Online Appendices

Scarcity Strategy in Crowdfunding: An Empirical Exploration of Reward Limits

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Appendix A. Validation of Reward Limits Classification

We first discuss the validation procedure regarding the classification of reward limits into three forms – *limited edition*, *price discount* and *early access*. Following prior research using computer-based approaches (e.g., Huang et al. 2017), it is necessary to verify classification accuracy through human evaluation. To validate our keyword-based approach, we recruited two coders (two PhD students in the Information Systems field) who are familiar with crowdfunding to manually label a sample of limited rewards and compare their labels with the outputs of the computer-based classification. Specifically, we randomly sampled 1,500 limited rewards from our data and the coders were asked to label whether the limited rewards belong to each form based on their descriptions. Each coder was independently assigned a total of 1,000 limited rewards with 500 limited rewards overlapping between the two coders to allow for assessment of inter-coder reliability.

For validation purposes, we check both inter-coder reliability (agreement between coders) and the keyword-coder accuracy (i.e., agreement between the keyword-based approach and the human coder). We calculate two measures – agreement rate and Cohen's Kappa (Cohen 1960) – to validate both coding and classification. Table A1 presents the two measures in different samples and scenarios. The results suggest that the coding process is valid given the high inter-rater reliability, and our keyword-based classification approach is sufficiently accurate given its high consistency with human coding.

Table A1. Inter-rater Agreement Among Coders and Keyword-based Classification

Sample	Measure	Limited Edition	Price Discount	Early Access	Overall
Inter-rater between coders	Agree	97.0%	96.6%	97.6%	91.7%
	Kappa	0.934	0.927	0.886	0.877
Keyword vs. Coder 1	Agree	98.3%	98.6%	99.2%	96.5%
	Kappa	0.962	0.971	0.962	0.948
Keyword vs. Coder 2	Agree	99.1%	98.1%	98.5%	95.7%
	Kappa	0.980	0.959	0.928	0.934

Notes. Inter-rater sample compares the label between two coders and includes 500 limited rewards; Keyword vs. Coder samples compare keyword-based classification and coders' label, and include 1,000 limited rewards each; Agree: total agreement rate; Kappa: Cohen's Kappa; Agreement for each form of reward limits captures the agreement on specific form; Overall agreement captures the joint agreement on all three forms.

Appendix B. Interaction Effects for Different forms of Reward Limits

We report and discuss the estimation results of the interaction effects between different forms of reward limits and the goal-directed mechanism in this appendix. Similar to Model 22 and 23 in Table 10, we include variables on the multiple forms of reward limits and interact them with *AboveGoal* in Model 1 and 2 in Table B1. For limited edition rewards, the negative effect of depleted reward tiers emerges only after a campaign is overperforming, and when a campaign is underperforming, there is a positive effect of *NumLERewardGone*. This result suggests that for backers who prefer limited edition rewards, their role of supporters is more salient compared with the role of consumers. They tend to focus on the noneconomic value of rewards and have a strong intention to help a campaign to succeed. This role of supporter is particularly salient before goal attainment. As more limited edition rewards become depleted, even though they may not obtain exclusivity, they tend to support a project via other rewards due to the non-economic values (e.g., willingness to help) they are seeking. However, after a campaign has achieved its funding goal, backers' desire to be exclusive and unique becomes more salient (i.e., more like consumers), possibly due to the decreased need to help the campaign succeed.

For price discount rewards, we observe that the negative effect of *NumPDRewardGone* turns weaker when a campaign is overperforming. We conjecture that backers who intend to choose the discounted rewards focus on the economic value from the reward. Their role of consumers is more pronounced than the role of supporters. As backers (as consumers) are typically not fully informed about the actual quality of the rewards (Chan and Parhankangas 2017), observing more backers choose price discount reward tiers (due to the depleted status) signals the quality of the reward and reduces the potential uncertainties of the campaign. It may also trigger herding behaviors that result in more contributions (Kuppuswamy and Bayus 2017).[1,2] It is likely that the signaling effect and potential herding

[1] We further considered a different interaction approach by adding performance indicators that capture different levels of overperforming rather than only using the *AboveGoal* indicator. The results show that the negative effect of *NumPDRewardGone* is weaker for campaigns with a greater extent of overperformance, which lends support for argument on the signaling effect and herding behavior.

[2] Considering the lack of significance of *NumPDRewardGone* × *AboveGoal* in Model 2 when the number of new backers is the dependent variable, more contributions only come from higher pledged amounts from backers rather than a greater number of backers.

behavior may weaken the negative effect of depleted price discount rewards. Taken together, the results potentially imply different mechanisms between limited edition and price discount when interacting with the goal-directed mechanism.

