Online Appendix to: The Negative Consequences of Loss-Framed Performance Incentives

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A.1 Density of Dealers in DMAs

Figure A.1 shows the distribution of the number of participants in the DMAs assigned to each group. The outlier is the same control group DMA referenced in the discussion of Figure 4, containing the unusually large dealers.

Figure A.1: Distribution of DMA Dealer Count By Condition

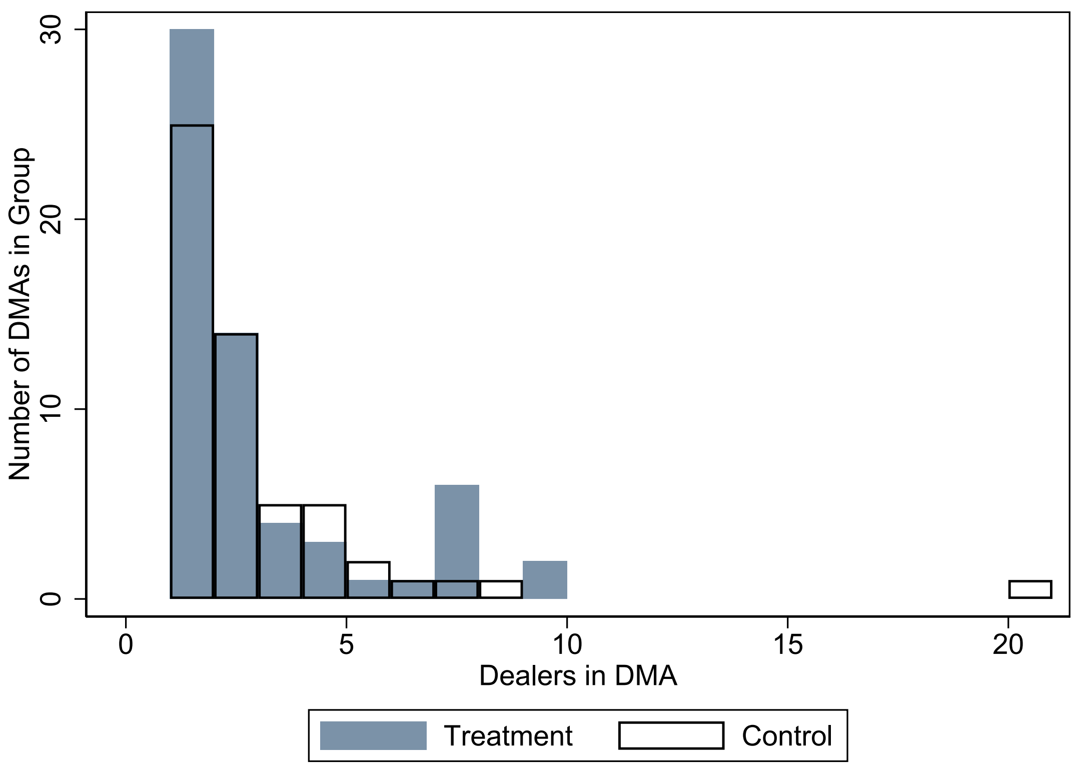
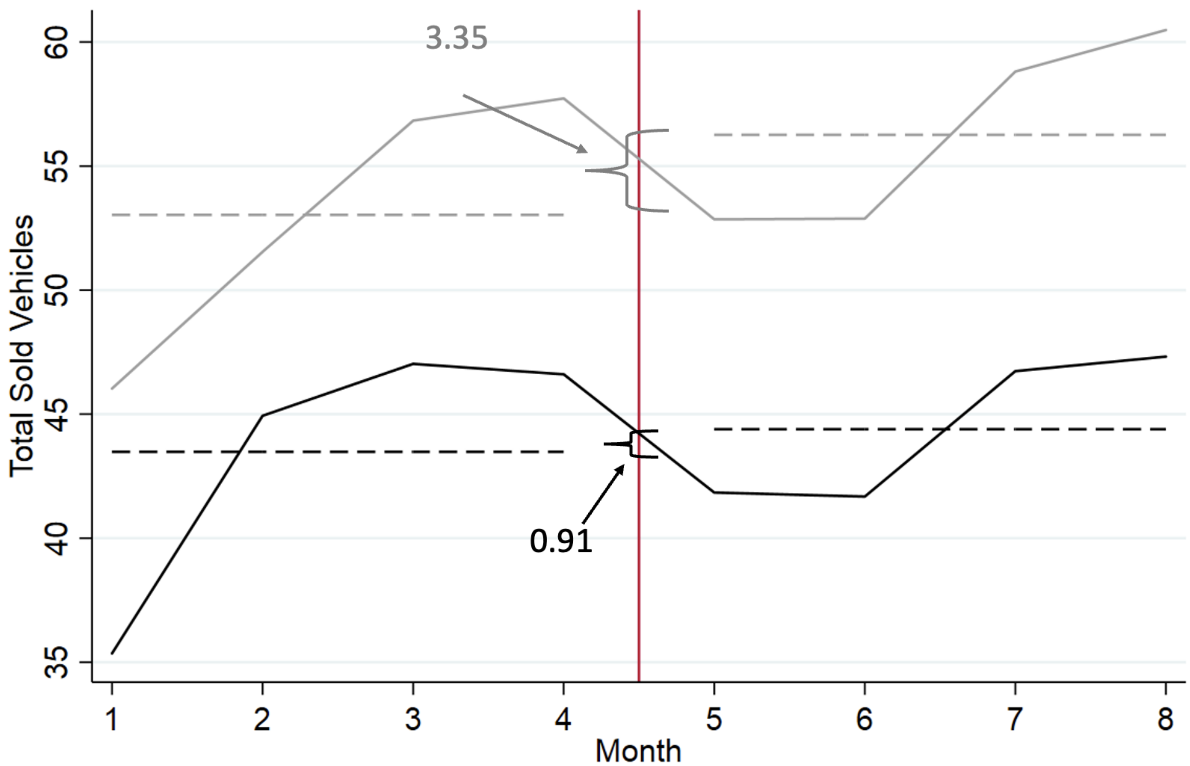


Figure A.2: Monthly Trends in Sold Vehicles for Participating Dealers



Notes: This figure shows average total dealer sales by month for dealers assigned to treatment (in black) and control (in grey). The first four months are the pre-treatment period and months 5-8 are those where the treatment group is assigned treatment. Horizontal dashed lines represent the average monthly sales across each four month period. Note that the period specific averages do not equal the sum of model-specific averages in Table 1 because some dealers only carry one model-group.

