Does it Pay to Know the Prices in Health Care?

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Abstract

Consumers have rarely known the price of medical care before receiving it, but access to price information is becoming more common. For a group of consumers with access to price information, I use detailed data on search, health care use, and negotiated prices to show that search leads consumers to pay significantly lower prices. I provide suggestive evidence that insurance coverage inhibits the use of price information which could rationalize the relatively low rates of search. The results suggest that availability of price information could have large impacts on prices, but that these impacts are mitigated by insurance coverage.

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1 Introduction

Health care spending accounts for 18% of the United States economy and has grown faster than GDP in 42 of the past 50 years. As a result, containing health care costs has become a primary concern both for the private and public sectors. But consumers know very little about prices in health care. In fact, there is a tremendous amount of price dispersion (Ginsburg 2010). It would seem that there are large returns to divulging prices to consumers; lowering search costs has reduced prices in other markets (e.g. Brown and Goolsbee 2002, Goldmanis, Hortaçsu, Syverson and Emre 2010). However, many arguments have been made that price information might have little effect on prices in health care: health insurance reduces the incentives to use the information, patients could be unwilling to switch providers, or patients might need care immediately.

In this paper, I estimate whether access to and use of price information affects the prices paid for medical care. I use a unique dataset in which a group of consumers gained access to price information provided by Compass Professional Health Services (Compass hereafter). The novel feature of the data is a direct measure of search: Compass tracks the use of its price information and thereby measures search behavior.

To estimate the impact of search on prices, I take advantage of variation in the date different consumers received access to Compass's price information. In particular, I instrument for search with whether or not the consumer was part of a pilot program that granted them access before the remaining consumers. This isolates variation in search that is unlikely to be correlated with any individual's demand for search, prior knowledge of prices, or other factors that could lead to a spurious relationship between search and prices. The reduced form estimates imply that access to price information reduces the prices paid by 1.6%. The effect is concentrated among consumers with stronger incentives to search and not found for care that is unlikely to be searched for, e.g. emergency care. Lending credence to the reduced form's identifying assumption, I do not find any evidence that those who received access earlier were on different trends than those who received access later.

These price reductions can come from two sources, consumers switching to lower-priced providers and from the entire distribution of prices falling. I find evidence consistent with the former, that access significantly increases the probability of seeing a new provider. Because the consumers in my data are a negligible fraction of any given health care market, it is unlikely that the distributions of prices fell in response to the availability of Compass's price information. Despite spending less on care, it is not clear that consumer welfare increased because the lower prices might come at the cost of lower quality care. Although the results are only suggestive, I do not find evidence that access to Compass leads consumers to receive lower quality hospital care.

With access increasing the probability of searching by 9% - 15%, the instrumental variables estimator finds that search reduces prices by 10% - 17%. In addition to being large, the estimates are plausible. In my data, moving from the 90th percentile down to the median reduces the price of a specific procedure in a specific market by 35%.

If search reduces prices paid by 10% - 17%, then why are consumers searching for less than 20% of their care? A prominent, yet largely untested, explanation is moral hazard in search (Dionne 1981, Dionne 1984, Akin and Platt 2014). Health insurance reduces consumers' exposure to price differences and thereby reduces their incentives to search. I provide suggestive evidence of moral hazard in search using variation in the marginal price for care on the date the consumer gained access to Compass. Consumers who had met their deductibles by the time they gained access faced a 50% lower marginal price for care, but were comparable to those who had not met their deductibles on many other dimensions. Those who had met their deductibles were 90% less likely to subsequently search. Based on this estimate, the elasticity of search with respect to the out-of-pocket price is approximately 1.8. The estimate is robust to many different specifications. Because the consumers' deductibles are reset on January 1, 2011, having met the deductible in 2010 does not directly affect the marginal price for care at the start of 2011. I find that those who had met their deductibles in 2010 were no less likely to search in 2011 once their deductibles had been reset.[1] Although this evidence is only suggestive, it is consistent with the idea of moral hazard in search.

Two important limitations to my findings stem from the fact that they are based upon the employees of a single firm that chose to purchase price information. First, if these consumers are particularly responsive to insurance coverage or prone to using price information, it becomes difficult to generalize my results to the population at large. Second, I am not able to capture the general equilibrium effects of insurance on prices (via search). The literature on insurer-provider bargaining finds that being able to steer patients to particular providers impacts network formation (e.g. Ho 2009, Ho and Lee 2013) and lowers prices (e.g. Sorensen 2003, Wu 2009). My estimates do not capture these changes to the bargaining process and so likely understate the impacts of access to price information and search.[2]

This paper is related to two recent studies on price transparency in health care. Whaley, Schneider Chafen, Pinkard, Kellerman, Bravata, Kocher and Sood (2014) use data from a different price information supplier, Castlight Health, and compare the prices that searchers and non-searchers pay. They find that searching is associated with a 13%-14% lower price for both laboratory tests and advanced imaging procedures. They ameliorate concerns about biases in their estimates by showing that searchers had been receiving slightly higher prices before access to the search tool and that searching for one type of procedure does not help searchers obtain lower prices (relative to non-searchers) on other types of procedures. My results complement theirs by studying the impact of price information and search for all types of procedures, by taking advantage of plausibly exogenous variation in search, and by directly examining the association between insurance coverage and search. Christensen, Floyd and Maffett (2014) study whether transparency laws that lead to publicly available

[1] Because consumers are forward looking, not only the current or spot price of care matters, but the future price of care could matter as well (Keeler, Newhouse and Phelps 1977, Aron-Dine, Einav, Finkelstein and Cullen 2012). How this affects the interpretation of this result is discussed when the result is presented. Throughout the paper, prices paid should be understood as spot prices.

[2] Cutler and Dafny (2011) point out that making price information public could facilitate collusion between providers and so actually lead to higher prices in equilibrium. Evidence for this effect has been seen in the Danish concrete industry (Albæk, Møllgaard and Overgaard 1997).

price information reduce charge prices for hip replacements. They find that charges for hip replacements fell by 7% in states that adopted the laws while charges for a less shoppable procedure, appendectomies, did not change. My results on prices paid are not directly comparable to theirs because the relationship between charge prices and transaction prices is unclear. However, my suggestive results on moral hazard in search provide a foundation for their findings and suggest one reason for their relatively small impacts: by the end of their sample, only 13% of privately insured individuals had high-deductible health plans that would have given them an incentive to search (The Kaiser Family Foundation and Health Research & Educational Trust 2014). However, as the fraction of consumers in these plans continues to rise, from 4% to 20% between 2006 and 2014, it becomes more likely that transparency laws will have larger impacts.

This paper is also related to the broader literature on consumer-directed health care (CDHC). Empirical work in this area has found that health care expenditures fall when consumers are put onto less generous insurance plans (Parente, Feldman and Christianson 2004, Buntin, Damberg, Haviland, Kapur, Lurie, McDevitt and Marquis 2006, Dixon, Greene and Hibbard 2008, Haviland, Sood, McDevitt and Marquis 2011). Because these papers do not have search data, it is difficult to empirically differentiate expenditure reductions due to increased consumer search from those due to reduced care use. My results fill this gap and provide evidence consistent with the premise of CDHC.

