# The Effect of Insurance Coverage on Preventive Care *

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**Abstract**

Despite the large growth in health insurance products that preferentially cover certain types of care, little is known about how utilization responds to changes in the complexity of coverage. Using administrative data from a large company, this paper examines the implementation of an insurance benefit design which differentially increased the price of non-preventive care while decreasing the price of prevention. Leveraging a difference-in-differences research strategy, we find that preventive care utilization did not increase and even declined due to the differential price change. This evidence indicates a meaningful negative cross-price effect, suggesting that non-preventive care and preventive care are complements.

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One of the major policy challenges in the United States is reducing the rising costs of medical care. Some believe that cost-cutting objectives can be met through demand-side utilization control measures such as increasing patient marginal prices for medical services. Increasingly, health insurance plans require high patient marginal contributions for most health care procedures but require no patient contributions for certain preventive services. The basic idea behind such an insurance design is that high marginal prices for most services will discourage the use of non-essential services while free preventive care will promote health maintenance activities.

Although historically health insurance plans covered different types of care on equal terms, plans that have different cost-sharing rules for preventive care and curative care (non-preventive care) have grown tremendously over the last decade. According to the Kaiser Family Foundation, the fraction of people with employer-sponsored insurance enrolled in plans with an annual deductible exceeding $1,000 for single coverage has increased from 6% in 2006 to 45% in 2016 (KFF (2016)). At the same time, there has been a trend towards exempting preventive care from any patient out-of-pocket cost; according the Kaiser Family Foundation, just under half of individuals enrolled in employer-sponsored plans had a preventive care cost-sharing exemption in 2006 (KFF (2006)) while all individuals enrolled in such plans today have access to preventive care at no out-of-pocket cost. While this trend toward differential cost-sharing pre-dated the Affordable Care Act of 2010 (ACA), the ACA further propelled this trend forward by mandating that health insurance plans provide preventive care services at no cost to patients.[1] Thus, health insurance plans now provide mandated full coverage for preventive services, while the out-of-pocket cost of all other care has increased steadily through the growing prevalence of higher deductible health plans.

Several prior papers investigate the impact of the generosity of cost-sharing on the utilization of health care services including the RAND Health Insurance Experiment (e.g., Newhouse and Group (1993), Keeler and Rolph (1988)) and more recent quasi-experimental and experimental studies (e.g., Kowalski (2016), Finkelstein et al. (2012) Cabral and Mahoney (2014)). Broadly speaking, these papers find that health care utilization is price-sensitive, in that when the out-of-pocket price for all care increases, the amount of care consumed declines. Despite the growing importance of health insurance plans that preferentially cover some types of care relative to others, there is very little research exploring the effect of differential cost-sharing. While prior empirical estimates are sparse, policymakers and academics have pointed to the potential promise of differential cost-sharing as a tool to encourage certain types of high-value, under-utilized medical care (e.g., Baicker, Mullainathan and Schwartzstein (2015), Baicker and Goldman (2011)).[2] At the same time, the growing complexity of health insurance cost-sharing arrangements can raise concerns about potential unintended consequences of added cost-sharing complexity arising from complementarities across different types of care (e.g. Chandra, Gruber and McKnight (2010), Goldman and Philipson (2007)) or from consumer mis-perception of the complexities of health insurance contracts (e.g. Handel and Kolstad (2015)).

In this paper, we use proprietary medical claims data from a large manufacturing company, Alcoa Inc., to explore how preventive care behavior responded to an insurance coverage change which increased patient prices for curative care while simultaneously decreasing patient prices for prevention. The firm's ben-

[1] For a description of the preventive procedures covered under the ACA mandate, see: `http://kff.org/health-reform/fact-sheet/preventive-services-covered-by-private-health-plans/`.

[2] Some have also put forward a related concept of value-based insurance design, which involves more sophisticated tailoring of cost-sharing to individual-specific risk factors (Pauly and Blavin (2008), Chernew et al. (2010), Chernew, Rosen and Fendrick (2006)).

efit change provides variation to analyze the effect of moving from uniform pricing of curative and preventive care to differential pricing. The increasing prevalence of insurance options that differentially price curative and preventive care poses the obvious question to researchers: using a traditional non-differentially priced insurance plan as a starting point, can we increase marginal patient contributions for curative care while encouraging preventive care usage? From the perspective of policymakers, this is a particularly important question as the answer could guide them in their goal of reducing medical costs while promoting prevention. Our results can be interpreted as the effect of such an insurance coverage change on preventive care usage.

While the own-price reduction for preventive care associated with the firm's benefit change might encourage the use of preventive services, the increase in the price all other care could either depress or encourage preventive care utilization depending on whether preventive care and curative care are substitutes or complements. While there has been substantial theoretical interest in the link between curative and preventive health care (e.g., Ehrlich and Becker (1972), Zweifel and Manning (2000), Ellis and Manning (2007)), there is little empirical evidence on the effect of curative care prices on preventive care behavior. The few empirical studies that analyze cross-price effects within health care tend to focus on the setting of prescription drug utilization and spillovers on medical utilization (e.g., Chandra, Gruber and McKnight (2010), Gaynor, Li and Vogt (2007), Goldman, Joyce and Zheng (2007)).[3] In the present paper, we contribute to this literature by investigating the potential for important cross-price effects between curative care and preventive care. Interestingly, our results reveal that increasing patient prices for curative care depresses not only curative care spending but can also discourage the use of free preventive care services, suggesting that curative care and preventive care are complements.

During our period of analysis, Alcoa Inc. employed roughly 48,000 individuals across the United States. The company introduced new health insurance plans to its employees beginning in 2004, and the roll out of these new plans was staggered across employee groups due to variation in union contract expiration dates. The plans available to company employees before this change had out-of-pocket prices for preventive care and curative care that were uniform and low. The new insurance plans required greater patient contributions for curative care while exempting preventive care from any patient charges. The net effect of the benefit change was that the price of a preventive care procedure was reduced from $10-15 to zero while the price of curative care rose substantially through increases in coinsurance payments and the introduction of deductibles up to $1,500. We leverage the staggered introduction of the new benefit design across employee groups in a difference-in-differences framework to estimate the effect of moving from the old plans with uniform pricing of curative and preventive care to the new plans with differential pricing.

We estimate the effect of the insurance benefit change on the utilization of five specific preventive care services: cervical cancer screenings, breast cancer screenings, colorectal cancer screenings, cholesterol screening, and child immunizations. Within the overall population, we find that preventive care utilization does not increase, and we see a 4 percentage point reduction in annual colorectal cancer screenings. In addition, we find evidence that the reduction in preventive care utilization was more pronounced among rural enrollees. Among rural enrollees, there was a 4.4 percentage point reduction in annual cervical cancer screenings, 8.5 percentage point decline in annual colorectal cancer screening, 3.6 percentage point reduction in annual cholesterol screening, and a 6.3 percentage point reduction in annual early child immunizations. In contrast, nonrural enrollees show no such reduction in preventive care utilization for these

[3] One exception is a study by Phelps and Mooney (1993) which investigates correlations between common expensive hospital procedures and later long-run use of other services which may substitute for these procedures.

procedures.

Overall, the difference-in-differences analysis reveals that enrollees did not increase preventive care utilization in response to the benefit change and some subgroups, namely rural enrollees, meaningfully decreased preventive care utilization despite the fact that the benefit change decreased the price of prevention. These results reveal that there is a meaningful cross-price effect in that the increase in the price of curative care depressed preventive care usage, indicating that preventive care and curative care are complements. We investigate the relationship between preventive care and curative care in more depth through a few ancillary empirical tests. For instance, we show that enrollees who cut back most on preventive care utilization also demonstrated greater reductions in the utilization of all other care in response to the benefit change. In addition, we find evidence indicating that an urgent curative care visit increases the probability of a subsequent preventive care claim soon after. Overall, the results of these ancillary tests indicate that doctor advice during curative care visits may play a meaningful role in reminding and informing patients of recommended preventive care procedures. This channel may explain why policies aimed at discouraging more discretionary curative care visits, like the benefit change studied presently or the broader national trend of increasing health insurance deductibles, may have the unintended consequence of discouraging subsidized prevention.

The remainder of the paper proceeds as follows. Section 1 describes the data and the environment. In Section 2, we describe the expected effect of a differential price change on preventive care usage. Section 3 presents the analysis of the effect of the firm's differential price change on the use of preventive care services. Section 4 further explores the relationship between curative and preventive care services. Lastly, we conclude in Section 5.

# 1 Background and Data

The data come from Alcoa, Inc., a large, multinational manufacturing firm that annually employed roughly 48,000 employees within the United States residing within 24 different states during our sample period. For each worker, the data include information on wages, company tenure, type of job (hourly or salary), age, sex, location, chosen health insurance plan, and medical claims data. The employee population is divided into "benefit groups" that reflect specifics of the company's business model. Based on benefit group divisions, the company assigned each worker a menu from which to choose a health insurance plan. Each worker chooses from this menu of health insurance contracts, and employee decisions and option menus are reflected in the data. In addition, each employee could select to insure his/her spouse and children through the various family option pricing offered by the company. When an employee chooses to enroll family members, the data include the age, sex, and medical claims information for these family members.

