Buying Data from Consumers

The Impact of Monitoring in U.S. Auto Insurance

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October 27, 2025

Abstract

New technologies have enabled firms to elicit granular behavioral data from consumers in exchange for lower prices and better experiences. This data can mitigate asymmetric information and moral hazard, but it may also increase firms' market power if kept proprietary. We study a voluntary monitoring program by a major U.S. auto insurer, in which drivers accept short-term tracking in exchange for potential discounts on future premiums. Using a proprietary dataset matched with competitor price menus, we document that safer drivers self-select into monitoring, and those who opt-in become 30% safer while monitored. Using an equilibrium model of consumer choice and firm pricing for insurance and monitoring, we find that the monitoring program generates large profit and welfare gains. However, large demand frictions hurt monitoring adoption, forcing the firm to offer large discounts to induce opt-in while tempering the profitability of heavy ex-post rent extraction. A counterfactual data portability policy would further reduce the firm's incentive to elicit monitoring data, leading to less monitoring and lower consumer welfare in equilibrium.

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New technologies have made it easier than ever for individuals to credibly document granular details of their behavior. Firms can elicit this data to offer tailored products and personalized pricing, which may in turn benefit consumers and give rise to voluntary data sharing.

The potential benefits of behavioral data are especially compelling in the auto insurance industry. Granular driving data allows insurers to identify drivers with lower accident risk more precisely than algorithms based on public data, which pool consumers based on demographic features and sparse accident records alone (Cohen and Einav 2007). This can benefit both the insurer, who can more easily target safe drivers, and drivers, who may receive price discounts by demonstrating safe driving behavior.

However, consumers must volunteer to share their data in order for these benefits to manifest. Those who qualify for safe-driving discounts may not share their data due to fear of reclassification risk or other concerns (Handel, Hendel, and Whinston 2015). Furthermore, firms typically retain property rights over the data that they collect. This incentivizes more data collection, but it also generates a competitive advantage that may lead to higher markups in the future (Jin and Wagman 2021).

In this paper, we develop an empirical framework to examine the trade-offs for consumer welfare and firm profit that result from consumer data sharing in the context of an auto-insurance monitoring program ("pay-how-you-drive") in the United States. New customers are invited to plug a simple device into their cars, which tracks and reports their driving behavior for up to six months (Figure A.1). In exchange, the insurer uses the data to better assess accident risk and adjust future premiums. Unlike traditional pricing factors such as age or accident records, monitoring data is not available to other firms. In 2017, insurers serving over 60% of the $267 billion U.S. auto insurance industry offered monitoring options.[1] Similar programs have been introduced in other industries, such as life insurance and consumer lending (Figure A.2).[2] However, despite their growing relevance, empirical evidence on the effects of consumer data sharing mechanisms and their impact on welfare across different sides of the market is sparse.

We construct a novel dataset that combines proprietary individual-level panel data from a major U.S. auto insurer (hereinafter referred to as "the firm") and price menus offered by its competitors. Our panel covers 23 states and spans 2012 to 2016, during which the firm's

[1] 2017 annual report of the National Association of Insurance Commissioners. Our research window is from 2012 to 2016.

[2] The Vitality program from life insurer John Hancock tracks and rewards exercise and health-related behaviors. Ant Financial incentivizes users to conduct more personal finance transactions in exchange for borrowing discounts.

monitoring program—the first major program in the industry—was introduced. For each consumer in our panel, we observe demographic characteristics, price menus from top competing insurers, insurance contracts purchased, and realized insurance claims. For each consumer who opts into the monitoring program, we observe a monitoring score reflecting her safe driving performance and the corresponding premium adjustments. Taken together, our analysis uses a panel dataset of over 1 million consumers and 50 million insurance quotes.

We first establish two key facts about the monitoring program as it is observed in our data. First, the monitoring program induces safer driving. Monitoring only occurs in the first period of insurance for new customers who opt in. Using a difference-in-differences estimator to capture within-consumer across-period variation in insurance claims, we find a 30% *moral hazard* effect: drivers who opt in to monitoring become safer on average. However, this behavioral change accounts for only 61% of the risk differences between consumers in the monitored and unmonitored groups. Even after the monitoring period, the monitored group remains safer on average, and individual monitoring scores remain highly predictive of risk. This suggests that monitoring uncovers persistent, previously unobserved heterogeneity in risk, leading to *advantageous selection* into the program.

These reduced-form analyses allow us to separately identify the effects of moral hazard and selection, extending a rich empirical literature that has thus far relied on correlations between plan choice and claims (Chiappori and Salanie 2000; Cohen and Einav 2007; Jeziorski, Krasnokutskaya, and Ceccarini 2019). However, quantifying the welfare impact of the monitoring program, or of prospective data regulations, requires a structural understanding of how heterogeneity in consumer risk and preferences shapes insurance demand, selection patterns, and firm pricing in equilibrium (Einav, Finkelstein, and Levin 2010). To do this, we develop an equilibrium model of insurance and monitoring.

On the demand side, we estimate a dynamic model that captures complex correlations between consumers' choice of insurance and monitoring opt-in, and their accident risk. Consumers have heterogeneous private risk types and preferences—risk aversion, inertia costs, and disutility from being monitored—and choose an insurance plan from a personalized price menu with options offered by the firm and its competitors. When consumers engage with the firm for the first time, they can opt in to monitoring and receive an upfront discount. At the end of each period, accidents arrive based on consumers' risk types. In addition, if a consumer is monitored, a score is realized from a noisy distribution that is correlated with their risk type. In expectation, better scores lead to higher discounts in future periods.

Consumers are thus incentivized to drive more safely when monitored.

On the supply side, we model firm pricing before and after the monitoring period to reflect the trade-off between eliciting more consumer data and profiting from this data in a competitive market. By offering a high upfront discount, the firm can encourage more consumers to opt in to monitoring. In order for this to be profitable, however, the firm must extract some of the surplus generated from monitoring in renewal periods. In this sense, the amount of monitoring data that is created (due to equilibrium monitoring participation) is endogenous to the firm's dynamic pricing strategy. In addition, the firm's ability to profitably "invest-and-harvest" through the monitoring program is tempered by competition as it must attract and retain consumers in the first place.

Taken together, our model captures several forces that drive consumers' monitoring opt-in choice. First, consumers anticipate that their risk will be lower during the monitoring period if they opt in. Some may also expect higher future discounts and thus stand to gain more from monitoring. But the expected benefit of such future discounts is moderated by elevated reclassification risk due to noise in the monitoring process. Finally, consumers may incur various unobserved costs due to monitoring, including privacy loss, increased effort while driving, and additional decision-making. We quantify the combined effect of these costs with a heterogeneous monitoring disutility term.

Our model fits the data closely across key moments—claims, monitoring scores, pricing factors, coverage shares, monitoring opt-in, renewal attrition, and selection patterns. The fit is stable across major product-menu changes, including the introduction of monitoring and a state-level mandatory-minimum increase. Further, estimates from the pricing and score models demonstrate that monitoring sharpens risk rating: both premiums and scores increase with claims risk, leading the monitored group to face a steeper price-risk gradient and, consequently, to exhibit advantageous selection into monitoring. The average consumer has modest risk aversion (1.41×10^{-5}), but faces sizable switching frictions: \$333 per period for switching firms, or \$113 to opt in to monitoring.

Our counterfactual simulations indicate that both consumers and the firm benefit from the status quo compared to a scenario in which no monitoring program exists: total annual surplus increases by \$9.79 per capita per year even though the monitoring firm only captures 11.85% of the market. But monitoring take-up is low—due, in part, to choice frictions, reclassification risk, and competition from cheap plans offered by other insurers. The optimal pricing of monitoring, all else equal, would entail significantly more aggressive prices in the initial period—a 9% surcharge and 77% discount in the unmonitored and monitored pools, respectively—and much higher rent capture post monitoring among monitored drivers. Due to the large surplus generated by the monitoring program and the firm's natural incentive to "invest" in it, counterfactual policies—such as enforcing a data portability regulation[3] or a floor on the monitoring discount—would further reduce monitoring take-up. This ends up hurting consumers by weakening the firm's dynamic incentives to invest and by raising reclassification risk in equilibrium.

The paper proceeds as follows. Section 1 describes our data and provides background information on auto insurance and monitoring. Section 2 conducts reduced-form tests to evaluate the effects of moral hazard and selection that are induced by the monitoring program. Section 3 presents our structural model, identification arguments, and estimation procedures to recover key cost and demand parameters. Section 4 discusses estimation results. Section 5 proposes a model of monitoring pricing and studies the welfare implications of the monitoring program in the status quo as well as counterfactuals involving optimal pricing and a ban on proprietary data. Section 6 revisits our results in relation to the literature and concludes.

1 Background and Data

In this section, we provide background information on U.S. auto insurance and the monitoring program we study. We also describe our datasets.

1.1 Auto Liability Insurance

Auto insurers in the U.S. collected $431 billion dollars of premiums in 2024.[4] Our focus is the largest segment: private passenger third-party liability insurance, which covers injuries and property damage inflicted on other parties in an at-fault accident. The contract specifies an indemnity limit with no deductibles, so that the insured consumer would be covered up to that limit. In all states we study, liability insurance is mandatory, with the minimum required coverage ranging from $25,000 to $100,000.[5]

[3] This is similar to enforcing a ban on proprietary data and algorithms. One example is Article 20 of the General Data Protection Regulation (De Hert, Papakonstantinou, Malgieri, Beslay, and Sanchez 2018).

[4] Source: NAIC 2025 report. This number includes commercial ($72 billion) and private passenger auto insurance ($359 billion). Within the latter, third-party liability make up a majority at $182 billion.

[5] All states that we study follow an "at-fault" tort system and mandate liability insurance. In practice, liability insurance is specified by three coverage limits. For example, 20/40/10 means that, in an accident, the insurer covers liability for bodily injuries up to $40,000

Pricing Insurance prices are heavily regulated. Firms collect large amounts of consumer information in risk-rating, and are required to publish filings that detail their pricing rules.[6] In general, a pricing rule can be summarized as follows, where the price (p) of a policy is:

$$p = \text{base rate} \times \text{characteristics factor} \times \text{coverage factor} + \text{loading cost} \tag{1}$$

Within each firm, prices vary by observable characteristics, coverage choice, and time. Base rates vary only by state and time. Characteristic factors include information about the driver (e.g., age, education level), their vehicle (e.g. vehicle safety features), and their home location (e.g., zipcode-level population density) that are verified and cross-referenced among various public and industry databases. They also contain information from claim and credit databases, including records of past at-fault accidents, traffic violations (e.g. DUI, speeding), and financial records (e.g. delinquency, bankruptcy). Choosing a higher coverage scales prices by a positive factor. Finally, firms charge a loading cost that includes markups and overhead for operational and marketing expenditures.

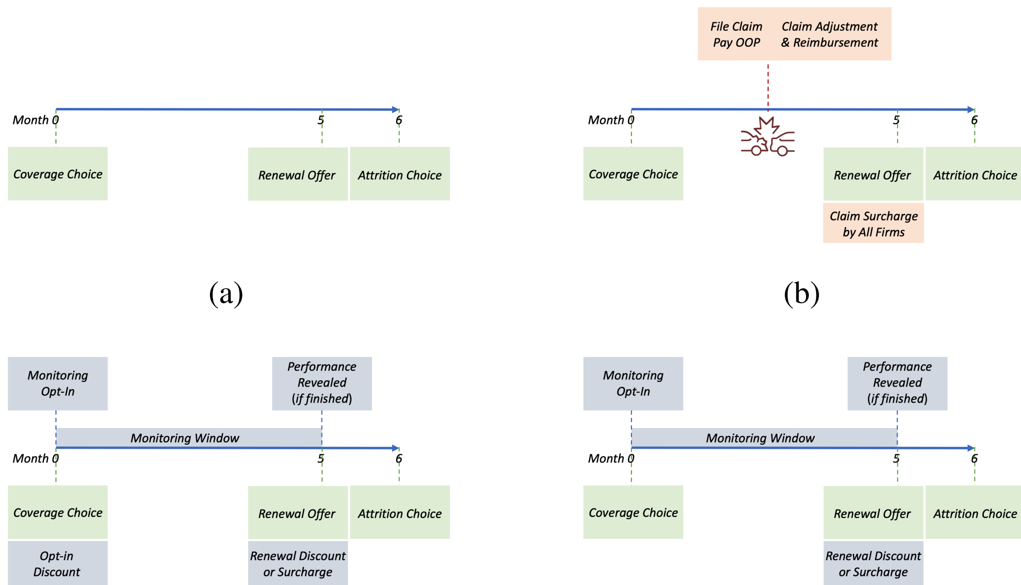
Pricing regulations vary by state and time, but a primary goal across the board is to limit third-degree price discrimination: excessively high prices that hurt affordability, and excessively low prices that raise insurers' default risk. In general, "price optimization" across consumer segments—beyond risk-rating—is generally not allowed, and is explicitly outlawed in 15 states. We introduce the regulatory price controls pertaining to the monitoring program in more detail when we introduce our pricing model in Section 5.

Timing and Dynamics Figure 1 illustrates the timing of new customer interactions with auto insurance in our setting. As shown in panel (a), new customers provide their characteristics at time $t = 0$ ("the initial period"). The firm then assigns each customer (also referred to as a *consumer* throughout the paper) a risk class and generates a personalized price menu from which they select a coverage option (or choose to purchase insurance from another firm). Contracts last six months with no long-term commitment. Toward the end of the period (around month five), the firm issues a renewal price menu, and consumers decide whether to stay or switch. The renewal price menu follows the same structure as outlined in Equation (1), albeit with different characteristics factors.[7]

overall, but no more than $20,000 per victim; it also covers liability for property damage (cars or other infrastructure) for up to $10,000. We quote the highest number here.

[6] Except in Wyoming, which is not in our dataset.

[7] We model renewal pricing changes carefully in Section 3.1, where we separate out the first renewal pricing rules (for both monitored and unmonitored drivers) from subsequent ones. This is because while consumers may see price changes in every renewal due to changes



(a) (b)

(c) (d)

Figure 1: Timing Illustration of Auto Insurance and Monitoring Program

If an auto accident occurs during the coverage period (Figure 1b), the consumer files a claim immediately. After assessment and adjustment, the firm reimburses the consumer based on their chosen coverage level. The claim is then recorded in industry databases and triggers a surcharge at renewal irrespective of whether the consumer stays with the current insurer or switches to another firm.

Dataset 1 - Panel data from an auto insurer Our first dataset comes from a national auto insurer in the U.S. This is a panel that spans 2012 to 2016, and covers 23 states. For tractability, we focus only on *single-driver-single-vehicle* insurance policies sold via the direct channel (online or phone) so that the consumer of the insurance policy is also the driver being insured. We observe more than 1 million consumers for an average duration of 2.86 periods (2.98 periods in our Illinois estimation sample)[8]. The date range spans periods before and after the monitoring program was introduced.

At the beginning of each period, we observe a number of individual characteristics for each

in their observables, the first renewal sees an added change due to the shift from the new-customer pricing algorithm to the renewal one, leading to a typically bigger renewal price change compared to subsequent renewals.

[8]The panel is right-censored, but the censoring is plausibly uninformative.

consumer as outlined in Table A.1. We also see the price paid given their coverage and monitoring choice. Table 1a presents summary statistics of prices, coverage levels, and claims. The average consumer is 33 years old, drives a 2005 vehicle, lives in a zip code with an average annual income of $125 thousands, and has 0.25 recorded at-fault accidents (Table A.1). Per six-month period, the average consumer pays $398 in liability premium and files 0.04 liability claims (about 1 in eleven years). We also observe their assigned risk class, representing the actuarially-fair price assigned to each consumer excluding the coverage factor, markups, and fees. For the average consumer, this is set at $270 per period.

Dataset 2 - Competitor price menus in Illinois Our structural model and counterfactual exercises require accounting for consumers' outside options both with respect to monitoring and to the choice of insurer. To capture the latter, we focus on a single state and augment our main dataset with price menus from the firm's primary competitors. Specifically, we obtain liability coverage quotes offered by the firm's top five competitors in Illinois, ranked by market share.[9] The quotes are generated using Quadrant Information Services' proprietary software, which extracts insurers' filed prices across states and consumer segments. For each consumer in our dataset, we obtain a set of competing prices by precisely matching their characteristics, the state, and the calendar day of their insurance purchase. Table 1b reports the resulting quotes for the five most common liability coverage options across competitors. Our structural analyses focus on Competitors 1 and 2, which offer, respectively, the median and lowest average prices among the top five competitors. Together with the monitoring firm, they comprise the top three firms by market share in Illinois.

1.2 Monitoring Program

Our research focuses on the firm's voluntary monitoring program for new customers.[10] The monitoring process is summarized in Figures 1c and 1d. When consumers first arrive, they choose whether to opt in to monitoring as they review the coverage price menu. All consumers are provided with information on the kinds of driving behavior that are tracked and

[9] We focus on Illinois for three reasons. First, the price menu data significantly expands our dataset, and we choose one state in order to combat the curse of dimensionality in our counterfactual simulations. Second, Illinois featured a change of mandatory minimum plans that provides valuable variation that assists in our model identification. Third, the matching of consumer observables when obtaining competitors' price menus may be imperfect. One way to evaluate this is to also pull price-menu data from Quadrant Information Services for the monitoring firm, and compare that to our observed prices paid for the coverage chosen. Illinois had the best match across all states in squared error terms.

[10] In our setting, monitoring was offered as a one-time program for new customers only.

Table 1: Summary Statistics

(a) Premium, Coverage and Claims (All States, Per 6-month Period)

	Mean	SD	Min	p50	p75	Max
Total premium ($)	658.20	363.83	122.00	574.00	812.00	3,112.00
Liability premium ($)	397.85	210.68	93.59	348.62	481.61	1,902.72
Risk class ($)	270.31	178.99	0.00	223.75	331.58	1,650.20
Total claim count (1e-2)	10.57	42.25	0.00	0.00	0.00	300.00
Liability claim count (1e-2)	4.45	27.35	0.00	0.00	0.00	300.00
Total claim ($000)	0.32	2.81	0.00	0.00	0.00	558.56
Liability claim ($000)	0.16	2.13	0.00	0.00	0.00	526.01
Liability coverage limit ($000)	112.56	113.11	25.00	50.00	100.00	500.00
Has mandatory minimum limit (%)	42.60	49.45	0.00	0.00	100.00	100.00
Has property coverage (%)	62.74	48.35	0.00	100.00	100.00	100.00
Tenure (number of renewals)	2.87	2.39	1.00	2.00	4.00	11.00

(b) By Coverage and Firm (Illinois)

	Liability coverage ($000)					Market Share	
	40	50	100	150	300	Raw	Direct
Average Quotes ($)							
Monitoring firm	312.92	327.35	364.14	402.52	476.53	9.7	11.8
Competitor 1	325.87	350.73	391.10	433.31	468.44	12.8	10.4
Competitor 2	260.25	280.53	316.41	349.76	407.36	54.4	22.2
Competitor 3	377.11	338.78	376.19	403.72	463.11	7.7	15.6
Competitor 4	359.73	442.86	466.27	504.12	610.36	7.7	3.1
Competitor 5	434.27	492.46	548.88	609.42	709.94	7.8	3.2
Coverage share at monitoring firm (%)	19	39	20	19	3		
Average liability claim ($)	154.96	155.42	154.21	140.60	126.21		
Average liability claim count	0.05	0.04	0.04	0.03	0.03		

(a) *Note:* This table reports summary statistics of our main panel data (all 23 states, full unbalanced panel). Risk class is the actuarially-fair premium derived by the company's actuarial team for liability coverage. $N = 6,292,713$.
(b) *Note:* This table reports the average quotes and claims of the monitoring firm and its top 5 competitors by market share in Illinois, which is the focus of our structural and counterfactual analyses. The total number of consumers is 55,302. The total number of quotes is 7,488,568. In Illinois, the mandatory minimum and the most popular coverage changed from $40,000 to $50,000 during the research window, which is why the former had lower share and slightly lower claims. Competitor 3 experienced significant premium reduction over time, which explains why its $40,000 plan is more expensive on average than its $50,000 plan. The market share is based on each firm's aggregate liability premiums written over our research window, obtained from the National Association of Insurance Commissioners' annual reports.

penalized or rewarded—high mileage, driving at night, high speed, and harsh braking—but the exact mapping to discounts is opaque. In addition, the firm offers an opt-in discount on the first-period premium independent of performance. It also spells out the mean and range of the renewal discount that would be applied to all subsequent (renewal) periods.[11]

Consumers who opt in receive a simple device via mail within a week. They then have until the end of month five to accumulate some 100-150 days of monitored driving. If completed, the firm evaluates their performance and includes an appropriate renewal discount when giving renewal quotes.[12] If an accident occurs, monitoring data do not influence claim reporting, handling, or future premium adjustment. Monitoring continues after any disruptions from the accident. During the monitoring period, monitored drivers receive real-time feedback on their performance. Key statistics of recorded trips are posted online. The firm also offers active reminders through media such as text messages, mobile app push notifications, and beeping from the monitoring device when punishable behaviors are recorded.

Nevertheless, monitoring data is *proprietary*. We verify this by confirming that the firm's monitoring information does not appear anywhere in its competitors' price filings. More generally, other firms face many practical hurdles in getting and using monitoring information. First, verifying monitoring outcomes with consumers alone is difficult, labor-intensive, and may be subject to legal liability.[13] More importantly, firms may have different preexisting risk assessments, underlying costs, and markups for serving the same type of consumers.

The program we analyze is the first to achieve large-scale commercialization in the U.S. While competing programs exist, the most prominent ones were offered by agency-driven insurers and lagged behind ours in terms of technology and adoption rate.[14] In aggregate, monitoring generally took up a small fraction of the market during our research window, estimated to be under 4% in 2014 and consisted largely of the program that we study (Ptolemus Consulting 2016).[15] In our structural and counterfactual analysis, we thus abstract

[11] The opt-in discount varied between 0, 5, and 10% over the course of our estimation sample. The average discount was 4%.

[12] 27% of drivers who start monitoring do not finish. Our main analysis treats these drivers as unmonitored and jointly consider consumers' decision to *start and finish* monitoring. This is because 97% of non-finishers drop out during a two-month grace period (no penalty) in which the firm communicates projected renewal discounts to monitored drivers (afterwards, dropping out results in the maximum amount of renewal surcharge). We thus consider non-finishers as if they have reversed their opt-in decision after forming the correct belief about their monitoring outcome. Non-finishers also have a similar risk profile as other opt-out consumers:, when we separate non-finishers from the opt-out group, their average claim count is only 4% below the latter.

[13] The privacy policy agreed upon at monitoring opt-in prevents the firm from sharing personally identifiable data.

[14] Based on various marketing announcements, the highest opt-in rate achieved by a competing program was less than 1% in 2012 and less than 6% in 2016. In contrast, the *finish* rate of monitoring was already around 15% for our monitoring program in 2012 (Fig. B.1a).

[15] Public filings do contain some information on these programs, but it is difficult to interpret reliably. For instance, Reimers and Shiller (2019) uses public filings to construct a dataset of the introduction timeline of U.S. auto-insurance monitoring programs. However, the program we analyze is shown to have been introduced up to four years before what our proprietary data suggests across U.S. states.

away from competing programs.

The industry shifted towards smartphone-based monitoring in the late 2010s and early 2020s. Compared to the device-based program that we study, smartphone-based ones are much less accurate but critically solve the cost bottleneck associated with hardware and signal transmission. As a result, most insurers adopted a continuous monitoring model. Even so, consumers are not monitored all the time, but rather they need to opt into being evaluated on select trip segments. One can consider this model as accepting short-term evaluations (on a few trips) for a discount on the full term.

