

Does It Pay to Know Prices in Health Care?[†]

By ETHAN M.J. LIEBER*

Consumers rarely know the price of medical care before they consume it. I use variation in the timing of access to a new source of price information to show how access to and search for price information leads consumers to pay significantly less for care. I provide suggestive evidence that insurance coverage inhibits the use of price information, rationalizing the relatively low rates of search. The results indicate that availability of price information could have large impacts on prices even in the absence of general equilibrium effects. (JEL D82, D83, G22, I11, I13)

Health care spending accounts for 18 percent 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 of public policy as well as the private sector. Unlike most markets, consumers know very little about prices in health care. Research in other markets has found that increasing price transparency reduces prices (e.g., Brown and Goolsbee 2002; Goldmanis et al. 2010), but it is not clear the same will be true in health care: health insurance insulates patients from prices, reducing the benefits of switching to a lower priced provider.

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 the employees of a large firm gain 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 so directly measures search.

I begin by estimating how access to price information affects the prices paid for care. A subset of the employees were given access to price information in a pilot program to determine whether it was worth purchasing for all of the employees. The employees who gained access early did not sign up, volunteer, or select into the pilot program in any way; they were given access early because they worked for the company's corporate offices. I estimate a differences-in-differences model that takes advantage of this variation in access both across groups and over time. The results suggest that access to price information reduces the average price paid by

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1.6 percent. The effect is smaller for employees who had less incentive to search and the impact is concentrated among care that is more amenable to search, e.g., non-primary care, less complex care, and nonemergency care.

The identifying assumption is that absent access, the corporate and noncorporate employees would have experienced the same changes in prices over time. I do not find any evidence that those who received access earlier were on different trends than those who received access later. In addition, placebo tests that assign false dates of access do not produce results similar to the actual date of access.

These estimated price reductions can come from many sources. On the demand side, the employees might switch to lower priced providers. They might forego care when they learn that prices are higher than anticipated and obtain care when prices are lower than anticipated. On the supply side, providers might lower prices in response to increased consumer search. In the data, I find evidence that access significantly increases the probability of seeing a new provider. 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. On the supply side, it is unlikely that there was a response because the employees in my data are a negligible fraction of any given health care market.

To estimate how search itself affects prices, I pursue an instrumental variables strategy in which I instrument for search with access to price information. In the first stage, access increases search by between 9 percent and 15 percent. The IV strategy estimates that search reduces prices by 10–17 percent. Although large, the estimates are plausible. There is a tremendous amount of price dispersion in health care (Ginsburg 2010). In my data, moving from the ninetieth percentile of the price distribution to the fiftieth percentile reduces the price by 35 percent.

If search reduces prices paid by 10–17 percent, then why are consumers searching so infrequently? A prominent, yet largely untested, explanation is moral hazard in search (Dionne 1981, 1984; Akin and Platt 2014). Health insurance reduces consumers' exposure to price differences and thereby reduces their incentives to search. I provide 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 percent 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 percent less likely to subsequently search.¹ Based on these estimates, the elasticity of search with respect to the out-of-pocket price is approximately 1.8. This evidence suggests that moral hazard in search could play an important role in health care.

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

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 et al. 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.

price information, it becomes difficult to generalize my results to the population at large. Second, my results only reflect a short-run, partial equilibrium response and not 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 et al. (2014) use data from a different price information supplier to compare the prices that searchers and nonsearchers pay. They find that searching is associated with a 13–14 percent 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 nonsearchers) 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 price information reduce charge prices for hip replacements. They find that charges for hip replacements fell by 7 percent 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 percent of privately insured individuals had high-deductible health plans that would have given them an incentive to search (Claxton, et al. 2014). However, as the fraction of consumers in these plans continues to rise, from 4 percent to 20 percent 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 et al. 2006; Dixon, Green, and Hibbard 2008; Haviland et al. 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 I provides background information on pricing in health care and price information firms like Compass.

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

Section II describes the data. Section III presents the empirical strategies and results used to investigate the impacts of access to price information and search on prices paid. Section IV presents the empirical strategy and results for the analysis of moral hazard in search. Section V concludes.

I. 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. 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.³ As seen in Table 1, prices vary considerably for a mammogram, a routine and relatively homogeneous procedure.⁴ 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 eight separate markets, most of them major metropolitan areas. On average, the median payment rate for inpatient care is 47 percent lower than the maximum payment. In the large claims database I use (discussed in Section II), for a given geographic market and narrowly defined procedure, moving someone from the ninetieth percentile of the price distribution down to the median reduces the price by 35 percent on average.

Despite the large amount of dispersion, prices negotiated between insurers and providers are generally not publicly known (Stockwell Farrell et al. 2010; United States Government Accountability Office 2011; Painter and Chernew 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 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, Floyd, and Maffett 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 toward greater price transparency.

How do consumers search with price information firms? 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

³There 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 2006; Tappata 2009). There is also a growing literature in insurance choice and frictions in Medicare Part D (e.g., Abaluck and Gruber 2011; Miller 2014; Ho, Hogan, and Scott Morton 2015; Polyakova 2015).

⁴These are not the widely available charge data, but the actual prices negotiated 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.

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

Notes: 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 and Blue Shield. All providers are within a 20 mile radius of zip code 03101 (located in the most populous city in New Hampshire).