A.2 Parallel Trends Assumption

To help assess the parallel-trend assumptions that are central to difference in differences designs, Figure A.2 shows time-paths of sales over the first eight months of 2017. This window includes the four-month pre-treatment window and the four months during which the treatment group was prepaid. Several patterns are evident from this figure. First, the observable pretrends in both groups are encouragingly similar. Second, the estimated treatment effect is evident in the larger gap between groups in months 5-8. We believe this figure provides reassurance that our attempt to match pre-trends with our randomization procedure was successful, and some assurance that the treatment effects we study are clearly apparent in minimally processed data.

A.3 Assessment of Selection

In this paper, we have focused on the negative effects of loss framing *conditional on participation in our experiment*. While prior research has assumed that such effects should be positive, several papers have emphasized the potential for negative effects arising by selecting who participates (Imas, Sadoff and Samek, 2016; de Quidt, 2017). To the extent that treatment assignment was random conditional on participation, these concerns should not confound our estimated treatment effects, although they do influence our interpretation of who is averaged in average treatment effects.

For comparability to prior research and to completely examine existing accounts of the negative effects of loss framing, we now examine the predictors of selection into our experiment.

Despite the manufacturer's initial belief that this program would be highly desirable to dealers (due to providing early cash flow), comparatively few dealers opted into our experiment: 294 dealers opted in, 336 actively opted out, and 597 opted out through non-response. This low rate of opt-in could potentially be interpreted as prima facie evidence that dealers did in fact anticipate loss aversion, and thus avoided a situation that might induce it. While we do believe that this is a partial explanation, we note that several alternative explanations of the low participation rate are present. First, dealers may have failed to participate purely because they did not know of this opportunity. However, given the manner in which the opportunity was advertised, we believe this was unlikely to be a major factor.[1] Second, dealers may have feared that they would be unable to make the necessary clawback payments if targets were not met. In practice, several fail-safes ensured that dealers would face relatively few severe negative consequences if this situation arose, but lack of knowledge of those fail-safes or remaining concerns could possibly drive behavior.[2] Third, dealers may have been averse to accepting the administrative costs that a change in

[1]Multiple emails were sent to the incentive program's contact at each dealer. CarCo regional dealer representatives indicated that dealers almost certainly read at least one of the multiple invitation emails. Since the incentive program is crucial for their sales profits, program communications are high priority. In addition, CarCo's regional dealer representatives followed up with non-respondents to encourage participation. Based on these considerations, we believe effectively all non-respondents knowingly defaulted to non-participation.

[2]Based on the researcher's concerns about the negative consequences of failure to repay, CarCo agreed to automatically unenroll any dealer failing repayment. Among participating dealers, this condition was never triggered.

accounting practices would require.[3] In sum, while the low rate of participation is consistent with the concern that loss-averse agents will select out of loss framed contracts, we cannot firmly establish that this is this is the mechanism driving nonparticipation.

To examine how selection into the experiment influences the composition of our sample, we began by testing for differences between the three groups of dealers (opt-in, opt-out, and non-respondent) across the variables available in CarCo's internal data.

To begin, we emphasize that we find no statistically distinguishable differences in treatment group assignment across the three participation groups ($\chi^2 = 3.00, p = 0.223$). Treatment was assigned to 48.3% of participants, 54.0% of opt-outs, and 48.6% of non-respondents.

Among CarCo's internal data, we see evidence of selection into the program across several observable dimensions. Although participants and opt-out dealers had statistically indistinguishable monthly sales volume (48.5 vs. 43.2, $p = 0.162$), non-respondents had substantially fewer sales than both (31.1, $p < 0.001$).[4] Second, and consistent with these volume differences, there are significant cross-group differences in whether or not the dealers carry both model groups ($\chi^2 = 10.93, p = 0.004$). Non-respondents (86%) are less likely to do so than both participants (90%) and opt-out dealers (93%). Third, we find substantial differences across regions in participation rates, and particularly in the rate of opt-outs through non-responses.

These differences reject the hypothesis that dealers' willingness to be exposed to loss-framing through participation in our experiment is completely random conditional on observables. While the random assignment of treatment preserves the validity of estimated treatment effects within this group, we sought to collect more dealer-level data to help us better understand the features of the dealers who selected into our study.

We generate our dealer-level data by combining CarCo's sales and incentive records with supplemental data from the National Establishment Time Series (NETS). NETS aggregates data on most establishments in the United States (Barnatchez, Crane and Decker, 2017), including car dealers, providing data on ownership structure, employment, and financial

[3] Regional dealer representatives informally indicated that many of the dealers claimed that their lack of participation was due to avoidance of what they viewed as an accounting "headache," combined with little need for the early cash flow.

[4] These averages do not equal the sum of model-group average sales in Table 1 because some dealers carry only one model group.

Table A.1: Group Means by Participation Status for Dealers with NETS Data

	Selection Group			Group Difference Test
	Participants	Opt-Out	Non-Respondents	
Year Founded	1976.49	1976.67	1974.25	$F = 0.98$
	(27.16)	(27.76)	(27.39)	[0.38]
Employees	55.13	50.86	51.98	$F = 0.73$
	(46.70)	(45.30)	(40.27)	[0.48]
2015 DUNS Score	2.47	2.40	2.41	$F = 0.85$
	(0.60)	(0.52)	(0.56)	[0.43]
Has DUNS Score	0.70	0.62	0.66	$\chi^2 = 3.50$
	(0.46)	(0.49)	(0.47)	[0.17]
2015 Min Paydex	73.16	74.22	73.17	$F = 1.31$
	(8.73)	(6.34)	(9.59)	[0.27]
Has Paydex Score	0.81	0.75	0.80	$\chi^2 = 3.37$
	(0.39)	(0.43)	(0.40)	[0.19]
Publicly-Held	0.00	0.02	0.02	$\chi^2 = 3.78$
	(0.06)	(0.14)	(0.15)	[0.15]
Part of Group	0.18	0.23	0.26	$\chi^2 = 5.79$
	(0.39)	(0.42)	(0.44)	[0.06]

Notes: Summary statistics are presented for three groups: those who chose participation, those who opted-out, and those who did not respond and therefore were non-participants by default. Means with standard deviations in parentheses for each group. F-statistics from ANOVA are presented for continuous variables. Chi-squared tests are for dichotomous variables. P-values are in brackets. Each line only includes those dealers with a populated NETS field.

strength data from Dun & Bradstreet.[5] We successfully matched 877 non-participants and 276 participants via phone, address, name, and ownership data. We then examined differences in sales, ownership, age, employment, and financial health.[6]

Table A.1 presents group means and statistical tests of differences across the three groups for the 1,153 dealers with NETS data. We see few observable differences between the three groups. In particular, no statistical differences are detected in the age or size of the firm, in the Dun & Bradstreet measures of financial health, in the Paydex measures of reliable bill payment, or in publicly-held status. One potential difference is seen in the "Part of Group" variable. Participants are more likely to be stand-alone dealers (18%) than opt-out (23%) or non-respondents (26%). These differences could arise if dealers that were part of larger organizations were less likely to have autonomy to change accounting systems or rules.