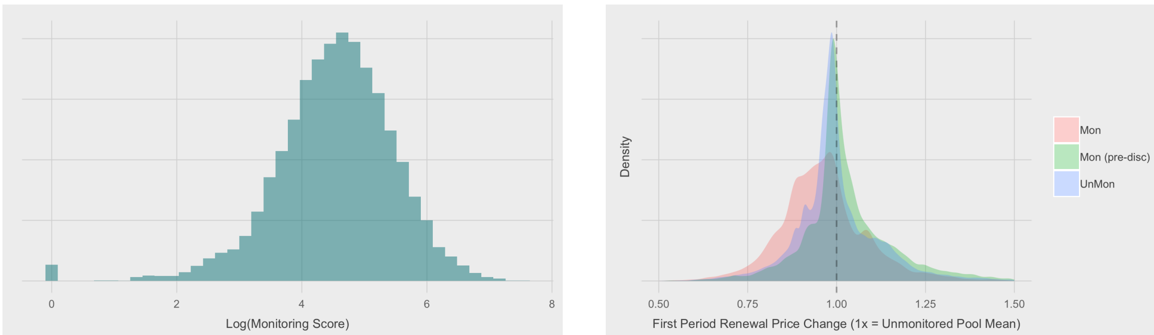
Dataset 3 - Monitoring Our data on the firm's monitoring program includes its pricing schedule, consumers' opt-in choices, and realized monitoring scores and renewal discounts for consumers who opted in.

Monitored drivers' performance is summarized by a one-dimensional monitoring score, whose distribution is plotted in Figure 2a. The score *increases* with the amount of punishable behavior recorded for a given monitored driver. We interpret this score as the output of a fixed monitoring technology that reveals information about consumers' *future* accident risk, given observations of driving behavior during the monitoring period. Figure 3 illustrates this predictive content by plotting the average claim count in the second period by monitoring status and monitoring score quintile (based on performance in the first period). Compared to unmonitored drivers, those who finish monitoring are 22% safer. Among finishers, the quintile of their monitoring score strongly predicts their second-period risk, ranging from 60% better to 40% worse than the opt-out pool.

The firm's monitoring pricing is discussed in detail in Section 5 as well as in Appendix B. Notably, as we show through an event study summarized in Figure B.1b, baseline pricing did not change when the monitoring program was introduced. However, at the end of their first period of insurance, consumers who opted in to monitoring faced different renewal quotes that included performance-based monitoring discounts or surcharges based on their monitoring scores. Figure 2b compares the distribution of first-renewal price changes: normalizing the average price change for an unmonitored driver to be one, the average monitored driver received an extra discount of 7%. Among monitored drivers, those with higher monitoring scores received smaller discounts or higher surcharges (Figure B.2). Moreover, consistent

The discrepancy is largely due to the various trials and R&D efforts employed by the firm to fine-tune the structure and technology of its monitoring program.

with the firm's upfront communication with consumers at the time of purchase, monitoring discounts and surcharges persisted beyond the first renewal as shown in Figure A.3.



(a) (b)

Figure 2: Monitoring Score and Renewal Discounts

Notes: (a) plots the density of the (natural) log of monitoring score for all monitoring finishers. The lower the score the better. Drivers who received a zero score plugged in the device continuously for enough days but did not drive. We ignore these drivers in all subsequent tests. (b) plots the benchmarked (per firm request) distribution of renewal price changes at the first renewal, by monitoring group. 1x represents the average renewal price change factor for the unmonitored group. The one-time monitoring opt-in discount (applied on the first period premium) is taken out in order to isolate the renewal discount for monitored drivers. "Mon" and "UnMon" are monitored and unmonitored groups, while "Mon (pre-disc)" is the renewal price change for monitored drivers without the monitoring discount.

Figure 3: Comparison of subsequent claim cost across monitoring groups

Notes: This is a bin-scatter plot comparing average claim counts in the second period ($t = 1$, after monitoring ends) across various monitoring groups. The benchmark is the unmonitored pool, which is the "opt-out" group. Group "opt-in" includes all monitored drivers that finished the program per the definition in section 1.2. Groups "1" to "5" split the "finish" group based on the quintile of drivers' monitoring scores. A lower monitoring score means better performance.

2 Reduced-form Evidence

This section measures the moral hazard and selection effects associated with the monitoring program. Consumers who opt in to monitoring become safer when monitored. Despite this behavioral change, monitoring reveals persistent and previously unobserved risk differences across consumers, which in turn drive advantageous selection into the program.

2.1 Risk Reduction and the Moral Hazard Effect

If monitoring technology can effectively capture accident risk and individuals' risk is modifiable, then we should expect monitored drivers to be safer during the initial monitoring period than subsequent unmonitored ones—a (reverse) moral hazard effect.[16]

We now directly measure this effect by comparing claim outcomes for monitored consumers before and after monitoring ends. We construct a balanced panel over the first three periods (18 months) consisting of all consumers that are eligible for monitoring, and adopt a *difference-in-differences* approach to control for spurious trends in claim rates across periods that are irrelevant to monitoring. Among monitored drivers, we take the first difference in claim counts[17] between the monitoring and post-monitoring periods. This difference is then benchmarked against its counterpart among unmonitored consumers (the control group). Our main specification adds exhaustive observable controls and coverage fixed effects:

$$C_{it} = \alpha + \tau m_i + \omega \mathbf{1}_{post,t} + \theta_{mh} m_i \cdot \mathbf{1}_{post,t} + \mathbf{x}_{it}' \beta + \mathbf{y}_{it}' \psi + \varepsilon_{it} \tag{2}$$

Here, i and t index the consumer and period in our panel dataset, respectively. C denotes the claim count, and m_i is a consumer-specific indicator for whether i finished monitoring. The vector $\mathbf{x}$ denotes a rich set of observable characteristics that the firm uses in pricing, which includes state fixed effects and variables outlined in Table A.1. y denotes fixed effects for the liability limit chosen as well as an indicator for whether the consumer has purchased property coverage at the start of each period, as summarized in Table 1. Last, we add a specification controlling for driver fixed effects to account for selection on consumer unobservables.

[16]See Fama (1980) and Holmström (1999). A similar setting is studied in Taylor (2004) and Fudenberg and Villas-Boas (2006): consumers may refrain from buying expensive items online if they know doing so will label them as inelastic shoppers and lead to higher future prices.

[17]Throughout our reduced-form analyses, we use claim count as our cost proxy. This is because claim severity is extremely noisy and skewed. This is also common practice in the industry, where many risk-rating algorithms are set to predict risk occurrence only. We therefore present our estimates mostly in percentage comparison terms.

Table 2: **Estimation Results: Moral Hazard Effect**

	dependent variable: claim count (C)										
explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	Parallel Trend/Placebo		
constant	0.048***	0.010	-0.011		0.048***	0.007	-0.013		0.047	0.144	0.055
	(0.000)	(0.009)	(0.009)		(0.000)	(0.009)	(0.009)		(0.095)	(0.092)	(0.099)
monitoring indicator (m)	-0.015***	-0.013***	-0.012***		0.010***	0.008***	0.009***		-0.005***	-0.005***	-0.004**
	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.002)
post monitoring indicator ($\mathbf{1}_{post}$)	-0.001*	-0.003***	-0.003***	-0.002***	-0.001*	-0.003***	-0.003***	-0.003***	-0.003***	-0.004***	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
monitoring duration (z)					-0.029***	-0.024***	-0.024***				
					(0.002)	(0.002)	(0.002)				
interaction ($\mathbf{1}_{post} \times m$)	0.008***	0.008***	0.007***	0.008***	-0.003	-0.004*	-0.004*	-0.003*	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
interaction ($\mathbf{1}_{post} \times z$)					0.014***	0.015***	0.015***	0.014***			
					(0.002)	(0.002)	(0.002)	(0.002)			
observables controls (x)	No	Yes	Yes	No	No	Yes	Yes	No	Yes	Yes	Yes
coverage fixed effects	No	No	Yes	No	No	No	Yes	No	Yes	Yes	Yes
driver fixed effects	No	No	No	Yes	No	No	No	Yes	No	No	No
implied moral hazard effect (%)	32.02%	31.33%	29.97%	31.44%	33.88%	34.04%	33.08%	33.81%	-1.68%	1.9%	-1.46%
pre / post periods - "1st diff"	$t = 0/1-2$	$t = 0/1-2$	$t = 0/1-2$	$t = 0/1-2$	$t = 0/1-2$	$t = 0/1-2$	$t = 0/1-2$	$t = 0/1-2$	$t = 1/t = 2$	$t = 2/t = 3$	$t = 3/t = 4$
treatment group - "2nd diff"	finishers	finishers	finishers	finishers	all monitored	all monitored	all monitored	all monitored	finishers	finishers	finishers
N	2,861,093	2,861,090	2,861,090	2,751,432	2,999,487	2,999,484	2,999,484	2,884,146	1,834,285	1,172,780	700,498

Notes: This table reports results of equation (2). The datasets consists of users that are eligible for monitoring and have stayed throughout the pre / post periods (balanced panel). Compared to columns (5) to (8), columns (1) to (4) remove all drivers that started but did not finish monitoring. *Notes:* This table reports results of equation (2). The datasets consists of users that are eligible for monitoring and have stayed throughout the pre / post periods (balanced panel). Relative to Columns (1) to (4) removes all drivers that have started monitoring but have not finished. The estimate on the interaction term ($\mathbf{1}_{post} \times m$ or z) measures the "treatment effect" of monitoring ending on claim count across periods. We first balance our panel data to include all drivers who stay until the end of the third semester ($t = 3$). This gives us two renewal semesters ($t \in \{1,2\}$) after the monitoring semester ($t = 0$). We control for a full set of observables, including driver and vehicle characteristics and tiers (past records of violations or claims). Continuous observable characteristics are normalized. We report estimates with and without these controls. Columns (3) and (6) are our main specification. Column (3) focuses on monitored drivers who finished within the first period, while Column (6) introduces additional variation in monitoring duration and timing and looks at all monitoring finishers. Columns (1,2,4,5) show robustness of our estimates to observable and coverage fixed-effect controls. The right-most columns are placebo tests for parallel trends among treatment/control groups after monitoring ends. We first try to detect a similar change from $t = 1$ to $t = 2$. We drop all observations from period 0, and roll the post-period cutoff one period forward, so that $1_{post,t} = 1 \iff t \geq 2$ (changed from $t \geq 1$). Naturally, we look at the future trends of monitored drivers who finished within the first semester and drop other monitored finishers. We find similar results by repeating this test in subsequent periods. As we need to balance panels, number of drivers drop in these tests.

Among consumers who are monitored, the actual duration of monitoring varies due to logistical discrepancies such as mailing and installation times; some drivers also start the program but drop out before finishing. We thus introduce an additional specification that adds treatment intensity z_i and the interaction with monitoring timing to our main specification, using all drivers who are monitored. Here, z_i is calculated as the fraction of days monitored in the first period minus the same fraction in post-monitoring periods. [18]

Results are reported in Table 2. We find a large moral hazard effect. Column 3 corresponds to the specification in Equation (2), which shows that the average claim count for monitored consumers is 0.007 or 21.93% higher after the monitoring period. Adjusting for the average monitoring duration of first-period monitoring finishers of 131 days—only a fraction of the monitoring period—the moral hazard effect would be 29.97%. This result is stable across specifications and only gets slightly bigger as we add controls; for instance, adding additional variation in monitoring duration (treatment intensity) generates a similar result of 33.08% (derived by setting $z = 1$ and adding up the two interaction coefficients in column 7). We test for parallel trends between the monitored and unmonitored groups by repeating our main specification in balanced panels encompassing subsequent unmonitored periods, which also serves as a placebo check. As columns 9-11 show, no differential claim change across periods can be detected.

Our main results above use a balanced panel covering the first three periods of drivers' engagement with the monitoring firm only. Below, we double the time horizon to six periods (three years) and use the full unbalanced panel. We adapt specification 2 slightly to look at the claim progression over each of the six periods across monitoring groups. The fixed-effect estimates for each policy period, θ_t, are illustrated in Figure 4. The level difference between the grey and the orange lines after period 0 represents a persistent difference in risk between opt-in consumers and their opt-out counterparts—a selection effect quantified in the next subsection. There is an additional risk reduction in period 0 among opt-in consumers, which closely tracks the moral hazard effect shown in Table 2 column (3).

$$C_{it} = \alpha + \tau m_i + \omega_t \mathbf{1}_t + \theta_t m_i \cdot \mathbf{1}_t + \mathbf{x}'_{it}\beta + \varepsilon_{it} \tag{3}$$

[18] Monitoring duration may be correlated with claims due to reasons other than moral hazard, and particularly if claims itself leads to a non-finishing status. However, only 13 claims occurred within 7 days before or after monitoring drop-out out of more than a hundred claims that we observe for monitored drivers who did not finish. Further, if some monitored consumers drop out as they discover that they cannot change their risk, our estimate here would encompass both the moral hazard effect and a selection on moral hazard effect *a la* Einav, Finkelstein, Ryan, Schrimpf, and Cullen (2013).

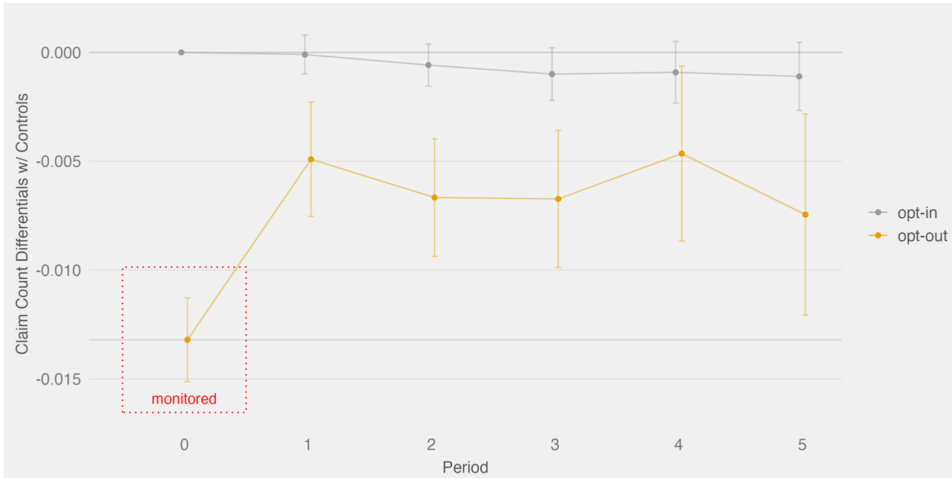


Figure 4: Claim Progression across Monitoring Groups

Notes: This graph reports the fixed effect estimates of eq. (3) using the full unbalanced panel. In Appendix C.1, we show that our results are robust to using a six-period balanced panel. The grey line plots ω_t while the orange line plots $\omega_t + \theta_t$, both against insurance periods t. The red box is superimposed ex-post to represent the period when opt-in consumers are monitored. Error-bars report 95% confidence interval.

Robustness and Discussion We show in Appendix C.1 that our results in this section are robust to whether we use a balanced panel or not. We also investigate heterogeneity in the moral hazard effect in Appendix C.2. Overall, systematic heterogeneity across driver characteristics or plan options is rare, although we find that drivers who are initially assigned a higher risk class tend to experience a larger moral hazard effect.

We discuss two important caveats of our results. First, monitoring mitigates moral hazard because it builds a reputation mechanism by signaling consumers' persistent risk level, *not* because it directly rewards effort during monitoring. The magnitude of risk reduction can be different in the latter setting.[19] Second, our estimates measure a treatment-on-treated effect. Although we find little observed heterogeneity in the moral hazard effect among opt-in consumers, it is possible that the majority of unmonitored drivers are less capable of altering their risk. The moral hazard effect that we have identified is thus likely larger than the population average.[20] To avoid external validity concerns, our counterfactual analysis maintains the opt-in structure of the monitoring program and does not extrapolate to scenarios where

[19] We are also unable to disentangle the "Hawthorne effect" from consumers' responsiveness to financial incentives in our estimate. Since consumers must be aware of the data collection to be incentivized for it, we consider this effect as part of the moral hazard effect.

[20] We also suppress selection on moral hazard in counterfactuals (Einav, Finkelstein, Ryan, Schrimpf, and Cullen 2013). In equilibrium, the firm assesses the signal that monitored consumers send based on their future claim records when they are no longer monitored, which corresponds to the renewal discount it gives. Thus, risk reduction is compensated only to the extent that it correlates with consumers' unmonitored risk type. If safer consumers' risk levels are also more responsive to incentives, as suggested by a pure effort cost model, selection on the incentive effect can be important. In particular, perfect revelation of a continuum of risk types is possible, as characterized in Mailath (1987), with a monotonicity condition similar to the single-crossing condition. However, consumers likely have multidimensional heterogeneity in reality, so consumers' performance during monitoring may not perfectly reveal their risk types (Frankel and Kartik 2016).

the monitoring rate is significantly higher than in the data.

2.2 Private Risk and the Selection Effect

Our analysis above suggests that monitored drivers remain safer than their counterparts even after the monitoring period.[21] This suggests that the monitoring pool may be advantageously selected. Table 3 reports the results of regressing claim counts in the first period only ($t = 0$) on a monitoring indicator, with the same control specifications—and produces nearly identical coefficients—as columns (1) to (3) of Table 2. The coefficient on the monitoring indicator suggests that the risk differential between the two groups is larger than the moral hazard effect discussed in the previous section: the latter only accounts for 61.10% of the risk differential. This suggests that consumers possess private information about their accident risk, driving advantageous selection into monitoring.

Table 3: First-Period Claim Comparison Across Monitoring Groups

	Dependent variable: claim count (C)		
	(1)	(2)	(3)
monitoring indicator (m)	-0.016***	-0.013***	-0.012***
	(0.001)	(0.001)	(0.001)
Constant	0.048***	0.023**	-0.001
	(0.0003)	(0.010)	(0.011)
observables controls (x)	No	Yes	Yes
coverage fixed effects	No	No	Yes
risk reduction (%)	-36.03%	-29.58%	-27.16%
Observations	898,925	898,925	898,925

Notes: This table reports the results of a regression where the dependent variable is first-period claim count, and the independent variables are the monitoring indicator and controls. Mirroring Table 2 columns (1) to (3), the monitoring group consists of all monitoring finishers. *p<0.1; **p<0.05; ***p<0.01

Selection into monitoring also implies that the technology is effective at capturing previously unobserved differences in drivers' risk types. In Appendix Figures A.5 and A.6, we demonstrate that both a driver's choice to be monitored and their first-period monitoring score strongly and persistently predict future-period claim counts. For example, receiving a score that is one standard deviation above the mean is associated with a 29% higher average

[21]For instance, the coefficients on the monitoring indicator (or monitoring intensity) in Table 2 are large and negative, and the claim count differentials for monitored drivers, depicted in orange in Figure 4, are persistently lower than their unmonitored counterparts in grey.

claim count in the second period. Controlling for claims has little impact on these estimates, which suggests that the high-frequency monitoring data is highly informative of driver risk relative to sparse claims records.

A potential concern is that monitored drivers may learn to become persistently safer due to monitoring. This does not influence our moral hazard estimates, which are derived from the observed behavioral change between the monitoring period the periods afterward regardless of the pre-monitoring level of consumer risk. However, if consumers' post-monitoring risk is reduced persistently due to monitoring, ignoring such effect may exaggerate the degree of advantageous selection. In Appendix C.3, we use consumers' public "at fault accident" (AFA) records to test for this possibility. Although the AFA records are substantially noisier than our proprietary claim records, we find persistent gaps between the average risk captured by AFAs among monitored and unmonitored drivers before and after they engage with the monitoring firm, consistent with our main results.

2.3 Renewal Elasticity

The welfare benefit of monitoring may be limited if the firm extracts large rents during renewal periods. However, when pricing the monitoring score, the firm must consider that a monitored consumer can choose not to renew and leave for another firm. We quantify the price elasticity of renewal with the following regression:

$$\mathbf{1}_{it}^{\text{renewed}} = \alpha M_i + \theta \log p_{it}^{\text{renewal}} + \theta_M M_i \cdot \log p_{it}^{\text{renewal}} + \psi s_i + \mathbf{x}_{it}' \beta + \varepsilon_{it}. \tag{4}$$

The dependent variable indicates whether consumer i renewed their insurance plan in period t. We augment the consumer-specific monitoring indicator m to get M, which admits three discrete monitoring *groups*: unmonitored consumers, monitored consumers that received discounts, and monitored consumers who received no discount or even a surcharges. We include the same controls $\mathbf{x}$ as Column (3) of Table 2. We also add the consumer-specific monitoring score s_i, normalized among monitored consumers and set to 0 for unmonitored ones. The parameters of interest are θ and θ_M, which combines with M to give price elasticities across monitoring groups.

The regression above may produce biased estimates if renewal prices are endogenous to unobserved demand factors or competitor pricing. For robustness, we adopt an instrumentalvariable approach to compute price elasticity. We narrow the sample down to 30-day windows around rate revision events within each state, and instrument for renewal pricing using Z_{it}, an indicator for whether the consumer arrived after the rate revision in their market took effect. Our estimation adds rate revision fixed effects to the controls of both the first-stage and the reduced-form regressions (Equations 4 and 5). The exclusion restriction is supported by an event-study argument: consumers arriving right before and after a rate revision event should be similar, and the price differential they face is thus independent from their idiosyncratic unobservable demand factors.

$$\log p_{it}^{\text{renewal}} = \tau M_i + \omega Z_{it} + \omega_M M_i \cdot Z_{it} + \phi s_i + \mathbf{x}_{it}' \gamma + v_{it}. \tag{5}$$

Figure 5 shows that both OLS and IV regressions produce similar price elasticity estimates across monitoring groups. During renewal, monitored consumers who receive discounts are less price sensitive than the average unmonitored consumer, while the opposite is true for those who receive no discount or who receive a surcharge. This implies that the firm faces a steeper residual demand curve when giving discounts and a flatter one when imposing surcharges. For safe monitored drivers—those who proved to be less risky than the firm initially expected—this suggests that the firm can extract an informational rent by raising markups after the monitoring period. However, it is unclear how the firm should react to the higher price sensitivity among monitored but discount-ineligible consumers. While steering the riskiest drivers toward competitors is likely profitable, many risky monitored drivers may still yield positive marginal profits. Moreover, the firm's ability to surcharge is directly constrained by regulatory price controls—a feature we discuss in detail in Section 5.2 after introducing our pricing model.

The need for structural models Taken together, our results in this section show that monitoring produces highly informative signals on consumers' future accident risk, which effectively separates drivers who are otherwise indistinguishable to the firm. This leads to advantageous selection into the program and safer driving when consumers are monitored. We have thus separately identified the effects of moral hazard and selection.

However, in order to evaluate the welfare impact of monitoring and the trade-offs associated with data or pricing regulations, a structural model is needed for three reasons. First, monitoring affects consumer welfare through several interacting channels that cannot be combined without estimating risk preference. Beyond an immediate reduction in accident risk, moni-

Figure 5: Price Elasticity of Renewal Acceptance by Monitoring Groups

Notes: The dots report the renewal price elasticity estimates across monitoring groups using OLS or IV, which correspond to $\hat{\theta} + \hat{\theta_M} \cdot M$ in Equation (4). They measure the average percentage point increase in renewal likelihood with respect to 1% increase in renewal price. M are indicators for three monitoring groups (from left to right): monitored consumers that receive price discounts, monitored consumers that receive no price discounts or surcharges, unmonitored consumers. Lines reports 95% confidence interval, with standard errors clustered on the consumer level.

tored drivers may purchase higher coverage in anticipation of future discounts, but they may also be exposed to greater premium volatility due to reclassification risk (Handel, Hendel, and Whinston 2015). Second, consumers make three discrete choices: insurance coverage, monitoring opt-in, and attrition during renewals. Their inter-dependence and correlation with consumer risk jointly determine the degree of asymmetric information (between consumers and firms and across firms), shaping pricing and efficiency in equilibrium. Third, the pricing of monitoring is inherently dynamic and multidimensional; it is also subject to intense regulatory pressure that may have lead to overly conservative pricing.[22] Simulating how prices would change in counterfactuals therefore requires modeling the firm's main pricing levers—how it encourages consumer opt-in ex ante (to obtaining the monitoring scores), and how it limits attrition while extracting rents ex post.

In Sections 3 and 4, we estimate a structural model of accident risk and consumer choice. In Section 5, we present a model of firm pricing across monitoring groups both pre- and post-monitoring, which facilitates welfare and counterfactual calculations.

[22] For example, as shown in Appendix B, the firm did not raise price for the unmonitored pool, which is primarily to appease some state regulators' aversion to price increase (Ben-Shahar 2023).

3 A Model of Demand for Auto Insurance and Monitoring

We model the dynamic insurance choices of a panel of consumers, beginning with their first interaction with our firm. In the initial period, each consumer chooses a level of coverage (y) along with a choice of whether to opt in to the monitoring program $m \in \{0,1\}$. In each subsequent (renewal) period, monitoring is no longer offered, but the consumer again chooses a coverage level (y) and whether to stay with the firm or switch to another (f).