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.⁵

In conjunction with the increase in transparency, consumers are facing greater health care costs. Between 2004 and 2014, worker contributions for insurance premiums have risen approximately 45–53 percent in real terms, from \$278 per month to \$402.⁶ In addition to higher premiums, consumers are experiencing less generous cost-sharing. Between 2006 and 2014, the share of covered workers in high-deductible health plans rose from 4 percent to 20 percent (Claxton et al. 2014). A number of studies, (e.g., Parente, Feldman, and Christianson 2004; Buntin, 2006; Dixon, Greene, and Hibbard 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 are simply purchasing less care as in Brot-Goldberg et al. (2015).

II. Data

The data come from one of Compass's large corporate clients.⁷ 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

⁵This 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.

⁶Reported figures are for family coverage in real 2014 dollars. Single coverage has risen from \$59 per month to \$90 per month over the same time frame, 2004–2014.

⁷The identity of the client must remain anonymous due to a nondisclosure agreement.

about the insurance plans from which the employees chose. Corporate office employees gained access to Compass on September 27, 2010; noncorporate employees gained access at the start of 2011.

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.⁸ 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 claims include information about the 387,774 distinct procedures: exactly what procedure was performed (using the American Medical Association's CPT billing codes), the employee who used the care (including family members covered by the employee's health insurance 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 70 standard deviations above the rest of the sample.⁹

The top panel of Table 2 shows that in the final three months of 2010, 12 percent of the corporate employees searched for price information at least once in that time period. For 2010, the employees chose between the two insurance plans described in Table 3. In both the corporate and noncorporate groups, approximately half of the employees chose the more generous insurance plan. This plan had a deductible of \$600 compared to a deductible of \$1,250 for the less generous plan. The demographic information presented in the next five rows of Table 2 indicates that corporate office employees were slightly older, had larger families, and lived in slightly higher socioeconomic status zip codes than noncorporate employees.¹⁰

The bottom panel of Table 2 provides information on employees' care use. The mean price of procedures obtained by corporate employees was \$146; for noncorporate employees, the mean price was \$142. There is a large amount of dispersion around the mean price as well. For a given market, procedure, and setting, the ninetieth percentile of the price distribution is 35 percent larger on average than the median. Figure 1 shows kernel density estimates of the price distributions

⁸The specific categories included are contacts classified as about prices, prices and quality, scheduling appointments, coordination of care, and care road map. More than 88 percent 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.

⁹Including 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.

¹⁰Because 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 percent, and that 78 percent 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).

TABLE 2—SUMMARY STATISTICS FOR EMPLOYEES

	Corporate	Noncorporate
<i>Panel A. Search and demographic data</i>		
Called for price information in 2010	0.12 (0.33)	— —
Chose generous insurance plan	0.50 (0.50)	0.50 (0.49)
Number of people covered by plan	2.82 (1.43)	2.01 (1.42)
Age	42.69 (8.01)	38.89 (8.46)
Median household income	\$63,966 (19,651)	\$48,225 (16,407)
Fraction with college or more	0.40 (0.16)	0.27 (0.14)
Fraction white	0.80 (0.14)	0.78 (0.17)
<i>Panel B. Medical claims data</i>		
Price	\$146 (1,081)	\$142 (1,245)
Per person health spending, pre-access 2010, conditional on positive spending	\$3,865 (21,085)	\$3,774 (22,269)
Per person health spending, pre-access 2010	\$3,667 (20,555)	\$2,194 (6,340)
Fraction met deductible, pre-access 2010, conditional on positive spending	0.32 (0.47)	0.29 (0.44)

Notes: Means and standard deviations reported. 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 5,564 noncorporate employees. Bottom panel based on 89,575 corporate procedures and 298,199 noncorporate procedures. “Per person” indicates variable averaged over those covered by employee’s insurance plan. Type of care determined using American Medical Association’s CPT codes.

for care received between January 1, 2010 and the date the corporate employees gained access to Compass. More precisely, I regress the natural log of the price on market-procedure-setting fixed effects and plot the kernel density estimates of the residuals. As seen in the figure, not only the means, but the entire distributions of prices received by corporate and noncorporate employees are similar. As discussed in Section III, the similarity of prices across the groups plays an important role in the empirical strategy.

In addition to receiving similar prices, corporate and noncorporate employees who obtain a positive amount of care receive similar amounts of care. Table 2 presents health expenditures per person covered by the employee’s insurance. Corporate employees who had received care before access consumed \$3,865 on average. During that same time period, noncorporate employees who had received care consumed only slightly less, \$3,774. Figure 2 shows the distributions of health care expenditures for corporate and noncorporate employees who had received care. The corporate employees spent slightly more than the noncorporate employees. Although the distributions are comparable, noncorporate employees were considerably less likely to have consumed any care. Overall, noncorporate employees spent only 60 percent

TABLE 3—MAIN FEATURES OF INSURANCE PLAN OPTIONS

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

Notes: 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 percent instead of 20 percent. \$150 co-pay for emergency visits in 2010 only.

