regressions include time-varying household demographic controls, state and county characteristics, state and year fixed effects, and state specific linear time trend. Standard errors in parentheses are corrected for clustering at the state-level. Primary care physician visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-4. RD estimates of Medicaid fee bump on office-based visits by provider type, heterogeneity by scope of practice laws, MEPS 2011-2014

	Independent practice			With physician involvement		
	Overall effect	Extensive margin	Intensive margin	Overall effect	Extensive margin	Intensive margin
Primary care physicians						
Post	-0.012	-0.016	0.004	0.004	0.004	0.016*
	(0.010)	(0.013)	(0.011)	(0.005)	(0.006)	(0.009)
Observations	[80,064]	[80,064]	[9,071]	[258,192]	[258,192]	[28,472]
Mid-level primary care providers						
Post	0.003	0.001	0. 166***	0.001	0.000	0.137**
	(0.004)	(0.005)	(0.042)	(0.003)	(0.003)	(0.069)
Observations	[80,064]	[80,064]	[1,727]	[258,192]	[258,192]	[2,543]
Other non-physician providers						
Post	-0.006	-0.009	0.102	0.005	0.006	-0.097
	(0.023)	(0.018)	(0.136)	(0.008)	(0.005)	(0.081)
Observations	[80,064]	[80,064]	[3,271]	[258,192]	[258,192]	[6,826]
Physician specialists						
Post	-0.017	-0.004	-0.139	0.006	0.005	0.011
	(0.023)	(0.013)	(0.132)	(0.006)	(0.006)	(0.042)
Observations	[80,064]	[80,064]	[6,166]	[258,192]	[258,192]	[11,429]

Notes: RD model specified as linear function of time, fully interacted with dummy for post fee bump years. All regressions include time-varying household demographic controls, state and month fixed effects. Primary care visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Physicians in other specialty are considered physician specialists. Mid-level primary care providers are visits with nurses, nurse practitioners, and physician assistants. All other non-physician providers are considered other providers. Standard errors in parentheses are corrected for clustering on the distance from the cutoff. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-5. RD estimates of Medicaid fee bump on outpatient department and emergency room usage by type of visit, MEPS 2011-2014

	(1)	(2)
Primary care visit in office vs. outpatient department		
Post	-0.010	-0.001
	(0.011)	(0.010)
Post $\times$shortage		0.003
		(0.013)
Observations	[47,934]	
Non-urgent ER visit		
Post	0.001	-0.001
	(0.009)	(0.012)
Post $\times$shortage		0.001
		(0.015)
Emergent, Primary care treatable ER visit		
Post	-0.001	0.012
	(0.011)	(0.013)
Post $\times$shortage		-0.024
		(0.024)
Emergent, Preventable ER visit		
Post	0.017	0.035
	(0.019)	(0.023)
Post $\times$shortage		-0.024
		(0.034)
Emergent, Non-preventable ER visit		
Post	-0.001	-0.018
	(0.011)	(0.014)
Post $\times$shortage		-0.002
		(0.021)
Observations	[7,064]	

Notes: RD estimates specified as linear function of time, fully interacted with dummy for post fee bump years. All regressions include time-varying household demographic controls, state and month fixed effects. Standard errors in parentheses are corrected for clustering on the distance from the cutoff. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-6. Fixed effects estimates of a $10 Medicaid fee increase on outpatient department and emergency room usage by type of visit, MEPS 2008-2015

	(1)	(2)
Primary care visit in office vs. outpatient department		
Fee	0.000	-0.001
	(0.001)	(0.001)
Fee×shortage		0.003**
		(0.001)
Observations	[85,656]	
Non-urgent ER visit		
Fee	-0.003*	-0.004*
	(0.002)	(0.002)
Fee×shortage		0.001
		(0.002)
Emergent, Primary care treatable ER visit		
Fee	0.001	0.002
	(0.003)	(0.003)
Fee×shortage		-0.004*
		(0.002)
Emergent, Preventable ER visit		
Fee	-0.003	-0.001
	(0.003)	(0.003)
Fee×shortage		0.001
		(0.003)
Emergent, Non-preventable ER visit		
Fee	-0.004	-0.006
	(0.003)	(0.004)
Fee×shortage		-0.001
		(0.002)
Observations	[12,086]	

Notes: All regressions include time-varying household demographic controls, state and county characteristics, state and year fixed effects, and state specific linear time trend. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-7. Fixed effects estimates of a $10 Medicaid fee increase on perceived access to care and quality, MEPS 2008-2015