Consumers are forward looking with standard von Neumann-Morgenstern utility under constant absolute risk aversion with parameter γ. As such, they make their decisions in each period t with respect to their expected consumption utility in period t and the expected continuation value from the subsequent periods. At the beginning of each period, each consumer observes a menu of insurance plans along with their respective premiums and coverage limits. If an accident occurs over the course of the period, then the consumer expects to pay an out-of-pocket liability expenditure (henceforth "OOP") that depends on the severity of the accident and the coverage limit chosen at the beginning of the period. In addition to these direct monetary costs, the consumer may face choice frictions against overriding the default mandatory-minimum plan, opting in to monitoring in the initial period, or switching plans or firms in renewal periods.

Putting these factors together, a representative consumer who makes choice $d = (y, m, f)$ in period t obtains the following flow consumption value h, conditional on their characteristics x_t, their previous period choice d_{t-1} and the realization of accident losses in that period A_t:

$$h(d|x_t, A_t, d_{t-1}) = -\underbrace{p_t(d|x_t)}_{\text{price}} - \underbrace{\eta_t(d|d_{t-1})}_{\text{choice friction}} - \underbrace{e(d|A_t)}_{\text{OOP}}. \tag{6}$$

For a given set of observables x_t, each firm f offers a price menu p, which specifies prices of a common set of plans Y, where $y \in Y$ indexes coverage levels increase monotonically with price. Customers at the monitoring firm may also receive a multiplicative discount or surcharge associated with their monitoring choice and performance. The choice friction $\eta_t(d|d_{t-1})$ is composed of several components. In the first period, it includes an additive disutility factor associated with monitoring ξ, which captures unobserved factors such as hassle costs and privacy concerns. In renewal periods, it includes additive inertia costs for switching plans η^y and for switching firms η^f. The OOP corresponds to the portion of accident expenditures that exceeds the coverage level of plan y: $e(A_t, d) = \max(A_t - y_d, 0)$.

At the start of each period, the consumer observes their current price menu, but they do not yet know whether an accident will occur, or what the premiums in future periods will be. To account for the stochastic nature of these variables, we assume that the consumer correctly anticipates their distributions and can take expectations with respect to them. As we detail in Section 3.1, OOP is drawn from a Poisson-Pareto distribution that is truncated below by each plan's coverage limit.

While we observe the price menus offered to consumers across firms and plans, our estimation requires pricing *rules*—that is, a model of how price menus would evolve at renewals based on the (potentially different) realization of stochastic accidents and monitoring scores. Let f^0 denote the monitoring firm; the renewal price menu facing its customers is given by:

$$p_{t+1}(d|\tilde{x}_t, A_t, s, \nu_t) = \begin{cases} p_t(f^0, y_d|\tilde{x}) \cdot (1+r_c)^{\mathbf{1}\{A_t>0\}} \cdot r_t(m_d|\tilde{x}, s, \nu_t) & \text{if } f_d = f^0 \\ p_0(f_d, y_d|\tilde{x}) \cdot (1+r_c)^{\mathbf{1}\{A_t>0\}} & \text{if } f_d \neq f^0. \end{cases} \tag{7}$$

Here, $\tilde{x}$ represents consumer observables that don't relate to accidents, monitoring or pricing and r_c represents a fixed rate-increase factor (an *accident surcharge*) for each new accident accrued.[23] As accident records are public, this affects pricing from all firms in the market. As a simplifying assumption, we assume that consumers anticipate at most one accident per period.[24] Apart from the accident surcharge, renewing consumers' premium evolves based on a renewal factor $r_t(m|x_t, s, \nu_t)$ that depends on their observables, an additive price shock ν_t, and, if they had opted into the monitoring program, their monitoring score s. Non-renewing consumers instead face a new-customer ($t = 0$) price menu from their chosen competitor.[25]

Putting these pieces together, we summarize the consumer's dynamic decision problem in each period t. Denoting the tuple of stochastic variables $Q_t = (A_t, \nu_t, s)$, the consumer chooses their best option d based on the expectation with respect to Q_t of their consumption utility in time t and the continuation value $V(d|Q_t; x_t, p_t, d_{t-1})$, plus an idiosyncratic Type 1 extreme value shock ε_{dt}:

$$d_t = \arg\max_d \left\{ \mathbb{E}_{Q_t} \Big[u(h(d|Q_t; x_t, p_t, d_{t-1})) + \delta V(d|Q_t; x_t, p_t, d_{t-1}) \Big] + \varepsilon_{dt} \right\}. \tag{8}$$

[23] Note that the realizations of A_t, s and ν_t are observable to the firm at the time that renewal prices are offered.

[24] This simplification is motivated empirically as we discuss below. However, our model is able to allow for multiple accidents per period in principle. In this case, r_c would be applied for each accident and the total OOP for the period would be a sum of $\max(A_{tn} - y_d, 0)$ across n accidents.

[25] Consumers have a three-period planning horizon in our model, and so they need to anticipate one more price evolution at the competitor firm when considering switching to another firm after the first period. Here, instead of estimating a separate pricing rule for each competitor, we assume that the expected price evolution at competitors follows the non-monitoring renewal factor $r_t(m = 0|x_t, s = 0, \nu_t)$.

To define the continuation value, we make five simplifying assumptions. (i) We assume that consumers make their choices each period with respect to a finite horizon, using two-period look ahead (e.g., internalizing three periods of utility at each decision point). (ii) We assume that consumers anticipate at most one accident occurrence within a decision horizon (e.g., across three periods). (iii) We assume that after the first renewal period, consumers anticipate a renewal rate change only if an accident occurs. (iv) We assume that consumers anticipate making a decision to switch firms or plans only upon a renewal price change (i.e., at the first renewal, or upon an accident). (v) We assume that consumers' outside options consist of plans from the firm's top two competitors.

These assumptions are empirically motivated. Fewer than 0.5% of drivers in our sample incur multiple accidents within a range of three periods. Only 1.3% of drivers make multiple switches—either to a new plan or a new firm—across up to 9 renewals per driver. Among observed switches, nearly half (44.2%) occur at the first renewal, when the firm transitions from its new-customer pricing algorithm to its renewal-pricing algorithm. The lower frequency of switching after the first renewal corresponds greater pricing stability: absent accidents, the average renewal rate change is -0.14%. Our choice of a three-period planning horizon matches the average tenure of drivers in our estimation panel (2.98 periods). We discuss this in detail and conduct robustness exercises with alternative planning horizons in Sections 4.3 and 5.4.

Finally, the extent to which consumers attrit in renewal periods may depend on the prices they expect to receive from competitors. Because the auto insurance market is highly fragmented with over fifty firms, we focus on the top three by market share. As we show in Table 1b, this includes the monitoring firm and Competitors 1 and 2, which represent the median and the lowest price-setters, respectively, among the top-five competitors for which pricing data are available. Together, these firms account for 44.4% of the direct (non-agency) market (42.1% of the total market), of which 11.8% corresponds to the monitoring firm. Given their prominence and the representativeness of their pricing, we use these two firms to approximate consumers' outside options in our demand model.

3.1 Econometric Specification

Our model involves several sets of parameters, each relating to a different component of drivers' expectations when making a decision: costs in the form of claims risk and renewal prices and preferences captured by utility parameters such as risk aversion and switching frictions. We assume that consumers have rational expectations regarding the costs involved with each insurance plan under their consideration. Their utility parameters can thus be interpreted as explaining residual variation in choices beyond what can be explained by underlying costs. Because our goal is to fit micro-data choice variation as opposed to aggregate market shares, our model allows for substantial heterogeneity in each of its key parameters. Almost all latent parameters are allowed to vary with a vector of 28 observable characteristics. In addition, we model asymmetric information between drivers and insurers by endowing each driver with a *private* risk type captured by a time-invariant component of accident risk that is known to the consumer but unobserved by the firm. The firm knows only the distribution of private types when setting prices, while each consumer internalizes their private type when selecting plans and the monitoring participation.

In the remainder of Section 3.1, we outline our econometric specification. We start by modeling risk: claims, OOP, a driver-specific private risk component, and a reverse moral hazard effect due to monitoring. We then move on to modeling how the monitoring score captures risk and affects renewal price. Finally, we specify utility as well as consumers' risk aversion and choice frictions. Sections 3.2 and 3.3 provide an overview of identification and estimation.

Claims risk and OOP Following actuarial convention, we model accident arrivals as the sum of two independent Poisson draws: major (maj) and minor (min) ones.

$$N_{it} = N_{it}^{\text{maj}} + N_{it}^{\text{min}} \quad \text{and} \quad \lambda_{it} = \lambda_{it}^{\text{maj}} + \lambda_{it}^{\text{min}}, \quad \text{where } N_{it}^{\bullet} \sim \text{Poisson}(\lambda_{it}^{\bullet}). \tag{9}$$

The severity of major accidents is fat-tailed and follows a Pareto distribution with a position—and hence a minimum—of $\$10,000$. The severity of minor accidents follows a log-normal distribution and and is typically below $\$10,000$.

$$S_{it}^{\text{maj}} \sim \text{Pareto}(10^4, \alpha_{it}^{\text{maj}}) \quad \text{and} \quad \log(S_{it}^{\text{min}}) \sim \mathcal{N}(\mu_{it}^{\text{min}}, \sigma_{it}^{\text{min}}). \tag{10}$$

This distinction is conceptually important because the minimum coverage option in our Illinois panel is $40,000. As such, while any accident would trigger a rate increase for renewal prices, only major accidents can lead to OOP. Under our model, the expected value of a consumer's OOP is the product of their expected number of major accidents, as determined by λ^{maj}, and the expected cost of a major accident, as determined by α^{maj} and their chosen liability coverage. We model the severity shape parameters as generalized linear models of observables, including zip code income and vehicle class:

$$\alpha_{it}^{\text{maj}} = f(\theta_0^{\alpha_{\text{maj}}} + \mathbf{x}_{it}' \cdot \theta_1^{\alpha_{\text{maj}}}) \tag{11}$$

$$\mu_{it}^{\text{min}} = \theta_0^{\mu_{\text{min}}} + \mathbf{x}_{it}' \cdot \theta_1^{\mu_{\text{min}}}. \tag{12}$$

Since $\alpha^{\text{maj}} < 1$ implies infinite Pareto variance, we parametrize it with a scaled inverse-logit GLM where $f(x) = 1 + Logit^{-1}(x) \cdot 10$ in the estimation procedure. This bounds the domain in a numerically efficient way while allowing for a sufficiently wide range of possible values. We further assume that consumers do not anticipate accident losses that are beyond half a million dollars, which is the largest claim we observe. Taken together, the accident loss within a period A_{it} is given by $N_{it}^{\text{maj}} \cdot S_{it}^{\text{maj}} + N_{it}^{\text{min}} \cdot S_{it}^{\text{min}}$, which is then split into a covered amount based on the consumer's chosen coverage level and a residual incurred by the consumer as an OOP.

Private risk and moral hazard Our analysis in Section 2.2 suggests that accident risk may change while the driver undergoes monitoring, reflecting a moral hazard effect. It also contains a persistent private component that cannot be predicted by observables but can be partially revealed through monitoring scores. Motivated by these findings, we model drivers' accident rates as follows:

$$\log \lambda_{it}(m,z) = \underbrace{\hat{\lambda}(x_{it})}_{\text{public base rate}} + \underbrace{m \cdot \theta_0^{\text{mh}} + z \cdot \theta_1^{\text{mh}}}_{\text{moral hazard effect}} + \underbrace{\varepsilon_i^{\lambda}}_{\text{private type}} \quad \text{and} \tag{13}$$

$$\text{where } \hat{\lambda}(x_{it}) = \theta_0^{\lambda} + \mathbf{x}_{it}' \cdot \theta_1^{\lambda} \tag{14}$$

$$\log \lambda_{it}^{\text{maj}}(m,z) = \log \lambda_{it}(m,z) + \theta_0^{\text{maj}}, \tag{15}$$

The accident rate for consumer i in period t is the sum of two components: a public signal modeled as a linear combination of observables $\mathbf{x}_{it}$, and a private error term ε_i^{λ} that is consumer-specific, time-persistent, and unobservable to firms. When consumers opt into monitoring ($m = 1$) in the first period, we model the moral hazard effect as a mechanical shift in their accident rate λ that depends on their monitoring intensity z. Equation (15) models a consumer's major accident rate as a scalar fraction ($\exp(\theta_0^{\text{maj}})$) of their total accident rate.[26]

[26]As major accidents are very rare, we do not model heterogeneity in the fraction of major accidents.

Monitoring Score If a consumer opts into monitoring, their driving is tracked for several months, yielding a large set of unstructured data that the firm synthesizes into a one-dimensional monitoring score through a predictive algorithm. As we showed in Section 2.2, the monitoring score is positively correlated with private risk. However, there remains a substantial amount of random variation due to noise in the raw driving data and the interpretation of the data through the algorithm. To capture both of these forces, we model the monitoring score of an individual i as follows:

$$s_i(m,M) = \theta_0^s + \theta_1^s \cdot \varepsilon_i^\lambda + \theta_2^s \cdot \hat{\lambda}_{it=0} + \varepsilon_i^s \quad \text{where } \varepsilon_i^s \sim N(0,\sigma^s). \tag{16}$$

The monitoring score linearly interpolates between a driver's public risk type $\hat{\lambda}_{it=0}$ (based on observables) and their private type ε_i^λ, plus an idiosyncratic mean-zero shock that captures the noise in the monitoring process (and thus, the added reclassification risk). The relative magnitudes of the coefficients on the risk components determine the extent to which the monitoring score updates the firm's prior beliefs about a driver's risk type. As such, a privately safer driver would expect their score to reflect a lower risk relative an average driver with the same observables. However, the idiosyncratic shock ε_i^s, which accounts for noise in the monitoring process, introduces additional reclassification risk, which may prevent some risk-averse drivers from opting in even if they expect a discount on average and suffer no monitoring disutility.

Renewal Prices The renewal factors, $r_t(m|x_{it}, s_i, \nu_{it})$ as described in Equation 7, govern how consumers' price menus would have evolved under different monitoring and claim realizations. We model them as follows:

$$r_t(m|x_{it}, s_i, \nu_{it}) = \begin{cases} \theta_0^{r0} + \theta_1^{r0} \cdot RC_{it} + \nu_{it} \cdot \sigma^{r0}, & \text{if } t = 0 \text{ and } m = 0, \\ \theta_0^{r1} + \theta_1^{r1} \cdot RC_{it} + \theta_2^{r1} \cdot s_i + \nu_{it} \cdot \sigma^{r1}, & \text{if } t = 0 \text{ and } m = 1, \\ \theta_0^{r} + \theta_1^{r} \cdot RC_{it} + \nu_{it} \cdot \sigma^{r}, & \text{if } t > 0, \end{cases} \tag{17}$$

where $\nu_{it} \sim N(0,1)$.

A consumer i who faces a price p for a given plan in period t expects to face a renewal price of $p \cdot r_t(m|x_{it}, s_i, \nu_{it})$ for period $t+1$ if they do not incur an accident. The first renewal factor ($r_{t=0}$) is distinct from that of subsequent periods in two ways. First, it is generally higher—in large part because it corresponds to a shift from the firm's initial-period pricing algorithm to a distinct renewal pricing algorithm. Second, it incorporates additional information about private risk obtained through the monitoring program. After the first renewal ($t > 0$), monitoring no longer plays an active role in pricing and the renewal factors $r_{t>0}$ tend to vary much less. Nonetheless, as renewal factors enter prices multiplicatively, monitored drivers carry forward their discount or surcharge into perpetuity.

To capture these different renewal factor paths in a parsimonious manner, we model r_t as a linear function of a consumer's risk class *RC*—the actuarially fair premium based on the firm's risk rating, which condenses the information contained in observables x for the sake of pricing and is taken as data—as well as an intercept, their monitoring opt-in choice m and score s (if relevant), and an idiosyncratic mean-zero error term ν. We estimate a different set of coefficients θ^r and error variance σ^r for the first renewal with and without monitoring, and for subsequent renewals, as characterized in Eq. 17.

Taken together, we have six parameters to capture each pricing regime and four to capture each monitoring regime. We observe three pricing regimes and three monitoring regimes over our research period, captured by a total of thirty parameters.

Risk Aversion and Utility Specification We evaluate utility using a normalized second-order Taylor approximation of von Neumann-Morgenstern utility around income w. This approach is similar to Cohen and Einav (2007), and belongs to a broader class of "negligible third-derivative" models (Barseghyan, Molinari, O'Donoghue, and Teitelbaum 2018). The main advantage is that it simplifies the nonlinearity of the utility function, allowing us to numerically integrate over stochastic accident occurrence states while analytically reducing the stochasticity associated with claim severity, monitoring score, and renewal prices.

$$u_{it}(h) = (w_{it} + h) - \frac{\gamma_{it}}{2}(w_{it} + h)^2. \tag{18}$$

We use drivers' home zip code income in the corresponding calendar year as a proxy for w. This is supported by many recent surveys.[27] Moreover, given our utility specification, any heterogeneity in income levels can be re-parameterized to be captured by risk aversion γ_{it}, which we allow to vary flexibly based on a rich set of observables (Jeziorski, Krasnokutskaya, and Ceccarini 2014):

$$\gamma_{it} = \exp(\theta_0^\gamma + \mathbf{x}_{it}' \cdot \theta_1^\gamma). \tag{19}$$

[27] The median U.S. household only has around \$1,700 in liquid wealth (Kaplan, Violante, and Weidner 2014). We halve the annual zipcode income to match the six-month period.

Choice Frictions Our demand model allows for four types of utility frictions: (i) a fixed effect for the default plan featuring the mandatory minimum coverage; (ii) a monitoring disutility; (iii) a plan-switching cost; and (iv) a firm-switching cost. In each case, the friction enters consumers' utility as an additive scalar parameter that varies with consumer observables and applies for a subset of menu options. In this way, these frictions explain the mean residual variation in choices that are not explained by the direct plan values. To capture this flexibly across different types of drivers, we model each friction as a linear regression of observables.

3.2 Identification

We now provide an informal discussion of the variation in our data that allows us to identify the parameters of our model.

Claims Risk Parameters We observe a panel of claim events (and non-events) for a large number of consumers. Under a Poisson model, each individual's average claims rate is a sufficient statistic for their risk parameter. Because accidents are rare, however, we gain precision by pooling in the cross-section through the regression function described in Equation (13). This cross-sectional variation is especially important for identifying the severity parameters (based on observed claim severity), as many drivers do not experience a major claim in our sample. The private risk component ε_i^λ can be thought of as a random intercept in the Poisson regression function. In this sense, our panel of claims is sufficient to identify it in distribution. In addition, for consumers who entered monitoring, we also observe monitoring scores that are informative of private risk as described in Equation (16).

Utility Parameters The identification of utility parameters such as consumers' risk aversion and utility frictions relies on rich variation in the pricing and offerings of insurance coverage and monitoring. For each consumer, we observe a menu of choices with different characteristics and prices determined by the timing of their arrival at the firm. Our claims and price models map these choices into expected costs for the current and subsequent periods. The utility parameters then rationalize the cost trade-offs that induce choice shares that match those observed in the data.

Our Illinois panel covers several baseline pricing regimes and telematics pricing regimes. As we demonstrate in Table A.5 and Figures B.3 and B.4, each regime exhibits a distinct relationship between claims risk and renewal pricing. These relationships arise from changes in both the assignment and the pricing of drivers' risk classes and monitoring scores as part of major rate revisions approved by state commissioners and updates to the monitoring algorithms. As we showed in Section 2.3, the average price elasticity estimated within narrow windows around these revision events—during which consumers' exposure to the new rates is plausibly random—is similar to the average elasticity across the full sample (Equation (5) and Figure 5). As such, although our structural analysis uses the whole sample in order to leverage other sources of variation, we expect that restricting attention to these quasi-experimental windows would yield similar results as well.

We also observe variation in the menu of plans available to different consumers and over time. For instance, consumers who arrived before monitoring was introduced or whose vehicles were older than 1995 were not eligible for the monitoring program. Moreover, a change to the mandatory minimum requirement was implemented during our sample period, exogenously changing the baseline coverage from \$40,000 to \$50,000.

The identification of the risk aversion γ_{it} and friction parameters closely follows the literature. Different γ_{it} values imply different gradients of Δu_{it} across the multiple coverage options observed in the data. Conditional on risk parameters, risk aversion is therefore identified from how coverage shares vary with the contract space and with pricing gradients across coverage options, as studied in Kircher, Ericson, Spinnewijn, and Starc (2015) and Barseghyan, Molinari, O'Donoghue, and Teitelbaum (2018). In addition, similar to Handel, Hendel, and Whinston (2015), consumers' risk aversion affects their plan choice both because higher coverage reduces the risk of OOP (a static channel) and because more price fluctuations create reclassification risk (a dynamic channel). The friction parameters—plan-specific fixed effects and path-dependent switching costs—are modeled analogously to Handel (2013), which rationalizes features of the data not captured by risk and risk aversion alone, including the observed share of the mandatory minimum plan, the attrition and plan-switching rates in renewals, and the monitoring opt-in rate.

Claim Severity and Renewal Pricing Parameters Our models of claim severity and renewal pricing are meant to parsimoniously represent the ways in which these empirical objects co-vary with observables in our data. Per actuarial standards, claim severity is considered to be orthogonal to accident risk, and largely depends on the characteristics of the vehicle being driven and the area being driven in. As such, it can be estimated separately from our claim occurrence and demand models. We accomplish this through a standard generalized linear model of drivers' observable characteristics that is identified in the cross-section. Since major claims are sometimes capped above by coverage choice, we incorporate such truncation, whenever observed, in the Pareto maximum likelihood estimation.

Similarly, our renewal pricing parameters model the pricing rule based on observed renewal price changes. We fit this using straightforward OLS regressions based on the realized price menu offered to each consumer throughout their tenure with the firm. This is a fixed algorithm based on observable characteristics in practice, and so the parameters are identified in the cross-section.

3.3 Estimation

We estimate our model of insurance cost and demand using a penalized maximum likelihood procedure with a Bayesian bootstrap in two steps. In the first step, we estimate our model of claim severity conditional on an accident as characterized in Equation (10) and our model of renewal prices as characterized in Equation (17). In the second step, we take the parameter estimates from the claim severity and renewal price models as inputs, and jointly estimate our model of claim occurrence, monitoring score and insurance demand. Our choice to estimate claim severity and renewal prices separately from demand reflects a key assumption: consumers have rational expectations about OOP and the evolution of prices. That is, rather than allow the distributions of severity and renewal prices to be influenced by consumers' revealed preferences (and beliefs) through their insurance plan choices, we treat these objects as objective facts and estimate them from the realized claims and realized renewal price panels, respectively, alone.

By contrast, our joint estimation of claim occurrence, monitoring score and insurance choice ensures that drivers' private information regarding their risk is consistent across the different parts of the model, such that (for instance) a driver's decision to enter monitoring is informative of their underlying risk type. The contributions of claim occurrences to the likelihood function are given directly by Poisson distribution as characterized in Equation (9) and the contributions of the monitoring scores are given by the Gaussian error distribution characterized in Equation (16). Because claim occurrences are rare, we complement our individual likelihood contributions with the likelihood of aggregate moments based on the distributions of the population averages of claim occurrence (major and minor) and choice (coverage, monitoring, and attrition) shares in our sample.[28] The likelihood contribution of each observed insurance choice d_{it} is given by the mixed logit choice probability of d_{it} induced by the Type 1 extreme value shock in the dynamic utility specification characterized in Equation (8).

Although in principle, it is possible to compute standard errors directly from the Maximum Likelihood estimator, the multi-step estimation procedure and high dimensionality of our parameter space make this approach computationally intractable. Instead, we compute standard errors through a joint Bayesian bootstrap procedure. For each of our estimation components (e.g. the claim severity estimator, the renewal price estimator and the joint claim risk, monitoring score and insurance choice estimator) we draw independent observation weights for each observation in our panel according to a Dirichlet distribution 100 times. We then perform the full estimation procedure for each of the 100 sets of draws and compute standard errors for each parameter based on the standard deviation among draw estimates.