The remainder of the paper proceeds as follows. Section 2 provides background information on pricing in health care and price information firms like Compass. Section 3 describes the data. Section 4 presents the empirical strategy and results for the impact of search on prices. Section 5 lays out the two empirical strategies I use to estimate the impact of insurance on search and presents the results. Section 6 concludes.

2 Prices and Price Information in Health Care

For those with private health insurance, the price of care is determined by negotiations between insurers and providers.[3] Evidence suggests that these negotiations reduce prices for insurers with greater bargaining power (e.g. Cutler, McClellan and Newhouse 2000, Sorensen 2003) and relative to the previous cost-based system of provider payments (e.g. Dranove, Shanley and White 1993). Despite these negotiations, even for narrowly defined procedures, there is a tremendous amount of price dispersion.[4] As seen in Table 1, prices vary considerably for a mammogram, a routine and relatively homogeneous procedure.[5] Within a small geographic market, consumers with insurance from CIGNA can pay between \$202 and \$496 for a mammogram. Those insured by Blue Cross and Blue Shield can pay anywhere from \$251 to \$470. Table 1 of Ginsburg (2010) reports private insurer payment rates to hospitals for 8 separate markets, most of them major metropolitan areas. On average, the median payment rate for inpatient care is 47% lower than the maximum payment. In the large claims database I use (discussed in Section 3), for a given geographic market and narrowly defined procedure, moving someone from the 90th percentile of the price distribution down to the median reduces the price by 35% on average.

Despite the large amount of dispersion, prices negotiated between insurers and providers are generally not publicly known (Stockwell Farrell, Finocchio, Trivedi and Mehrotra 2010, United States Government Accountability Office 2011, Robert Wood Johnson Foundation 2012, Rosenthal, Lu and Cram 2013). Only very recently have firms and insurers begun to provide consumers access to prices. In 2012, CIGNA unveiled a website available to its insureds that helps them compare providers' prices; WellPoint has had similar resources for its

[3]Medicare and Medicaid set their own prices and providers then choose whether to accept Medicare and Medicaid patients at those prices.

[4]There is a long literature that explores the impact of search frictions on equilibrium prices, price dispersion, and changes in prices over time (e.g. Stigler 1961, Diamond 1971, Burdett and Judd 1983, Hortaçsu and Syverson 2004, Hong and Shum 2008, Tappata 2009).

[5]These are not the widely available charge data, but the actual prices contracted on between the providers and insurers. They are available on New Hampshire's HealthCost website: www.nhhealthcost.org. All providers are within a 20-mile radius of zip code 03101.

insureds since 2009; and a number of private firms that are not insurers have begun to supply price information as well. In addition to private market efforts to increase transparency, more than 30 states require hospitals to disclose charges for common procedures and post them online (Christensen et al. 2014). Although there are concerns that price transparency could foster collusion and actually lead to higher prices (Cutler and Dafny 2011), the trend appears to be towards greater price transparency.

How do consumers search with price information firms? Compass Professional Health Services, the source of data for this article, is a private firm that supplies price information.[6] Compass is typically hired by a self-insured firm on behalf of the firm's employees. The client firm's employees are then able to use Compass's services without paying any fees. To obtain prices, the employee contacts Compass, indicates what care she needs, and provides information on her geographic location and health insurance. Compass then supplies a list of prices negotiated between insurers and providers.[7]

In conjunction with the increase in transparency, consumers are bearing a larger fraction of their health expenses. Worker contributions for insurance premiums have risen 80-90% in the past ten years. In addition to higher premiums, consumers are also facing less generous cost-sharing. Between 2006 and 2014, the share of covered workers in high-deductible health plans rose from 4% to 20% (The Kaiser Family Foundation and Health Research & Educational Trust 2014). A number of studies, (e.g. Parente et al. 2004, Buntin et al. 2006, Dixon et al. 2008, Haviland et al. 2011), have shown that health care expenditures tend to fall when consumers are switched to high-deductible health plans. Although this is consistent with the hypothesis of consumer-directed health care–that consumers will shop around and find lower-priced providers when given incentives to do so–it is not direct evidence of this behavior. Without data on search itself, it is difficult to refute the possibility that consumers

[6] In addition to providing price information, it reviews its clients' previous medical bills, checks for generic alternatives to branded pharmaceuticals, schedules medical appointments, explains the details of insurance contracts, and a number of other health care concierge services. However, the majority of clients' inquiries are for price estimates.

[7] This is the allowed amount on the medical claim. This information can be combined with a consumer's non-linear insurance contract to reflect the consumer's out-of-pocket price.

are simply purchasing less care.

3 Data

The data come from one of Compass's large corporate clients.[8] The client owns and operates restaurants throughout the United States. It offers health benefits to employees who are in senior positions at the restaurants (e.g. manager, head chef) and those who work in the corporate offices. The client self-insures, but contracts with a major insurer to administer the health plans. The data include the date that employees gained access to Compass, a measure of when the employees contacted Compass for price information, the employees' medical claims, and information about the insurance plans from which the employees chose. Corporate office employees gained access to Compass on September 27th, 2010; non-corporate employees gained access at the start of the next year.

The unique feature of these data is the direct measure of consumer search. Employees may contact Compass via telephone or email, but the great majority of contacts were over the phone; for simplicity, I will refer to all inquiries for price information as calls to Compass or search. Although Compass provides a number of services to its clients, my measure of search only includes calls in which the employee would have been given price information.[9] The data do not include information on exactly which procedure was called about, but do include which employee called, the date of the call, and whether the contact was about price information.

The claims data consist of all the employees' medical claims from 2009 and 2010. The 387,774 claims include many different pieces of information used in the analysis: exactly what procedure was performed (using the American Medical Association's CPT billing codes), which employee was the patient (including people covered by the employee's health insurance

[8]The identity of the client must remain anonymous due to a confidentiality agreement.

[9]The specific categories included are contacts classified as about prices, prices and quality, scheduling appointments, coordination of care, and care road map. More than 88% of calls in these categories were about prices. Excluded contacts were those classified as questions about insurance, prescription reviews, bill summaries, and getting medical records.

policy), the "setting" of the care (hospital inpatient, hospital outpatient, hospital imaging, physician imaging, physician's office, and global imaging facility), the transacted price for that procedure, and the date that the procedure took place. One employee is excluded from the sample because she had two procedures with an average price more than seventy standard deviations above the rest of the sample.[10]

The top panel of Table 2 shows that in the final three months of 2010, 12% of the corporate employees searched for price information at least once in that time period. The demographic information presented in the next three rows of Table 2 indicates that corporate office employees lived in slightly higher socioeconomic status zip codes than non-corporate employees.[11] However, the bottom panel of Table 2 indicates that they were not receiving substantially different prices for their care. In addition, they did not appear to be getting care that came from price distributions with systematically higher variance. Specifically, for procedures for which the distribution of prices in that market were available, the 90th percentile of the distribution was 36% higher than the median. For non-corporate employees, the corresponding number is 34%.[12] Although corporate and non-corporate employees are consuming different amounts of care, they do not appear to be paying different prices or pulling from different price distributions. As discussed in Section 4, the similarity of prices across the groups plays an important role in the empirical strategy.