Access to care	Have USC		USC is person		Unable to receive treatment		Delayed in receiving treatment	
Fee	0.004	0.003	0.005	0.006	-0.002*	-0.001	-0.002*	-0.001
	(0.003)	(0.003)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)
Fee×Shortage		0.004*		-0.003		-0.002***		-0.001
		(0.002)		(0.003)		(0.001)		(0.001)
Observations	[45,257]		[39,849]		[45,695]		[45,688]	
Perceived quality of care	Explained all options		Helped make decisions		Respected alternative treatments		Ask medications other doctors give	
Fee	0.004**	0.003*	0.008*	0.006	0.002	0.000	0.004	0.002
	(0.002)	(0.002)	(0.005)	(0.007)	(0.005)	(0.005)	(0.004)	(0.004)
Fee×Shortage		0.004**		-0.002		0.010		0.004
		(0.002)		(0.005)		(0.005)		(0.007)
Observations	[39,150]		[37,993]		[36,633]		[38,787]	
Self-reported health	Poor/Fair physical		Excellent physical		Poor/Fair mental		Excellent mental	
Fee	-0.003*	-0.003	0.007*	0.008**	-0.002	-0.002	0.004	0.001
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)
Fee×Shortage		-0.002*		-0.005		0.001		0.001
		(0.001)		(0.004)		(0.002)		(0.003)
Observations	[45,768]		[45,768]		[45,768]		[45,768]	
Had in the previous year	Cholesterol check		Blood pressure check		Flu shot			
Fee	0.001	0.004	0.001	-0.001	0.012**	0.009*		
	(0.004)	(0.003)	(0.004)	(0.003)	(0.006)	(0.006)		
Fee×Shortage		-0.007		-0.002		0.001		
		(0.006)		(0.007)		(0.004)		
Observations	[13,413]		[14,047]		[13,960]			
Any ancillary service in primary care visit	Laboratory test		X-rays					
Fee	-1.188**	-1.341**	-0.688	-0.803				
	(0.561)	(0.551)	(0.487)	(0.549)				
Fee×Shortage		0.353		0.353				
		(0.425)		(0.423)				
Observations	[71,862]		[71,862]					

Notes: All regressions include time-varying household demographic controls, state and county characteristics, state and year fixed effects, and state specific linear time trend. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-8. Fixed effects estimates of a $10 Medicaid fee increase on prescription drug access, MEPS 2008-2015

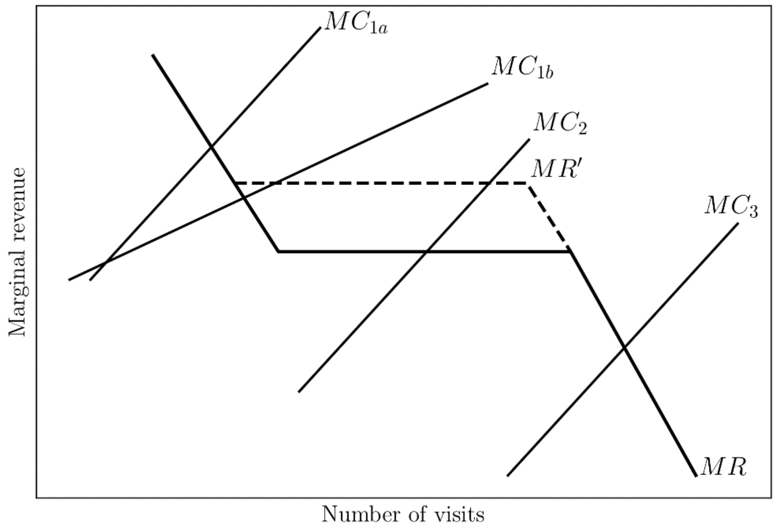
	(1)	(2)
All classes		
Fee	0.008***	0.007**
	(0.003)	(0.003)
Fee×Shortage		0.004
		(0.003)
Cardiovascular medications		
Fee	-0.001	-0.001
	(0.001)	(0.001)
Fee×Shortage		-0.001
		(0.001)
Antibiotics		
Fee	0.006**	0.004*
	(0.003)	(0.002)
Fee×Shortage		0.007**
		(0.003)
Mental health medications		
Fee	0.005**	0.004*
	(0.002)	(0.002)
Fee×Shortage		0.005**
		(0.002)
Diabetes medications		
Fee	0.000	0.000
	(0.001)	(0.001)
Fee×Shortage		0.000
		(0.001)
GI medications		
Fee	0.003*	0.002
	(0.002)	(0.002)
Fee×Shortage		0.001
		(0.001)
Respiratory medications		
Fee	0.005*	0.003
	(0.003)	(0.003)
Fee×Shortage		0.006*
		(0.003)
observations	[45,768]	

Notes: All regressions include time-varying household demographic controls, state and county characteristics, state and year fixed effects, and state specific linear time trend. Standard errors in parentheses are corrected for clustering at the state-level. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Appendix A. Model of Provider Behavior

As illustrated in Figure 3-C-1, providers face a downward sloping demand curve for private patients and a fixed unit price for treating Medicaid patients. Providers choose a quantity of output such that marginal revenue equals marginal cost. For a provider with a high marginal cost illustrated by the marginal cost curve MC_{1a} or MC_{1b}, the point of intersection will be on the downward-sloping portion of the curve and only private patients will be seen. In contrast, a provider with intermediate marginal cost curves like MC_2 will participate in the private and public markets, with the total number of patients determined by the intersection of the marginal cost curve and the Medicaid price. A provider with the lowest marginal cost curves like MC_3 will also see a mix of public and private patients, but because there is a limit to the number of publicly insured patients in the market, the marginal patient will be a private patient. When Medicaid rates rise relative to private market fees, providers like those represented by MC_{1b} will participate in Medicaid and those with a marginal cost like MC_2 that treat a mix of public and private patients will treat a greater number of public patients. Such changes, however, will have no effect on providers like those represented by MC_{1a}.