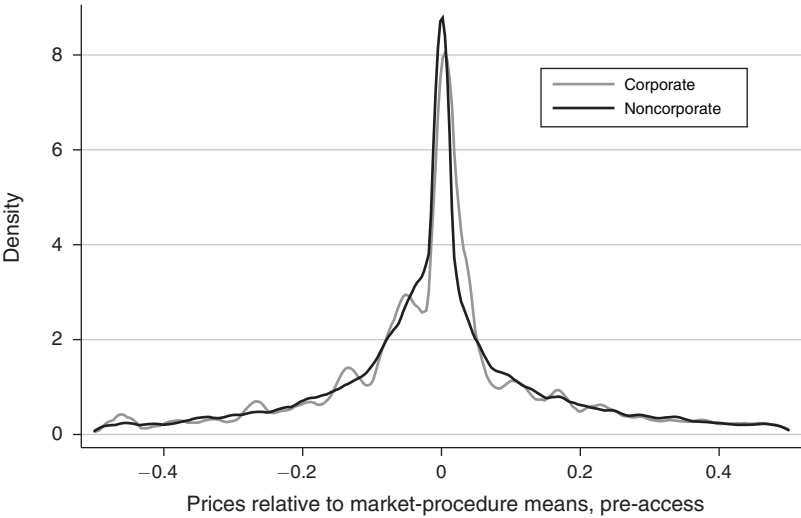


FIGURE 1. DISTRIBUTION OF PRICES BEFORE SEPTEMBER 27, 2010

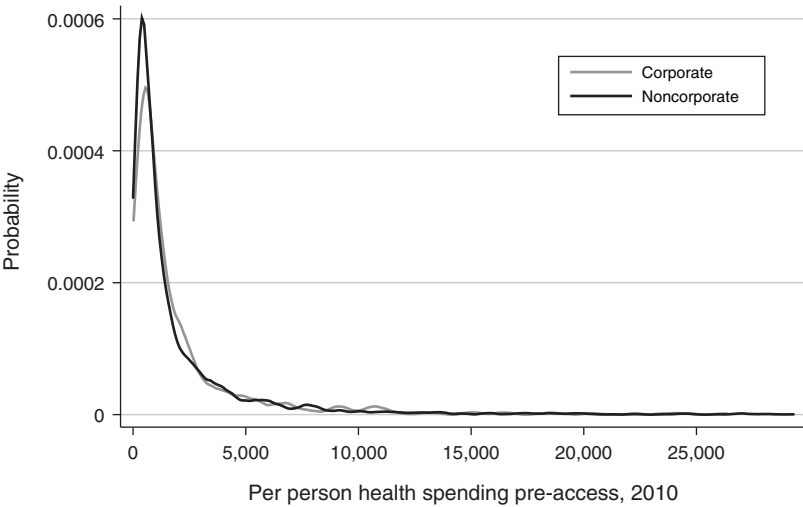


FIGURE 2. DISTRIBUTION OF EXPENDITURES BEFORE SEPTEMBER 27, 2010

of what the average corporate employees spent. This difference raises some concern that all noncorporate employees might not be adequate controls for the behavior of corporate employees and motivates the use of the matching approach taken in parts of the empirical analysis.

III. Access, Search, and Prices Paid for Care

A. Empirical Strategy

On September 27, 2010, the corporate office employees gained access to Compass. This was a pilot program to determine whether the company should hire Compass for all of its employees. The employees were not asked to volunteer, they were simply given access if they worked for the corporate offices. In addition, they did not know that they would be receiving access, so there is little scope for their pre-access behavior to have been affected. On January 1, 2011, the noncorporate employees were also given access to Compass.

I take advantage of this difference in timing to estimate how access to price information affects prices paid for care. Using the claims data from 2009 and 2010, I estimate the differences-in-differences specification

$$(1) \quad \ln(\text{price}_{ijmt}) = (\text{post}_t \times \text{corporate employee}_i) \beta_1 + Z_c \gamma \\ + \lambda_w + \lambda_{jm} + \lambda_i + \varepsilon_{ijmt}.$$

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-of-pocket reduction. $\text{post}_t \times \text{corporate employee}_i$ is the differences-in-differences variable; Z_c is an indicator for whether the employee had hit the coinsurance portion of her insurance plan; λ_w are week-by-year fixed effects that remove any trends or seasonality in prices; λ_{jm} are market-by-procedure-by-setting fixed effects (settings are hospital inpatient, hospital outpatient, doctor's office, global imaging facility, or other imaging facility); λ_i are employee fixed effects; and ε is an error term.¹¹ The main effects for post_t and $\text{corporate employee}_i$ 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 (CBSA) as the market when giving information to its clients and that convention is followed in this analysis.¹² 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 noncorporate employees would have experienced the same percentage change in prices after September 27, 2010 had neither group been given access to Compass. Even if there

¹¹ For the full sample, there are 104 week-by-year fixed effects, 5,580 employee fixed effects, and 68,876 market-procedure-setting fixed effects.

¹² Core-Based Statistical Areas are Metro and Micropolitan Statistical Areas defined by the Office of Management and Budget.

are systematic differences in the amount of care used, it is the trend in prices that is critical to the empirical analysis. I run pre-trend tests to show that corporate office employees were not on different price trends prior to access and placebo tests to show that counterfactual dates of access do not produce similar estimates. These tests lend credence to the internal validity of the estimates, but their external validity is less clear. For example, my sample of employees is somewhat more educated than the general population.¹³ If education is associated with the returns to having price information, then my estimates will overstate the impacts of price information for the population as a whole.