4 Estimation Results

4.1 Model Fit

Our model estimates generate predictions that match our data well along multiple dimensions. To demonstrate this, Table 4 presents a comparison of key moments in our data and their predicted model analogues for claims occurrence, monitoring scores and pricing factors. Table 5 presents a comparison of moments for coverage choice shares, monitoring opt-in share for new customers, and attrition shares for renewal customers. This includes moments that capture selection patterns, i.e., how claim costs vary across alternatives, for both coverages and monitoring opt-in.[29] In each case, our model predictions are able to recover the distributions of outcomes observed in our data to a high level of granularity. Furthermore, the model replicates choice shares and selection patterns across major product-menu changes, including the introduction of the monitoring program and a change in the mandatory minimum coverage requirement in Illinois from $40,000 to $50,000.

[28] Augmenting our maximum likelihood objective with population moment likelihoods is similar to the regularization procedures described in Chernozhukov and Hong (2003) and Beaulac (2023) and is meant to prevent overfitting the likelihood of sparse claims observations on the basis of the dense insurance choice observations.

[29] We do not observe claims outcomes for attrited consumers.

Table 4: Fit of Claim Risk, Monitoring Score, and Renewal Pricing

Risk & Score			**Pricing**		
Moment	Data	Predicted	Moment	Data	Predicted
Poisson claim counts			*First renewal pricing factor*		
first moment	0.039	0.039	first moment	1.129	1.126
major claims	0.004	0.004	second moment	1.293	1.271
second moment	0.051	0.045	covariance with risk	0.044	0.041
N	199,368		*N*	55,272	
Monitoring score			*Latter renewal pricing factor*		
first moment	4.301	4.322	first moment	0.999	1.002
second moment	19.166	19.404	second moment	1.010	1.005
covariance with risk	0.154	0.142	covariance with risk	0.034	0.036
N	8,106		*N*	103,344	

Notes: This table reports the fit of model predictions to key data moments. Monitoring scores are only available for eligible new customers that have completed monitoring. For customers that have left the firm at a renewal, we do not observe claim realization for that period. For customers that have left the firm during a period, we do not observe the renewal pricing factor for the following period.

Table 5: Fit of Choice Shares (Coverage, Monitoring, and Attrition) and Selection

	New customers						Renewal customers			
	Pre-Mtr		Post-Mtr							
	Pre-MM Chg				Post-MM Chg		Pre-MM Chg		Post-MM Chg	
	Data	Pred	Data	Pred	Data	Pred	Data	Pred	Data	Pred
Coverage Share										
40,000	46.90	45.30	45.20	47.30			36.50	37.50		
50,000	14.20	19.60	12.90	19.30	51.80	51.30	10.70	11.50	45.10	45.00
100,000	17.30	15.60	19.90	15.70	25.60	23.70	16.50	16.30	20.80	21.00
150,000	18.30	12.30	18.00	11.40	19.70	16.30	18.20	16.60	18.90	19.00
300,000	3.30	7.10	3.90	6.30	3.00	8.70	3.30	2.80	3.50	3.40
Coverage Selection										
40,000	100.00	100.00	100.00	100.00			100.00	100.00		
50,000	30.40	34.60	14.20	26.00	100.00	100.00	28.10	24.30	100.00	100.00
100,000	29.00	30.20	21.90	23.70	32.40	31.00	37.40	34.20	32.30	45.80
150,000	34.30	24.10	33.00	17.50	27.70	19.70	45.00	41.10	41.10	46.60
300,000	6.30	12.90	4.40	9.70	4.10	8.90	7.10	6.70	6.80	7.80
Monitoring Opt-in										
Share			14.10	14.30	21.00	19.40				
Selection			9.70	15.10	21.80	23.00				
Attrition										
Share							14.90	15.53	12.73	12.53
N	8,623		21,040		24,864		51,550		113,400	

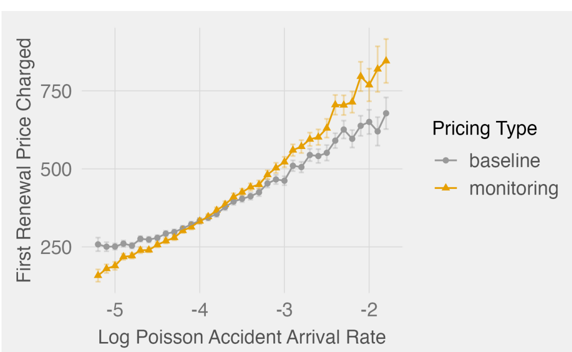
Notes: This table reports the fit of model predictions to key data moments. All quantities except the number of observations are reported in percentage point units. Coverage selection patterns are reported as the total claim cost of each coverage benchmarked against (divided by) that of the mandatory minimum plan. Monitoring selection patterns are reported as the total claim cost of opt-in customers divided by that of the customers who did not opt-in. We do not observe claim realization for renewal customers that left the firm. “Pre-Mtr” and “Post-Mtr” separate new customers to the firm before and after the introduction of monitoring. “Pre-MM Chg” and “Post-MM Chg” separate customers before and after the State raised mandatory minimum coverage from $40,000 to $50,000.

4.2 Risk Rating and Selection

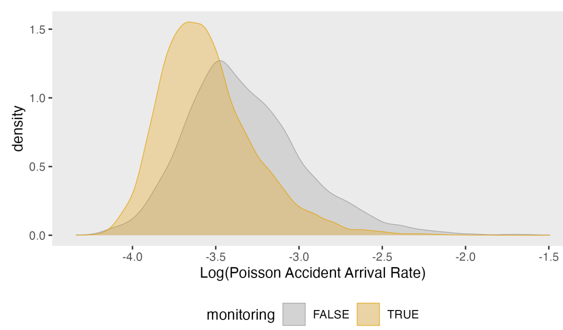
Our estimates allow us to directly quantify and visualize the effectiveness of the firm's risk rating with and without monitoring, as well as consumers' selection into the program.

Risk Rating The firm's risk rating capability is captured by our pricing and monitoring score models. The former captures how prices evolve dynamically (from the observed initial level) based on consumers' risk class and, if available, their monitoring scores. The latter directly connects the scores with consumers' claim risk.

Figure 6a shows a strong positive correlation between the first renewal price that a consumer faces and their log claim risk (λ). Monitored consumers are subject to a steeper pricing curve, in which consumers with lower risk types see lower prices and vice versa. The resulting advantageous selection can be seen in Figure 6b. Monitored drivers face a stochastically lower claim risk distribution (in the first-order sense) than unmonitored drivers. Our data and estimation cover several baseline pricing regimes and monitoring regimes. The figures above focus on the latest pricing and monitoring regime, which serves as the baseline for our counterfactual analyses. Figure B.4 compares risk rating across regimes.



(a)



(b)

Figure 6: Risk Rating and Advantageous Selection into Monitoring

Notes: (a) is a binned scatter plots first-renewal price (with and without monitoring) against consumers' estimated Poisson claim risk. We focus on the first-renewal price of the standard $50,000 coverage plan. (b) plots the density of the Poisson claim rate by monitoring opt-in.

Table 6: Model Parameter Summary

Parameter	mean	Q25	Q50	Q75	Q95	Correlation Log Baseline Risk	Correlation Log Private Risk
Claim Rate (1e-2)	3.94	1.34	2.38	4.52	11.30	0.66	0.45
	(0.12)	(0.11)	(0.14)	(0.16)	(0.43)	(0.03)	(0.02)
Log Claim Rate	-3.69	-4.31	-3.74	-3.10	-2.18	1.00	0.61
	(0.06)	(0.10)	(0.07)	(0.04)	(0.04)	(0.00)	(0.03)
- Private Component	0.08	-0.19	0.10	0.41	0.98	0.55	1.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.00)
Expected Cost to Insurer ($)	205.23	69.71	125.02	236.87	595.45	0.67	0.45
	(13.01)	(6.92)	(10.29)	(14.31)	(56.53)	(0.03)	(0.03)
Risk Aversion (1e-05)	1.43	0.86	1.28	1.83	3.02	-0.12	-0.10
	(2.24)	(1.12)	(1.87)	(2.92)	(5.24)	(0.07)	(0.03)
Default Plan FE ($)	36.11	-12.24	27.38	85.86	136.12	0.54	0.02
	(7.42)	(8.21)	(8.67)	(11.24)	(14.67)	(0.06)	(0.02)
Plan Switching Cost ($)	542.77	527.57	541.65	556.58	582.23	-0.24	-0.07
	(53.77)	(55.00)	(53.97)	(51.16)	(48.01)	(0.12)	(0.04)
Firm Switching Cost ($)	332.83	296.98	325.79	361.39	430.40	0.52	0.10
	(16.76)	(21.77)	(19.36)	(18.76)	(27.50)	(0.07)	(0.03)
Monitoring Disutility ($)	112.56	76.06	106.69	142.76	208.76	-0.08	-0.22
	(8.27)	(9.89)	(7.78)	(10.87)	(20.72)	(0.12)	(0.03)

Notes: This table reports the distributions of key parameters from our model. Columns are moments/correlations across individuals. Risk and choice frictions (default plan FE, switching costs, and monitoring disutility) are reported on a per period basis. Parentheses show bootstrap standard errors.

4.3 Parameter Estimates

Table 6 presents the empirical distributions of several key economic parameters: claim risk, risk aversion, monitoring disutility and the three remaining utility frictions. Additional results are in the appendix: Table A.3 includes additional severity and pricing latent parameters. Tables A.4, A.5, and A.6 present all hyper-parameters outlined in Section 3.1.

Accidents are rare: the average consumer in our panel faces 0.039 accidents per six-month period in expectation. The distribution is right-skewed: the riskiest 5% of drivers expect 0.113 accidents per period—about three times more than the average. Among accidents, major ones—defined as those with Pareto losses greater than $10,000—account for 10.88%. Combining the accident occurrence models for minor and major accidents with their respective severity models (Equations 10), the expected cost of fully insuring a consumer is $205.23, of which $113.62 is due to major accidents. If a consumer chooses two of the most popular plans, \$40,000 or \$50,000 coverage limits, their expected out-of-pocket expenditures would be \$14.26 and \$10.66, respectively.

Consumers exhibit mild risk aversion, with an average absolute risk-aversion parameter of 1.43×10^{-5}. At this level, an individual with average income would require a certain payment of \$7.17 to be indifferent to a 50-50 lottery of winning or losing \$1,000. Across the distribution, this certainty equivalent is \$2.09 at the 5th percentile and \$15.09 at the 95th percentile. Our estimate is lower than prior studies: for example, Cohen and Einav (2007) report an average of 2.6×10^{-5}, corresponding to \$13 in the lottery interpretation. However, this difference is expected, as our focus is on mandatory liability insurance, whereas optional coverage for one's own car tends to be purchased by more risk-averse consumers. Moreover, prior literature has found that consumers tend to exhibit very risk averse behaviors for small risks and the opposite for large ones (Sydnor 2010). For example, Chetty (2006) estimates that, based on labor supply choices, the constant relative risk aversion parameters are around 1. At our income level, this corresponds to about \$12.12 in the lottery interpretation.

Consistent with prior literature such as Handel (2013),[30] choice frictions are economically significant. On average, consumers forgo \$333 in potential gains per period by not switching firms, relative to an average premium of \$578. Switching plans across periods is even less common than switching firms, corresponding to an implied plan-switching cost of \$543. Similarly, the magnitude of the monitoring disutility is large: on average, consumers will only opt in if they anticipate an expected gain of at least \$113 per period.

Although our model does not impose any explicit correlations among utility parameters, we detect meaningful ex-post correlations driven by the correlated choice patterns in the data. Log accident risk is strongly positively correlated with firm-switching cost ($\rho = 0.52$) and with preference for the default minimum coverage plan ($\rho = 0.54$). Accident risk is weakly negatively correlated with risk aversion ($\rho = -0.12$) and monitoring disutility ($\rho = -0.08$). At the individual level, the correlation between the private log risk component (known ex-ante to the consumer but not to the firm) with switching cost, preference for minimum coverage, risk aversion, and monitoring disutility are $\rho = 0.10, 0.02, -0.10, -0.22$. Most of the correlation between risk and firm switching cost or preference for the default plan is accounted for by observable characteristics, and so their effect on selection of private risk is limited. However, privately safer drivers tend to suffer higher monitoring disutility, which may dampen advantageous selection into monitoring.

[30] The average switching cost is estimated to be \$2,032 per year.

Robustness and Discussion The most binding assumption in our model is that consumers have a three-period planning horizon. In Appendix Tables A.8 and A.7, we re-estimate the model under two- and four-period horizons. Compared to the estimates in our baseline model (Table 6), the main difference is that the planning horizon influences the magnitude of choice frictions needed to rationalize the observed choice patterns, beyond what can be explained by the direct certainty-equivalent value of insurance.

For instance, we estimate an average firm switching cost of \$333 under our three-period benchmark, down from \$568 with a two-period horizon and slightly above the \$329 with a four-period horizon. The planning horizon and switching costs move in opposite directions because both affect how much today's choice "sticks," and affects future-period utility. In a three-period model, lower switching costs make the period-0 decision easier to undo at renewals, so its consequences persist less, mimicking a shorter effective horizon. When we explicitly shorten the model to two periods, we need higher switching costs to keep current choices binding and amplify their future consequences, mimicking a longer horizon.

Conversely, the one-time monitoring disutility estimate increases with the modeled planning horizon: \$113 in the baseline, versus \$90 and \$208 in the two- and four-period models, respectively. As with switching costs, these differences in estimates make sense as the frictions are fundamentally trying to explain the same choice patterns but with different levels of insurance value to rationalize around. Monitoring disutility is only relevant in the first period and consumers must opt-in voluntarily. A longer planning horizon typically thus implies more value from a discount and the monitoring disutility needed to rationalize not opting in is higher with a longer horizon.

These results illustrate that it may be difficult to separately identify consumers' planning horizon from the magnitude of their choice frictions. We thus do not pursue a model in which the time horizon is determined endogenously. Instead, we show that, despite differences in the magnitude of choice frictions, our main counterfactual results remain robust to alternative planning-horizon assumptions in Section 5.4.

5 The Value of the Monitoring Program and Counterfactual Data Regulation

In this section, we develop a supply-side model which, together with our demand estimates, facilitates counterfactual simulations to evaluate the welfare and profit impact of the monitoring program. We begin by comparing the status quo regime to a scenario in which monitoring is not available. This comparison establishes a baseline quantification for the value that the monitoring program provides. We then examine how this value depends on the firm's pricing strategy, competitive pricing responses, and potential data portability regulations.

5.1 A Model of Firm Pricing with the Monitoring Program

As we discussed in Sections 1.1 and 3, special programs like monitoring are typically priced as multiplicative factors to a baseline price menu $p_{t=0}(y|x)$ across coverage options y and observable classes x. In keeping with this, we model firms' pricing strategies through a vector of multiplicative adjustment factors that scale the baseline price menus observed in our data. We term these adjustment factors as pricing *levers* and denote them by $\boldsymbol{\kappa}$ to distinguish them from other (fixed) pricing factors. This approach allows us to abstract away from regulatory and actuarial details that are largely unrelated to monitoring, such as the risk-rating associated with driver age or vehicle types. Accordingly, we denote the observed baseline price menu for a representative individual by p, suppressing the notation for x and y. Similarly, we denote draws of renewal factors $r_t(m|s,x,\nu)$ as defined in Equation (17) by $r(m|s)$. The observed baseline renewal price menu is thus $p \cdot r(m|s)$. Because monitoring only affects renewal factors in the first period, we omit the period index t.

The goal of our exercise is to study how the monitoring firm can affect equilibrium outcomes by differentiating prices across the monitored and unmonitored pools, and among monitored consumers after the monitoring period concludes. To this effect, we consider pricing strategies parameterized by a vector of four levers $\boldsymbol{\kappa} = \{\kappa_{0s}, \kappa_{0d}, \kappa_{1s}, \kappa_{1d}\}$. The first lever κ_{0s} multiplies all prices, setting a new baseline for all drivers. The second lever κ_{0d} multiplies the initial price offered to monitored drivers, generating an opt-in discount. The remaining levers κ_{1s} and κ_{1d} modify the renewal price that monitored drivers are offered depending on the realization of their monitoring scores. Together, the pricing levers $\boldsymbol{\kappa}$ generate the following progression of prices for prospective consumers.

Initial pricing for unmonitored drivers ($t = 0$) In their first period with the firm, a consumer who opts out of monitoring receives the modified baseline price. That is, if the consumer were offered a price p under the status quo, then their initial period price would become $\kappa_{0s} \cdot p$ under the pricing strategy $\boldsymbol{\kappa}$.

Renewal pricing for unmonitored drivers ($t \geq 1$) At renewal, unmonitored consumers are offered the baseline progression of renewal prices.[31] That is, at their first renewal, a consumer who did not enter monitoring would draw a renewal factor $r(0|0)$ according to the distribution described in Equation (17). Absent a claim, their price for the following period would be $\kappa_{0s} \cdot p \cdot r(0|0)$. If a claim were to occur, then their renewal price would further increase by the claim factor r_c defined in Equation (7).

Initial pricing for monitored drivers ($t = 0$) A consumer who opts in to monitoring receives a *one-time* discount in the first period. That is, if the consumer were offered a price p under the status quo, then their price for the monitoring period would become $\kappa_{0s} \cdot \kappa_{0d} \cdot p$ under the pricing strategy $\boldsymbol{\kappa}$.

Renewal pricing for monitored drivers ($t \geq 1$) The monitoring score generated in the initial period provides the firm with a posterior update of each monitored driver's private risk type. To leverage this information, the firm applies a different lever for drivers whose scores outperform or underperform the firm's expectations. For drivers who outperform expectations (e.g., they received a monitored renewal factor lower than the unmonitored one $r(1|s) \leq r(0|0)$ in the data), the firm applies lever κ_{1d}, whereas for drivers who underperform, the firm applies κ_{1s}. We model the renewal factor in each case as a mixture between $r(1|s)$ and $r(0|0)$ as follows:

$$r^*(m = 1|s) = \begin{cases} \kappa_{1d} \cdot r(1|s) + (1 - \kappa_{1d}) \cdot r(0|0) & \text{if } r(1|s) \leq r(0|0) \\ \kappa_{1s} \cdot r(1|s) + (1 - \kappa_{1s}) \cdot r(0|0) & \text{if } r(1|s) > r(0|0). \end{cases} \tag{20}$$

Here, κ_{1d} can be interpreted as a *rent-sharing* parameter for monitored consumers who prove to be safer drivers. For instance, if a consumer were to receive a 30% discount at renewal after monitoring in our data, $\kappa_{1d} = 0$ would reduce their discount to 0 so that their renewal price

[31] At renewal, the firm has no incentive to modify its baseline pricing for unmonitored customers: it gains no additional information about these drivers, nor does it have reasons to nudge them toward a particular plan choice as it did with monitoring in the first period.

would be equal to that of a driver who did not enter monitoring. Because this consumer is known to be less costly to insure than the average unmonitored driver, the increase in renewal price would be accrued directly to firm profits. If $\kappa_{1d} = 1.5$, their discount would instead increase by a half to 45%, so that a larger share of the cost savings revealed by monitoring were given back to the consumer. Similarly, κ_{1s} controls the degree to which monitored consumers who prove to be costlier to insure may see higher prices—a *risk surcharge* parameter.

Note that because the opt-in discount κ_{0d} is only applied in the first period, the baseline price at renewal is the same for monitored and unmonitored drivers. As such, a consumer who completes their monitoring period with score s and no accidents would expect a renewal price of $\kappa_{0s} \cdot p \cdot r^*(1|s)$ in the subsequent period.

Firm Profits and Variable Costs To choose $\boldsymbol{\kappa}$ optimally, the firm maximizes its aggregate expected profits across all consumers in the market. For each consumer, it internalizes their risk and preference types, integrating over possible realizations of claims, renewal price shocks, and scores to anticipate their claims costs, initial-period plan choice, renewal price changes and possible attrition or resorting into different coverage levels. We assume that the firm does not experience time-discounting, but that its profit planning horizon matches consumers' welfare horizon of three periods in our baseline model.

The marginal profit from a consumer in a given period is given by the expected premium under each plan, less the expected cost of insuring them given the plan's coverage, multiplied by the probability of that consumer choosing the plan. We assume that the cost of insuring a consumer in a given period is determined by the sum of their expected claims in that period (subject to the deductible and limits of their chosen plan) and an additive *loading cost* as defined in Equation (1), reflecting overhead as well as expenses to administer the plan and potential claims. For monitored drivers and during the initial period only, the firm benefits from reduced risk but incurs a *variable cost of monitoring*. Based on our interviews with the firm's management team, this variable cost is about $35.

Discussion The four pricing levers in our model allow the firm to exercise an "invest-and-harvest" strategy that generates ex-ante ambiguous welfare effects for different types of drivers. The renewal levers κ_{1s} and κ_{1d} determine how the firm prices the monitoring data. A lower rent-sharing lever κ_{1d} or higher surcharge lever κ_{1s} transfers less of the informational surplus generated by the monitoring technology to consumers, representing a higher level of extraction from monitored drivers—"harvesting" the data—but it also discourages drivers from opting in to monitoring in the first place. These considerations may affect drivers with higher or lower risk asymmetrically. The profit-maximizing degree of advantageous selection is therefore an empirical question: it depends, among other factors, on how the additional risk information revealed by monitoring correlates with risk aversion, price sensitivity, and monitoring disutility.

Conditional on the renewal levers, κ_{0s} and κ_{0d} function as "investment" levers to encourage initial enrollment in monitoring. A higher κ_{0s} and a lower κ_{0d} encourage more drivers to opt in, generating more data and potential for rent extraction in renewal periods. The trade-off is lower inframarginal profit in the initial period and the risk of losing monitoring-averse consumers to competitors. As such, the optimal levels of $\boldsymbol{\kappa}$ depend on the competitive landscape as well.

5.2 Equilibrium Game

The monitoring firm's four pricing levers interact strategically with its competitors' pricing decisions. To capture these equilibrium dynamics, we consider a Bertrand pricing game in which prices are set simultaneously by our firm and a *focal* competitor.

Recall from Section 3 that our competitive landscape consists of the three largest firms in the market—the monitoring firm and two competitors whose prices enter our demand estimation in renewal choices. Among these competitors, one offers the lowest average price and the other offers the median among all competitors in our dataset. For our main results, we assume that the focal competitor is the low-price firm. The median-price competitor is treated as passive, giving consumers a realistic outside option whose price remains unaffected by monitoring. To match observed market shares, this passive firm is modeled as a composite residual firm that absorbs all market shares unserved by the two active players. Section 5.4 examines robustness to alternative market and competitor definitions.

Competitor Pricing As discussed in Section 1.2, we assume that competitors do not offer monitoring on their own. However, the focal competitor can adjust its pricing in response to the monitoring program in two ways. First, the competitor can adjust its baseline pricing via a baseline lever κ_0^c akin to κ_{0s}. Second, if monitoring data becomes fully portable so that the competitor is able to observe and interpret a driver's monitoring score in the same way that the monitoring firm can, then the competitor can also counter the monitoring firm's renewal offer to monitored drivers using a competing set of levers κ^c_{1s} and κ^c_{1d}, parameterized similarly to their counterparts:

$$r^{c*}(m=1|s) = \begin{cases} \kappa^c_{1d} \cdot r(1|s) + (1-\kappa^c_{1d}) \cdot r(0|0) & \text{if } r(1|s) \leq r(0|0) \\ \kappa^c_{1s} \cdot r(1|s) + (1-\kappa^c_{1s}) \cdot r(0|0) & \text{if } r(1|s) > r(0|0). \end{cases}$$

Under this model, a driver considering the focal competitor without monitoring would face a competitor price of $p^c \cdot \kappa^c_{0s}$. A monitored driver considering renewal with score s would face a competitor price of $p^c \cdot \kappa^c_{0s} \cdot r^{c*}(1|s)$.

Counterfactual Scenarios and Regulations We consider five counterfactual scenarios. "Status Quo" reflects the most recent baseline and monitoring pricing regimes for all firms, which covers the majority of our data after the introduction of monitoring. The "No Monitoring" scenario removes the monitoring program altogether. As we discuss in Appendix B, the introduction of monitoring did not alter baseline prices. We thus assume that both the monitoring firm and the focal competitor would have continued to set their baseline levers κ_{0s} and κ^c_{0s} to be 1 in this counterfactual.[32] Comparing these two scenarios allows us to evaluate the impact of the monitoring program.

