

Medical Bill Shock and Imperfect Moral Hazard*

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Abstract

Consumers are sensitive to medical prices when consuming care, but delays in price information may distort moral hazard. We study how medical bills affect household spillover spending following utilization of shoppable services, leveraging variation in insurer claim processing times. Households increase spending by 22% after a scheduled service, but then reduce spending by 11% after the bill arrives. Observed bill effects are consistent with resolving price uncertainty; bill effects are strongest when pricing information is particularly salient. A model of demand for healthcare with delayed pricing information suggests households misperceive pricing signals prior to bills, and that correcting these perceptions reduce average (median) spending by 16% (7%) annually.

Keywords: *Ex-post* moral hazard, price transparency, learning, low-value care

JEL codes: I12, I13, D01, D90

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1 Introduction

Health insurance plays a vital role in protecting consumers against the risk of volatile, unpredictable health and financial shocks. However, incomplete information plagues health insurance markets, ultimately leading both public institutions (e.g., governments) and private organizations (e.g., insurers) to provide sub-optimal coverage (Einav and Finkelstein, 2018; Dave and Kaestner, 2009).

A primary information friction in healthcare markets is “*ex-post* moral hazard,” the extent to which consumption is price sensitive.¹ Moral hazard ultimately justifies exposing consumers to out-of-pocket (OOP) cost-sharing for health services (Chandra et al., 2010; Goldman and Philipson, 2007), for example, through increasing enrollment in high-deductible health plans (HDHPs) to limit high consumption of potentially low-value health services (Geyman, 2012). Price pressures may harm households who either delay or forego necessary medical care² or reduce consumption of high-value health services such as preventive care.³

While consumers are responsive to prices when making healthcare consumption decisions, there is ongoing uncertainty about the extent to which consumers have access to accurate and timely information about the marginal costs of care (Lieber, 2017). Recent work has focused on consumer knowledge of the *ex-ante* OOP price of a service, such as how consumers search across multiple medical providers offering the same service at different prices (Brown, 2017). In this paper, we highlight an overlooked feature of medical demand under price uncertainty with significant implications for models of moral hazard: lack of timely *ex-post* pricing information.

Consumers are rarely given accurate information about service prices at the point of consumption, much less information about their own expected OOP contribution.⁴ Service prices vary across healthcare organizations, payers, and service bundles (Fronsdal et al., 2020; Patel et al., 2023), even for very common services (Gruber, 2022; Cooper et al., 2019).⁵ Fur-

¹Referring to elastic healthcare demand as “moral hazard” is a now widely-used abuse of notation, beginning with Arrow (1963). Specifically, moral hazard refers to how, conditional on health, consumption adapts to care prices (Pauly and Blavin, 2008; Cutler and Zeckhauser, 2000). Previous work has underscored its role in decision-making (Kowalski, 2016a; Duarte, 2012; Dunn, 2016).

²Consumers exposed to higher rates of cost-sharing are more likely to report delaying medical care, a finding exacerbated among low-income households (Kullgren et al., 2010) or those with high-cost chronic conditions (Fu et al., 2021; Gaffney et al., 2020).

³While value-based insurance designs—where certain high-value services are carved out of cost-sharing obligations—have become more prevalent (Chernew et al., 2007), confusion about insurance contracts may still affect take-up (Hoagland and Shafer, 2021; Shafer et al., 2021).

⁴Notably, health services are characterized by a total amount billed by physicians (a “sticker” price); a negotiated total price approved by the patient’s insurer; and the relative fraction of that negotiated price that is the patient’s OOP responsibility. Importantly, price transparency for medical pricing must take into account these various prices, including the relative lack of information contained in sticker prices.

⁵Appendix Figure A.1 illustrates some of the variations in prices for common services in our sample.

thermore, *ex-post* haggling over reimbursement between insurers and providers may prolong consumers’ price uncertainty. Patient OOP costs vary over contracts and time as a function of prior consumption at the household level; hence, residual uncertainty about realized spending affects marginal prices for care consumed even before bills arrive, and consumers must form expectations about their already realized expenses in the interim.

We isolate the causal impact of receiving a medical bill on household spillovers in health-care consumption, by studying how households with employer-sponsored insurance (ESI) in the US make collective spending decisions after one household member incurs a significant health expenditure.⁶ We use exogenous variation in the time consumers wait for their bills—driven by variation at the insurer-clinician level in claim submission and processing times—to identify the effect of a bill on spending, compared to household spending before the event and spending during the household’s “interim period” between a service and a bill. We formalize this three-way comparison between post-bill spending and both pre-event and interim spending using a modification of a triple differences regression.

We find strong evidence that a bill’s arrival changes household behavior, implying that patients and households face real uncertainty about their bills. In the interim period between the service and its bill, household members increase their total health spending by about 21.8% (roughly \$27 per person per week). However, once the bill arrives, consumption drops significantly by 10.9%, half of the post-service increase. We provide evidence that these bill effects are concentrated among bills that provide meaningful pricing information to households, including information about the expected OOP costs for care consumed between the service date and the bill’s arrival. In particular, households who learn that they have not yet met a deductible (meaning their marginal cost-sharing is 100% for services not carved out of coverage) exhibit larger estimated effects than those whose index service spending meets the deductible threshold.

We show evidence suggesting bill effects are driven primarily by the pricing information they provide, rather than by other channels. We observe the largest responses to a bill when it is most informative of prices, such as for households just shy of meeting their deductible. Second, we demonstrate that households appear to learn about their marginal prices over time. Other potential mechanisms, including liquidity constraints or supplier-

⁶Specifically, we use health service classified as “shoppable” by the Centers for Medicare & Medicaid Services (CMS) as index events (CMS, 2019). Shoppable services include scheduled services with significant price uncertainty, meaning that (a) households may form expectations about their prices and subsequent marginal costs and respond appropriately through demand increases and (b) bills may provide meaningful price information to consumers (see Section 2 for more discussion). We exclude the household member who received the service to estimate spillover responses among the unaffected household members and identify the causal effect of a bill’s arrival in changing these responses. Our results are robust to using a broader definition of index health events, including unplanned injuries and surgeries.

induced responses, do not sufficiently explain observed spending changes. Finally, bills shift *where* households seek these services (e.g., from a hospital to an outpatient clinic).

These features of observed bill effects—which suggest households face real uncertainty about marginal prices before a bill’s arrival—may lead to deviations in medical decision-making from typical model predictions under full information. We therefore develop and estimate a model of “imperfect moral hazard” in which households face delays in information about their OOP spending to date. We use the exogenous variation in our data to identify the implied distribution of spending signals and household learning over time for all medical services, moving beyond the reduced-form restrictions using only shoppable services as index events. The model estimates suggest—in keeping with our reduced-form results—that households receive noisy spending signals prior to a bill’s arrival. On average, these signals are larger than the true bill, meaning that households are likely to incorrectly assume that they have met their deductible while waiting for a bill. Delayed resolution of price uncertainty results in 85% of households spending more on care than they would under real-time claims adjudication. We estimate that the average (median) affected household spends \$364 (\$94) more per household member per plan year.⁷ We also find strong evidence of consumer learning, with price uncertainty affecting households significantly more at the beginning of a plan year.

We present the first model of healthcare demand under delayed information and highlight its implications for consumption and welfare; hence, our work makes several important contributions. First, we add to an ongoing literature on dynamic responses to cost-sharing, including the strategic delay of services such as dental care (Cabral, 2017a) and models of “forward-looking” moral hazard (Aron-Dine et al., 2015; Baicker et al., 2015). In contrast to previous work modeling uncertainty in expected end-of-year prices (Einav et al., 2015), we highlight the role of pricing uncertainty in the short-run demand for care. Recent work has found patients will defer care when anticipating future price changes (Hettinger, 2022; Johansson et al., 2023). Although there is strong evidence for the role of dynamic moral hazard in healthcare (Klein et al., 2022; Diaz-Campo, 2022), our results highlight that information about *ex-post* prices changes real-time decisions about both when and where to receive care, even for services which cannot be strategically delayed.

Our findings also fit into a larger discussion of the usefulness of price transparency policies in mitigating large levels of healthcare consumption in the United States (Muir et al., 2012; Zhang et al., 2020). In contrast to previous work—which highlighted how the availability

⁷Although this leads to greater total spending, this is not to say that increased consumption is nominatively of lower value to the household. For example, this increase in consumption could be an increase in services that are beneficial to the household but were previously delayed or foregone due to liquidity constraints.

of price information may change the strategic decisions of patients shopping for a service (Gondi et al., 2021; Reed et al., 2005)—we highlight a new mechanism through which price transparency may affect *future* care decisions, even across household members. Our findings suggest that shortening periods of price uncertainty may affect decision-making for the entire household. Policies that decrease the information gap between consumption and accurate price information—such as real-time claims adjudication for physical health claims, similar to prescription drug claims adjudication (Hartzema et al., 2011)—would reduce fluctuations in consumption decisions, improving household welfare to the extent that unexpected price shocks are eliminated.

Finally, we contribute to a broad and growing literature related to bureaucracy in medicine (Brot-Goldberg et al., 2023; Shi, 2022; League, 2022). In addition to large expenses related to administrative burden in the healthcare system, our work highlights that administrative frictions directly affect medical consumption decisions, both for the affected enrollee and others in a household. Idiosyncratic differences in clinician practice management and physician-insurer interactions, including hospital or physician delays in submitting claims, insurer delays or errors in following provider group contracts, or disagreements between insurers and providers, may exacerbate these frictions. Faced with these uncertainties, households are left to form unreliable expectations about the costs of their care while making demand decisions. In this sense, our work is closely related to recent work that found providers are less willing to treat patients with greater degrees of billing uncertainty, such as publicly insured patients (Dunn et al., 2020). We complement this work by highlighting these effects on the demand side, where patients and their families have less choice over administrative frictions, but still face real costs for delayed information.

Health care is not the only setting where marginal price uncertainty affects consumption decisions. “Bill shock” is common in other industries including household utilities, cell phone services, and even college education financing (Grubb and Osborne, 2015). Our work furthers models of demand under marginal price uncertainty by providing a tractable estimation of consumer beliefs and learning. Our model is related to others where individuals learn about uncertain prices of goods (including financial assets and agricultural goods) (Ngangoue, 2021; Boyd and Bellemare, 2020); however, our model does not rely on consumer inattentiveness to past consumption, but underscores the role of delayed information arising from complex contracts involving multiple parties.⁸ Studying these complex contracts has

⁸Our work is also related to the literature on learning models with delays in belief updating (Karlsson et al., 2009; Peng, 2005). However, in these models, delays typically arise endogenously as consumers either choose to delay learning or have limited information processing abilities. In contrast, our model exploits exogenous variation in the delayed *arrival* of information outside the consumer’s control, but which still affects the marginal utility and costs associated with choices retroactively.

the added advantage that we avoid concerns about endogenous price setting at the supplier level, given that bill shock arises as a disconnect between insurers and physicians rather than from a single supplier such as a cell phone provider (Grubb, 2015).⁹ Finally, studying bill shock in ESI is particularly salient given that it comprises roughly 6% of US GDP.

We discuss the setting of shoppable services and the data in Section 2. We then present our methods and identifying assumptions in Section 3, followed by our empirical results in Section 4. We incorporate these findings into a model of imperfect moral hazard in Section 5. Finally, Section 6 highlights the relevance of these findings for the optimal design of insurance contracts.

2 Setting and Data

2.1 Data

We use data on household healthcare utilization from the Merative (formerly IBM Watson Health) MarketScan *Commercial Claims and Encounters* Database, which contains detailed inpatient, outpatient, and pharmaceutical claims for a sample of households enrolled in ESI through large U.S. firms. Each observation includes diagnostic, procedural, and payment information, including the date of service and the corresponding date on which the insurer paid their portion of the claim. The data also includes household, firm, and insurance plan identifiers and characteristics.¹⁰

We limit our analytical sample to enrollees in one of eight large firms between 2006 and 2018.¹¹ Our final sample includes 386,240 households with two or more members, full eligibility, and continuous enrollment across their window of observation. Throughout, spending data has been normalized to 2022 USD using the Consumer Price Index for All Urban Consumers series.

Table 1 presents summary statistics for the full sample as well as the sample subset with insurance plan identifiers.¹² Households tend to be young and relatively low-risk, with an

⁹This is in contrast to endogenous price setting in the context of *ex-ante* prices for specific medical services, as discussed in Brown (2017).

¹⁰We use the empirical set of enrollees in a plan to estimate insurance plan characteristics, following previous literature (Hoagland, 2022; Marone and Sabety, 2022). See Hoagland (2022) Appendix A for a detailed description of this methodology and an evaluation of the quality of these inferences.

¹¹Firms were randomly selected from a larger sample of firms with plan identifiers available; plans include both high-deductible health plans (HDHPs) and other plan types (HMOs and zero-deductible plans), and have a start date of January 1 for all observed years. Insurance plan identifiers are only available through 2013, as discussed below.

¹²The MarketScan data includes insurance plan identifiers for a subset of plans for which information can easily be abstracted from summary plan description booklets made available by the insurer (Hansen, 2017). Typically, these plans represent the largest firms in the data, and include detailed information on the

	Full Sample	Plan-Identified Sample
Panel A: Demographics		
Age (individual)	31.67 (0.000)	31.15 (0.000)
% female (individual)	0.51 (0.000)	0.51 (0.000)
Risk score	0.29 (0.000)	0.29 (0.000)
Family size	3.08 (0.000)	3.10 (0.000)
Panel B: Medical Utilization		
Total medical spending (individual)	\$4,764 [\$975] (0.002)	\$4,406 [\$887] (0.002)
% of individuals with no spending	0.17 (0.000)	0.20 (0.000)
OOP medical spending (individual)	\$650 [\$198] (0.000)	\$562 [\$167] (0.000)
Household deductible deductible > 0	—	\$1,040.24 (0.001)
% Households with zero deductible	—	0.26 (0.000)
Household coinsurance rate	—	0.29 (0.000)
% individuals with shoppable services	0.06 (0.000)	0.06 (0.000)
Total cost, shoppable service	\$5,572 [\$3,721] (0.011)	\$5,645 [\$3,814] (0.015)
OOP, shoppable service	\$691 [\$388] (0.002)	\$574 [\$290] (0.002)
Years	2006–2018	2006–2013
N_{families}	368,237	367,445
$N_{\text{individuals}}$	1,357,392	1,311,554

Notes: Enrollees include employees and their covered dependents. Risk scores are calculated using the CMS-HCC 2014 community model. Household plan characteristics are calculated as discussed in Section 2. Spending values are reported in 2022 USD. Standard errors are reported in parentheses; medians (when reported) are in brackets.

Table 1. Household Summary Statistics

average age of 31.7 years and between 3 and 4 household members. Insurance coverage is more generous than average, although the conditional average deductible is over \$1,000, and household members who select into shoppable services typically spend close to a full year’s OOP costs on that service alone. Note that the sub-sample with plan identifiers does not appear substantially different from the full sample, an important fact given that we use the plan-identified sample in our structural approach (Section 5). Households in the plan-identified sample incur slightly lower OOP costs than the full sample; however, this is likely indicative of decreasing insurance coverage generosity over time, given that the latest 5 years of data are excluded in this sub-sample.

number and different types of plan offerings made available to employees (including at what level), as well as “financial provisions, health service benefits, managed care features and health coverage types.” In the current data, this practice ended after 2013.

2.2 CMS Shoppable Services

To evaluate how household utilization responds to the resolution of pricing uncertainty, we study household services that are both planned and expected to incur a significant—but unknown—OOP cost. These two criteria are useful to ensure that we are studying a setting where household strategic decision-making is most likely to occur as well as one where bills provide useful information to households. We therefore identified 30 CMS “shoppable services,” common services that patients can schedule in advance and for which there exists substantial variation in charges across providers (CMS, 2019; White and Eguchi, 2014).¹³ Our services broadly include pathology (biopsies), radiology (electrocardiograms), and surgical services (spinal fusions). Overall, CMS shoppable services constituted 16% of overall OOP spending for individuals on ESI in 2017 (Bloschichak et al., 2020), making them an important area of study for both patients and policymakers.¹⁴

We selected shoppable services as index events to prioritize several attributes of household decision-making, particularly in the context of planned service consumption. First, we focus on settings where bills provide meaningful price information to consumers; our results are not informative about services where co-payments, which are typically collected by the clinical provider at the point of service, constitute all cost-sharing. Co-payments as the sole source of cost-sharing are common for low-acuity acute primary care visits. Second, we select planned, scheduled services as index events as households may form expectations—even before the service is realized—that they will meet a deductible, giving time for scheduled increases in household medical demand. This plausibly strategic behavior motivates a focus on shoppable services over other health shocks such as hospitalizations or injuries, as it most closely mirrors the ideal experiment for our research question (i.e., how is household demand affected by price uncertainty?). Finally, our service selection is not based on the relative quality or value of a service (such as urgent or non-urgent hospitalizations); we evaluate how affected households select into high- or low-value services post-exposure in Section 4 (Fadlon and Nielsen, 2021; Card et al., 2009). Taken together, using shoppable services as index events best allows us to estimate how pricing information changes household demand for healthcare by focusing on services where bills are most likely to drive decision-making.

¹³We identified these services in the claims data using Current Procedural Technology (CPT) codes for outpatient and inpatient services and Diagnostic Related Groups (DRGs) for inpatient hospitalizations. The complete list of services is available in Appendix Table A.1. CMS shoppable services also include commonly used hospital evaluation and management (E&M) codes; we did not include these in our sample due to the substantially lower average cost of these services compared to other categories.

¹⁴Effective January 1, 2021, hospitals must publish standard charges for these services online, including negotiated rates. This does not affect our analytical sample (which goes through 2018). Prior to implementing this rule, there has been little empirical evidence found that patients engage in price shopping for these procedures ahead of time (Mehrotra et al., 2017).

However, our results are robust to more general inclusion criteria for the index events. For example, we show in Appendix Table A.2 that our main effects appear even in settings when *ex-ante* strategic service delays may not be as apparent, such as following an unplanned hospitalization. We consider separately how household members respond to an unplanned hospitalization of another family member for injuries, poisonings, or appendectomies.¹⁵ Even in this broad set of events—which now encompasses over 200,000 index events and 126 million individual-week observations—we continue to find robust evidence that household members reduce their spending by comparable amounts when a bill arrives; we discuss this more in Section 4.1.

We construct the baseline sample by identifying all shoppable services consumed among the households remaining in our analytic sample after requiring two or more members, full eligibility, and continuous enrollment. In the primary specifications, we limit index services to the first shoppable service consumed within a household plan-year—this removes 2.79% of the 814,795 identified shoppable services. However, this restriction is relaxed in our learning specification (Subsection 4.3.2) and the structural model of imperfect moral hazard (Section 5), which estimates equilibrium responses based on all medical encounters rather than a single focal event. Throughout, we use both never-treated and not-yet-treated households as control groups (see Section 3 for in-depth discussion).

2.3 Bill Dates & Waiting Times

One limitation of our data is that we do not view the exact date consumers received their first bill for their index event; instead, we observe the date the insurance plan adjudicated the claim and initiated payment to the clinical provider. An Explanation of Benefits (EOB) is generated at this point, making it the earliest possible date when a patient will receive definitive information about their deductible and coinsurance obligations. Clinicians seldom collect cost-sharing that is not co-payments before the adjudication of the claim by the insurer (Ippolito and Vabson, 2023; Mohama, 2021). Hence, patients as consumers are unlikely to have perfect pricing information before this date.¹⁶

We, therefore, use the earliest EOB generation date for a bundle of claims as a proxy for the arrival of new pricing information for consumers. Some consumers may receive same-day electronic notifications of EOB generation, while others will have a delay imposed by the postal service. In addition, we do not observe when patients *consume* the new information

¹⁵See Appendix Table A.2 for details on services included in this robustness check.

¹⁶Over our study period, a greater number of healthcare facilities and insurers began to offer price transparency tools to consumers. Takeup of these tools by consumers remains extremely low throughout our sample (Zhang et al., 2020); in addition, these tools do not remove all pricing uncertainty (especially for OOP prices), but rather provide patients with credible intervals of expected OOP costs.

that either an electronic or paper EOB provides. The effects of any measurement error here are expected only to attenuate our findings.¹⁷ Since our proxy measures the earliest possible date at which households have exposure to accurate pricing information, noise in our context always leads to a misclassification of the post-bill indicator to be 1 when it should be 0, rather than the other way around. Hence, the resulting coefficient on the post-bill indicator will be a weighted average of true post-bill effects and contamination from the interim period for any misclassified treatment dates; as long as the treatment effects of a bill’s arrival are of opposite sign than service effects (for example, if spillover household consumption increases following a service but then declines after the bill arrives), any contamination bias will attenuate the correction parameter towards zero. Similar logic applies to the use of any price transparency tools, as any use of these tools will attenuate the value of new and accurate information on household decision-making.

