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:

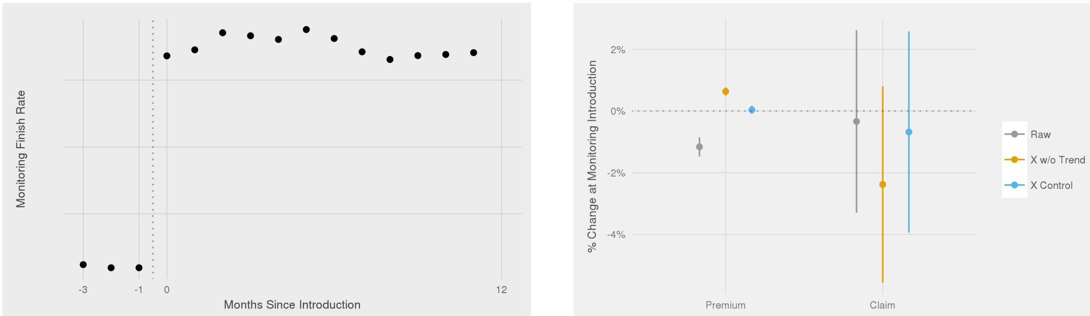
$$dep.\ var._i = \alpha + \gamma Qtr_i + \kappa \mathbf{1}_{post,i} + \theta \cdot Qtr_i \times \mathbf{1}_{post,i} + \mathbf{x}_i'\beta + \xi_{y,i} + \varepsilon_i \tag{22}$$

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

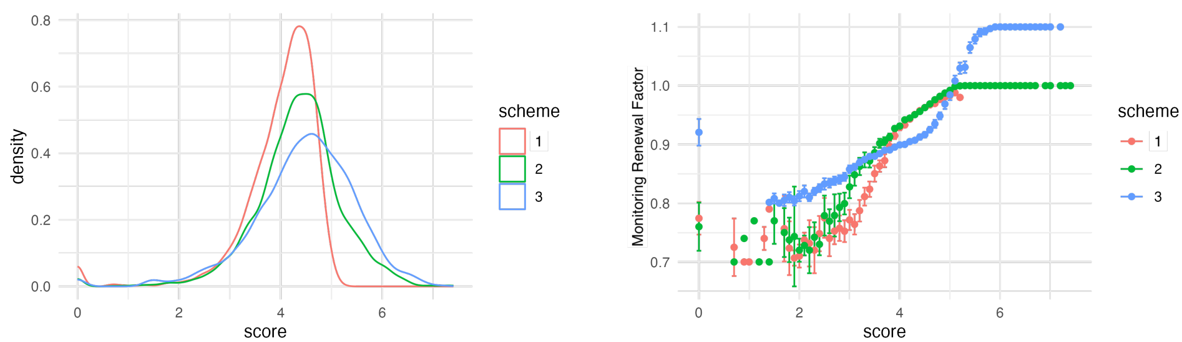


(a) (b)

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.

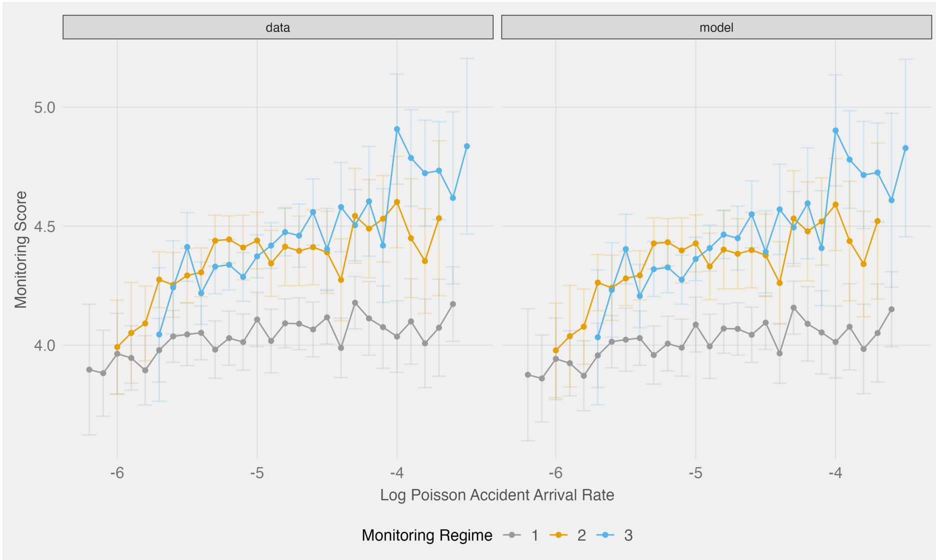


Figure B.3: Monitoring Score vs. Accident Risk by Monitoring Regimes

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.

