NBER WORKING PAPER SERIES

THE EFFECT OF PRIMARY CARE VISITS ON HEALTH CARE UTILIZATION: FINDINGS FROM A RANDOMIZED CONTROLLED TRIAL

Cathy J. Bradley
David Neumark
Lauryn Saxe Walker

Working Paper 24100
http://www.nber.org/papers/w24100

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2017, Revised July 2018

Bradley, Saxe and Neumark's research was supported by AHRQ grant number R01-HS022534, "Incentives for Primary Care Use: A Randomized Controlled Trial in a Safety Net Setting." The authors are grateful to Ms. Heather Saunders for project coordination, Drs. Chun-Chieh Hu and Bassam Dahman for statistical and programming support, Ms. Megan Eguchi for assistance with programming, Dr. Dexiang Gao for statistical consultation, the interviewers who collected and coded the data, the many subjects who generously donated their time to the project, and the Virginia Coordinated Care program that allowed us to enroll their patients and provided access to medical claim files. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

The Effect of Primary Care Visits on Health Care Utilization: Findings from a Randomized Controlled Trial

Cathy J. Bradley, David Neumark, and Lauryn Saxe Walker

NBER Working Paper No. 24100
December 2017, Revised July 2018
JEL No. I12,I14,I18

ABSTRACT

We conducted a randomized controlled trial, enrolling low-income uninsured adults in Virginia (United States), to determine whether cash incentives are effective at encouraging a primary care provider (PCP) visit, and at lowering utilization and costs. Subjects were randomized to four groups: untreated controls, and one of three incentive arms with incentives of $0, $25, or $50 for visiting a PCP within six months of group assignment. We used the exogenous variation generated by the experiment to obtain causal evidence on the effects of a PCP visit. We observed modest reductions in non-urgent emergency department use and increased outpatient use, but no reductions in overall costs. These findings in utilization are consistent with the expectation that PCPs offer an alternative to the emergency department for non-emergent conditions. Total costs did not decline because any savings from avoiding the emergency department were offset by increased outpatient utilization.

Cathy J. Bradley
University of Colorado-Denver
13001 E. 17th Place, B119
Bldg. 500, Rm N6203L
Aurora, CO 80045
CATHY.BRADLEY@ucdenver.edu

Lauryn Saxe Walker
Virginia Commonwealth University
School of Medicine Health Behavior and Policy
830 East Main St.
Richmond, VA 23219
saxel@mymail.vcu.edu

David Neumark
Department of Economics
University of California, Irvine
3151 Social Science Plaza
Irvine, CA 92697
and NBER
dneumark@uci.edu

A randomized controlled trials registry entry is available at ClinicalTrials.gov (NCT02922855)

Increasing access of the low-income population to good quality and lower-cost health care is a priority of the United States (U.S.) Agency for Healthcare Research and Quality (https://www.ahrq.gov/workingforquality/about/index.html). Low-income adults below age 65 who neither have access to employer-based health insurance nor qualify for Medicaid rely on the safety net system to meet their health care needs.[1] Often, patients seen in the safety net system have preventable health conditions that escalate to a crisis, requiring high-cost emergency department visits and inpatient care. These health care settings are associated with expensive utilization, poor coordination and follow-up care, and reduced well-being (Asplin et al. 2005, Johnson et al. 2015). In contrast, the primary care setting is viewed as an efficient means to diagnose and treat conditions before they reach a severity level requiring expensive procedures and hospitalization (Peterson et al. 2011). Historically, however, low-income patients have limited access to PCPs and do not routinely seek preventive care (Sommers et al. 2017, Pitts et al. 2010). Partly in response to this problem, the U.S. Centers for Medicare and Medicaid Services spend millions each year on demonstration projects, managed care arrangements, case managers, and patient-centered homes—all of which are aimed at improved service delivery and cost reduction (https://innovation.cms.gov).

In a prior study, we compared the effect of cash incentives to low-income uninsured patients, treated in a safety net setting, to seek initial primary care visits. Incentives may be substitutes for or complements to programs that improve care coordination and reduce costs. In our study, all patients were assigned to a community-based PCP and provided free or low-cost health care. The trial found that small cash incentives (i.e., $25, $50) encouraged primary care visits, and that subjects were more responsive to higher incentives. Relative to the group that

[1] Medicaid is an insurance program in the United States for certain categories of low-income uninsured adults.

received no cash incentive, the odds of a PCP visit increased by 36% for the $25 incentive group and 56% for the $50 incentive group. This study is described in more detail elsewhere (Bradley and Neumark 2017). The study did not address whether healthcare utilization subsequent to the initial PCP visit was reduced relative to those who did not see a PCP.

In the present paper, we explore whether the initial PCP visit changes utilization and costs, using the random assignment from the experiment to provide exogenous variation in PCP visits. This exogenous source of variation is important because otherwise correlations between unobserved determinants of health care utilization and costs and whether one visited a PCP could drive the relationship between PCP visits and utilization and costs. We also estimate the direct effect of incentives on health care utilization and costs (presuming that PCP visits are the mechanism through which incentives work). These reduced form estimations provide insight into the different levels of incentives that result in the largest changes in utilization and costs.

We study utilization (e.g., emergency department, outpatient) and total health care costs within 12 months after study enrollment, broken up into the six-month period in which the incentives apply, and the subsequent six months. We thus provide new evidence on whether primary care alters utilization patterns and reduces high-cost care in a low-income safety net population. This study is relevant given the financial strain on the U.S. safety net system to provide care, and a low-income population that has considerable health care needs (Gold, Pitrelli, et al. 2014, Bazzoli, Fareed, and Waters 2014, Pickens et al. 2017). In addition, there is a strongly held belief that primary care can reduce health care costs through improved coordination of care and better management of chronic conditions (Heintzman et al. 2014). By capitalizing on a randomized controlled trial, the study provides a more convincing test of this hypothesis than has been obtained from observational studies.

Our findings suggest that cash incentives encourage the desired behavior of PCP utilization but may also have unintended consequences for other types of health care utilization—outpatient care in particular. We conclude that although an initial PCP visit can be effectively incentivized, and many patients continued to have subsequent visits with the PCP, overall health care utilization may not be reduced, and may even increase in the short-run.

1. Utilization following health insurance coverage

The best method for controlling costs once insurance coverage is provided, particularly for low-income previously uninsured adults in the United States, is vigorously debated among policymakers, insurers, and health care providers. Often, primary care is touted as the solution for cost control, but our assessment of the available U.S.-based studies suggests that we need to learn more about this relationship, and we need more rigorous evidence. Corresponding to the outcomes in our study, in reviewing this U.S.-based literature we focus on utilization and costs; we do not address potential health benefits, quality of life improvements, or improvements in satisfaction with health care providers.

Many studies find that, in the first year of health insurance coverage following a period of uninsurance, health care utilization increases (Cunningham, Sabik, and Bonakdar Tehrani 2016, Finkelstein et al. 2016, Finkelstein et al. 2012, O'Malley et al. 2016). In the United States, following Oregon's 2008 Medicaid expansion to low-income uninsured childless adults, for example, overall health care encounters increased by 35% and PCP encounters increased by 22% for those newly insured (Gold et al. 2014). Likewise, those newly insured following health care reform in Massachusetts increased utilization during the first year of coverage, including emergency care (Lee et al. 2015). Enrollees with no prior public insurance had 12% higher odds of an emergency department visit within 12 months following enrollment in the Massachusetts

Medicaid program. Increased utilization following insurance coverage was also reported in patients newly insured by private health insurance plans (Franks, Cameron, and Bertakis 2003). Some researchers hypothesize that higher utilization following coverage is attributable to pent-up demand for health care services, suggesting that the newly insured make up for forgone health services once insured (Heintzman et al. 2014, Buchmueller et al. 2005).

Researchers propose that primary care may be a way to reduce overall utilization (Heintzman et al. 2014)once a population obtains health insurance coverage (Heintzman et al. 2014), as PCPs provide preventive care and treat chronic conditions, and thereby help patients avoid the emergency department (Hadley 2007, Hadley and Cunningham 2004). This hypothesis has received considerable attention from U.S. safety net providers that care for low-income uninsured adults—often uncompensated. In the absence of insurance options, many safety net providers devised their own coverage programs based on managed care principles to improve health and reduce costs in the populations they serve. During the 1990s and early 2000s, prior to the passage of the Affordable Care Act (ACA), these programs were widespread in the United States. Many programs offered access to community-based primary care providers, including access to medical homes that provided comprehensive and coordinated care through a team of affiliated providers.[2] The various approaches we describe below highlight the critical interest in reducing costs and the willingness of institutions to make large investments to do so. However, the effectiveness of these approaches at intercepting the tendency for low-income patients to seek care in the emergency department and reducing costs to the safety net system is unclear.

A randomized trial to evaluate utilization of uninsured patients assigned to Milwaukee Cares, a coordinated care coverage program administered by the Medical College of Wisconsin,

[2] See, for example, http://www.cjaonline.net/project-access/.

did not find reduced utilization for its beneficiaries in the first year following enrollment (Mackinney et al. 2013). Similarly, The Access Partnership in East Baltimore (Maryland) did not change emergency department utilization, although fewer emergency department visits resulted in an inpatient admission (Block et al. 2013). Only one published study reported reduced utilization in the first year of enrollment; p(Block et al. 2013). Patients participating in Project Access Dallas (Texas) reported fewer emergency department visits and hospital days than similar patients who were not enrolled in the program (DeHaven et al. 2012). However, this study did not focus directly on the effects of PCP visits on health care utilization, and it is unclear whether those assigned to the program were systematically different from the control population, raising concerns about potential selection bias.