In summary, we see relatively modest evidence of observable selection into participation in our experiment. We acknowledge that we cannot rule out perhaps interesting unobservable differences in dealers by participation status, but we note these would be very unlikely to bias our treatment estimates given our random assignment procedure.

A.4 Robustness to Outliers

As discussed in Section 1.4, notable differences exist in the average pre-intervention sales between the treatment and control groups. As we argued in our discussion of Figure 4, this difference can be attributed to the random assignment of an outlier DMA. A single DMA contained five of the six largest participating dealers, each with average monthly sales several multiples of the average of other dealers. Due to the natural concern that these influential outliers meaningfully drive results, we exclude this DMA as well as its matched partner, and present summary statistics in Table A.2 and difference in differences models in Table A.3. With its exclusion, average sales across both model groups in the pre-period are statistically indistinguishable ($p = 0.451$), with means of 43.5 and 45.5 for the treatment and control group, respectively. We note that these numbers are not simply the sum of the preperiod

[5] CarCo did not share internal data on dealership financial or ownership structure with us for legal reasons.

[6] The NETS data fields have varying levels of completeness. Employment data, for example, is complete for approximately 90% of the matched dealers. Dun & Bradstreet data, however, is complete for only about two-thirds.

Table A.2: Summary Statistics Excluding Outlier DMA and its Match

	LARGE-BONUS MODEL GROUP					
	Treatment Group			Control Group		
	Pre	Post	Total	Pre	Post	Total
Vehicles	29.59	31.56	30.58	34.42	36.49	35.46
Sold	(21.58)	(23.85)	(22.75)	(45.76)	(46.69)	(46.22)
Target	27.07	32.25	29.66	28.80	35.37	32.09
Sales	(19.48)	(22.17)	(21.02)	(30.73)	(38.66)	(35.07)
Hit 110%	0.56	0.44	0.50	0.63	0.49	0.56
Target	(0.50)	(0.50)	(0.50)	(0.48)	(0.50)	(0.50)
Hit 100%	0.61	0.49	0.55	0.68	0.54	0.61
Target	(0.49)	(0.50)	(0.50)	(0.47)	(0.50)	(0.49)
	SMALL-BONUS MODEL GROUP					
	Treatment Group			Control Group		
	Pre	Post	Total	Pre	Post	Total
Vehicles	15.14	14.16	14.65	14.29	14.71	14.50
Sold	(14.07)	(13.47)	(13.78)	(13.63)	(14.79)	(14.22)
Target	12.39	14.69	13.54	12.27	14.71	13.49
Sales	(9.91)	(11.92)	(11.02)	(10.79)	(12.91)	(11.96)
Hit 110%	0.64	0.44	0.54	0.62	0.46	0.54
Target	(0.48)	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)
Hit 100%	0.70	0.50	0.60	0.69	0.51	0.60
Target	(0.46)	(0.50)	(0.49)	(0.46)	(0.50)	(0.49)

Notes: Summary statistics of monthly sales performance by model group and treatment excluding the largest DMA in the control group. Means and standard deviations presented in all cells. The first two rows present monthly sales and monthly target thresholds. The rows below summarize the probability of hitting earning the larger fixed bonus for exceeding the 110% threshold, the smaller fixed bonus for exceeding the standard target threshold.

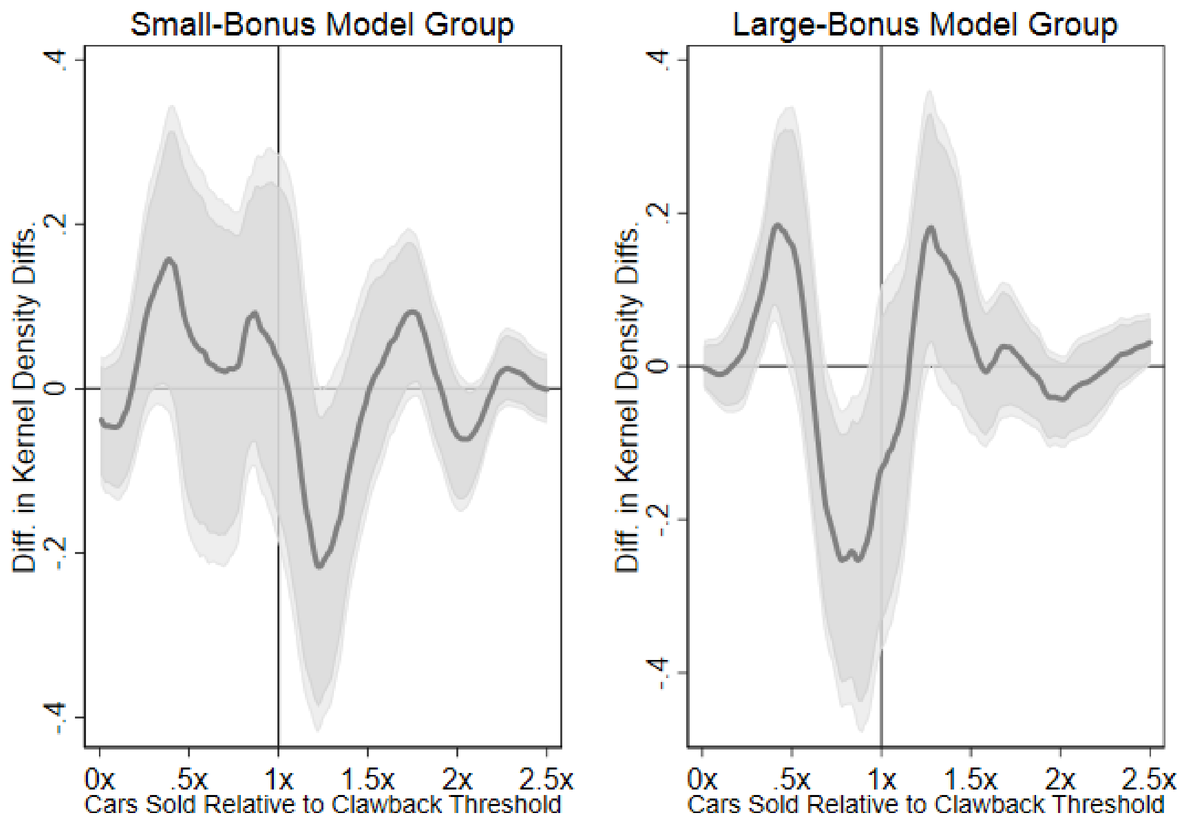
Table A.3: Difference-in-Difference Estimates Excluding the Outlier DMA and its Match