Figure 3-C-1. Provider response to Medicaid reimbursement rate increase



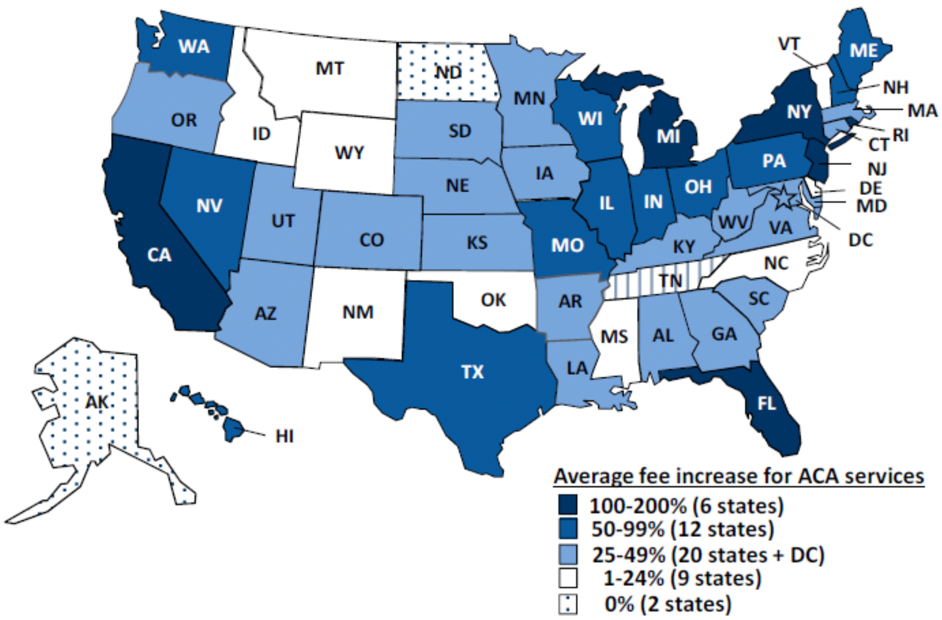
Notes: A simple model of the supply response to the Medicaid fee bump based on Sloan, Mitchell, and Cromwell (1978). The effect of fee bump, illustrated by the shift of the marginal revenue curve from MR to MR′, will vary across providers with different marginal cost curves.

Appendix B. Medicaid Managed Care Payments

We re-scale the fee-for-service rates by the managed care to fee-for-service payment ratio for primary care services and use these rates for those enrolled in managed care plans. That is, letting fee_{st}^{FFS} denotes the Medicaid fee-for-service reimbursement rate in state s in year t, $\left(\frac{fee^{MC}}{fee^{FFS}}\right)_s$ the managed care-to-fee-for-service payment ratio in state s, the imputed payment for managed care services in each state-year before the fee bump is $fee_{st}^{FFS} \times \left(\frac{fee^{MC}}{fee^{FFS}}\right)_s$. These payment ratios come from a Government Accountability Office report documenting the difference between managed care and fee-for-service payments under Medicaid at the state level in 2010 (GAO 2014).

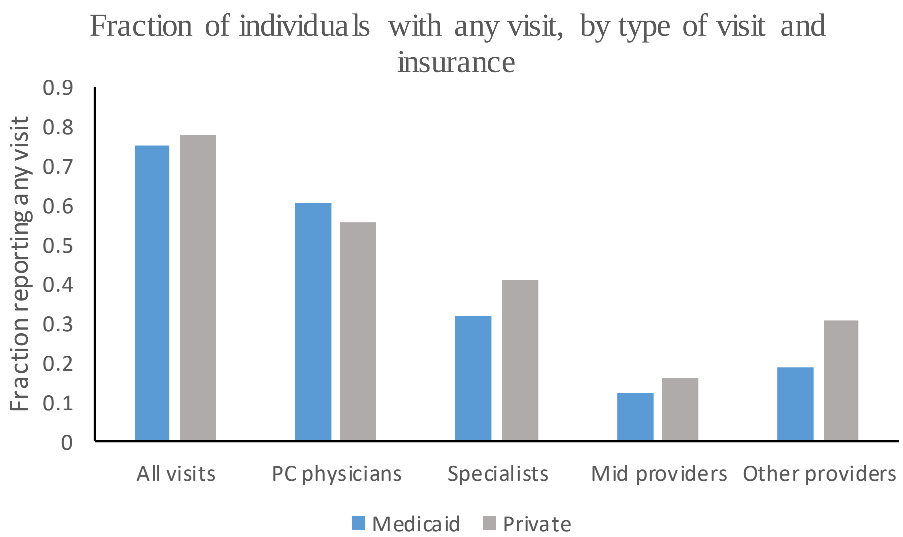
Appendix C. Supplementary Figures and Tables