[10]Including this employee in the analysis does not qualitatively affect the results. Neither does excluding just the two outlier procedures and using that employee's other medical claims.

[11]Because I observe limited individual demographics, I match employees' five-digit zip codes to the demographic information from the 2010 Census. The Census Bureau reports that the median income in 2010 for households with the head younger than 65 years of age was $56,850, that the fraction of the population 25+ with a bachelor's degree from 2008-2012 was 28.5%, and that 78% of the population reported being white in 2012. Per insured person, employees spent about $2,664 on health care; this is somewhat less than the national average of $3,583 (Health Care Cost Institute 2012).

[12]Because price information firms have to reverse engineer their price estimates, there are some markets and procedures for which these firms will not have price estimates. This occurs in approximately 20% of the claims in in my data.

4 Search and the Prices Paid for Care

Empirical Strategy

In the raw data, search is associated with a 15% reduction in the price paid for care.[13] However, the decision to search for price information is likely related to a number of factors that could also affect the price paid for care. For example, the employee's previous knowledge of prices could be a strong determinant of both her decision to search as well as the prices she pays. If the employees who do not call Compass tend to receive low prices already, then a simple regression of prices on search will understate the impact of search. Because of this, I use variation in search due to differences in the dates that employees gained access to Compass. In particular, I instrument for search with access to Compass.

On September 27th, 2010, the corporate office employees gained access to Compass. Only the corporate office employees had access because the client firm wanted to evaluate Compass prior to hiring them for their entire insured workforce.

Because prices could be rising over time, a simple pre-post design could yield misleading estimates. Instead, I take advantage of the employees who did not gain access to Compass in 2010, the non-corporate employees, to remove the underlying time trends. Initially, I focus on the reduced form of access on prices. Transacted prices for corporate employees are compared to those of non-corporate employees before and after September 27th in a differences-in-differences regression design. The empirical specification is

$$ln(price_{ijmt}) = (post10_t * corporate\ employee_i)\beta_1 + Z_c\gamma + \lambda_w + \lambda_{jm} + \lambda_i + \varepsilon_{ijmt} \qquad (1)$$

where $price_{ijmt}$ is the negotiated price for person i, procedure j, in market m, at time t. The transacted price is used to capture the total price change, not just the employee's out-

[13]This compares the percentage deviations from the procedure's average price in the market for procedures for which the employees searched and those for which they did not search. The percentage deviation is calculated as $(price - mean\ price)/mean\ price$ and weighted by the mean price for that procedure in the geographic market. For procedures not called about, the average percentage deviation is -1.2; for those called about it is -16.2.

of-pocket reduction. $post10_t * corporate\ employee_i$ is the differences-in-differences variable; Z_c includes indicators for whether the employee had hit the coinsurance portion of her insurance plan, whether the procedure is being billed by a doctor's office, inpatient hospital department, outpatient hospital department, global imaging facility, other imaging facility (e.g. hospital), or physician imaging (for interpretation of the image), and for whether the procedure is an emergency, imaging, surgical, or other type of procedure; λ_w are week fixed effects; λ_{jm} are market-procedure fixed effects; λ_i are employee fixed effects; and ε is an error term. The main effects for $post10_t$ and $corporate\ employee_i$ are not explicitly included in the regression because they are not separately identifiable from the week and employee fixed effects. Compass treats the Core-Based Statistical Area as the market when giving information to its clients and that convention is followed in this analysis.[14] Standard errors are clustered at the market level to account for any correlations in the residuals within a market over time.

The key identifying assumption in equation (1) is that corporate and non-corporate employees would have experienced the same percentage change in prices after September 27th, 2010 had neither group been given access to Compass. Even if there are systematic differences in the amount of care used, it is the trend in prices that is critical to the empirical analysis. I run pretrend tests to evaluate whether corporate-office employees were on different price trends prior to access.

After the reduced form analysis, I present the corresponding instrumental variables estimates. As mentioned previously, I observe the date that employees searched, not the procedures for which they searched. To estimate the relationship between search and prices, I need to map the dates of search onto procedures for which employees received price information. I use three approaches. First, I assume that any medical care the employee received within thirty days of calling Compass is medical care for which she received price information. Unlike many goods, there is a significant time lag between deciding to purchase certain

[14]Core-Based Statistical Areas are Metro and Micropolitan Statistical Areas defined by the Office of Management and Budget.

types of medical care and actually being able to consume it (Coyte, Wright, Hawker, Bombardier, Dittus, Paul, Freund and Ho 1994, Bell, Crystal, Detsky and Redelmeier 1998).[15] The thirty day window allows enough time for the employee to have received the care she obtained price information for without being overly inclusive. Second, I use the same thirty day period as before, but assume employees do not forget the price information they have previously obtained. Specifically, if an employee searched for a procedure in the past, I mark any subsequent occurrence of that procedure as having been searched for as well. And lastly, I create an upper bound by counting any procedure after the first search as something the person received information about. Although this clearly overstates the information available to the employee, it will provide a lower bound on the impact of search on prices.

Results for Access and Prices Paid

The results from estimating the differences-in-differences specification are presented in Table 3. If interpreted causally, the baseline estimate implies that gaining access to Compass reduced prices paid by 1.6%. Although the price data are noisy, the estimate is statistically significant at conventional levels.

Because there is so much variation in health care prices, one might worry that outliers are driving the results. To address this concern, I winsorize the top and bottom 5% of observations and re-estimate equation (1). The results are shown in column (2). The point estimate falls slightly in magnitude to -0.014, but remains highly statistically significant.

Access to price information is unlikely to affect all types of care equally (Bloche 2006, Sinaiko and Rosenthal 2011, Robert Wood Johnson Foundation 2012). Emergency care does not seem particularly amenable to search because of the urgent nature of the problem. To assess this possibility, I interact the differences-in-differences variable with an indicator for whether or not the person had emergency care on the given day and present the results

[15] Coyte et al. (1994) and Bell et al. (1998) surveyed hospitals and found that the median waiting time for a consultation for a knee replacement was between two weeks and 25 days. The mean wait time was 3.2 weeks while the 90th percentile of the distribution was 4 weeks (Coyte et al. 1994).

in column (3) of Table 3. The implied impacts of access to price information are shown separately for non-emergency and emergency care along with the main effect.[16] For non-emergency care, the point estimate is extremely similar to the baseline result. I do not find any evidence that access to price information reduces the prices paid for emergency care.