	(1) OLS	(2) PPML	(3) OLS	(4) PPML	(5) OLS	(6) PPML
$\text{Treated}_d \times \text{Treatment period}_m$	-1.57	-0.032	-0.16	0.004	-1.44	-0.098
	(1.14)	(0.023)	(0.88)	(0.023)	(0.73)	(0.045)
Model Group	Pooled	Pooled	L-B	L-B	S-B	S-B
Dealer Fixed Effects	X	X	X	X	X	X
Month Fixed Effects	X	X	X	X	X	X
Observations	2280	2280	2189	2189	2144	2144

Notes: This table presents regressions predicting the dealer/month-specific number of vehicles sold. Odd numbered columns present OLS estimates of equation 1. Even numbered columns present Poisson Pseudo-Maximum Likelihood (PPML) estimates of equation 2. We first present analysis pooling the two model groups together (cols 1 and 2), followed by analysis of large-bonus model group (cols 3 and 4) and small-bonus model group (cols 5 and 6). Standard errors are clustered at the DMA level.

in Table A.2 because not all dealerships carry both model groups. Regression results are presented in Table A.3. Average monthly sales decrease by 1.57 vehicles per month or 3.2% with nearly all losses coming from the small-bonus model group. With the outlier DMA excluded, the magnitude and significance of our diff-in-diff results are somewhat reduced, but the same general patterns remain. DKDD results, presented in Figure A.3, remain largely unchanged.

Figure A.3: The Impact of Loss Framing on the Distribution of Sales While Excluding the Outlier DMA and its Match



Notes: This figure shows difference-in-kernel-density-differences estimates of the impact of loss framing on the distribution of sales achieved. The x-axes show the amount of sales relative to clawback threshold. The two subfigures present estimates derived from each model group. Confidence regions, based on 10,000 bootstrap iterations resampled by DMA, are shaded. The 90% confidence region is shaded darkly and the 95% confidence region is shaded lightly. Kernel: Epanechnikov; Bandwidth: .1x; Sample Sizes: 2,108 (left panel) and 2,166 (right panel).

A.5 Robustness to the Exclusion of Four Flipped DMAs

As explained in the main text, two matched pairs of DMAs were reassigned by CarCo in order to avoid neighboring DMAs with cross-DMA competition having different treatment assignment. In Table A.4 we repeat our difference-in-difference models with similar but

Table A.4: Difference-in-Difference Estimates Excluding Four Flipped DMAs

	(1) OLS	(2) PPML	(3) OLS	(4) PPML	(5) OLS	(6) PPML
$\text{Treated}_d \times \text{Treatment period}_m$	-1.47	-0.030	-0.08	0.007	-1.41	-0.096
	(1.14)	(0.023)	(0.89)	(0.024)	(0.74)	(0.046)
Model Group	Pooled	Pooled	L-B	L-B	S-B	S-B
Dealer Fixed Effects	X	X	X	X	X	X
Month Fixed Effects	X	X	X	X	X	X
Observations	2256	2256	2165	2165	2120	2120

Notes: This table presents regressions predicting the dealer/month-specific number of vehicles sold. Odd numbered columns present OLS estimates of equation 1. Even numbered columns present Poisson Pseudo-Maximum Likelihood (PPML) estimates of equation 2. We first present analysis pooling the two model groups together (cols 1 and 2), followed by analysis of large-bonus model group (cols 3 and 4) and small-bonus model group (cols 5 and 6). Standard errors are clustered at the DMA level.

smaller effect sizes. DKDD results, presented in Figure A.4, remain largely unchanged.

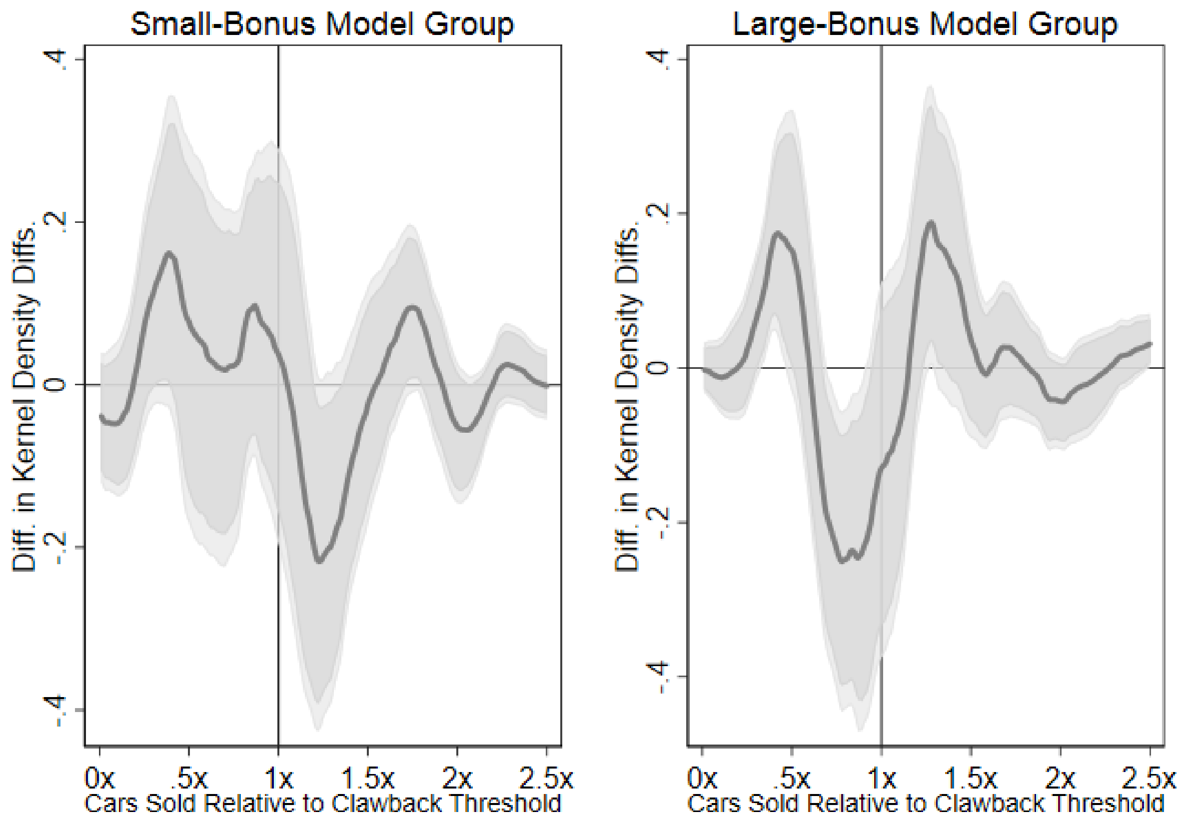
A.6 Treatment Effect in Second Treatment Window