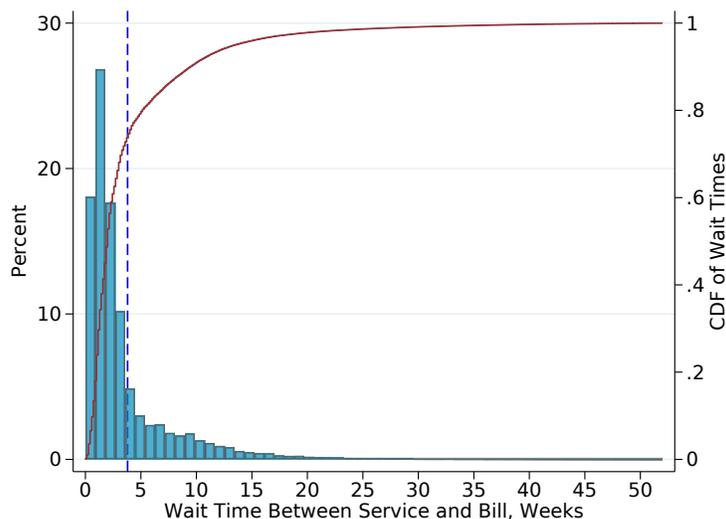
Figure 1 presents the distribution of wait times (in weeks) between a shoppable service and the date the plan paid their portion of the bill. There is substantial variation in this wait time, with roughly 60% of bills being paid by insurers within the first four weeks, and the rest taking longer than a month for payment to be settled.

We claim that the length of the waiting period between a service and a bill is exogenous to households, allowing us to identify the causal impact of receiving information on spillover utilization. Appendix Figure A.2 illustrates the substantial variation in this length both within and across years. Waiting times tend to be higher at the beginning of a calendar year and the first month of each quarter when insurers have billing changes and new policies to incorporate into their processing algorithms.¹⁸ Waiting times are also affected by other time-varying features of the healthcare system that are exogenous to the household, including the rate at which physicians submit claims to insurers for reimbursement. The exact variation in bill waiting times is, therefore, the result of interactions between an insurer—typically chosen at the employer level in our context, rather than the household level—and specific physicians or hospitals. Even if households attempted to choose general practice providers based on the relative efficiency of billing with their specific insurer, this is unlikely to be

¹⁷Note that in our context, measurement error only goes in one direction, as EOBs are not generated by payers prior to plan payment decisions (Denning, 2014; Davis-Jacobsen and Plemons, 2008). Hence, there are no instances in which consumers will learn precise information about non-copayment OOP obligations before the plan payment date. Note that individual line items with delayed adjudication do not change the information arrival date in our context.

¹⁸Waiting times are also affected by more general health policies, such as the national transition to the International Classification of Diseases, 10th Revision, Clinical Modification (ICD-10-CM), in October 2015. This transition increased billing complexity by roughly five times and, subsequently, the rate of administrative friction in processing billing information (Caskey et al., 2014). Even major health disruptions, such as the COVID-19 pandemic, can overwhelm payer processing of claims, occasionally even drastically increasing wait times for bills (Snowbeck, 2022).

Figure 1. Variation in Wait Times Between Service Date and Bills' Arrival



Notes: Figure depicts distribution of wait times between a service date and the date the insurer paid their portion of the bill, measured in weeks. Only services included as shoppable health events in our analytical sample are shown here. Vertical dashed blue line indicates the average duration of the waiting period (3.9 weeks). Red curve and secondary y -axis indicate the cumulative fraction of bills with a waiting time less than or equal to x .

a driver in the household choice of physicians and hospitals from whom they receive the shoppable services in our data (e.g., the surgeon who performs a mastectomy). Hence, the variation in the length of time a household waits for their bills is both unpredictable and exogenous at the consumer level; we discuss this more in Section 3.

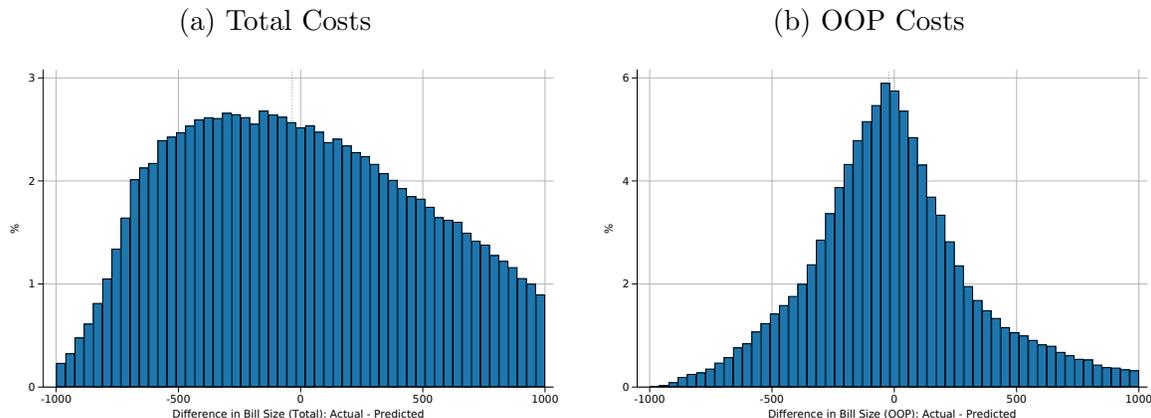
2.4 Descriptive Evidence for Bill Shock

Finally, we document the extent to which bills in our sample vary, with particular emphasis on how bills might convey new pricing information that patients were unable to access before their arrival. This is important as residual variation in OOP payments or total service costs—after incorporating information about providers, service types, and dates of services—gives a sense of how a bill’s arrival may generate “bill shock” altering consumer choices.

Appendix Figure A.1 illustrates the variation in prices (both total billed and OOP) for a particular service in our sample (routine vaginal delivery). To investigate this further, we regress prices on provider, service type, year, and relative week-of-year fixed effects and compare realized to predicted prices. Figure 2 reports the results, showing broad variation in predicted prices both for insurers and patients. Note that predictions for total bill size are slightly right-skewed—reflecting the highly skewed nature of overall prices—while the distribution of predicted OOP costs exhibits far less skew. Importantly, conditional on these

fixed effects, the average (median) absolute residual is \$443.07 (\$416.03) for total costs, and \$262.50 (\$197.20) for patient OOP costs. Appendix Figure A.3 performs a similar analysis on wait times to highlight that the bill’s arrival time also displays large variation even conditioning on these fixed effects; we observe that the average (median) absolute residual in the time spent between the service date and the paid date is 3.4 (2.7) weeks.

Figure 2. Quantifying Bill Shock: Difference between Realized and Predicted Service Prices



Notes: Figures each show differences between realized prices and predicted prices across all shoppable services in analytic data. Predicted prices use regression on year, week of year, service type, and provider fixed effects. Panel (a) shows differences in total billed costs, while panel (b) shows differences in OOP payments. Figures are truncated with absolute differences below \$1,000 (all measured in 2022 USD).

This evidence motivates the ideal comparisons underlying our proposed estimation. To test for bill effects on household medical decision-making, an ideal experiment would compare households who received the same service on the same date from the same provider. These households would implicitly have the same *ex-ante* price distribution; however, one household would randomly receive their bill (their *ex-post* price realization) faster than the other. Direct comparison of household behaviors during this time when only one household has received their bill would identify estimated bill effects.

3 Methods

An important feature of delayed pricing information is that household responses to shoppable services necessarily take place in two stages: first, households must respond to the event based on *expectations* about costs relative to a deductible; second, those responses evolve only after new information—in the form of a bill or EOB—resolves uncertainty. We leverage these two distinct periods in a triple-differences regression framework to estimate spillover

responses *separately* for the periods before and after a bill’s arrival.¹⁹

We estimate the causal impact of a bill on total *spillover* spending for all household members excluding the focal consumer of the shoppable service.²⁰ We estimate how a bill affects total spillover spending (measured per week per household-member) in household i at week t of year y as given by Equation 1:

$$\mathbb{E}[\text{spend}_{ity}] = \exp \{ \beta_1 \mathbb{1}(\text{post_service}_{ity}) + \beta_2 \mathbb{1}(\text{post_bill}_{ity}) + \alpha_{\mathcal{I}} + \tau_t + \delta_y + \mu_S + \xi_{\text{MD}} \}, \quad (1)$$

where the two main regressors are indicators for the consumption of a shoppable service and, subsequently, receipt of a bill. We consider robustness to controlling for various time-invariant fixed effects, including for households ($\alpha_{\mathcal{I}}$), years (τ_t), relative week of the year (δ_y , to account for within-year seasonality in spending), index service type (μ_S) and provider fixed effects (ξ_{MD} , for the providers performing the focal service).²¹

Since our results are consistent across these specifications, the identifying variation driving our results comes from *idiosyncratic variation* in billing delays within an organization-service level at a given time. That is, we assume that bill delays are exogenous conditional on household, time (year and week of year), event type, and provider fixed effects (consistent with the descriptive evidence discussed in Section 2.4). This assumption maps intuitively to the thought experiment described above, comparing households consuming the same service from the same provider at the same point in time, but with different wait times for pricing realizations. As households and providers have no control over their bill wait times at this level, this exogeneity assumption is plausible; we discuss potential identification threats below.

We use Poisson regression to estimate multiplicative effects on spending. This allows us to deal with the skewed nature of our (non-negative) spending data while appropriately including weeks with zero spending.²² Our estimator will be consistent as long as the con-

¹⁹We refer to this as a “triple difference” regression because of the multiple margins of treatment arising from the bill’s delay post-service. That is, a truer expression of Equation 1 as a DDD regression is $\mathbb{E}[\text{spend}_{ity}] = \exp \{ \beta_1 \mathbb{1}(\text{service} \times \text{post})_{ity} + \beta_2 \mathbb{1}(\text{service} \times \text{post} \times \text{bill})_{ity} + FES \}$, with the bill arrival dividing the post-treatment period into two.

²⁰There is strong evidence that individual health events generate spillovers affecting spillover utilization, including for this data (Hoagland, 2022; Fadlon and Nielsen, 2019). There may be situations where information is not shared fully across a household (for example, young adults in the household covered on a plan, but no longer living at home). This would tend to bias our estimated results towards zero, a problem discussed in other work (Kowalski, 2016b).

²¹We use the notation \mathcal{I} to indicate the household of individual i .

²²Before consuming any shoppable services, an average of 31% of households consume any health services in a given week, spanning inpatient and outpatient care and pharmaceutical purchases. Hence, at least *ex-ante*, we do not expect that our regressions suffer from too many zeros, which might make it difficult to detect treatment effects given small variations in bill timing (Wooldridge, 1999). Our results are also robust to zero-inflated Poisson methods.

ditional mean of the dependent variable is correctly specified, as is the case in ordinary least squares (OLS) regression; additionally, Poisson regression allows us to avoid the inconsistency of regression coefficients induced by heteroskedasticity in a log-linear transformed model (Santos Silva and Tenreyro, 2006) and concerns from nonlinear transformations of the dependent variable (Mullahy and Norton, 2022).²³

Our parameter of interest, β_2 , is identified from billing delays; however, one may be concerned our estimator also reflects time-varying responses in utilization relative to the initial medical event. Given that the shoppable service may affect both the focal individual and others in the household to the extent that it represents a health shock, one might be concerned that β_2 reflects a general downward spending trend for the family in event time as the shock fades. We address this in two ways: first, we directly estimate dynamic treatment effects in Section 4.2, accommodating time-to-event dummies in a typical “event-study” design; second, we show throughout that our results are robust to the inclusion of linear time trend controls before and after the index event (Appendix Tables A.3 and A.4). Our estimates are unchanged across specifications, possibly because shoppable events do not generally reflect health “shocks” affecting a household in the way unplanned or more expensive shocks do (Fadlon and Nielsen, 2019; Hoagland, 2022); hence, there is no evidence of a downward trend in spending between the event and the bill’s arrival.

3.1 Testing for Possible Selection into Bill Delays

A critical identifying assumption of our parameter of interest ($\beta_{\text{post_bill}}$ in Equation 1) is that a bill’s arrival is exogenous at the household level. Previous work has highlighted the potential endogeneity inherent when attempting to estimate demand elasticities to major health events, especially for planned consumption (Duarte, 2012). We do not estimate demand elasticities in these models (that is, $\beta_{\text{post_service}}$ is not a demand elasticity); instead, we measure how the total volume of household responses—including both strategic and non-strategic responses—change following a bill. Therefore, our estimation has no potential endogeneity concerns, as long as bill arrival times are exogenous to the household.

A first-order concern for this exogeneity is that bill wait times may be associated with underlying patient risk, which potentially introduces selection bias. For example, if a claim for a more medically complex patient takes longer for insurers to process (even within procedure types), waiting times may be systematically longer for the most at-risk patients in our sample. This could artificially inflate our regression coefficients if risk is correlated across households and riskier households spend more on average.

²³Regressions use “ppmlhdf” to handle high-dimensional fixed-effects (Correia et al., 2020).

However, we find no evidence that billing delays are associated with patient risk. We can test these claims directly by comparing the price an individual paid for a service and the time they waited for their bill. We regressed spending measures on time bins for bill delays of 5 days each (that is, a wait time of 0–4 days, 5–9 days, and so on), adjusting for year, week of year, event type, and provider fixed effects (Appendix Figure A.4). We would expect to observe significant relationships in the coefficients for these bins if there were correlations between more medically complex patients and billing delays. We do not observe any such relationships for either total cost or patient OOP.²⁴

We also present comparisons of the trends in billing delays across procedure types in Appendix Figure A.5. Across spending measures, we consistently find a lack of correlation between the cost of a procedure and wait times. This is true even for services with the largest variation in expected costs, such as vaginal deliveries and arthroscopy.²⁵ Taken together with the regression results above, these findings confirm that patient health status appears uncorrelated with the amount of time for a claim to be adjudicated.

Finally, a remaining concern is that intra-household correlations in health status may bias our results (Meyler et al., 2007; Cawley and Ruhm, 2011). Given that the shoppable service claim could be plausibly correlated with the health needs of other household members, households with more costly focal services may also tend to have greater healthcare spending than other households. Hence, if these households also wait longer for their bills (for example, because waiting times are endogenous to health), we may overestimate a bill’s effect on spending due to simply the prolonged waiting period for high-spending households. However, this concern arises only if there are remaining endogeneity issues between bill delays and size, as discussed above; otherwise, level differences in healthcare spending needs across households are adjusted for in Equation 1 using standard parallel trends assumptions and household fixed effects. We would only overestimate treatment effects in this context if affected household members’ health were to idiosyncratically worsen between the (planned) shoppable service and the bill, an unlikely outcome given the short time intervals analyzed.

²⁴Results are robust to measuring bill size in levels or relative to an individual’s average pre-event spending (percentage changes). Results are also robust to excluding physician fixed-effects, addressing the potential concern that perhaps physicians, rather than patients, drive bill delay in strategic ways.

²⁵Appendix Figure A.5 shows shoppable services with total costs under \$1,000 may take about 3 days longer to be processed; these constitute less than 4% of services in our data. This effect runs in the opposite direction of the initial concern: if riskier/more expensive patients receive bills faster, we would not expect to find large swings in spending attributable only to higher-spending households waiting longer for their bills. Finally, throughout the paper, our results are robust to controlling for procedure-specific time trends, to accommodate any concerns about variation in bill wait times across shoppable service types.

4 Empirical Results

4.1 Effect of Bills on Spending

We investigate how households exhibit differential spending responses to an index service before and after a bill’s arrival, estimating Equation 1. Table 2 presents the regression results: we find robust evidence that although spillover spending increases after a focal member receives care, a medical bill’s arrival causally affects these responses. Without including information on bill arrival timing, the overall spending increase is roughly 40.2% of average weekly per-person household spending (about \$48 per person-week). Such spending increases are consistent with prior literature but may be driven by a number of factors including correlated health within households (Meyler et al., 2007), strategic delays in seeking care until after a high-cost event (Cabral, 2017b; Kowalski, 2016b), or responses to new information about health systems or health risk (Hoagland, 2022).²⁶

	Main Models		Alternative Specifications			
Post Service	0.402*** (0.0032)	0.218*** (0.0032)	0.597*** (0.0032)	0.472*** (0.0032)	0.486*** (0.0033)	0.464*** (0.0032)
Post Bill		-0.109*** (0.0030)	-0.080*** (0.0030)	-0.096*** (0.0030)	-0.076*** (0.0031)	-0.077*** (0.0030)
$\overline{\text{spend}}_{it}$	\$120.49	\$120.49	\$120.49	\$120.49	\$120.49	\$120.49
Household FEs	X	X	X	X	X	X
Year FEs	X	X		X	X	X
Week of Year FEs	X	X			X	X
Provider FEs	X	X				X
Event Type FEs	X	X				
Observations	61,860,735	61,860,735	61,860,735	61,860,735	61,860,735	61,860,735

Notes: Table presents triple-difference Poisson regression estimates for a bill’s effect on households’ spillover health spending. Focal consumers are excluded from the outcome. Regression coefficients illustrate the expected change in log household spending (measured per person-week) associated with the service date and bill arrival (both measured as dummy variables). Throughout, standard errors are clustered at the household level. For robustness, see Appendix Tables A.3 and A.4.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2. Estimated Impact of Bill Arrival on Household Health Spending

Surprisingly, our results suggest bills meaningfully affect spillover spending. Before pricing information, we estimate spillover spending increases by 21.8% post-service in our preferred specification; however, a bill causes reductions in this increase by 10.9% from baseline,

²⁶Note that this spending is normalized to the per-person level, and results are robust to excluding newborns (following childbirth) from the sample.

roughly half of the increase. This decline is consistently estimated across specifications and robust to multiple approaches.²⁷ Bill effects amount to spending reductions of \$13 per person per week for the average household, or about \$317 in per-person annual spending.

We also test to ensure that our results are robust to potential measurement error in the date bills actually arrive, given our use of plan payment dates as a proxy. If plan payment dates are unrelated to household information, we may be simply splitting the post-service period into two random periods and attaching significance to spurious differences between them. To test this possibility, we conducted placebo tests, estimating Equation 1 on artificial data that randomly assigned new wait times based on the empirical distribution of bills.²⁸ The results of 1,000 placebo regressions are reported in Appendix Figure A.6; placebo coefficients are centered close to zero and generally indistinguishable from a null effect. Taken with the results from Table 2, this suggests that it is unlikely that our results are spurious correlations from a semi-random splitting of the post-service period.

Finally, we show that these results are not dependent on the choice of shoppable services as the index event, but that bill effects are present in a broader set of events including unplanned hospitalizations for injuries and appendix surgeries (Appendix Table A.2). Even in an expanded set of events—which now encompasses over 200,000 index events and 126 million individual-week observations—we continue to find robust evidence that household members reduce their spending when a bill arrives. Estimated effects are 7.4% in the unplanned hospitalization sample, compared to 10.9% in the main specification, and are robust to pooling across both samples or further separating injuries and appendix surgeries. Interestingly, we note that the post-service coefficient approximately doubles in value when considering planned index events; this supports the hypothesis that households engage in strategic behavior, co-planning their healthcare utilization conditional on the realization of some health needs such as shoppable services. However, this strategic behavior does not appear to drive the responses to a bill’s arrival.²⁹

²⁷Alternative approaches include (a) incorporating procedure-specific fixed effects and time trends, allowing for potential differences in household behavior following different types of shoppable services (Appendix Table A.3); (b) excluding household fixed effects; (c) excluding the small fraction of households who switch plans soon after a shoppable service (i.e., whose spending is censored at the end of the plan year); and (d) controlling for linear time trends before and after the service or dynamic treatment effects for the shoppable service independent of bill arrival time (Appendix Table A.4).

²⁸For each shoppable service, we fixed the service date and artificially varied the bill arrival date as the service date plus a random draw of a wait time, drawn from the empirical distribution of waits (Figure 1).