Our own assessment of data from the Virginia Coordinated Care program, a community-based primary care program, found that newly enrolled recipients used more health care services than those who had been enrolled for two or more years even though recipients had access to free primary care within the community (Bradley et al. 2012). However, in the second and third years following enrollment, health care utilization declined, perhaps because beneficiaries' health status improved during the initial period. The study, like many others, suffered from the lack of a control group.

Many of these coordinated care programs continue to exist, particularly in states that did not expand Medicaid. In states that expanded Medicaid, a number of methods to control costs have been pursued, including PCP assignment. Moreover, some states went so far as to dis-incentivize emergency department use by instituting penalties in the form of high co-pays on beneficiaries who used the emergency department for non-emergent needs (Cunningham, Sabik, and Bonakdar Tehrani 2016, Sabik and Gandhi 2016).

As this review indicates, there is little rigorous evidence on whether these primary care approaches are effective at controlling costs. We overcome this limitation by implementing a randomized controlled trial of cash incentives to visit an assigned PCP among patients newly provided coverage through a community-based PCP program. Using assignment to incentive groups, we then disentangle the influence PCPs have on utilization and costs from other unobservables that can drive PCP visits as well as utilization and costs. . This approach advances what is known about whether low-cost incentives encourage desired health seeking behavior (i.e., PCP visits), and more importantly whether PCPs are an effective mechanism through which incentives can steer health care utilization away from expensive emergency departments and inpatient care.

2. The experiment

Starting in August 2013, (Bradley and Neumark 2017)subjects were identified and enrolled through a community-based primary care program established by the Virginia (United States.) Commonwealth University Medical Center (VCUMC), the state's largest safety net provider (Bradley and Neumark 2017). The primary care program, known as Virginia Coordinated Care (VCC), provided access to primary care for uninsured subjects who had household incomes below 100% of the federal poverty level, had no other health insurance coverage, and resided within a 30-mile radius of VCUMC. Once enrolled in VCC, patients were assigned to a community-based PCP and provided free or low-cost care (Bradley et al. 2012).

2.1 The Virginia Coordinated Care program

The Virginia Coordinated Care program is funded by VCUMC and is intended to deliver care to uninsured adults who lived in the Richmond, Virginia metropolitan area. The study population is similar to the population that would have been newly enrolled in Medicaid had

Virginia expanded Medicaid during the time of the experiment.[3] That group, like the study population, comprises childless adults, many of whom are likely to have multiple chronic conditions. These adults are likely eligible for subsidized coverage on the health insurance exchange established under the ACA, but given their very low income, they are unlikely to purchase coverage.

Once enrolled in the program, patients were assigned to community-based primary care providers near where they lived. Primary care providers received a monthly management fee and fee-for-service rates comparable to approximately 110 percent of the Medicaid fee schedule in Virginia.

2.2 Intervention

Eligibility criteria for the randomized controlled trial were: no prior VCC coverage in the past 12 months or were a VCC re-enrollee with no PCP or specialist visit in the prior 9 months; aged 21-64 years; spoke English; and resided in the community (e.g., not homeless or living at a drug or alcohol rehabilitation facility). Subjects also had to have a phone number where they could be reached. We randomized subjects to groups so that there were equal distributions by gender, race, and 5-year age bands. Subjects were randomized to either the incentive group or a group of untreated controls.

Subjects assigned to the incentive groups were given $10 to complete a baseline survey. At the end of the baseline interview, subjects were told the group to which they had been randomly assigned and given six months to see their primary care provider. If we received a health care claim indicating that the subject saw their PCP with this six-month period, we paid the subject either $25 or $50 depending on group assignment. Subjects were paid only once and

[3] The Virginia legislature approved expanding Medicaid to low-income childless adults in 2018.

only for the first PCP visit. Subjects in the $0 group did not receive a payment other than the initial $10 to complete the baseline survey.

The untreated controls comprised 415 subjects who were not contacted by study interviewers, but for whom subjects' medical claim files were collected. The untreated controls allow us to observe behavior in the absence of activities associated with the randomized controlled trial. It is possible that the experiment has an independent effect, which would be underestimated if we only compared the $25 and $50 incentive groups to the $0 group. Following the baseline interview, 413 subjects were assigned to the $0 group, 407 to the $25 group, and 408 to the $50 group. Health care claims for all four groups (e.g., laboratory, diagnostic, other outpatient services, specialty care visits, emergency department visits, and inpatient stays) were collected for a 12-month period following study enrollment.

Institutional review boards at Virginia Commonwealth University and the University of Colorado approved the study protocol. The trial was registered with ClinicalTrials.gov (NCT02922855). The registration specified the outcomes we study in this paper in addition to self-reported mental and physical health outcomes that will be assessed in the future.

3. Methods

3.1 Outcomes

We separate the analysis into the first six-month period (when PCP visits were incentivized), and the subsequent six months (post-incentive period); the latter period provides the cleanest evidence on how the earlier PCP visit (or the incentives) influenced utilization and costs. We are most interested in whether PCP visits are associated with a reduction in emergency department visits, particularly visits for non-emergent care, which is where we expect PCP visits to be a preferable substitute for the emergency department. The experiment may also have

affected other categories of utilization, and it is not clear *a priori* that other types of utilization will decline. For example, due to diagnosis and treatment, follow-up visits to the PCP may occur along with referrals and visits to outpatient and specialty providers. For this reason, we also study these services. In the short-run these visits may increase health care costs, but if health status is improved over time, these visits, including expensive emergency department and inpatient utilization, should diminish. The evidence on utilization and costs in the second six-month period may give us some indication of longer-run effects on these outcomes, although an even longer time frame would provide more evidence.

We measure utilization in different ways, to capture what occurred, when it occurred, and intensity. Our primary outcomes are any PCP visit, any emergency department visit, any non-emergent visit, and any outpatient visit.[4] We also specify PCP, emergency department, and outpatient visits as two or more visits. Visit types were identified through the use of a service location variable in the claim files. Outpatient visits associated only with lab draws are not counted as separate visits. Once categorized into three service locations (emergency department, inpatient, and outpatient), outpatient visits were further categorized into PCP and outpatient and specialty care. The claims files originating from the VCC program contain a PCP indicator flag for all visits associated with a PCP. Outpatient visits not associated with a PCP are considered outpatient and specialty care. These specialty care visits occurred in doctors' offices, outpatient hospital clinics, provider-based clinics, and ambulatory surgical centers.

We convert billed charges to the Virginia Medicaid fee-for-service reimbursement rates using procedure codes. Medicaid reimbursement rates were chosen because the population we

[4] In addition, we estimated days until the first PCP visit during the initial six-month incentive period. Those who received either $25 or $50 saw the PCP approximately 19 and 25 days sooner respectively (results not shown).

study is most similar to Medicaid beneficiaries in states that expanded Medicaid to low-income childless adults. Reimbursement rates are intended to approximate costs and generally have less variability than charges.

Utilization and costs were estimated for all four groups—the three groups randomized to the incentivized arm and the untreated controls. In some estimations, we combine the incentive groups into a single category ($0, $25, or $50 vs. untreated; and $25 or $50 vs. $0). In addition, we estimate models for the three incentivized groups alone, providing comparisons across the levels of cash incentives.

We estimate models for several utilization outcomes in addition to total costs. To avoid the potential problem of searching for significant results, these outcomes were listed *a priori* in our clinicaltrials.gov registration. Nonetheless, we also report statistical results for our estimations accounting for multiple hypothesis testing, providing adjusted p-values in the appendix (Appendix Tables 2a, 2b, and 2c), based on the Simes procedure to control the "false discovery rate." In some cases, the adjusted p-values become larger than those conventionally associated with statistical significance, meaning that in these cases, our unadjusted tests falsely reject the hypothesis of no effect. Nonetheless, the findings remain qualitatively important and suggestive of areas where a PCP visit (or incentive group) may increase (or decrease) other categories of utilization or cost. In addition, we perform power calculations for each outcome we study, and footnote in the tables coefficients that exceed the one-sided detectable effect size calculated for 80% power and alpha = 0.05.

3.2 Control variables

We had data from administrative records on sex, age, race/ethnicity, and marital status for all subjects who participated in the study. Subjects enrolled in the incentivized arm were

interviewed prior to randomization, with additional information collected on: education (high school diploma or less, some college degree, and Bachelor's degree or better); employed (yes/no); monthly income (<$1500, $1500 to $2000, and $2001+); smoking status (yes/no); drug/alcohol problems (yes/no); whether the subject had health insurance prior to enrollment in VCC (yes/no); whether subject's usual source of care was in the emergency department; and whether the patient had two or more chronic conditions. The interviews also elicited additional information about the frequency of emergency department utilization in the 12 months prior to enrollment; visits were categorized as 0 to 1, 2 to 5, and 6 or more. We also calculated respondents' composite scores on a subset of the PROMIS domains (e.g., depression, anxiety, social role, pain interference). PROMIS is a psychometrically sound instrument that measures patient reported physical, mental, and social domains (http://www.healthmeasures.net). (National Institute of Health 2017) Higher scores for the depression, anxiety, and pain interference domains indicate worse health status, and a higher score for social role indicates better status. We measured satisfaction with the health care system, referring back to their last health care visit regardless of provider type and prior to randomization, using the Patient Satisfaction Questionnaire (PSQ)-18 (https://www.rand.org/health/surveys_tools/psq.html). (Health 2017) The PSQ measures satisfaction with medical care by addressing six aspects of care: technical quality, interpersonal manner, communication, financial aspects of care, time spent with doctor, and accessibility of care. Satisfaction with prior health care providers is likely to influence future use and the provider from whom a patient seeks care. We also include physician fixed effects for the assigned PCP, regardless of whether the patient had a visit.