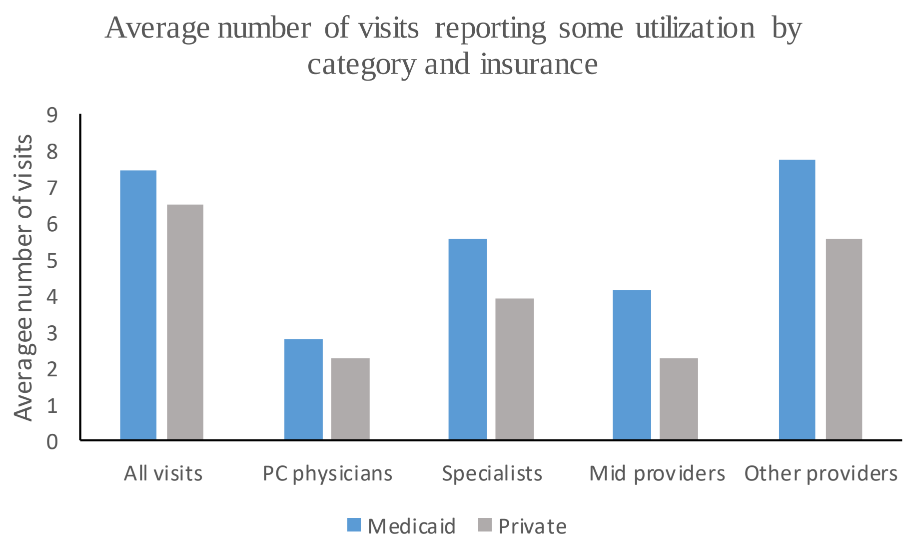
Figure 3-C-1. Heterogeneous effect of Medicaid fee bump across states in 2013



Note: Tennessee has no Medicaid FFS program.
Source: Zuckerman and Goin (2012).

Figure 3-C-2. Office visit utilization across types of care and insurance coverage

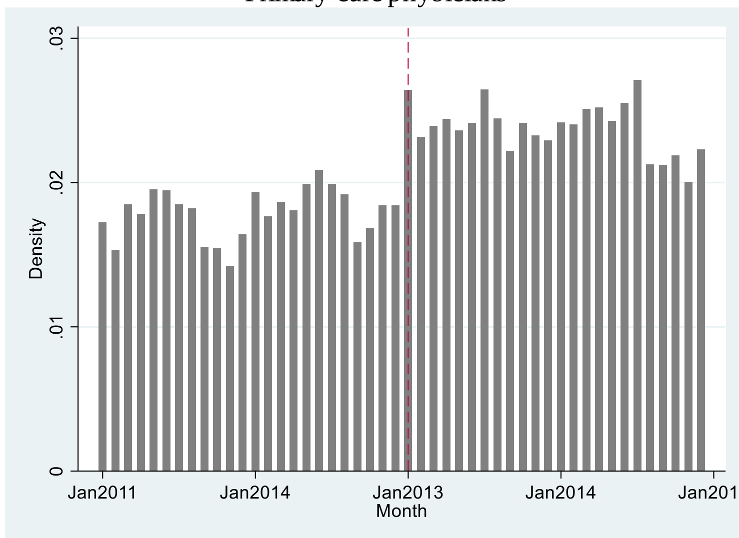




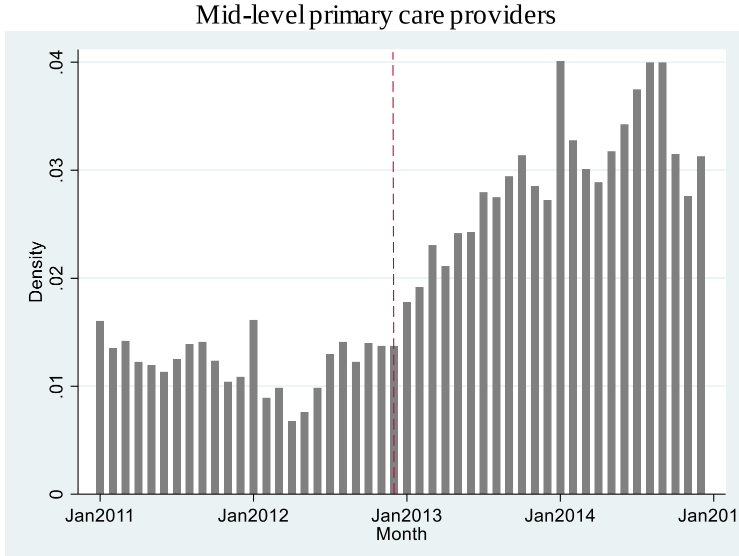
Notes: Authors' calculations from the 2008-2015 MEPS. Primary care (PC) visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Physicians in other specialty are considered physician specialists. Mid-level primary care providers are visits with nurses, nurse practitioners, and physician assistants. All other non-physician providers are considered other providers.