People who have met the deductible of their insurance contract could be less likely to search and so less likely to obtain price reductions with access to price information. I interact the differences-in-differences estimator with an indicator for whether or not the employee had met her deductible and estimate this version of equation (1). Column (4) of Table 3 reports the implied effects. Access to price information reduced transacted prices by 1.9% for employees who had not met their deductibles, but it did not have a statistically significant impact for those who had.

Another concern is that patients will not be able to search effectively for complicated care: As the bundle of medical care consumed increases in complexity, the probability of receiving accurate price estimates falls. I proxy for the complexity of care with the number of procedures a person receives in a day. On average, employees receive almost 7 procedures per day, but there is a long right tail with some employees receiving more than 50 procedures in a day. I interact an indicator for receiving 20 or more procedures in a given day with the differences-in-differences estimator and present the results in column (5). Access is associated with a 1.6% reduction in prices paid when the employee has fewer than 20 procedures on a given day, but little measurable impact on days with 20 or more procedures.

Calling and receiving price information might not only affect the price people pay for care, but also the quantity of care received. For instance, an employee might call to learn the price for a procedure, find out it is much more expensive than anticipated, and choose

[16] Specifically, I estimate

$$\begin{aligned} ln(price_{ijmt}) =& (post10_t * corporate\ employee_i)\beta_1 + (post10_t * corporate\ employee_i * emergency_{it})\beta_2 + \\ &+ emergency_{it}\beta_3 + Z_c\gamma + \lambda_w + \lambda_{jm} + \lambda_i + \varepsilon_{ijmt}. \end{aligned}$$

The results presented are $\hat{\beta}_1$ (non-emergency care), $\hat{\beta}_1 + \hat{\beta}_2$ (emergency care), and $\hat{\beta}_3$ (main effect of having emergency care that day).

to not receive that care. On the other hand, the employee might learn the price is much lower than anticipated and choose to receive the care when she would not have without calling. Ex ante, it is not clear in which direction more accurate price information pushes quantity. However, the effect on quantity is unlikely to affect the estimated impacts on prices in my empirical setting for a number of reasons. First, these employees are a small part of their health care markets and so slight variations in their care use will have approximately no impact on prices. And second, transacted prices are negotiated between insurers and providers infrequently. For changes in quantity induced by search to affect prices, the insurer would have to track search and negotiate prices more frequently. In principle, this could have happened; in practice, the insurer was not kept apprised of the employees' search habits. As a result, it is unlikely that the reduction in prices was due directly to a change in quantity of care consumed.

The identifying assumption for the differences-in-differences framework is that the corporate and non-corporate employees would have continued on the same trend had neither group gained access to Compass. Although this is not directly testable, I can test whether the two groups of employees were on the same trends prior to access to price information. If they were not, then it casts serious doubt on the validity of the identifying assumption. First, I interact an indicator for being a corporate employee with a linear trend (in weeks). Second, I test for whether the corporate office employees were on a different linear trend in 2009 or in 2010 before they had access.[17] This is distinct from the first approach because it only uses information from before access to Compass to estimate the differential trends. And lastly, I include week dummies interacted with whether the person was a corporate employee for the 20 weeks preceding access. The results for these tests are presented in Table 4

Column (2) of Table 4 shows that that the differential linear trend is not statistically distinguishable from zero. Column (3) shows that corporate office employees were not on

[17]Because the regression specification has a set of week fixed effects, I create a new variable that is the week interacted with whether the person is a corporate employee. I create separate variables for 2009 and for the portion of 2010 before corporate employees had access.

differential linear trends in either 2009 or the months of 2010 in which they had access. Column (4) presents the estimated differences in prices paid by corporate employees in the five weeks preceding access to Compass.[18] There is no clear downward trend that would suggest the differences-in-differences coefficient is simply picking up a spurious correlation. Of the twenty differential effects estimated, two were statistically significant at the ten percent level and one was significant at the five percent level. This is expected due simply to random chance and does not create concern that the corporate and non-corporate employees were on differential price trends before access.

The corresponding IV estimates are given in Table 5. First stage estimates are shown in Appendix B. Each column presents the estimated impact of search on prices for one of the mappings described in the previous subsection between calls and procedures about which the employee gained information. As seen in column (1), when any procedure obtained within 30 days of calling is treated as one the employee received price information about, search is estimated to reduce the price by 17%. Although this is a very large price reduction, it is a reasonable one. Recall that on average, moving someone from the 90th percentile down to the median of the price distribution would reduce prices by approximately 35%. Searching for price information achieves about half of that reduction.

In columns (2) and (3), results are presented for the other two methods of assigning search to procedures. Although the estimated impact decreases slightly in magnitude, it remains quite large and statistically distinguishable from zero. In each case, the first stage is strong and provides little concern about small sample bias (Stock and Yogo 2002).

There are two ways that access to and search for price information can lower prices: (i) by lowering the rates negotiated between providers and insurers and (ii) switching employees from higher-priced to lower-priced providers. As mentioned previously, the employees are a negligible fraction of any given health care market and as such, their gaining access to price information is very unlikely to have affected the price negotiations between providers

[18]The full results for all twenty weeks are presented in Appendix Table A.1.

and insurers. Thus, the observed impact of access to price information on transacted prices should be coming through employees switching to lower-priced providers. A subset of the American Medical Association's CPT billing codes indicate whether the patient was a new or established patient.[19] This effectively indicates whether or not the patient was seeing a new doctor.

For this subset of procedures, I test whether access to Compass increases the probability of seeing a new physician. In particular, I modify the reduced form specification in equation (1) by replacing the price outcome with an indicator for seeing a new physician, drop the fixed effects for procedure-market (they completely determine the outcome variable), and initially, drop the employee fixed effects as well. Employee fixed effects were removed due to the concern that employees who go to the doctor multiple times in a given year could be unrepresentative of the employees more generally. However, in practice, I show that specifications with and without employee fixed effects produce very similar results.

Marginal effects are presented Table 6. The first column suggests that access to Compass increases the probability of seeing a new physician by 2.7 percentage points.[20] Because only 17% of the visits are to new doctors, this is a 16% increase in the probability of seeing a new doctor. This baseline specification uses variation both across employees and within an employee over time. If the corporate office employees who went to the doctor after they had access to price information happened to live in markets where patients often switch physicians, then the results could be spurious. When market fixed effects are included, the point estimate changes very little and still implies a very large response to access to price information.

In column (3), employee fixed effects are included. This removes the possibility that

[19]This distinction is made on the codes that physicians use to bill for the time they spend with a patient. For example, on the AMA's website, https://ocm.ama-assn.org/OCM/CPTRelativeValueSearchResults.do?locality=3&keyword=99213, one can see that code 99213 is used for an "Office or other outpatient visit for the evaluation and management of an *established* patient ...." CPT code 99201 is for new patients: "Office or other outpatient visit for the evaluation and management of a *new* patient ..." (emphasis added).