It is plausible that access leads to lower prices because employees are using the price information, i.e., searching. This suggests estimating

$$(2) \quad \ln(\text{price}_{ijmt}) = \text{searched}_{ijmt}\beta_1 + Z_c\gamma + \lambda_w + \lambda_{jm} + \lambda_i + \varepsilon_{ijmt},$$

where price_{ijmt} is the price that employee i paid for procedure j in market m at time t , searched_{ijmt} indicates whether the employee searched for price information for that procedure, Z_c is an indicator for the employee having met her deductible, λ_w are week fixed effects, λ_{jm} are market-procedure-setting fixed effects, and λ_i are employee fixed effects. However, search_{ijmt} is likely to be correlated with omitted variables that affect the price of care (e.g., previous knowledge of prices). Instead of estimating equation (2) directly, I instrument for whether an employee searched with whether the employee had access to price information. The first stage is given by

$$(3) \quad \text{searched}_{ijmt} = (\text{post}_t \times \text{corporate employee}_i)\beta_1^1 + Z_c\gamma^1 + \lambda_w^1 + \lambda_{jm}^1 + \lambda_i^1 + \varepsilon_{ijmt}^1.$$

Because I use the 2009 and 2010 claims data in the IV analysis, access to Compass is the differences-in-differences variable used previously: $\text{post}_t \times \text{corporate employee}_i$.

As mentioned before, I observe the date that someone on the employee's health plan searched, not the procedures for which they searched. This creates two ambiguities. First, 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 30 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 types of medical care and actually being able to consume it (Coyte et al. 1994; Bell et al. 1998).¹⁴ The 30-day window allows enough time for the employee to have received the care she obtained price information for without being

¹³ In its Digest of Education Statistics, The National Center for Education Statistics reports that approximately one-third of Americans between 25 and 64 years old had a bachelor's degree or higher in 2012. However, 40 percent of the corporate employees in my sample (those who are able to search) have a bachelor's degree or more.

¹⁴ 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 2 weeks and 25 days. The mean wait time was 3.2 weeks while the ninetieth percentile of the distribution was 4 weeks (Coyte et al. 1994).

overly inclusive. Second, I use the same 30-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 count 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.

The second ambiguity is related to who called for price information. When an employee's health plan covers multiple individuals, I cannot observe exactly which person called. Although this may appear problematic, it is not obvious that the person who makes the call will be the same person who receives care (especially in the case of children) and it seems likely that the family will share the information it receives. When using the mappings described above, I assume that a call for price information applies to care received by any person covered by the employee's health plan. To the extent that price information is not shared within the family, I will tend to overcount the amount of care for which individuals have price information. As with the third method of assigning calls to procedures, this will tend to understate the impact of search on prices in the IV analysis.

B. Results for Access and Prices Paid

The results from estimating equation (1) are presented in Table 4. If interpreted causally, the baseline estimate implies that gaining access to Compass reduced prices paid by 1.6 percent on average. 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 percent of observations and 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; Painter and Chernew 2012). Primary care might be less amenable to search because patients have built relationships with their providers. These relationships could make them less price sensitive for this type of care. To assess this possibility, I interact the differences-in-differences variable with an indicator for whether or not the employee received only primary care on the day of the procedure.¹⁵ As seen in column 3, for non-primary care, the estimated impact of access is the same as the baseline. However, the impact of access for primary care is closer to zero and statistically distinguishable from the impact on non-primary care. Combining the coefficients suggests that the impact of access on prices for primary care is a statistically insignificant 0.008.

¹⁵Specifically, I estimate

$$\ln(\text{price}_{ijmt}) = (\text{post}_t \times \text{corporate employee}_i) \beta_1 + (\text{post}_t \times \text{corporate employee}_i \times \text{primary only}_{it}) \beta_2 \\ + \text{primary only}_{it} \beta_3 + Z_c \gamma + \lambda_w + \lambda_{jm} + \lambda_i + \varepsilon_{ijmt}.$$

The coefficients β_1 and β_2 are presented.

TABLE 4—EFFECT OF ACCESS TO PRICE INFORMATION ON PRICES PAID

	Baseline (1)	Winsorized (2)	Separate effects for			
			Primary care (3)	Number of procedures (4)	Met deductible (5)	Emergency care (6)
Post × corporate employee	−0.016 (0.004)	−0.014 (0.003)	−0.016 (0.004)	−0.016 (0.004)	−0.019 (0.004)	−0.018 (0.004)
Interactions with:						
Primary care			0.024 (0.012)			
15+ procedures on day				0.011 (0.06)		
Met deductible					0.009 (0.004)	
Emergency care						0.011 (0.019)
Adjusted R^2	0.925	0.947	0.925	0.925	0.925	0.925
Observations	387,774	387,774	387,774	387,774	387,774	387,774

Notes: Dependent variable is $\ln(\text{price})$. Regressions include week-year, employee, and market-procedure-setting fixed effects, and indicators for whether employee had fulfilled deductible. Columns 3–6 are baseline specification where the DD estimator interacted with specified indicator. 15+ procedures on day indicates employee had more than 15 procedures on day of the procedure. Emergency care indicates employee received emergency care that day. Standard errors are clustered by market.

Another concern is that patients will not be able to search effectively for complicated care. As the bundle of medical care 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 15 or more procedures on a given day with the differences-in-differences estimator and present the results in column 4. Access is associated with a 1.6 percent reduction in prices paid when the employee had fewer than 15 procedures that day, but little measurable impact on days with 15 or more procedures. This result is robust to specifying alternative procedure cutoffs, e.g., 20 procedures on the day.

People who have met the deductible of their insurance contracts 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 5 of Table 4 reports the results. Access to price information had larger impacts for employees who had not met their deductibles (1.8 percent reduction) than for employees who had met their deductibles (0.7 percent reduction).