The medical claims data offer a detailed look at the health care behavior of the individuals. For each claim, the data reflect the date of the service, the billed total cost of the service, the out-of-pocket cost of the service, the type of service, the type of facility in which the service was performed, and the specialty of the medical professional that delivered the service. Descriptions of the services vary in the level of specificity. In this paper, we examine the preventive care services that we can unambiguously identify.

## 1.1 Description of Differential Price Change

Prior to 2004, a subset of company employees were offered a standardized menu of options for health insurance which we will refer to as the "old menu". The company began to replace the old menu of health insurance plans with a "new menu" of plans starting in 2004.[4] The benefit design change was rolled out to enrollees over a number of years due to staggered expiration dates of union contracts. Both before and after the employees were "treated" with the insurance benefit change, employees selected health insurance plans from a menu of health insurance policies. It is important to note that we use the switch from the old menu to the new menu and the plausibly exogenous staggered timing of this switch to identify the effect of the policy change on prevention; we avoid using the employees endogenous plan selections within the old and new insurance menus.[5]

Table 1 describes the old and new menus of plans, in addition to the the share of the sample enrolled in each plan. The plans on the old menu required $10-15 co-pays for both curative and preventive doctor visits. Ninety-nine percent of people on the old menu plans faced no deductible. The new plans differed in their level of cost-sharing for both preventive and curative care. Curative care on the new plans was subject to a deductible and coinsurance. The patients were responsible for 10 percent of charges beyond the deductible, while individual deductibles ranged from $0 to $1,500. Preventive care was free of charge on the new plans. The health insurance employee brochure highlighted the specific preventive services that were free of cost on the new menu plans. An additional important detail to note is that the new and old menu plans differed only in their cost-sharing terms and pricing; plans on both menus used the same provider network.

The net effect of the change on the patient cost of prevention was a decline of $10-15.[6] Generally, the cost of curative care increased for people as a result of benefit change. For most services, the 10 percent coinsurance payment alone was larger than the $10-15 co-pays that people paid on the old plans. Sizable deductibles many faced on the new menu plans increased curative prices further. Overall, the benefit change lead to a 35 percentage point increase in the fraction of individuals facing an annual deductible applicable to curative care.[7] The empirical results capture the effect of moving from uniform pricing of preventive and curative care at quite low levels to differentially increasing the price of curative care while exempting prevention from any financial cost.

## 1.2 Description of Preventive Procedures

Preventive care describes services ranging from nutrition counseling to mammograms to diabetes monitoring. This empirical study focuses on a few specific procedures: cervical cancer screening (the papanicolaou

[4] Some of the company's business divisions had different menus of health insurance contracts and were not eligible for the switch to the new menu contracts. The differences in benefits packages reflect aspects of the subsidiary business model of the company and appear to be uncorrelated with health care utilization (conditional on observed information). We focus on the subset of employees that were undergoing the benefit change from the old menu plans to the new menu plans.

[5] Busch et al. (2006) use a subset of the data employed in this paper to investigate the immediate effect of the benefit change on the use of some preventive care services. The authors present a comparison of means using the two years of data available at the time of their study, and they find no evidence that the policy change affected patient preventive care usage. The study summarized in the present paper extends this analysis in a number of ways. First, the analysis in this paper fully utilizes the staggered natural experiment in the data by using data over five years. Because the company's insurance benefit change was rolled out over a number of years to different company employees, the longer data set allows us to take advantage of this variation. Second, we are able to apply methods that utilize within enrollee information over time using the longer data. This is especially important because many of the preventive procedures studied are not typically done annually making it critical to have more than two years of data to estimate changes in behavior. Third, we are able to investigate some of the channels through which the benefit change influenced prevention through examining the timing of care. With this richer data, we find significant and economically meaningful heterogeneity of reactions to the benefit change between rural and nonrural enrollees. In addition, we reveal evidence consistent with a meaningful cross-price effect, a channel through which decreasing the generosity of curative care coverage may depress prevention.

[6] There is one exception to this treatment: under the old menu cost-sharing for mammograms differed depending on the facility which preformed the screening; at some facilities mammograms had no patient co-pay requirement, while at others there was a $10-15 co-pay. The new menu exempts mammograms, as with other preventive services, from any patient charges. In this sense, the price drop for mammograms was smaller than the price drop for the other preventive care procedures.

[7] See Appendix Table A2 for the results of a difference-in-differences regression illustrating how the benefit change influenced the plan composition among individuals in the samples analyzed.

test, more commonly the pap smear test), breast cancer screening (mammography), colorectal cancer screening, and cholesterol screening. Additionally, we examine annual child immunization rates.[8] According to the company's health insurance documentation, all of these services were exempt from payment under the new plans. We focus our empirical investigation on individuals who are eligible for the procedures above. For cervical cancer screening, we define the eligible group as women older than age 21 but younger than age 65. Women over the age of 40 are defined as eligible for breast cancer screenings. Adults over age 50 are defined as eligible for colorectal cancer screening, and adults 18 years or over are defined as eligible for cholesterol screenings. Our examination of child immunizations is limited to children age 4 and younger. Details on the eligibility definition choices for these procedures are contained in Appendix A.

### 1.3 Description of Samples for Estimation

We restrict attention to the company enrollees subject to the benefit change described above.[9] The sample used in the estimation is compared to the total company population in Table 2. The analysis period covers 2003-2007. Moving from left to right in Table 2, one can see how the sample size decreases with further restrictions. Approximately 57% employee-year observations had the relevant benefit designs. When the company adopted the new menu of plans, it would have liked to switch all employees on the old menu to the new menu in 2004. However, because of staggered expiration dates of union labor contracts, the company was only able to switch a subset of these employees (including all non-unionized salary workers) in the first year of implementation. As a result, all salary workers were treated in 2004, while the treatment of hourly workers was staggered from 2004 onward. To keep treatment and control groups as homogeneous as possible, we make the further sample restriction of looking only at hourly employees and their relations. This group of hourly employees is described in column 3 of Table 2.

This unbalanced panel contains varying numbers of people across years due to new hires, retirees, and job leavers. We do not know the prior (future) insurance coverage status for those who began at (or parted with) the company during the sample period. The company is quite generous with medical care coverage, both before and after the benefit change, and estimates using the unbalanced panel may be biased. This is of particular concern in this setting since the studied preventive care services are highly optional, meaning some of these services may easily be re-timed. Thus, we further restrict the sample to the balanced panel described in column 4. This sample contains only people that were enrolled in either the old or new plans during the whole period from 2003 to 2007. The balanced panel described in column 4 is used to investigate the effect of the benefit change on adult screenings. However, to study early child immunizations, we employ an unbalanced panel of children 4 years of age and younger.[10]

Table 3 summarizes some characteristics of the hourly balanced sample. Panel A describes the balanced sample by the year of introduction of the new plans with the last column representing the untreated group which consists of people for which the new plans had not been introduced as of 2007. Because of the stag-

[8] Our measure of child immunizations is based on the immunizations that are easily identified in the claims data. We have reason to believe this underestimates actual immunizations children receive because children may receive immunizations outside the normal medical system. However, we have no reason to believe that this underestimation varied systematically with the staggered benefit change so our estimation method is unaffected. This is discussed at length in Appendix A.

[9] For some company locations, employees were offered a HMO option in addition to the menus discussed above. The HMO plan was a fundamentally different sort of plan than the PPO-type plans offered on the old and new menus. Very little switching occurs in the data between the HMO option and the new or old menu plans, and there are no claims data available for those employees that select the HMO option. People who opted for the HMO plan during the time period studied are dropped from the data used for estimation.

[10] The unbalanced panel is necessary because we only look at children under 4 years old and the time period we consider is 5 years long. The unbalanced panel of small children is less problematic than an unbalanced panel of adults for the screening analysis because the cause for the unbalance in this sample is primarily age restrictions rather than parental job changes.

gered introduction dates of the new plans, groups that were treated in later years will serve as controls in earlier years in the empirical estimation. For this reason, we would like the treatment groups to be as homogeneous as possible. There are a total of 14,225 people in the hourly sample including employees, spouses, and dependents. Among those in the balanced sample (described in Table 3 Panel B column 1), 53% live in rural locations, 53% are male, the mean age is 33 years and the mean annual medical expenditures is $2,072. Inspecting Table 3 Panel A, the treatment groups look relatively similar on many observable characteristics, such as age, sex, and mean medical spending. One exception is that those treated in 2006 less often live in rural areas than those in the other treatment groups. While rural and nonrural enrollees look quite similar on many observable characteristics (comparing columns 2 and 3 of Table 3 Panel B), prior research documents differences in rural and nonrural health care delivery systems and access to care. To address this potentially important heterogeneity, we control for the observable characteristics in the empirical analysis including rural status, and we also separately analyze the effect of the benefit change by rural and nonrural locations. In addition, we also show the empirical analysis is robust to excluding those transitioning to the new menu in 2006 (the treatment group that looks the most dissimilar from the remaining groups). Appendix Table A1 describes the eligible samples used in the estimation by preventive procedure type.