[20]Estimating the regressions in Table 6 as linear probability models produces results that are extremely similar to those presented.

the particular employees who went to the doctor after gaining access to Compass were inherently more likely to see a new physician. Once the employee fixed effects are included, the identification comes from an employee who had multiple physician visits in a single year; at least one member of that employee's family saw a doctor prior to access while another (or the same) member of that employee's family saw a doctor after access to Compass. The point estimate increases slightly in magnitude. Overall, these estimates suggest that having access to price information affects which providers employees went to and provides supporting evidence for the price reductions found previously.

Lastly, it is important to consider how access to Compass has affected the quality of care the employees receive. If the price reductions come at the cost of lower quality care, then it is not clear that welfare will increase in the long run.[21] I merge Medicare's Hospital Compare quality measures onto these data to test whether access to price information affected the quality of hospital care the employees received. Specifically, I average each hospital's process of care measure for heart attacks, heart failure, pneumonia, and surgical patients to create a single quality index.[22] Once again using the variation in access to Compass, I estimate equation (1) where the average of the quality measures is the dependent variable.

As seen in the first column of Table 7, gaining access to price information does not appear to be strongly linked to the quality of care received. If the point estimate were correct, then it would suggest that gaining access to information actually increases the quality of care received, though only by a thirteenth of a standard deviation. Column (2) shows that the results do not change when I take the natural log of the dependent variable. We might think that employees just choose a hospital and not the amount of care they receive once at that hospital. In that case, there should only be one observation per employee-hospital. I use this

[21] Dranove and Satterthwaite (1992) show that if consumers only observe noisy signals of price and quality, an increase in the precision of price information can actually reduce consumer welfare in the long run. This result relies on a reduction in quality by producers that is large enough to offset the gains from lower prices.

[22] Physician specific measures of quality are not publicly available. I used the process of care measures that indicate the fraction of the time the hospital follows treatment guidelines for patients who present with the specified conditions. These measures have been shown to be correlated with actual outcomes by Peterson, Roe, Mulgund, DeLong, Lytle, Brindis, Smith, Pollack, Newby, Harrington, Gibler and Ohman (2006) and Shekelle (2007) among others.

restriction in column (3) and find similar results. Because the measures of quality are noisy at best (Doyle, Jr., Graves, Gruber and Kleiner 2015) and might not be measures relevant to the actual type of care received, these results on quality are merely suggestive. However, they do suggest that reduced prices are not coming at the expense of quality of care.

If the use of price transparency tools can reduce the prices paid by 10-17%, then why are the employees not calling for most of their care? One potentially important reason explored below is moral hazard in search.

5 Moral Hazard in Search

Generally, price dispersion gives consumers an incentive to search. Although there is considerable price dispersion in health care, health insurance insulates consumers from price differences and so could lead to less search. Dionne (1981) first discussed how this type of moral hazard is distinct from other forms of moral hazard (e.g. Pauly 1968, Ehrlich and Becker 1972). It was further studied theoretically (Dionne 1984, Akin and Platt 2014), but has received very little empirical attention because data on search are rare.

Empirical Strategy

I use variation based on differences in employees' marginal price for care on September 27th, 2010–the date the corporate office employees first gained access to Compass's price information. As seen in Table 8, in 2010, the employees had standard, nonlinear, preferred provider organization (PPO) insurance plans that included annual deductibles, cost-sharing provisions (coinsurance rates and copays), and out-of-pocket maximums to cap employees' total expenditure risk.[23] Because employees had consumed different amounts of care before the date they gained access to Compass, they were in different cost-sharing regions of their insurance plans. Approximately 31% of the corporate employees had met their deductibles

[23]The plans were administered by a single health insurer and had a single network of providers. The employees were also able to choose an HMO or EPO plan, but none did so.

by the date they gained access to Compass. On average, an employee's marginal price of care fell by 50% when their deductibles were met.

The data are at the employee by week unit of observation. They begin in September of 2010 when corporate employees gained access to Compass and extend through the first thirteen weeks of 2011. Initially, I only use data from 2010. The sample is restricted to corporate employees who received insurance through the company in both 2010 and 2011. Although the exact procedure the employee seeks price information for is not observed, the date she called for prices is. I estimate probits of the form

$$Pr(called_{it} = 1) = \Phi(met\ deductible\ by\ access_i \delta_1 + \lambda_t + X_{it}\gamma_1) \qquad (2)$$

where $called_{it}$ indicates whether employee i called Compass for price information in week t, Φ is the normal cumulative density function, $met\ deductible\ by\ access_i$ indicates whether the employee had met her deductible by the date she gained access to Compass's price information, λ_t is a set of week fixed effects, and X_{it} is a set of control variables that includes a cubic in cumulative spending on medical care for the employee up to date $t-1$ and demographic information based on the employee's five-digit zip code: per-capita income, gender, education levels, unemployment level, log of the population, and race. Standard errors are clustered by employee to allow for arbitrary autocorrelation patterns.

Table 9 presents summary stats for employees who had and those who had not met their deductibles by access. In the final months of 2010, employees who had met their deductibles were more likely to have a medical claim in a given week and to have a somewhat larger number of people covered by their insurance plan. Although those who had met their deductibles spent more on medical care on average, the median spending in both groups was zero. These differences are not surprising given that these employees had to use more care in order to hit their deductibles in the first place. However, aside from differences in the use of care, the employees who had met their deductibles are very similar to those who had not on other important demographic dimensions: income, education, and race.

Results

Column (1) of Table 10 reports the estimated marginal effects where only the demand controls and week fixed effects have been included. The results show that employees who had met the deductible were 1.5 percentage points less likely to search for price information in a given week. Relative to the average calling rate, this is a 90% difference. On average, meeting the deductible reduced the out-of-pocket price by 50%. Combining the estimates implies that the elasticity of the probability of search with respect to the fraction of the price consumers have to pay is approximately 1.8.

In the second column, controls for employee characteristics and demographics from the employee's five-digit zip code are included. The point estimate changes very little. To see these results graphically, I estimated the specification from column (2) without the indicator for having met the deductible by access, created residuals from that regression, and then plotted those residuals by whether the person had met her deductible or not. The average for each group in each week is plotted in Figure 1. We can see that in almost every single week of 2010, those who had met their deductibles by access were less likely to search.

Because the demand for medical care is likely related to both search and whether the person had met her deductible by the date of access, the remaining three columns of Table 10 include more flexible controls for previous medical spending to assess the sensitivity of the estimated marginal effect. Column (3) presents the results when a fifth order polynomial of past spending is included. Column (4) breaks previous spending into $200 bins and includes those bins. In these specifications, there is potential for search to feed back into the demand controls because search in the first week of access could have impacts on the demand controls in the later weeks of 2010. Column (5) shuts down this concern by including a third order polynomial of cumulative medical spending up to the date of access to Compass. This measure of demand for care does not vary over time for an individual and is completely determined before the employees had access to Compass. The estimated impacts change very little across all of these specifications.

As an additional robustness test, I restrict the sample to those who are within $200 of the deductible threshold and test whether those just above the threshold are less likely to search for price information than those just below the threshold. To the extent that employees are forward looking in their health care consumption, the difference in search behavior between those just above and below the threshold will be attenuated. The results are presented in Appendix C. The estimated marginal effects are consistent with those found in Table 10, but are estimated with very little precision.