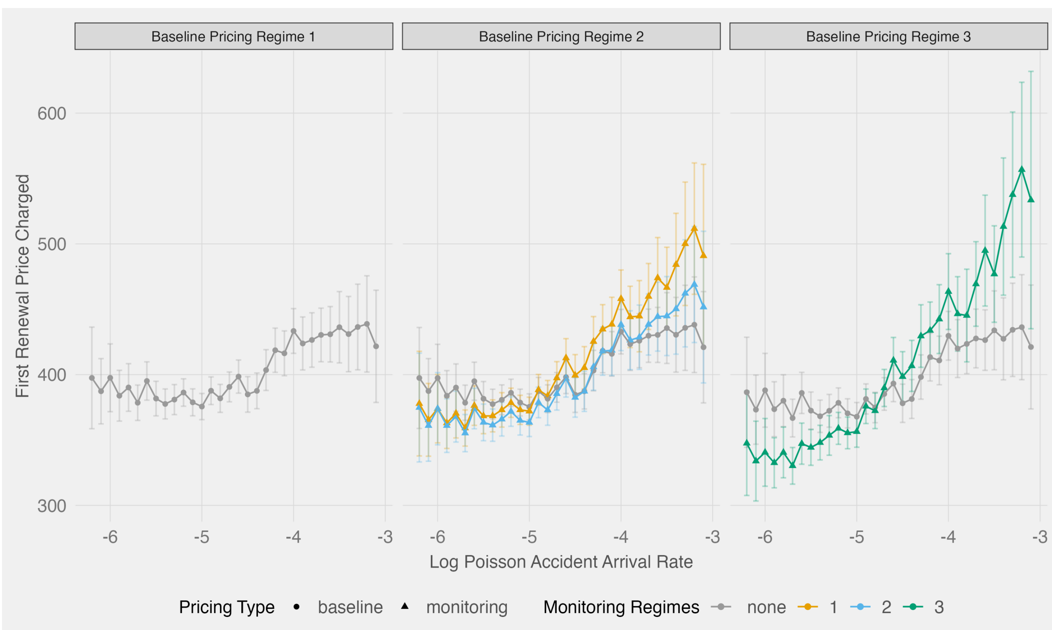


Figure B.4: Renewal Price vs. Accident Risk by Pricing and Monitoring Regimes

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.

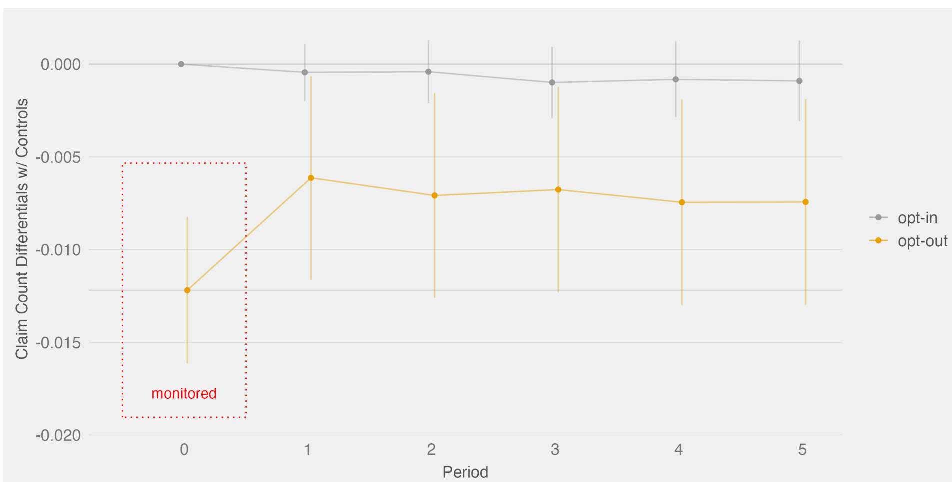


Figure C.1: Claim Progression across Monitoring Groups (Balanced Panel)

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.