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 in the final column of Table 4. For non-emergency care, the point estimate is similar to the baseline result. For emergency care, the estimated impact of access is much smaller in magnitude, but

I lack the statistical power to statistically distinguish the impact of access on emergency and non-emergency-care.

The identifying assumption for the differences-in-differences framework is that the corporate and noncorporate 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. 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.¹⁶ 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 5.

Column 2 of Table 5 shows that that the differential linear trend is not statistically distinguishable from zero. Column 3 shows that corporate office employees were not on differential linear trends in either 2009 or the months of 2010 in which they did not have access. Column 4 presents the estimated differences in prices paid by corporate employees in the five weeks preceding access to Compass.¹⁷ There is no clear downward trend that would suggest the differences-in-differences coefficient is simply picking up a spurious correlation.

In addition to the pretrend tests, I have run placebo tests. These tests change the date of corporate access to an alternative date, e.g., February 1, 2009, and then estimate equation (1). In this example, the hypothetical access date sets the differences-in-differences estimator to one for the corporate group for all care received on or after February 1, 2009. Using the first day of each month between February, 2009 and August, 2010 provides 19 placebo access dates. Out of these dates, none produce a statistically significant impact of access.

As we saw in Table 2, corporate employees were more likely to have used care than noncorporate employees. This raises concerns that some of the noncorporate employees might not be good controls in the differences-in-differences specification. Because of this, I implement a matching estimator to compare corporate employees to noncorporate employees who are similar on their observables. For each corporate employee i , I match noncorporate employee j to i if j is in the same CBSA as i and if

$$(4) \quad \|X_j - X_i\| < m.$$

In practice, I let X be per person health spending in 2009 and choose m in two ways. First, I let m vary from \$50 to \$150. Second, I let m be between 4 percent and 12 percent of the corporate employee's 2009 per person health spending. I stack the matched samples and estimate equation (1) on the matched data. The

¹⁶ Because the regression specification includes week-year fixed effects, I create a new variable that is the week-year 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.

¹⁷ The full results for all 20 weeks are presented in online Appendix Table A.1.

TABLE 5—ACCESS TO PRICE INFORMATION AND PRICES PAID: PRE-TREND TESTS

	Baseline (1)	Add corporate linear trend (2)	Pre-period linear trends (3)	Pre-period dummies (4)
Post × corporate employee	−0.016 (0.004)	−0.011 (0.005)	−0.023 (0.005)	−0.017 (0.004)
Linear trend × corporate emp.		−0.0001 (0.0001)		
Linear pre-trends × 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^2	0.925	0.925	0.925	0.925
Observations	387,774	387,774	387,774	387,774

Notes: Dependent variable is $\ln(\text{price})$. 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-setting, and indicators for whether the employee had fulfilled the deductible. Standard errors are clustered by market.

market-procedure-setting fixed effects are estimated separately by matched group; i.e., the first corporate employee and her matched noncorporate employees have a different set of market-procedure-setting fixed effects from the second corporate employee and her matched noncorporate employees.¹⁸ I weight the regression to account for the matching of multiple noncorporate employees to a single corporate employee.¹⁹

The results are presented in Table 6. With a \$50 matching window, the estimate suggests that access to price information reduces prices paid by 1.7 percent.

¹⁸ In principle, all of the covariates could be allowed to vary by the matched group. However in practice, the employee and week fixed effects are not strongly correlated with the differences-in-differences variable after the procedure-market-setting-matched group fixed effects have been removed, and so have little impact on the estimated impact of access. For computational ease and statistical efficiency, I do not include these additional interactions in the estimation.

¹⁹ The noncorporate observations matched to a given corporate employee are weighted so that they sum to one. Intuitively, this creates an “average” noncorporate employee against whom the corporate employee is being compared. More specifically, if corporate employee i is matched to three different noncorporate employees, each of those noncorporate employees’ observations will be given a weight of one-third. The corporate employee’s observations will all receive a weight of one.

TABLE 6—ACCESS TO PRICE INFORMATION AND PRICES PAID: MATCHING ESTIMATOR

	$m = \$50$ (1)	$m = \$100$ (2)	$m = \$150$ (3)
<i>Panel A. Matches within m dollars</i>			
Post \times corporate employee	−0.017 (0.008)	−0.019 (0.006)	−0.017 (0.007)
Observations	282,375	411,704	538,198
	$m = 4$ percent	$m = 8$ percent	$m = 12$ percent
<i>Panel B. Matches within m percent</i>			
Post \times corporate employee	−0.017 (0.005)	−0.014 (0.006)	−0.014 (0.006)
Observations	198,903	306,073	413,804

Notes: Dependent variable is $\ln(\text{price})$. Corporate employees matched to noncorporate employees by market and 2009 per person health care spending. Panel A finds matches based on dollar amounts, panel B on percentages. Matching regressions include week-year, employee, and market-procedure-setting-matched group fixed effects, and indicators for whether the employee had fulfilled the deductible. Standard errors are clustered by market.

Columns 2 and 3 widen the range in which matches are found, but produce very similar point estimates. As seen in columns 4–6, matching on percentages produces qualitatively similar results. Overall, these results are consistent with those found in the unmatched differences-in-differences approach and provide additional evidence that access reduced prices paid.