As described in Section 1.2, an "equal treatment" requirement led us to design our intervention so that all experimental participants faced four months in treatment condition and four months in the control condition. We have focused our attention on the estimated impact of treatment in the first four-month treatment window, prior to conditions being flipped. Conceptually, we believe that analysis of treatment effects in the first window have the cleanest interpretation. When examining the second window, the prior treatment of the control group is expected to affect results, leading to a confounding of a difference-in-differences design. For completeness, we attempt to estimate treatment effects in this window using synthetic control methods (Abadie, Diamond and Hainmueller, 2010; Robbins, Saunders and Kilmer, 2017).

The aim of our synthetic control analysis is to compare the sales among dealers receiving treatment in the second window to that of comparable dealers who did not participate in the experiment.[7] We constructed the synthetic control group by matching on dealer

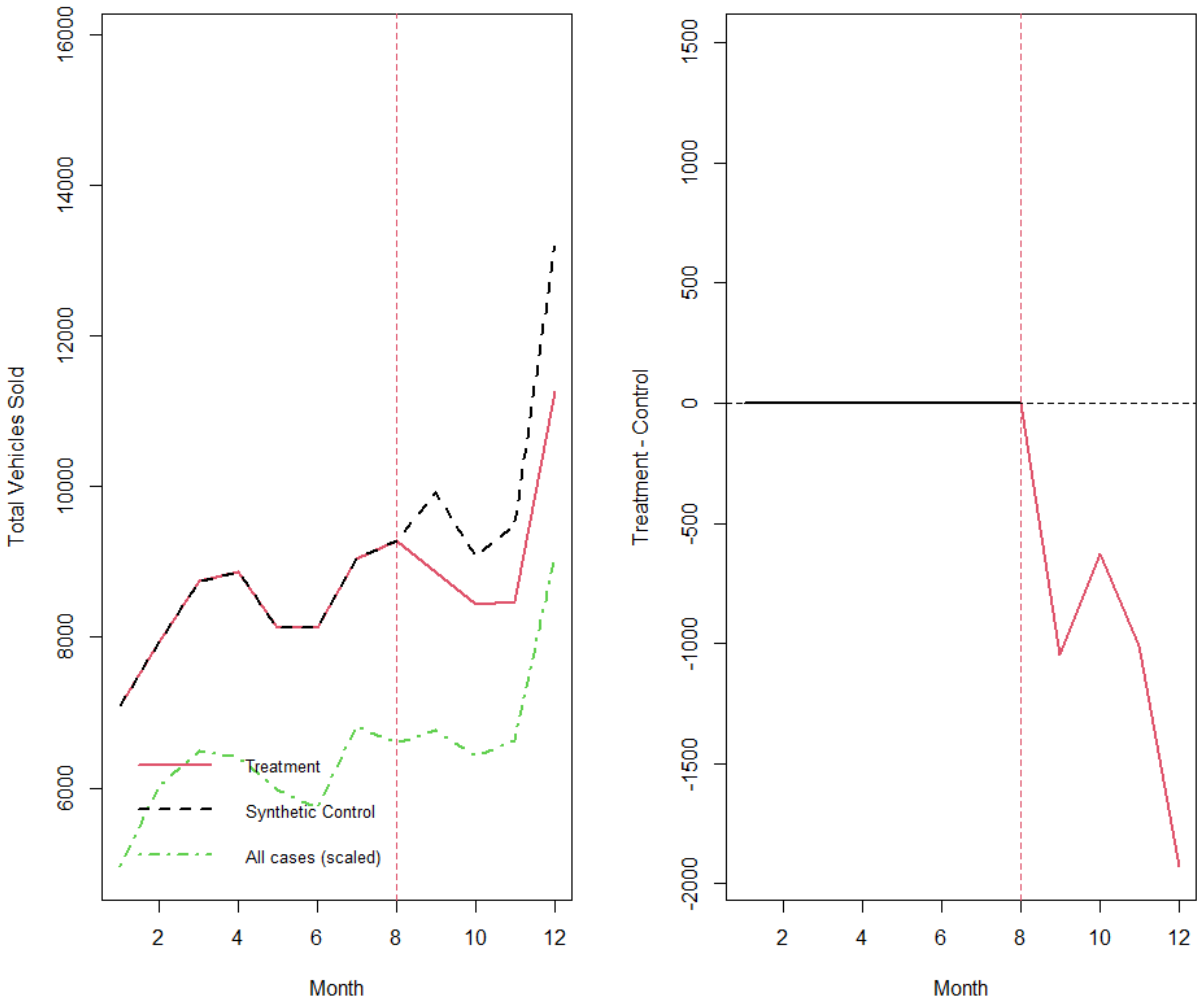
[7] Because random assignment occurred before participation decisions, we specifically restrict attention to non-participants who were assigned to control.

Figure A.4: The Impact of Loss Framing on the Distribution of Sales While Excluding the Four Flipped DMAs



Notes: This figure shows difference-in-kernel-density-differences estimates of the impact of loss framing on the distribution of sales achieved. The x-axes show the amount of sales relative to clawback threshold. The two subfigures present estimates derived from each model group. Confidence regions, based on 10,000 bootstrap iterations resampled by DMA, are shaded. The 90% confidence region is shaded darkly and the 95% confidence region is shaded lightly. Kernel: Epanechnikov; Bandwidth: .1x; Sample Sizes: 2,085 (left panel) and 2,142 (right panel).