Next, we consider counterfactual scenarios in which the monitoring firm sets profit-maximizing levers $\boldsymbol{\kappa}$. First, a "Partial" equilibrium scenario holds fixed the focal competitor's pricing. We then allow the latter to respond in the "Full" equilibrium scenario. However, as monitoring data is proprietary to the firm, the focal competitor can only set κ^c_{0s}.

A recurring policy proposal in the context of monitoring is to mandate data portability.[33] Just as accident records must be observable to all insurance firms, a data portability law would require firms to share any monitoring data that they collect with the whole industry (at least upon a drivers' request). The reasoning behind this proposal is that data portability would increase competition and mitigate asymmetric information in the insurance market: if competitors are able to discern less costly drivers as well as the monitoring firm can,

[32] In practice, the pricing of the monitoring program, including its interaction with baseline prices for unmonitored drivers, was coarsened and simplified relative to a profit-maximizing price schedule in order to appeal to consumers and regulators (Ben-Shahar 2023). In Appendix B, we show that the discount schedule for monitored drivers was coarse and lumpy (Figure B.2), while the baseline prices for unmonitored drivers were also totally unaffected by the introduction of monitoring (Figure B.1b).

[33] For example, the EU-U.S. Insurance Dialogue Project's 2018 Big Data issue paper (with NAIC participation) identified data portability as an emerging consumer issue for U.S. state insurance supervisors. The Federal Trade Commission's "Data To Go: An FTC Workshop on Data Portability." (2020) further examined data portability's potential benefits and risks for competition and switching.

then they can undercut the monitoring firm's renewal prices, limiting its ability to extract rents. However, if competition at renewal eliminates the firm's ability to profit from the information generated by the monitoring program, it may not be incentivized to invest in the program in the first place. Moreover, while competitors would benefit from additional data on driver risk, the pricing of such information can further heighten reclassification risk associated with monitoring, thereby reducing opt-in. Nonetheless, monitoring can remain attractive under a data portability mandate for two reasons. First, the moral hazard effect and reduced risk during the monitoring period is unaffected by the mandate. Second, large switching frictions give the monitoring firm a competitive advantage in retaining safe drivers at renewal, even if its information advantage is removed.

To assess this policy, we consider a "Data Port." counterfactual. This scenario equalizes firms' information sets at renewal and allows them to compete using the full set of post-monitoring price levers. In order to simulate realistic regulatory constraints, we restrict the domain of feasible price levers for the firms in two ways: the ex-post risk surcharge cannot exceed what we observe in the data ($\kappa_{1s}, \kappa_{1s}^{c} \leq 100\%$), and the rent sharing factor must be non-negative ($\kappa_{1d}, \kappa_{1d}^{c} \geq 0$). These constraints reflect the regulatory principle that monitoring programs should primarily reward safe drivers through premium reductions rather than penalize risky drivers (Ben-Shahar 2023).[34] Taking the constraints imposed by regulation one step further, we consider a final "Disc. Floor" scenario that also restricts rent sharing be no less than the level observed in the data ($\kappa_{1d}, \kappa_{1d}^{c} \geq 100\%$), essentially enforcing a discount floor.

Loading Cost Calibration We do not observe the loading costs for either the monitoring firm or its competitors. As such, we apply the following calibration procedure to approximate them. Modeling each firm's loading cost as a uniform per-consumer additive scalar, we maintain the standard assumption that the baseline prices observed for each firm maximize the firm's expected profits given its loading cost. Since the pricing of monitoring had no bearing on baseline prices in reality, we calibrate loading costs using unmonitored drivers only. Specifically, we perform a one-dimensional grid search for each firm, looking for a value that rationalizes each of the observed baseline prices ($\kappa_{0s} = \kappa_{0s}^{c} = \kappa_{0s}^{c,\text{passive}} = 1$) as mutual best responses in the "No Monitoring" scenario.

[34]For example, New York's regulatory framework explicitly requires usage-based insurance programs to be "discount only" so as not to "negatively impact policyholders."

Calibrating Firm Brand Effects Our dataset covers the customers of the monitoring firm only. To conduct market-level counterfactual simulations, we assume that these consumers are representative of the full market and compute outcomes on a per-consumer, per-year basis. Specifically, we assume that (1) the distribution of risk and preference types in our sample matches that of the full market; and (2) in the first period, consumers choose one the three firms in addition to coverage levels and monitoring participation according to our model in Equation (8), with the modification that their flow utilities also include firm-level ("brand") fixed effects. To calibrate these brand effects, we normalize the monitoring firm's brand effect to zero, and conduct a two-dimensional grid-search for the brand effects of the two competitors that best match the status quo market shares. Since the passive competitor is a composite residual firm, its market share and the brand effect will naturally be large.

Grid Search and Multiple Equilibria Due to complex interactions between consumer costs, demand and the multi-plan choice menu offered by each firm, the payoff space for each firm may not be concave with respect to its pricing parameters. As such, it is possible for multiple equilibria (or no pure strategy equilibria) to exist. To identify equilibrium outcomes for each of our counterfactual regimes, we perform an exhaustive grid search over plausible price parameter tuples. Whenever there are multiple equilibria, we select the one that maximizes the monitoring firm's profit.

5.3 Counterfactual Simulation Results

For each of the five counterfactual scenarios in Table 7, we present results in three categories: (1) surplus and surplus division into consumer welfare, focal firm and competitor profits; (2) quantity measures such as insurance coverage, market share, and monitoring adoption; and (3) pricing levers, given by the $\boldsymbol{\kappa}$ and $\boldsymbol{\kappa}^{\boldsymbol{c}}$, subject to regulatory caps.

All welfare and profit measures are reported in annualized, per-capita dollars, computed by scaling the per-period total quantities to match the yearly time frame and then averaging across all consumers in the market (*not* weighted by the likelihood of coming to the monitoring firm). Because consumer welfare is inherently nonlinear, we report certainty-equivalent changes relative to the "no monitoring" scenario. "Total surplus" equals the sum of the changes in consumer welfare and industry profits.

As part of the quantity measures, we report the expected amount of insurance coverage (in thousands of dollars) associated with consumers' plan choices, regardless of firm. We also report the market share of the monitoring program overall and separately among new customers ("Initial-Period") and existing customers ("Renewal"), all in percentage terms.

The pricing levers for the monitoring firm ($\boldsymbol{\kappa}$) and the competitor ($\boldsymbol{\kappa}^c$) are reported directly. For instance, a baseline lever κ_{0s} of 1.09 corresponds to a 9 percentage point surcharge over the observed lever of 1.00. Similarly, an initial-period monitoring lever κ_{0d} of 0.23 corresponds to a 77% discount relative to baseline pricing, and a 73 percentage point discount over the observed lever of 4%.

Table 7: Counterfactual Simulation Results

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria: Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ$ p.c.y.)	-	+7.43	+5.97	+8.72	+7.36	+6.79
Firm Profit ($ p.c.y.)	30.04	35.57	38.43	38.00	36.82	37.08
Competitor Profit ($ p.c.y.)	71.66	68.48	67.64	67.30	67.84	67.55
Industry Profit ($ p.c.y.)	101.70	104.05	106.06	105.3	104.66	104.64
Total Surplus (Δ$ p.c.y.)	-	+9.79	+10.34	+12.32	+10.33	+9.73
Quantity						
Coverage ($000 p.c.y.)	108.53	108.62	108.66	108.88	108.70	108.67
Initial-Period Firm Market Share (%)	10.24	11.85	12.84	12.85	12.39	12.36
Renewal Firm Choice Prob (%)	14.52	16.25	16.76	16.8	16.37	16.36
Monitoring Market Share (%)	0.00	2.59	6.91	6.78	6.09	6.36
Pricing Levers						
Baseline Factor (κ_{0s})	1.00	1.00	1.09	1.08	1.08	1.09
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.23	0.24	0.23	0.30
Risk Surcharge Factor (κ_{1s})	-	1.00	0.58	0.59	0.57	0.50
Rent Sharing Factor (κ_{1d})	-	1.00	0.05	0.00	0.00	1.00
Competitor Pricing Levers						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.99	0.99	0.99
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: This table reports counterfactual simulation results, with the "Current Regime" corresponding to model fit on data after the introduction of monitoring. The market is modeled as consisting of three players: the monitoring firm, a focal competitor, and a composite passive competitor representing the residual market. The focal competitor is defined as the firm offering the lowest average premium for the most popular coverage plan, while the passive competitor corresponds to the median-priced firm. Brand effects are calibrated to $0, $118, and $640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to $84, $25, and $209, respectively.

[1] Units are given in brackets. "Δ" denotes changes relative to the "No Monitoring" benchmark; "p.c.y." stands for per capita per year; "$" indicates dollar terms; and "%" indicates percentage-point terms. Pricing parameters have a step size of 1 percentage point, or 0.01.

[2] Scenario definitions: In "No Monitoring," all customers are mechanically made ineligible for monitoring without changing baseline prices. "Partial Equilibrium" computes the profit-maximizing equilibrium when the focal competitor's pricing is held fixed. "Full Equilibrium" computes the Nash equilibrium in which the focal competitor can only adjust its baseline surcharge. "+ Data Port." computes a Nash equilibrium where both firms observe monitoring outcomes and can commit to future prices subject to ex-post risk surcharge $\leq$ 100% and rent sharing $\geq$ 0. "+ Disc. Floor" further restricts that rent-sharing $\geq$ 100%.

Assessing the Monitoring Program The first two columns of Table 7 illustrate the welfare impact generated by the monitoring program, as it is implemented in our data. Although the monitoring firm only captures an 11.85% market share, the average consumer in the entire insurance market gains \$7.43 per year in certainty equivalent from the availability of monitoring. The monitoring firm gains \$5.53 while the competitor firms lose \$3.18 per-capita per-year. Summing across all parties, the combined total surplus improves by \$9.79 per-capita per-year due to the introduction of the monitoring program.

Optimal Pricing of the Monitoring Program In the "Partial" equilibrium regime, profit-maximizing pricing amplifies the "invest-and-harvest" dynamic for the monitoring firm. The firm raises baseline prices by 9% while offering a 77% one-time discount to encourage opt-in, regardless of the ex-post monitoring outcome. However, the firm shares little rent with safer drivers (only 0.05) compared to the 1.00 benchmark observed in the data, exploiting its information advantage and consumers' high switching costs. This is notable because baseline prices are already higher than observed in the data. For riskier consumers, however, the surcharge also falls from 1.00 in the data to 0.58, indicating that the firm still expects positive profits from most of them despite higher risk.

On net, firm profits increase by 8.0% relative to the status quo, eroding some of the consumer welfare gains from monitoring. Nonetheless, total surplus rises as monitoring adoption more than doubles to 6.91% of the overall market, and the average consumer chooses a higher coverage level.

In the "Full" equilibrium scenario, the focal competitor responds to the monitoring firm's optimal pricing by lowering baseline prices by just 1%. The monitoring firm reacts by scaling back ex-ante "investment": lowering both the baseline surcharge and the upfront opt-in discount. However, ex-post rent extraction is exacerbated: safe drivers receive 0% rent sharing, while risky drivers face a percentage point higher surcharge.

Data Portability When consumer data portability is enforced, the competitor can in principle selectively "poach" consumers after monitoring. However, our results show that the only equilibrium is a corner solution in which the competitor poaches as little as possible: no discount for safe drivers and maximum surcharge for risky drivers. This weakly increases the competitor's pricing for monitored drivers, which depresses monitoring opt-in ex-ante by raising reclassification risk in the event of a bad monitoring score. The monitoring firm thus responds by slightly increasing its upfront discount and lowering its ex-post surcharge.

Why is poaching unsustainable in equilibrium? The payoff matrix reveals that, as soon as the competitor sets an interior value for κ^c_{1s} and κ^c_{1d} to attract monitored drivers, the monitoring firm can always profitably counter by lowering its baseline surcharge (κ_{0s}) or raising its upfront discount (κ_{0d}). Both strategies steal market share from the competitor while having opposite effects on monitoring opt-in and inframarginal profits. This allows the firm to fine-tune its response to, and effectively counter, any poaching.

In equilibrium, data portability affects consumer welfare primarily by increasing reclassification risk, rather than by intensifying competition. But this does not mean that poaching has no effect. Our final counterfactual scenario adds a mandated discount floor requiring that rent-sharing with safe drivers by either firm be no less than the observed level ($\kappa_{1d}, \kappa^c_{1d} \geq 1.00$x). This effectively forces the competitor to poach safe drivers with a high $\kappa^c_{1d} > 1$. The results show that the monitoring firm counters lower ex-post rent extraction by lowering ex-ante "investments," raising its initial-period surcharge and lowering discounts, ultimately reducing welfare and profits.

Taken together, our results show that ex-post price controls—capping the surcharge for risky drivers, setting safe driver discount floors, or data portability mandates—can harm firms' dynamic incentives to invest in monitoring while raising reclassification risk. In our case, the welfare benefits of monitoring through accident reduction and information generation are so large that these harms overwhelm the benefits of increased competition. As such, these regulations wind up harming consumers.

5.4 Sensitivity and Discussion

In this section, we test the robustness of our results to several key modeling assumptions. We focus on alternatives to our specification of (1) the firm's variable monitoring cost, (2) how forward-looking consumers and firms are, (3) regulatory constraints on monitoring pricing, and (4) pre-existing market structure and the focal competing firm.

Overall, our findings of substantial welfare benefits from the monitoring program and of harms from a prospective data portability regulation remain qualitatively identical and quantitatively robust to our main specification. Most quantities remain highly stable across specifications, while industry profits and firms' pricing levers vary more noticeably in magnitude, albeit only in one alternative specification each.

Marginal Cost of Monitoring Tables A.9 and A.10 present scenarios in which the marginal cost of monitoring is 50% higher or lower than our baseline of $35. Our results remain qualitatively similar to the baseline.

Time Horizons In Tables A.11 and A.12, we show that our results are qualitatively identical when we adopt the two-period and four-period look-ahead models. Quantitatively, both scenarios see lower consumer welfare gains compared to the baseline, but for different reasons. A shorter horizon reduces the future benefits of monitoring, diminishing the firm's investment and monitoring opt-in rates. A longer horizon does the opposite, but the first-period-only claim reduction effect is also spread out to more periods, ultimately reducing the average per-period gain. In addition, although the data sharing equilibrium still features the corner solution of minimal poaching from the competitor, the monitoring firm now counters primarily by raising its baseline surcharge while scaling back upfront discounts. This triggers a higher baseline surcharge by the competitor, further eroding consumer welfare.

Ex-post Price Regulations Our baseline simulations assume that the rent-sharing factors $(\kappa_{1d}, \kappa^c_{1d})$ are non-negative, and that the risk-surcharge factors $(\kappa_{1d}, \kappa^c_{1s})$ are no higher than the level observed in the data. Table A.13 shows that, when we relax these constraints to +/-200%, our results remain very stable. In particular, the data portability regime sees another corner solution in which the focal competitor does not poach by charging the maximum possible price for both safe and risky drivers after monitoring. This reinforces the idea that the monitoring firm's pricing levers are flexible enough to effectively counter any ex-post competitor poaching in equilibrium, preserving the firm's ability to offer and profit from monitoring.[35]

Pre-existing Market Structure The monitoring firm only has 12% market share in our data. Our baseline results also pit it against the lowest-pricing competitor. What if its baseline market position were enhanced? Or if our model allowed it to directly compete against other firms?

[35] In our testing, this result holds even if the competitor does not commit to future prices, and if we further widen the constraints.

Table A.14 presents results in which the market consists of the monitoring firm, and the minimum- and median-pricing competitors only. By replacing the composite residual competitor with the medium-pricing competitor, the monitoring firm's market share increases to 27%. While the results remain qualitatively similar to the baseline, both the benefit of monitoring and the firm's pricing of it are more pronounced.

Table A.15 simulates a scenario in which we switch the focal competitor with the passive composite competitor that has 66% market share. Facing a more dominant firm, the monitoring firm scales back its upfront discount but increases ex-post rent-sharing to retain safe drivers. This reduces the monitoring opt-in rate relative to the baseline in our main specification. The surplus impact of monitoring thus decreases. The welfare impact, however, increases whenever the competitor is allowed to lower its prices in equilibria and decreases otherwise.

6 Related Literature and Conclusion

Across many markets, firms buy data directly from consumers—with attractive rewards for data-sharing and readily available sensor technologies—and keep what they collect proprietary. The data is then used to mitigate information asymmetries, strengthen competitive advantage, and extract rents from consumers. In our paper, we obtain novel datasets on a voluntary monitoring program in the competitive U.S. auto insurance industry, which give us direct visibility into how proprietary monitoring data is collected and used beyond prior empirical analyses that have relied on simulations (Bordhoff and Noel 2008) and state-firm level aggregates (Hubbard 2000; Reimers and Shiller 2018; Porrini, Fusco, and Magazzino 2020). We also develop an empirical framework that embeds the firm's data elicitation strategy—and consumers' data-sharing choices—inside the broader product (insurance) market pricing and choice problems. This allows us to account for the value of monitoring data to the firm based on its subsequent use in price discrimination, risk-rating, competitive cream-skimming, and risk reduction.

Our paper first separately identifies moral hazard from selection. This extends the literature that has relied on correlations between plan choice and claims, such as Chiappori and Salanie (2000) and Cohen and Einav (2007). Abbring, Chiappori, and Zavadil (2008) and Jeziorski, Krasnokutskaya, and Ceccarini (2019) examine moral hazard through dynamic incentive variation in experience-rated pricing, where surcharges for further accidents rise with each accident. Moral hazard thus implies declining claim rates after an accident. However, recent evidence suggests that drivers become inherently more cautious following “near misses,” even when no incentive changes occur(Shum and Xin 2022).

We also build on a structural literature on measuring the extent and welfare implications of information asymmetries in insurance markets (Einav, Finkelstein, and Cullen 2010). Like Cohen and Einav (2007), Barseghyan, Molinari, O’Donoghue, and Teitelbaum (2013), and Jeziorski, Krasnokutskaya, and Ceccarini (2019), we use consumer choices and subsequent claims data from an auto insurer to identify consumers’ accident risk and risk preferences. Our estimates highlight a similar information asymmetry between the insurer and its consumers, which drives advantageous selection into monitoring. Beyond modeling selection, the monitoring program allows both the firm’s information set and that of its competitors (in our counterfactuals) to be determined endogenously in equilibrium with insurance prices and quantities. To do this, we expand the canonical framework in two main ways.

First, the revelation of risk information takes time. When consumers decide whether or not to opt in to monitoring, they anticipate the effect that the monitoring score they receive will have on their renewal price. Our choice model reflects the dynamic nature of monitoring decisions, as well as uncertainty about future premiums that consumers face given their risk types and risk preferences. This allows us to predict how changes in opt-in discounts and renewal pricing alter selection into monitoring (i.e. how many and what kinds of consumers choose to opt in), and consequently, the amount of information that is revealed.

Second, monitoring data is proprietary, and firm pricing must account not only for selection into the program but also for selection into the firm. Accounting for competitor pricing is thus crucial to understanding consumers’ outside options and the firm’s pricing incentives. Compared to existing studies, we introduce new individual-level competitor pricing data based on price filings; we also expand the canonical model to admit consumer choice along both the intensive and extensive margins. Similar work include Honka (2012) that uses survey data and Cosconati, Xin, Wu, and Jin (forthcoming), which uses administrative data.

We also contribute to the empirical literature on the role of data in industrial organization. First, monitoring mirrors more traditional modes of hard information disclosure such as restaurant health score cards (Jin and Leslie 2003) and used-car photos (Lewis 2011). Our study differs conceptually in that consumers have control over information disclosure, although their disclosure decisions are influenced by firm pricing. Nonetheless, our findings highlight similar sources of efficiency benefits from richer information as well as the signaling incentives and frictions related to its disclosure. Second, our final counterfactual considers a regulation that would require public disclosure of monitoring data, which connects to recent policy debates regarding data portability and algorithmic transparency (Jin and Wagman 2021). We find that the loss of proprietary control over monitoring data not only heightens the reclassification risk associated with monitoring but also erodes the firm's ability to recoup its upfront "investment," consisting primarily of the cost of opt-in discounts used to encourage participation. Imposing a minimum discount floor would exacerbate these effects, further reducing consumer welfare. This highlights the trade-off between curbing markups and protecting the firm's incentive to produce data in the first place (Posner 1978; Hermalin and Katz 2006).

Finally, our findings provide empirical support for a recent theoretical literature on voluntary disclosure and personalized pricing. Monitoring data is a form of *hard* (verifiable) information that cannot easily be falsified. Although voluntary disclosure of such information by some consumers may lead a monopolist firm (Pram 2020), or one facing horizontal competition (Ali, Lewis, and Vasserman 2022), to infer that others have lower types, consumers can still obtain a Pareto improvement—corresponding to more coverage purchased and hence more risk-sharing in our setting—if there are gains from trade with consumer types that wouldn't be served without the additional information. We also show that consumer control over information sharing—realized through the ex-ante opt-in structure in our setting—mitigates consumer harm from price discrimination. As in Ichihashi (2020) and Montes, Sand-Zantman, and Valletti (2019), we argue that consumer control may thus obviate the need for ex-post regulations on exclusive data ownership.

We conclude by highlighting several promising avenues for future research. Our study focuses on an environment in which information acquisition is costly and offered primarily by one firm. As in-vehicle monitoring technologies become more widespread and available from vehicle manufacturers directly, the equilibrium dynamics of pricing vis a vis monitoring information may change. Beyond this, while our work focuses on the impact monitoring in the insurance context, future work can further quantify the full social value from reduced traffic congestion or loss of life years (Edlin 1998).

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A Additional Figures and Tables

Figure A.1: Examples of Telematics Devices in U.S. Auto Insurance

Notes: These are some examples of the in-vehicle telecommunication (or "telematics") devices used in monitoring programs in U.S. auto insurance. These devices can be easily installed by plugging them into the on-board diagnostics (OBD) port. The OBD-II specification that these monitoring devices rely on has been mandatory for all cars (passenger cars and light trucks) manufactured or to be sold in the U.S. after 1995.

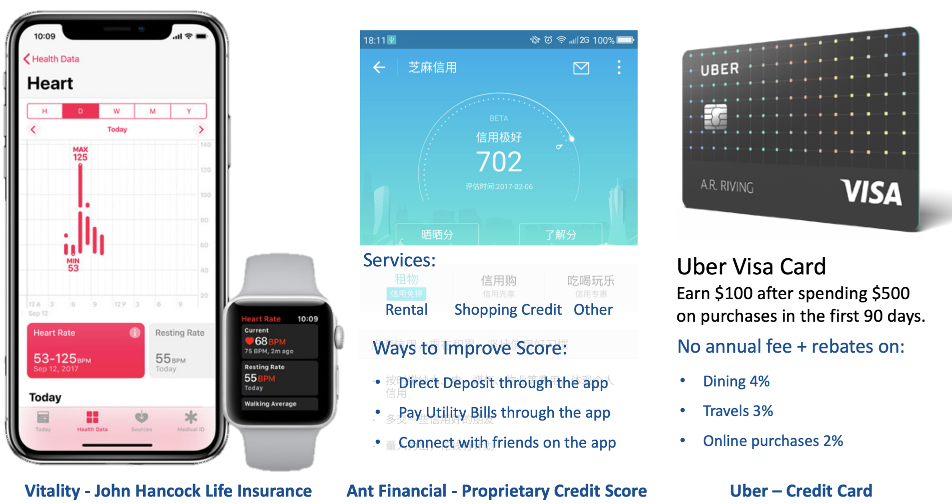


Figure A.2: Other Examples of Direct Transactions of Consumer Data

Notes: Examples of direct transactions of consumer data in other settings. The Vitality program from life insurer John Hancock tracks and rewards exercise and health-related behaviors in exchange for discounts on life insurance premiums. Ant Financial incentivizes users to conduct more personal finance transactions through the platform, such as setting up direct deposit or paying utility bills, in exchange for discounts on various borrowing and rental services. The Uber credit card offered much larger incentives for consumers to use it intensively than the transaction fees charged. One of the plausible business rationales is that the transaction data can be linked back to improve Uber's main businesses in ride sharing and in food delivery.