Table B1. Dynamics of Reward Limits – Interaction Effects

VARIABLES	Model 1 *ln(Pledge$_{it}$)*	Model 2 *ln(Backers$_{it}$)*
NumLEReward	**0.041*****	**0.012***
	(0.012)	(0.005)
NumLERewardGone	**0.074*****	**0.043*****
	(0.021)	(0.009)
NumPDReward	0.021	0.005
	(0.016)	(0.007)
NumPDRewardGone	**-0.102*****	**-0.074*****
	(0.025)	(0.014)
NumEAReward	-0.044	-0.008
	(0.031)	(0.012)
NumEARewardGone	-0.049	-0.034
	(0.048)	(0.026)
NumLEReward × AboveGoal	0.010	0.005*
	(0.006)	(0.003)
NumLERewardGone × AboveGoal	**-0.107*****	**-0.046*****
	(0.022)	(0.010)
NumPDReward × AboveGoal	-0.003	-0.005
	(0.013)	(0.005)
NumPDRewardGone × AboveGoal	**0.090****	**0.020**
	(0.027)	(0.013)
NumEAReward × AboveGoal	0.023	0.018
	(0.023)	(0.015)
NumEARewardGone × AboveGoal	0.020	-0.020
	(0.054)	(0.031)
AboveGoal	-0.752***	-0.272***
	(0.019)	(0.007)
Constant	2.418***	0.996***
	(0.156)	(0.069)
R^2	0.058	0.093
Other Form	Yes	Yes
Controls	Yes	Yes
Time Dummies	Yes	Yes
Number of Campaigns	31,480	31,480
Observations	1,029,061	1,029,061

Notes. All the models perform OLS with two-way fixed effects; Campaign fixed effects are implemented by within transformation and day fixed effects (dummies) are controlled for time trends; Cluster-robust standard errors in parentheses.
Significance Levels: *** p<0.001, ** p<0.01, * p<0.05

In summary, this interaction analysis yields nuanced insights into different forms of reward limits and the goal-directed effect. The main effect of goal-directed mechanism is salient for limited edition and not for price discount. We speculate that backers who pursue limited edition rewards value exclusivity more than the economic benefits such that they tend to factor the noneconomic values and the success of a campaign into their contributions. Therefore, they are unlikely to be sensitive to the depletion of limited edition rewards when the campaign is underperforming, as the supporter role of helping the campaign to be successful complements the noneconomic value. On the other hand, backers who intend to choose price discount rewards value economic benefits more such that they emphasize their own economic benefits more than the campaign's success. As they behave more like consumers, the current fundraising performance is more likely to create a signaling and herding effect instead of activating the backer-as-supporter role.[3]

[3] We further compare the changes of reward limit status before and after the attainment of funding goal for limited edition and price discount rewards. The frequency of changes for depletion before and after goal attainment are similar for limited edition rewards; whereas for price discount rewards, more limited rewards are depleted after goal attainment. It implies that relatively more limited edition rewards are depleted before goal attainment than price discount rewards so that limited edition is more pertinent to goal achievement than price discount. This lends support to the mechanism on the role of backer-as-supporter for backers who prefer limited edition rewards.

Appendix C. Robustness of Matching

In this appendix, we complement the details of the matching procedure in our main analysis and present robustness checks regarding our matching analysis.

C1. Alternative Matching Approach

In the campaign level analysis, both propensity score matching and Mahalanobis distance matching produce similar results on the positive effects of incorporating reward limits. Specifically, for propensity score matching, we estimate the propensity score with a probit model and use 1-to-1 nearest neighbor matching with replacement based on the propensity score. For Mahalanobis distance matching, we employ similar 1-to-1 nearest neighbor matching but select the control group based on all the covariates (i.e., the Mahalanobis distance between covariates). In our main analysis, to ensure the completeness of sample, our matching approaches keep all the campaigns in the treatment group to calculate the treatment effects.