3.3 Analytical approach

Differences between the incentive groups and untreated controls were analyzed using χ^2 tests for categorical variables and *t*-tests for difference in means.

We used two-stage least squares (2SLS) to estimate whether PCP visits incentivized by the experiment changed health care utilization and costs, using the experimental assignment to cash incentive groups or the untreated control group as an exogenous source of variation in PCP visits. In the first stage, we estimate a model for the probability of a PCP visit during the first six months (the "incentive period," as indicated by the *1* subscript), using:

$$PCP_{i1} = \pi_0 + GRP_i\pi_1 + X_i\pi_2 + \upsilon_{i1} \qquad (1)$$

where PCP_{i1} is measure of PCP visits during the first six months. Correspondingly, we define PCP_{i2} as the corresponding measure for subject *i* seeing a PCP in the second six-month period following the incentive period (the "post-incentive" period). In our analyses, PCP_{i1} and PCP_{i2} are simply indicator variables for any PCP visit. The coefficients in π_1 capture the effect of group assignment—whether to untreated controls, $0, $25, or $50, or, in some specifications we estimate, a combination of cash incentives. X includes dummy variables for each PCP, to control for differences in PCP characteristics that might affect access or other outcomes (e.g., ease of getting an appointment), as well as other controls listed in the table notes and described above. We estimate equation (1) using a linear probability model.

In the second stage, we estimate the effects of a single PCP visit on measures of utilization, and on costs in the incentive period and the post-incentive period—denoted generically Y_{i1} and Y_{i2}—using the predicted probability of a PCP visit from equation (1) as an instrumental variable for PCP_{i1} in the equation:

$$Y_{ij} = \alpha_Y + \beta_Y PCP_{i1} + X_i\gamma_Y + \varepsilon_{ij}^Y, j = 1,2 \qquad (2)$$

where *Y* can represent each of the dependent variables we study (e.g., emergency department use, outpatient use, cost).

We offer two sets of estimations, one based on a stronger set of instruments that includes assignment to the different incentive groups and the untreated control group, and a second set of instruments (for a correspondingly smaller sample) including only the assignment to the different incentive groups. The first set of instruments incorporates the experimental effect of contacting subjects and asking questions about their health and potentially offering information about the VCC. The second set of instruments is weaker, but perhaps more valid given the uniform exposure to the experimental conditions. We test the strength of the instruments using the Kleibergen-Paap Wald F-statistic and the five tests invoked by the *weakiv* command in Stata.[5,6]

To study the reduced form effects of assignment to incentive groups, we estimated Ordinary Least Squares (OLS) regressions for the outcomes of interest, including our controls, estimating the effects of group assignment ($0, $25, $50, and untreated controls in estimations that included all four groups; and ($0, $25, and $50 in estimations that included just the incentive groups). These estimations are informative about the effects of different levels of cash incentives to which subjects were randomized, and about assignment to the incentive arm. We use models of the form:

$$Y_{ij} = \pi_0{}'_Y + GRP_i\pi_1{}' + X_i\pi_2{}' + \upsilon^Y{}_{ij} \ , j = 1,2 \quad (3)$$

Like for equation (2), we estimate equation (3) for the first six-month period, and for the second six-month period, indexed by *j*. We estimate equation (3) using OLS, which is a linear

[5] 10 subjects were missing information on a few variables. We used multiple imputation methods with 10 imputations to impute missing data in the regressions. Results from the regressions using imputed data were nearly identical to those obtained when subjects with missing data were dropped.

[6] We also estimate equations (1) and (2) for some of our discrete utilization measures using an IVPROBIT model. The estimated marginal effects were nearly the same as the coefficients reported for the 2SLS model (available upon request).

probability model for discrete outcomes such as any ED visits, it is a linear probability model. Our outcomes also include 2 or more emergency department visits (any non-emergent ED visits), any and two or more outpatient and specialty visits, and total costs.

We report estimations for total costs summed across each category of utilization. Total costs were skewed to the right and there were 187 subjects without claims. Therefore, we transformed costs into its natural logarithm and replaced zeroes with the minimum value minus 0.01. We also tested specifications using the inverse hyperbolic sine (IHS) of costs, which can accommodate zeroes and skewness and is interpreted as a percentage much in the same way as coefficients derived from the natural log transformation (Ravallion 2017). Because it is likely that the zeroes reflect no utilization during the study period, the IHS transformation may better represent observed behavior in the study sample.[7,8]

In all estimations, standard errors are clustered at the physician level. Analyses were conducted using SAS version 9.3 (SAS Institute) and Stata version 14.0 (Statacorp).

4. Results

4.1 Descriptive statistics

Table 1 reports demographic and health characteristics. Recall that many of these are measured from the interviews, which were administered only to the cash incentive groups. The groups were generally well balanced, although the $50 group reported slightly lower social function ($p<0.10$), less satisfaction with financial aspects of care ($p<0.05$), and more emergency department visits than observed in the $0 and $25 group ($p<0.10$).

4.2 Utilization

[7] In alternative estimations (not shown), we handled zeroes by adding 1 before transforming costs to the natural logarithm. This resulted in slightly larger coefficients, but with very qualitatively similar results.
[8] We also estimated total costs using GLM with the log link function. Coefficients are not statistically signficant and are reported in the appendix (available on request).

Table 2 reports unadjusted health care utilization and costs differences between the different experimental groups. In the first six months—when PCP visits were incentivized—subjects randomized to the cash incentive arm visited a PCP more often than subjects in the untreated control group ($p<0.01$), and the subjects in the $25 and $50 groups saw a PCP more often than subjects in the $0 group ($p<0.10$, $p<0.05$, respectively). In the second six-month period, after the incentives for a PCP visit in the first six-month period, the number of PCP visits was statistically significantly higher for the $25 and $50 groups individually, and combined, relative to the untreated controls and relative to the $0 group, suggesting that an ongoing relationship with the PCP may have been established following the initial incentivized visit.

Table 2 shows that emergency department visits declined for all groups in the second six-month period, and inpatient visits were uncommon among all subjects.[9] Outpatient and specialty care visits declined for all groups from the initial six-month period to the second six-month period. There is evidence that outpatient and specialty visits were higher in the incentivized groups, in both periods (for both relative to the untreated controls, and relative to the $0 incentive group). One might expect these visits to increase along with an increase in PCP utilization, at least in the short run, because diagnostic tests are performed and referrals are made for treatment.

Median total costs in the first six months following study enrollment ranged between $860 and $1,193 across the groups. In the second six-month period, median costs were considerably lower for all groups. Mean total costs were much larger than the median, ranging from $4,723 ($0 incentive group) to $5,290 (untreated controls) in the first six-month period. In

[9] We do not report the estimations from regression models for inpatient visits because few patients had inpatient stays and the results were statistically insignificant. Estimates are available upon request.

the second six-month period, untreated controls experience the greatest decline in mean total costs (down to $3,298).

We note two concerns with the cost distribution. First, 187 observations report no health care utilization (true zeroes); these observations are, unsurprisingly, disproportionately in the untreated and $0 groups (31.5% in each). We compared baseline characteristics of these subjects to those of subjects with claims. Among those interviewed, subjects without claims had fewer chronic conditions, greater social role function, lower pain interference, and lower satisfaction with health care providers, suggesting that they may have been in better health than other subjects and may have had a lower need (or desire) to seek a PCP visit or other forms of care.[10] We tested the sensitivity of the results to excluding these subjects from the sample and found that results were virtually unchanged (results not shown). Second, costs over the 12 months have a wide range—from $18 to $203,592—leading to a distribution skewed to the right. The 95th percent of costs exceeds $41,500, and is similar across the groups. We address the issues of zero costs and a strongly skewed distribution in our analytical approach.

4.3 Effects of PCP visits

Table 3 reports the estimates from a linear probability model that predicts the likelihood of any PCP visit. These are the first-stage estimates used in subsequent 2SLS models. In addition to the full models, we report estimates where we combine the cash incentive groups and do a simple comparison to the untreated controls (in the top panel), and where we combine the $25 and $50 incentive groups and do a simple comparison to the $0 incentive group (in the lower panel). These specifications provide more parsimonious comparisons, and we do the same in the

[10] Note that overrepresentation of no claims cases among the untreated controls and $0 incentive group does not imply a lack of balance between groups. Rather, those in less need of health care appear to have been less likely to seek out health care if assigned to the untreated controls or the $0 incentive group.

reduced form estimations reported later. However, in the 2SLS estimation, we use the first-stage equations in which the groups are separated.

The incentive groups ($25 and $50) were statistically significantly more likely to visit the PCP than the untreated controls ($p<0.01$, top panel), by 9 and 12 percentage points, respectively. Relative to the $0 group, the $50 group was nearly 7 percentage points more likely to visit the PCP ($p<0.01$) and the incentive groups combined were 5.5 percentage points more likely to visit the PCP than the $0 group ($p<0.01$).

Table 4 reports estimates from the two-stage least squares estimations. In the first six-month period, the estimates indicate that a PCP visit increased outpatient visits by about 56 percentage points for any outpatient visit and 58 percentage points for two or more visits in the estimates using the incentive groups and the untreated controls. For these same groups, the specifications for the natural log of costs indicate that costs increased considerably as a result of a PCP visit (about 6.5 times higher; $e^{2.08}$-1). The IHS transformation yielded a similar estimate (about 4 times higher costs). Total costs in the first six-month period were also higher for the analysis limited to the incentive groups.