Because the employees' deductibles are reset on January 1st each year, whether an employee met her deductible in 2010 does not directly change her marginal price for care at the beginning of 2011. On the other hand, if there were some unobserved characteristic that makes employees more likely to have met their deductibles in 2010 and simultaneously less likely to call for price information, regressing search behavior in 2011 on the 2010 $met\ deductible\ by\ access_i$ variable should produce estimates similar to those in Table 10.[24] Although the set of insurance plans available to the employees changed with the beginning of the new year, week fixed effects absorb the average impact of this event on employees who had and had not met their deductibles in 2010.

I re-estimate equation (2) using the first 13 weeks of 2011 (the same amount of time used in the analysis of 2010 data). The results from this exercise are presented in Table 11. In each specification, the point estimate is very small, positive, and nowhere near statistically significant.[25] These same results can be seen in Figure 1. Starting in January, 2011, there

[24] There is evidence that consumers are forward looking and their current consumption of medical care is affected by their marginal price of care later in the year (Aron-Dine et al. 2012). If demand for care is positively correlated across time, then those who met their deductibles in 2010 would perceive a lower marginal price for care at the start of 2011 and search less. As long as the person is not perfectly forward looking or if there is uncertainty about future medical needs, the perceived marginal price for care should be greater than the actual marginal price for care obtained when the deductible is met, so the magnitude of the search reduction should be smaller in 2011 than in 2010.

[25] The number of observations does not exactly match that from the analysis in 2010 because 16 employees left their jobs in week 10 of the new year.

does not appear to be a systematic relationship between an employee's 2010 deductible status and her 2011 search.

6 Conclusion

There are huge information gaps in the market for health care, but these are shrinking as governments, insurers, and private companies begin to provide price information. However, it is not clear that providing information will affect outcomes.

I use a unique dataset with a direct measure of search to show that search does affect prices. I find that search reduces the price paid for care by 10% - 17%. These price reductions were concentrated in types of care that were easier to plan for in advance and for employees who had greater incentives to search. In addition, I find evidence that the mechanism through which prices were reduced was employees' choice of providers. Once employees gained access to price information, they became much more likely to visit a provider they had not seen previously. Although searching appears to reduce the prices paid considerably, a relatively small amount of search occurs. I provide suggestive evidence that more generous insurance coverage leads to less search: employees who faced a lower marginal price of care on the date they gained access to Compass searched less during the remainder of the year. The results suggest that search is quite responsive to coverage; the estimated elasticity of search with respect to out-of-pocket price is 1.8. Taken together, the results suggest that access to price information could have large impacts in the market for health care, but that considering consumers' incentives to search is of primary importance.

There are important limitations to the findings. Because they are based upon the employees at a single firm that chose to hire Compass, there are concerns about external validity. The mechanism through which access to information and search can affect prices is also limited in my empirical work. In particular, I am not able to observe any general equilibrium changes to prices from impacts on insurer-provider bargaining, increased competition between providers, or other supply side reactions to the availability of price information and the incentives to use it. And lastly, it is not clear that reduced expenditures translate directly into consumer welfare gains because lower prices might come at the cost of lower quality.

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Table 1: Price Dispersion for Mammograms

	CIGNA	Harvard Pilgrim	BCBS
Dartmouth Hitchcock South	202	340	328
Elliot Hospital	259	317	310
Derry Imaging Center	263	334	330
St. Joseph Hospital	279	225	358
Southern NH Radiology Consultants, PC	283	275	251
Catholic Medical Center	323	513	438
Concord Hospital	369	882	355
Southern NH Medical Center	369	356	419
Parkland Medical Center	496	477	470

Prices, in dollars, for a mammogram by provider. Data publicly available at New Hampshire HealthCost website. Prices for patients on a PPO plan with the specified insurer. BCBS is Blue Cross & Blue Shield. All providers are within a 20 mile radius of zip code 03101 (located in most populous city in New Hampshire).

Table 2: Summary Statistics for Employees

Search and demographic data		
	Corporate	Non-corporate
Called for price information in 2010	0.120	-
	(0.325)	-
Median household income	$63,966	$48,225
	(19,651)	(16,407)
Fraction with college or more	0.396	0.268
	(0.159)	(0.138)
Fraction white	0.795	0.776
	(0.138)	(0.170)
Medical claims data		
	Corporate	Non-corporate
Price	$146	$142
	(1081)	(1245)
90th-50th percent difference in price	36	34
	(21)	(22)
Fraction claims billed by		
Physician	0.852	0.820
	(0.355)	(0.384)
Hospital inpatient	0.004	0.007
	(0.067)	(0.084)
Hospital outpatient	0.064	0.094
	(0.244)	(0.292)
Other	0.080	0.079
	(0.271)	(0.269)

Means and standard deviations are reported for the sample. Called for price information in 2010 indicates fraction of employees who contacted Compass at some point when they had access (in 2010). Top panel for 644 corporate employees and 5564 non-corporate employees. Bottom panel based on 89,575 corporate procedures and 298,199 non-corporate procedures. Type of care is determined using the American Medical Association's CPT codes. 90th-50th percent difference is averaged across procedure by market specific price distributions.

Table 3: Effect of Access to Price Information on Prices Paid

			Separate effects for		
	Baseline	Winsorized	Emergency care	Met deductible	Number of procedures
	(1)	(2)	(3)	(4)	(5)
Post10 * corporate employee	-0.016***	-0.014***			
	(0.004)	(0.003)			
Implied estimate for:					
Non-emergency care			-0.016***		
			(0.004)		
Emergency care			-0.005		
			(0.018)		
Not met deductible				-0.019***	
				(0.004)	
Met deductible				-0.009	
				(0.006)	
Below 20 procedures on day					-0.016***
					(0.004)
Above 20 procedures on day					-0.006
					(0.022)
Adjusted R-squared	0.925	0.947	0.925	0.925	0.925
N	387,774	387,774	387,774	387,774	387,774

Dependent variable is Ln(price). Regressions include week-year, employee, and market-procedure fixed effects, and indicators for whether employee had fulfilled deductible. Columns 3 - 5 are baseline specification where DD estimator interacted with specified indicator. Emergency care indicates employee received emergency care that day. Below 20 procedures on day indicates employee had fewer than 20 procedures on day of the claim. Standard errors clustered by market. * $p<.10$, ** $p<.05$ *** $p<.01$

Table 4: Access to Price Information and Prices Paid: Pretrend Tests

	Baseline (1)	Add corporate linear trend (2)	Pre-period linear trends (3)	Pre-period dummies (4)
Post10 * corporate employee	-0.016***	-0.011**	-0.023***	-0.017***
	(0.004)	(0.005)	(0.005)	(0.004)
Linear trend * corporate emp.		-0.0001		
		(0.0001)		
Linear pretrends * corp. emp.				
2009			-0.0002	
			(0.0002)	
2010			-0.0003	
			(0.0002)	
Weeks preceding access				
1				0.023
				(0.025)
2				-0.005
				(0.013)
3				-0.030*
				(0.018)
4				-0.011
				(0.010)
5				0.009
				(0.013)
Adjusted R-squared	0.925	0.925	0.925	0.925
N	387,774	387,774	387,774	387,774