²⁹One may be concerned that the gap between the point estimates in bill effects is indicative of differential household responses to “planned” health shocks (shoppable services) rather than to more truly unplanned shocks (injuries). We examine this in two ways in Appendix Table A.5: first, by assessing how results change when we compare extensive margin effects to continuous changes in spending, and second, by disaggregating bill effects across several categories of spillover spending, including unplanned ED visits and planned elective surgeries. We do not find evidence that this difference in coefficients is consistently estimated across specifi-

4.2 Dynamic Treatment Effects

We next assess the dynamic treatment effects induced by a bill’s arrival using a two-way fixed-effects (TWFE) approach. The static estimation (Equation 1) inherently estimates many pairwise comparisons between households who have received a bill and households still waiting for one; to the extent that these pairwise comparisons are different at different points in relative time due to heterogeneous treatment effects, our static estimation procedure may lead to biased regression coefficients (Goodman-Bacon et al., 2022). For example, if there are larger bill effects in the short-run that dissipate quickly, our static comparisons may over- or under-report the true effects depending on the distribution of these comparisons in our sample. To address these concerns, we estimate a dynamic version of Equation 1.

Such an approach is complicated by the fact that there are two events in the triple-differences specification with potentially differential timing: the consumption of the index event and the bill’s arrival. As we are interested only in the effects of the latter, we estimate dynamic effects on a matched sample of households consuming the same service at the same time and organization. Once this matched sample is constructed, we estimate the differential impact of the first bill to arrive on household spillover spending, compared to households who had similar index service experiences but who have not yet received their bill.

We use a nearest-neighbor matching process at the household level, matching on index service type, provider, year, and event date. The final matched sample consists of 35,734 matched sets, with an average (median) of 6 (4) households in each set. That is, 214,404 of the 368,240 (58%) households are represented in the matched sample. Once matched, we compare the effect of the first bill’s arrival within a group on spillover household spending using a TWFE version of Equation 1:

$$\mathbb{E}[\text{spend}_{it_y}] = \exp \left\{ \sum_{k=-T}^T \gamma_k \mathbb{1} \{t - E_{it} = k\} + \alpha_{\mathcal{I}} + \tau_t + \delta_y + \mu_{\text{Service}} + \xi_{\text{MD}} \right\}, \quad (2)$$

where E_{it} indicates a set of time-to-first-bill dummies within the matched groups, with the week before the first bill’s arrival as the omitted reference group.³⁰

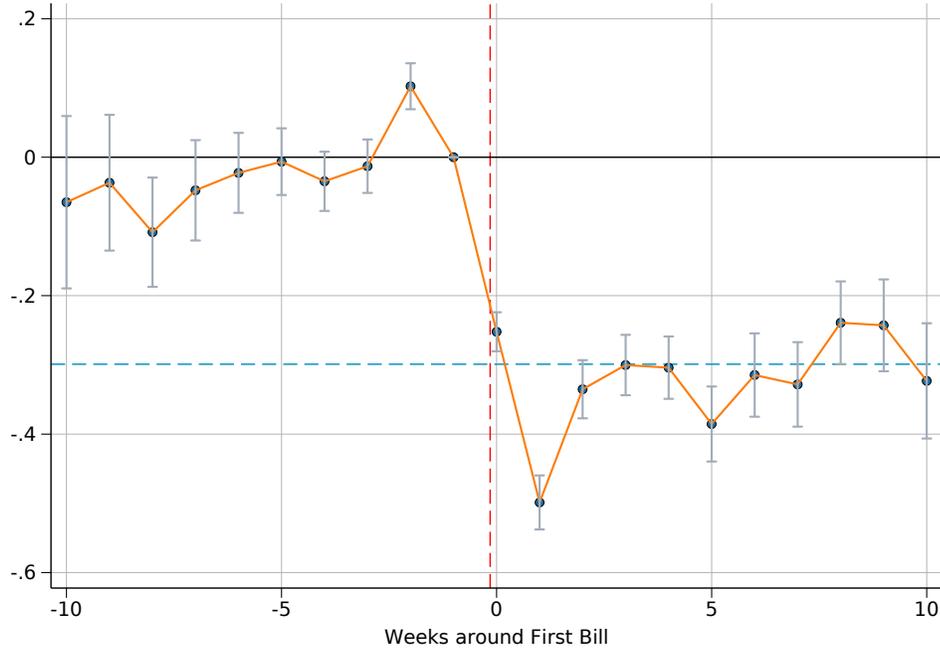
Figure 3 presents the results. The pre-trend period—defined as the time between the index service consumption date and the first bill’s arrival—reflects very little change in household spending across groups.³¹ Beginning in the week the bill arrives, affected household

cations, meaning that there is not evidence of consistently stronger bill effects in response to shoppable focal events compared to unplanned injuries.

³⁰Note that by virtue of the matching process, these results are virtually consistent across the set of fixed-effects included in the regressions.

³¹There is some evidence of differential spending patterns two weeks prior to a bill’s arrival, with early-treated households increasing their spending slightly before a bill arrives; however, note that this moves in

Figure 3. Dynamic Treatment Effects of Bill Arrivals on Household Spending



Notes: Figure shows estimated coefficients and 95% confidence intervals for the TWFE regression estimated in Equation 2 on the matched sample, as discussed in Section 4.2. Standard errors are clustered at the household level.

members decrease their spending by over 20%, with average decreases of approximately 32% over the following weeks. This is consistent with the 32.8% reduction in spending between the post-service and post-bill periods estimated in Equation 1 (Table A.4).³² In particular, the strongest effects are observed in the first week immediately following the bill, where spending is estimated to decline by roughly 50% relative to the post-service period. However, note that the average untreated household in this matched data waits an additional 3 weeks after the first bill’s arrival for their own bill; hence, there is substantial attrition across the time periods limiting the ability to extrapolate long-term dynamic treatment effects.

Given the variation in the timing of index service consumption, as well as the possibility for heterogeneous treatment effects, we consider the robustness of our regression results from Equation 2 in the context of the novel staggered treatment design estimators (Goodman-Bacon et al., 2022). In particular, we follow the approach of Wooldridge (2021), the only updated TWFE specification to be shown to be robust to nonlinear modeling such as Poisson

the opposite direction from a pre-trend that would bias our result, and hence is likely attributed to noise in the reduced sample.

³²Note that Equation 2 mechanically includes linear time trends around the time of the event; hence, we compare the coefficients to the version of Equation 1 that included these linear time trends.

regression. This approach incorporates a fully flexible regression capturing full heterogeneity in treatment effects by including household/year/week-of-year interactions in the main model. By doing so, the ATT can be consistently estimated, including with dynamic weighted averages in the style of Equation 2. We estimate this approach and compare our results in Appendix Figure A.7; we do not find significant differences across our estimates. In particular, the Mundlak estimator reveals a similar pattern of spending reductions following a bill’s arrival; this estimator, if anything, finds stronger spending reductions than a typical TWFE framework, estimating an average treatment effect closer to a 60% reduction in spending.³³

Given the differences in sample construction and estimation, it is important to use caution when interpreting the estimated dynamic treatment effects in the context of the broader results of both our reduced-form evidence and structural approach. In particular, this estimation does not include any pre-service spending as a control group (including years in which households did not consume an index service). Similarly, the interpretation of dynamic treatment effects is limited by the relatively short wait times between services and their bills, as well as relatively short differences in wait times across households consuming similar services. Despite these caveats, however, the matching and event study approach corroborates our earlier findings that the arrival of a bill meaningfully changes household health consumption in the short run, with particularly strong effects immediately after a bill’s arrival.

4.3 Pricing Information as a Mechanism for Bill Effects

Our results show robust evidence that households change their spending patterns following a bill’s arrival, observable in the raw data as well as static and dynamic regression specifications. We next turn to a discussion of the potential mechanisms generating these effects.

Bills may affect the healthcare utilization of households for multiple reasons. Perhaps most saliently, households may learn about (OOP) prices for services; pricing information may affect spending decisions either through updated beliefs about household cost-sharing or due to distortions from liquidity constraints. However, other features of a bill—unrelated to prices—may also contribute to observed results. A bill may provide information about whether contracts (do not) cover specific procedures or include certain providers in their net-

³³Note that estimating the Mundlak estimator in this large dataset with high-dimensional fixed-effects Poisson regression is quite computationally intensive; hence, results here are presented only for on a 50% sample of the (matched) data, which has already been significantly reduced in size from the full analytic dataset based on the matching process. Hence, the discrepancy between the regressions likely arises from (a) differences in comparisons of heterogeneous treatment effects where a typical TWFE regression places more weight on less likely comparisons (such as households who wait more than 6 weeks for a bill) and (b) sampling differences across the two estimators.

works, or reveal discrepancies between a patient’s understanding of a service and a provider’s billing (for example, up-coding practices). This information may alter future healthcare spending if it erodes trust in the healthcare system (Webb Hooper et al., 2019). Finally, bill effects may be driven by a focal *provider* rather than the household, who may respond to insurance denials or low reimbursement rates by affecting the provision of future services.

We consider each of these potential mechanisms in turn. First, we show that households are most responsive to bills that provide meaningful pricing information, particularly when viewed through the lens of a household’s deductible. Second, we show that households exhibit less bill shock after repeated health events, suggesting households learn from bills and update their beliefs accordingly. Finally, we show that our results are not driven by alternative concerns including liquidity constraints and provider-driven effects.

Even considering all the information contained in a bill, however, may not be sufficient to resolve all pricing uncertainty at the household level. Residual uncertainty may persist to the extent that patients are not given negotiated prices for services, do not know which of their services are subject to their deductible, or may be under-informed about how deductibles or cost-sharing more generally work in their contexts. In each of these cases, household decision-making may be affected by persistent uncertainty even after the bill arrives.

4.3.1 Heterogeneity Across Deductible Contributions

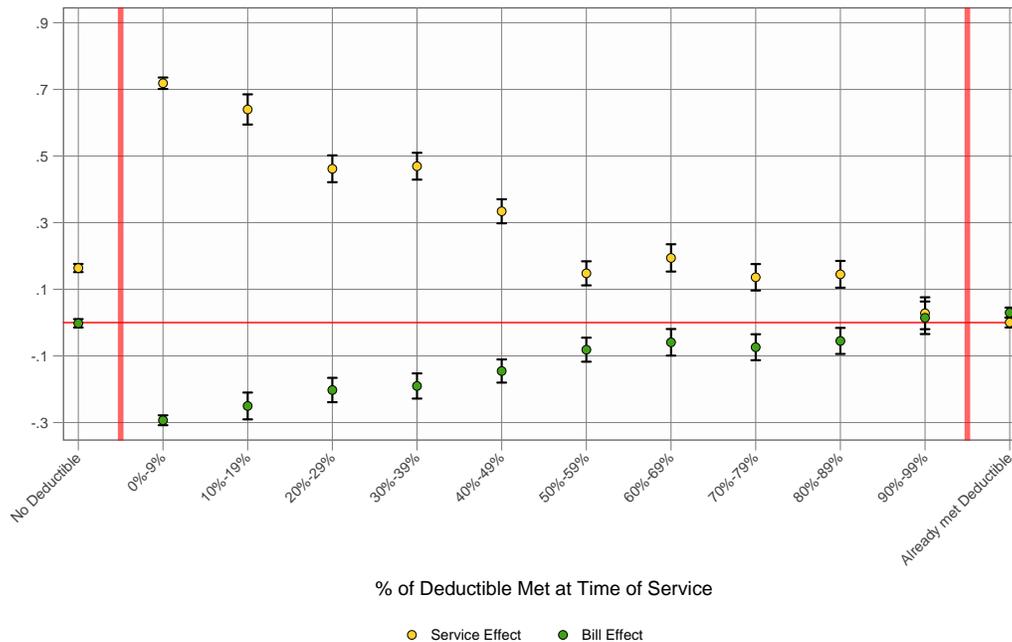
We first assessed how bill effects differed across household spending histories, relying on the intuition that these households may find bills differentially informative.

Figure 4 presents results stratified by decile of household deductible spending prior to the event.³⁴ The spending responses for both the interim period (yellow) and the post-bill correction (green) are shown. Both responses are largest for households who have spent little towards their deductible before the event: post-service spending increases are estimated to be over 70% for households with less than 10% of their deductible met and become statistically insignificant for households close to their deductible. Correspondingly, post-bill corrections are estimated to be as high as 30% (nearly 50% of the post-spending increase) for households with less than 10% of their deductible met; these effects similarly converge to precise zeroes for households close to meeting their deductibles. Finally, households whose marginal cost is not bill dependent—including those in zero-deductible plans as well as those with no remaining deductible—exhibit no responses to bills.³⁵

³⁴Appendix Figure A.8 shows the equivalent versions of Figures 4 and 5 measured in levels (dollars) of deductible remaining rather than percentages. Similar to the figures presented in the text, the appendix figures show bill effects declining with remaining deductible amounts, and a clear discontinuity in bill effect magnitudes for index events that push households across their deductible.

³⁵Even among those enrolled in zero-deductible plans, there is an average post-service spending increase

Figure 4. Heterogeneous Bill Effects Across Household Deductible Status at Time of Service



Notes: Figure shows estimated coefficients and 95% confidence intervals for $\mathbb{1}\{\text{Post_Service}_{it}\}$ and $\mathbb{1}\{\text{Post_Bill}_{it}\}$ in Equation 1 by decile of household deductible spending prior to the event. Standard errors are clustered at the household level. Regressions adjust for total size of bill and linear time trends by group. Deductibles are imputed based on previous literature (Hoagland, 2022; Zhang et al., 2018).

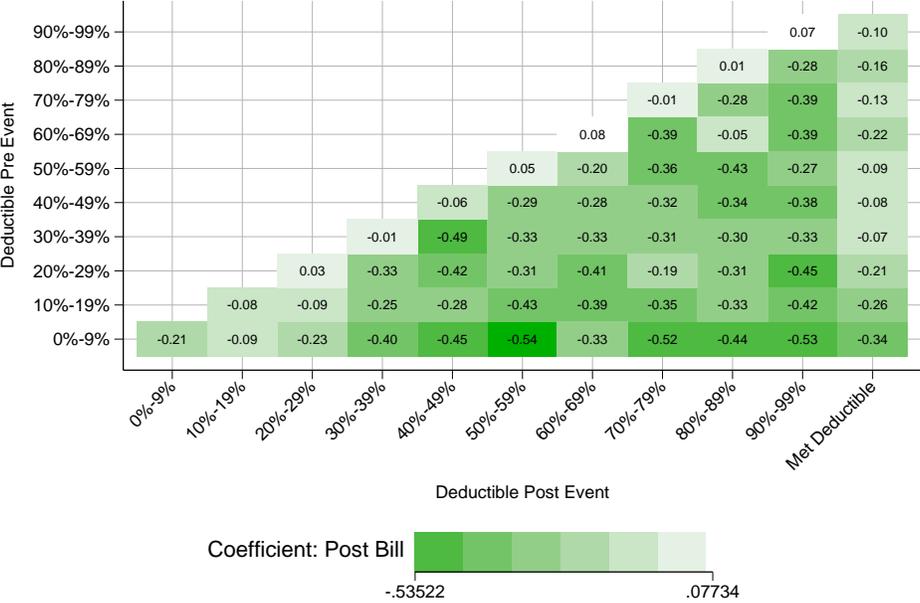
These results suggest that households find bills especially relevant when they contribute meaningful information about future expected cost-sharing; these effects are magnified for households who have yet to contribute much to their deductible, who may be relatively under-informed of their spending relative to a deductible.

While variation in pre-event spending provides useful information, further insight can be gained by leveraging a second dimension of variation: the relative OOP cost of the shoppable service itself. Exploring these two dimensions simultaneously leverages the fact that bills may be uninformative for specific plan types (e.g., those with zero deductible, as shown), but may also be more or less informative depending on where households end up on a

of roughly 15%. This could be due to the fact that household health is correlated across members (Meyler et al., 2007; Cawley and Ruhm, 2011), so that household members may jointly increase health spending even without taking into account changes in marginal prices. However, these results also suggest that while households unaffected by changes in prices may increase spending post-service, these changes are not reflected in post-bill effects, as this coefficient precisely overlaps zero. This makes intuitive sense given the quasi-random timing of bills; as post-service increases—even due to correlated household health—are randomly distributed over post-service time, they are not affected by the quasi-random timing of the bill’s arrival in settings where that bill did not provide meaningful pricing information.

cost-sharing curve. Price information is most valuable to households when it communicates whether they have crossed the threshold of their deductible; hence we identify the salience of pricing information—separate from other forms of learning—by comparing household responses to high- and low-cost events, considering both pre-event deductible contributions *and* the resulting change in deductible spending after the scheduled health consumption.

Figure 5. Heterogeneous Bill Effects By Household Deductibles and Service Cost



Notes: Figure depicts coefficients for $\mathbb{1}\{\text{Post_Bill}_{it}\}$ in Equation 1 across deciles of household deductible spending prior to *and* following events. Sample is restricted to those with non-zero, unmet deductibles. Each row indicates a different decile of deductible spending prior to the event, while each column indicates deciles following the event. Regressions include linear time trends by group as additional control for overall bill size. Standard errors are clustered at the household level.

Figure 5 presents the results. We restrict our attention to households with a non-zero, unmet deductible at the time of service, and separately estimate Equation 1 across cells of households who have similar deductible spending both before and immediately following the shoppable service. The figure depicts a two-way heat map of estimated bill responses across cells.³⁶ Consistent with Figure 4, we find that households starting at lower levels of their deductible exhibit greater sensitivity to their bill; in the figure, this is seen by comparing bill responses as one moves down a given column. We directly observe that the OOP cost of a service changes responses; moving to the right for a given row, we observe coefficients much closer to zero for households that do not move across deciles of spending, but that weakly increase as the service becomes more expensive.

³⁶For each regression, the control group is households who did not consume a shoppable service over the course of the year. See Appendix Figure A.9 for the corresponding figure of service effects.

Most notably, comparing household responses across the discontinuous threshold of meeting the deductible is particularly informative about mechanisms. Across all levels of pre-event spending, estimated coefficients are at least 55% higher when a bill left households just short of meeting their deductible, rather than services that pushed households into a lower marginal-cost region of their contract. The average effect of receiving a bill for the shoppable service declines by 39.5% from -0.42 to -0.17 . The effects on either side of the deductible threshold are statistically distinct and statistically indistinguishable from 0 on the right side of the cutoff (Appendix Table A.6).

Finally, we conduct two robustness tests to explore the relationship between a bill—with its communicated pricing information—and estimated demand effects. First, we explore how household responses to a bill vary based on the “bill shock” it communicates. We compare a household’s actual bill to its predicted value, as described in Figure 2, and group households based on the predicted differences. This analysis tests two features of our effects: first, bill effects should be largest for households with the largest bills relative to their expectations; second, we can directly examine households with smaller than expected bills. For these households, bill effects may go in either direction: households may have expected to pay more for a service, meaning that a bill constitutes a positive income shock and may therefore increase demand; on the other hand, negative bill shock also implies that households are further away from their deductible, meaning that marginal expected prices may have increased, reducing demand. In Appendix Figure A.10, we show that bill effects are increasing in (positive) bill shock—households with the largest bills relative to their expectations have bill effects twice as large as the baseline results.³⁷ On the other hand, we observe negligible bill effects for bills that are close to or below their expected price. To the extent that predicted bill prices are accurate, this result may be driven by households responding more to expectations about marginal prices of care than the income shock inherent in a bill; on the other hand, unobserved private information—such as a focal patient’s health or the severity of their event—may contribute to measurement error in bill shock, potentially limiting the interpretability of these results.

Second, we perform a falsification test exploring the impact of a bill’s arrival for households who have already met their OOP max prior to the index event. The intuition for this test is that not only should the index service be free to the household, but households who have met their OOP max before the event should not expect the bill’s arrival to convey any new information about their spending patterns or outstanding payments. Hence, we do not expect that a bill’s arrival—or, for that matter, service consumption—should mean-

³⁷Note that we use quantiles here, instead of deciles, in order to maximize the probability of detecting significant results even for households with small amounts of bill shock.

ingly alter the spending of other household members. This is exactly what we find; the results are reported in Appendix Table A.7. In alternative specifications, we observe *positive* bill effects for this subsample; however, once we condition on the fixed effects used in our primary specification (reflecting the identifying assumptions required for quasi-random bill arrival timing), we do not observe any significant changes in household consumption after a bill arrives, if the OOP max has already been met.

Overall, these findings illustrate that households are much more responsive to a bill when it contains important information about future marginal costs, consistent with price sensitivity driving household responses. Accounting for heterogeneity in household responsiveness to bills—including both spending histories and the cost of services—suggests that households are most responsive to a bill when its pricing information is highly relevant. These findings are consistent with a model where consumers overestimate their progress towards crossing a cost-sharing threshold, such as a deductible. Once a medical bill provides definitive information about contributions, however, individuals correct their spending in response to information that they are not as close to these discrete changes in marginal costs as they initially anticipated.