However, in the second six-month, post-incentive, period only the coefficient for non-emergent visits, using the incentive groups and the untreated controls, was statistically significant. In this analysis, a PCP visit led to an approximately 19 percentage points lower

probability of having a non-emergent visit in the emergency department relative to untreated controls ($p<0.10$). [11,12]

We note that the F tests for the instruments were above 10 (see the table notes), the generally accepted cut-off for weak instruments, when using all four groups (incentive groups plus the untreated controls) in the IV estimations. The F tests are less than 10 when using only the three groups randomized to incentives. The footnote to Table 4 also explains how the table reports results from the other weak instruments tests. For most outcomes, these tests further suggest that instruments are generally strong when using the incentive groups and untreated controls, and also for the cost estimations when using only the incentive groups. Two possibilities may weaken the results when using the incentive groups only to estimate emergency department and outpatient visits. First, there could be a strong experimental effect in addition to the incentives, and we lose that variation when we use only the incentive groups. Second, given the better PROMIS scores and lower emergency department utilization reported at baseline, the health status of the $0 group may be better than health status of the $25 and $50 groups, which may be why there was a higher proportion of no health care utilization in the $0 group. However, we controlled for health status based on interview data, to the extent possible.

4.4 Effect of group assignment (reduced form estimations)

[11] In addition, in the appendix (Appendix Table 1) we report coefficients from estimating the effect of a PCP visit in the first six months on the *number* of PCP, emergency department, and outpatient visits. Positive and statistically significant coefficients were found for PCP and outpatient visits in the second six-month period when using the incentive groups and the untreated controls. A positive and statistically significant coefficient was also found for outpatient visits in the second six-month period when using only the incentive groups.

[12] In this case, and for many other results, the 2SLS estimate is larger than the mean of the dependent variable. That can happen, of course, especially with the rescaling of the reduced form estimate by the first-stage estimate. The reduced form estimates reported below reveal how the experimental assignment affected the dependent variables we study, and these estimates are smaller than the dependent variable means. We also report results for number of visits as a measure of utilization in the appendix (Appendix Table 1). There is evidence that PCP visits increased the likelihood of additional PCP visits by more than 2 additional visits ($p<0.01$) and outpatient visits by 4-6 visits ($p<0.10$).

Next, we turn to reduced form estimations for insight into the effects of the experimental assignment (Table 5). For the second six-month, post-incentive, period, the estimated effects of the experiment on emergency department utilization suggests a reduction—in particular, a lower likelihood of two or more emergency department visits when we compare the cash incentive groups to the untreated controls. Relative to the untreated controls, subjects in the $0 incentive group were 4.9 percentage points ($p<0.01$) less likely to have two or more emergency department visits, and this differential is not statistically significantly larger for the $25 or $50 groups (3.4 percentage points). This evidence suggests that participation in the experiment, on its own, had an effect on behavior. Those assigned to any incentive group were 3.9 percentage points less likely to have two or more emergency department visits than the untreated controls. Non-emergent visits to the emergency department declined in the second six-month period (by 3 and 2.3 percentage points for $25 and $50 groups, respectively) relative to untreated controls.

The last two columns of Table 5 report results for outpatient visits. In the second six-month period, the estimated differentials are not statistically significant, except for the effect on any visit for the $25 incentive group relative to the untreated controls (6.3 percentage points increase). Further investigation into the reason for these visits revealed that several subjects in the $25 group were receiving daily radiation treatments, leading to the continued high use of outpatient and specialty care.

Finally, Table 6 reports cost results. In the initial six-month period, we observe statistically significant coefficients for the $50 group. The $50 group had about 32% ($p<0.10$) higher costs ($e^{0.276}-1$) than the untreated controls. Among the incentive groups, the $50 group had 29% higher costs than the $0 group ($p<0.05$). The inverse hyperbolic sine transformation yielded similarly qualitative results, but with larger coefficients. In the second six-month period,

after the experimental incentives for a PCP visit applied, there is no clear evidence of higher costs, and the estimated differentials are smaller.

4.5 Who responds to incentives?

The next set of estimations tests whether subjects who are healthier responded differently to the incentives for PCP visits than those who have health conditions that require treatment or monitoring. Policymakers and program administrators are likely to be interested in knowing whether the incentives were more effective for subjects who had health conditions and needed to see a PCP, or instead stimulated wellness visits where, to the subject's knowledge, no underlying health condition existed. These visits may be a concern if they stimulate unnecessary health care utilization. In the first case, the incentives "nudge" a subject who may be delaying needed care, while in the second case, the incentives encourage preventive behavior or arguably, unnecessary visits. In addition, if unhealthy subjects were prompted to see the PCP, then changes in subsequent utilization were more likely due to improvements in health, in addition to alterations in health care seeking patterns.

We define "healthy" three ways: 1) no drug or alcohol problems, no self-reported high blood pressure, diabetes, lung disease or cancer, no more than one emergency department visit in the past 12 months, no anxiety or depression; 2) meets the conditions for definition 1 but can report high blood pressure; and 3) self-report excellent or very good health status. Because these definitions rely on self-reported data, we cannot include the untreated controls in the analysis.

Table 7 reports the findings. There is rather clear evidence that the increases in PCP visits owing to the experimental cash incentives were driven by the unhealthy sample. In contrast, there were no significant effects for the healthy samples. (The healthy samples are smaller and the estimates less precise, but the estimates are not consistently positive, whereas every estimate

is positive for the unhealthy samples.) This evidence might be viewed positively, as indicating that the incentives did not appear to encourage unnecessary visits. Furthermore, this evidence suggests that our evidence of increases in outpatient and specialty visits, and in costs, was likely to have arisen from the treatment of adverse health conditions prompted by the PCP visit—consistent with the evidence of more visits and costs being stronger in the initial six-month period when the incentivized PCP visit would have occurred. These patients may have been diverted from the emergency department for treatment of non-emergent conditions.

5. Conclusions

We test the effect of a small cash incentive program to encourage an initial primary care provider (PCP) visit and use the experimentally-induced variation in PCP visits to estimate the effects of PCP visits on outpatient and emergency department utilization, and on total costs, using two-stage least squares (2SLS) estimations to control for endogenous variation in the likelihood of an initial PCP visit that could be correlated with other unmeasured determinants of health care utilization or costs. We estimate utilization and costs for two periods, the first six months following enrollment in the experiment, during the period when the experiment provides incentives for a PCP visit, and second six months following the incentive period.

The groups assigned to positive cash incentives for a PCP visit were more likely to have a PCP visit and outpatient and specialty care visits relative to untreated controls, and to have higher costs, during the six-month incentive period. The 2SLS estimates of the effects of PCP visits on utilization and costs point to PCP visits increasing utilization and costs during the first six months of the experiment, when subjects were incentivized to visit their PCP. In the second six-month, post-incentive period, utilization and costs were no higher for those who visited their PCP, but there is some evidence that non-emergent ED visits were lower. This evidence suggests

that the incentives to visit the PCP led to diagnosis and treatment of medical conditions, but subsequently may have helped divert patients from non-emergent ED care.

There is other evidence consistent with this interpretation. First, we find that subjects who saw the PCP continued to do so after the initial visit. Therefore, if a policymaker's goal is to increase PCP utilization and encourage low-income previously uninsured adults, particularly those who have health care needs, to establish a relationship with a PCP, small cash incentives may be effective and have other positive effects including fewer non-emergent emergency department visits. Total costs, though, are unlikely to decrease—and more likely to increase—in the short-term. Interestingly, we find some evidence of lower ED use even for patients assigned to the $0 incentive group (in comparison to untreated controls), which suggests that simply calling newly enrolled beneficiaries and explaining what it means to be assigned to a PCP—as we did in the experiment—may also achieve some benefits.

The second type of evidence consistent with the interpretation that the incentives to visit the PCP led to diagnosis and treatment of medical conditions, is that the effects on PCP visits were apparent for unhealthy subjects, but not for healthy subjects. This evidence suggests that patients who needed health care may have been steered away from the emergency department, particularly for non-emergent care, and toward PCP visits. Put differently, this evidence suggests that the incentives did not stimulate unnecessary PCP utilization among the healthy.

Overall, we conclude that in a low-income previously uninsured sample with poor baseline health, small cash incentives are effective at encouraging a PCP visit and perhaps effective at leading to a longer-term relationship with a PCP and fewer non-emergent emergency department visits, but may result in higher health care costs in the short-term. Realistically, given the poor health status of the patient population, use of the emergency department for non-

emergent visits may be the only outcome we can expect a PCP to reduce in the short-term by offering a viable substitute for the emergency department.

6. Discussion

Practical implementation of an incentive program modeled on our experiment could be costly. Nonetheless, existing programs to steer patients away from expensive utilization are also costly, and an incentive program could be complementary to or a substitute for such a program. The population most like the subjects who participated in our experiment are those newly enrolled in U.S. Medicaid expansions to low-income childless adults. It is conceivable that case workers could contact newly enrolled beneficiaries to assess baseline health and inform them about PCP assignment and other benefits, which is equivalent to our $0 incentive group. If the effects were similar to those in our data, in which approximately 68% of the $0 group go to the PCP, versus about 61% of subjects with no contact with study coordinators (Bradley and Neumark, 2017), a 7 percentage points increase can be achieved by personal contact with patients, without incentives. Approximately 75% of incentive patients, regardless of whether they received an incentive of $25 or $50, go to the PCP. Roughly, an average of 300,000 non-aged, non-disabled adults newly enroll annually in Medicaid.[13] Therefore, a $25 incentive would cost $7.5 million to gain a 14 percentage point increase in PCP utilization. Although we do not show a short-term cost savings associated with PCP visits, a cost reduction is not the only reason an insurance provider would want to incentivize PCP visits. There may be longer-term health benefits (and potentially cost savings), improved quality of life, and higher patient satisfaction

[13] See https://www.medicaid.gov/medicaid/program-information/medicaid-and-chip-enrollment-data/report-highlights/index.html and https://www.kff.org/medicaid/state-indicator/distribution-of-medicaid-enrollees-by-enrollment-group/?currentTimeframe=0&sortModel=%7B%22colId%22:%22Location%22,%22sort%22:%22asc%22%7D.

associated with having an established PCP relationship. We await additional data from our study to address these outcomes.