# 2 Expected Effect of Differential Price Change

To understand the expected effect of the company's differential price change, it is important to explore the relationship between preventive and curative care. If one views preventive and curative care as unrelated, then a change in the price of curative care would not affect preventive care usage (ignoring income effects). Under this assumption, the company's differential price change would unambiguously encourage the use of preventive services.[11] This view is perhaps unrealistic because there are a number of reasons why preventive and curative care are related. However, despite substantial theoretical interest in the link between curative and preventive care (e.g., Ehrlich and Becker (1972), Zweifel and Manning (2000), Ellis and Manning (2007), etc), there is little empirical evidence on the effect of curative care prices on preventive care behavior.

[11]The own-price decline would suggest that preventive care would weakly increase if that was the only change in price. The expected magnitude of the own-price response of preventive care is not completely clear from the prior literature. The gold standard in this literature are those papers based on the RAND Health Insurance Experiment, a large-scale experiment in the 1970s in which families were randomly assigned to health insurance coverage of varying generosity. The experimental design allows researchers to test how preventive care usage varied with insurance coverage through random assignment of insurance plans. While some families were assigned to plans with patient coinsurance requirements of 25 percent or more, other families were assigned to the free care plans that required no patient contribution. According to Newhouse and Group (1993), usage of preventive health services was 7 percent lower for women and 4 percent lower for men in the co-insurance plans as opposed to the free care plan. The difference was larger, approximately 12 percent for women and 10 percent for men, between preventive care usage of those assigned to the free care plan and those assigned to plans with deductibles. However, there are several reasons why estimates from the RAND experiment should be interpreted with caution in the context of modern health insurance settings. Cancer screenings are an important form of preventive care, and new innovations in cancer treatments since the RAND experiment have probably affected attitudes toward cancer screenings. Additionally, the RAND plans were likely more salient to experimental subjects than plan details typically are in the context of employer provided health insurance. Unlike the situation studied in this paper, the RAND insurance plans all priced preventive care and curative care the same in terms of insurance contributions. The RAND experiment provides the opportunity to investigate a simultaneous price change of preventive and curative care in the same direction, while this paper examines a price change in opposite directions. The uniform pricing of preventive and curative care in the RAND plans means there was little ambiguity in the cost faced by the experimental subjects for a doctor visit. Additionally, the uniform pricing RAND plans might have encouraged more use of services among those on the free plan thus facilitating more interaction with doctors among these subjects. If we think doctors play a large role in supplying information to patients and reminding patients to do preventive care, the differences in curative care coverage among the RAND plans could have been driving the preventive care results of the RAND experiment. For these reasons, in the RAND experiment the own- and cross-price effects most likely operate in the same direction to encourage more preventive care usage in the free care plans. In the natural experiment studied in this paper, on the other hand, the own- and cross-price effects most likely operate in opposing directions. Thus, the RAND estimates can be viewed as an upper bound on the expected effect on preventive care usage from an insurance policy change that differentially affects curative and preventive care patient prices.

Before continuing, we will define a useful distinction commonly made between two types of prevention: *primary prevention* aims to reduce disease incidence (for example, flu shots) while *secondary prevention* aims to mitigate consequences given a disease will occur (for example, cancer screenings).[12] Both types of prevention are fundamentally related to curative care. Primary prevention is clearly related to curative care as it is done to prevent future curative care usage. On the other hand, secondary prevention is mechanically related to curative care because curative follow-up procedures are often ordered when secondary preventive screenings yield positive test results. Although it may be clear that preventive care and curative care are related, how exactly would the curative care price change affect preventive care usage? Below we outline some channels through which the change in the price of curative care could affect preventive care usage.

First, more generous coverage of curative care may deter investment in prevention (Ehrlich and Becker (1972), Ellis and Manning (2007)), a concept termed *ex ante moral hazard* in the literature. When considering a short term change in curative care coverage, ex ante moral hazard most cleanly applies to primary prevention with short run health consequences (for example, flu shots). It is unclear how secondary prevention such as cancer screenings should respond to a short term change in curative coverage because screenings can lead to increased curative care expenditures in the short term in order to avoid more serious (and potentially more costly) curative care sometime in the future. Even though ex ante moral hazard has raised a reasonable amount of theoretical interest, there is little empirical evidence that supports the concept (for a review, see Zweifel and Manning (2000)). Since the concept most directly applies to primary prevention, ex ante moral hazard is less applicable to the adult screenings we examine in this paper though it is potentially important for child immunizations, the only primary preventive procedure studied in this paper.

Second, there may be an indirect effect if doctor advocacy is an important influence on preventive care usage. Prior research from the RAND Health Insurance Experiment and other more recent studies on the impact of cost-sharing reveal that an increase in the patient cost for care generally discourages doctor visits.[13] Patients that react to an increase in the price of curative care by rationally reducing their use of curative care may interact less frequently with their doctors. If doctor advice and reminders play a central role in preventive care decisions, we might see preventive care usage decline in response to the benefit change studied in this paper.[14] Throughout this paper this effect will be referred to as the *doctor interaction effect*. Some empirical tests reported in Section 4 reveal suggestive evidence on the presence of the doctor interaction effect.

Third, imperfect salience of a preventive care cost-sharing exemption may lead people to cut back on prevention when the cost of curative care increases. The preventive care cost-sharing exemption was probably not the most salient feature of the company's benefit change. The majority of care is curative care, and the price of this care increased substantially. If the curative care price increase was the only salient feature of the benefit change, then imperfectly informed enrollees may have believed incorrectly that the price increased for preventive care and rationally reduced preventive care usage as a result.

In summary, the doctor interaction effect and imperfect benefit change salience are justifications for a negative cross-price elasticity of preventive care with respect to the price of curative care. In the context

[12] See Kenkel (2000) for further examples and a more detailed discussion of this classification.

[13] Prior studies on the impact of cost-sharing on health care utilization generally find that health care utilization is price-sensitive: when the out-of-pocket price increases for all types of care people engage in less utilization (e.g., Newhouse and Group (1993), Finkelstein et al. (2012), Kowalski (2016), Cabral and Mahoney (2014)).

[14] Some would call this sort of effect doctor-induced demand while others may interpret this as doctors following best practices. We don't take a stand in this paper as to whether this effect is desirable or not; we just note that this effect may be important when evaluating a differential price change.

of the benefit change and its effect on child immunizations, ex ante moral hazard can be interpreted as an argument for a positive (or less negative) cross-price elasticity of child immunizations with respect to curative care prices. Because the company changed the prices of preventive care and curative care in opposite directions, the effect of the policy change on preventive care usage can be thought of as a combination of two opposing effects: the own-price effect (positive) and the cross-price effect (likely negative). Thus, the net expected effect of the policy change on preventive care usage is ex ante ambiguous.

# 3 Analysis of Differential Price Change

As discussed earlier, the company introduced new plans starting in 2004 which differentially changed the marginal prices for curative and preventive care. Taking advantage of the exogenous variation in the introduction of the new health plans, we use difference-in-differences regression analysis to identify the effect on annual usage rates of the four preventive screenings and child immunizations. In addition to estimating the effect of the benefit change in the overall population, we repeat the analysis separately for rural and nonrural enrollees because of documented differences in health care delivery across these types of locations (e.g., Chan, Hart and Goodman (2006), Casey, Call and Klingner (2001)).

## 3.1 Annual Preventive Procedure Rates

To investigate the impact of the policy change on annual preventive care usage rates, a difference-in-differences regression model is used. The effect of the benefit design change is estimated procedure-by-procedure using the estimating equation below:

$$procedure_{it} = \beta_o + \beta_1 treat_{it} + \sum_T \alpha_T \mathbb{1}(t = T) + \sum_g \delta_g \mathbb{1}(treatmentgroup_i = g) + \gamma X_{it} + \epsilon_{it}. \tag{1}$$

The observations used in estimation are at the individual-year level, where $i$ denotes the individual and $t$ denotes the year. The dependent variable, $procedure_{it}$, is an indicator variable that equals to one when the relevant preventive procedure was done by individual $i$ in year $t$.[15] The variable $treat_{it}$ is a binary variable that takes the value one when the individual was on the new menu plans. The regressions include year and treatment group fixed effects. The regressions also include additional controls, represented above by $X_{it}$, including age fixed effects, an employee indicator, US Census region fixed effects, and an indicator for rural status. Because we would expect there to be within-person correlation in the dependent variable, standard errors are clustered at the person level. The estimation is done on the balanced panel sample for the four adult screenings described in Appendix Table A1 columns 1 through 4. The unbalanced sample of young children described in Appendix Table A1 column 5 is used to estimate the treatment effect on child immunizations.[16]

Table 4 presents the difference-in-differences results. For each analyzed procedure, this table reports the regression results for pooled specifications and separately by rural status. Across all five preventive procedures, in the pooled sample there is no evidence of an increase in utilization associated with the benefit change. In fact, annual colorectal cancer screenings show a statistically meaningful 4 percentage point decline in the pooled sample (p-value is 0.004), or 29% decline relative to the mean annual screening

[15]Probit and logit specifications (results not reported) have qualitatively similar results as the baseline linear specification.