4.3.2 Evidence of Household Learning from Bills

Although these results suggest households respond to more informative bills, it may not be that households learn from bills over time. Hence, we assess belief updating in two ways: first, by showing that households respond differently to bills for an initial index event compared to subsequent ones; and second, by considering how households exhibit forward-looking behavior in bill responses.

First, we consider how bills impact households differently for an initial shoppable service than for subsequent consumption, defined as index services that the household consumes in later years.³⁸ Table 3 presents the results. We observe spending corrections from a bill’s arrival only for *initial* shoppable services, with negligible effects for subsequent exposures. On the other hand, we continue to observe significant—although muted—post-service responses for households even for later services.³⁹

Second, we consider how bill responses vary across a calendar year; this, importantly, tests whether households are forward-looking in their response to bills. Households may

³⁸We limit attention to households who experience at least two shoppable events across different years in order to avoid capturing forward-looking behavior in this exercise.

³⁹One might be concerned those with repeated events are higher-spending and higher-risk on average. We do not observe this to be true on average (Table 3); in addition, these results hold even considering childbirths as the only shoppable service. In this case, risk is less likely to be a predictor of multiple births, yet we observe bill effects are still reduced for subsequent deliveries compared to initial ones.

	Full Specification	Repeated Services
Post Service	0.218*** (0.0032)	0.268*** (0.0058)
Post Bill	-0.109*** (0.0030)	-0.083*** (0.0056)
2 nd Service * Post Service		-0.176*** (0.0070)
2 nd Service * Post Bill		0.118*** (0.0071)
$\overline{\text{spend}}_{it}$	\$120.49	\$135.78
Observations	61,860,735	29,344,385

Notes: Column 1 presents results from Table 2. Column 2 restricts treated group to households with ≥ 2 shoppable services in different years. Throughout, standard errors were clustered at the household level, and regressions included family, year, relative week of year, and provider (of shoppable service) fixed effects.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3. Reduced-Form Evidence of Learning

react to bills’ pricing information either as it conveys information about spot prices or as it updates household beliefs about end-of-year prices (Aron-Dine et al., 2015). Examining how households respond to services based on the time they are consumed sheds light on how households weigh these different prices. Figure A.11 presents results from stratifying our sample by the week a shoppable service was consumed.⁴⁰ We observe a strong gradient: households respond much more to services consumed in the first quarter of the year than in the last quarter. This is likely mechanical, given that as a year progresses, more households will have crossed their deductible thresholds, weakening or eliminating any incentives to respond to cost-sharing for shoppable services towards the end of the year. However, it does suggest households are more responsive to “deductible-crossing events,” and have incentives to attempt to strategically delay some care until after significant health events.

On the other hand, the bill effects presented in panel (b) of Appendix Figure A.11 are constant across the calendar year, except for the final 7 weeks of a year, in which the effects dissipate.⁴¹ This suggests that although households’ strategic behavior evolves over a year, bill effects correcting misperceptions in that behavior are constant across the year. These

⁴⁰Results are comparable if instead, we stratify by the week a bill arrived.

⁴¹Most likely, this is due to the reduced information content of a bill arriving at the end of the year, given that plans and marginal prices reset at the beginning of January. This may also be influenced by the increased difficulty of scheduling services towards the end of a plan year (Shukla et al., 2021).

findings are interesting, given that they show bill responses are not attenuated over a year by changes in forward-looking household behavior.⁴²

4.3.3 Considering Alternative Mechanisms

The evidence presented above suggests households respond to the pricing information contained in a bill *and* update their beliefs about how spending affects future cost-sharing; however, additional mechanisms may affect our estimates of the bill effect. In particular, a remaining concern is that household spending decisions may be particularly affected by liquidity constraints (Gross et al., 2022). Households facing liquidity constraints may respond to a bill by curtailing their medical spending not because they corrected beliefs about their position on a cost-sharing curve, but simply because information about prices—particularly if those prices were under-estimated—may affect household purchasing power.

Although our data does not provide us with rich information on household incomes or consumption, we follow the approach of previous literature to investigate whether variations in the timing of expected household income shocks may change bill effects (Gross et al., 2022). We examine heterogeneity across the week of month of a bill’s arrival, relying on the intuition that if income effects drive our results, bills arriving just before the end of the month should have stronger observed effects than those arriving near the beginning. We do not find evidence that this is the case (Appendix Table A.8). Household medical spending increases in the middle of a month, with spending in the second week of the month approximately 50% higher than spending at the end of a month; this is consistent with income affecting the timing of household health purchases. However, the corresponding bill effects are statistically indistinguishable from one another, suggesting that households reduce their spending by a consistent amount across a month. This evidence is largely suggestive, however, and should be carefully considered in future research with billing and income data.

Finally, observed bill effects may be driven by a single provider, rather than by changes to household demand. However, we observe that our results are robust to excluding all spillover claims obtained from the provider of the index service (e.g., a general practice physician or a hospital system). Appendix Table A.9 reports strong bill effects even after removing these claims, suggesting that bill effects are driven by households rather than physicians.⁴³

⁴²Importantly, each of the firms with plan identifying information in our sample has a plan start date of January 1; hence, we are unable to perform the relatively simple test of re-running our analysis on a subsample of affected households whose deductible contributions are not weakly increasing over the calendar year. This is an interesting exercise for future work in this area.

⁴³Note that this analysis has several limitations. First, we are not able to identify all providers of shoppable services, and there is little documentation for why MarketScan data has some provider IDs missing and not others. Second, pharmaceutical spending cannot be reliably assigned to a physician identifier. Finally, control group years without an index service are omitted from this calculation.

4.4 What Services Are Affected?

4.4.1 Do Bills Only Affect Strategic Delaying of Services?

One important question when considering our results is whether bill effects merely represent delays in the use of care or a more fundamental change in the quantity and type of medical care households seek. For example, a bill informing households they have yet to meet a deductible may generate a new wave of strategic delays, where services are further postponed until cost-sharing thresholds are crossed. However, we find evidence that households alter *where* they seek care even for services that cannot be strategically delayed.

	Regression Coefficients		Pre-Treatment Averages	
	Post Service	Post Bill	% ≥ 0	Conditional Mean
All Spending: Injuries and Infections				
Total Bill Effect	0.151*** (0.0128)	-0.014 (0.0131)	3.6%	\$251
Physician Office	-0.008 (0.0087)	0.040*** (0.0090)	3.2%	\$127
Hospital Campus (incl. outpatient)	0.258*** (0.0199)	-0.040* (0.0201)	0.5%	\$996

Notes: Table presents estimated coefficients evaluating bill effects on non-strategic health spending (Appendix Table A.11). “Physician office” includes outpatient non-hospital offices and urgent care centers; “hospital campus” includes outpatient hospital services, inpatient admissions, and emergency departments. Column (3) indicates the fraction of pre-treatment weeks with positive spending; column (4) presents pre-treatment weekly averages, conditional on positive spending. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4. Bill Effects on Care for Injuries and Infections

We define a set of such services spanning injuries—including broken bones, dislocations, and sprains—and common respiratory and gastrointestinal infections (defined in Appendix Table A.11). Using these services as an outcome (rather than an index event, as in Appendix Table A.2), we assess bill effects (Equation 1) and stratify by the place of service where care was administered. Table 4 presents the results, highlighting that post-service effects are entirely contained in hospital-based care for injuries (a 26% increase), while bill effects constitute a transition back to physician office-based care. This exercise is merely descriptive, as there are multiple hypotheses to consider. One possible rationalization of these results, however, is that households may perceive reduced financial barriers post-service to seeking care at a hospital until a bill provides additional information.

4.4.2 Heterogeneous Effects by Type of Care

Finally, we explore how bills affect consumption across broad categories of medical services, including hospital care, outpatient services, and pharmaceutical spending.⁴⁴ This decomposition allows us to examine whether household responses to a bill vary with any measure of perceived or real quality of care. Particularly, we examine how bills affect the future utilization of typically high-value health services such as preventive screenings or behavioral health services as well as typically low-value care such as unnecessary pre-operative screenings or imaging services.

Households consistently respond to bills by reducing consumption across a variety of services (Appendix Table A.10). Spillover use of inpatient services—including preventable hospitalizations—falls by roughly 17.0% post-bill; however, this effect is not significant ($p = 0.053$) and provides only suggestive evidence of effects on low-value inpatient services (Agency for Healthcare Research and Quality, 2007).⁴⁵ However, outpatient services—such as general practitioner and specialist visits, labs, and preventive care—exhibit stronger bill responses.⁴⁶ We do not observe bill effects among household demand for prescription drugs, perhaps because of already high pre-event levels of pharmaceutical consumption.

Surprisingly, we do not observe that households change their utilization of low-value care following a bill’s arrival. These services, which include services such as imaging for lower-back pain, misuse of prescription medications to manage migraines and bacterial infections, or unnecessary pre-operative screenings, are determined by the recommendations of the Choosing Wisely campaign (Colla et al., 2015). We find that households increase their use of low-value care by 6.6% following a major service, and then further by another 2.8% once the bill arrives. This may be a result of a “cascade of care” effect associated with increased consumption of general medical care, which in turn prompts downstream increases in physician ordering of low-value services (Ganguli et al., 2020). Physicians typically retain control over when low-value services are performed, in order to reduce their own uncertainty, liability, or “just to be safe” (Colla and Mainor, 2017).

⁴⁴Appendix Table A.12 includes detailed descriptions of the construction of each of these variables.

⁴⁵Whether this increase is an over-utilization of unnecessary care or simply increased access to relevant hospital services—particularly considering the “layperson standard” for hospital care—is an open question which warrants future research (Siegfried et al., 2019).

⁴⁶Some outpatient services, such as chiropractic care, are affected neither by the consumption of a shoppable service nor its accompanying bill; others, such as behavioral health services, exhibit the opposite pattern from the overall bill effects. This is presumably because these services have more inelastic demand and lower rates of cost-sharing generally.

5 Model

Based on the empirical findings from our reduced-form analysis, we propose a simple model of “imperfect” moral hazard. In this model, consumers with imperfect beliefs about prices and spending are price-responsive in demanding care; however, accurate pricing information may lag consumption by weeks or months while still affecting the spot prices of care in the interim. Given these delays, consumers form expectations about their realized OOP spending and the implied marginal cost of care in each period. The modeling exercise presented here is, therefore, a useful one when considering how models of moral hazard in healthcare may be augmented to incorporate the effects of delayed pricing information.

We focus our attention on noisy signals of household spending to construct a model of elastic demand for healthcare services under pricing uncertainty. That is, we assume that health shocks are perfectly observed in each period (Einav et al., 2013), leaving only patient OOP costs unknown. This uncertainty can be decomposed into two parts: first, consumers have uncertainty about the OOP spending they have incurred to date, due to quasi-random delays in receiving pricing information through bills; and second, consumers have residual uncertainty about price variation and plan coverage information that persists over time. We estimate the effect of resolving the first type of uncertainty through bill arrivals in order to match the reduced-form evidence presented above.

The model we present expands on the reduced-form evidence in three key ways. First, we estimate household bill effects across the full set of household services, rather than focusing exclusively on shoppable services as index events. The structural estimation of spending signals allows us to generalize beyond the previously narrow definition of index events. Second, the model proposes and estimates mechanisms underlying these effects. These include incorporating differences in household decision-making from noisy signals, but also the potential for households to both incorrectly perceive these signals and learn from them over time. Finally, the model allows for simple counterfactual exercises illustrating how correcting these errors may change decision-making. In particular, we consider how correcting estimated bias in spending signals is predicted to change household decision-making, and compare these results against eliminating all uncertainty in coverage and predicted healthcare costs. Doing so illustrates how one potential mechanism explaining our results—incorrect perceptions of spending relative to a deductible—fits into a much more complex decision-making process given the many uncertainties households face when consuming medical care.

5.1 Model Details

In each period t , an individual i receives a health shock λ_{it} , which represents a combination of both acute fluctuations in health status and persistent health needs. Patients then choose an appropriate level of medical spending m_{it} —measured in the dollar value of the services— in response to λ_{it} , spending histories, and individual preferences.⁴⁷ Following [Einav et al. \(2013\)](#), we calibrate individual patient utility as a quadratic loss function in the distance between selected health spending and the unobserved health shock:

$$u_{it} = (m_{it} - \lambda_{it}) - \frac{1}{2\omega_i}(m_{it} - \lambda_{it})^2 - c_{ijt}(m_{it}; M_{\mathcal{I}t}). \quad (3)$$

Here, ω_i is an individual time-invariant “moral hazard” parameter capturing individual heterogeneity in demand responsiveness to the price of services.⁴⁸ In addition, $c_{ijt}(m_{it}; M_{\mathcal{I}t})$ denotes the OOP costs associated with m_{it} , which depends on the piecewise-linear cost-sharing contract of individual i ’s chosen insurance plan, j , as well as the OOP spending to date at the household level, $M_{\mathcal{I}t} = \sum_{i \in \mathcal{I}} \sum_{k=1}^{t-1} m_{ik}$. Note that an individual’s OOP costs for services are weakly decreasing in $M_{\mathcal{I}t}$, given the cost-sharing structure.

Under full information, patients know both the value of $M_{\mathcal{I}t}$ and how it affects $c_{ij}(\cdot)$. Furthermore, in the case where cost-sharing is linear at all stages of the contract, a patient’s marginal OOP cost is given by $c_{ijt} \in [0, 1]$, where $c = 1$ applies to all services before a deductible has been met and $c = 0$ applies for all services after an OOP-max has been met. Between the deductible and the OOP-max, c is typically in the open interval $(0, 1)$. The optimal choice of m_{it} in each period is simply the solution to the first-order condition:

$$1 - \frac{1}{\omega_i}(m_{it} - \lambda_{it}) - c_{ijt} = 0 \Rightarrow m_{it}^* = \max[0, \lambda_{it} + \omega_i(1 - c_{ijt})]. \quad (4)$$

That is, medical expenses in each period are chosen so that the marginal utility of those services is equal to the marginal (known) OOP cost. In particular, as c changes from 1 to $c < 1$, individuals will have a discontinuous increase in their consumption.

We suppose that $M_{\mathcal{I},t}$ is not known with certainty at the time a service is performed. Rather, household spending can be divided into two components: spending for services whose bills have already arrived (where prices are known), and spending for services without

⁴⁷We assume shocks are measured in dollars to compare health production and OOP spending, consistent with previous versions of this model ([Einav et al., 2013](#)). This is useful as it is tractable and incorporates rational responses to nonlinear pricing; individuals close to a deductible will choose to slightly increase their consumption, anticipating the approaching nonlinear change in marginal costs ([Marone and Sabety, 2022](#)). To be consistent with Section 4, we model spending choices at the per-person week level.

⁴⁸The individual-specific moral hazard parameter ω_i has a helpful interpretation as the incremental spending induced by a move from no insurance to full insurance ([Einav et al., 2013](#)).

accurate pricing information yet available. For ease of notation, suppose that each bill takes τ weeks to arrive, so that a bill for a service procured in week t would arrive in week $t + \tau$.⁴⁹ Based on these components, households respond to a signal of their spending θ :

$$\theta_{it} = \underbrace{\sum_{k=0}^{t-\tau} \sum_{i \in \mathcal{I}} c_{ij}(m_{ik})}_{\text{known spending}} + \underbrace{\sum_{k=t-\tau+1}^t \sum_{i \in \mathcal{I}} s_i(m_{ik}|x_{ik})}_{\tilde{\theta}_{it}=\text{unknown spending}}, \quad (5)$$

where $s_i(m_{ik}|x_{ik})$ represents service-specific signals of spending, which may depend on individual, household, and service level characteristics.

Hence, the timing of the model in each period t is as follows:

1. Individuals form expectations about their spending histories M_{it} , based on θ_{it} .
2. Individual health shocks λ_{it} are realized.
3. Spending decisions m_{it}^* are made based on realized health shocks and beliefs about spending histories, which govern the perceived marginal cost of additional units of care \hat{c}_{it} .
4. A new signal of spending $s_{it}(m_{it}^*)$ is received for the individual and all household members enrolled in the same plan. Household members update their expectations of M_{it} , and we proceed to period $t + 1$.

5.2 Parameterizing Price Signals

We model spending signals as noisy, potentially biased signals affecting household beliefs about their future cost-sharing responsibilities. That is, we suppose that signals follow a normal distribution around a constant multiple of the true OOP spending with some noise:

$$s_i(m_{it}|x_{it}) \sim \mathcal{N}(\beta \cdot c_{ijt}(m_{it}), \sigma_s^2), \quad (6)$$

where β indicates the potential for patients to inflate (or deflate) their true OOP spending by a constant parameter before the bill arrives, and σ_s^2 quantifies the noise inherent in signals.⁵⁰ As mentioned in Section 4.3, uncertainty about cost-sharing may persist about more than simply OOP spending, as households may not fully internalize negotiated prices or what

⁴⁹Note that in the empirical estimation of the model, the length of time between a service and bill's arrival is allowed to vary across services; this assumption is only made in this section for ease of exposition.

⁵⁰Note that allowing β to be a random coefficient varying across individuals is a simple extension of the model; for the present purposes, however, we focus on an average of β across the population of interest.

services are subject to a deductible. Based on these assumptions, household expectations of their OOP spending to date can be written as

$$\mathbb{E}[\theta_{it}] = \sum_{i \in \mathcal{I}} \sum_{k=0}^t (1 - D_{ik}) \beta c_{ijk}(m_{ik}) + D_{ik} c_{ijk}(m_{ik}), \quad (7)$$

where D_{ik} is a binary variable indicating if the bill for services performed in a week k has arrived ($D_{ik} = 1$) or not ($D_{ik} = 0$). Based on the household's expected value of θ_{it} , households have a well-defined expectation over the marginal cost of future consumption given by⁵¹

$$\mathbb{E}[\hat{c}_{it}] = Pr(\theta_{it} \leq d) \cdot 1 + (1 - Pr(\theta_{it} \leq d)) \cdot c, \quad (8)$$

where d is the deductible facing individual i . Given the distributional assumptions placed on the signals $s(\cdot)$, this probability can be directly computed as

$$Pr(\theta_{it} \leq d) = \Phi \left(\frac{d - \mathbb{E}[\theta_{it}]}{\sum_{k=t-\tau+1}^t \sigma_s^2} \right), \quad (9)$$

where $\mathbb{E}[\theta_{it}]$ is as defined in Equation 7.

This model allows for households to systematically over- or under-inflate their true OOP spending absent a bill's arrival, measured by the parameter β ; however, lack of pricing information could shape behavior simply due to household risk aversion given an unknown marginal price \hat{c} . Intuitively, as signals become noisier (σ_s^2 increases) or bills take longer to arrive (so that $\sum_{k=t-\tau+1}^t \sigma_s^2$ increases), the probability an individual faces a lower marginal cost increases, potentially resulting in over-spending relative to an individual who immediately receives accurate pricing information. Importantly, this can happen in the model even if signals are unbiased (so that $\beta = 1$).

Estimation of the model requires identification of β as well as the size of the noise parameter σ_s^2 . Additional unobservable parameters in the model include heterogeneity in moral hazard ω_i and individual health shocks λ_{it} . When estimating the model, we calibrate these hyper-parameters to match moments predicted by both previous research and training data not used in the structural estimation. We use the estimated regression coefficients predicted by [Einav et al. \(2013\)](#) to capture variation in moral hazard parameters across households.⁵² We model individual-level health shocks as draws from an individual-specific

⁵¹Note that $c < 1$ in general. In practice, we estimate the model on the sample of individuals enrolled in plans with non-zero deductibles, to cleanly capture how OOP misperception may affect discontinuous changes in the marginal cost of spending across deductible thresholds.