An incentive program would need to be evaluated in terms of its cost-effectiveness. For example, following an evaluation of preventive care in Japan, researchers concluded that subjects respond to health signals from wearable devices and seek medical checkups (Iizuka et al. 2017). These researchers caution that while checkups can prevent chronic diseases, they may also result in over-use of care. In contrast, if the threshold for a health signal (analogous to our incentives) is set too low, then medical care could be underutilized. Additional research is needed to determine an incentive level that results in efficient use of care and to evaluate such a program in terms of its long-term cost-effectiveness.

More time may be required to improve health status, as opposed to altering health seeking patterns, in this population. When health conditions are severe, a PCP visit—even multiple visits—may not be sufficient to improve health status. Moreover, the evidence suggests that the incentives, by encouraging an initial PCP visit, initially boost outpatient and specialty visits. This evidence suggests that part of the effect of the PCP visit our experiment induced is that the PCP detects heath conditions, orders diagnostic tests, and refers subjects to specialists, resulting in an increase in outpatient utilization—some of which may be unnecessary. The frequency of these visits lessens in the second six-month period that follows the incentivized PCP visit, suggesting either resolution (or lessening) of the health care problem, or determination of a treatment course that does not require additional outpatient follow-up.

There are four main limitations to our study. First, the study is limited to a 12-month period with only six months following the incentive period. This may be insufficient time to observe whether the PCP relationships are sustained in the longer term, and if, over the longer

term, these relationships improved health status and altered long-term utilization patterns. Second, it is possible that some subjects received health care outside the VCU medical center. However, visits outside the medical center are likely to be minimal. Subjects reside within 30 miles of the medical center, which makes the center a convenient location for health care. The center is the largest safety-net provider in the region and offers comprehensive care. More importantly, when subjects received care outside the VCU medical center, VCC often received a claim, which we included in our analyses. Third, our study is specific to a U.S. urban, low-income safety net population with many health care needs, and may not generalize outside the U.S. safety net, or to higher-income, healthier, or rural populations. Fourth, we assume that health care utilization and costs were only affected by a PCP visit in our two-stage estimations. However, there are other conditions like patient income and differences in patient health status that may affect utilization. In this very low-income population (90% with monthly income below $1,500), income is uniformly low, which may limit whether the results can be generalized to a wealthier population who may require larger incentives to achieve the same results. Health status, on the other hand, may have been better in the $0 group as evidenced by a disproportionate number with no health care utilization.

It has been hypothesized that, in addition to pent-up demand, an important source of higher utilization following Medicaid coverage among the previously uninsured in the U.S. is inadequate access to primary care (Heintzman et al. 2014). Arguments have been made for having both health insurance coverage and a usual source of care in the primary care setting (DeVoe et al. 2003, DeVoe et al. 2011). Using a rigorous study design and capitalizing on exogenous variation in the likelihood of an initial PCP visit, we show modest evidence that PCPs reduce non-emergent emergency department utilization in the short-term and may, in fact,

initially increase costs. In the high health needs patients we study, in which nearly two-thirds report two or more chronic conditions, PCPs may have very little ability to reduce health care utilization in the short-term. Health status, among the very sick, may take years to improve. Ultimately, however, longer-term evidence is needed to determine whether policies or programs that encourage PCP visits lead to a sustained relationship with that provider and alter patterns of care so as to improve health and reduce costs.

References

Asplin, B. R., K. V. Rhodes, H. Levy, N. Lurie, A. L. Crain, B. P. Carlin, and A. L. Kellermann. 2005. "Insurance status and access to urgent ambulatory care follow-up appointments." *Jama* 294 (10):1248-54. doi: 10.1001/jama.294.10.1248.

Bazzoli, G. J., N. Fareed, and T. M. Waters. 2014. "Hospital financial performance in the recent recession and implications for institutions that remain financially weak." *Health Aff (Millwood)* 33 (5):739-45. doi: 10.1377/hlthaff.2013.0988.

Block, L., S. Ma, M. Emerson, A. Langley, L. Torre Dde, and G. Noronha. 2013. "Does access to comprehensive outpatient care alter patterns of emergency department utilization among uninsured patients in East Baltimore?" *J Prim Care Community Health* 4 (2):143-7. doi: 10.1177/2150131913477116.

Bradley, C. J., S. O. Gandhi, D. Neumark, S. Garland, and S. M. Retchin. 2012. "Lessons for coverage expansion: a Virginia primary care program for the uninsured reduced utilization and cut costs." *Health Aff (Millwood)* 31 (2):350-9. doi: 10.1377/hlthaff.2011.0857.

Bradley, C. J., and D. Neumark. 2017. "Small Cash Incentives Can Encourage Primary Care Visits By Low-Income People With New Health Care Coverage." *Health Aff (Millwood)* 36 (8):1376-1384. doi: 10.1377/hlthaff.2016.1455.

Buchmueller, T. C., K. Grumbach, R. Kronick, and J. G. Kahn. 2005. "The effect of health insurance on medical care utilization and implications for insurance expansion: a review of the literature." *Med Care Res Rev* 62 (1):3-30. doi: 10.1177/1077558704271718.

Cunningham, P., L. M. Sabik, and A. Bonakdar Tehrani. 2016. "Trends in Hospital Inpatient Admissions Following Early Medicaid Expansion in California." *Med Care Res Rev*. doi: 10.1177/1077558716669433.

DeHaven, M., H. Kitzman-Ulrich, N. Gimpel, D. Culica, L. O'Neil, A. Marcee, B. Foster, M. Biggs, and J. Walton. 2012. "The effects of a community-based partnership, Project Access Dallas (PAD), on emergency department utilization and costs among the uninsured." *J Public Health (Oxf)* 34 (4):577-83. doi: 10.1093/pubmed/fds027.

DeVoe, J. E., G. E. Fryer, R. Phillips, and L. Green. 2003. "Receipt of preventive care among adults: insurance status and usual source of care." *Am J Public Health* 93 (5):786-91.

DeVoe, J. E., C. J. Tillotson, S. E. Lesko, L. S. Wallace, and H. Angier. 2011. "The case for synergy between a usual source of care and health insurance coverage." *J Gen Intern Med* 26 (9):1059-66. doi: 10.1007/s11606-011-1666-0.

Finkelstein, A. N., S. L. Taubman, H. L. Allen, B. J. Wright, and K. Baicker. 2016. "Effect of Medicaid Coverage on ED Use - Further Evidence from Oregon's Experiment." *N Engl J Med* 375 (16):1505-1507. doi: 10.1056/NEJMp1609533.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, and K. Baicker. 2012. "THE OREGON HEALTH INSURANCE EXPERIMENT: EVIDENCE FROM THE FIRST YEAR." *Q J Econ* 127 (3):1057-1106.

Franks, P., C. Cameron, and K. D. Bertakis. 2003. "On being new to an insurance plan: health care use associated with the first years in a health insurance plan." *Ann Fam Med* 1 (3):156-61.

Gold, H. T., K. Pitrelli, M. K. Hayes, and M. M. Murphy. 2014. "Decision to adopt medical technology: case study of breast cancer radiotherapy techniques." *Med Decis Making* 34 (8):1006-15. doi: 10.1177/0272989X14541679.

Gold, R., S. R. Bailey, J. P. O'Malley, M. J. Hoopes, S. Cowburn, M. Marino, J. Heintzman, C. Nelson, S. P. Fortmann, and J. E. DeVoe. 2014. "Estimating demand for care after a medicaid expansion: lessons from Oregon." *J Ambul Care Manage* 37 (4):282-92. doi: 10.1097/jac.0000000000000023.

Health, RAND. 2017. "Patient Satisfaction Questionnaire from RAND Health." https://www.rand.org/health/surveys_tools/psq.html.

Heintzman, J., R. Gold, S. R. Bailey, and J. E. DeVoe. 2014. "The Oregon experiment re-examined: the need to bolster primary care." *Bmj* 349:g5976. doi: 10.1136/bmj.g5976.

Johnson, T. L., D. J. Rinehart, J. Durfee, D. Brewer, H. Batal, J. Blum, C. I. Oronce, P. Melinkovich, and P. Gabow. 2015. "For many patients who use large amounts of health care services, the need is intense yet temporary." *Health Aff (Millwood)* 34 (8):1312-9. doi: 10.1377/hlthaff.2014.1186.

Lee, J., R. Ding, S. L. Zeger, A. McDermott, G. Habteh-Yimer, M. Chin, R. S. Balder, and M. L. McCarthy. 2015. "Impact of Subsidized Health Insurance Coverage on Emergency Department Utilization by Low-income Adults in Massachusetts." *Med Care* 53 (1):38-44. doi: 10.1097/mlr.0000000000000279.

Mackinney, T., A. M. Visotcky, S. Tarima, and J. Whittle. 2013. "Does providing care for uninsured patients decrease emergency room visits and hospitalizations?" *J Prim Care Community Health* 4 (2):135-42. doi: 10.1177/2150131913478981.

National Institute of Health. 2017. "PROMIS - Patient-Reported Outcomes Measurement Information System." Healthmeasures.net. http://www.healthmeasures.net/index.php?option=com_content&view=category&layout=blog&id=147&Itemid=806.