Figure A.5: Synthetic Control Estimates of Treatment Effect in Second Treatment Window



Notes: This figure shows results from a synthetic control model, estimating the impact of the control group receiving the loss-framing treatment in months 9–12. The vertical axes represent the total monthly sales across the treatment and synthetic control groups. The synthetic control group is built from non-participants in the treatment group and matches on dealer size (in units sold), dual-dealer status, and pre-treatment sales trends. The model estimates a treatment of -11%.

size (in units sold), dual-dealer status, and the pre-trends in sales that were used in our algorithm for random assignment. The estimated model, represented in Figure A.5, shows a treatment effect of loss framing of -11% ($p = 0.015$)[8]—substantially larger than our estimate

[8] All estimates are derived using the R package microsynth, with p-values generated through the permutation method with 10,000 permutations applied.

of -3.8% in our primary analysis. Though we believe our difference-in-differences estimates are conceptually preferable to these synthetic control estimates (as they do not require comparisons to a group that has endogenously opted out of treatment), we note that both methods provide evidence of a negative effect of loss framing.

A.7 Interview and Survey Evidence

In this paper, we have focused on the observed reaction to the treatment using objective sales data. As a complement to this evidence, we conducted informal interviews and formal surveying of participants after the experiment. Fourteen dealers were interviewed, and sixty-nine dealers (23%) responded to the online survey. Overall, the interviews and surveys suggested reasonably universal perceived importance of the incentive program, but more heterogeneous reactions to the loss framing treatment. Some dealers indicated perceptions that were clearly in line with loss-averse evaluation, while others provided assessments of the program more in line with neoclassical considerations. The interviews also indicated both awareness of, and efforts directed towards, the need to optimize efforts across the two model groups, consistent with the exacerbation of multitasking issues that we have discussed.

A.7.1 Interview Evidence

The interviews were conducted over the phone in May, 2018, with either the general manager or principal (owner) of 14 participating dealers. Dealer interviews were arranged by CarCo with a mix of targeted regions and experimental conditions. Participation was not random nor representative of the dealers as a whole. The interviews, which lasted 20 to 30 minutes, were relatively unstructured, but focused on five key questions: 1. “How do the [program] targets influence formal policies or managerial attention in a given month for both you or other dealers?” 2. “How do you think about the two separate brands and respective targets, both before and after achieving the targets?” 3. “When you received the prepayments, did this affect any of your policies or attention, and how was the cash used?” 4. “How do you imagine other dealers might deal with these prepayments, and do you see them as generating potential benefits or problems for these other dealers?” 5. “What do you see as the immediate strengths and weaknesses of the current setup, where 110% has such a large immediate payoff?"

The interviews revealed relatively consistent views on the role of the incentive program in dealer strategy and policy. Respondents universally emphasized that the incentive program represents the majority of their sales profits. One dealer noted that "you gotta get there. If you don't hit the 110 number, your operating profit is next to nothing." Another stated that "it's critical to reach those plateaus for achieving profitability." They also consistently noted how the program dominates their sales strategy. "It's an integral part of how we shape and process just about everything." Another dealer explained that "(the incentive plan) is running our business. It's the most important thing. Financially, it's crucial—55-65% of operating profit." Several dealers also noted that hitting the objectives early changed their behavior, saying "it allows us to start focusing on margin," while also noting that it makes them "relaxed." Dealers also explained that the incentive program is tightly tied to the incentives of the sales managers and salespeople.

Many indicated that they carefully track progression toward the targets throughout the month, adjusting resources and pricing based on the likelihood of hitting the target. One dealer reported that "if we don't hit a fast start, then we back off. If we're close, we use incentives to get there." This resource and attention allocation also applied across brands. A dealer noted that "if we're cruising on (large-bonus model group), we'll put attention to (small-bonus model group). If we get to the 20th of the month, we'll focus on the one closest to the 110% target." One dealer noted that "we think about (the brands) entirely separately, until we're close. Then I'll move people." Another explained that "mid-month, when it looks likely to hit one more than the other, we allocate resources accordingly." Another dealer noted the difference between the brands. "We make a lot more money off (large-bonus model group) so we focus there. We're not going to take a (large-bonus model group) buyer and put them in (small-bonus model group)."

Although dealers were reluctant to discuss attempts to move customers across months and brands, several indicated that they had some ability to do so, and many more explained that they "know there are dealers who play that game," or opaquely explained "if we're having a slow month, we think about next month." Some dealers also indicated the importance of having smooth earnings across months. "You can't have peaks and valleys in your sales year. You need to be consistent."

The interviewed dealers indicated a mix of responses to the prepayment condition. One mentioned that "the reason I signed up was for an interest-free loan," with some other dealers expressing similar sentiments. Another mentioned that "this is simply cash up front, and we put it in a separate account," explaining that "my sisters are very accounting oriented." Others, however noted the motivational effect it had on their business. One dealer noted that "nobody wanted to give it back." Another explained that "it's like giving my son $100—it's going to be spent. Your back's in the corner so you've got to produce." Another also mentioned that "nobody wanted to give it back," referring to it as "a little extra spice in the stew," and explaining that he "didn't want to have that conversation with the owner." One successful dealer explained that the prepayment did not impact their behavior during the experiment because "I knew I would hit my goals. I would be hesitant to do it this year, because I don't want it yanked back from me." Another stated that it "always felt like there was a lot more pressure. It changed the intensity." Two dealers also noted that the prepayment did help with cash flow.

A.7.2 Post-Experiment Survey

The post-experiment survey was conducted in July, 2018, and was distributed to the primary contacts at all 294 participating dealers. The survey asked about the use of the advanced funds as well as the respondent's perception about how the advanced funds changed behavior throughout the month. Follow-up emails were used to attempt to increase participation, resulting in sixty-nine dealers (23%) completing the survey. Responses from the survey are presented in Tables A.5 and A.6.