⁵²Note that these regression models result in individual-level predictions for ω_i ; in estimation, we aggregate these to the household level by taking the mean of $\log(\omega_i)$ across all members $i \in \mathcal{I}$.

shifted lognormal distribution; this distribution captures both the skewed nature of the observed spending data and the probability that an individual has zero consumption in a period. That is, each individual in each period draws λ_{it} from a distribution $F(\mu_i, \sigma_i, \kappa_i)$ such that

$$\log(\lambda_{it} - \kappa_i) \sim \mathcal{N}(\mu_i, \sigma_i^2). \quad (10)$$

We calibrate the three hyper-parameters $(\mu_i, \sigma_i, \kappa_i)$ to match the empirical distribution of weekly spending using individuals *not* included in the structural estimation. These include individuals enrolled in plans with no deductible, as well as patients enrolled in any type of plan between 2014 to 2018. Individuals in this sample are grouped into cells based on patient demographics—including age, sex, risk score quartile, and relationship to the main employee—and the empirical distribution in each cell is matched to the shifted lognormal moments.⁵³ Once these parameters are identified, individual-period shocks are drawn for each member of the model sample and then summed to the household-period level.⁵⁴

Given these calibrations, identification of the main parameters (β, σ_s^2) come from exogenous variation (at the household level) in the length of time required for a bill to arrive after different health services. In contrast to the reduced-form evidence presented in Section 4, the model leverages variation in the waiting periods associated with *all* medical claims in a given household-year. This variation may exist across services as well as across households; importantly, underlying variation in $\tilde{\theta}_{it}$ which artificially moves households above or below their deductible is central to identifying how household expectations of \hat{c} evolve in ways that most closely fit the observed choice data. We therefore can identify the bias parameter β using the quasi-randomness of bill timing, where a bill generates a discrete change in marginal costs correcting household expectations (similar to the evidence presented in Section 4). On the other hand, identification of the variance parameter σ_s^2 leverages variation across households and services to identify the spread of the signal distribution.

5.3 Household Learning

Given reduced-form results suggesting that households learn from repeated interactions with shoppable services, we model household belief formation about marginal prices over time. To assess this, we incorporate household learning about the potential bias parameter β , where

⁵³This is done using three properties of a shifted lognormal distribution: $\bar{\lambda} = \exp(\mu + \frac{1}{2}\sigma^2) + \kappa$, $\lambda^M = \exp(\mu) + \kappa$, and $\frac{\text{sd}(\lambda)}{\lambda} = \sqrt{\exp(\sigma^2) - 1}$, where λ^M denotes the median. The solution to this system of equations given the moments of the empirical distribution of λ identifies the three hyperparameters μ, σ, κ .

⁵⁴For shocks to be meaningful, we restrict $\lambda_{\mathcal{I}t} < m_{\mathcal{I}t}$ in each period.

households may learn from each bill’s arrival.⁵⁵ Households are assumed to have prior beliefs about β which follow a normal distribution with a mean $\mu_{\beta,0}$ and variance $\sigma_{\beta,0}^2$:

$$\hat{\beta}_{i0} \sim \mathcal{N}(\mu_{\beta,0}, \sigma_{\beta,0}^2). \quad (11)$$

When a bill arrives for a household, it conveys information about the true realized prices of medical services. We model these bills as unbiased signals centered around the true value for $c_{ijt}(m_{it})$. This means that in each period, households receive a learning signal ℓ_{it} about β that is centered at 1 and normally distributed with a variance σ_ℓ^2 :

$$s_{it} \sim \mathcal{N}(1, \sigma_\ell^2). \quad (12)$$

We assume that households update their prior beliefs conditional on their observed signal following Bayes’ Rule. Assuming normal distributions for both the prior and posterior allows for closed-form solutions for household beliefs about β at each period, and is consistent with previous learning models (Crawford and Shum, 2005).

Incorporating learning requires estimating four parameters of interest. First, the average prior mean $\mu_{\beta,0}$ and unchanging spending signal variance σ_s^2 dictate the spread of OOP spending signals households receive in each period prior to learning (where $\mu_{\beta,0}$ is similar to β in the non-learning model). Rather than assume constant household bias, we measure dispersion in potential under-information across households with $\sigma_{\beta,0}^2$; the speed with which household biases are corrected by bills is governed by the variance of the learning signal, σ_ℓ^2 .

Estimated learning parameters allow analysis of both the spread of beliefs before any information and the speed with which beliefs converge. As in the simpler case of the model, identification of the four parameters ($\mu_{\beta,0}, \sigma_s^2, \sigma_{\beta,0}^2, \sigma_\ell^2$) stems from exogenous variation in bill timing. Within-household variation in expenditures relative to pending OOP expenditures serves to identify both the starting point of household beliefs (the prior mean) and the rate of convergence (the signal variance). Similarly, variation in choices across households identifies the spread of beliefs—summarized in the prior variance—and the spread of signals in each subsequent period, σ_s^2 .

5.4 Model Results

We estimate the model presented in Section 5 for 240,111 households in our analytical sample enrolled in plans with nonzero deductibles from 2006 to 2013. For each household-week, we

⁵⁵For now, we model each signal as having an equal impact; future extensions of this model could flexibly model heterogeneous signals based on the total cost of a service or by different service types. We also do not model learning about the spread of the distribution of signals over time.

simulate household health shocks and draw household moral hazard parameters; then, for different values of β , we estimate household signals of underlying OOP spending and associated marginal costs, \hat{c}_{it} . Taken together, these estimates produce a prediction of spending $m_{it}(\beta)$, which differs as β changes. Our primary measure of model fit is the root mean squared error (RMSE) between observed and predicted levels of weekly spending at the household level.

	No Learning		Learning	
	Estimate	95% C.I.	Estimate	95% C.I.
Panel A: Parameter Estimates				
β or $\mu_{\beta,0}$	1.73	[1.695,1.765]	2.58	[2.532,2.628]
σ_s	\$15.20	[\$14.95,\$15.45]	\$14.77	[\$14.54,\$15.00]
σ_β	—	—	0.12	[0.109, 0.135]
σ_ℓ	—	—	0.09	[0.070,0.106]
Panel B: Implied Spending				
Mean	\$2,415	[\$2,413.86,\$2,416.75]	\$2,350	[\$2,342.64, \$2,357.37]
Median	\$1,302	[\$1,300.64, \$1,304.91]	\$1,276	[\$1,268.67, \$1,283.34]
Panel C: Counterfactual Spending				
Recentered Priors				
% Households Affected	98.9%		85.3%	
Δ Mean Spending	-\$392 (16.2%)	[-\$394.05,-\$390.63]	-\$364 (15.5%)	[-\$370.64,-\$356.76]
Δ Median Spending	-\$166 (12.7%)	[-\$167.81,-\$164.39]	-\$94 (7.4%)	[-\$95.48, -\$92.60]
Real-time Information				
% Households Affected	72.0%		85.0%	
Δ Mean Spending	-\$1,163 (48.2%)	[-\$1,167.78,-\$1,158.90]	-\$792 (33.7%)	[-\$798.67,-\$784.53]
Δ Median Spending	-\$489 (37.6%)	[-\$493.90,-\$485.01]	-\$204 (16.0%)	[-\$205.83, -\$201.97]

Notes: Table presents results from estimating both variations of the model described in Section 5. Panel A reports the baseline parameter estimates; Panel B reports implied spending at the household-year level given these equilibrium parameters. Panel C reports how these parameters are expected to change under two counterfactual scenarios: recentered priors ($\beta = \mu_{\beta,0} = 1$) and real-time information ($\beta = 1, \sigma_s^2 = 0$). All currencies are reported in 2022 USD.

Table 5. Model Results and Implied Spending

Table 5 reports the results, based on 50 simulations with different individual health shocks.⁵⁶ We estimate significant bias in household perceptions of OOP spending and considerable noise in spending signals. The average spending signal in the model without learn-

⁵⁶See Appendix Figure A.12 for a visual depiction of the parameter space in the non-learning model.

ing is centered at 173% of true OOP spending, with a standard deviation of \$15.20.⁵⁷ The equilibrium model parameters have two primary implications. First, there is a great deal of uncertainty at the household level in estimating OOP spending and prices; the large estimated standard deviation of price signals may well lead risk-averse households to have lower expected utility and therefore to change spending decisions. Second, in addition to overall uncertainty, the model implies that households respond to price signals as though they are biased, with the average price signal being inflated by 73% relative to its true OOP cost. This is consistent with the findings of Section 4, and suggests that households may over-estimate their spending relative to a deductible prior to receiving price information.

The model results are similar even once household learning is permitted in the model, as reported in the third and fourth columns of Table 5. The initial prior mean for spending signals increases from 1.73 to 2.58, suggesting the greatest mismatch between household perceptions and spending signals at the beginning of the year. This is intuitive, as early in the plan year, the majority of households are attempting to ascertain if a spending event met their deductible, meaning we would expect noisier spending signals earlier in a plan year. However, household learning takes place relatively quickly: Figure 6 shows the average value of $\hat{\beta}$ across the sample by week of year.⁵⁸ Within the first six weeks of the year, average household beliefs about β converge to below the 1.73 estimated in the non-learning model; for the rest of the year, they remain close to the overall average, which is estimated to be about 1.47.⁵⁹ Similar to the non-learning model, there remains both potential bias in the center of the spending signal distribution and considerable noise in its spread.

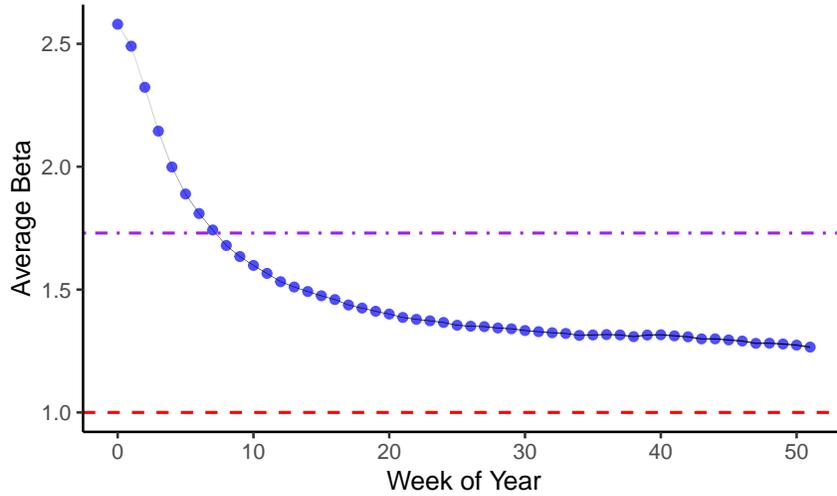
We therefore assess the extent to which (a) correcting household beliefs and (b) eliminating all uncertainty in price signals affect implied spending. The first simulation is equivalent to aiding consumers to be better informed about healthcare prices before consumption; the second simulation completely eliminates all pricing uncertainty—this would require both some form of immediate claims adjudication as well as a hypothetical way to eliminate any other form of consumer uncertainty with regards to spending (as discussed above). Hence, the second simulation is only used for comparison to the first. In both models, the implied levels of household OOP spending are consistent with the baseline averages (Panel B). In the non-learning model, recentering household priors (that is, imposing that $\beta = 1$ for all

⁵⁷See Appendix Figure A.13 for a depiction of the implied probabilities a household has met their deductible for a \$1,000 deductible, as a function of expected spending $\mathbb{E}[\theta_{it}]$.

⁵⁸There is relatively little variation across households, with the estimated prior standard deviation equal to 0.12. Put into context, we estimate that 95.5% of households (two standard deviations on either side of the mean) have priors in the interval (2.34, 2.82). Similarly, the model suggests that bills provide precise information about β , as σ_ℓ is estimated to be 0.09, meaning 96% of signals are within the interval (0.82, 1.18). This is well below both the non-learning average of $\beta = 1.73$ and the learning model average of 1.47.

⁵⁹Appendix Figure A.14 presents further results illustrating heterogeneity in beliefs across a year.

Figure 6. Evolution of Beliefs about β Across Plan Year



Notes: Figure depicts average value of simulated β across the relative week of a plan year for the full sample, with 95% confidence intervals shaded in black. Simulations are performed based on the median equilibrium parameters of the model discussed in Section 5.3. Dashed purple line represents the average estimated value of β in the non-learning model, 1.73.

households) reduces predicted spending for 98.9% of households, with conditional changes in spending of \$392 (16.2%) on average and \$166 (12.7%) at the median. Although this constitutes a significant difference in household spending, it represents only a fraction of predicted spending changes from total price uncertainty. Eliminating all such uncertainty is predicted to lower total spending for 85.8% of households, with only 1/3 of the change explained by household under-estimation of prices.⁶⁰ We find similar effects even considering a model enriched with household learning; eliminating household bias in β results in spending changes for 85.3% of households with average (median) declines of 15.5% (7.4%).

Taken together, the model results indicate that households may incorrectly process spending signals prior to a bill’s arrival while reinforcing that substantial noise remains in those signals regardless of potential bias. In the interim period, while awaiting a bill, households consume medical care as though they are considerably closer to reaching their deductible than they are in reality. What’s more, spending signals are quite noisy, leading households with substantial uncertainty in their spending even in the absence of bias. Correcting household bias about OOP prices (relative to a deductible) changes behavior only marginally compared to providing real-time pricing information and eliminating all periods of billing uncertainty.

⁶⁰Note that fewer households change spending when uncertainty is eliminated as some households received correct/under-inflated signals randomly, even with a large β .

6 Discussion & Conclusion

This paper assesses how households respond to a medical bill as a form of resolving price uncertainty, potentially affecting future household consumption. When a household member consumes a service with a nontrivial—but unknown—amount of OOP spending, other household members increase their spending. However, bill arrivals meaningfully change these increases; we estimate that a bill’s arrival leads to a reduction in household health spending of 10.9%. These results are robust to numerous alternative specifications and appear consistent with a mechanism where bills reduce or eliminate pricing uncertainty, especially for households close to changes in their insurance contracts such as meeting a deductible.

We encapsulate our findings in a model of “imperfect moral hazard” with delayed learning about prices. Our model, just as our reduced-form evidence suggests, indicates that consumers face uncertainty when making consumption decisions with lagged pricing information and that they may overestimate the probability that they have met a household deductible, particularly early in an insurance contract period. Our model allows us to consider household learning from medical events and place these perception errors into the context of broader pricing uncertainty.

Our analysis provides several important contributions to models of price uncertainty and household moral hazard in healthcare; however, our results should be viewed in the context of their limitations. First, by limiting our analysis to households enrolled in group ESI plans, we are unable to determine how price uncertainty affects consumption decisions for other populations, such as couples on Medicare or low-income households covered by Medicaid or the ACA Marketplaces. Examining other populations—particularly populations with greater income constraints—would shed additional, important light on the extent to which price uncertainty leads to sub-optimal allocations of care. Second, while our results suggest that households would make different spending decisions without price uncertainty—in particular, consuming less care on average—we are unable to say anything about the welfare effects of these decisions given our current data. Finally, we use an imperfect proxy in our exposure (EOB generation), which may introduce measurement error into our estimates. As discussed in Section 2, this error is likely to attenuate our estimates, not because the measurement error is classic, but instead because measurement error in true bill arrival introduces contamination bias from the interim period when households do not know OOP spending. If consumers overestimate OOP prices before the bill arrives, any regressions misclassifying $\mathbb{1}\{\text{Post_Bill}\} = 1$ when it should be 0 will attenuate the correction parameter $\beta_{\text{Post_Bill}}$ to zero.

The analysis we present could be extended in several meaningful ways. First, future work could incorporate observed payment interactions between patients and physicians, rather

than relying on claims data alone. Data on physician practices—including how quickly physicians submit claims to payers for medical claims and send bills to patients—may provide insights into both the source of variation in processing times as well as the potential policy benefits of reducing the length of provider billing cycles. Future work may also consider the spillover effects of bill shock from healthcare consumption on other, non-health household consumption decisions.

More generally, future research could build on the learning model presented here. This could include a more flexible framework for belief formation, a more thorough treatment of heterogeneity across services, or allowing learning parameters to be covariate-dependent. In particular, exploring the health equity concerns associated with learning about prices could provide valuable insight into the persistence of health disparities in accessing even high-value preventive services (Teutsch et al., 2020; McMorrow et al., 2014). Finally, future work could explore the impact of real-time claim adjudication on consumer spending responses (Orszag and Rekhi, 2020). This could be especially policy-relevant when exploring how heterogeneity across payers and providers (e.g., integrated care practices) could be used to leverage improved price transparency.

Increasing understanding of how consumers form expectations about their health needs and utilization is a vital component of designing optimal insurance contracts and health policies. Economic modeling and health policy alike are well-served by incorporating delayed learning as we assess how consumers make health decisions in real time.

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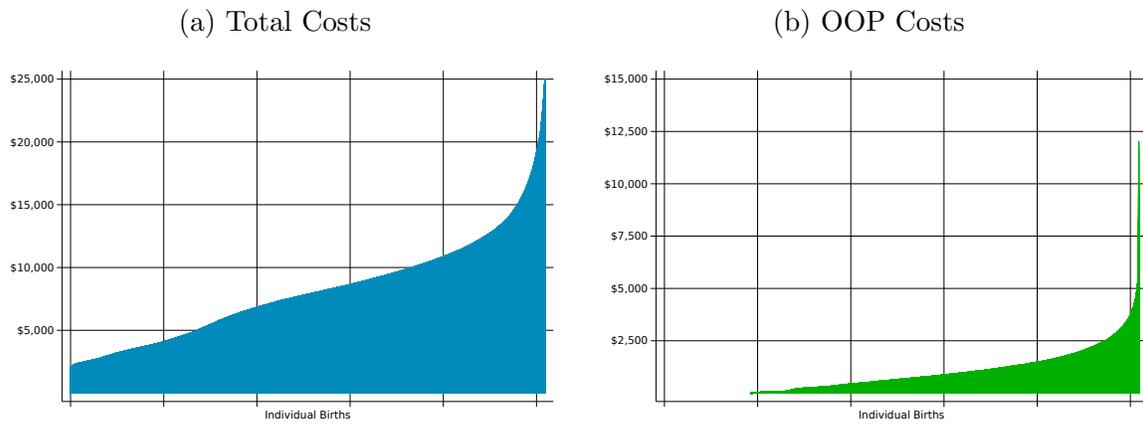
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A Appendix

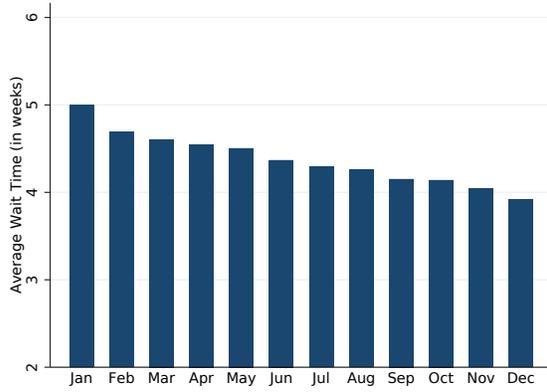
Figure A.1. Variation in Prices for CPT 59400: Routine Vaginal Delivery



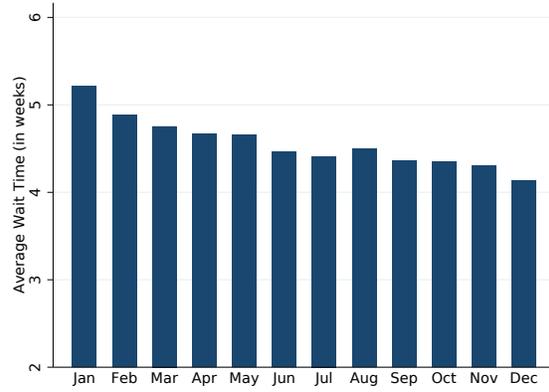
Notes: Figures show variation in total and OOP costs associated with CPT code 59400: “Routine obstetric care including antepartum care, vaginal delivery (with or without episiotomy, and/or forceps) and postpartum care.” Each vertical bar represents a unique encounter in our analytical data set, with the height of the bar corresponding to the price (all measured in 2022 USD).