To assess the quality of matching, we compare the covariates across the treatment and control groups after matching. As shown in Table C1, compared with the unmatched sample (before matching), the standardized biases (in percentage) are reduced to acceptable magnitudes in both matched samples. The covariates are also quite similar in terms of magnitude, suggesting the effectiveness of matching. However, in Table C1, the *t*-tests are still significant for several campaign level indicators. On the one hand, this may be due to the large sample size (large sample sizes generate significant *t*-tests even though the magnitudes of the differences are quite small). On the other hand, we can further consider stricter criteria in our matching procedure to balance the *t*-test comparison and enhance matching quality. Unfortunately, using stricter matching criteria will inevitably result in the dropping of a greater number of observations from the treatment group, which may undermine the generalizability of results and induce other potential sample selection issues. Therefore, to balance matching strictness and sample representativeness, we conduct additional matching analyses that enforce stricter criteria but keep the treatment sample at an acceptable level.

Table C1. Comparison between Treatment and Control Group – General Matching

Variable	Sample	Treated	Control	%bias	*t*-test
	Unmatched	8.855	8.602	14.0	12.53***
ln(Goal)	PSM	8.855	8.887	-1.7	-1.70
	Mahalanobis	8.855	8.795	3.3	3.45***
ImageCount	Unmatched	8.697	3.276	58.5	50.29***
	PSM	8.697	8.681	0.2	0.13
	Mahalanobis	8.697	7.521	12.7	10.09***
HasVideo	Unmatched	0.710	0.502	43.6	38.85***
	PSM	0.710	0.699	2.4	2.36*
	Mahalanobis	0.710	0.709	0.2	0.24
	Unmatched	32.37	33.09	-6.4	-5.74***
CampaignDuration	PSM	32.37	32.88	-4.2	-4.16***
	Mahalanobis	32.37	32.27	0.9	0.96
	Unmatched	7.732	7.187	47.7	42.03***
ln(DescriptionLength)	PSM	7.732	7.744	-1.1	-0.97
	Mahalanobis	7.732	7.675	4.9	4.57***
	Unmatched	6.363	6.076	41.7	36.99***
ln(RiskLength)	PSM	6.363	6.366	-0.3	-0.32
	Mahalanobis	6.363	6.329	5.0	4.91***
	Unmatched	8.510	4.557	82.9	71.69***
NumReward	PSM	8.510	8.618	-2.2	-1.72
	Mahalanobis	8.510	7.737	16.2	13.7***
	Unmatched	5.083	4.256	52.5	47.10***
ln(AvgRewardPrice)	PSM	5.083	5.129	-2.9	-2.91**
	Mahalanobis	5.083	4.988	6.0	6.42***
	Unmatched	0.278	0.208	12.8	11.23***
ln(NumCreated)	PSM	0.278	0.281	-0.6	-0.49
	Mahalanobis	0.278	0.267	1.9	1.74
	Unmatched	0.649	0.251	44.4	38.52***
ln(NumBacked)	PSM	0.649	0.598	5.7	4.50***
	Mahalanobis	0.649	0.576	8.1	6.72***

Notes. Unmatched: sample without matching; PSM: sample with propensity score matching; Mahalanobis: sample with Mahalanobis distance matching.
Significance Levels: *** p<0.001, ** p<0.01, * p<0.05

Specifically, we consider three alternative matching approaches toward stricter matching. The results of ATTs and the comparisons of covariates after matching are presented in Table C2 and C3, respectively. First, we further reduce the caliper of propensity score matching from 0.01 to 0.0001 (C-PSM; propensity score matching with stricter caliper). This approach does not sacrifice many observations in the treatment group (3,474 observations out of 17,884 dropped), and the results are consistent with our main analysis. In the comparison of covariates, the standardized biases and *t*-values are further reduced to lower magnitude compared with our main analysis. Second, we adopt the trimming method (Smith and Todd 2005) to restrain the common support region of the propensity scores. This approach drops those observations with the lowest propensity score density to the matched sample in the treated sample (T-PSM; propensity score matching with trimming) (Caliendo and Kopeinig 2008). The results in Table C2 and C3 show consistent ATTs and better matching outcomes after trimming 20% of the treatment group (3,576 observations out of 17,884 dropped). In further sensitivity analyses, the results remain consistent when trimming 5%, 10%, 15%, 20% and 25% of the campaigns in the treatment group (Frölich 2004). Third, we restrict the Mahalanobis distance matching with caliper to directly guarantee higher similarity between covariates in treatment and control groups (C-Mahalanobis; Mahalanobis matching with stricter caliper). This approach, however, discards a large number of observations in the treatment group given the magnitude of Mahalanobis distance. To balance the significantly reduced sample size after matching (i.e., representativeness of sample and generalizability of results) and strictness of matching criteria (i.e., higher similarity between treated and matched sample), we choose the caliper that would restrain the common support to around a quarter of campaigns in the treatment group (13,304 out of 17,884 dropped). Our results in Table C2 and C3 show similar treatment effects as our main analysis, and all of the covariates are not significantly different across treatment and control groups after matching. These analyses suggest that our matching results are robust to various stricter criteria and the treatment effects are consistent under higher matching quality (i.e., all the covariates are not significantly different).