C. Sources of Estimated Price Reductions

Receiving access to price information might affect prices in health care in a number of ways. On the demand side, it might lead employees to search for price information and switch to lower priced providers. In addition, it might lead employees to search and adjust their use of care. For instance, an employee might call to learn the price for a procedure, find out it is much more expensive than anticipated, and choose to not receive that care; alternatively, the employee might learn the price is much lower than anticipated and choose to receive the care. In this example, prices paid could fall because of an adjustment on the extensive margin even without employees switching providers. On the supply side, access to price information could increase insurers' bargaining power and allow them to negotiate lower prices than before. In my data, it is not likely that there are supply side responses to the employees' access to price information because the employees are spread throughout the United States and are effectively a zero measure set of health care consumers in any given market. As such, their insurer's bargaining power is not likely to have changed.

First, I test whether access to price information increases the probability that an employee switches providers. A subset of the American Medical Association's CPT billing codes indicate whether the patient was a new or established patient. This distinction is made on the codes that physicians use to bill for the time they spend with a patient. For example, CPT code 99213 is used for an "Office or other outpatient visit for the evaluation and management of an *established* patient," while CPT

code 99201 is for new patients: “Office or other outpatient visit for the evaluation and management of a *new* patient” (emphasis added).²⁰ There are ten such CPT codes that effectively indicate whether or not the patient was seeing a new doctor.

For this subset of procedures, I estimate

$$(5) \quad \text{new physician}_{ijmt} = (\text{post}_t \times \text{corporate employee}_i) \beta_1 + Z_c \gamma \\ + \lambda_{j'} + \lambda_w + \varepsilon_{ijmt}.$$

$\text{new physician}_{ijmt}$ indicates whether procedure j was provided by a physician who is new to employee i in market m at time t . Z_c contains indicators for the setting of the care, whether the employee had met her deductible, and demographics; and λ_w are week-year fixed effects. Because the indicator for seeing a new physician is based entirely on the CPT code for the procedure, including the procedure-market-setting fixed effects would perfectly predict whether the visit was to a new physician or not. However, for each CPT code for a new patient, there is a corresponding CPT code for an established patient. I group corresponding codes together and include a set of these modified procedure fixed effects, $\lambda_{j'}$, to partially control for the types of care employees are receiving. 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.

Estimates are presented in Table 7. The first column suggests that access to Compass increases the probability of seeing a new physician by 2.4 percentage points.²¹ Because only 17 percent of the visits are to new doctors, this is a 14 percent 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 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.

²⁰ This information was obtained from the American Medical Association’s website, <https://ocm.ama-assn.org/OCM/CPTRelativeValueSearchResults.do?locality=3&keyword=99213>.

²¹ Estimating the regressions as probits produces results that are extremely similar to those presented.

TABLE 7—IMPACT OF ACCESS ON SEEING A NEW DOCTOR

	Baseline (1)	Market fixed effects (2)	Employee fixed effects (3)
Post \times corporate employee	0.024 (0.007)	0.022 (0.007)	0.031 (0.010)
CBSA fixed effects		X	
Employee fixed effects			X
Mean dependent variable	0.169	0.169	0.169
Adjusted R^2	0.026	0.031	0.120
Observations	63,704	63,704	63,704

Notes: Dependent variable is whether patient is seeing a new doctor. Data only include procedures differentiating between new and established patients. All columns include indicators for modified procedure code (described in text), type of setting where procedure performed, demographics, and week fixed effects. Standard errors are clustered by market.

If employees are switching providers, then it becomes particularly 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.²² 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.²³ 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 8, 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 1/13 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 restriction in column 3 and find similar results. Because the measures of quality are noisy at best (Doyle, Jr. et al. 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.

It is difficult to directly address the extent to which changes in the quantity of care affect the prices employees paid for care. In the short run, the transaction prices

²² 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.

²³ 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 et al. (2006) and Fierer (2007) among others. The sample does not contain a sufficient number of observations to use the disease-specific measures of hospital quality.

TABLE 8—IMPACT OF ACCESS ON QUALITY OF HOSPITAL CARE

	Baseline (1)	ln(quality) (2)	One observation per visit (3)
Post × corporate employee	0.200 (0.174)	0.002 (0.002)	0.101 (0.324)
Mean of dependent variable	97.072		96.964
SD of dependent variable	2.358		2.488
Adjusted R ²	0.854	0.834	0.875
Observations	23,123	23,123	8,415

Notes: 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 are clustered by market.

negotiated between the insurer and the providers will not have changed. But when employees search and learn additional information, people who expected prices to be higher than they actually are might now choose to go to the doctor while people who expected prices to be low might now choose to not go to the doctor. Although I do not observe the employee’s prior beliefs about prices, I do see that at least 90 percent of searches are followed by use of care.²⁴ This suggests that for no more than 10 percent of searches did the employee expect a much lower price and subsequently decide to forego care. However, because there is so much dispersion in prices, even small changes on the extensive margin could play an important role in explaining the observed price reductions.

Access to price information leads to lower transacted prices at least in part because consumers search. Because the impact of searching on prices is likely to vary across individuals, the IV regression will provide a LATE parameter that only applies to a subset of the population. As such, it is useful to get a better sense of who searches. Online Appendix Table B.1 provides summary statistics on corporate employees who searched in 2010 and corporate employees who did not search in 2010. Relative to each procedure’s mean price in their market, searchers were not obtaining substantively different prices from nonsearchers and were quite similar on a number of demographic dimensions. However, searchers had consumed somewhat more care per covered person and were less likely to have chosen the more generous insurance plan. Thus, the IV estimate reflects impacts for people who were not getting unusually high prices, but were tending to use more care.