Figure 3-C-3. Histogram of office-based primary care visits

Primary care physicians



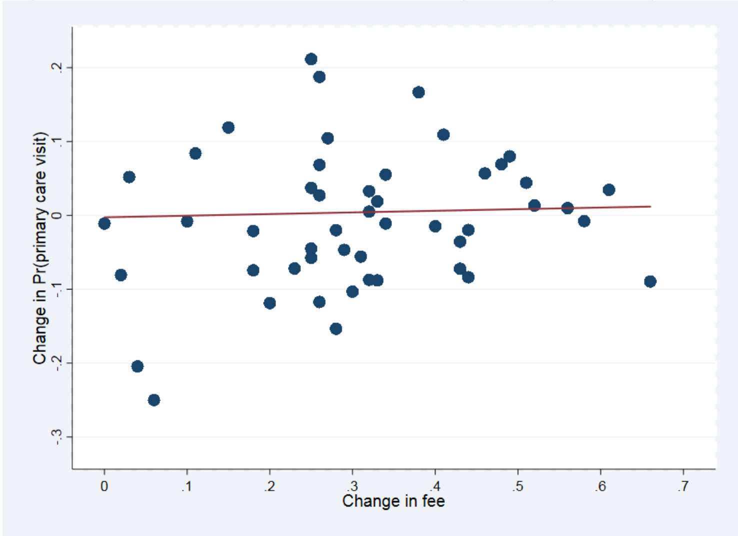
Mid-level primary care providers



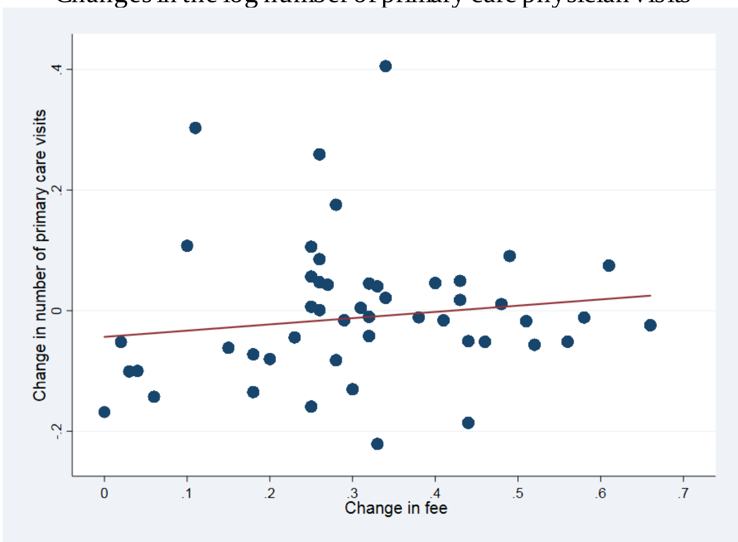
Notes: The histogram of office-based primary care visits is presented for visits from the 2008-2015 MEPS. Primary care visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Mid-level primary care providers are visits with nurses, nurse practitioners, and physician assistants.

Figure 3-C-4. Average change in outcome variables by size of fee increase

Changes in the fraction of individuals with any primary care physician visit



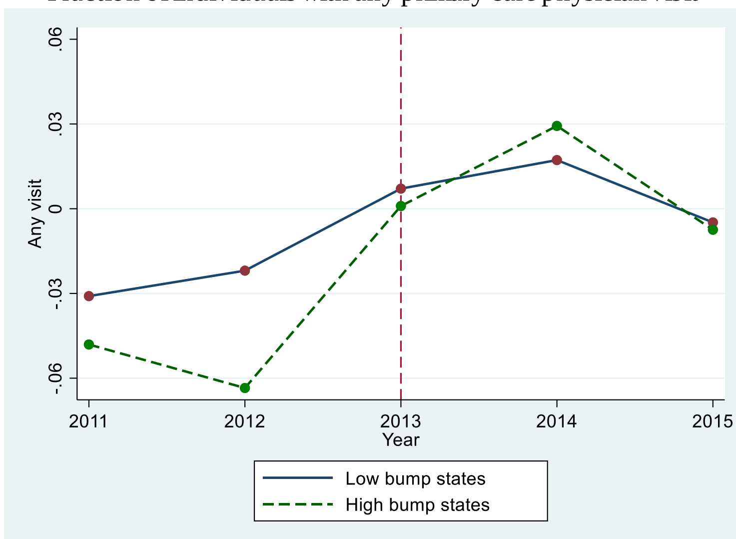
Changes in the log number of primary care physician visits



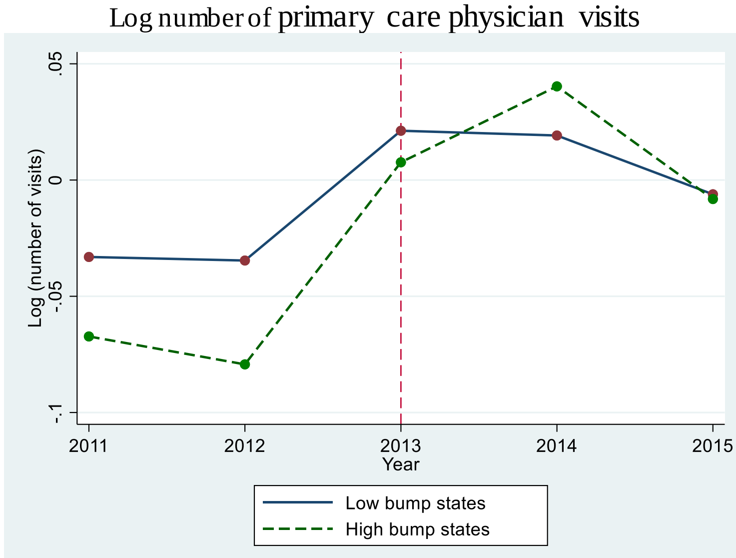
Notes: the figures plot the average change in the outcomes at the state level between 2011-2012 (before) and 2013-2014 (after). Primary care visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics.