Table A.1: Summary Statistics on Observable Characteristics

Panel A: Binary Indicators (Mean)

ABS Ind. (0.26)	Credit Avail. Ind. (0.96)	Preferred Customer Ind. (0.07)
Age $\geq$ 21 Ind. (0.94)	Credit Report Ind. (0.95)	Prior Insurance Ind. (0.46)
Age $<$ 25 Ind. (0.27)	Female Ind. (0.48)	Prior Insurance with Lapse Ind. (0.09)
Age $>$ 60 Ind. (0.03)	Garage Verification Ind. (0.94)	Safe Device Ind. (0.26)
Class C Vehicle Ind. (0.72)	Homeowner Ind. (0.15)	Vehicle on Lease Ind. (0.39)
Clean Record Ind. (0.64)	Out-of-state Ind. (0.14)	
College Ind. (0.68)	Post Grad Ind. (0.32)	

Panel B: Continuous Variables

Variable	Mean	Std. Dev.	Min	P_{25}	P_{50}	P_{75}	Max
Calendar Month	5.95	3.39	1	3	6	9	12
Driver Credit Tier	112.34	34.44	0	92	106	121	242
Length of Prior Insurance	1.20	1.28	0	0	1	2	4
License Year Cat.	1.91	1.42	0	0	3	3	3
Log Zipcode Income	11.40	0.95	8.32	11.00	11.61	12.01	13.20
Ownership Length Cat.	0.35	0.87	0	0	0	0	4
Population Density Percentile	48.75	22.62	0	32	51	66	99
Record - DUI Count	0.04	0.23	0	0	0	0	6
Record: Accident Points	1.53	2.79	0	0	0	2	87
Record: At-Fault Accident Count	0.25	0.56	0	0	0	0	14
Risk Class	0.25	0.19	0.00	0.12	0.20	0.33	1.00
Vehicle Model Year	2004.56	6.62	1980	2000	2005	2010	2017
Zipcode Income ($'000)	124.66	92.82	4.11	59.99	109.77	163.68	542.65

Notes: This table presents summary statistics for the observable characteristics used in our reduced-form analysis. Panel A shows the means for binary indicator variables. "Avail." is short for "Available", and "Ind." for "Indicator". Panel B provides detailed distributional statistics for continuous variables. "Cat." represents categorical variables that are coarsened and discretized by segmenting the raw continuous variable. Zipcode income is winsorized at the 1st and 99th percentiles. This table includes all new customers that had single-driver-single-vehicle policies, with $N = 2,686,897$. To mitigate multicollinearity, we compute variance inflation factors (VIFs) by regressing drivers' risk class on all other observables and exclude variables with VIFs exceeding 5. For instance, drivers' age (mean 33.44) is removed as a result.

Table A.2: Summary Statistics on Observable Characteristics (Illinois)

Panel A: Binary Indicators (Mean)

ABS Ind. (0.23)	Credit Avail. Ind. (0.97)	Preferred Customer Ind. (0.08)
Age $\geq$ 21 Ind. (0.95)	Credit Report Ind. (0.93)	Prior Insurance Ind. (0.51)
Age $<$ 25 Ind. (0.24)	Female Ind. (0.47)	Prior Insurance with Lapse Ind. (0.09)
Age $>$ 60 Ind. (0.03)	Garage Verification Ind. (0.94)	Safe Device Ind. (0.36)
Class C Vehicle Ind. (0.94)	Homeowner Ind. (0.17)	Vehicle on Lease Ind. (0.42)
Clean Record Ind. (0.67)	Out-of-state Ind. (0.16)	
College Ind. (0.76)	Post Grad Ind. (0.45)	

Panel B: Continuous Variables

Variable	Mean	Std. Dev.	Min	P_{25}	P_{50}	P_{75}	Max
Calendar Month	5.97	3.38	1	3	6	9	12
Driver Credit Tier	103.73	29.84	59	86	99	113	242
Length of Prior Insurance	1.40	1.35	0	0	1	3	4
License Year Cat.	2.41	1.16	0	3	3	3	3
Log Zipcode Income	11.61	1.06	8.32	11.18	11.82	12.30	13.20
Ownership Length Cat.	0.56	1.07	0	0	0	1	4
Population Density Percentile	57.65	25.92	0	39	58	81	99
Record - DUI Count	0.02	0.14	0	0	0	0	4
Record: Accident Points	1.33	2.54	0	0	0	2	55
Record: At-Fault Accident Count	0.26	0.59	0	0	0	0	8
Risk Class	0.21	0.15	0.00	0.11	0.17	0.27	1.00
Vehicle Model Year	2005.78	6.30	1980	2002	2006	2011	2017
Zipcode Income ($'000)	164.43	130.80	4.11	71.45	135.54	219.40	542.65

Notes: This table presents summary statistics for the observable characteristics used in our structural estimation and counterfactual simulations. Panel A shows the means for binary indicator variables. "Avail." is short for "Available", and "Ind." for "Indicator". Panel B provides detailed distributional statistics for continuous variables. "Cat." represents categorical variables that are coarsened and discretized by segmenting the raw continuous variable. Zipcode income is winsorized at the 1st and 99th percentiles. This table includes all new customers that had single-driver-single-vehicle policies, with $N = 93,077$. To mitigate multicollinearity, we compute variance inflation factors (VIFs) by regressing drivers' risk class on all other observables and exclude variables with VIFs exceeding 5. For instance, drivers' age (mean 33.70) is removed as a result.

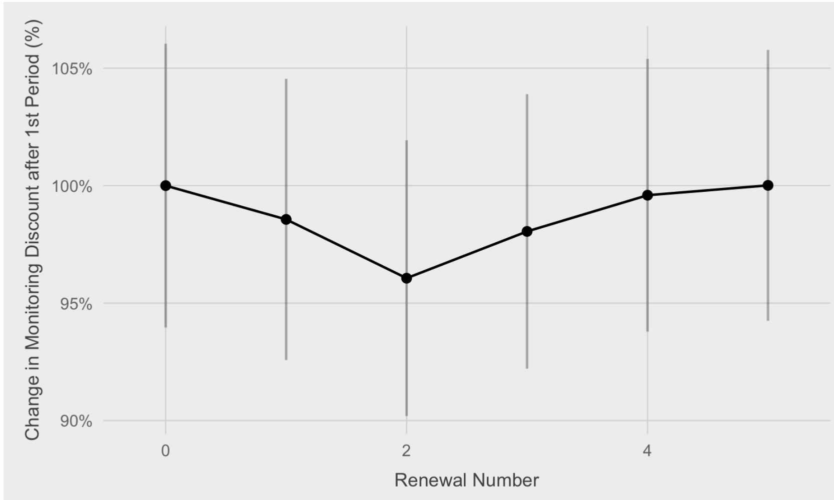


Figure A.3: Persistence of Monitoring Discount

Notes: This graph plots the empirical progression of monitoring discount for all monitoring finishers in one state that stayed with the Firm until at least the end of the 5th periods (so we observe monitoring discount in the renewal quote for the 6th period). The benchmark is monitoring discount in the first renewal quote ($t = 0$). Fluctuations and noises are due to ex-post adjustments. Monitored drivers can report mistakes in their records and have their discount adjusted.

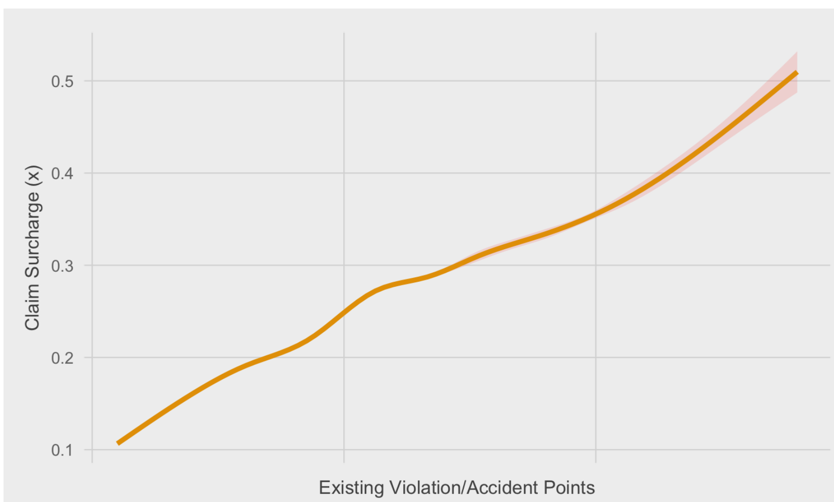


Figure A.4: Renewal Price Claim Surcharge

Notes: This graph plots the empirical claim surcharge function for at-fault accidents. Claim surcharge varies with existing violation points and calendar time. 0.1 means 10% surcharge. This differs from the filed factors because the latter is applied on the base rate only, while this function represents the surcharge percentage on top of overall premium. This is done by regressing renewal price change on violation point last period and current period at-fault claim, controlling for all other observables.

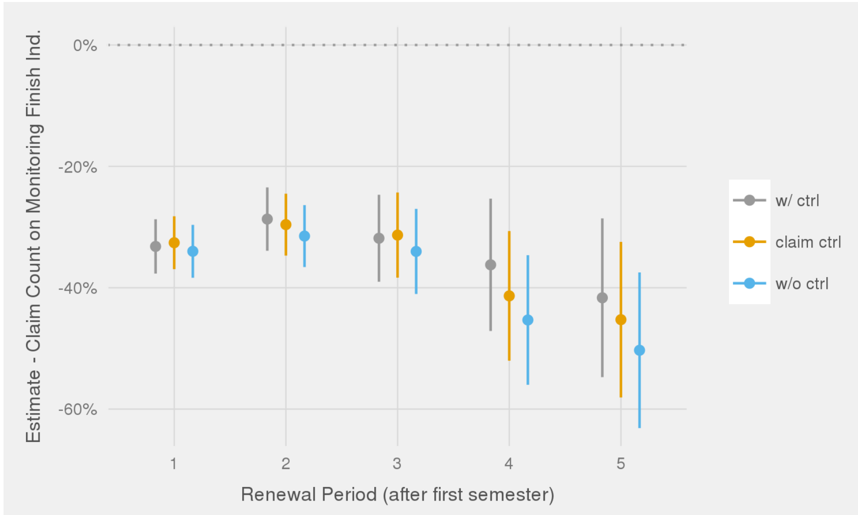


Figure A.5: Estimates - dynamic informativeness of monitoring participation

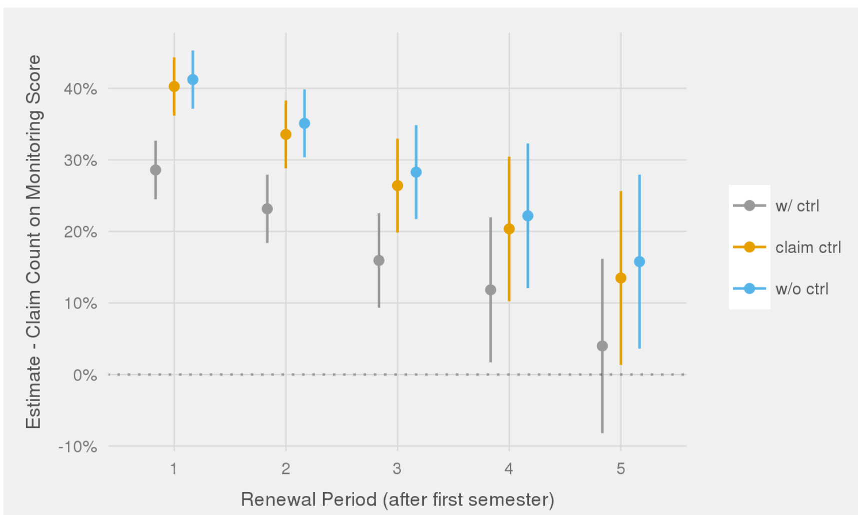


Figure A.6: Estimates - dynamic informativeness of monitoring score

Notes: Figures A.5 and A.6 report, in percent increase terms, the estimate for $\theta_{s,t}$ and $\theta_{m,t}$ for renewal periods $t = 1$ to 5 from the following regression:

$$C_{it} = \alpha_t + \theta_{m,t} m_i + \theta_{s,t} s_i + \mathbf{x}'_{it} \beta_{\mathbf{t}} + \varepsilon_{it} \tag{21}$$

Here, m is an indicator for finishing monitoring and s denotes the monitoring scores. The latter is normalized among monitored consumers and set to 0 for others. For each $t > 0$, we take all drivers who stayed with the firm until at least the end of period t. $\theta_{s,t}$ is the coefficient on monitoring score of i, and $\theta_{m,t}$ is that on monitoring finish indicator of i. Monitoring score is normalized, and defaulted as 0 for unmonitored drivers. So $\theta_{s,t}$ measures the effect of getting a score one standard deviation above the mean during the monitoring period ($t = 0$). $\theta_{m,t}$ compares unmonitored drivers with the average monitoring finisher. To further translate these effects into percent increase terms, we divide the estimates by the average claim count in period t of all *monitored* drivers. The horizontal axis represents different regressions for different renewal period $t > 0$. Different colors within each t value represent different specifications of control variables (x_{it}). The grey (left-most) series represents estimates from regressions with the full set of x_{it}; the orange (middle) one includes only claim records revealed since $t = 0$; the blue (right) series includes no control.

Table A.3: Additional Latent Parameter Summary

Parameter	mean	Q25	Q50	Q75	Q95	Correlation Log Baseline Risk	Correlation Log Private Risk
Severe Accident Risk (1e-2)	0.43	0.15	0.26	0.49	1.23	0.66	0.45
	(0.04)	(0.02)	(0.02)	(0.04)	(0.09)	(0.03)	(0.02)
Accident Severity Pareto Shape	1.50	1.48	1.50	1.53	1.58	0.22	-0.10
	(0.08)	(0.07)	(0.08)	(0.09)	(0.10)	(0.09)	(0.04)
Minor Accident Mean	395.53	349.57	394.26	442.05	501.76	0.04	0.12
	(13.59)	(15.43)	(14.86)	(16.73)	(16.24)	(0.08)	(0.03)
Baseline Renewal Factor - First Renewal	1.12	1.09	1.12	1.14	1.23	-0.44	-0.24
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.03)	(0.01)
Baseline Renewal Factor - Subsequent Renewals	1.00	0.99	1.01	1.02	1.03	-0.48	-0.20
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.02)	(0.00)
Renewal Factor w Telematics	1.23	1.05	1.12	1.31	1.85	0.20	0.73
	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)	(0.03)	(0.01)

Notes: This table reports the distributions of key parameters from our model. Columns are moments/correlations across individuals. Parentheses show bootstrap standard errors.

Table A.4: Remaining Parameter Estimates and Moral Hazard Effects

		Moral Hazard Effect			
Variable	Estimate	Regime	Monitoring Opt-in Coef.	Monitoring Intensity Coef.	Implied Moral Hazard Effect
Log Major Accident Fraction	-2.218***	1	-0.139***	-0.121***	22.895%
	(0.116)		(0.023)	(0.042)	
Log Minor Accident Severity SD	0.784***	2	-0.150***	-0.156***	26.361%
	(0.024)		(0.025)	(0.023)	
Logit Error SD	0.046	3	-0.166***	-0.179***	29.178%
	(0.037)		(0.033)	(0.025)	
Log Risk Private Component SD	4.076***				
	(0.205)				

Notes: This table reports parameter estimates for our renewal price and monitoring score models. Parentheses show bootstrap standard errors.

Table A.5: Renewal Price and Monitoring Score Hyper Parameter Estimates

Regime	Component	Log Score	First-Renewal w/ Monitoring	First-Renewal No Monitoring	Subsequent Renewal
1	Intercept	4.799***	0.711***	0.920***	1.028***
		(0.019)	(0.003)	(0.004)	(0.019)
1	Public Signal	-0.063***	-0.138***	-0.177***	-0.045***
		(0.015)	(0.004)	(0.003)	(0.015)
1	Private Signal	7.558***	0.043***	–	–
		(0.024)	(0.002)	–	–
2	Intercept	4.861***	0.905***	0.912***	0.950***
		(0.051)	(0.005)	(0.004)	(0.003)
2	Public Signal	0.392***	-0.067***	-0.183***	-0.046***
		(0.028)	(0.004)	(0.003)	(0.002)
2	Private Signal	4.973***	0.019***	–	–
		(0.042)	(0.002)	–	–
3	Intercept	5.932***	0.493***	1.022***	0.962***
		(0.022)	(0.005)	(0.004)	(0.005)
3	Public Signal	0.079***	-0.102***	-0.067***	-0.041***
		(0.020)	(0.003)	(0.003)	(0.004)
3	Private Signal	9.524***	0.093***	–	–
		(0.015)	(0.001)	–	–
4	Intercept	–	–	–	0.937***
		–	–	–	(0.004)
4	Public Signal	–	–	–	-0.044***
		–	–	–	(0.003)

Notes: This table reports estimates for our renewal price and monitoring score models. Parentheses show bootstrap standard errors.

Table A.6: Latent Parameter Loadings on Observables

X Variable	Log Baseline Accident Risk	Log Risk Aversion	Plan Switching Cost (10e3)	Firm Switching Cost (10e3)	Default Plan FE (10e3)	Monitoring Disutility (10e3)	Log Minor Accident Severity (10e3) Mean	Major Accident Severity (10e3) Pareto Shape
(Intercept)	-3.668***	-4.564***	2.103***	0.224*	0.126*	0.197***	0.288***	-2.769***
	(0.140)	(0.227)	(0.084)	(0.122)	(0.069)	(0.044)	(0.099)	(0.301)
Female Ind.	0.236	0.012	-0.038	0.005	-0.022	-0.014	0.019	-0.055
	(0.271)	(0.160)	(0.040)	(0.107)	(0.068)	(0.067)	(0.037)	(0.057)
Driver License Year	-0.122	-0.397	-0.046	0.030	0.034	-0.002	-0.000	-0.111***
	(0.208)	(0.292)	(0.046)	(0.133)	(0.066)	(0.040)	(0.041)	(0.038)
Home Ownership	-0.081	-0.055	-0.027	0.003	-0.054	-0.037	-0.022	-0.049
	(0.244)	(0.084)	(0.027)	(0.102)	(0.053)	(0.060)	(0.040)	(0.043)
Out-of-State License	-0.138	0.211**	0.033	-0.048	-0.059	0.018	-0.014	-0.079*
	(0.200)	(0.098)	(0.023)	(0.134)	(0.051)	(0.055)	(0.044)	(0.041)
Credit Report Ind....7	-0.053	-0.084	-0.060	0.019	0.071*	-0.003	0.028	-0.024
	(0.204)	(0.219)	(0.046)	(0.149)	(0.037)	(0.043)	(0.043)	(0.026)
Vehicle on Lease Ind.	0.057	0.384**	-0.000	-0.009	-0.074	-0.103	-0.001	-0.027
	(0.274)	(0.152)	(0.044)	(0.112)	(0.078)	(0.070)	(0.036)	(0.065)
ABS Ind.	0.370*	0.103	-0.033	0.112	0.117	0.234***	0.006	0.059
	(0.206)	(0.194)	(0.055)	(0.113)	(0.107)	(0.077)	(0.039)	(0.040)
Airbag Ind.	0.030	-0.251	-0.044	0.011	0.075	0.071	0.018	0.064
	(0.253)	(0.156)	(0.039)	(0.124)	(0.063)	(0.073)	(0.041)	(0.056)
Car Ownership Years	-0.559**	-0.584***	0.070*	0.024	0.004	-0.139*	0.053	0.010
	(0.260)	(0.216)	(0.037)	(0.163)	(0.115)	(0.072)	(0.044)	(0.037)
Class C Ind.	-0.235	0.359	-0.067	-0.200*	0.034	0.020	0.017	-0.019
	(0.170)	(0.247)	(0.047)	(0.104)	(0.082)	(0.016)	(0.015)	(0.017)
Garage Verified Ind.	-0.607**	0.477**	-0.078	-0.059	0.058	0.052	0.032	0.031
	(0.245)	(0.213)	(0.047)	(0.153)	(0.070)	(0.050)	(0.043)	(0.021)
Has Prior Ins.	-0.548**	0.529*	-0.017	-0.117	-0.256***	-0.043	0.043	-0.067
	(0.261)	(0.278)	(0.053)	(0.122)	(0.072)	(0.082)	(0.040)	(0.055)
– w/ Lapse	-0.094	0.440***	-0.021	-0.069	-0.010	-0.027	-0.012	-0.016
	(0.180)	(0.109)	(0.019)	(0.124)	(0.081)	(0.072)	(0.044)	(0.033)
Total Accident Records	0.453***	-0.084*	-0.007	0.030	0.011	-0.030	0.030***	0.005
	(0.095)	(0.045)	(0.007)	(0.066)	(0.034)	(0.034)	(0.010)	(0.006)
Total DUI Records	-1.003***	0.397***	-0.001	-0.011	0.001	0.003	-0.012	0.004
	(0.020)	(0.010)	(0.002)	(0.032)	(0.011)	(0.008)	(0.010)	(0.003)
Has Credit Report....18	0.628***	0.188	-0.058	0.054	-0.007	-0.010	0.049*	-0.021
	(0.133)	(0.208)	(0.048)	(0.096)	(0.050)	(0.034)	(0.028)	(0.018)
Age < 25	0.516*	0.071	-0.005	-0.008	0.033	-0.012	-0.066*	0.029
	(0.308)	(0.235)	(0.049)	(0.153)	(0.079)	(0.079)	(0.039)	(0.056)
Age > 21	-0.187	-0.134	-0.053	-0.049	-0.015	0.051	0.020	0.015
	(0.142)	(0.246)	(0.050)	(0.123)	(0.068)	(0.063)	(0.034)	(0.020)
Age > 60	-0.340***	-0.034	0.011*	0.074	-0.034	0.002	0.009	-0.010
	(0.080)	(0.031)	(0.006)	(0.101)	(0.029)	(0.024)	(0.029)	(0.017)
College Ind.	0.438**	-0.347*	-0.065	0.019	0.006	0.043	0.034	-0.010
	(0.218)	(0.191)	(0.044)	(0.139)	(0.057)	(0.068)	(0.046)	(0.045)
Post Grad Ind.	-0.760***	0.555**	0.057	0.056	-0.090	0.079	-0.064	0.003
	(0.292)	(0.260)	(0.047)	(0.105)	(0.074)	(0.109)	(0.040)	(0.051)
Delinq. Score*	0.631***	0.438***	-0.027	0.138	0.099*	-0.002	-0.024	0.019
	(0.160)	(0.101)	(0.017)	(0.140)	(0.058)	(0.040)	(0.029)	(0.021)
Population Density	0.182	-0.409***	-0.004	-0.081	0.003	-0.011	-0.137***	0.023
	(0.201)	(0.101)	(0.036)	(0.133)	(0.084)	(0.039)	(0.041)	(0.026)
Violation Record (Points)	0.472***	0.391***	-0.022**	0.209**	0.017	-0.011	0.026	-0.008
	(0.087)	(0.055)	(0.010)	(0.105)	(0.042)	(0.023)	(0.018)	(0.013)
Zipcode Income	-0.041	-3.228***	0.012	-0.001	-0.019	0.009	-0.075**	0.012
	(0.097)	(0.044)	(0.009)	(0.056)	(0.060)	(0.013)	(0.032)	(0.009)
Model Year	0.222**	-0.692***	-0.019	0.052	-0.045	-0.041	0.042	-0.010
	(0.108)	(0.131)	(0.036)	(0.124)	(0.040)	(0.028)	(0.028)	(0.017)
Quarter-Year	-0.086	0.193	-0.066*	0.069	0.034	0.106*	0.005	0.024
	(0.388)	(0.197)	(0.036)	(0.117)	(0.060)	(0.064)	(0.039)	(0.039)
Risk Class	1.237***	0.566***	-0.040*	0.579***	0.015	-0.070	0.011	-0.029
	(0.168)	(0.137)	(0.022)	(0.224)	(0.106)	(0.059)	(0.032)	(0.020)

Notes: This table reports hyper-parameter estimates for all latent parameters that are outlined in Section 3.1. Parentheses show bootstrap standard errors.