[16]As discussed in Section 1, we employ an unbalanced panel of children 4 years of age and younger to investigate the effect of the benefit change on early child immunizations. The unbalanced panel is necessary because we only look at children under 4 years old and the time period we consider is 5 years long. The unbalanced panel of small children is less problematic than an unbalanced panel of adults used in the screening analysis because the cause for the unbalance in this sample is primarily age restrictions rather than parental job changes.

rate. For the remaining four procedures, the estimates are statistically indistinguishable from zero in the pooled sample. Importantly, however, the pooled estimates mask interesting heterogeneity in the response to the benefit change among rural and nonrural enrollees.

Among rural enrollees, four preventive care procedures show large and statistically meaningful reductions in response to the benefit change: cervical cancer screenings decline by 4.4 percentage points (p-value is 0.05), or 10.7% of the mean rate of annual screening; colorectal cancer screenings decline by 8.5 percentage points (p-value $< 0.001$); cholesterol screenings decline by 3.6 percentage points (p-value is 0.002), or 15.0% of the mean rate of annual screening; and child immunizations decline by 6.3 percentage points (p-value is 0.004) or 14.7% of the mean annual immunization rate. Though the point estimate for breast cancer screenings among the rural population is also negative, the standard error does not allow us to rule out no effect on these screenings.

While rural enrollees display a statistically and economically meaningful reduction in prevention in response to the benefit change, we see no such reduction among nonrural enrollees. Across the procedures examined, the estimates indicate that the nonrural enrollees had no statistically meaningful change in their preventive care utilization in response to the benefit change, with the exception of a 3.8 percentage point increase in breast cancer screenings among nonrural enrollees that is marginally statistically significant (p value is 0.07).

Overall, the results indicate there was no meaningful increase in the utilization of preventive procedures within the population of enrollees overall, and there was a sizable reduction in preventive care utilization among rural enrollees. The fact that preventive care utilization did not increase (and in fact decreased for some) in response to the benefit change suggests that there was a negative cross-price effect from increasing patient cost-sharing for curative care that dominated the own-price decline for prevention. In other words, this evidence suggests that preventive care and curative care are complements. In Section 4, we explore some potential mechanisms behind this relationship between curative and preventive care further.

### 3.2 Robustness Analysis

Next, we investigate the robustness of the main estimates presented in Table 4. Below we describe additional analysis which investigates the robustness of the main analysis to alternative specifications, alternative sample definitions, and alternative methods to evaluate the significance of our estimates.

**Re-Timing of Preventive Care** First, we investigate the robustness of the results with respect to the potential re-timing of preventive care. If individuals endogenously re-timed their preventive care utilization, this would potentially interfere with the identification assumption employed in the estimation. One could imagine that the benefit change could have caused a surge in usage of preventive services right before or right after the new menu introduction depending on the beliefs of the enrollees about the coverage changes. A simple way to assess the potential importance of this issue is to plot the utilization of preventive services around the months surrounding the transition to the new menu to visually inspect the data for this possibility. Appendix Figure A1 plots the relationship between preventive care utilization and the timing of the transition, revealing no meaningful evidence of problematic re-timing of care around this threshold. In addition, we also analyze the potential importance of this issue by re-estimating the difference-in-differences specifications at the monthly level, omitting the month just before and just after the transition to the new menu for each transition group. The results displayed in Appendix Table A3 illustrate that the estimates are robust to repeating the analysis ignoring preventive procedures done in the December proceeding the benefit change or the January following the benefit change for each of the treatment groups.

**Comparability of Treatment Groups** Another potential identification concern relates to the comparability of the treatment groups. As discussed in Section 1, the individuals who transitioned to the new menu of plans in 2006 look somewhat different on observables than the individuals in the remaining treatment groups (see Table 3). One may be worried that the results could be sensitive to the inclusion of this different treatment group, if these differences in observables translate to differences in the expected trend in preventive care utilization. To ensure the results are robust, we repeat the difference-in-differences estimation excluding the 2006 treatment group. The results reported in Appendix Table A4 are qualitatively very similar to the baseline results.

**Permutation Tests** One potential concern about difference-in-differences analysis is that serial correlation can bias standard errors, potentially leading to over-rejection of the null hypothesis of no effect (Bertrand, Duflo and Mullainathan (2002)). Our baseline specification addresses serial correlation within individual by clustering standard errors at the individual-level. As an alternative way to address the broader issue of serial correlation, we also implement a series of permutation tests for the coefficients of interest. We begin by randomly drawing a placebo treatment timing (the transition year to the new menu) for each employee group, where employee groups are defined by job location and actual treatment group affiliation. We then estimate equation 1 as if the placebo treatment timing is the actual treatment timing. We repeat this procedure for 1000 placebo treatments for each specification in Table 4 representing a decline in preventive care utilization. We plot the resulting distributions of placebo treatment estimates along with the actual treatment estimates in Appendix Figure A2. The implied p-values on the main coefficients of interest based on the empirical distributions of placebo treatments indicate that estimated decline in preventive care utilization among rural enrollees because of the benefit change is robust to this potential concern.

# 4 Relationship Between Curative and Preventive Care

As discussed in Section 2, there are a few reasons why we might expect preventive care and curative care would be complements. While we cannot separately identify how much salience or doctor interactions drive the observed preventive care utilization response, the data does allow us to investigate the potential importance of these mechanisms a bit further.

In order for the doctor interaction mechanism to affect preventive screening rates through the benefit change, there must have been a decline in outpatient visits because of the benefit change. We investigate this hypothesis further by utilizing the difference-in-differences variation to estimate the effect of the benefit change on the annual number of outpatient visits. In addition, we separately analyze outpatient visits by whether the visit is associated with a procedure that we can identify as preventive. Table 5 reports the results. The annual number outpatient visits declined by 0.37 visits per-capita (p-value is 0.001) in the pooled sample or 4.4% of the mean number of outpatient visits. In the pooled sample, outpatient visits without a preventive procedure declined by roughly the same amount, 0.34 visits per capita (p-value is 0.002), while there was no statistically meaningful decline in outpatient visits with a preventive procedure in the pooled sample.

As with the analysis of preventive procedures, there is substantial heterogeneity in the reduction in outpatient visits across rural and nonrural enrollees. Among rural enrollees, the annual number of outpatient visits declined by 0.57 visits per-capita (p-value is 0.002) or 7% of the mean value. In addition, rural enrollees cut back on both outpatient visits with and without preventive procedures (columns 5 and 8). In contrast, among nonrural enrollees we see a smaller decline in outpatient visits, approximately 1 percent, and this effect is not statistically significant. As expected based on the analysis of preventive procedures in Table 4, we see no detectable change in the number of outpatient visits with a preventive procedure for the nonrural population. Overall, the estimated coefficients and the heterogeneity is consistent with the explanation that doctor interactions played an important role in the observed heterogeneity in the preventive care usage changes. That is, the decline in curative care visits associated with the benefit change is more pronounced in the rural population for whom we also see larger declines in prevention, providing suggestive evidence on the role of doctor interactions as a mechanism behind the complementarities between curative care and preventive care.

We now turn to more direct evidence of the doctor interaction effect. The doctor interaction effect can be decomposed into short-term and long-term effects. A doctor's visit may serve as a reminder for a patient to return for a preventive screening thereby influencing the short-term behavior of a patient. On the other hand, interacting with a doctor on a regular basis may inform a patient of the merits of prevention, and this information can have a long-term effect on a patient's preventive care behavior. While both short-term and long-term effects may be important, the test we employ to investigate the doctor interaction effect is limited to identifying a short-term doctor interaction effect.

To test for the doctor interaction effect without complications from reverse-causality, the effect of urgent curative care visits on cervical cancer screenings is examined. The reason we restrict attention to urgent curative visits is because more discretionary curative visits could be related to personal attributes that may be correlated with differences in preventive care usage unrelated to the doctor interaction effect. We focus on the sample eligible for cervical cancers screenings because it is easy to identify a common and urgent condition among this sample in the insurance claims data: urinary tract infections. Approximately 25 percent of the sample eligible for cervical cancer screenings has at least one urinary tract infection in the period we examine. Urinary tract infections generally require a visit to the doctor as treatment involves prescription antibiotics. Visits associated with urinary tract infections are a good source of conditional random variation that can be used to measure the effect of an urgent curative care visit on the probability of a subsequent cervical cancer screening. It should be highlighted that we flexibly control for individual average annual outpatient visits in this analysis to separate out the effect of a recent urgent doctor visit for an infection from the person-specific component of preventive habits that may be associated with frequency of doctor visits.