O'Malley, J. P., M. O'Keeffe-Rosetti, R. A. Lowe, H. Angier, R. Gold, M. Marino, B. Hatch, M. Hoopes, S. R. Bailey, J. Heintzman, C. Gallia, and J. E. DeVoe. 2016. "Health Care Utilization Rates After Oregon's 2008 Medicaid Expansion: Within-Group and Between-Group Differences Over Time Among New, Returning, and Continuously Insured Enrollees." *Med Care* 54 (11):984-991. doi: 10.1097/mlr.0000000000000600.

Peterson, K., M. Helfand, L. Humphrey, V. Christensen, and S. Carson. 2011. "VA Evidence-based Synthesis Program Reports Evidence Brief: Effectiveness of Intensive Primary Care Programs." In *VA Evidence-based Synthesis Program Evidence Briefs*. Washington (DC): Department of Veterans Affairs (US).

Pickens, G., Z. Karaca, E. Cutler, M. Dworsky, C. Eibner, B. Moore, T. Gibson, S. Iyer, and H. S. Wong. 2017. "Changes in Hospital Inpatient Utilization Following Health Care Reform." *Health Serv Res*. doi: 10.1111/1475-6773.12734.

Pitts, S. R., E. R. Carrier, E. C. Rich, and A. L. Kellermann. 2010. "Where Americans get acute care: increasingly, it's not at their doctor's office." *Health Aff (Millwood)* 29 (9):1620-9. doi: 10.1377/hlthaff.2009.1026.

Ravallion, Martin. 2017. "A concave log-like transformation allowing non-positive values." *Economics Letters* 161:130-132. doi: 10.1016/j.econlet.2017.09.019.

Sabik, L. M., and S. O. Gandhi. 2016. "Copayments and Emergency Department Use Among Adult Medicaid Enrollees." *Health Econ* 25 (5):529-42. doi: 10.1002/hec.3164.

Sommers, B. D., B. Maylone, R. J. Blendon, E. J. Orav, and A. M. Epstein. 2017. "Three-Year Impacts Of The Affordable Care Act: Improved Medical Care And Health Among Low-Income Adults." *Health Aff (Millwood)* 36 (6):1119-1128. doi: 10.1377/hlthaff.2017.0293.

Table 1. Sample characteristics and health risks by incentive group and untreated controls, N=1,643

	Untreated controls (N=415)	$0 (N=413)	$25 (N=407)	$50 (N=408)
Demographic characteristics				
Female	43.85	46.24	46.92	48.52
Race			++	+
White	28.67	29.29	27.27	28.43
Black	64.09	66.34	68.79	67.15
Other	6.98	3.87	2.70	3.43
Missing	0.24	0.48	1.22	0.98
Hispanic	2.65	2.18	1.97	3.19
Missing	1.20	0	0	0
Married/partnered	10.84	13.55	9.82*	12.00
Missing	0.48	0.73	0.98	0.49
Mean age	44.93 (11.20)	45.90 (11.22)	45.75 (10.95)	45.50 (11.07)
Education	N/A			
High school or less		59.32	57.73	64.95
Some college		30.75	31.69	25.49
Bachelor's degree or higher		7.99	9.82	9.31
Missing		1.93	0.73	0.24
Monthly income	N/A			
<$1500		91.76	92.38	94.60
$1500 - $2000		5.56	3.68	3.67
>$2000		2.17	3.43	1.47
Missing		0.48	0.49	0.24
Employed	N/A	27.11	29.23	24.26
Health insurance coverage prior to VCC	N/A	22.51	22.35	23.03
Health conditions and behaviors				
Smoker	N/A	49.15	46.68	51.96
Missing		0	0	0.49
Drug/alcohol problems	N/A	16.22	14.25	12.74
Missing		0.48	0.24	0.73
2 or more chronic conditions	N/A	60.53	65.36	62.50
PROMIS score	N/A			
Anxiety		54.32 (11.21)	53.51 (11.10)	55.46 (11.60)
Depression		54.18 (11.46)	53.51 (11.04)	55.35 (11.85)
Social role		45.67 (11.67)	45.92 (11.63)	44.19 (11.00)*
Pain interference		57.95 (11.53)	58.97 (11.40)	58.99 (11.35)
Missing		2.17	3.43	5.14
PSQ-18 score	N/A			
General satisfaction		3.45 (0.98)	3.48 (0.91)	3.44 (0.96)
Technical quality		3.51 (0.70)	3.49 (0.69)	3.44 (0.73)
Interpersonal manner		3.69 (0.77)	3.70 (0.72)	3.63 (0.71)
Communication		3.72 (0.79)	3.76 (0.78)	3.68 (0.82)
Financial aspects		3.25 (1.01)	3.26 (0.96)	3.10 (1.02)**
Time spent with doctor		3.46 (0.90)	3.51 (0.89)	3.39 (0.92)
Accessibility and convenience		3.21 (0.84)	3.21 (0.83)	3.22 (0.78)

	Untreated controls (N=415)	$0 (N=413)	$25 (N=407)	$50 (N=408)
Missing		4.35	4.17	4.41
Get most prior care at the ED	N/A	37.28	33.90	40.44
Prior ED utilization	N/A			*
0 to 1		56.66	56.01	47.06
2 to 5		35.10	35.87	44.60
6 or more		8.23	7.86	8.33
Missing		0	0.24	0

Notes: N/A = not applicable; ED=Emergency Department; standard deviations are in parentheses for age and PROMIS scores. Remaining results are reported as percentages. Information on chronic conditions considered for the variable "two or more chronic conditions" was collected from the patient during the initial interview. Tests of statistical significance relative to the untreated controls, and relative to the $0 incentive group, were estimated using the χ^2 test for categorical variables, and the two-sample *t*-test for continuous variables. For tests relative to the $0 incentive group, statistical significance is reported as: $^{*}p<0.10$, $^{**}p<0.05$. For tests relative to the untreated control group, statistical significance is reported as: $^{+}p<0.10$, $^{++}p<0.05$. These symbols are reported in separate rows for the tests for categorical variables (for the label corresponding to the categories).

Table 2. Utilization and costs by incentive groups and untreated controls, N=1,643

	Untreated controls (N=415)	**$0 (N=413)**	**$25 (N=407)**	**$50 (N=408)**	**$0, $25, $50 (N=1,228)**	**$25, $50 (N=815)**
1st 6 months (incentive period)						
Any PCP visit	0.62 (0.02)	0.68 (0.02)$^{+}$	0.73 (0.02)$^{+++}$	0.76 (0.02) $^{+++**}$	0.72 (0.01)$^{+++}$	0.75 (0.02)$^{+++**}$
PCP visits	2.08 (0.13)	2.28 (0.12)	2.64 (0.15)$^{+++*}$	2.70 (0.14)$^{+++**}$	2.54 (0.08)$^{+++}$	2.67 (0.11)$^{+++**}$
ED visits	0.68 (0.07)	0.59 (0.06)	0.64 (0.07)	0.57 (0.05)	0.60 (0.03)	0.61 (0.04)
Non-emergent ED visits	0.05 (0.01)	0.06 (0.01)	0.03 (0.01)*	0.05 (0.01)	0.05 (0.01)	0.04 (0.01)
Inpatient admissions	0.13 (0.03)	0.12 (0.02)	0.11 (0.02)	0.12 (0.02)	0.12 (0.01)	0.12 (0.01)
Outpatient & specialty visits	2.69 (0.25)	2.93 (0.24)	3.47 (0.24)$^{++}$	3.15 (0.21)	3.18 (0.13)$^{+}$	3.31 (0.16)$^{++}$
Median total costs ($)	860	1,036	1,193	1,187	1,144	1,188
Mean total costs ($)	5,290 (720)	4,723 (656)	4,905 (592)	4,950 (532)	4,859 (344)	4,928 (398)
2nd 6 months (post-incentive period)						
Any PCP visit	0.48 (0.02)	0.51 (0.02)	0.50 (0.02)	0.52 (0.02)	0.51 (0.01)	0.51 (0.02)
PCP visits	1.21 (0.09)	1.27 (0.09)	1.65 (0.15)$^{+++**}$	1.52 (0.12)$^{++*}$	1.48 (0.07)$^{++}$	1.59 (0.10)$^{++**}$
ED visits	0.55 (0.07)	0.45 (0.06)	0.55 (0.07)	0.46 (0.05)	0.49 (0.03)	0.50 (0.04)
Non-emergent ED visits	0.05 (0.01)	0.04 (0.01)	0.04 (0.02)	0.02 (0.01)$^{+}$	0.04 (0.01)	0.03 (0.01)
Inpatient admissions	0.10 (0.02)	0.11 (0.02)	0.10 (0.02)	0.07 (0.02)	0.09 (0.01)	0.08 (0.01)
Outpatient & specialty visits	2.04 (0.19)	2.05 (0.18)	2.68 (0.22)$^{++**}$	2.60 (0.23)$^{+*}$	2.44 (0.12)$^{+}$	2.64 (0.16)$^{++**}$
Median total costs ($)	316	390	529	486	453	506
Mean total costs ($)	3,298 (479)	3,559 (560)	3,984 (615)	3,671 (479)	3,737 (320)	3,827 (390)

Notes: PCP=Primary Care Provider; ED=Emergency Department; standard errors of the mean are reported in parentheses. Tests of statistical significance relative to the untreated controls and relative to the $0 incentive group are reported using the two-sample *t*-test for continuous variables. For tests relative to the untreated controls, statistical significance is reported as: $^{+}p<0.10$, $^{++}p<0.05$, $^{+++}p<0.01$. For tests related to the $0 incentive group, statistical significance is reported as: $^{*}p<0.10$, $^{**}p<0.05$.