Table A.7: Latent Parameter Summary - Four-Period Horizon

Parameter	mean	Q25	Q50	Q75	Q95	Correlation Log Baseline Risk	Correlation Log Private Risk
Claim Rate (1e-2)	3.22	0.89	1.77	3.75	9.99	0.67	0.36
	(0.11)	(0.10)	(0.15)	(0.19)	(0.55)	(0.04)	(0.02)
Log Claim Rate	-3.99	-4.72	-4.03	-3.28	-2.30	1.00	0.38
	(0.06)	(0.09)	(0.07)	(0.05)	(0.05)	(0.00)	(0.03)
- Private Component	0.01	-0.16	-0.06	0.19	0.61	0.35	1.00
	(0.00)	(0.01)	(0.00)	(0.01)	(0.03)	(0.03)	(0.00)
Expected Cost to Insurer ($)	163.49	45.32	90.19	191.93	509.26	0.67	0.36
	(13.43)	(6.75)	(11.24)	(17.70)	(51.28)	(0.04)	(0.02)
Risk Aversion (1e-05)	8.83	3.79	6.86	11.72	22.40	-0.35	0.03
	(1.47)	(0.71)	(1.19)	(1.94)	(3.76)	(0.09)	(0.03)
Default Plan FE ($)	17.00	-38.56	7.63	63.74	152.20	0.61	-0.00
	(5.74)	(7.75)	(6.12)	(6.78)	(12.49)	(0.06)	(0.02)
Plan Switching Cost ($)	548.45	535.60	549.96	562.22	577.66	-0.47	0.06
	(60.00)	(59.45)	(60.08)	(60.46)	(62.42)	(0.15)	(0.04)
Firm Switching Cost ($)	329.04	242.31	292.79	383.53	582.57	0.41	0.09
	(13.62)	(14.98)	(13.10)	(17.74)	(30.41)	(0.08)	(0.03)
Monitoring Disutility ($)	207.65	141.64	205.25	271.56	369.05	-0.12	-0.07
	(9.53)	(8.14)	(9.43)	(12.49)	(20.83)	(0.10)	(0.02)

Notes: This table reports the distributions of key parameters from our model. Columns are moments/correlations across individuals. Risk and demand frictions (default plan FE, switching costs, and monitoring disutility) are reported on a per period basis. Parentheses show bootstrap standard errors.

Table A.8: Latent Parameter Summary - Two-Period Horizon

Parameter	mean	Q25	Q50	Q75	Q95	Correlation Log Baseline Risk	Correlation Log Private Risk
Claim Rate (1e-2)	3.66	0.45	1.17	3.40	12.13	0.48	0.39
	(0.12)	(0.14)	(0.18)	(0.20)	(0.49)	(0.05)	(0.03)
Log Claim Rate	-4.37	-5.41	-4.45	-3.38	-2.11	1.00	0.48
	(0.09)	(0.13)	(0.09)	(0.05)	(0.04)	(0.00)	(0.03)
- Private Component	0.07	-0.22	0.08	0.41	1.03	0.44	1.00
	(0.01)	(0.00)	(0.01)	(0.01)	(0.03)	(0.03)	(0.00)
Expected Cost to Insurer ($)	182.74	21.71	57.93	170.42	603.81	0.47	0.39
	(11.15)	(7.47)	(10.24)	(14.15)	(49.66)	(0.05)	(0.03)
Risk Aversion (1e-05)	4.46	2.17	3.55	5.75	11.03	-0.36	-0.11
	(0.91)	(0.49)	(0.75)	(1.16)	(2.16)	(0.06)	(0.02)
Default Plan FE ($)	64.63	-52.13	60.93	174.98	341.19	0.46	-0.01
	(7.02)	(12.53)	(8.38)	(14.72)	(24.97)	(0.06)	(0.02)
Plan Switching Cost ($)	715.99	644.87	697.43	773.08	896.89	0.53	-0.02
	(63.56)	(67.42)	(64.43)	(62.43)	(58.83)	(0.17)	(0.02)
Firm Switching Cost ($)	567.87	423.37	534.52	669.66	948.73	-0.10	-0.05
	(26.63)	(25.22)	(29.78)	(32.71)	(41.79)	(0.11)	(0.03)
Monitoring Disutility ($)	84.23	-39.49	77.41	199.94	387.56	-0.12	-0.13
	(14.33)	(14.35)	(14.51)	(22.37)	(38.54)	(0.09)	(0.02)

Notes: This table reports the distributions of key parameters from our model. Columns are moments/correlations across individuals. Risk and demand frictions (default plan FE, switching costs, and monitoring disutility) are reported on a per period basis. Parentheses show bootstrap standard errors.

Table A.9: Counterfactual Simulation Results - 50% Lower Monitoring Cost

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria: Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ$ p.c.y.)	0.00	+7.43	+6.54	+8.52	+8.19	+7.71
Firm Profit ($ p.c.y.)	30.04	35.88	39.26	38.82	37.55	37.87
Competitor Profit ($ p.c.y.)	71.66	68.48	67.21	67.11	67.29	66.85
Industry Profit ($ p.c.y.)	101.70	104.36	106.46	105.93	104.83	104.72
Total Surplus (Δ$ p.c.y.)	0.00	+10.09	+11.31	+12.75	+11.32	+10.73
Quantity						
Coverage ($000 p.c.y.)	108.53	108.62	108.74	108.92	108.78	108.78
First-Period Firm Market Share (%)	10.24	11.85	13.02	12.94	12.64	12.65
Renewal Firm Choice Prob (%)	14.52	16.25	17.00	16.88	16.71	16.76
Monitoring Market Share (%)	0.00	2.59	7.23	7.23	6.55	6.88
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.09	1.09	1.08	1.09
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.20	0.20	0.20	0.25
Risk Surcharge Factor (κ_{1s})	-	1.00	0.60	0.60	0.50	0.50
Rent Sharing Factor (κ_{1d})	-	1.00	0.10	0.20	0.00	1.00
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.99	0.99	0.99
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to $0, $118, and $640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to $84, $25, and $209, respectively.

Table A.10: Counterfactual Simulation Results - 50% Higher Monitoring Marginal Cost

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria: Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ$ p.c.y.)	0.00	+7.43	+5.71	+8.75	+7.27	+7.09
Firm Profit ($ p.c.y.)	30.04	35.27	37.64	37.22	36.13	36.35
Competitor Profit ($ p.c.y.)	71.66	68.48	68.09	67.52	68.17	67.64
Industry Profit ($ p.c.y.)	101.70	103.75	105.73	104.74	104.31	103.99
Total Surplus (Δ$ p.c.y.)	0.00	+9.48	+9.74	+11.80	+9.88	+9.38
Quantity						
Coverage ($000 p.c.y.)	108.53	108.62	108.56	108.83	108.65	108.66
First-Period Firm Market Share (%)	10.24	11.85	12.59	12.70	12.21	12.29
Renewal Firm Choice Prob (%)	14.52	16.25	16.50	16.66	16.17	16.32
Monitoring Market Share (%)	0.00	2.59	6.20	6.24	5.48	5.98
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.08	1.07	1.07	1.08
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.30	0.30	0.30	0.35
Risk Surcharge Factor (κ_{1s})	-	1.00	0.60	0.60	0.60	0.50
Rent Sharing Factor (κ_{1d})	-	1.00	0.00	0.00	0.00	1.00
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.99	0.99	0.99
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to $0, $118, and $640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to $84, $25, and $209, respectively.

Table A.11: Counterfactual Simulation Results - Two-Period Time Horizon

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria: Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ\$ p.c.y.)	0.00	+5.79	+5.56	+5.56	+5.51	+5.32
Firm Profit (\$ p.c.y.)	30.20	36.03	36.16	36.16	36.11	36.10
Competitor Profit (\$ p.c.y.)	83.62	79.40	79.68	79.68	79.70	79.83
Industry Profit (\$ p.c.y.)	113.82	115.43	115.85	115.85	115.82	115.92
Total Surplus (Δ\$ p.c.y.)	0.00	+7.41	+7.58	+7.58	+7.51	+7.42
Quantity						
Coverage (\$000 p.c.y.)	114.41	114.70	114.67	114.67	114.66	114.65
First-Period Firm Market Share (%)	9.81	11.80	11.79	11.79	11.77	11.70
Renewal Firm Choice Prob (%)	13.38	15.92	15.85	15.85	15.83	15.74
Monitoring Market Share (%)	0.00	3.50	3.77	3.77	3.74	3.61
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.01	1.01	1.01	1.01
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.90	0.90	0.90	1.00
Risk Surcharge Factor (κ_{1s})	-	1.00	0.60	0.60	0.60	0.50
Rent Sharing Factor (κ_{1d})	-	1.00	0.00	0.00	0.00	1.00
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	1.00	1.00	1.00
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to \$0, \$118, and \$640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to \$84, \$25, and \$209, respectively.

Table A.12: Counterfactual Simulation Results - Four-Period Time Horizon

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria: Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ\$ p.c.y.)	0.00	+3.58	+2.37	+7.53	-0.69	-1.80
Firm Profit (\$ p.c.y.)	20.63	30.43	33.07	31.30	32.65	32.93
Competitor Profit (\$ p.c.y.)	43.59	40.15	41.37	39.87	40.92	43.19
Industry Profit (\$ p.c.y.)	64.23	70.58	74.44	71.16	73.57	76.13
Total Surplus (Δ\$ p.c.y.)	0.00	+9.93	+12.59	+14.47	+8.65	+10.10
Quantity						
Coverage (\$000 p.c.y.)	96.23	96.23	95.74	97.22	94.76	94.59
First-Period Firm Market Share (%)	9.14	11.84	11.49	11.39	12.31	11.01
Renewal Firm Choice Prob (%)	12.59	15.17	14.45	14.33	15.70	13.92
Monitoring Market Share (%)	0.00	3.74	7.37	7.07	6.78	6.76
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.11	1.08	1.07	1.13
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.00	0.00	0.25	0.15
Risk Surcharge Factor (κ_{1s})	-	1.00	0.90	1.00	0.80	0.80
Rent Sharing Factor (κ_{1d})	-	1.00	0.50	0.40	0.70	1.00
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.96	1.04	1.03
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to \$0, \$118, and \$640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to \$84, \$25, and \$209, respectively.

Table A.13: Counterfactual Simulation Results - 2X Monitoring Pricing Constraints

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria			
			Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ\$ p.c.y.)	0.00	+7.43	+6.62	+8.49	+8.40	+7.36
Firm Profit (\$ p.c.y.)	30.04	35.57	38.42	38.00	35.64	36.57
Competitor Profit (\$ p.c.y.)	71.66	68.48	67.44	67.46	67.83	67.30
Industry Profit (\$ p.c.y.)	101.70	104.05	105.86	105.45	103.47	103.87
Total Surplus (Δ\$ p.c.y.)	0.00	+9.79	+10.79	+12.25	+10.17	+9.54
Quantity						
Coverage (\$000 p.c.y.)	108.53	108.62	108.68	108.86	108.65	108.69
First-Period Firm Market Share (%)	10.24	11.85	12.89	12.78	12.17	12.36
Renewal Firm Choice Prob (%)	14.52	16.25	16.88	16.70	16.36	16.48
Monitoring Market Share (%)	0.00	2.59	6.72	6.65	4.99	6.17
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.08	1.08	1.05	1.08
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.25	0.25	0.35	0.30
Risk Surcharge Factor (κ_{1s})	-	1.00	0.60	0.60	0.50	0.50
Rent Sharing Factor (κ_{1d})	-	1.00	0.00	0.00	0.30	1.00
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.99	0.99	0.99
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	2.00	2.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	-2.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to \$0, \$118, and \$640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to \$84, \$25, and \$209, respectively.

Table A.14: Counterfactual Simulation Results - Concentrated Market

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria			
			Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ\$ p.c.y.)	0.00	+12.57	+13.11	+15.59	+14.65	+14.67
Firm Profit (\$ p.c.y.)	66.19	75.70	78.12	76.86	75.52	75.78
Competitor Profit (\$ p.c.y.)	177.02	166.33	165.63	165.64	166.18	165.66
Industry Profit (\$ p.c.y.)	243.20	242.04	243.75	242.50	241.70	241.44
Total Surplus (Δ\$ p.c.y.)	0.00	+11.40	+13.65	+14.88	+13.14	+12.91
Quantity						
Coverage (\$000 p.c.y.)	111.98	112.02	112.00	112.22	112.12	112.10
First-Period Firm Market Share (%)	23.63	26.67	27.24	26.91	26.61	26.61
Renewal Firm Choice Prob (%)	28.18	30.92	30.96	30.49	30.32	30.37
Monitoring Market Share (%)	0.00	5.50	10.10	9.96	9.40	9.46
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.05	1.05	1.05	1.05
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.55	0.55	0.50	0.60
Risk Surcharge Factor (κ_{1s})	-	1.00	0.40	0.40	0.40	0.30
Rent Sharing Factor (κ_{1d})	-	1.00	0.30	0.30	0.20	1.00
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.99	0.99	0.99
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to \$0, \$118, and \$640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to \$84, \$25, and \$209, respectively.

Table A.15: Counterfactual Simulation Results - Median-Pricing Composite Competitor

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria: Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ$ p.c.y.)	0.00	+5.80	+6.96	+11.25	+10.65	+10.07
Firm Profit ($ p.c.y.)	29.85	32.94	33.71	32.97	32.66	32.77
Competitor Profit ($ p.c.y.)	225.62	221.71	220.30	219.42	219.71	220.20
Industry Profit ($ p.c.y.)	255.46	254.66	254.01	252.38	252.37	252.97
Total Surplus (Δ$ p.c.y.)	0.00	+5.00	+5.52	+8.18	+7.56	+7.58
Quantity						
Coverage ($000 p.c.y.)	107.59	107.66	107.66	107.82	107.78	107.74
First-Period Firm Market Share (%)	10.86	11.82	12.16	12.13	12.01	11.91
Renewal Firm Choice Prob (%)	18.39	19.23	19.28	19.30	19.22	18.93
Monitoring Market Share (%)	0.00	1.78	3.29	3.24	2.98	3.21
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.02	1.01	1.01	1.02
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.70	0.70	0.65	0.65
Risk Surcharge Factor (κ_{1s})	-	1.00	0.30	0.30	0.40	0.30
Rent Sharing Factor (κ_{1d})	-	1.00	1.30	1.20	1.10	1.20
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.99	0.99	0.99
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to $0, $118, and $640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to $84, $25, and $209, respectively.

B Analysis of Observed Firm Pricing [For Online Publication]

Baseline pricing and monitoring's cream skimming effect The monitoring firm may wish to increase prices for unmonitored drivers both to encourage monitoring adoption and to reflect a cream skimming effect—advantageous selection into monitoring may draw safer drivers out of the unmonitored pool, raising the latter's average risk. To test this, we exploit the staggered introduction of monitoring across states using a regression discontinuity design. We compare prices and average costs in the unmonitored pool a year before and after monitoring began, controlling for state fixed effects, seasonality, and observable driver and coverage characteristics. To avoid contamination from attrition, we restrict attention to the first period ($t = 0$). Formally, we estimate:

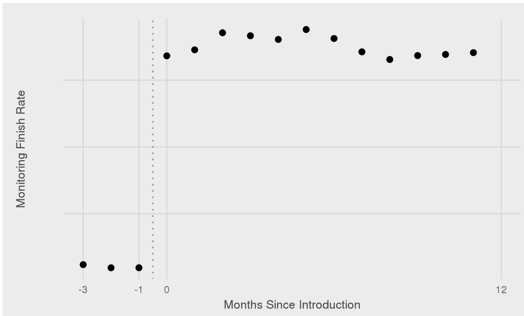
$$dep.\ var._i = \alpha + \gamma Qtr_i + \kappa \mathbf{1}_{post,i} + \theta \cdot Qtr_i \times \mathbf{1}_{post,i} + \mathbf{x}_i' \beta + \xi_{y,i} + \varepsilon_i \tag{22}$$

where Qtr_i denotes the driver's arrival quarter and $post_i$ indicates whether entry occurred after monitoring was introduced. The monitoring opt-in rate is plotted in Figure B.1(a). We examine both price (p_i) and claim count (C_i) as dependent variables. The coefficient θ captures the effect of the introduction of the monitoring program on the unmonitored pool.

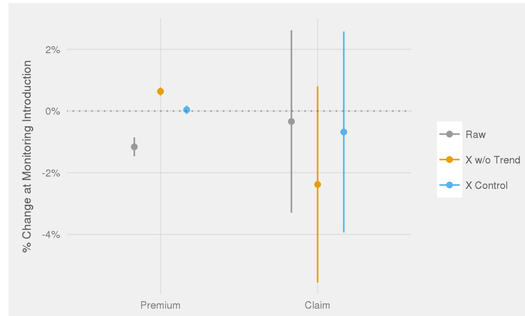
Estimates for $\hat{\theta}$, reported in Figure B.1(b), show no statistically significant price increases and or average cost inflation in the unmonitored pool. Since monitoring represents only a small share of the market, even strong selection into monitoring has limited effect on unmonitored drivers. Moreover, the firm does not pursue customers who initially opt out, making full unraveling unlikely. Finally, because monitoring programs require approval from state commissioners, any baseline price changes are subject to regulatory scrutiny. Taken together, these results suggest that the current regime is largely welfare-neutral for unmonitored drivers.

Ex-post monitoring pricing and rate revisions For each monitored driver, the firm processes the driving data and derive a one-dimensional monitoring score. Ex-post monitoring pricing maps the driver's monitoring score to a persistent discount or surcharge.

Our Illinois panel covers three baseline pricing regimes and three monitoring pricing regimes. As mentioned above, changes in monitoring pricing across monitoring regimes resulted from both the generation of monitoring scores (Figure B.2a) and how the firm set monitoring discounts corresponding to monitoring tiers (Figure B.2b). In terms of the former, we show



(a)

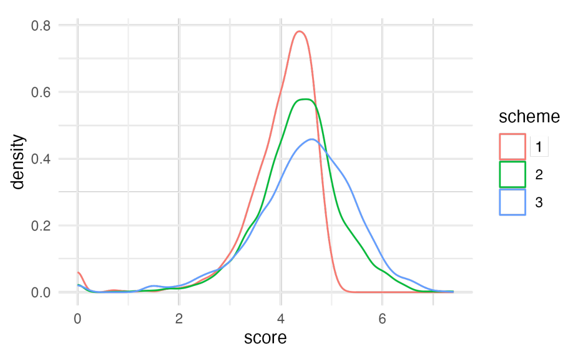


(b)

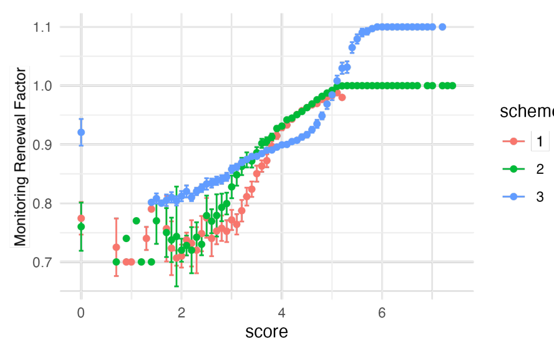
Figure B.1: Monitoring Opt-In Rate and Price/Claim Effect Around Introduction

Notes: (a) plots the progression of monthly monitoring finish rate around the introduction of monitoring. The monthly finish rate was below 0.1% in all months before monitoring introduction. The reason why it is not exactly zero is due to small-scale trials. We throw out states that introduced monitoring in the first three months or the last 12 months of our research window so that the results do not pick up changes in state composition. (b) reports regression-discontinuity estimate θ of equation (22), where the horizontal axis distinguishes dependent variable used; while different colors and positions represent different specifications of control variables (x_{it}). The grey (left-most) series represents estimates from regressions with the full set of x_{it}; the orange (middle) one includes a full set of observables, including flexible controls for trend and seasonality. These effects are translated in percentage terms by dividing the average of the dependent variable in the period immediately before monitoring introduction. We look at only first period outcomes, and include all *unmonitored* drivers arriving at the monitoring firm a year before or after. States that introduced monitoring within a year after the beginning or a year before the end of our research window are excluded. The running variable is quarter since monitoring introduction.

that monitoring scores increase with our estimated latent accident risk, but much more so for those produced by the second and third monitoring regimes (Figure B.3). Such increased precision across monitoring regimes was at least partially priced in: across monitoring regimes, monitoring has always increased the already positive relationship between renewal prices and latent accident risk that previously relied only on characteristics-based risk rating, and such increase is further strengthened in latter monitoring regimes (Figure B.4).



(a)



(b)

Figure B.2: Monitoring Score: Distribution and Discount Mapping by Monitoring Regimes

Notes: (a) This graph plots the density of monitoring scores across the three monitoring regimes. Our counterfactual simulations are based on the third monitoring regime. (b) This graph is a binned scatter plot of the monitoring discounts given to monitored drivers against their monitoring score. The Y axis is monitoring renewal factor, where 0.8 implies a 20% monitoring discount and 1.1 means a 10% surcharge. Our counterfactual simulations are based on the third monitoring regime.

Figure B.3: Monitoring Score vs. Accident Risk by Monitoring Regimes

Notes: The X-axis represents the estimated Poisson risk arrival rate while the Y-axis plots monitoring score, data or predicted by our model, by monitoring regimes. Figure 6(b) corresponds to monitoring regime 3 in the right panel.

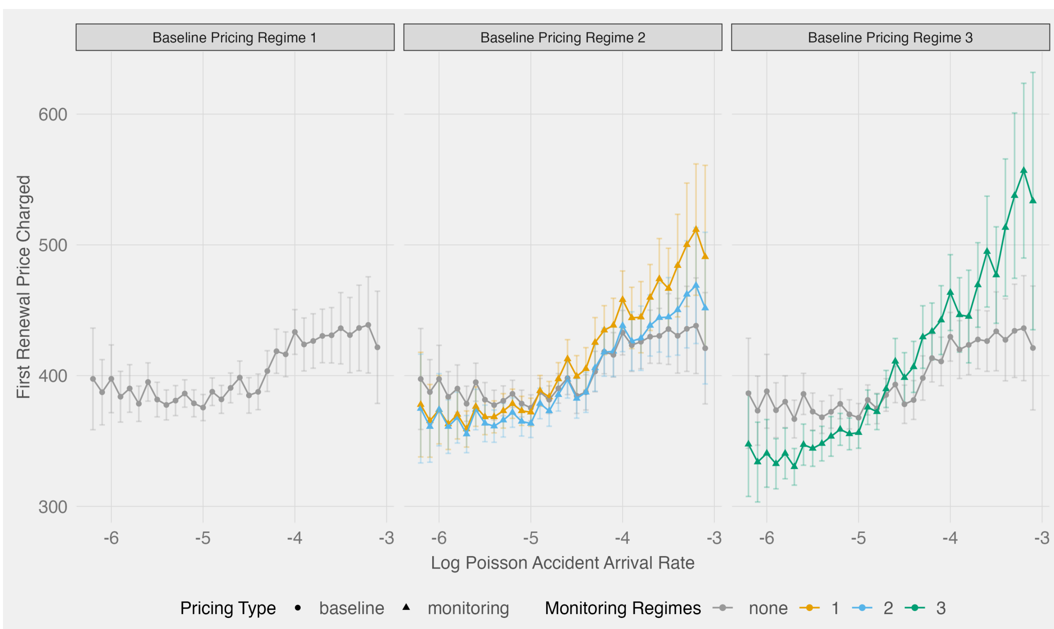


Figure B.4: Renewal Price vs. Accident Risk by Pricing and Monitoring Regimes

Notes: The X-axis represents the estimated Poisson risk arrival rate while the Y-axis plots the average first-renewal price for consumers in various risk-bins. We focus on the standard $50,000 limit plan. Each panel corresponds, chronologically, to a pricing regime for the baseline (unmonitored) consumer pool in grey round dots. The colored triangle dots plot the monitoring pricing regimes for consumers that opt into monitoring. Figure 6(a) corresponds to right-most panel.

C Additional Robustness Checks [For Online Publication]

C.1 Moral Hazard Effects using Full Unbalanced Panel

Our main results in Table 2 (Section 2) are derived with a balanced panel to mitigate the concern of selective attrition by monitoring status. We then extend the time horizon but use the full unbalanced panel to produce Figure 4. This section tests the robustness of both results to panel balancing. Table C.1 replicates Table 2 in the full unbalanced panel, while Figure C.1 replicates Figure 4 in the balanced panel. The results presented here are almost identical to their counterparts in Section 2, except for changes in power due to the differences in sample size. This suggests that our main moral-hazard results are unaffected by consumer attrition at renewals.