A Single-Spell Cox Proportional Hazard Model is used to test for the impact of urinary tract infection visits on the probability of having a cervical cancer screening soon after. To make the sample as homogeneous as possible, the eligible sample is restricted to those in the first (and largest) treatment group to switch over to the new plans. The spell examined is the time between an individual's latest pap test in 2003 until their next pap test, and individuals without a pap test in 2003 were assigned the start date of January 1, 2003.[17] The results are reported in Table 6. "Infection prior month" is an indicator variable that equals to one if in the last 28 days the individual was seen for a urinary tract infection. In some specifications we flexibly control for an individual's average annual number of outpatient visits. The results in columns 1 and 2 indicate that there is a large and significant doctor reminder effect (a short run doctor interaction effect). In the specification which controls for average annual outpatient visits displayed in column 2, we see that an urgent curative care visit causes a nearly fivefold increase in the hazard rate for having a cervical cancer screening for the following month.[18] Since doctors likely have less time to chat with patients about

[17]We choose this time period to examine because this sample has already made the transition to the new menu plans and so insurance coverage is constant from 2004 to 2007.

[18]As expected, the estimated effect of a urinary tract infection visit is larger when we omit controls for average annual outpatient visits.

preventive care during urgent visits than during non-urgent curative visits, the reported effect of urinary tract infection visits likely underestimates the effect of an average curative care visit on the probability of a subsequent cervical cancer screening. For the same reason, these results even further underestimate the effect of a marginal curative care visit as a marginal visit is presumably less urgent than the average visit. This evidence suggests that patients rely on reminders from their doctors in order to make preventive care decisions.

Beyond controlling for the average annual outpatient visits, one might still be worried that that the correlations in the timing of urgent visits and cancer screening timing is does not represent causation but instead some other attributes of the patient. To address this concern, we estimate specifications that include a placebo variable,"Infection next month," which indicates when an individual is treated for a urinary tract infection in the subsequent 28 days.[19] The results of this additional specification are reported in Table 6 columns 3 and 4. With these alternative specifications, we find that "Infection next month" is not a significant predictor of preventive care utilization and the coefficient on "Infection prior month" is largely unchanged from columns 1 and 2. Thus, these additional specifications provide reassurance that the significant association between "Infection prior month" and preventive care represents a plausibly causal relationship (as opposed to some other unobservables correlated with infections).

# 5 Conclusion

Despite substantial theoretical interest in the link between curative health care (non-preventive health care) and preventive health care (e.g., Ehrlich and Becker (1972), Zweifel and Manning (2000), Ellis and Manning (2007), etc), there has been little prior empirical evidence on the relationship between curative care and preventive care. This paper examines this relationship utilizing administrative data on health insurance enrollees from a large manufacturing company. The company we study altered its employee health insurance benefit design by reducing the price of prevention to zero while increasing the price of curative care substantially. Leveraging variation in the timing of the roll-out of this benefit change, our difference-in-differences analysis reveals that there was no increase in preventive care utilization in response to the benefit change, and preventive care utilization meaningfully declined for some groups of enrollees, notably rural enrollees. Overall, this evidence indicates that the increase in the price for curative care discouraged the use of preventive services, suggesting that preventive care and curative care are complements.

Motivated by the main difference-in-differences results, we further investigate the relationship between preventive care and curative care in more depth through a few ancillary empirical tests. For instance, we show that enrollees who cut back most on preventive care utilization also demonstrated greater reductions in the utilization of all other care in response to the benefit change. In addition, we find evidence indicating that an urgent curative care visit increases the probability of a subsequent preventive care claim soon after. Overall, the results of these ancillary tests indicate that doctor advice during curative care visits may play a meaningful role in reminding and informing patients of recommended preventive care procedures.

Complementarities between curative care and preventive care may explain why policies aimed at discouraging more discretionary curative care visits, like the adoption of higher deductible health plans, may have the unintended consequence of discouraging subsidized prevention. More broadly, our findings sug-

[19] In practice, we define "Infection next month" as a variable that indicates if the infection takes place in the subsequent 28 days omitting the first week (7 days) after the current date. That is, the indicator turns on if the infection takes place between 8 days and 36 days after the current date. We omit procedures/tests billed within the first week after the current date as it is relatively common for procedures/tests associated with the current visit to be billed a few business days after the visit took place if it takes some time to process the ordered test/procedure. To remove any of this mechanical effect from procedures ordered at a visit with preventive care, we thus define "Infection next month" as described above.

gest that the national trend of rising out-of-pocket costs for curative care through the growing prevalence of higher deductible health plans may discourage the use of preventive services, undermining the goal of the ACA mandate that preventive care be provided at no cost to the patient.

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Table 1: Benefit Change: Old and New Menu Description

| | Old Menu | | |
|---|---|---|---|
| | Plan 1 | Plan 2 | Plan 3 |
| Enrollment: Baseline Sample | 36.0% | 63.0% | 1.0% |
| Deductible | None | None | $1,000/person<br>$2,000/family |
| Physician Copayment | $10/visit | $15/visit | $15/visit |
| Out-of-Pocket Maximum | $1,000/person<br>$2,000/family | $2,500/person<br>$5,000/family | $5,000/person<br>$10,000/family |

| | New Menu | | | | |
|---|---|---|---|---|---|
| | Plan 1 | Plan 2 | Plan 3 | Plan 4 | Plan 5 |
| Enrollment: Baseline Sample | 54.6% | 35.8% | 2.0% | 1.0% | 6.7% |
| Deductible (curative care only) | None | $250/person<br>$500/family | $500/person<br>$1,000/family | $750/person<br>$1,500/family | $1,500/person<br>$3,000/family |
| Physician Coinsurance (curative care only) | 10% | 10% | 10% | 10% | 10% |
| Preventive Care | free | free | free | free | free |
| Out-of-Pocket Maximum | $2,500/person<br>$5,000/family | $2,750/person<br>$5,500/family | $3,500/person<br>$7,000/family | $3,750/person<br>$7,500/family | $4,500/person<br>$9,000/family |

Notes: This table displays the old menu and new menu plan details. In addition, the table displays the enrollment shares in each plan among individual-years in the baseline sample.

Table 2: Sample Definition

| | All Company Employees | Employees Subject to Benefit Change | Hourly Employees Subject to Benefit Change (Unbalanced Panel) | Hourly Employees Subject to Benefit Change Throughout Period (Balanced Panel) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Employee-years | 229,944 | 129,994 | 69,360 | 30,394 |
| % Male | 76.5% | 74.9% | 74.9% | 74.7% |
| % White | 76.7% | 76.6% | 68.9% | 68.4% |
| % Rural | 28.1% | 38.8% | 51.6% | 52.4% |
| Mean Wage | $47,934 | $49,416 | $33,424 | $37,243 |
| Mean Age | 43.3 | 42.1 | 40.5 | 42.3 |
| Median Tenure | 12.4 | 10.6 | 8.7 | 11.4 |

Notes: This table only includes employees and not their dependents or spouses. Employees are restricted to be ”active” employees in each of the columns. This restriction excludes employees on leave, retired, on disability leave, etc. Employees are classified as rural if the associated employee job site is in an area that the US Census defines as a non-urbanized area. The balanced panel in column 4 takes the sample in column 3 and makes the further restriction that the employees be on either the new or old menus continuously throughout the five year period from 2003 to 2007.

Table 3: Summary Statistics

| Panel A: Summary Statistics by Treatment Group (year of transition to new menu) | | | | | |
|---|---|---|---|---|---|
| | transition in 2004 | transition in 2005 | transition in 2006 | transition in 2007 | never transition |
| | (1) | (2) | (3) | (4) | (5) |
| Number of observations | 47,922 | 10,259 | 6,411 | 3,513 | 1,156 |
| Number of unique individuals | 9,867 | 2,102 | 1,308 | 715 | 233 |
| % Rural | 58.9 | 56.2 | 3.3 | 45.1 | 44.5 |
| % Male | 52.1 | 55.0 | 53.9 | 54.5 | 57.9 |
| Mean Medical Expenses | $2,151 | $1,734 | $2,089 | $2,006 | $1,929 |
| Mean Age | 33.8 | 31.4 | 31.6 | 33.6 | 36.7 |
| Associated Employee | | | | | |
| Median Tenure | 13.3 | 10.8 | 9.7 | 11.4 | 10.4 |
| Mean Wage | $40,259 | $36,072 | $43,350 | $45,002 | $39,760 |

| Panel B: Summary Statistics by Rural Status | | | |
|---|---|---|---|
| | All | Rural | Nonrural |
| | (1) | (2) | (3) |
| Number of observations | 69,261 | 36,283 | 32,978 |
| Number of unique individuals | 14,225 | 7,513 | 6,712 |
| % Rural | 52.8 | 100 | 0 |
| % Male | 52.9 | 53.5 | 52.2 |
| Mean Medical Expenses | $2,072 | $2,063 | $2,082 |
| Mean Age | 33.3 | 32.3 | 34.3 |
| Associated Employee | | | |
| Median Tenure | $40,158 | $39,056 | $41,364 |
| Mean Wage | 12.4 | 12.7 | 12.1 |

Notes: This table displays summary statistics for the balanced sample of employees and associated dependents. Panel A displays summary statistics by treatment group (year of transition from the old menu to the new menu of health insurance options). Panel B displays summary statistics by rural status. Individuals are classified as rural if the associated employee job site is in an area that the US Census defines as a non-urbanized area. Wage and tenure statistics are calculated from the employee information or associated employee information for spouses and dependents.