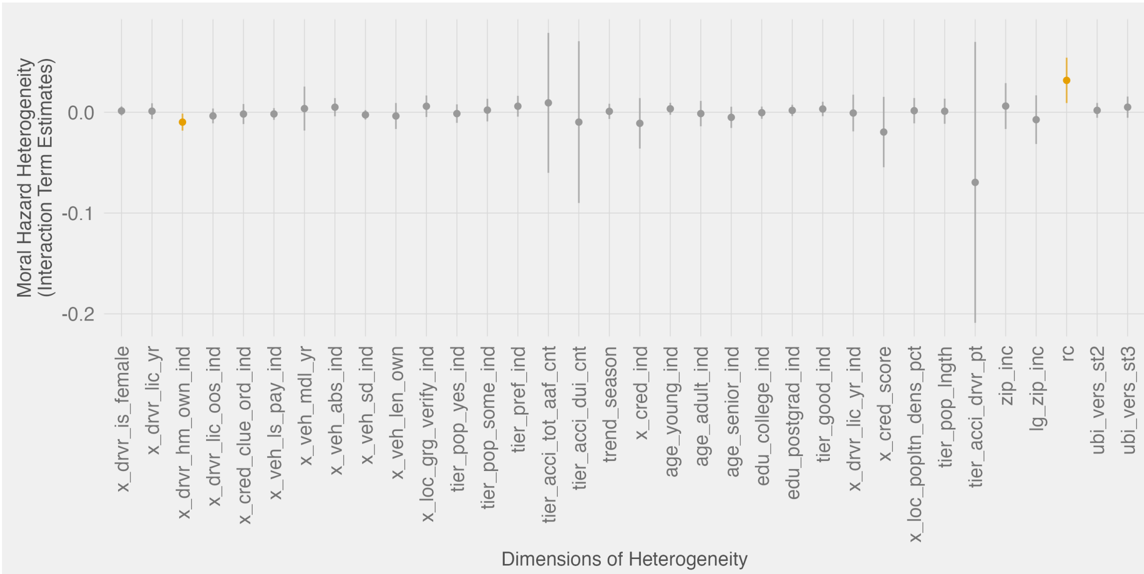


Figure C.2: Heterogeneity in Incentive Effect across Coverage Choice

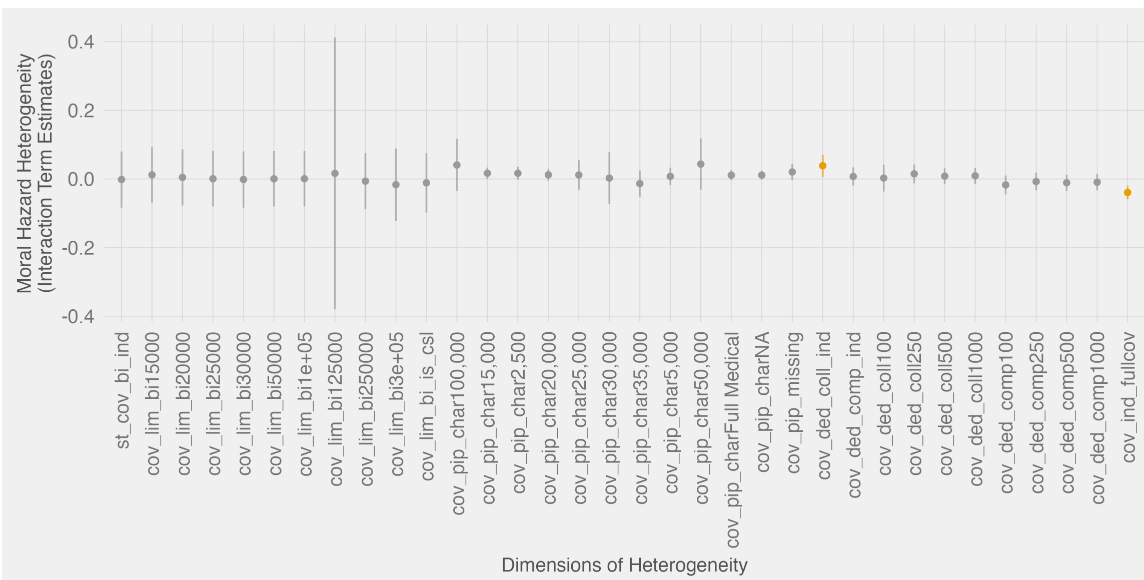


Figure C.3: Heterogeneity in Incentive Effect across Observables

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.

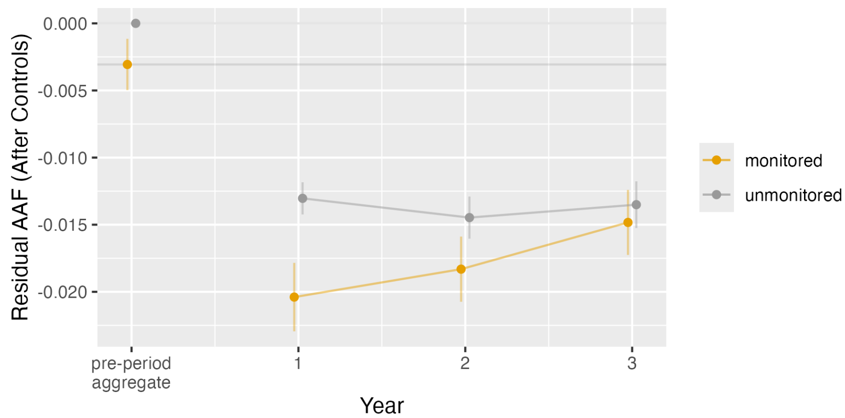


Figure C.4: At-Fault Accident Violation Progression by Monitoring Groups

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.