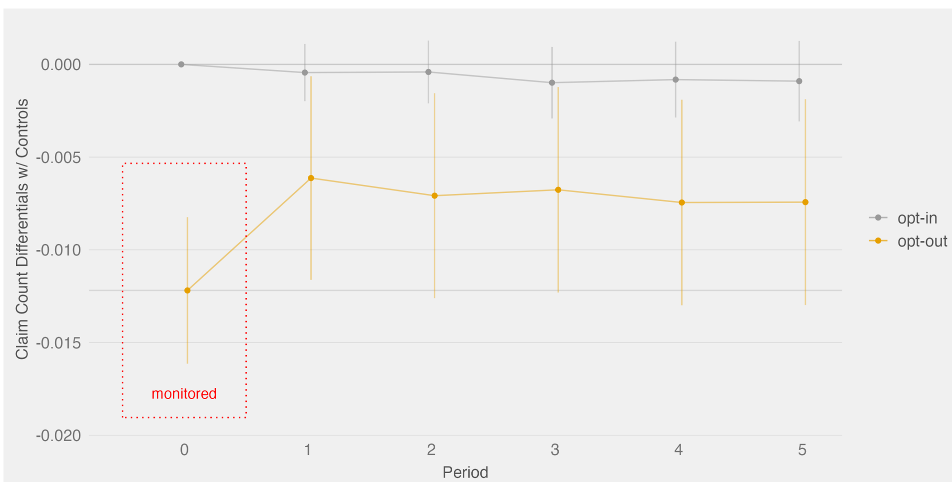


Figure C.1: Claim Progression across Monitoring Groups (Balanced Panel)

Notes: This graph reports the robustness fixed effect estimates of eq. (3), where we use a balanced panel for the first six periods (three years). The grey line plots ω_t while the orange line plots $\omega_t + \theta_t$, both against insurance periods t. The red box is superimposed ex-post to represent the period when opt-in consumers are monitored. Error-bars report 95% confidence interval.

Table C.1: **Estimation Results: Moral Hazard Effect (Full Unbalanced Panel)**

	dependent variable: claim count (C)										
explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	Parallel Trend/Placebo		
constant	0.048***	0.006	-0.015		0.048***	0.004	-0.017		0.032	0.031	0.026
	(0.000)	(0.009)	(0.009)		(0.000)	(0.009)	(0.009)		(0.063)	(0.063)	(0.063)
monitoring indicator (m)	-0.015***	-0.013***	-0.012***		0.010***	0.008***	0.009***		-0.005***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
post monitoring indicator ($\mathbf{1}_{post}$)	-0.003***	-0.004***	-0.004***	-0.003***	-0.004***	-0.004***	-0.004***	-0.003***	-0.006***	-0.005***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
monitoring duration (z)					-0.029***	-0.024***	-0.025***				
					(0.002)	(0.002)	(0.002)				
interaction ($\mathbf{1}_{post} \times m$)	0.008***	0.007***	0.007***	0.008***	-0.005**	-0.006***	-0.006***	-0.004**	-0.000	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
interaction ($\mathbf{1}_{post} \times z$)					0.016***	0.018***	0.017***	0.015***			
					(0.002)	(0.002)	(0.002)	(0.002)			
observables controls (x)	No	Yes	Yes	No	No	Yes	Yes	No	Yes	Yes	Yes
coverage fixed effects	No	No	Yes	No	No	No	Yes	No	Yes	Yes	Yes
driver fixed effects	No	No	No	Yes	No	No	No	Yes	No	No	No
implied moral hazard effect (%)	32.49%	30.16%	29.5%	31.66%	35.94%	35.13%	34.68%	34.28%	-1.61%	-1.8%	-3.5%
pre / post periods - "1st diff"	$t = 0/1-5$	$t = 0/1-5$	$t = 0/1-5$	$t = 0/1-5$	$t = 0/1-5$	$t = 0/1-5$	$t = 0/1-5$	$t = 0/1-5$	$t = 1/2-5$	$t = 1-2/3-5$	$t = 1-3/4-5$
treatment group - "2nd diff"	finishers	finishers	finishers	finishers	all monitored	all monitored	all monitored	all monitored	finishers	finishers	finishers
N	4,068,061	4,068,056	4,068,056	3,958,532	4,258,003	4,257,997	4,257,997	4,142,800	3,150,912	3,150,912	3,150,912

Notes: This table reports results of equation (2). The datasets consists of users that are eligible for monitoring regardless of whether they have stayed throughout the pre / post periods (unbalanced panel). Compared to columns (5) to (8), columns (1) to (4) remove all drivers that started but did not finish monitoring. The estimate on the interaction term ($\mathbf{1}_{post} \times m$ or z) measures the "treatment effect" of monitoring ending on claim count across periods. This gives us two renewal semesters ($t \in \{1,2\}$) after the monitoring semester ($t = 0$). We control for a full set of observables, including driver and vehicle characteristics and tiers (past records of violations or claims). Continuous observable characteristics are normalized. We report estimates with and without these controls. Columns (3) and (6) are our main specification. Column (3) focuses on monitored drivers who finished within the first period, while Column (6) introduces additional variation in monitoring duration and timing and looks at all monitoring finishers. Columns (1,2,4,5) show robustness of our estimates to observable and coverage fixed-effect controls. The right-most columns are placebo tests for parallel trends among treatment/control groups after monitoring ends. We first try to detect a similar change from $t = 1$ to $t = 2-5$. We drop all observations from period 0, and roll the post-period cutoff one period forward, so that $1_{post,t} = 1 \iff t \geq 2$ (changed from $t \geq 1$). Naturally, we look at the future trends of monitored drivers who finished within the first semester and drop other monitored finishers. We find similar results by repeating this test in subsequent periods.

C.2 Heterogeneity in Moral Hazard Effects

Reduced-form and Model Estimates We investigate heterogeneity in the moral hazard effect across consumers with different observable characteristics and different insurance coverage choices. We adapt Equation (3) to the following specification:

$$C_{it} = \alpha + [\tau m_i + \omega \mathbf{1}_{post,t} + \theta_{\mathbf{mh}} m_i \cdot \mathbf{1}_{post,t}] \cdot (1, \mathbf{x}_{i0}, y_{i0})' + (\mathbf{x}_{it}, \mathbf{y}_{it})' \beta + \varepsilon_{it} \tag{23}$$

Here, $(1, \mathbf{x}_{i0}, y_{i0})'$ indicates the initial characteristics and coverage choice of consumer i and is thus time-invariant. This vector is interacted with the first and second differencing terms in Equation (2) to uncover heterogeneity in the moral hazard effect. The interaction coefficients are presented in Figures C.2 and C.3.

As the figures show, heterogeneity in the moral hazard effect is limited. The most notable variation is by drivers' initial risk class. A positive coefficient here is intuitive: a higher risk class implies higher baseline prices and greater potential saving from the monitoring program. Drivers with higher risk class may thus be more incentivized to exert effort and reduce their accident risk.

However, such responses to incentives do not hold across other price shifters. For instance, the coefficient on zipcode income is not significant, and we do not see smaller moral hazard effects as monitoring rewards became less financially generous over the three monitoring regimes: the coefficients on state-level regime indicators (ubi_vers_st2 and ubi_vers_st3) are both positive and insignificant. This is echoed in our Illinois estimation sample, where the discount schedule shrank and the surcharge schedule grew over time, as shown in Figure B.2. However, as Table A.4 shows, the moral hazard effect increased across the three monitoring regimes in Illinois. This suggests that the moral hazard effect might have been more heavily influenced by non-financial features of the program design (such as improved feedback mechanisms during the monitoring period) than by the scope of potential savings.

Model and Counterfactual Robustness We conduct a robustness exercise allowing the moral hazard effect to vary across risk-class in our estimation and counterfactual simulations. Tables C.2 and C.3 show the fit of the model, which compared to Tables 4 and 5, shows no clear sign of improvement. This is unsurprising given that, when limited to the Illinois estimation data, the coefficient on risk class is no longer economically or statistically significant (Table C.4). The counterfactual results are shown in Table C.5. Our main counterfactual results remain qualitatively identical and quantitatively similar.

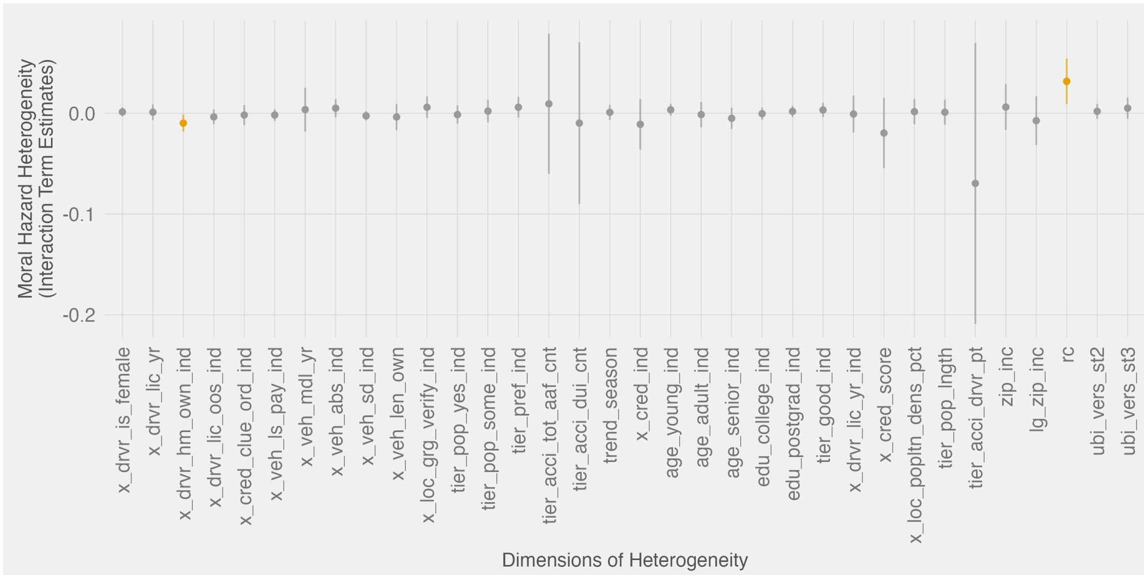


Figure C.2: Heterogeneity in Incentive Effect across Coverage Choice

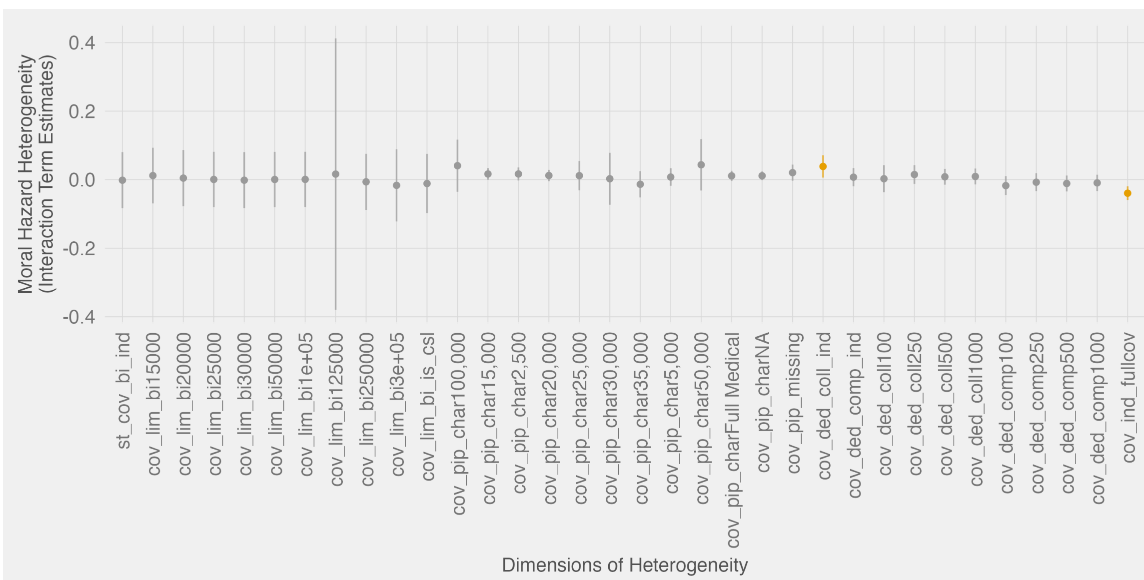


Figure C.3: Heterogeneity in Incentive Effect across Observables

Notes: These figures plot the estimated coefficients $\hat{\theta_{\mathbf{mh},\mathbf{x}}}$ in Equation (23) as well as the corresponding 95% confidence intervals. A positive coefficient means that drivers with higher values (or a 1 in the case of binary variables) in the variables listed in the horizontal axis saw higher claim increase after monitoring, hence have larger moral hazard effect. The statistically significant variables are: 'x_drvr_hm_own_ind' (home ownership indicator), 'rc' (risk class), 'cov_ded_coll_ind' (driver has collision coverage ind.), 'cov_ind_fullcov' (driver has both collision and comprehensive coverage ind.).

Table C.2: Fit of Claim Risk, Monitoring Score, and Renewal Pricing

Risk & Score			**Pricing**		
Moment	Data	Predicted	Moment	Data	Predicted
Poisson claim counts			*First renewal pricing factor*		
first moment	0.039	0.039	first moment	1.129	1.126
major claims	0.004	0.004	second moment	1.293	1.271
second moment	0.051	0.045	covariance with risk	0.044	0.041
N	199,368		*N*	55,272	
Monitoring score			*Latter renewal pricing factor*		
first moment	4.301	4.317	first moment	0.999	1.002
second moment	19.166	19.360	second moment	1.010	1.005
covariance with risk	0.154	0.142	covariance with risk	0.034	0.036
N	8,106		*N*	103,344	

Notes: This table reports the fit of model (incorporating risk-class-based heterogeneity in the moral hazard effect) predictions to key data moments. Compared to Tab. 4, the only change detectable (more than 0.001) are the monitoring score moment predictions.

Table C.3: Fit of Choice Shares (Coverage, Monitoring, and Attrition) and Selection

	New customers						Renewal customers			
	Pre-Mtr		Post-Mtr							
	Pre-MM Chg				Post-MM Chg		Pre-MM Chg		Post-MM Chg	
	Data	Pred	Data	Pred	Data	Pred	Data	Pred	Data	Pred
Coverage Share										
40,000	46.90	46.40	45.20	47.70			36.50	37.60		
50,000	14.20	19.20	12.90	19.00	51.80	50.10	10.70	11.50	45.10	45.00
100,000	17.30	15.20	19.90	15.60	25.60	24.20	16.50	16.30	20.80	21.00
150,000	18.30	12.10	18.00	11.40	19.70	16.80	18.20	16.60	18.90	19.00
300,000	3.30	7.10	3.90	6.30	3.00	9.00	3.30	2.80	3.50	3.40
Coverage Selection										
40,000	100.00	100.00	100.00	100.00			100.00	100.00		
50,000	30.40	32.60	14.20	25.00	100.00	100.00	28.10	24.40	100.00	100.00
100,000	29.00	28.70	21.90	22.90	32.40	32.10	37.40	34.30	32.30	45.90
150,000	34.30	23.10	33.00	17.10	27.70	20.60	45.00	41.30	41.10	46.50
300,000	6.30	12.40	4.40	9.50	4.10	9.30	7.10	6.80	6.80	7.90
Monitoring Opt-in										
Share			14.10	14.80	21.00	19.90				
Selection			9.70	15.30	21.80	22.90				
Attrition										
Share							14.90	15.33	12.73	12.53
N	8,623		21,040		24,864		51,550		113,400	

Notes: This table reports the fit of model (incorporating risk-class-based heterogeneity in the moral hazard effect) predictions to key data moments. Compared to Tab. 5, there are slight shifts in coverage and monitoring shares and selection patterns.

Table C.4: Moral Hazard Effects (with Risk-Class Heterogeneity)

Regime	Monitoring Opt-in Coef	Monitoring Intensity Coef	Risk Class Coef
1	-0.105***	-0.084***	-0.002
	(0.023)	(0.042)	(0.007)
2	-0.109***	-0.149***	-0.004
	(0.025)	(0.022)	(0.008)
3	-0.129***	-0.131***	-0.008
	(0.032)	(0.025)	(0.012)

Notes: This table reports parameter estimates for the moral hazard effect, similar to Table A.4 but incorporating heterogeneity with respect to drivers' risk class. Parentheses show bootstrap standard errors.

Table C.5: Counterfactual Simulation Results - Heterogeneous Moral Hazard Effect

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria			
			Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ$ p.c.y.)	0.00	+7.60	+6.74	+8.77	+7.57	+7.10
Firm Profit ($ p.c.y.)	29.71	35.43	38.37	37.94	36.74	37.01
Competitor Profit ($ p.c.y.)	72.01	68.73	67.49	67.63	68.12	67.70
Industry Profit ($ p.c.y.)	101.72	104.17	105.86	105.57	104.85	104.72
Total Surplus (Δ$ p.c.y.)	0.00	+10.04	+10.87	+12.61	+10.70	+10.09
Quantity						
Coverage ($000 p.c.y.)	108.67	108.79	108.99	109.11	108.92	108.92
First-Period Firm Market Share (%)	10.13	11.80	12.97	12.76	12.34	12.35
Renewal Firm Choice Prob (%)	14.37	16.18	16.95	16.70	16.32	16.37
Monitoring Market Share (%)	0.00	2.69	7.33	6.80	6.19	6.53
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.09	1.08	1.08	1.09
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.20	0.25	0.25	0.30
Risk Surcharge Factor (κ_{1s})	-	1.00	0.60	0.60	0.50	0.50
Rent Sharing Factor (κ_{1d})	-	1.00	0.00	0.00	0.00	1.00
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.99	0.99	0.99
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to $0, $118, and $640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to $84, $25, and $209, respectively.

C.3 Learning Effects from Monitoring

Our baseline specification assumes that monitoring does not alter drivers' persistent risk types. In other words, we do not model learning effects whereby drivers permanently reduce their risk after being monitored. Allowing for such learning would not change our moral hazard estimates, but it could reduce the extent of advantageous selection, since part of the post-monitoring risk gap between monitored and unmonitored groups could then reflect learning rather than selection.

Evidence from Public Records of At-Fault Accidents Because monitoring begins in the first policy period, the firm's internal claims data cannot capture any pre-existing risk differences between monitored and unmonitored drivers. To assess this, we turn to drivers' public-record at-fault accidents (AFA records). For all new customers, the firm purchases a LexisNexis report containing a historical aggregate measure of at-fault accidents, covering one to five years of prior records depending on driver experience and state reporting rules. This measure enters the firm's risk-class assignment and thus pricing. For renewals, the firm does not purchase new reports; instead, it reports actuarially adjusted claims to LexisNexis and internally updates AFA records accordingly.

Compared to claims data, AFA records are delayed when claims involve complex adjustments that spill over into future periods. Moreover, some claims, especially minor ones, may be missing from AFA records due to administrative leniency or incomplete adjustments. Overall, the mean trailing-twelve-month AFA records for a renewal customer is 0.018, compared to the average claim records of 0.05 per six-month period.

However, these limitations are not systematically affected by monitoring. We can therefore replicate the difference-in-differences analysis in Section 3, replacing claims with the occurrence of at-fault accidents from the public record. We find that the monitored group has 8.7% lower AFA in the pre-period (the level gap shown in Figure C.4 divided by the mean AFA in the unmonitored group of the corresponding period), which changed to 38.8% during monitoring, 21.9% and 7.4% post-monitoring.

Consistent with our main results, monitored drivers appear safer even in the pre-period, indicating the presence of advantageous selection. The moral hazard effect is evident during monitoring, when AFA records further diverge between monitored and unmonitored groups, but this effect completely disappears when we re-examine the AFA records between one to two years after monitoring (at the beginning of year 3). We thus conclude that monitoring's learning effect, as measured by its persistent impact on accident risk, is likely negligible.

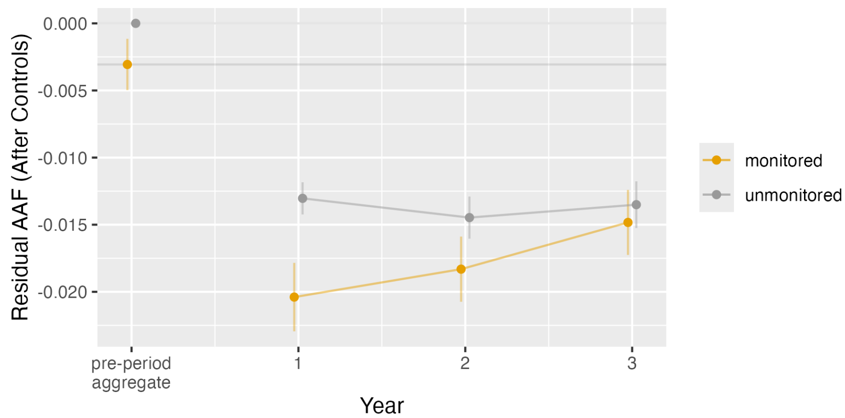


Figure C.4: At-Fault Accident Violation Progression by Monitoring Groups

Notes: This graph reports the year fixed-effect estimates across monitoring groups of a revised version of eq. (3), where we use public-record at-fault accidents as the dependent variable. For renewal customers, we focus on the beginning of policy periods 1 (right after monitoring), 3 (year 1 post-monitoring), and 5 (year 2 post-monitoring). For new customers, historical aggregates cover one to five years depending on experience and reporting rules. Risk class is excluded from controls; results remain similar when it is included. The grey line plots ω_t and the orange line $\omega_t + \theta_t$. Error bars denote 95% confidence intervals.

Counterfactual Robustness As a robustness check, we re-estimate the model and counterfactuals under the assumption of a 10% learning effect, i.e., that monitored drivers' accident risk falls permanently by 10% after monitoring.[36] Table C.6 shows that learning increases both consumer and producer surplus, leading to more aggressive monitoring pricing in the initial period and greater rent-sharing in renewals. However, our main results—the welfare benefits from monitoring and the harm of data portability regulation—remain unchanged.

[36]We choose 10% because, in our analysis above, relative to the pre-period, the change in the monitored-unmonitored AFA gap is -13.2% and +1.2% one and two years after monitoring. Even if we completely ignore the reporting delay in AFA records and the last data point, 10% would represent the larger end of learning effects consistent with the data.

Table C.6: Counterfactual Simulation Results - 10% Learning Effect

Metrics[1]	Scenarios[2]					
	No Monitoring	Current Regime	Counterfactual Equilibria Partial	Full	+ Data Port.	+ Disc. Floor
Surplus Division						
Consumer Welfare (Δ\$ p.c.y.)	0.00	+9.47	+8.95	+10.75	+9.62	+9.25
Firm Profit (\$ p.c.y.)	28.17	36.90	41.60	41.14	39.60	40.15
Competitor Profit (\$ p.c.y.)	77.98	73.45	71.27	71.31	71.75	71.23
Industry Profit (\$ p.c.y.)	106.15	110.35	112.87	112.45	111.35	111.39
Total Surplus (Δ\$ p.c.y.)	0.00	+13.67	+15.66	+17.05	+14.82	+14.48
Quantity						
Coverage (\$000 p.c.y.)	110.58	110.68	110.96	111.13	110.90	110.95
First-Period Firm Market Share (%)	9.37	11.81	13.98	13.87	13.26	13.50
Renewal Firm Choice Prob (%)	13.31	15.81	17.52	17.34	16.85	17.09
Monitoring Market Share (%)	0.00	3.55	9.75	9.66	8.57	9.37
Pricing						
Baseline Factor (κ_{0s})	1.00	1.00	1.12	1.12	1.11	1.13
Initial-Period Monitoring Factor (κ_{0d})	-	0.97	0.20	0.20	0.20	0.20
Risk Surcharge Factor (κ_{1s})	-	1.00	0.50	0.50	0.50	0.40
Rent Sharing Factor (κ_{1d})	-	1.00	0.60	0.60	0.50	1.00
Competitor Pricing						
Baseline Factor (κ^c_{0s},%)	1.00	1.00	1.00	0.99	0.99	0.99
Risk Surcharge Factor (κ^c_{1s})	-	-	-	-	1.00	1.00
Rent Sharing Factor (κ^c_{1d})	-	-	-	-	0.00	1.00

Notes: Grid search steps for some pricing parameters are coarsened: 0.05 step size for the initial-period monitoring factors, and 0.1 step size for the risk surcharge and rent sharing factors. Brand effects are calibrated to \$0, \$118, and \$640 for the three firms, respectively, and per-period per-customer marginal costs are calibrated to \$84, \$25, and \$209, respectively.