Figure 3-C-5. Test for monotonicity assumption in the fixed-effects model

Fraction of individuals with any primary care physician visit



Log number of primary care physician visits



Notes: Trends in primary care physician visits (after differencing out individual and state-level characteristics) for states above and below the median fee increase from 2012-2013. Primary care visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics.

Table 3-C-1. RD estimates of Medicaid fee bump on office-based visits among privately insured sample above 400% of FPL by provider type, MEPS 2011-2014

	(1) Linear	(2) Quadratic
Primary care physicians		
Extensive margin	0.004	0.004
	(0.004)	(0.004)
	[366,216]	[366,216]
Intensive margin	0.006	0.006
	(0.006)	(0.006)
	[34,935]	[34,935]
Mid-level primary care providers		
Extensive margin	0.003	0.003
	(0.002)	(0.002)
	[366,216]	[366,216]
Intensive margin	0.031	0.022
	(0.037)	(0.036)
	[6,641]	[6,641]
Other non-physician providers		
Extensive margin	0.005	0.005
	(0.004)	(0.004)
	[366,216]	[366,216]
Intensive margin	-0.023	-0.027
	(0.025)	(0.025)
	[23,687]	[23,687]
Physician specialists		
Extensive margin	-0.005	-0.005
	(0.004)	(0.004)
	[366,216]	[366,216]
Intensive margin	-0.001	-0.001
	(0.014)	(0.014)
	[25,010]	[25,010]

Notes: RD model in the first column specified as linear function of time, fully interacted with dummy for post fee bump years. The specification in the second column is quadratic in time. All regressions include time-varying household demographic controls, state and month fixed effects. Primary care physician visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Physicians in other specialty are considered specialists. Mid-level primary care providers are visits with nurses, nurse practitioners, and physician assistants. All other non-physician providers are considered other providers. Standard errors in parentheses are corrected for clustering on the days from the cutoff. Sample size is reported in square brackets. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-C-2. RD estimates of Medicaid fee bump on office-based visits by provider type, MEPS 2011-2014

	Log(Visits)						Log(Expenditures)					
	Overall effect		Extensive margin		Intensive margin		Overall effect		Extensive margin		Intensive margin	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Primary care physicians												
Post	0.004	0.001	0.004	0.000	0.013*	0.013*	0.040*	0.020	0.004	0.000	0.167***	0.131***
	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.008)	(0.024)	(0.030)	(0.005)	(0.007)	(0.040)	(0.044)
Post×shortage		0.010		0.011		0.002		0.054		0.011		0.038
		(0.008)		(0.010)		(0.014)		(0.046)		(0.010)		(0.069)
Observations	[338,256]		[338,256]		[37,543]		[338,256]		[338,256]		[37,543]	
Mid-level primary care providers												
Post	0.002	0.003	0.000	0.000	0.158***	0.211***	0.005	0.006	0.000	0.000	0.446***	0.344**
	(0.003)	(0.003)	(0.002)	(0.002)	(0.059)	(0.077)	(0.010)	(0.013)	(0.002)	(0.002)	(0.139)	(0.165)
Post×shortage		-0.003		-0.002		-0.128*		-0.007		-0.002		0.073
		(0.004)		(0.005)		(0.073)		(0.025)		(0.005)		(0.267)
Observations	[338,256]		[338,256]		[4,270]		[338,256]		[338,256]		[4,270]	
Other non-physician providers												
Post	0.005	0.000	0.005	0.003	-0.042	-0.082	0.032	0.020	0.005	0.003	0.017	-0.021
	(0.007)	(0.008)	(0.005)	(0.007)	(0.055)	(0.060)	(0.027)	(0.036)	(0.005)	(0.007)	(0.130)	(0.156)
Post×shortage		0.016		0.016*		0.135		0.047		0.016*		0.127
		(0.012)		(0.009)		(0.101)		(0.049)		(0.009)		(0.234)
Observations	[338,256]		[338,256]		[10,097]		[338,256]		[338,256]		[10,097]	
Physician specialists												
Post	0.002	0.000	0.003	0.000	-0.013	-0.004	0.022	0.007	0.003	0.000	0.107	0.129
	(0.006)	(0.008)	(0.006)	(0.007)	(0.040)	(0.056)	(0.027)	(0.035)	(0.006)	(0.007)	(0.086)	(0.129)
Post×shortage		0.006		0.006		-0.007		0.038		0.006		0.153
		(0.009)		(0.009)		(0.063)		(0.047)		(0.009)		(0.208)
Observations	[338,256]		[338,256]		[17,595]		[338,256]		[338,256]		[17,595]	

Notes: RD model specified as quadratic function of time, fully interacted with dummy for post fee bump years. All regressions include time-varying household demographic controls, state and month fixed effects. Primary care visits are visits with a physician specializing in general practice, family medicine, internal, or pediatrics. Physicians in other specialty are considered physician specialists. Mid-level primary care providers are visits with nurses, nurse practitioners, and physician assistants. All other non-physician providers are considered other providers. Standard errors in parentheses are corrected for clustering on the distance from the cutoff. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-C-3. Balance of covariates around the cutoff, MEPS 2011-2014