Table A.5: Questions and Responses From the Post-Experiment Survey

1. Which of the following employees have financial incentives tied to SFE bonuses? (SELECT ALL THAT APPLY)

Total Responses	General Manager	Sales Manager	Sales Consultant	Service Technicians	Administrative Staff	Other
69	47	59	23	5	19	12
100.0%	68.1%	85.5%	33.3%	7.2%	27.5%	17.4%

2. Did any of these employees receive advanced funds when participating in the pilot program (i.e., received bonuses or incentives at the beginning of the month)? (SELECT ALL THAT APPLY)

Total Responses	General Manager	Sales Manager	Sales Consultant	Service Technicians	Administrative Staff	Other
69	6	5	1	1	0	10
100.0%	8.7%	7.2%	1.4%	1.4%	0.0%	14.5%

3. How did you immediately use the advanced funds when you received them at the first of each month? (SELECT ALL THAT APPLY)

Total Responses	Marketing/ Advertising	Customer Incentives	Advanced Employee Comp.	Pay Down Inventory	Recorded as Income	Set Aside Until End of Month	Other
68	7	8	2	1	7	49	6
100.0%	10.3%	11.8%	2.9%	1.5%	10.3%	72.1%	8.8%

4. How did the advanced funds influence the external pressure you felt to meet SFE targets?

Total Responses	Strongly decreased 1	2	3	No change 4	5	6	Strongly increased 7
69	3	1	5	37	2	7	14
100.0%	4.3%	1.4%	7.2%	53.6%	2.9%	10.1%	20.3%

5. How did the advanced funds influence the internal motivation you felt to meet SFE targets?

Total Responses	Strongly decreased 1	2	3	No change 4	5	6	Strongly increased 7
69	1	1	5	36	4	4	18
100.0%	1.4%	1.4%	7.2%	52.2%	5.8%	5.8%	26.1%

6. Compared to your usual management approach, how did the advanced funds change the way you approached the sales process to reach your monthly target?

	Less than before 1	2	3	No change 4	5	6	Much more than before 7
a. Closely tracked progress toward monthly SFE targets	0 0.0%	1 1.4%	4 5.8%	47 68.1%	4 5.8%	5 7.2%	8 11.6%
b. Emphasized meeting SFE targets with my salesforce	0 0.0%	1 1.4%	3 4.3%	46 66.7%	3 4.3%	4 5.8%	12 17.4%
c. Encouraged aggressive pricing to meet SFE targets	0 0.0%	1 1.4%	4 5.8%	45 65.2%	3 4.3%	4 5.8%	12 17.4%

Table A.6: Questions and Responses From the Post-Experiment Survey (Continued)

7. How did the Advanced Funds Pilot Program change your focus on either [small-bonus model group] or [large-bonus model group] before reaching either of the 110% targets?

Total Responses	No change	Increased focus on small-bonus group	Increased focus on large-bonus group	Increased focus on closest group to targets	Other
69	48	3	2	11	5
100.0%	69.6%	4.3%	2.9%	15.9%	7.2%

8. How did the Advanced Funds Pilot Program change your focus on either [small-bonus model group] or [large-bonus model group] after reaching both of the 110% targets?

Total Responses	No change	Increased focus on small-bonus group	Increased focus on large-bonus group	Other
69	54	5	4	6
100.0%	78.3%	7.2%	5.8%	8.7%

9. Do you believe the advanced funds increased or decreased your group's overall sales?

Total Responses	Strongly decreased sales 1	2	3	No change 4	5	6	Strongly increased sales 7
69	0	0	5	44	4	5	11
100.0%	0.0%	0.0%	7.2%	63.8%	5.8%	7.2%	15.9%

10. Given your experience with the Advanced Funds Pilot, what would be your preferred payment timing for SFE moving forward:

Total Responses	Strongly prefer end of month 1	2	3	Indifferent 4	5	6	Strongly prefer advanced funds 7
68	24	1	3	10	4	4	22
100.0%	34.8%	1.4%	4.3%	14.5%	5.8%	5.8%	31.9%

11. How would you describe your cash flow constraints in a given month?

Total Responses	No constraints 1	2	3	4	5	6	Very high constraints 1
68	21	10	9	11	3	2	10
100.0%	30.4%	14.5%	13.0%	15.9%	4.3%	2.9%	14.5%

Only 23 dealers reported distributing advanced funds to employees, with this distribution typically going to a general manager or sales manager. The majority of respondents (49) reported setting the funds aside until the end of the month. Twenty-three dealers reported that they agreed that the advanced funds increased pressure to meet program goals, while 26 reported increased motivation. Nineteen dealers also reported that they increased their emphasis on meeting program targets under the prepayment condition, while 17 indicated that the prepayment condition made them more closely track progress toward the monthly targets.

Sixteen dealers reported that the prepayment condition changed their approach to the two brands, with 11 reporting that it increased their focus on whichever brand was closest to the target. Twenty dealers believed that the prepayment system increased their sales, while five believed it slightly decreased them. Finally, 30 dealers would prefer advanced funds moving forward, while 28 would prefer end-of-the-month and 10 were indifferent.

Collectively, the interviews and survey indicate that for a select set of dealers, the prepayment condition changed behavior and mind-frame in many dealers while having little effect in others. This is consistent with there being a variety of management practices across car dealers resulting from both franchise law protections and other sources of heterogeneity. We draw several important conclusions from these self-reported data. First, the prepayment condition was salient enough to successfully treat some dealers with loss-framing. Second, this loss-framing was not universal, which deflates any average treatment effects estimated from the study. Third, the responses of some treated dealers align closely with the gaming behaviors we discuss in our theoretical treatment of loss framing.