Figure A.2. Variation in Wait Times for Bills

(a) All Services

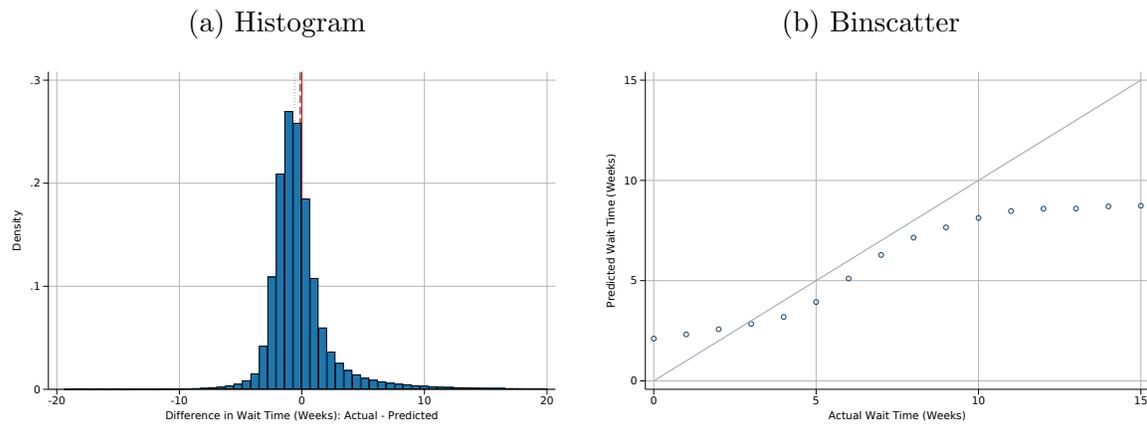


(b) CMS Shoppable Services



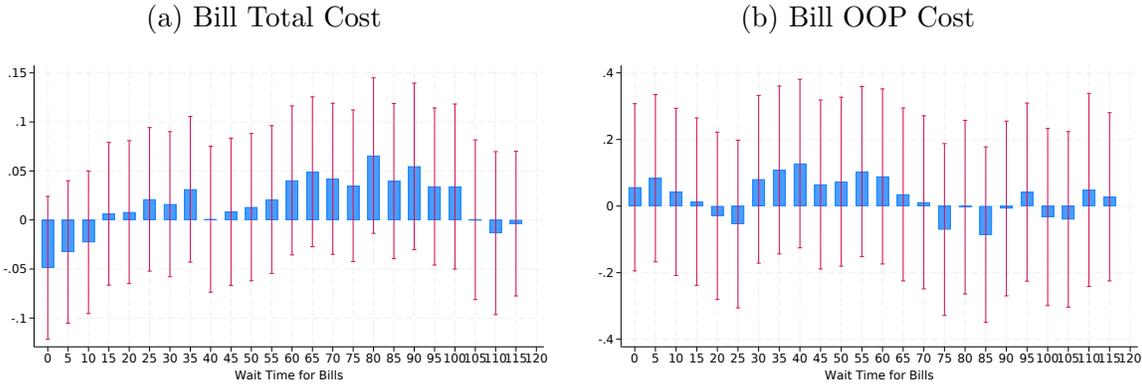
Notes: Indicates average wait time (in weeks) between date of service and date the insurer paid their portion of the claim (the earliest date at which definitive OOP information is known). Panel (a) illustrates variation in average wait times across months of the year (pooled across all years) for all claims in the analytical data; panel (b) limits the sample to only the shoppable services used as major health events in the text.

Figure A.3. Difference between Realized and Predicted Wait Times for Bills



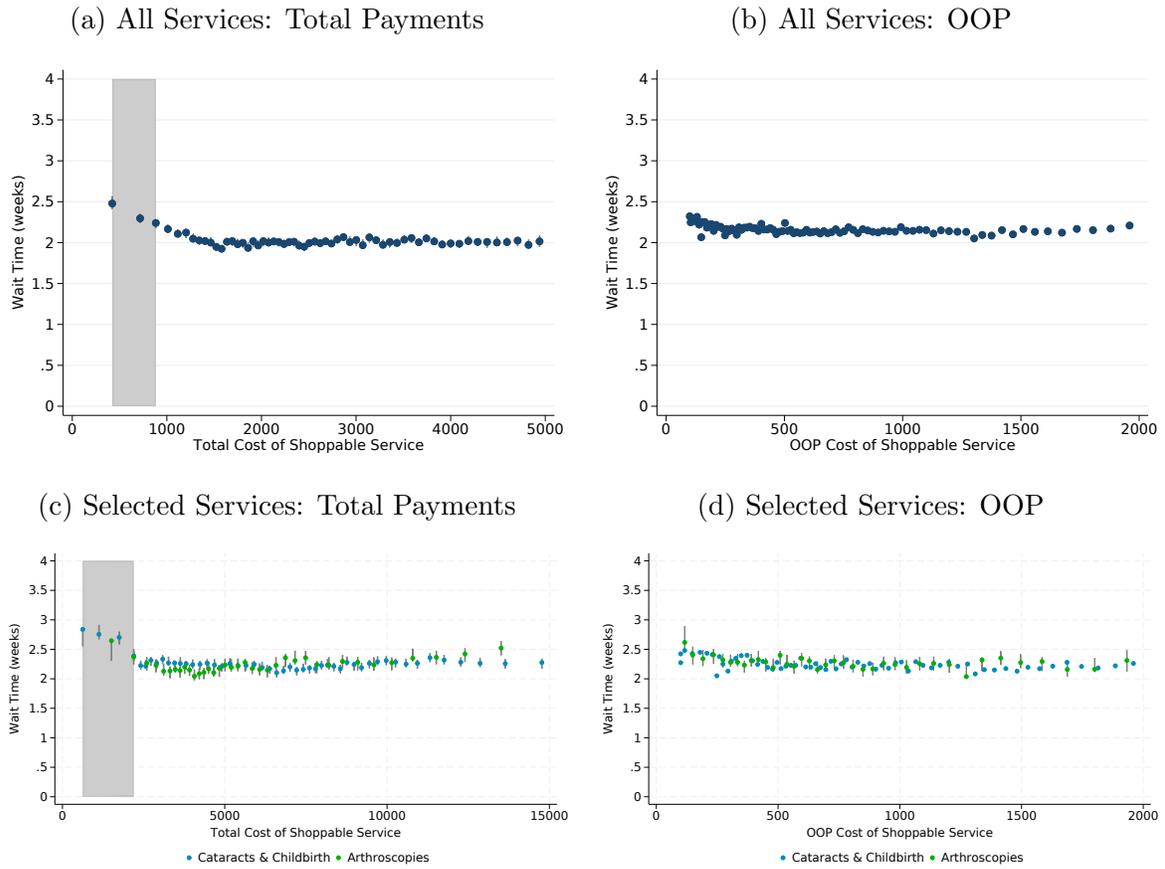
Notes: Figures each show differences between realized wait times (measured as the weeks waited between a service date and the claim paid date) and predicted wait times across all shoppable services in analytic data. Predicted wait times use regression on year, week of year, service type, and provider fixed effects. Panel (a) shows differences as a histogram, while panel (b) shows them as a binscatter.

Figure A.4. Relationship between Bill Delay and Total/OOP Cost for Shoppable Services



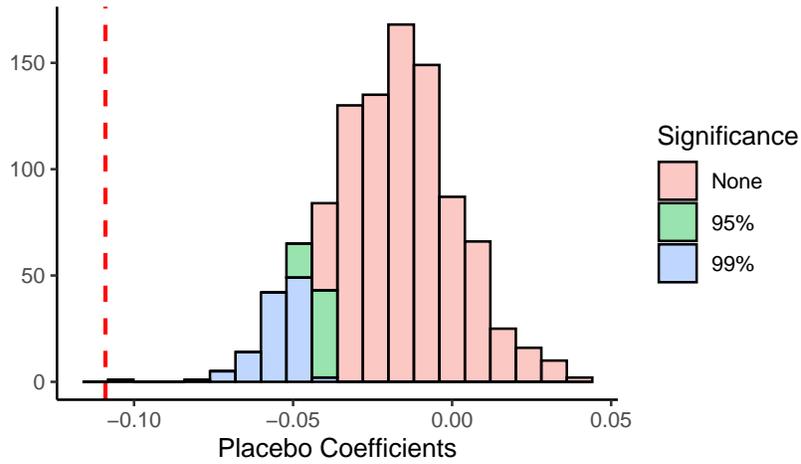
Notes: Figure shows estimated regression coefficients from Poisson regressions relating the overall spending associated with a shoppable service to bins for wait times (in days) for the bill to arrive. Regressions include fixed effects for year and week-of-year of shoppable service as well as service types and physicians. Panel (a) shows total billed cost, while panel (b) uses OOP cost only as the outcome variable. Wait times longer than 120 days (<2.5% of sample) are included in the final bin. 95% confidence intervals are shown (with unclustered standard errors). Results presented here are robust to varying bin width or to scaling outcome variables by pre-event levels (e.g., OOP/(pre-event OOP)).

Figure A.5. Examining Correlations Between Waiting Times and Service Type/Cost



Notes: Figures show binscatter regressions (using the “binsreg” package in Stata) between the size of a bill and the time elapsed between service and plan paid date. Selected services shown in the second row are those which we estimated to have the largest cost differences (in absolute value) between bills arriving under 30 days and those exceeding a month. These include arthroscopies (which had the largest positive differences) and cataract removals and vaginal deliveries (which had the largest negative differences). Panels (a) and (b) adjust for provider, procedure type, and year fixed effects; panels (c) and (d) adjust for provider and year fixed effects only.

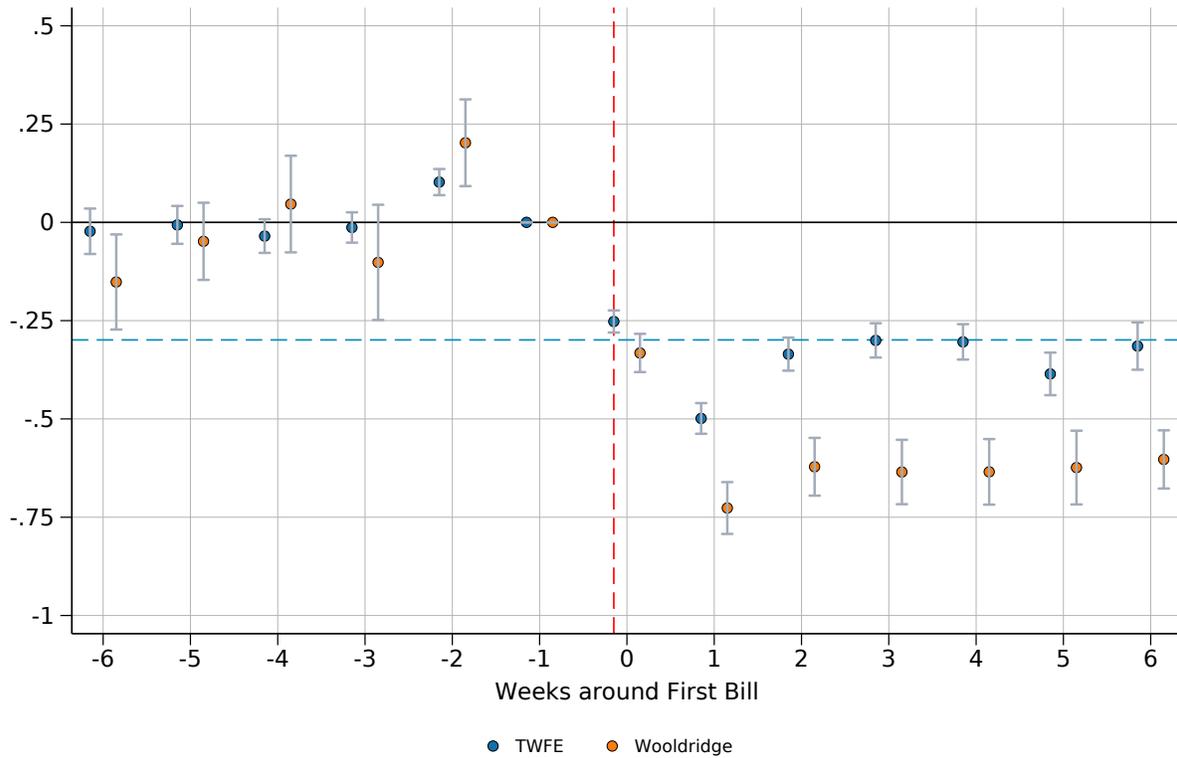
Figure A.6. Distribution of Placebo Regression Coefficients for $\beta_{\text{post_bill}}$



NOTE: Dashed line indicates true regression coefficient.

Notes: Figure shows the distribution of placebo regression coefficients for the dummy variable Post_Bill_{it} in Equation 1 ($n = 1,000$). Each placebo is constructed by artificially varying wait times for bills based on the empirical distribution of wait times in the analytical sample. Standard errors are clustered at the household level. Coefficients are color-coded based on statistical significance. The vertical dashed red line indicates the estimated coefficient of the preferred specification (Table 2).

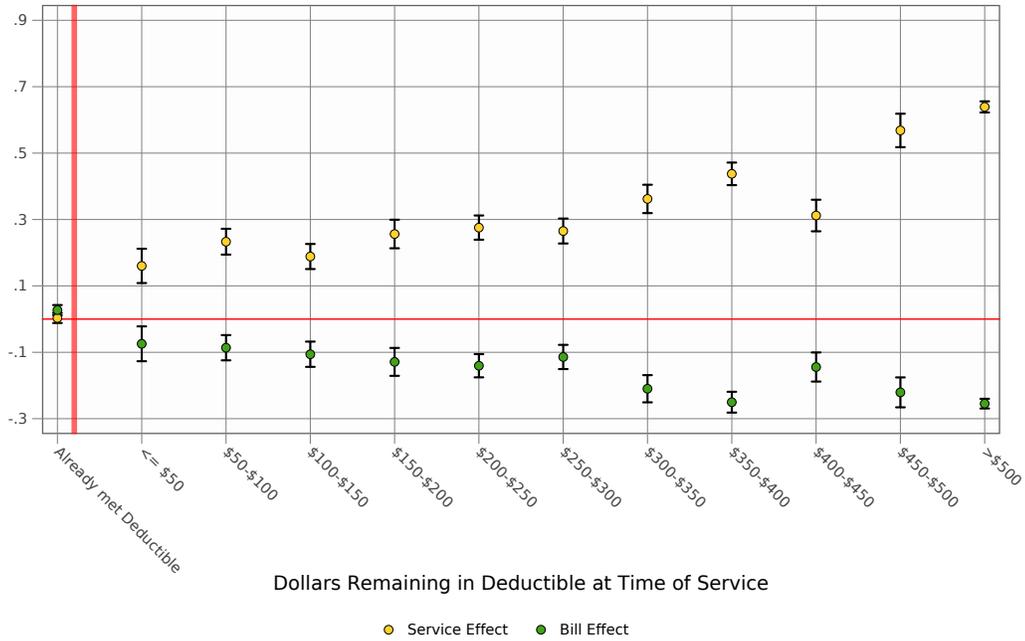
Figure A.7. Robustness of Dynamic Treatment Effects to New DID Estimation



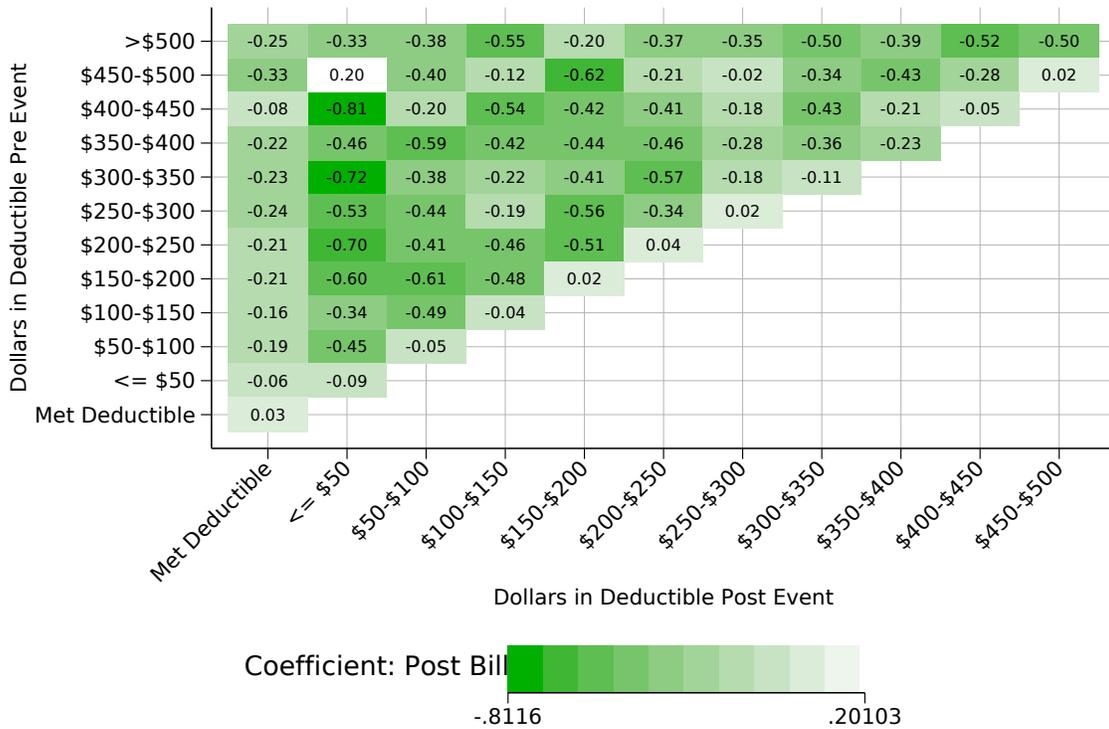
Notes: Figure shows estimated coefficients and 95% confidence intervals for the TWFE regression estimated in Equation 2 on the matched sample, as discussed in Section 4.2. Coefficients are compared to those obtained from using the Mundlak approach to TWFE estimation described by [Wooldridge \(2021\)](#), incorporating a full set of household/year/week-of-year interactions in the main regression. Note that due to computational limits, the Mundlak approach uses a random 50% sample of the analytic dataset. Dynamic treatment effects are calculated as weighted linear combinations of these coefficients, with standard errors are clustered at the household level.

Figure A.8. Deductible-Stratified Results Measured in Dollars

(a) One-Way Stratification (Figure 4)

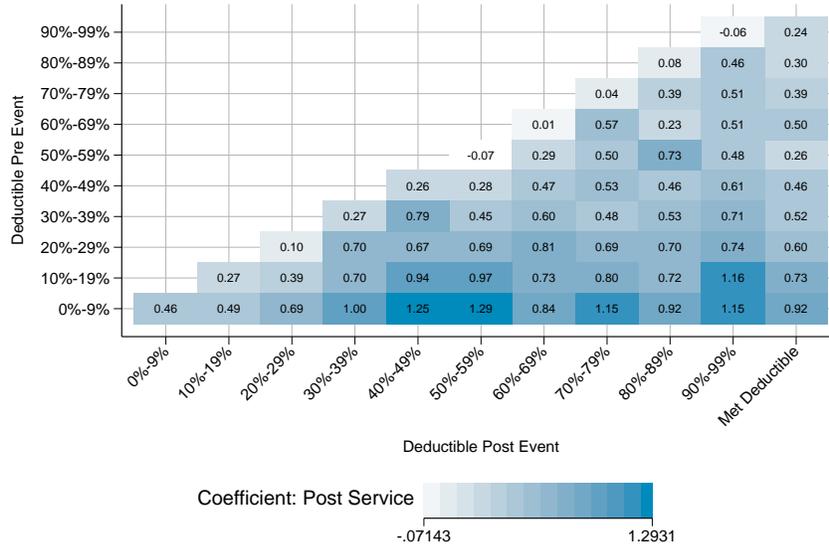


(b) Two-Way Stratification (Figure 5)



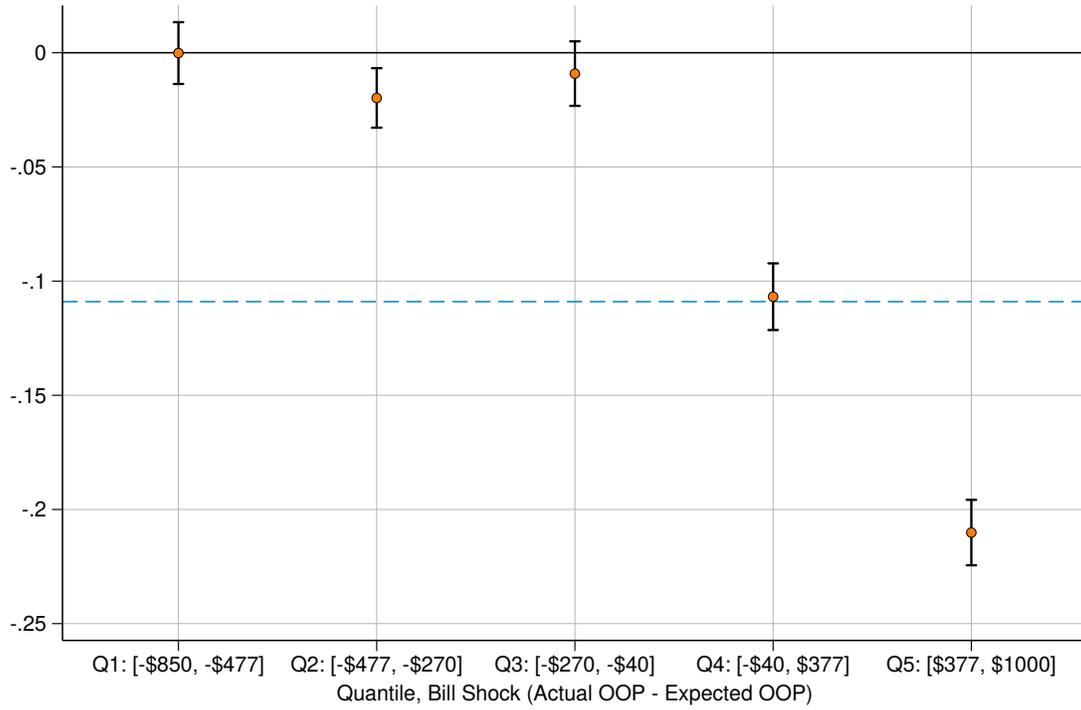
Notes: Figures show equivalent versions of Figures 4 and 5 with bins measured in levels (dollars) of deductible. Both panels follow methodology of main figures discussed in text. Panel (a) stratifies across deductible spending prior to an event, while panel (b) stratifies across spending prior to *and* following events. Here, sample is restricted to those with non-zero, unmet deductibles.