Table 3. First-stage estimations for any PCP visit, OLS, first six months (incentive period)

	Any PCP visit
Incentive & untreated controls	
N=1,643	
Untreated	Reference
$0	0.043 (0.029)
$25	0.092 (0.031)***
$50	0.124 (0.025)***
Untreated	Reference
$0, $25, & $50	0.086 (0.022)***
Incentive groups	
N=1,228	
$0	Reference
$25	0.041 (0.029)
$50	0.069 (0.027)**
$0	Reference
$25 & $50	0.055 (0.024)**

PCP=Primary care provider. Levels of statistical significance are: **p<0.05, ***p<0.01. Control variables for estimations using untreated controls and incentive groups are: age, gender, race, ethnicity (Hispanic), and marital status. Additional control variables for estimations using only incentive groups include: education, monthly income, employed, health insurance coverage in the 12 months prior to VCC enrollment, smoker, drug/alcohol problems; gets most of care at the emergency department, visits to the emergency department in the 12 months, having two or more chronic conditions, and PROMIS domains and PSQ-12 scores. Physician fixed effects are included.

Table 4. Effects of PCP visits on utilization and costs, 2SLS estimations

	Emergency department visits			Outpatient/specialty visits		Total costs ($)	
	Any visits	2+ visits	Any non-emergent ED visits	Any visits	2+ visits	Ln(costs)	IHS costs
1st 6 months (incentive period)							
Incentive & untreated controls	-0.163	-0.160	-0.078	0.556	0.579	2.028	3.873
	(0.173)	(0.135)	(0.107)$^{+}$	(0.257)$^{**+}$	(0.219)$^{***+}$‡	(0.811)**‡	(1.098)$^{***+}$‡
Sample means	33.72%	13.21%	4.08%	62.69%	47.60%	6.68	6.83
Incentive groups	-0.211	-0.055	-0.054	0.443	0.318	2.962	6.078
	(0.281)	(0.176)	(0.179)	(0.434)	(0.354)	(1.475)$^{**+}$	(2.234)$^{***+}$‡
Sample means	33.47%	12.79%	3.91%	64.66%	49.76%	6.74	6.93
2nd 6 months (post-incentive period)							
Incentive & untreated controls	0.123	-0.178	-0.189	0.260	0.217	0.975	1.405
	(0.176)	(0.152)	(0.104)$^{*+}$	(0.226)	(0.204)	(0.975)	(1.527)
Sample means	25.68%	11.44%	3.16%	49.85%	35.91%	5.73	5.33
Incentive groups	0.076	0.191	-0.162	-0.094	0.105	-0.029	-0.626
	(0.328)	(0.288)	(0.170)	(0.399)	(0.391)	(1.995)	(3.155)
Sample means	26.14%	10.50%	2.69%	51.22%	37.05%	5.79	5.42

Notes: IHS=Inverse hyperbolic sine; N=1,643 for incentive and untreated controls combined. N=1,228 for incentive group only. For the utilization analyses, the first-stage F=14.26 for incentive and untreated controls; F=6.32 for incentive groups. For the cost analyses, F=14.10 for incentive and untreated controls; F=5.90 for incentive groups. The F-statistics are slightly different for these analyses because of a handful of observations missing data on costs. Any PCP visit in the 1st 6 months is the endogenous variable in the 2SLS regressions. Assignment to treatment and untreated control groups are used as instrumental variables. (We use as the first-stage regressions the first and third set of estimates from Table 3. Standard errors are reported in parentheses and clustered at the physician level. Levels of statistical significance are: *p<0.10, **p<0.05, ***p<0.01. Otherwise, the notes are the same as in Table 3. In the cost estimations using the log of costs, we replace zero values with the minimum value minus 0.01. $^{+}$One or more robust tests for weak instruments were statistically significant at p<0.05 (out of 5 tests). ‡Actual effect size is greater than the one-sided detectable effect size calculated for 80% power and alpha = 0.05.

Table 5. Effect of incentives on utilization, reduced form estimations, linear probability models

	Emergency department			Outpatient/specialty	
	Any visit	**2+ visits**	**Any non-emergent ED visits**	**Any visit**	**2+ visits**
1st 6 months (incentive period)					
Incentive & untreated controls (N=1,643)					
Untreated	Reference	Reference	Reference	Reference	Reference
$0	-0.012	-0.024	-0.001	0.027	0.048
	(0.038)	(0.023)	(0.012)	(0.023)	(0.023)**
$25	-0.040	-0.034	-0.025	0.066	0.069
	(0.031)	(0.023)	(0.011)**+++	(0.032)**++	(0.029)**++
$50	-0.012	-0.020	-0.001	0.075	0.086
	(0.029)	(0.022)	(0.015)+++	(0.031)**++	(0.028)***++‡
Untreated	Reference	Reference	Reference	Reference	Reference
$0, $25, & $50	-0.021	-0.026	-0.009	0.056	0.067
	(0.029)	(0.019)	(0.011)	(0.022)**‡	(0.023)***‡
Sample means	33.72%	13.21%	4.08%	62.69%	47.60%
Incentive groups (N=1,228)					
$0	Reference	Reference	Reference	Reference	Reference
$25	-0.032	-0.014	-0.022	0.024	0.011
	(0.026)	(0.019)	(0.010)**	(0.033)	(0.022)
$50	-0.012	-0.001	0.0003	0.036	0.028
	(0.024)	(0.016)	(0.015)	(0.032)	(0.030)
$0	Reference	Reference	Reference	Reference	Reference
$25 & $50	-0.022	-0.008	-0.010	0.030	0.019
	(0.020)	(0.013)	(0.012)	(0.028)	(0.024)
Sample means	33.47%	12.79%	3.91%	64.66%	49.76%
2nd 6 months (post-incentive period)					
Incentive & untreated controls (N=1,643)					
Untreated	Reference	Reference	Reference	Reference	Reference
$0	-0.004	-0.049	-0.014	0.024	0.008
	(0.028)	(0.017)***‡	(0.013)	(0.031)	(0.032)
$25	0.019	-0.034	-0.030	0.063	0.038
	(0.028)	(0.019)*	(0.011)***+‡	(0.031)*	(0.027)
$50	0.009	-0.034	-0.023	0.022	0.021
	(0.028)	(0.018)*	(0.012)*++	(0.035)	(0.036)
Untreated	Reference	Reference	Reference	Reference	Reference
$0, $25, & $50	0.007	-0.039	-0.022	0.036	0.022
	(0.023)	(0.013)***‡	(0.009)**‡	(0.026)	(0.024)
Sample means	25.68%	11.44%	3.16%	49.85%	35.91%
Incentive groups (N=1,228)					
$0	Reference	Reference	Reference	Reference	Reference
$25	0.020	0.015	-0.017	0.021	0.016
	(0.026)	(0.020)	(0.012)	(0.035)	(0.035)
$50	0.002	0.014	-0.011	-0.014	0.006
	(0.030)	(0.024)	(0.014)	(0.033)	(0.035)
$0	Reference	Reference	Reference	Reference	Reference
$25 & $50	0.011	0.015	-0.014	0.003	0.011
	(0.023)	(0.020)	(0.013)	(0.030)	(0.029)
Sample means	26.14%	10.50%	2.69%	51.22%	37.05%

Notes: The levels of statistical significance are: *p<0.10, **p<0.05, ***p<0.01. In the OLS analysis performed on the full sample, the statistical significance of the joint test comparing the $25 incentive group and the $50 incentive group to the untreated control

group is indicated as follows: $^{++}p<0.05$, $^{+++}p<0.01$. Otherwise, notes are the same as reported in Table 3. ‡Actual effect size is greater than the one-sided detectable effect size calculated for 80% power and alpha = 0.05.

Table 6. Incentive effects on costs, reduced form estimations

	Ln(costs)	IHS costs
1st 6 months (incentive period)		
Incentive & untreated controls (N=1,638)		
Untreated	Reference	Reference
$0	-0.027	-0.082
	(0.133)	(0.188)
$25	0.143	0.273
	(0.121)	(0.161)*++
$50	0.276	0.515
	(0.127)**	(0.189)***++‡
Untreated	Reference	Reference
$0, $25, & $50	0.129	0.233
	(0.100)	(0.133)*
Sample means	6.68	6.83
Incentive groups (N=1,224)		
$0	Reference	Reference
$25	0.128	0.287
	(0.128)	(0.208)
$50	0.252	0.512
	(0.115)**	(0.183)***‡
$0	Reference	Reference
$25 & $50	0.190	0.399
	(0.111)*	(0.177)**
Sample means	6.74	6.93
2nd 6 months (post-incentive period)		
Incentive & untreated controls (N=1,638)		
Untreated	Reference	Reference
$0	0.057	0.134
	(0.159)	(0.230)
$25	0.219	0.350
	(0.148)	(0.233)
$50	0.080	0.119
	(0.162)	(0.260)
Untreated	Reference	Reference
$0, $25, & $50	0.118	0.199
	(0.117)	(0.180)
Sample means	5.73	5.33
Incentive groups (N=1,224)		
$0	Reference	Reference
$25	0.103	0.136
	(0.172)	(0.260)
$50	-0.030	-0.096
	(0.179)	(0.280)
$0	Reference	Reference
$25 & $50	0.036	0.020
	(0.145)	(0.220)
Sample means	5.79	5.42

Notes: IHS=inverse hyperbolic sine. The sample size is reduced by 5 subjects who had visits but were not charged. Levels of statistical significance: *p<0.10, **p<0.05, ***p<0.01. In the OLS analysis performed on the full sample, the statistical significance of the joint test comparing the $25 incentive group and the $50 incentive group to the untreated control group is indicated as follows: ++p<0.05. Otherwise, notes are the same as

reported in Table 3. In the log of costs estimations, we replace zero values with the minimum value minus 0.01.
‡Actual effect size is greater than the one-sided detectable effect size calculated for 80% power and alpha = 0.05.