Table 4: Difference-in-Differences Estimates: Effect of Benefit Change on Preventive Care Utilization

| | Cervical cancer screening | | | Breast cancer screening | | | Colorectal cancer screening | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| treat | -0.005 | -0.044** | 0.024 | 0.013 | -0.017 | 0.038* | -0.040*** | -0.085*** | -0.008 |
| | (0.013) | (0.022) | (0.017) | (0.017) | (0.028) | (0.021) | (0.014) | (0.023) | (0.017) |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural | | Rural | Nonrural |
| Mean of dependent var | 0.40 | 0.41 | 0.39 | 0.40 | 0.40 | 0.39 | 0.14 | 0.13 | 0.16 |
| N | 21480 | 10960 | 10520 | 14394 | 6870 | 7524 | 14203 | 6822 | 7381 |

| | Cholesterol screening | | | Child immunizations | | |
|---|---|---|---|---|---|---|
| | (10) | (11) | (12) | (13) | (14) | (15) |
| treat | -0.010 | -0.036*** | 0.008 | -0.019 | -0.063*** | 0.038 |
| | (0.007) | (0.012) | (0.010) | (0.016) | (0.022) | (0.026) |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural |
| Mean of dependent var | 0.26 | 0.24 | 0.29 | 0.45 | 0.43 | 0.46 |
| N | 48529 | 25048 | 23481 | 9701 | 5376 | 4325 |

Notes: This table reports results for OLS estimation of equation (1). The dependent variable is an indicator that is equal to one when the person has the procedure in the relevant year and zero otherwise. The samples used in each regression are described in Appendix Table A1. All robust standard errors are clustered at the person level. All the regressions contain the following controls: year fixed effects, treatment group fixed effects, US Census region fixed effects, an employee indicator, a rural status indicator, and age fixed effects. The variable "treat" is an indicator variable that is equal to one when the person is on the new menu of plans. The results are shown separately for the pooled sample and by rural status. Individuals are classified as rural if the associated employee job site is in an area that the US Census defines as a non-urbanized area. ** p-value <0.10, ** p-value<0.05, *** p-value<0.01

Table 5: Difference-in-Differences Estimates: Effect of Benefit Change on Outpatient Visits

| | Outpatient visits | | | Outpatient visits with preventive care | | | Outpatient visits with no preventive care | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| treat | -0.368*** | -0.568*** | -0.096 | -0.033 | -0.096** | 0.025 | -0.335*** | -0.472*** | -0.121 |
| | (0.112) | (0.184) | (0.153) | (0.029) | (0.044) | (0.040) | (0.108) | (0.177) | (0.147) |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural | | Rural | Nonrural |
| Mean of dependent var | 8.13 | 8.06 | 8.20 | 0.94 | 0.86 | 1.02 | 7.19 | 7.20 | 7.18 |
| N | 69261 | 36283 | 32978 | 69261 | 36283 | 32978 | 69261 | 36283 | 32978 |

Notes: This table reports results for OLS estimation of equation (1). The reported regressions investigate the effect of the benefit change on the number of outpatient visits. The dependent variables are: the number of outpatient visits (columns 1 through 3), the number of outpatient visits with an analyzed preventive procedure (columns 4 to 6), and the number of outpatient visits without an analyzed preventive procedure (columns 7 to 9). The balanced sample is used in all these regressions. All robust standard errors are clustered at the person level. All the regressions contain the following controls: year fixed effects, treatment group fixed effects, US Census region fixed effects, an employee indicator, a rural status indicator, and age fixed effects. The variable "treat" is an indicator variable that is equal to one when the person is on the new menu of plans. The results are show separately for the pooled sample and by rural status. Individuals are classified as rural if the associated employee job site is in an area that the US Census defines as a non-urbanized area. ** p-value <0.10, ** p-value<0.05, *** p-value<0.01

Table 6: Hazard Estimates: Association Between Outpatient Visit for Infection and Cervical Cancer Screening

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Infection prior month | 1.754*** | 1.605*** | 1.735*** | 1.599*** |
| | (0.127) | (0.128) | (0.129) | (0.130) |
| | [5.78] | [4.98] | [5.67] | [4.95] |
| Infection next month | | | 0.212 | 0.064 |
| | | | ( 0.223) | (0.225) |
| | | | [1.24] | [1.07] |
| Additional Controls | | | | |
| Mean annual number of outpatient visits | | x | | x |

Notes: A Single-Spell Cox Proportional Hazard model is fit to the data. The sample is restricted to those who transition to the new plans in 2004. The sample is limited to those who are eligible for a cervical cancer screening. In these columns, the duration begins on the date of screening in 2003 or Jan 1, 2003 if no screening was done in 2003. Only cervical cancer screenings done after 12 months without a screening are counted as failures in the hazard analysis. ”Infection prior month” is a dummy variable that equals one when the subject had a visit related to a urinary tract infection during the prior 28 days. Columns 3 and 4 include an additional placebo variable ”Infection next month”, which is a variable that indicates if the infection takes place in the subsequent 28 days omitting the first week (7 days) after the current date. That is, the indicator turns on if the infection takes place between 8 days and 36 days after the current date. We omit procedures/tests billed within the first week after the current date as it is relatively common for procedures/tests associated with the current visit to be billed a few business days after the visit took place if it takes some time to process the ordered test/procedure. To remove any of this mechanical effect from procedures ordered at a visit with preventive care, we thus define “Infection next month” as described above. Coefficients are displayed above with standard errors in parentheses and hazard ratios in brackets. Additional covariates included in all specifications are age indicators (5 year bins), US Census region fixed effects, a rural indicator, and an employee indicator. When noted, additional controls are included: a 4th degree polynomial in the mean annual outpatient visits for an individual. * $p<0.10$ , ** $p<0.05$, *** $p<0.01$

# APPENDIX
# (For Online Publication)

## A Background on Preventive Care Procedures

The US Preventive Services Task Force (USPSTF), a branch of the US Department of Health and Human Services, sets guidelines for preventive care procedures. According to discussions with medical professionals, these standards are commonly used by practitioners to make preventive care recommendations. According to the USPSTF, women age 21 through 64 should be tested regularly for cervical cancer by a pap smear test. For women below 30 years of age, it is recommended that they go to the doctor annually for a gynecological exam including a pap test. After having three consecutive normal pap smear tests, the guidelines indicate that a woman over 30 can elect to have pap tests every 2-3 years. Otherwise, women over 30 should continue to do annual pap tests. Because positive pap test results are quite common, many women over 30 years of age should continue to do annual screenings. The female company enrollees that were eligible for pap tests are summarized in Table A1 column 1.

Prior to 2009, the USPSTF advised women over age 40 to be screened for breast cancer by mammography every 1-2 years. The USPSTF cited evidence that such screening significantly reduces mortality from breast cancer. Over the course of her lifetime, one in eight women will develop breast cancer. The risk of breast cancer increases significantly with age, and family history plays a large role in this type of cancer: 20-30 percent of women with breast cancer have a close relative who had the disease. Although in November 2009 the USPSTF came out against routinely screening women below 50 years of age, the female population over 40 years of age is examined in this paper. Table A1 column 2 describes women that were over 40 years of age in the company population.

Colorectal Cancer is the third most common type of cancer and the second leading cause of cancer death in the United States. The USPSTF recommends screening for colorectal cancer using fecal occult blood testing, sigmoidoscopy, or colonoscopy in adults, beginning at age 50 years and continuing until age 75 years. Evidence suggests that any of the following three regimens will be approximately equally effective in life-years gained, assuming perfect adherence: (1) annual high-sensitivity fecal occult blood testing, (2) sigmoidoscopy every 5 years combined with high-sensitivity fecal occult blood testing every 3 years, and (3) screening colonoscopy at intervals of 10 years. Table A1 column 3 describes adults over 50 years of age in the company population.

Cardiovascular disease accounts for nearly half of all deaths in the United States, and nearly a third of coronary heart disease events are attributable to high cholesterol levels (above 200 mg/dL). Screening recommendations for cholesterol vary with patient pre-existing risk factors. The USPSTF recommends routine cholesterol screening among men over age 35 for lipid disorders. In addition, the USPSTF recommends screening both men and women over age 20 for lipid disorders if they are at increased risk of coronary heart disease. The USPSTF makes no recommendation for or against routine cholesterol screening in other adult populations. There is uncertainty about the optimal interval for screening. Based on existing evidence, some recommendations suggest screening every 5 years, shorter intervals for people who have lipid levels close to those demanding therapy, and longer intervals for those not at increased risk who have had repeatedly normal lipid levels. For the purpose of our analysis, we focus on cholesterol screening among adults over age 18 in the company population (summarized in Table A1 column 4).