Figure A.9. Heterogeneous Service Effects By Household Deductibles and Service Cost



Notes: Figure depicts estimated coefficients for $\mathbb{1}\{\text{Post_Service}_{it}\}$ in Equation 1 across deciles of household deductible spending prior to *and* following an event. Here, sample is restricted to individuals in a non-zero deductible plan who have not yet met their deductible at the time of service. Each row indicates a different decile of deductible spending prior to the event, while each column indicates deciles following the event. Regressions include linear time trends by group as additional controls. Standard errors are clustered at the household level.

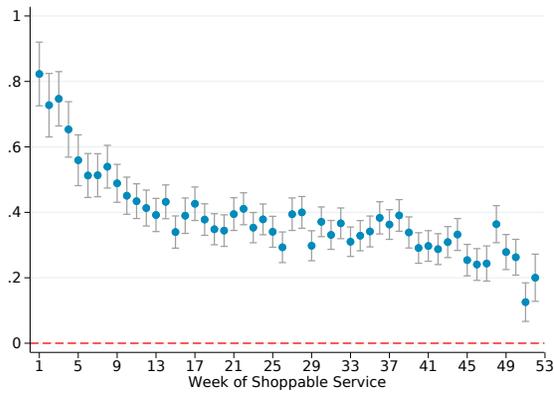
Figure A.10. Estimated Bill Effects by Size of Implied Bill Shock



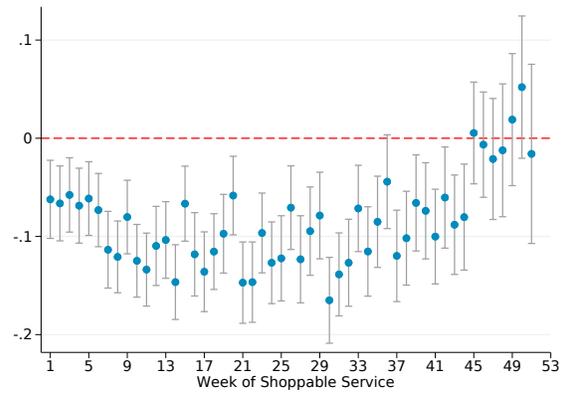
Notes: Figure shows estimated bill effects (from Equation 1) for bins of households based on the implied bill shock from their index event. Bill shock is defined as the actual OOP associated with the index event, minus the predicted OOP based on provider, year, week-of-year, and event type fixed effects (e.g., Figure 2); bill shock is limited to \$1,000 or less (this discards <5% of index events). Blue horizontal line indicates the estimate from the full sample regression (Table 2).

Figure A.11. Variation in Estimated Bill Effects Across Calendar Year

(a) Service Consumption Coefficients

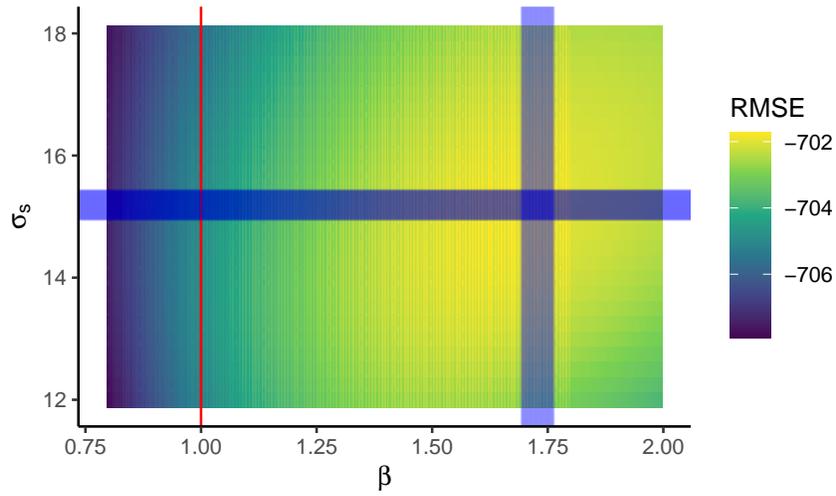


(b) Bill Arrival Coefficients



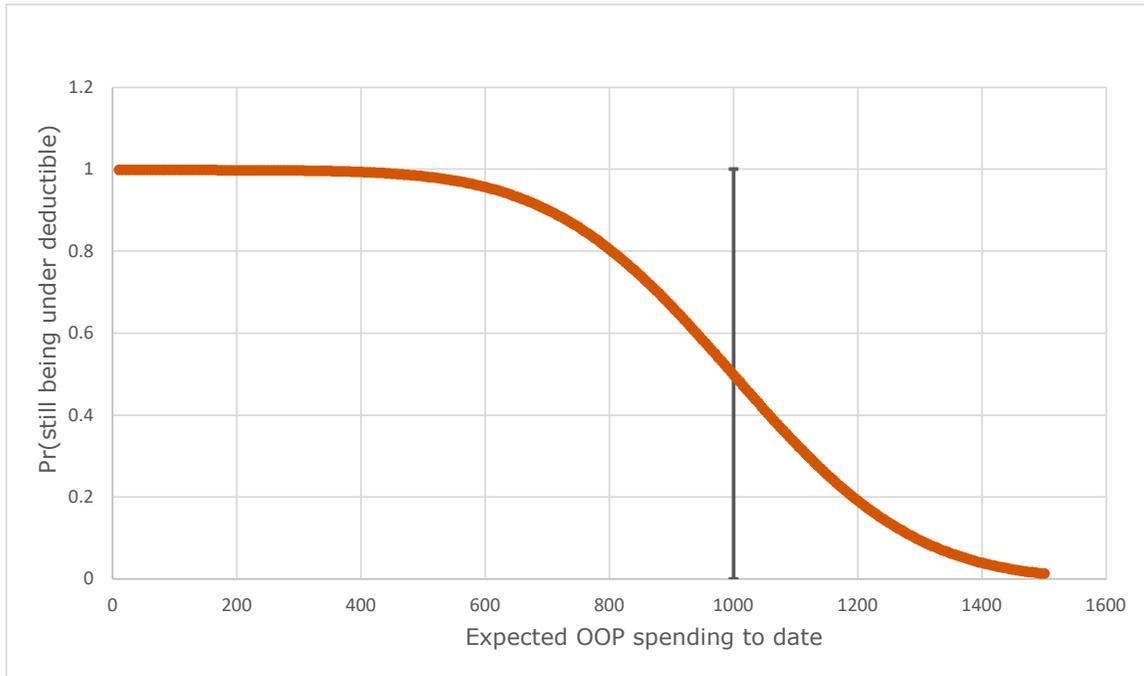
Notes: Separate estimation of service and bill effects stratified by relative week of year that shoppable service was consumed. Regressions adjust for household, provider, procedure type, and year fixed effects (results are robust to both inclusion and exclusion of relative week of year fixed effects).

Figure A.12. Model Results: Parameter Space Heatmap



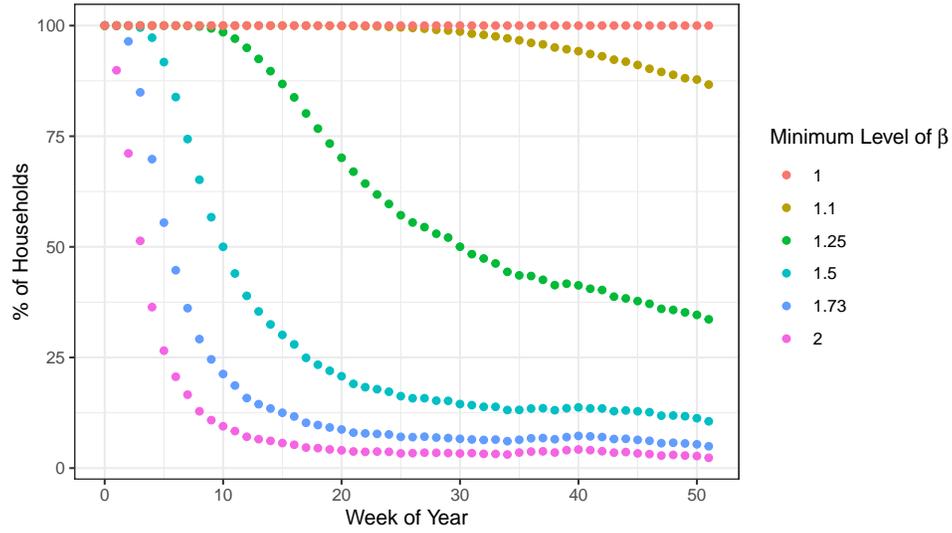
Notes: Figure depicts the relationship between chosen level of household pre-bill discounting parameter β and spending signal variance σ_s^2 , and the root mean squared error (RMSE) of the model presented in Section 5. RMSE is measured as the square root of the mean squared error between observed and predicted household spending at the weekly level. For each point in the parameter space, fill color denotes the median result of 50 simulations with independently drawn health shocks. The 95% bootstrapped confidence interval for both parameters is shaded in blue.

Figure A.13. Implied Probability of Being Under Deductible, Equilibrium Parameters



Notes: Figure shows implied probability of being still under the deductible for a household with the equilibrium model parameters ($\beta = 1.73, \sigma_s = 15.2$) and a \$1,000 deductible, as described in Section 5.

Figure A.14. Evolution of Beliefs about β Across Plan Year



Notes: Figure depicts the fraction of households in the sample with simulated β greater than or equal to some threshold β_{\min} for various thresholds. Simulations are performed based on the median equilibrium parameters of the model discussed in Section 5.3.

Type	Code	Service Description
DRG	216	Cardiac valve and other major cardiothoracic procedures w/ cardiac catheterization
DRG	460	Spinal fusion, except cervical
DRG	470	Major joint replacement or reattachment of lower extremity
DRG	473	Cervical spinal fusion
DRG	743	Uterine and adnexa procedures for non-malignancy
CPT	19120	Removal of 1 or more breast growth, open procedure
CPT	29826	Shaving of shoulder bone using an endoscope
CPT	29881	Removal of one knee cartilage using an endoscope
CPT	42820	Removal of tonsils and adenoid glands (patient younger than age 12)
CPT	43235	Diagnostic examination of esophagus, stomach, and/or upper small bowel
CPT	43239	Biopsy of the esophagus, stomach, and/or upper small bowel using an endoscope
CPT	45378	Diagnostic examination of large bowel using an endoscope
CPT	45380	Biopsy of large bowel using an endoscope
CPT	45385	Removal of polyps or growths of large bowel using an endoscope
CPT	45391	Ultrasound examination of lower large bowel using an endoscope
CPT	47562	Removal of gallbladder using an endoscope
CPT	49505	Repair of groin hernia (patient age 5 years or older)
CPT	55700	Biopsy of prostate gland
CPT	55866	Surgical removal of prostate and surrounding lymph nodes using an endoscope
CPT	59400	Routine obstetric care for vaginal delivery
CPT	59510	Routine obstetric care for cesarean delivery
CPT	59610	Routine obstetric care for vaginal delivery after prior cesarean delivery
CPT	64483	Injections of anesthetic and/or steroid drug into lower or sacral spine nerve root
CPT	66821	Removal of recurring cataract in lens capsule using laser
CPT	66984	Removal of cataract with insertion of lens
CPT	93000	Electrocardiogram, routine, with interpretation and report
CPT	93452	Insertion of catheter into left heart for diagnosis
CPT	62322	Injection of substance into spinal canal of lower back or sacrum
CPT	62323	Injection of substance into spinal canal of lower back or sacrum

Notes: Table shows list of procedures used to identify non-urgent “shoppable services,” which are the exposure of interest in the primary reduced-form specifications. Services are identified based on lists provided by the Center for Medicare and Medicaid Services (CMS), using the relevant Diagnostic Related Groups (DRGs) or Current Procedural Terminology (CPT) codes to identify procedures.

Table A.1. Shoppable Services Used in Analytical Sample

	Main	Alternative Specifications			
Post Service	0.137*** (0.0103)	0.365*** (0.0102)	0.367*** (0.0102)	0.366*** (0.0102)	0.300*** (0.105)
Post Bill	-0.074*** (0.0099)	-0.066*** (0.0101)	-0.069*** (0.0100)	-0.067*** (0.0100)	-0.062*** (0.0010)
$\overline{\text{spend}}_{it}$	\$93.82	\$93.82	\$93.82	\$93.82	\$93.82
Household FEs	X	X	X	X	X
Year FEs	X		X	X	X
Week of Year FEs	X			X	X
Provider FEs	X				X
Event Type FEs	X				
Observations	126,747,805	126,747,805	126,747,805	126,747,805	126,747,805

Notes: Table presents results from triple-difference Poisson regressions highlighting the role of a bill's arrival on health spending of affected household members (Equation 1). Here, we report results for households affected by unplanned hospitalizations for injuries and appendectomies. We identify index events as any household member admitted to a hospital with a principal diagnosis contained in the ICD chapter heading "Injury, poisoning and certain other consequences of external causes" (in ICD-10-CM, S00-T88), or whose primary diagnosis is in the heading "Diseases of appendix" (K35–K38). Event type fixed-effects are defined by diagnosis group using the first three characters of the ICD-10-CM diagnosis code. Throughout, standard errors were clustered at the household level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2. Robustness of Effects to Expanded Services: Injuries and Appendectomies

	Full Specification	Event-Specific Time Trends	No Household FEs
Post Service	0.218*** (0.0032)	0.253*** (0.0051)	0.726*** (0.0036)
Post Bill	-0.109*** (0.0030)	-0.071*** (0.0043)	-0.016*** (0.0038)
$\overline{\text{spend}}_{it}$	\$120.49	\$120.49	\$120.49
Observations	61,860,735	61,860,735	61,860,735

Notes: Robustness models include (a) a model including weekly time trends for each type of shoppable service in the sample, and (b) a model omitting household fixed effects. Throughout, standard errors were clustered at the household level, and regressions included family, year, relative week of year, event type, and provider (of shoppable service) fixed effects.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3. Robustness of Main Specification

	Main Models		Alternative Specifications			
Post Service	0.402*** (0.0022)	0.224*** (0.0033)	0.753*** (0.0033)	0.626*** (0.0034)	0.625*** (0.0034)	0.599*** (0.0033)
Post Bill		-0.067*** (0.0031)	-0.075*** (0.0033)	-0.082*** (0.0032)	-0.084*** (0.0033)	-0.085*** (0.0032)
$\overline{\text{spend}}_{it}$	\$120.49	\$120.49	\$120.49	\$120.49	\$120.49	\$120.49
Household FEs	X	X	X	X	X	X
Year FEs	X	X		X	X	X
Week of Year FEs	X	X			X	X
Provider FEs	X	X				X
Event Type FEs	X	X				
Observations	61,860,735	61,860,735	61,860,735	61,860,735	61,860,735	61,860,735

Notes: Table presents results from triple-difference Poisson regressions highlighting the role of a bill's arrival on health spending of affected household members. Includes linear controls for time trends before and after service. Regression coefficients displayed illustrate the expected change in log household spending (measured per person-week) associated with the service date and bill arrival (both measured as dummy variables). Throughout, standard errors were clustered at the household level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4. Robustness of DDD Regressions to Linear Time Controls

<i>Outcome: Spending Category</i>	Poisson	Pr(Spending > 0)				<i>N</i>
	Any	Any	Inpatient	ED	Elective	
Bill Effects: Shoppable	-0.109*** (0.0030)	-0.008*** (0.0004)	-0.028*** (0.0001)	-0.00002 (0.0001)	0.00002* (9.89e-6)	61,860,735
Bill Effects: Injury	-0.074*** (0.0099)	-0.019*** (0.0008)	-0.020*** (0.0002)	-0.034*** (0.0001)	-0.011*** (0.0001)	126,747,805

Notes: Table presents results from triple-difference Poisson regressions estimating a bill's effect on spillover health spending within a household, across specific categories of spending. Each cell represents the coefficient on Post_Bill in Equation 1, where the index event is defined by the row and the spending outcome is defined by the column. Index events include all shoppable services (as in Table 2) and unexpected injuries and appendectomies (as in Table A.2). Outcomes include all spending (estimated both continuously using Poisson regression in column (1), and as a binary outcome in column (2)) as well as spending on inpatient services (column 3), emergency department (ED) visits (column 4), and elective surgeries (column 5). Includes linear controls for time trends before and after service, and standard errors are clustered at the household level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5. Estimated Bill Effects by Categories of Health Spending

	Original Specification (Table 2)	Alternative Specification
Post Service	0.218*** (0.0032)	0.283*** (0.0043)
Post Bill	-0.109*** (0.0030)	-0.077*** (0.0042)
Post Bill \times Service Met Deductible		0.122*** (0.0212)
Household FEs	X	X
Year FEs	X	X
Week of Year FEs	X	X
Provider FEs	X	X
Event Type FEs	X	X
Observations	61,860,735	44,403,451

Notes: Table presents results from triple-difference Poisson regressions highlighting the role of a bill's arrival on health spending of affected household members. Results are split across households whose event did and did not move them across the threshold of the household deductible. Sample is restricted to households enrolled in plans with a nonzero deductible, which was unmet at the time of the shoppable service. Standard errors are clustered at the household level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6. Bill Arrival Effects Across Deductible Threshold

	Main	Alternative Specifications			
Post Service	-0.216 (0.1114)	0.698*** (0.1455)	0.465** (0.1603)	0.427** (0.1580)	0.450** (0.1366)
Post Bill	0.122 (0.1227)	0.363* (0.1635)	0.415** (0.1587)	0.415** (0.1571)	0.205 (0.1529)
$\overline{\text{spend}}_{it}$	\$93.82	\$93.82	\$93.82	\$93.82	\$93.82
Household FEs	X	X	X	X	X
Year FEs	X		X	X	X
Week of Year FEs	X			X	X
Provider FEs	X				X
Event Type FEs	X				
Observations	31,905,563	31,905,563	31,905,563	31,905,563	31,905,563

Notes: Table presents results from triple-difference Poisson regressions highlighting the role of a bill's arrival on health spending of affected household members. In this exercise, sample is limited to households who have met their OOP max prior to the consumption of the index service, plus the control group of not-yet-treated household-years. Includes linear controls for time trends before and after service. Regression coefficients displayed illustrate the expected change in log household spending (measured per person-week) associated with the service date and bill arrival (both measured as dummy variables). Throughout, standard errors were clustered at the household level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.7. Falsification Test: Bill Effects for OOP-Capped Households

	Week of Month			
	First	Second	Third	Last
Post Service	0.230*** (0.0066)	0.221*** (0.0061)	0.211*** (0.0066)	0.232*** (0.0066)
Post Bill	-0.074*** (0.0062)	-0.065*** (0.0058)	-0.064*** (0.0064)	-0.080*** (0.0064)
$\overline{\text{spend}}_{it}$	\$101.04	\$153.30	\$153.25	\$102.26
Observations	38,633,474	39,994,383	39,069,522	38,935,377

Notes: Table presents results from triple-difference Poisson regressions highlighting the role of a bill's arrival on health spending of affected household members (Equation 1). Columns stratify bill arrivals by relative week of month to test the hypothesis that households may respond differently to bills arriving at the end of the month versus the beginning due to liquidity constraints. Standard errors are clustered at the household level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8. Heterogeneity by Week of Bill Arrival