Table 7. Effects of incentives on PCP visits in first six months (incentive period), comparisons of healthy samples to remaining sample

	No drug/alcohol problems, hypertension, diabetes, lung disease or cancer		No drug/alcohol problems, diabetes, lung disease or cancer		Excellent or very good health	
	Healthy N=127	Unhealthy N=1,101	Healthy N=229	Unhealthy N=999	Healthy N=293	Unhealthy N=935
$0	Reference	Reference	Reference	Reference	Reference	Reference
$25	-0.082	0.050	0.041	0.045	-0.073	0.087
	(0.104)	(0.031)	(0.067)	(0.036)	(0.069)	(0.0323)**‡
$50	0.109	0.064	0.104	0.061	0.041	0.078
	(0.175)	(0.028)**	(0.091)	(0.030)*	(0.063)	(0.035)**
$0	Reference	Reference	Reference	Reference	Reference	Reference
$25 & $50	-0.001	0.057	0.065	0.053	-0.023	0.082
	(0.101)	(0.026)**	(0.063)	(0.030)*	(0.056)	(0.029)***‡
Sample means	53.54%	74.66%	63.76%	74.47%	66.21%	74.44%

Notes: Levels of statistical significance: *p<0.10, **p<0.05, ***p<0.01. Otherwise, notes are the same as reported in Table 3. ‡Actual effect size is greater than the one-sided detectable effect size calculated for 80% power and alpha=0.05.

Appendix Table 1. Effects of PCP visits on number of PCP, emergency department, and outpatient visits, 2SLS estimations

	Utilization
2nd 6 months (post-incentive period)	**PCP Visits**
Incentive & untreated controls	2.196 (0.672)$^{***+}$
Incentive groups	2.420 (1.902)
1st 6 months (incentive period)	**Emergency department**
Incentive & untreated controls	-0.771 (0.545)
Incentive groups	-0.506 (0.805)
***2nd 6 months* (post-incentive period)**	
Incentive & untreated controls	-0.522 (0.482)
Incentive groups	-0.240 (1.059)
***1st 6 months* (incentive period)**	**Outpatient**
Incentive & untreated controls	2.636 (1.792)
Incentive groups	1.771 (3.187)
***2nd 6 months* (post-incentive period)**	
Incentive & untreated controls	3.589 (1.977)*
Incentive groups	6.378 (3.589)*

Notes: N=1,643 for incentive and untreated controls combined. N=1,228 for incentive group only. For the utilization analyses, F=14.26 for incentive and untreated controls; F=6.32 for incentive groups. The levels of statistical significance are noted as follows: $^{*}p<0.10$, $^{***}p<0.01$. Notes are the same as reported in Table 3. $^{+}$One or more robust tests for weak instruments were statistically significant at $p<0.05$.

Appendix Table 2a. P-value comparison when controlling for the false discovery rate (Simes method), 2SLS estimates

	Incentive & untreated controls			Incentive groups		
	Estimated coefficient	Unadjusted p-value	Adjusted p-value	Estimated coefficient	Unadjusted p-value	Adjusted p-value
1st 6 months (incentive period)	*6 estimates*			*6 estimates*		
Any ED visits	-0.163	0.346	0.415	-0.211	0.452	0.678
2+ ED visits	-0.160	0.236	0.354	-0.055	0.752	0.761
Any non-emergent ED visits	-0.078	0.466	0.466	-0.054	0.761	0.761
Any outpatient visits	0.556*	0.031	0.062	0.443	0.306	0.678
2+ outpatient visits	0.579**	0.008	0.036	0.318	0.368	0.678
Ln(costs)	2.028**	0.012	0.036	2.962	0.045	0.270
2nd 6 months (post incentive period)	*6 estimates*			*6 estimates*		
Any ED visits	0.123	0.484	0.484	0.076	0.816	0.979
2+ ED visits	-0.178	0.242	0.380	0.191	0.506	0.979
Any non-emergent ED visits	-0.189	0.071	0.380	-0.162	0.342	0.979
Any outpatient visits	0.260	0.250	0.380	-0.094	0.813	0.979
2+ outpatient visits	0.217	0.288	0.380	0.105	0.787	0.979
Ln(costs)	0.975	0.317	0.380	-0.029	0.988	0.988

Notes: PCP=Primary Care Provider; ED=Emergency Department. Original coefficients are reported in the main text (Table 4). The levels of statistical significance for the estimates are: *p<0.10, **p<0.05.

Appendix Table 2b. P-value comparison when controlling for the false discovery rate (Simes method), reduced form estimations including untreated controls

		Estimated coefficient	Unadjusted p-value	Adjusted p-value		Estimated coefficient	Unadjusted p-value	Adjusted p-value
1st 6 months (incentive period)		*18 estimates*				*6 estimates*		
Any ED visits	$0	-0.012	0.743	0.891	$0, $25, $50	-0.021	0.469	0.469
	$25	-0.040	0.207	0.405				
	$50	-0.012	0.671	0.862				
2+ ED visits	$0	-0.024	0.303	0.454	$0, $25, $50	-0.026	0.181	0.301
	$25	-0.034	0.143	0.321				
	$50	-0.020	0.369	0.510				
Any non-emergent ED visits	$0	-0.001	0.875	0.925	$0, $25, $50	-0.009	0.400	0.469
	$25	-0.025	0.038	0.126				
	$50	-0.001	0.925	0.925				
Any outpatient visits	$0	0.027	0.248	0.405	$0, $25, $50	0.056*	0.018	0.054
	$25	0.066	0.047	0.126				
	$50	0.075	0.019	0.126				
2+ outpatient visits	$0	0.048	0.049	0.126	$0, $25, $50	0.067**	0.006	0.036
	$25	0.069	0.025	0.126				
	$50	0.086*	0.004	0.072				
Ln(costs)	$0	-0.027	0.839	0.925	$0, $25, $50	0.129	0.201	0.301
	$25	0.143	0.243	0.405				
	$50	0.276	0.035	0.126				
2nd 6 months (post incentive period)		*18 estimates*				*6 estimates*		
Any ED visits	$0	-0.004	0.869	0.869	$0, $25, $50	0.007	0.737	0.737
	$25	0.019	0.491	0.760				
	$50	0.009	0.738	0.830				
2+ ED visits	$0	-0.049*	0.007	0.081	$0, $25, $50	-0.039**	0.006	0.036
	$25	-0.034	0.079	0.237				
	$50	-0.034	0.078	0.237				
Any non-emergent ED visits	$0	-0.014	0.278	0.556	$0, $25, $50	-0.022*	0.022	0.066
	$25	-0.030*	0.009	0.081				
	$50	-0.023	0.054	0.237				
Any outpatient visits	$0	0.024	0.454	0.760	$0, $25, $50	0.036	0.173	0.346
	$25	0.063	0.053	0.237				
	$50	0.022	0.520	0.760				
2+ outpatient visits	$0	0.008	0.803	0.850	$0, $25, $50	0.022	0.361	0.433
	$25	0.038	0.162	0.364				
	$50	0.021	0.549	0.760				
Ln(costs)	$0	0.057	0.720	0.830	$0, $25, $50	0.118	0.320	0.433
	$25	0.219	0.145	0.364				
	$50	0.080	0.621	0.798				

Notes: PCP=Primary Care Provider; ED=Emergency Department. Original coefficients are reported in main text (Tables 5 and 6). The levels of statistical significance for the estimates are: *p<0.10, **p<0.05.

Appendix Table 2c. P-value comparison when controlling for the false discovery rate (Simes method), reduced form estimations, using only incentive groups

		Estimated coefficient	Unadjusted p-value	Adjusted p-value		Estimated coefficient	Unadjusted p-value	Adjusted p-value
1st 6 months (incentive period)		*12 estimates*				*6 estimates*		
Any ED visits	$25	-0.032	0.235	0.685	$25, $50	-0.022	0.293	0.495
	$50	-0.012	0.611	0.733				
2+ ED visits	$25	-0.014	0.448	0.685	$25, $50	-0.008	0.552	0.552
	$50	-0.001	0.921	0.981				
Any non-emergent ED visits	$25	-0.022	0.033	0.204	$25, $50	-0.010	0.384	0.495
	$50	0.000	0.981	0.981				
Any outpatient visits	$25	0.024	0.457	0.685	$25, $50	0.030	0.289	0.495
	$50	0.036	0.263	0.685				
2+ outpatient visits	$25	0.011	0.608	0.733	$25, $50	0.019	0.413	0.495
	$50	0.028	0.370	0.685				
Ln(costs)	$25	0.128	0.322	0.685	$25, $50	0.190	0.095	0.495
	$50	0.252	0.034	0.204				
2nd 6 months (post incentive period)		*12 estimates*				*6 estimates*		
Any ED visits	$25	0.020	0.431	0.881	$25, $50	0.011	0.629	0.906
	$50	0.002	0.946	0.946				
2+ ED visits	$25	0.015	0.441	0.881	$25, $50	0.015	0.458	0.906
	$50	0.014	0.555	0.881				
Any non-emergent ED visits	$25	-0.017	0.175	0.881	$25, $50	-0.014	0.272	0.906
	$50	-0.011	0.451	0.881				
Any outpatient visits	$25	0.021	0.538	0.881	$25, $50	0.003	0.906	0.906
	$50	-0.014	0.661	0.881				
2+ outpatient visits	$25	0.016	0.637	0.881	$25, $50	0.011	0.705	0.906
	$50	0.006	0.865	0.945				
Ln(costs)	$25	0.103	0.553	0.881	$25, $50	0.036	0.801	0.906
	$50	-0.030	0.867	0.945				

Notes: PCP=Primary Care Provider; ED=Emergency Department. Original coefficients are reported in main text (Tables 5 and 6).