Dependent variable is Ln(price). Column (2) adds linear trend interacted with corporate employee. Column (3) adds linear trends interacted with corporate employee for specified time periods (and equal to zero outside of those time periods). Column (4) includes indicators for the 20 weeks before corporate access interacted with indicator for corporate employees. All regressions include fixed effects for the week-year, employee, and market-procedure, and indicators for whether employee had fulfilled deductible. Standard errors clustered by market. * p<.10, ** p<.05, *** p<.01

Table 5: Impact of Search on Prices Paid

	One month (1)	One month or previous call (2)	Everything after first call (3)
Searched	-0.167***	-0.137***	-0.101***
	(0.044)	(0.035)	(0.026)
F-stat, first stage	37.12	40.59	44.51
N	387,774	387,774	387,774

Instrumental variables results. Dependent variable is Ln(price). First column is within 30 days; second column also counts procedures previously called about (according to 30 day measure); third column counts all procedures after an employee's first call. All regressions include fixed effects for the week-year, employee, and market-procedure, and indicators for whether employee had fulfilled deductible. Standard errors clustered by market. * p<.10, ** p<.05, *** p<.01.

Table 6: Impact of Access on Seeing a New Doctor

	Baseline (1)	Add market f.e. (2)	Add employee f.e. (3)
Post10 * corporate employee	0.027***	0.025***	0.026***
	(0.008)	(0.008)	(0.008)
CBSA fixed effects		x	
Employee fixed effects			x
Mean dependent variable	0.169	0.169	0.169
Pseudo R-squared	0.008	0.021	0.189
N	63,704	63,704	63,704

Dependent variable is whether patient is seeing a new doctor. Marginal effects reported. Data only include office visits. All columns include indicators for type of setting where procedure performed, demographics, and week fixed effects. Standard errors clustered by market. * p<.10, ** p<.05, *** p<.01.

Table 7: Impact of Access on Quality of Hospital Care

	Baseline	D.V. in logs	One observation per visit
	(1)	(2)	(3)
Post10 * corporate employee	0.200	0.002	0.101
	(0.174)	(0.002)	(0.324)
Mean of dependent variable	97.072		96.964
Std. dev. of dependent variable	2.358		2.488
Adjusted R-squared	0.854	0.834	0.875
N	23,123	23,123	8,415

Dependent variable is quality measure. All regressions include indicators for type of procedure, whether employee had fulfilled deductible yet, and fixed effects for the week-year fixed, employee, and market-procedure. Column (3) only uses one observation per hospital visit. Standard errors clustered by market. * p<.10, ** p<.05 *** p<.01

Table 8: Main Features of Insurance Plan Options

	High	Low
Deductible	\$600	\$1,250
Doctor visit	\$30 copay	\$30 copay
Hospital visit	20 % after deductible	20 % after deductible
Out-of-pocket maximum	\$2,000	\$5,000

Structure of PPO insurance plans offered to employees in 2010. High column indicates option with greater coverage. In-network amounts are listed. Out-of-network deductibles and maximums double, coinsurance rate 40 % instead of 20 %. \$150 copay for Emergency visits in 2010 only.

Table 9: Summary Statistics for Corporate Employees

	Met deductible by access	Not met deductible by access
Had a medical claim	0.45	0.28
Health spending	$359	$172
Number covered per employee	3.3	2.6
Per-capita income	$64,006	$63,090
Fraction with college or more	0.39	0.40
Fraction male	0.49	0.49
Fraction white	0.81	0.78
N	2,275	5,070

Unit of observation is employee by week for weeks of 2010 when corporate employees had access to Compass. Statistics based on 565 corporate employees insured through company in 2010 and 2011. Had medical claim indicates someone on employees' plan had a claim in a given week. Health spending is average spending per week. Number covered is unique individuals with a claim per employee. Per-capita income, education, gender, and race variables based on 5-digit zip code demographics.

Table 10: Deductible Status and Subsequent Use of Price Information

	Baseline (1)	Age, Family, and Zip-code demos (2)	5th order demand controls (3)	Demand controls bins (4)	Demand control pre-access (5)
Met deductible	-0.015**	-0.016***	-0.015**	-0.012**	-0.016**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Week f.e.	x	x	x	x	x
Demand controls	x	x	x	x	x
Age and family size		x	x	x	x
5-digit zip demographics		x	x	x	x
Mean of dependent variable	0.016	0.016	0.016	0.016	0.016
Pseudo R-squared	0.050	0.084	0.087	0.106	0.078
N	7,345	7,345	7,345	7,345	7,345

Dependent variable is whether employee sought price information in a given week in 2010. Only periods in which employees had access to Compass are included. Met deductible indicates employee had met deductible on her insurance plan by the week she gained access to Compass. Week fixed effects included. Demand controls is a cubic in cumulative medical spending up to the previous week. Age and family size includes age, age-squared, and number of people in employee's family covered by the insurance contract. Demographics from the employee's 5-digit zip code described in paper. Demand controls bins breaks previous spending into $200 bins and includes dummies for each bin. Demand control pre-access uses cumulative medical spending by the employee in 2010 up to the date she gains access to Compass. A third order polynomial in that measure is included. Standard errors clustered by employee. * p<.10, ** p<.05, *** p<.01.

Table 11: Deductible Status and Use of Price Information in the Next Year

	Base	Individual demos	Zip-code demos
	(1)	(2)	(3)
Met deductible	0.002	0.002	0.004
	(0.007)	(0.006)	(0.006)
Week f.e.	x	x	x
Demand controls	x	x	x
Age and family size		x	x
5-digit zip demographics			x
Mean of dependent variable	0.020	0.020	0.020
Pseudo R-squared	0.001	0.012	0.015
N	7,281	7,281	7,281

Dependent variable is whether employee sought price information in a given week in 2011. Only weeks in which employees had access to Compass are included. Met deductible indicates that the employee had met the deductible on her insurance plan by the week she gained access to Compass. Deductibles were reset on January 1, 2011. Demand controls are a cubic in the cumulative cost of her care up to the previous week. Age and family size includes variables for age, age-squared, and the number of people in the employee's family covered by the insurance contract. The demographics from the employee's 5-digit zip code are described in the paper. Standard errors clustered by employee. * p<.10, ** p<.05, *** p<.01.