	Post-policy sample		Pre-policy sample		p-value
	Mean	Std. Dev.	Mean	Std. Dev.	
Age 0-5	0.203	0.008	0.197	0.006	0.166
Age 6-17	0.376	0.007	0.390	0.007	0.541
Age 18-24	0.071	0.004	0.074	0.004	0.647
Age 25-34	0.078	0.004	0.074	0.003	0.532
Age 35-44	0.077	0.004	0.075	0.004	0.701
Age 45-54	0.094	0.005	0.095	0.004	0.720
Age 55-64	0.098	0.006	0.094	0.005	0.645
Female	0.535	0.006	0.533	0.006	0.586
Hispanic	0.297	0.016	0.308	0.020	0.772
Black	0.245	0.014	0.238	0.015	0.368
White	0.403	0.016	0.404	0.017	0.515
Other race	0.055	0.006	0.050	0.006	0.451
Married	0.128	0.006	0.114	0.006	0.276
No. of children 0-5	0.630	0.024	0.608	0.022	0.752
No. of children 6-17	1.311	0.030	1.344	0.032	0.724
HH size	3.845	0.047	3.848	0.052	0.515
BA degree	0.021	0.003	0.023	0.003	0.345
log(family income)	9.181	0.039	9.013	0.045	0.054
Midwest	0.191	0.012	0.198	0.013	0.464
South	0.354	0.017	0.353	0.016	0.596
West	0.246	0.019	0.251	0.019	0.729
Disability	0.088	0.006	0.089	0.006	0.578
Observations	[14,693]		[13,542]		

Notes: Post-policy sample uses the 2013-2014 MEPS. Pre-policy uses the 2011-2012 MEPS. P-values represent a two-sample t-test of post-policy versus pre-policy.

Table 3-C-4. Test for strict exogeneity in the fixed-effects model

	Any office visit in the past year	Log(Number of office visits)
Fee_t	0.009	0.013*
	(0.008)	(0.007)
Fee_{t+1}	-0.004	0.001
	(0.005)	(0.006)
Observations	[40,119]	[24,275]

Notes: All regressions include time-varying household demographic controls, state and county characteristics, state and year fixed effects, and state specific linear time trend. Standard errors in parentheses are corrected for clustering at the state-level. Sample size is reported in square brackets. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-C-5. Fixed effects estimates of a $10 Medicaid fee increase on primary care visits, alternative samples

Sample of children	
Any office visit in the past year	0.010**
	(0.004)
	[32,920]
Log(Number of office visits)	0.011**
	(0.005)
	[19,701]
Sample of adults	
Any office visit in the past year	0.001
	(0.006)
	[12,846]
Log(Number of office visits)	0.018*
	(0.011)
	[7,589]
Sample of families with children	
Any office visit in the past year	0.006*
	(0.004)
	[41,551]
Log(Number of office visits)	0.010**
	(0.004)
	[24,552]
2008-2014 Medicaid sample	
Any office visit in the past year	0.010*
	(0.006)
	38,878
Log(Number of office visits)	0.007
	(0.007)
	[23,298]
2011-2015 Medicaid sample	
Any office visit in the past year	0.007*
	(0.004)
	31,879
Log(Number of office visits)	0.013***
	(0.005)
	[18,812]

Notes: All regressions include time-varying household demographic controls, state and county characteristics, state and year fixed effects and state specific linear time trend. Standard errors in parentheses are corrected for clustering at the state-level. Sample size is reported in square brackets. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-C-6. Fixed effects estimates of a $10 Medicaid fee increase on the labor supply of providers, 2010 - 2015

Per capita number of primary care physicians	0.031 (0.031) [36,598]
Per capita number of nurse practitioners	0.064** (0.027) [36,598]
Per capita number of physician assistants	0.039* (0.022) [36,598]

Notes: All regressions include time-varying household demographic controls, state and county characteristics, state and year fixed effects, and state specific linear time trend. Standard errors in parentheses are corrected for clustering at the state-level. Sample size is reported in square brackets. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

Table 3-C-7. Fixed effects estimates of a $10 Medicaid fee increase on office-based primary care physician visits by insurance, heterogeneity by scope of practice laws

	Independent practice			With physician involvement		
	Overall effect	Extensive margin	Intensive margin	Overall effect	Extensive margin	Intensive margin
Covered by Medicaid						
Fee	-0.002	0.004	-0.016	0.014***	0.007**	0.010**
	(0.016)	(0.012)	(0.018)	(0.005)	(0.003)	(0.004)
	[5,021]	[5,021]	[2,983]	[40,747]	[40,747]	[24,307]
Covered by Private						
Fee	-0.003	0.003	-0.009	-0.002	-0.002	0.001
	(0.010)	0.008	(0.011)	(0.004)	(0.003)	(0.003)
	[14,528]	[14,528]	[7,857]	[82,147]	[82,147]	[47,171]

Notes: All regressions include time-varying household demographic controls, state and county characteristics, state, year fixed effects, and state specific linear time trend. Standard errors in parentheses are corrected for clustering at the state-level. Sample size is reported in square brackets. Significance level: ***p < 0.01. **p < 0.05. *p < 0.1.