Table C2. Robustness Tests of Matching

Outcomes	Metrics	C-PSM	T-PSM	C-Mahalanobis
Success	ATT	**0.023***	**0.032*****	**0.024***
	t-value	2.56	3.42	2.09
ln(Pledged)	ATT	**0.318*****	**0.331****	**0.305*****
	t-value	4.31	5.86	3.07
ln(Backers)	ATT	**0.245****	**0.262***	**0.213*****
	t-value	6.26	7.74	4.48
DaysToSuccess	ATT	**1.106*****	**1.145*****	**1.117****
	z-value	5.18	7.49	2.89

Notes. C-PSM: propensity score matching with stricter caliper; T-PSM: propensity score matching with trimming; C-Mahalanobis: Mahalanobis distance matching with stricter caliper. Hazard ratio is used for the ATT of *DaysToSuccess*; All the category, month and year dummies are included to estimate propensity score; Heteroskedasticity-consistent robust standard errors are used to obtain *t*-values; *N*=14,410 in treated and matched sample for C-PSM; *N*=14,308 for T-PSM; *N*=4,580 for C-Mahalanobis.
Significance Levels: *** p<0.001, ** p<0.01, * p<0.05

Table C3. Comparison between Treatment and Control Group – Stricter Matching

Variable	Sample	Treated	Control	%bias	*t*-test
	C-PSM	8.741	8.769	-1.6	-1.35
ln(Goal)	T-PSM	8.778	8.813	-1.9	-1.79
	C-Mahalanobis	8.734	8.727	0.4	0.27
	C-PSM	6.560	6.337	2.4	2.02*
ImageCount	T-PSM	6.881	6.844	0.4	0.36
	C-Mahalanobis	3.877	3.735	1.5	1.14
	C-PSM	0.664	0.666	-0.5	-0.41
HasVideo	T-PSM	0.685	0.683	0.4	0.41
	C-Mahalanobis	0.601	0.601	0.0	0.00
	C-PSM	32.35	32.69	-3.0	-2.64**
CampaignDuration	T-PSM	32.31	32.86	-5.0	-4.6***
	C-Mahalanobis	31.48	31.50	-0.1	-0.09
	C-PSM	7.583	7.602	-1.6	-1.34
ln(DescriptionLength)	T-PSM	7.631	7.659	-2.5	-2.19*
	C-Mahalanobis	7.441	7.435	0.5	0.32
	C-PSM	6.294	6.296	-0.3	-0.26
ln(RiskLength)	T-PSM	6.320	6.314	0.9	0.84
	C-Mahalanobis	6.219	6.209	1.5	0.91
	C-PSM	7.327	7.314	0.3	0.21
NumReward	T-PSM	7.380	7.325	1.2	1.24
	C-Mahalanobis	6.216	6.078	2.9	1.84
	C-PSM	4.946	5.010	-4.1	-3.65**
ln(AvgRewardPrice)	T-PSM	4.995	5.077	-5.2	-4.90***
	C-Mahalanobis	4.828	4.813	1.0	0.54
	C-PSM	0.260	0.269	-1.7	-1.36
ln(NumCreated)	T-PSM	0.264	0.278	-2.5	-2.13*
	C-Mahalanobis	0.025	0.025	0.0	-0.02
	C-PSM	0.505	0.510	-0.6	-0.47
ln(NumBacked)	T-PSM	0.547	0.568	-2.4	-1.91
	C-Mahalanobis	0.067	0.067	0.0	-0.04

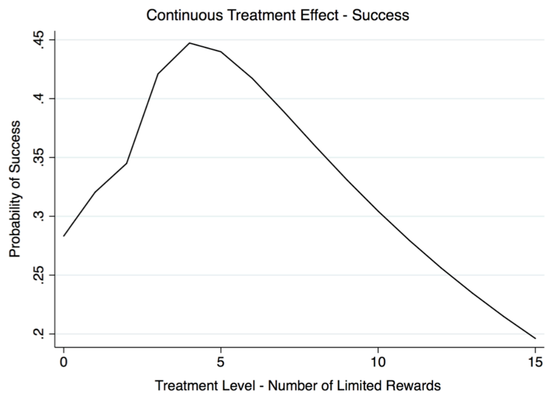
Notes. C-PSM: sample with propensity score matching and stricter caliper; T-PSM: sample with propensity score matching and trimmed treatment group; C-Mahalanobis: sample with Mahalanobis distance matching and stricter caliper.