We also consider a more general category of procedures in the claims data: child immunizations. There are recommended immunizations at all ages, however most recommended immunizations are heavily concentrated in the early years of life. Here the analysis is limited to enrollees that were 4 years of age and younger. The enrollee population 4 years of age and younger is described in Table A1 column 5. Identification of immunization claims in the data is more tricky than the identification of cancer screening claims. For the measure of immunizations used in this paper, we count all claims with the description specifying "immunization". From inspecting doctor visits for young children, we believe that other claim descriptions were sometimes recorded for doctor visits that included immunizations. Thus, the measure of immunizations used in this paper can be viewed as a proxy that underestimates actual immunizations but should be highly correlated with actual immunizations. Another reason that the immunization measure may not capture all immunizations is that it is fairly common for children to receive immunizations outside the normal medical delivery system. Because noise in the immunization measure is unlikely to be correlated with the staggered introduction of the new menu insurance plans, the noise does not interfere with our analysis as the observed changes in the immunization measure can be interpreted as truly suggestive of changes in the actual child immunization rate even if the immunization measure may not be meaningful in level.

# B Appendix Tables and Figures

Table A1: Summary of Estimation Samples

| | Baseline samples by eligibility for specific procedures | | | | |
|---|---|---|---|---|---|
| | Cervical Cancer Screening | Breast Cancer Screening | Colorectal Cancer Screening | Cholesterol Screening | Child Immunizations |
| | (1) | (2) | (3) | (4) | (5) |
| Number of observations | 21,480 | 14,394 | 14,203 | 48,529 | 9,701 |
| Number of unique individuals | 4,494 | 3,245 | 3573 | 10,382 | 4,975 |
| % Employees | 35.6 | 39.8 | 65.9 | 62.6 | 0 |
| % Spouses | 63.4 | 60.2 | 34.1 | 33.9 | 0 |
| % Dependents | 1.0 | 0 | 0 | 3.5 | 100 |
| % Rural | 51.0 | 47.7 | 48.0 | 51.6 | 55.4 |
| % Male | 0 | 0 | 55 | 54.0 | 51.3 |
| Mean age | 43.6 | 49.1 | 55.1 | 43.1 | 2.2 |
| Mean medical expenses | $3,199 | $3,459 | $3,743 | $2,559 | $1,193 |
| Associated employee | | | | | |
| Mean wage | $39,236 | $39,630 | $40,496 | $40,026 | $35,591 |
| Median tenure | 13.2 | 15.3 | 17.7 | 13.2 | 6.4 |
| Criteria | women, age≥21 & age≤65 | women, age≥40 | age≥50 | age≥18 | age≤4 |

Notes: This table summarizes the eligible populations used in the regression analysis in Table 4 in the text. Columns 1 through 4 each summarize the individuals eligible for the procedure in question from the balanced sample (described in Table 3). As discussed in the text, column 5 describes the unbalanced sample of children 4 years of age and younger used to analyze the effect of the benefit change on child immunizations. Individuals are classified as rural if the associated employee job site is in an area that the US Census defines as a non-urbanized area.

Table A2: Plan Enrollment by Rural Status

| Panel A: Plan Enrollment | | | |
|---|---|---|---|
| | All | Rural | Nonrural |
| | (1) | (2) | (3) |
| Old Menu | | | |
| Plan 1 | 36.0% | 36.6% | 35.5% |
| Plan 2 | 63.0% | 62.5% | 63.4% |
| Plan 3 | 1.0% | 0.8% | 1.1% |
| New Menu | | | |
| Plan 1 | 54.6% | 52.7% | 57.0% |
| Plan 2 | 35.8% | 38.5% | 32.3% |
| Plan 3 | 2.0% | 1.9% | 2.1% |
| Plan 4 | 1.0% | 1.1% | 0.9% |
| Plan 5 | 6.7% | 5.9% | 7.7% |

| Panel B: Impact of Benefit Change on Plan Has Deductible | | | |
|---|---|---|---|
| | | I(deductible) | |
| | (1) | (2) | (3) |
| treat | 0.344*** | 0.349*** | 0.353*** |
| | (0.006) | (0.009) | (0.008) |
| Sample restriction | | Rural | Nonrural |
| Mean of dependent var | 0.32 | 0.36 | 0.29 |
| N | 69261 | 36283 | 32978 |

Notes: Panel A above summarizes the plan enrollment on the old and new menus among the whole sample (column 1), the rural subsample (column 2) and the nonrural subsample (column 3). Panel B presents results from a difference-in-differences regression analyzing the impact of the benefit change on an indicator that the plan the individual is enrolled in has a deductible. For these regressions, we utilize the balanced sample used in the baseline analysis. Individuals are classified as rural if the associated employee job site is in an area that the US Census defines as a non-urbanized area.

Table A3: Robustness: Difference-in-Differences Analysis of Benefit Change Monthly Regressions

| Panel A: Monthly Regressions | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Cervical cancer screening | | | Breast cancer screening | | | Colorectal cancer screening | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| treat | -0.001 | -0.004* | 0.002 | 0.001 | -0.001 | 0.003* | -0.003** | -0.007*** | -0.000 |
| | (0.001) | (0.002) | (0.002) | (0.001) | (0.002) | (0.002) | (0.001) | (0.002) | (0.002) |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural | | Rural | Nonrural |
| Mean of dependent var | 0.035 | 0.036 | 0.034 | 0.034 | 0.034 | 0.033 | 0.013 | 0.012 | 0.014 |
| N | 257760 | 131520 | 126240 | 172728 | 82440 | 90288 | 170436 | 81864 | 88572 |
| | Cholesterol screening | | | Child immunizations | | | | | |
| | (10) | (11) | (12) | (13) | (14) | (15) | | | |
| treat | -0.001 | -0.004** | 0.000 | -0.001 | -0.008** | 0.010** | | | |
| | (0.001) | (0.002) | (0.001) | (0.003) | (0.004) | (0.004) | | | |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural | | | |
| Mean of dependent var | 0.032 | 0.028 | 0.035 | 0.067 | 0.067 | 0.067 | | | |
| N | 582348 | 300576 | 281772 | 116412 | 64512 | 51900 | | | |

| Panel B: Monthly Regressions: Exclude one month pre/post change | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Cervical cancer screening | | | Breast cancer screening | | | Colorectal cancer screening | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| treat | 0.000 | -0.003 | 0.003* | 0.002 | -0.001 | 0.004** | -0.003** | -0.007*** | -0.001 |
| | (0.001) | (0.002) | (0.002) | (0.001) | (0.002) | (0.002) | (0.001) | (0.002) | (0.002) |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural | | Rural | Nonrural |
| Mean of dependent var | 0.035 | 0.036 | 0.034 | 0.033 | 0.034 | 0.033 | 0.013 | 0.012 | 0.014 |
| N | 249587 | 127413 | 122174 | 167526 | 80032 | 87494 | 165680 | 79680 | 86000 |
| | Cholesterol screening | | | Child immunizations | | | | | |
| | (10) | (11) | (12) | (13) | (14) | (15) | | | |
| treat | -0.001 | -0.004** | 0.001 | 0.000 | -0.007* | 0.011** | | | |
| | (0.001) | (0.002) | (0.001) | (0.003) | (0.004) | (0.004) | | | |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural | | | |
| | 0.032 | 0.035 | 0.028 | 0.067 | 0.066 | 0.067 | | | |
| Mean of dependent var | | | | | | | | | |
| N | 564070 | 291282 | 272788 | 114596 | 63436 | 51160 | | | |

Notes: This table reports robustness analysis for the difference-in-differences analysis reported in Table 4. Panel A reports regressions analogous to those in Table 4 except at the monthly level; Panel B reports monthly level regressions omitting the month just before and just after the benefit change for each transition group. The dependent variable is an indicator that is equal to one when the person has the procedure in the relevant year and zero otherwise. The samples used in each regression are described in Appendix Table A1. All robust standard errors are clustered at the person level. All the regressions contain the following controls: year fixed effects, treatment group fixed effects, US Census region fixed effects, an employee indicator, a rural status indicator, calendar month fixed effects, and age fixed effects. The variable "treat" is an indicator variable that is equal to one when the person is on the new menu of plans. The results are show separately for the pooled sample and by rural status. Individuals are classified as rural if the associated employee job site is in an area that the US Census defines as a non-urbanized area. ** p-value $<0.10$, ** p-value$<0.05$, *** p-value$<0.01$

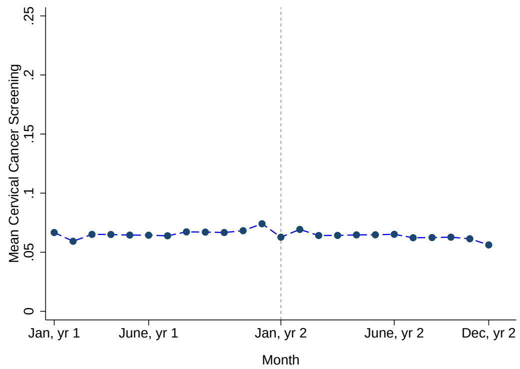
Table A4: Robustness: Difference-in-Differences Analysis of Benefit Change Dropping Group Transitioning to New Menu in 2006

| | Cervical cancer screening | | | Breast cancer screening | | | Colorectal cancer screening | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| treat | -0.008 | -0.047** | 0.038* | 0.013 | -0.020 | 0.041 | -0.045*** | -0.090*** | -0.002 |
| | (0.016) | (0.023) | (0.023) | (0.021) | (0.029) | (0.029) | (0.016) | (0.024) | (0.022) |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural | | Rural | Nonrural |
| Mean of dependent var | 0.40 | 0.41 | 0.39 | 0.40 | 0.40 | 0.40 | 0.14 | 0.13 | 0.16 |
| N | 19717 | 10901 | 8816 | 13281 | 6820 | 6461 | 13210 | 6764 | 6446 |