	Main	Alternative Specifications			
Post Service	0.262*** (0.0102)	0.252*** (0.0042)	0.254*** (0.0042)	0.250*** (0.0042)	0.251*** (0.0041)
Post Bill	-0.090*** (0.0096)	-0.084*** (0.0040)	-0.086*** (0.0039)	-0.081*** (0.0040)	-0.083*** (0.0039)
$\overline{\text{spend}}_{it}$	\$167.01	\$167.01	\$167.01	\$167.01	\$167.01
Household FEs	X	X	X	X	X
Year FEs	X		X	X	X
Week of Year FEs	X			X	X
Provider FEs	X				X
Event Type FEs	X				
Observations	61,860,735	61,860,735	61,860,735	61,860,735	61,860,735

Notes: Table presents results from triple-difference Poisson regressions highlighting the role of a bill's arrival on health spending of affected household members. Regressions omit all claims consumed at the provider of the focal index event (note that this means control group years without a focal index event are excluded). Includes linear controls for time trends before and after service. Regression coefficients displayed illustrate the expected change in log household spending (measured per person-week) associated with the service date and bill arrival (both measured as dummy variables). Throughout, standard errors were clustered at the household level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.9. Robustness to Omitting Claims from the Index Service Provider

	Regression Coefficients		Pre-Treatment Averages	
	Post Service	Post Bill	% ≥ 0	Conditional Mean
Hospital Care				
Emergency Department	0.123*** (0.0141)	0.017 (0.0147)	0.67%	\$929.98
Preventable Hospitalizations	0.412*** (0.0880)	-0.170 (0.0879)	0.04%	\$19,979.89
Outpatient Care				
Behavioral Health	-0.032* (0.0144)	0.029* (0.0142)	1.19%	\$119.47
Chiropractic Care	-0.015 (0.0160)	0.017 (0.0161)	1.86%	\$133.39
Evaluation & Management	1.469*** (0.0076)	-0.272*** (0.0064)	1.05%	\$121.45
Imaging	0.102*** (0.0119)	0.005 (0.0123)	2.55%	\$265.52
Lab Services	0.196*** (0.0123)	-0.147*** (0.0130)	3.96%	\$62.14
Low-Value Services	0.066*** (0.0094)	0.028** (0.0097)	6.58%	\$148.61
Preventive Care	0.349*** (0.0039)	-0.211*** (0.0040)	11.89%	\$120.79
Specialist Care	0.546*** (0.0221)	-0.108*** (0.0226)	0.57%	\$114.70
Prescriptions				
	0.013*** (0.0049)	0.010 (0.0050)	18.30%	\$147.14

Notes: Table shows coefficients from triple-difference regressions capturing service-specific effects of pricing information ($N = 59, 177, 995$, see Equation 1). Columns (1) and (2) present regression coefficients; column (3) indicates the fraction of pre-treatment weeks when spending was positive; and column (4) presents pre-treatment weekly averages, conditional on positive spending. See Appendix Table A.12 for a complete list of the CPT codes for each of the outpatient categories. All models include fixed effects for households, years, relative week of year, and providers, as well as linear time trends before and after the event; standard errors were clustered at the household level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10. Estimated Impact of Bill Arrival on Service-Specific Spending

Service Description		Code
Panel A: Infection Diagnoses	(ICD-9-CM)	(ICD-10-CM)
Acute Respiratory Infections	460-466	J00-J06
Pneumonia and Influenza	480-488	J09-J18
Otitis media	381-382	H65-H66
Streptococcal sore throat and scarlet fever	034	A38, J02
Whooping cough	033	A37
Infectious mononucleosis	075	B27
Chickenpox	052	B01
Urinary Tract Infections	590, 595, 599	N10-N12,N15,N16,N28,N30,N36,N39,R31
Food poisoning (bacterial)	005	A05
Other Intestinal Infections	008-009	A04,A08,A09
Nausea and Vomiting	536.2, 787	R11,R12,R13,R14,R15,R19
Dyspepsia	536.8,536.9	K30-K31
Panel B: Injuries	(ICD-9-CM)	(ICD-10-CM)
Fracture of Upper Limb	810-819	S12,S22,S32,S42,S52,S62
Fracture of Lower Limb	820-829	S72,S82,S92
Dislocation	830-839	S13,S23,S33,S43,S53,S63,S73,S83,S93
Sprains and Strains	840-848	Combined with above
Panel C: Place of Service Codes	(POS)	–
Physician Office	11, 72, 95	
Urgent Care Center	17,20	
Emergency Department	23	
Hospital (including on-campus outpatient)	21, 22, 28	

Notes: Table shows list of diagnoses used to identify care for non-delayable injuries and infections. Services are identified based on lists provided by the Center for Medicare and Medicaid Services (CMS), using the ICD-9-CM and ICD-10-CM diagnostic codes and place of service codes.

Table A.11. Identifying Injuries, Infections, and Places of Service

Outpatient Category	CPT Codes	
	Code Range	Code Values
Behavioral Health	90000-99999	90791-90792, 90801-90802, 90805-90807, 90832-90834, 90836-90840, 90845-90847, 90849, 90853, 96105, 96112-96113, 96116, 96121, 96125, 96130-96133, 96136-96139, 96156, 96158-96159, 96164-96165, 96167-96168, 96170-96171, 99483-99494
Chiropractic Care	90000-99999	97001, 97010-97014, 97018, 97022, 97026, 97032-97035, 97039, 97110-97113, 97116, 97124, 97140, 97161-97162, 97530, 97535, 97750, 98940-98943, 99211
Evaluation & Management	10000-19999	11976, 11981-11983
	30000-39999	36415-36416
	40000-49999	44388-44389, 44392-44394, 45300, 45303-45309, 45315-45317, 45320, 45330-45335, 45338-45340, 45378-45386
	50000-59999	57170, 58300-58301, 58340, 58565, 58600, 58605, 58611, 58615, 58670-58671
	70000-79999	71250, 74263, 74740, 76070-76071, 76075-76078, 76497, 76977, 77078-77083, 78350
	80000-89999	80061, 82270, 82274, 82465, 82947-82952, 83036, 83718-83721, 84478, 86580, 86592-86593, 86631-86632, 86689, 86701-86703, 86803-86804, 87110, 87270, 87320, 87340-87341, 87390-87391, 87490-87492, 87590-87592, 87620-87622, 87801, 87810, 87850, 88141-88143, 88147-88155, 88164-88167, 88174-88175, 88304-88305
	90000-99999	92015, 92507, 92551-92553, 92558, 92567, 92585-92588, 96040, 96110, 96127, 96160-96161, 96372, 97802-97804, 99173-99174, 99201-99205, 99211-99215, 99381, 99385-99387, 99395-99397, 99401-99404, 99411-99412, 99420
Imaging	10000-19999	10005-10006, 19081-19084
	20000-29999	29881
	70000-79999	70030, 70110, 70130, 70150, 70160, 70200, 70210, 70220, 70260, 70330, 70336, 70360, 70450, 70460, 70470, 70480-70482, 70486-70491, 70496-70498, 70540, 70543-70553, 71010, 71020, 71045-71048, 71100-71101, 71110, 71120, 71130, 71250,

		71260, 71275, 71550-71552, 71555, 72040, 72050-72052, 72070, 72082, 72100, 72110, 72114, 72125-72132, 72141-72142, 72146-72149, 72156-72159, 72170, 72191-72202, 72220, 73000, 73010, 73030, 73050, 73060, 73070, 73090, 73100, 73110, 73120, 73130, 73140, 73200-73202, 73206, 73218-73225, 73501-73503, 73521-73523, 73552, 73560-73564, 73590, 73600, 73610, 73620, 73630, 73650, 73660, 73700-73702, 73706, 73718-73725, 74000, 74018-74021, 74150, 74160, 74170, 74174-74178, 74181-74185, 74210, 74220, 74241, 74245-74250, 74261-74263, 74270, 74280, 74400, 75635, 76010, 76390-76391, 76536, 76641-76642, 76645, 76700, 76705-76706, 76770, 76775-76776, 76801, 76812, 76817, 76830, 76856-76857, 76870, 76881-76882, 76981, 77021, 77046-77049, 77052, 77057, 77063-77067, 77072-77077, 77080, 77085, 78012-78014, 78070-78071, 78206, 78215, 78226-78227, 78290, 78306, 78315, 78452, 78472, 78607-78608, 78707-78708, 78800, 78804, 78814-78816
	90000-99999	91200, 93000, 93005, 93010-93018, 93024-93025, 93040-93042, 93050, 93201-93205, 93208-93210, 93220-93237, 93241-93248, 93260-93261, 93264, 93268-93272, 93278-93299, 93303-93308, 93312-93321, 93325, 93350-93352, 93355-93356, 93451-93464, 93501-93505, 93508-93511, 93514, 93524-93533, 93536, 93539-93545, 93555-93556, 93561-93568, 93571-93572, 93580-93583, 93590-93603, 93607-93624, 93631, 93640-93644, 93650-93657, 93660-93662, 93668, 93701-93702, 93720-93724, 93727, 93731-93745, 93750, 93760-93762, 93770, 93784-93793, 93797-93799, 93880, 93926, 93970-93971, 93975
Lab Services	20000-29999	20610
	30000-39999	36415-36416
	80000-89999	80048, 80050, 80053, 80061, 80076, 81000-81003, 81025, 82000, 82003, 82009-82010, 82013, 82016-82017, 82024, 82030, 82040, 82042-82045, 82055, 82075, 82077, 82085, 82088, 82101, 82103-82108, 82120, 82127-82128, 82130-82131, 82135-82136, 82139-82140, 82143, 82145, 82150, 82154, 82157, 82160, 82163-82164, 82172, 82175, 82180, 82190, 82205,

82232, 82239-82240, 82247-82248, 82250-82252, 82261, 82270-82274, 82286, 82300, 82306-82308, 82310, 82330-82331, 82340, 82355, 82360, 82365, 82370, 82373-82376, 82378-82380, 82382-82384, 82387, 82390, 82397, 82415, 82435-82436, 82438, 82441, 82465, 82480, 82482, 82485-82489, 82491-82492, 82495, 82507, 82520, 82523, 82525, 82528, 82530, 82533, 82540-82544, 82550, 82552-82554, 82565, 82570, 82575, 82585, 82595, 82600, 82607-82608, 82610, 82615, 82626-82627, 82633-82634, 82638, 82642, 82646, 82649, 82651-82654, 82656-82658, 82664, 82666, 82668, 82670-82672, 82677, 82679, 82681, 82690, 82693, 82696, 82705, 82710, 82715, 82725-82726, 82728, 82731, 82735, 82742, 82746-82747, 82757, 82759-82760, 82775-82777, 82784-82785, 82787, 82800, 82803, 82805, 82810, 82820, 82926, 82928, 82930, 82938, 82941, 82943, 82945-82948, 82950-82953, 82955, 82960, 82962-82963, 82965, 82975, 82977-82980, 82985, 83001-83003, 83006, 83008-83010, 83012-83015, 83018-83021, 83026, 83030, 83033, 83036-83037, 83045, 83050-83051, 83055, 83060, 83065, 83068-83071, 83080, 83088, 83090, 83150, 83491, 83497-83500, 83505, 83516, 83518-83521, 83525, 83527-83529, 83540, 83550, 83570, 83582, 83586, 83593, 83605, 83615, 83625, 83630-83634, 83655, 83661-83664, 83670, 83690, 83695, 83698, 83700-83701, 83704, 83715-83719, 83721-83722, 83727, 83735, 83775, 83785, 83788-83789, 83805, 83825, 83835, 83840, 83857-83858, 83861, 83864, 83866, 83872-83874, 83876, 83880, 83883, 83885, 83887, 83890-83894, 83896-83898, 83900-83909, 83912-83916, 83918-83919, 83921, 83925, 83930, 83935, 83937, 83945, 83950-83951, 83970, 83986-83987, 83992-83993, 84022, 84030, 84035, 84060-84061, 84066, 84075, 84078, 84080-84081, 84085, 84087, 84100, 84105-84106, 84110, 84112, 84119-84120, 84126-84127, 84132-84135, 84138, 84140, 84143-84146, 84150, 84152-84157, 84160, 84163, 84165-84166, 84181-84182, 84202-84203, 84206-84207, 84210, 84220, 84228, 84233-84235, 84238, 84244, 84252, 84255, 84260, 84270, 84275, 84285, 84295, 84300, 84302,

		84305, 84307, 84311, 84315, 84375-84379, 84392, 84402-84403, 84410, 84425, 84430-84432, 84436-84437, 84439, 84442-84443, 84445-84446, 84449-84450, 84460, 84466, 84478-84482, 84484-84485, 84488, 84490, 84510, 84512, 84520, 84525, 84540, 84545, 84550, 84560, 84577-84578, 84580, 84583, 84585-84586, 84588, 84590-84591, 84597, 84600, 84620, 84630, 84681, 84702-84704, 84830, 84999, 85007, 85014, 85018, 85025, 85027, 85610, 85651-85652, 85730, 86003, 86038, 86140, 86580, 86592, 86880, 86900-86901, 87040, 87070, 87077, 87081, 87086, 87088, 87186, 87491, 87591, 87621, 87804, 87880, 88142, 88175, 88304-88305, 88312-88313, 88342, 88720
	90000-99999	94760, 99000-99001
Low-Value Services	20000-29999	29877-29879
	30000-39999	36222-36224
	70000-79999	70450, 70460, 70470, 70498, 70547-70553, 71010, 71015, 71020-71023, 71030, 71034-71035, 72010, 72020, 72052, 72100, 72110, 72114, 72120, 72131-72133, 72141-72142, 72146-72149, 72156-72158, 72200-72202, 72220, 78451-78454, 78460-78461, 78464-78465, 78472-78473, 78481-78483, 78491-78492
	80000-89999	82306, 82652, 87620-87625, 88141-88143, 88147-88155, 88164-88167, 88174-88175
	90000-99999	93000, 93005, 93010, 93015-93018, 93303-93308, 93312, 93315, 93318, 93350-93351, 93880-93882, 94010
Preventive Care	10000-19999	11976, 11981-11983
	30000-39999	36415-36416
	40000-49999	44388-44389, 44392-44394, 45300, 45303-45309, 45315-45317, 45320, 45330-45335, 45338-45340, 45378-45386
	50000-59999	57170, 58300-58301, 58340, 58565, 58600, 58605, 58611, 58615, 58670-58671
	70000-79999	71250, 74263, 74740, 76070-76071, 76075-76078, 76497, 76977, 77078-77083, 78350

Specialist Care	80000-89999	80061, 82270, 82274, 82465, 82947-82952, 83036, 83718-83721, 84478, 86580, 86592-86593, 86631-86632, 86689, 86701-86703, 86803-86804, 87110, 87270, 87320, 87340-87341, 87390-87391, 87490-87492, 87590-87592, 87620-87622, 87801, 87810, 87850, 88141-88143, 88147-88155, 88164-88167, 88174-88175, 88304-88305
	90000-99999	92015, 92507, 92551-92553, 92558, 92567, 92585-92588, 96040, 96110, 96127, 96160-96161, 96372, 97802-97804, 99173-99174, 99201-99205, 99211-99215, 99381, 99385-99387, 99395-99397, 99401-99404, 99411-99412, 99420
	10000-19999	11100, 17000, 17003-17004, 17110-17111, 17250
	40000-49999	43239, 47562
	80000-89999	82962
	90000-99999	92012-92014, 92587, 93010, 94010

Table A.12. Identifying Types of Outpatient Services

Category	Service	CPT Codes / Therapeutic Classes	Additional restrictions (age/sex restrictions, diagnosis or procedure codes)
All Pediatric	Vitamin D Screening	82306,82652	Age < 18
All Pediatric	Cervical Cancer Screening	87620,87621,87622, 87623, 87624, 87625, 88141, 88142, 88143, 88147, 88148, 88150, 88152, 88153, 88154, 88155, 88164, 88165,88166, 88167, 88174, 88175, G0123, G0124, G0141, G0143, G0144, G0145, G0147, G0148, P3000, P3001, Q0091	Age < 18, age >= 14, female
All Pediatric	Head imaging for headache	70450,70460,70470,70551,70552,70553	Age < 18, Diagnosis codes: 3390, 3391, 3460, 3461, 3462, 3464, 3465, 3467, 3468, 3469, 7840, 3393, G440, G441, G442, G444, G430, G431, G435, G437, G438, G439, 30781,33983, 33984, 33985, R51, R510, R519, G4483, G4484, G4485
All Pediatric	Antibiotics for upper respiratory infections	Antibiotics (multiple classes)	Diagnosis codes: 460,465, J00, J06, H65, H60, H61, H62, 3810, 3814
All Pediatric	Antibiotics for bronchiolitis	Antibiotics (multiple classes)	Diagnosis codes: 46611,46619, J210, J218
All Pediatric	Cough or cold medicine	Antitussives, Expectorants, Mucolytics, Cough/Cold Combinations	Age < 6
Adult Drugs	Opioids to treat migraines	Opiate Agonists, Opiate Part Agonists, Opiate Antagonists	Diagnosis codes: 346**, G43**
Adult Imaging	Head imaging for headache	70450,70460,70470,70551,70552,70553	Diagnosis codes: 3390, 3391, 3460, 3461, 3462, 3464, 3465, 3467, 3468, 3469, 7840, 3393, G440, G441, G442, G444, G430, G431, G435, G437, G438, G439, 30781,33983, 33984, 33985, R51, R510, R519, G4483, G4484, G4485
Adult Imaging	Imaging for lower-back pain	72010, 72020,72052, 72100, 72110, 72114,72120, 72200, 72202, 72220, 72131, 72132, 72133, 72141, 72142, 72146, 72147, 72148,72149, 72156, 72157, 72158	Diagnosis codes: 7213, 7226, 7242, 7243, 7244,7245, 7246,7385, 7393,7394, 8460, 8461, 8462, 8463, 8468, 8469, 8472, M432, M512, M513, M518, M533, M545, M541, M543, M998, 72190, 72210, 72252, 72293, 72402,72470, 72471, 72479, M47817, M532X7, M9903, M9904, S338XXA, S336XXA, S339XXA, S335XXA, M47819, M4647, M4806, M532X8

Table A.13. Identifying Low-Value Health Services

Category	Service	CPT Codes / Therapeutic Classes	Additional restrictions (age/sex restrictions, diagnosis or procedure codes)
Adult Imaging	Screening for carotid artery disease	36222, 36223, 36224, 70498, 70547, 70548, 70549, 93880, 93882, 3100F	Diagnosis codes: 430, 431, 434, 436, 781, I63, I66, R25, R26, R27, R29, R47, G45, H34, R55, R20, 4350, 4351, 4353, 4358, 359, 3623, 7802, 7820, I609, I619, 43301, 43311, 43321, 43331, 43381, 43391, 99702, V1254, 36284, 78451, 78452, 78459, I6789, I67848, I97811, I97821, Z8673, H3582
Adult Imaging	Cardiac imaging	0144T, 0145T, 0146T, 0147T, 0148T, 0149T, 0150T, 75552, 75553, 75554, 75555, 75556, 75557, 75558, 75559, 75561, 75562, 75565, 75571, 75572, 75573, 75574, 78451, 78452, 78453, 78454, 78460, 78461, 78464, 78465, 78478, 78480, 78459, 78481, 78483, 78491, 78492, 78494, 78496, 78499	
Adult Screening	Vitamin D Screening	82306, 82652	
Adult Screening	Cardiac testing for low-risk patients	93015, 93016, 93017, 93018, 93350, 93351, 78451, 78452, 78453, 78454, 78460, 78461, 78464, 78465, 78472, 78473, 78481, 78483, 78491, 78492, 93303, 93304, 93306, 93307, 93308, 93312, 93315, 93318, 3120F, 93000, 93005, 93010, G0366, G0367, G0368, G0403, G0404, G0405	
Adult Screening	Pre-operative testing before low-risk surgery	71010, 71015, 71020, 71021, 71022, 71023, 71030, 71034, 71035, 93303, 93304, 93306, 93307, 93308, 93312, 93315, 93318, 94010, 78451, 78452, 78453, 78454, 78460, 78461, 78464, 78465, 78472, 78473, 78481, 78483, 78491, 78492, 93015, 93016, 93017, 93018, 93350, 93351	Procedure codes for surgery: 19120, 19125, 47562, 47563, 49560, 58558
Adult Surgery	Arthroscopic surgery for knee osteoarthritis	29877, 29879, G0289	Diagnosis codes: 8360, 8361, 8362, 7170, S832, 71741, M23202, M23205

Table Notes: Pediatric low-value services are defined based on Chua et al. (2016). Adult low-value services are based on definitions given in Bhatia et al. (2015), Chandra et al. (2021), and Colla et al. (2014).