The IV estimates are given in Table 9 and the first stage estimates are shown in online Appendix A. Each column presents the estimated impact of search on prices for one of the mappings of calls to procedures described previously. 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 percent. Although this is a very large price reduction, it is a reasonable

²⁴This relies on assigning calls to particular procedures as in the IV analysis. The 90 percent estimate is for the most conservative of the mappings of calls to procedures.

TABLE 9—IMPACT OF SEARCH ON PRICES PAID

	One month (1)	One month or previous call (2)	Everything after first call (3)
Searched	−0.167 (0.044)	−0.137 (0.035)	−0.101 (0.026)
<i>F</i> -stat, first stage	37.12	40.59	44.51
Observations	387,774	387,774	387,774

Notes: Instrumental variables results. Dependent variable is $\ln(\text{price})$. 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-setting, and indicators for whether the employee had fulfilled the deductible. Standard errors are clustered by market.

one—searching achieves roughly half of the price reduction due to moving from the ninetieth percentile of the distribution down to the median.

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

IV. 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.

A. Empirical Strategy

I use variation based on differences in employees' marginal price for care on September 27, 2010—the date the corporate office employees first gained access to Compass's price information. As seen in Table 3, in 2010, the employees had standard, nonlinear, preferred provider organization (PPO) insurance plans that included annual deductibles, cost-sharing provisions (coinsurance rates and co-pays), and out-of-pocket maximums to cap employees' total expenditure risk.²⁵ Because employees had consumed different amounts of care before they gained access

²⁵ 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.

to Compass, they were in different cost-sharing regions of their insurance plans. Approximately 31 percent of the corporate employees had met their deductibles by the date they gained access to Compass. On average, an employee's marginal price of care fell by 50 percent when her deductible was 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 13 weeks of 2011. Initially, I only use data from 2010. The sample is restricted to corporate employees who received insurance through the company in 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

$$(6) \quad \Pr(\text{called}_{it} = 1) = \Phi(\text{met deductible by access}_i \delta_1 + \lambda_t + X_{it} \gamma_1),$$

where called_{it} indicates whether employee i called Compass for price information in week t ; Φ is the normal cumulative density function; $\text{met deductible by access}_i$ 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 that removes any week-to-week variation in the propensity to search; 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.

Interpreting the results from this empirical strategy requires care. It is not clear that the marginal price changes discontinuously when employees meet their deductibles. For example, consider two employees near the deductible threshold who are both going to go see the doctor for a simple procedure. In practice, the employee who is \$1 below the threshold does not face a significantly higher marginal price than the employee who is \$1 past the threshold because both employees will be beyond the threshold once they receive care. More generally, some employees who are below the threshold may behave as though they face a low marginal price for care. This will lead equation (6) to understate the impact of insurance coverage on search.

Table 10 presents summary statistics for employees who had and those who had not met their deductibles by access in 2010. Importantly, there appears to be little difference across the groups in their 2009 health care spending, age, income, and other demographics. There does appear to be a slight difference in family size; those who had met their deductibles by access were from somewhat larger families. Although the two groups are comparable, I supplement the empirical strategy with the same matching approach used earlier. In this case, employees who had not met their deductibles are matched to similar employees who had met their deductibles. I again match exactly on the geographic market and use ranges of 2009 spending to determine matches. I stack matched groups and estimate

$$(7) \quad \text{called}_{igt} = \text{met deductible by access}_i \delta_1 + \lambda_{tg} + X_{it} \gamma_{1g} + \eta_{igt}$$

TABLE 10—SUMMARY STATISTICS FOR CORPORATE EMPLOYEES

	Met deductible by access	Not met deductible by access	<i>p</i> -value of difference
Median per person health spending, 2009	\$1,177	\$1,070	0.14
Number covered per employee	3.3	2.6	0.03
Age	42	43	0.51
Per capita income	\$64,006	\$63,090	0.61
Fraction with college or more	0.39	0.40	0.53
Fraction white	0.81	0.78	0.11
Observations	175	390	

Notes: Unit of observation is the corporate employee. Statistics based on 565 corporate employees insured through company in 2010 and 2011. Met deductible by access is determined using 2010 data. Median health spending per covered individual reported for 2009.

TABLE 11—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 (0.006)	−0.016 (0.006)	−0.015 (0.006)	−0.012 (0.006)	−0.016 (0.006)
Week f.e.	X	X	X	X	
Demand controls	X	X	X	X	X
Age and family size		X	X	X	X
Five-digit zip demographics		X	X	X	X
Mean of dependent variable	0.016	0.016	0.016	0.016	0.016
Pseudo R^2	0.050	0.084	0.087	0.106	0.078
Observations	7,345	7,345	7,345	7,345	7,345

Notes: 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 five-digit zip code are described in the paper. Demand controls bins break previous spending into \$200 bins and include 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 are clustered by employee.

for each employee i in matched group g in week t . X_{it} is a third-order polynomial in previous medical spending. Note that the week fixed effects, λ_{tg} , vary for each matched group. In addition, the impact of past medical spending is allowed to vary by matched group.²⁶ Standard errors are clustered by employee.

B. Results

Column 1 of Table 11 reports the estimated marginal effects where only the demand controls and week fixed effects have been included. The results show that

²⁶In practice, only the first order term of the polynomial varies with matched group. When the second or third order terms are also allowed to vary by matched group, collinearities prevent the model from being estimated with any reliability.