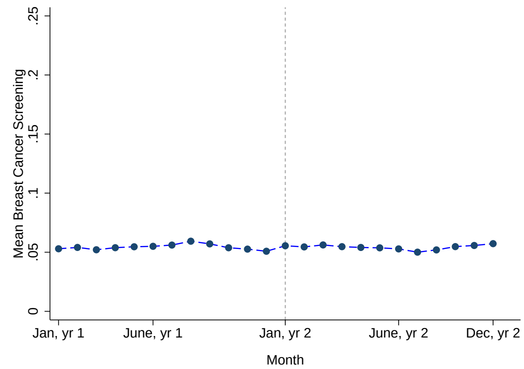
| | Cholesterol screening | | | Child immunizations | | |
|---|---|---|---|---|---|---|
| | (10) | (11) | (12) | (13) | (14) | (15) |
| treat | -0.007 | -0.037*** | 0.027** | -0.020 | -0.063*** | 0.046* |
| | (0.009) | (0.012) | (0.013) | (0.017) | (0.022) | (0.027) |
| Sample restriction | | Rural | Nonrural | | Rural | Nonrural |
| Mean of dependent var | 0.27 | 0.24 | 0.30 | 0.45 | 0.43 | 0.46 |
| N | 44250 | 24888 | 19362 | 9420 | 5376 | 4044 |

Notes: This table reports robustness analysis for the difference-in-differences analysis reported in Table 4. This table reports results for OLS estimation of equation (1), where the treatment group transitioning to the new menu in 2006 is removed. The dependent variable is an indicator that is equal to one when the person has the procedure in the relevant year and zero otherwise. The samples used in each regression are described in Appendix Table A1. All robust standard errors are clustered at the person level. All the regressions contain the following controls: year fixed effects, treatment group fixed effects, US Census region fixed effects, an employee indicator, a rural status indicator, and age fixed effects. The variable "treat" is an indicator variable that is equal to one when the person is on the new menu of plans. The results are show separately for the pooled sample and by rural status. Individuals are classified as rural if the associated employee job site is in an area that the US Census defines as a non-urbanized area. ** p-value $<0.10$, ** p-value$<0.05$, *** p-value$<0.01$

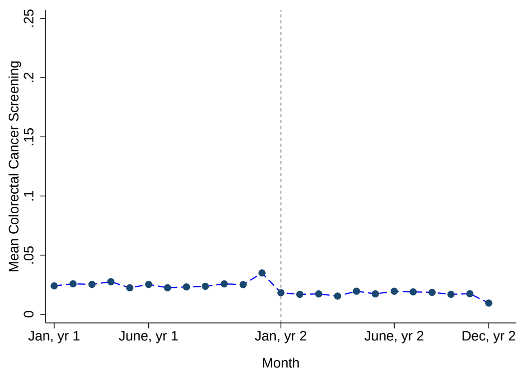
Figure A1: Robustness: Timing of Preventive Care in Years Surround Transition to New Menu



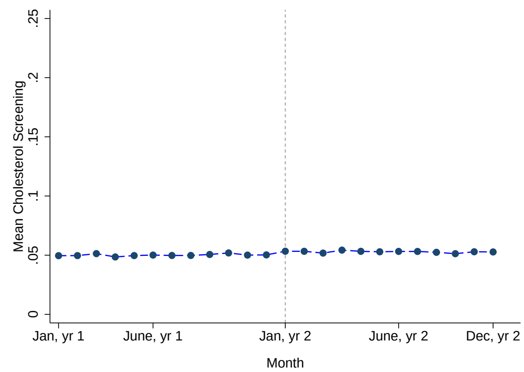

(a) Cervical Cancer Screening



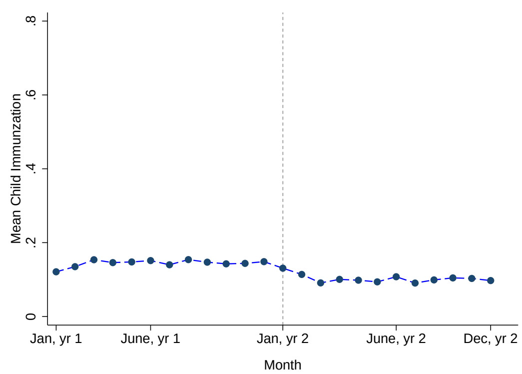

(b) Breast Cancer Screening




(c) Colorectal Cancer Screening



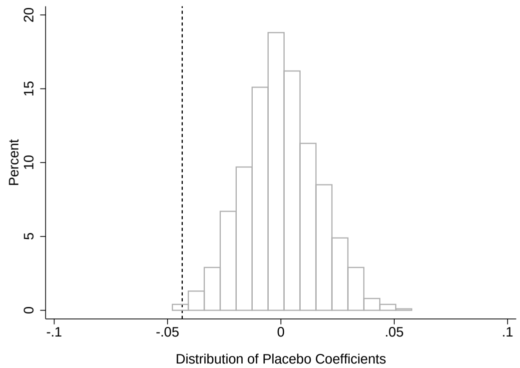

(d) Cholesterol Screening



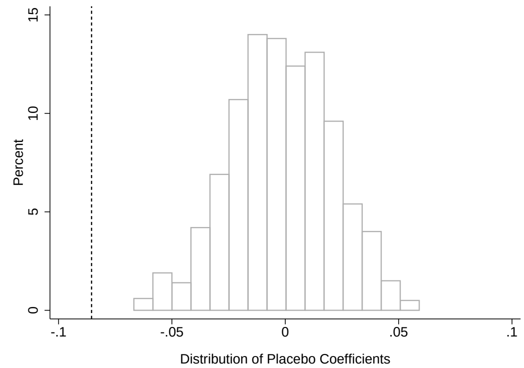

(e) Child Immunizations

Notes: Each panel of the figure above plots the mean procedure rate at the monthly level for those procedures analyzed in Table 4 removing calendar month fixed effects for the two year window surrounding the transition to the new menu. For these plots, the sample is pooled across the four transition groups. The samples used in each panel are described in Appendix Table A1.

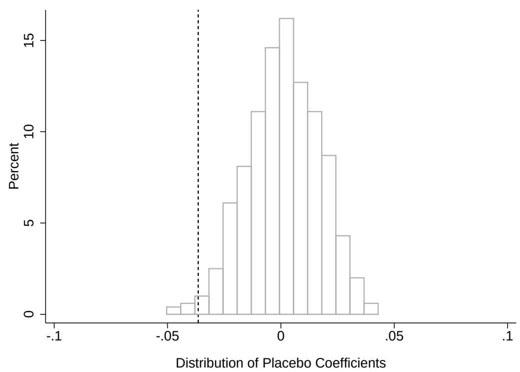
Figure A2: Robustness: Comparison of Actual Estimates vs. Placebo Treatment Estimates



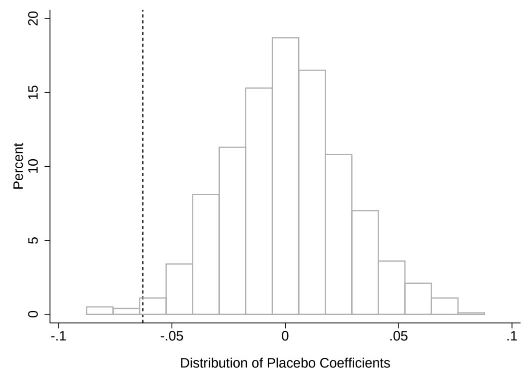

(a) Cervical Cancer Screening, Table 4 column 2




(b) Colorectal Cancer Screening, Table 4 column 8




(c) Cholesterol Screening, Table 4 column 11




(d) Child Immunizations, Table 4 column 14

Notes: This figure reports the results of permutation tests for some key coefficients in Table 4. Specifically, for the referenced regressions, we re-estimate the regressions replacing each individual's true treatment group assignment with a randomly assigned treatment status (transition year), where this random assignment is done at the employee group level where these groups are defined by job location and actual treatment group status. For each placebo treatment assignment, we repeat the difference-in-differences analysis. The histograms above plot the distribution of coefficients on the placebo treatment assignments, where we analyze 1000 placebo treatments for each specification analyzed. For comparison, the actual estimated coefficient from Table 4 is indicated with a vertical reference line in each panel. We also calculate the implied p-value on the estimated coefficient based on these permutation tests: cervical cancer screening panel (a) (p-value=0.002), colon cancer screening panel (b) (p-value $<$ 0.001), cholesterol screening panel (c) (p-value=0.013), child immunizations panel (d) (p-value=0.01).