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WORKING PAPERS

Fayaz Farkhad, B. and C. Meyerhoefer. "How Was Medical Care Utilization Affected by the Medicaid Primary Care Fee Bump?" (*Job Market Paper*)

Fayaz Farkhad, B. and C. Meyerhoefer. "How the Supplemental Nutrition Assistance Program Affects Labor Force Decisions" (under review, *Journal of Policy Analysis and Management*)

Fayaz Farkhad, B., C. Meyerhoefer and J. Dearden. "Within-Month Patterns of Medical Care Utilization and Food Expenditure among SNAP Households"

Meyerhoefer, C., S. Zuvekas, B. Fayaz Farkhad, J. Moeller and R. Manski. "The Demand for Preventive and Restorative Dental Services among Older Adults" (revised and resubmitted, *Health Economics*)

WORKS IN PROGRESS

The Impact of Medicaid Copayment Increases for Emergency Departments on Access to Care and Health Outcomes (with Chad Meyerhoefer and Adam Biener)

Correcting for Measurement Error Using Semi-Supervised Learning (with Reza Nazari)

RESEARCH ASSISTANT

Lehigh University — Summer 2015, 2016
Research Assistant to Muzhe Yang

National Bureau of Economic Research — Summer 2017, 2018
Research Assistant to Chad Meyerhoefer

TEACHING EXPERIENCE (AT LEHIGH)

Graduate Course Instructor

- Applied Microeconometrics (Adjunct Professor) — Fall 2018
- Excel Mastery — Summer 2017

Undergraduate Course Instructor

- Statistical Methods (Adjunct) — Fall 2017, Spring 2018 & 2019 (Scheduled)

Teaching Assistant

- Introduction to Finance — Fall 2014 - Fall 2016

CONFERENCE PRESENTATIONS

Fayaz Farkhad, B., C. Meyerhoefer and J. Dearden. "Within-Month Patterns of Medical Care Utilization and Food Expenditure among SNAP Households"

- Eastern Economics Association 43th Annual Conference, New York City, NY, February 2017.
- iHEA World Congress on Health Economics, Boston, MA, July 2017.
- AAEA Annual Meeting, Chicago, IL, August 2017.

Fayaz Farkhad, B. and C. Meyerhoefer. "How Was Medical Care Utilization Affected by the Medicaid Primary Care Fee Bump?"

- American Society for Health Economists (ASHE) Conference, Atlanta, GA, June 2018 (Poster Presserntation).
- APPAM Fall Conference, Washington, D.C., November 2018.
- Southern Economic Association Annual Meetings, Washington, D.C., November 2018.

Fayaz Farkhad, B. and C. Meyerhoefer. "How the Supplemental Nutrition Assistance Program Affects Labor Force Decisions"

- AAEA Annual Meeting, Washington, D.C., August 2018.
- APPAM Fall Conference, Washington, D.C., November 2018.

Meyerhoefer, C., S. Zuvekas, B. Fayaz Farkhad, J. Moeller and R. Manski . "Modeling Dental Service Use among the Elderly in the Presence of Endogenous Selection into Dental Coverage"

- American Society for Health Economists (ASHE) Conference, Atlanta, GA, June 2018.

OTHER PRESENTATIONS

Session Chair "Health Economics V", Southern Economic Association Annual Meetings, Washington, D.C., November 2018.

Discussant for the conference paper, titled "Effect of Discount Coupons and Education on Healthy Food Purchase and Obesity", Eastern Economic Association 43th Annual Conference, New York City, NY, February 2017.

Fayaz Farkhad, B. and C. Meyerhoefer. "Increasing Access to Primary Care without Bumping Up Costs: Evidence from the Medicaid Fee Bump", Lehigh University, Department of Economics, November 2018 (Scheduled).

Fayaz Farkhad, B., C. Meyerhoefer. "The Impact of Participation in SNAP on Labor Force Decisions", Lehigh University, Department of Economics, May 2017.

Fayaz Farkhad, B., C. Meyerhoefer and J. Dearden. "The Within-Month Pattern of Medical Care Utilizationa among SNAP Participants", Lehigh University, Department of Economics, February 2017.

AWARDS AND HONORS

Donald T. Campbell Best Graduate Research Paper Award, Lehigh University 2018
AAEA Early Career Professionals Travel Grant Summer 2017
Warren-York Dissertation Fellowship, Lehigh University 2017
Teaching Development Program Certificate, Lehigh University Spring 2017
Teaching Assistantship, Lehigh University 2014-2016
Ranked 10th among more than 8,000 Socio-Economic Systems Engineering Participants in the Iranian National Graduate Qualification Exam 2011

TECHNOLOGICAL SKILLS

Statistical Software: STATA, SAS.
Programming Languages: Mata, MATLAB, C++, Python.
Others: Maple, LaTeX.

SERVICE ACTIVITIES

Peer-Reviewing Activity: Professional Journal

- American Journal of Agricultural Economics
- Food Policy ($\times 2$)
- Agricultural and Food Economics ($\times 3$)

Abstract Reviewer of the Scientific Review Committee of the 8th Conference of the American Society of Health Economists

Professional Societies and Affiliations
American Economic Association, Agricultural and Applied Economics Association, International Health Economics Association, American Society of Health Economists, Southern Economic Association, Association for Public Policy Analysis & Management

University Service

- Graduate Student Senate Representative, Lehigh University Spring 2017

LANGUAGES

English, Azeri, Farsi, Turkish

REFERENCES

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