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 percent difference. On average, meeting the deductible reduced the out-of-pocket price by 50 percent. 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. 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 11 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 an impact on the demand controls in the later weeks of 2010. Column 5 shuts down this concern by using 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. 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 online Appendix C. The estimated marginal effects are consistent with those found in Table 11, but are estimated with very little precision.

The matching approach produces very similar results. These estimates are presented in Table 12. When employees are matched on geography and a narrow range (\$50) of 2009 health care spending, the estimates suggest that having met the deductible lowers the probability of searching by 1.7 percentage points. As the matching window for 2009 medical spending increases, the point estimates remain stable. Matching on a percentage of the corporate employee's 2009 per person health spending produces similar results and continues to suggest that having met the deductible by access leads to a lower probability of searching.

It is possible that some omitted, employee-specific variable that has nothing to do with the marginal price for care leads to the observed correlation. For example, employees who care much more about quality than price could be less likely to search for price information and more likely to have met their deductibles. If this were true, if employee's decisions about search were unaffected by the marginal price for care, then the search patterns observed in 2010 should be observed in 2011 even after deductibles had been reset. To test this alternative hypothesis, I estimate equation (6) using data from 2011 and report the marginal effects in Table 13.

As seen in column 1, employees who had met their deductibles by access in 2010 were no less likely to search in 2011 than employees who had not met their deductibles in 2010. In each specification, the point estimate is very small,

TABLE 12—DEDUCTIBLE STATUS AND USE OF PRICE INFORMATION: MATCHING ESTIMATOR

	<i>m</i> = \$50 (1)	<i>m</i> = \$100 (2)	<i>m</i> = \$150 (3)
<i>Panel A. Matches within <i>m</i> dollars</i>			
Met deductible by access	−0.017 (0.008)	−0.015 (0.008)	−0.016 (0.007)
Observations	18,746	35,685	50,999
	<i>m</i> = 4 percent	<i>m</i> = 8 percent	<i>m</i> = 12 percent
<i>Panel B. Matches within <i>m</i> percent</i>			
Met deductible by access	−0.013 (0.007)	−0.014 (0.007)	−0.015 (0.007)
Observations	6,396	9,750	12,649

Notes: Dependent variable is whether employee searched that week. Employees matched by market and 2009 per person health care spending. Panel A matches on dollar interval, panel B on percentage interval. Matching regressions include controls for previous medical spending and week fixed effects. Standard errors are clustered by employee.

TABLE 13—DEDUCTIBLE STATUS AND USE OF PRICE INFORMATION IN THE NEXT YEAR

	Base (1)	Individual demos (2)	Zip code demos (3)
Met deductible	0.002 (0.007)	0.002 (0.006)	0.004 (0.006)
Week f.e.	X	X	X
Demand controls	X	X	X
Age and family size		X	X
Five-digit zip demographics			X
Mean of dependent variable	0.020	0.020	0.020
Pseudo <i>R</i> ²	0.001	0.012	0.015
Observations	7,281	7,281	7,281

Notes: 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 five-digit zip code are described in the paper. Standard errors are clustered by employee.

positive, and nowhere near statistically significant.²⁷ These same results can be seen in Figure 3. Starting in January 2011, there does not appear to be a systematic relationship between an employee's 2010 deductible status and her 2011 search. This suggests it was not some time-invariant, person-specific factor that was driving the 2010 results.

²⁷ 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.

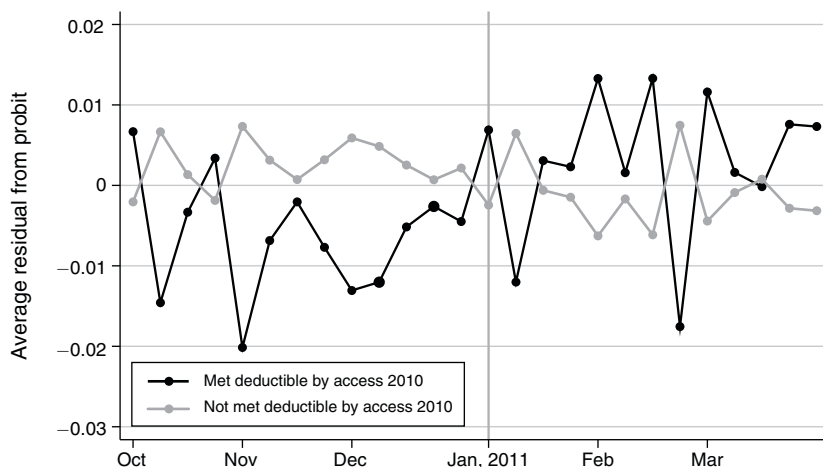


FIGURE 3. DEDUCTIBLE STATUS AT ACCESS AND SEARCH

V. 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. I use a unique dataset with a direct measure of search to show that access to price information reduces the prices paid for care by 1.6 percent on average. The reduction is concentrated in types of care that are easier to plan for in advance and for employees who have greater incentives to search. Once employees gain access to price information, they become much more likely to visit a provider they had not seen previously. Despite this, their quality of care does not appear to fall dramatically. I find that search itself reduces the price paid by 10–17 percent, but that a relatively small amount of search occurs. I provide 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 insurance coverage; the estimated elasticity of search with respect to out-of-pocket price is 1.8. Taken together, access to price information could have large impacts in the market for health care, but 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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