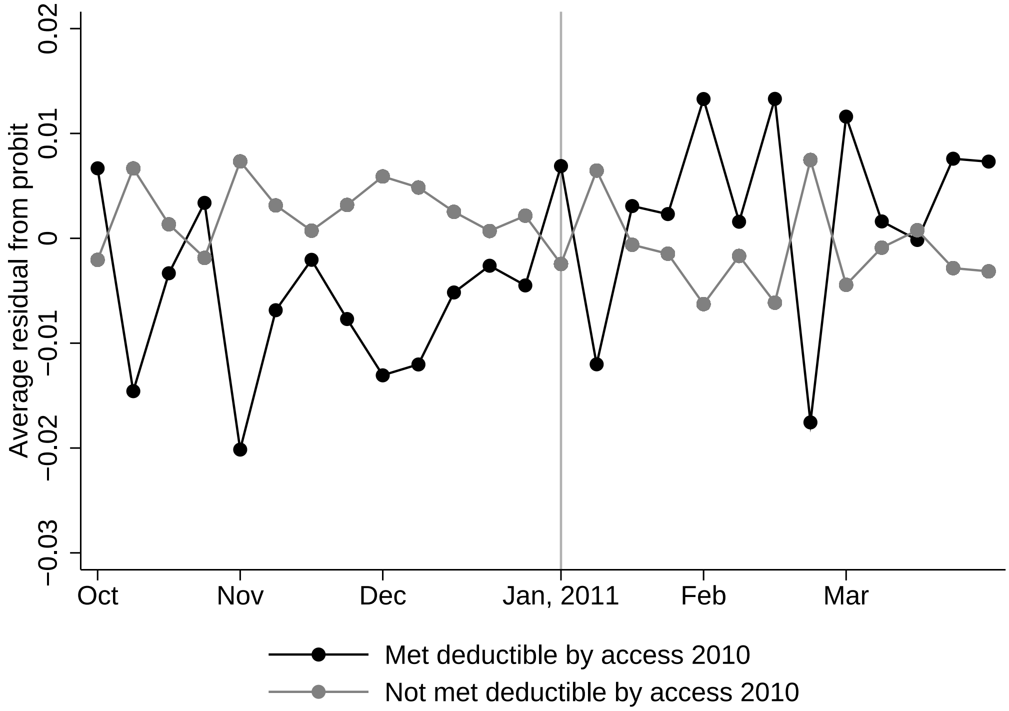

Figure 1: Deductible Status at Access and Search

Online Appendix

A Additional Tables

Table A.1: Testing for Differential Pretrend of Corporate Employees

	Baseline	Pre-period dummies
Post * corporate employee	-0.016***	-0.017***
	(0.004)	(0.004)
Corporate * weeks preceding access: 1		0.023
		(0.025)
2		-0.005
		(0.013)
3		-0.030*
		(0.018)
4		-0.011
		(0.010)
5		0.009
		(0.013)
6		-0.018
		(0.023)
7		-0.023
		(0.015)
8		-0.039*
		(0.022)
9		-0.001
		(0.014)
10		-0.020
		(0.022)
11		0.027
		(0.016)
12		-0.033**
		(0.016)
13		-0.019
		(0.023)
14		0.024
		(0.024)
15		-0.011
		(0.027)
16		0.033
		(0.025)
17		0.002
		(0.017)
18		0.020
		(0.024)
19		-0.012
		(0.036)
20		-0.002
		(0.017)

Dependent variable Ln(price). Column includes indicators for 20 weeks before corporate access interacted with corporate indicator. See Table 4 for listing of controls.. Standard errors clustered by market. * p<.10, ** p<.05, *** p<.01

B First Stage Results

In this appendix, I present the first stage results that show how access to Compass affected the probability of receiving price information about a particular procedure. The estimating equation is

$$called_{ijmt} = (post10_t * corporate\ employee_i)\beta_1 + Z_c\gamma + \lambda_w + \lambda_{jm} + \lambda_i + \varepsilon_{ijmt} \qquad (3)$$

where $called_{ijmt}$ indicates whether employee i had sought price information for procedure j in market m at time t. As described in the main text, $called_{ijmt}$ is not directly observable and I use three different ways to map observed calls into it.

The first stage estimates are presented below in Table B.1. As seen in column (1), when the first definition of $called_{ijmt}$ is used, access increases the probability of having price information about a particular procedure by just over 9 percentage points. When it is assumed that employees do not forget the price information they were previously given (column (2)), the impact of access on price information increases slightly. And when the extreme assumption is made that employees receive information about every procedure they have after their first call, the impact of access on procedures searched for increases to 15.3%. In each case, the first stage F-statistic is larger than 37.

Table B.1: The Impact of Access on Searching for Price Information

	One month (1)	One month or previous call (2)	Everything after first call (3)
Post * corporate employee	0.093***	0.113***	0.153***
	(0.015)	(0.018)	(0.023)
F-stat	37.12	40.59	44.51
Adjusted R-squared	0.998	0.998	0.998
N	387,774	387,774	387,774

Dependent variable whether person called within specified time period of getting care. Column (1) within 30 days; column (2) also counts procedures previously called about; column (3) counts all procedures after an employee's first call. Regressions include week-year, employee, and market-procedure fixed effects, and indicator for whether employee had fulfilled deductible. Standard errors clustered by market. * p<.10, ** p<.05, *** p<0.01.

C Search Near the Deductible Threshold

In this appendix, I present additional evidence on the impact of health insurance coverage on search. In particular, I restrict the sample to employees who have access to Compass and are within \$200 of the deductible threshold. I estimate whether those just below the deductible threshold are more likely to search than those who are just above it. To the extent that the employees are forward-looking in their consumption of health care, this comparison will tend to understate the association between the marginal price for care and search behavior. In the raw data, those just above the threshold searched in 1.4% of the weeks while those just below the threshold searched in 2.7% of the weeks.

As seen in column (1) of Appendix Table C.1, a simple probit regression of calling on search closely reproduces this raw difference in means. Unfortunately, there is very little statistical power to distinguish the marginal effect from the null hypothesis. The point estimate itself suggests employees who had met their deductibles were 70% less likely to search in a given week than those who had not met their deductibles. Although this difference is consistent with the associations seen in the main text, it could be driven by omitted time trends: as weeks pass, more people are likely to have met their deductibles. Column (2) includes week fixed effects that account for this possibility. The point estimate falls slightly, but remains economically large. In the final column, controls for the demand for care are included. As in the main text, this is a cubic in cumulative medical spending up to the previous week. There is little difference in the estimated marginal effects between columns (2) and (3). Although these estimated impacts of meeting the deductible on search are very imprecisely estimated, they are consistent with the results presented in the main text and suggest a role for moral hazard in search.

Table C.1: Search and Meeting the Deductible

	Baseline	Add week f.e.	Add demand control
	(1)	(2)	(3)
Met deductible	-0.011	-0.009	-0.008
	(0.012)	(0.011)	(0.010)
Week fixed effects		x	x
Demand controls			x
Mean dependent variable	0.017	0.017	0.017
Pseudo R-squared	0.011	0.171	0.197
Observations	479	479	479

Marginal effects from probit regressions presented. Sample restricted to corporate employees in 2010 within $200 of deductible threshold. Demand controls are a cubic in the cumulative medical spending up to the previous week. Standard errors clustered by employee. * p<.10, ** p<.05, *** p<.01.