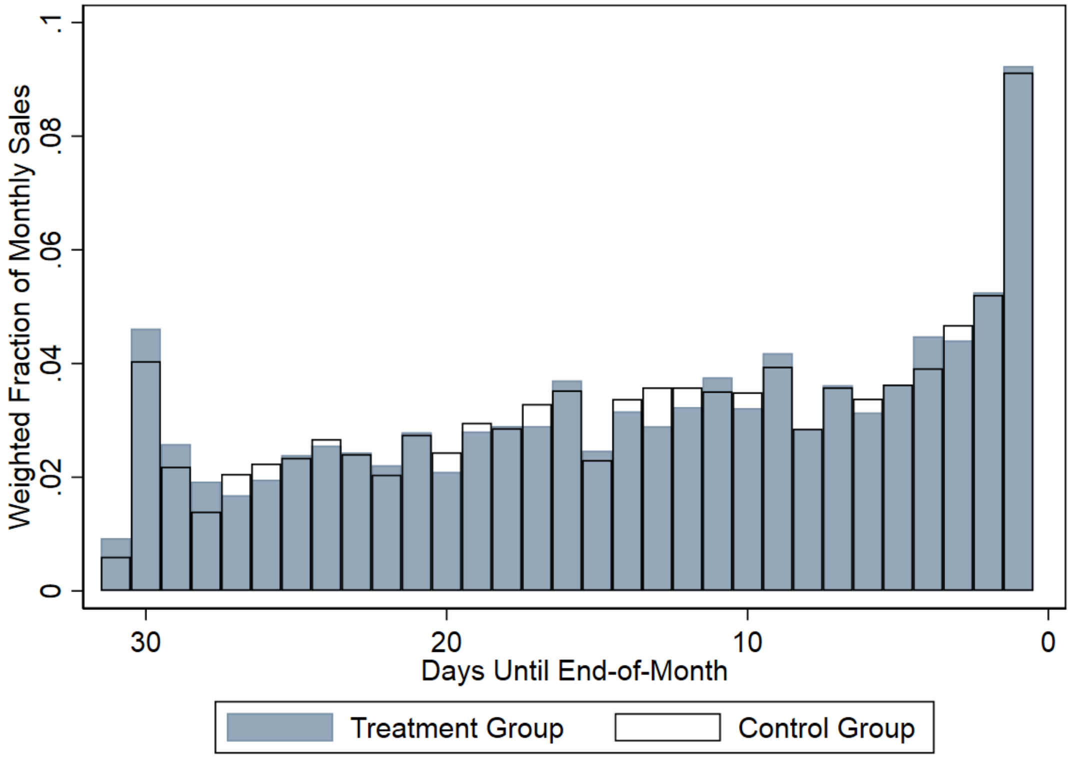
A.8 Timing of Sales

Another commonly considered “gaming” behavior among car dealers is attempting to shift sales not across model groups, but across time periods of evaluation. Ultimately, we are unpowered to provide interesting tests of this channel for potential gaming in our experiment, but we present some attempts to examine this here.

Figure A.6 shows the distribution of sales by day-of-the-month in each group during the pre-period, demonstrating some daily responsivity to the intertemporal incentives induced by these contracts.[9] As previously noted in, e.g., Larkin (2014), high-powered incentives measured over discrete time windows can result in high effort to close sales at the end of the window of measurement. Consistent with this consideration, we see that sales are particularly concentrated at the end of the month. Also consistent with Larkin (2014), we see increased sales at the beginning of the month, likely from when dealers delayed closing deals at the end of a month where the target was unreachable. As with our analysis of within-month responsivity to direct incentives, there is no statistically detectable difference in means between treatment and control groups (T-test: $p = 0.692$). There is a small distributional difference that is statistically distinguishable because of the large number $(57,005)$ of daily

[9]Each day-of-the-month is inverse-weighted by the number of days during the four-month period when dealers were open. Nearly all dealers were closed January 1-3 and on Sundays.

Figure A.6: Pre-treatment Daily Sales Timing by Treatment Assignment

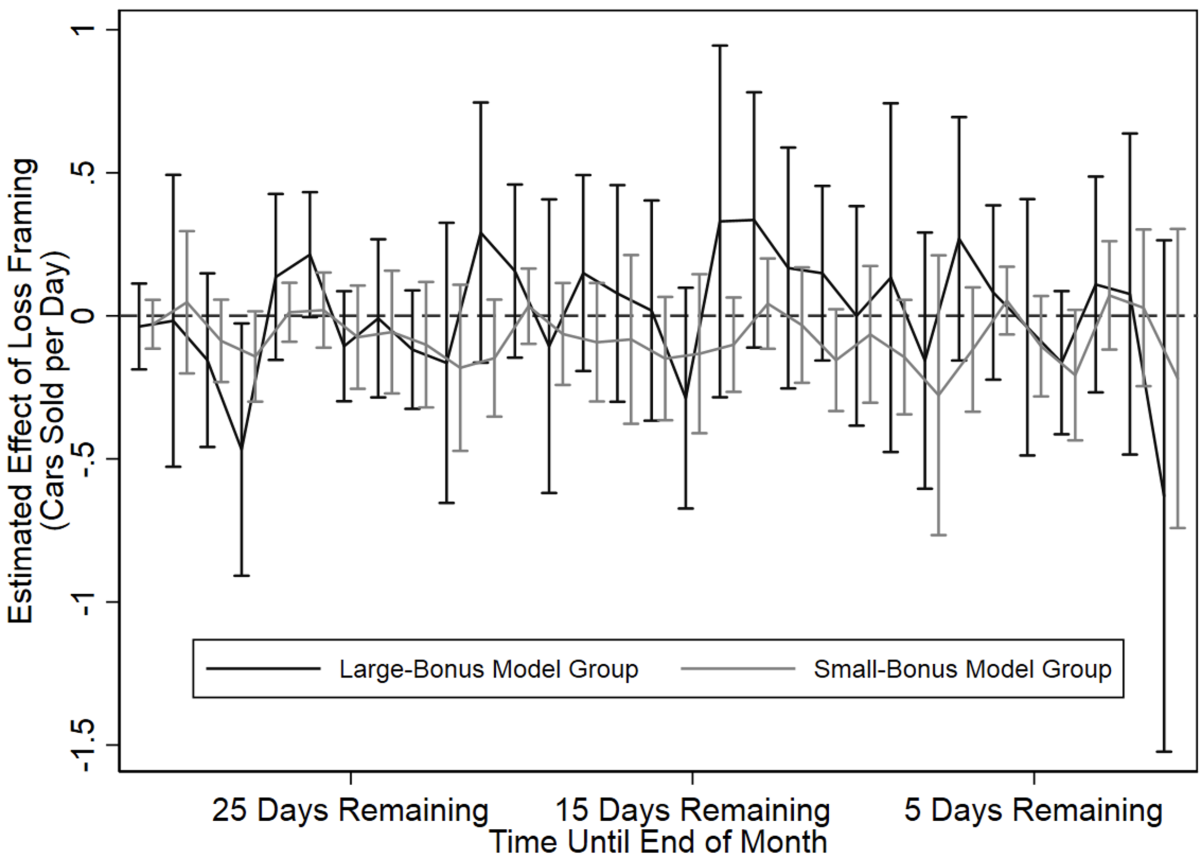


Notes: This figure shows the percentage of monthly sales occurring on each day of the month in the four months prior to the experiment. We correct for days when dealers are closed (Sundays and the New Year's holiday) by inverse-weighting sales by the number of open days during the four month period.

observations (Kolmogorov-Smirnov: $p = 0.033$).

To test for the possibility that loss framing influenced the incentives to sell cars on particular days (e.g., at the beginning or end of the month), we re-estimated regressions (3) and (5) from Table 2, restricting the data to each possible number of days until the end of the month. Results are presented in Figure A.7. As seen in the figure, we do not find evidence that treatment effects operate at a specific time of the month; however, given that the average dealer sells fewer than one car of either brand per day, on average, the size of the confidence intervals in this figure illustrate that we are underpowered to rule out substantial intertemporal effects.

Figure A.7: Day-Specific Estimated Treatment Effects on Cars Sold



Notes: This figure shows the estimated treatment effect from regressions (3) and (5) in Table 2, with the sample restricted to include only the day of the month indicated on the X-axis.