Significance Levels: *** $p<0.001$, ** $p<0.01$, * $p<0.05$

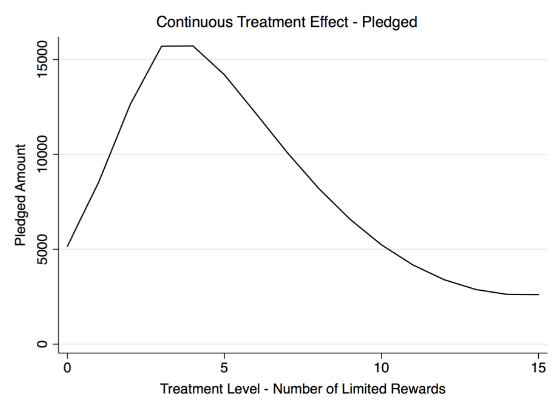
C2. Continuous Treatment Effect

Another aspect of matching robustness relates to the binary treatment effect vs. a continuous treatment effect of reward limits. Our main analysis considers a binary treatment effect for incorporating reward limits and then examines effective number of limited rewards given the decision of incorporating reward limits. However, the combination of these two analyses may suggest an overall multi-level treatment of reward limits and the decision of setting reward limits could be endogenously made at each treatment level. To understand whether this affects our findings and derive further insights, we regard the number of limited rewards as a continuous treatment and estimate the continuous treatment effects (campaigns without reward limits still serve as the control group). To obtain such treatment effects, we use the same covariates in propensity score matching to estimate *generalized propensity score* at each level of treatment (Hirano and Imbens 2004, Imai and Van Dyk 2004) with a generalized linear model (GLM) (Guardabascio and Ventura 2014). Then the conditional expectation of the outcome variable is estimated based on the treatment variable and the generalized propensity score. A dose-response function that captures the relationship between treatment variable and outcome variable is constructed by averaging the conditional expectations at each treatment level (Guardabascio and Ventura 2014). We plot the conditional expectation of the four outcome variables at each number of limited rewards (i.e., the dose-response function) in Figure C1. Empirically, we constrain the level of treatment to 15 as only less than 1% of campaigns include more than 15 limited rewards, which could not ensure the accuracy of estimating generalized propensity scores at these treatment levels. The plots in Figure B1 confirm our findings in the campaign level analysis – all of the dose-response functions exhibit inverted U-shaped relationships between the number of limited rewards and crowdfunding performance. In summary, based on the continuous treatment effect, this result further suggests that the number of limited rewards has an overall inverted U-shaped effect on crowdfunding performance, starting from non-treated occasion to the multi-level treatment occasion.

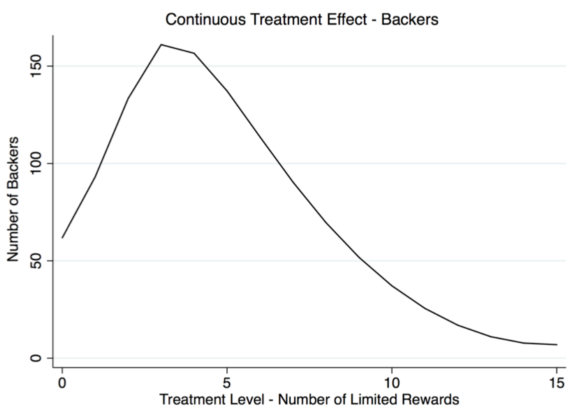
Figure C1. Continuous Treatment Effect for Number of Limited Rewards



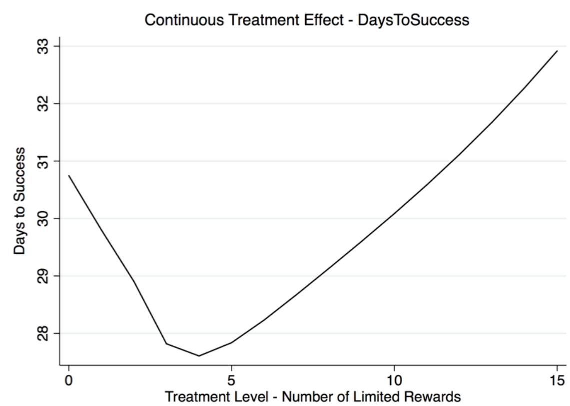
(a) Campaign Success



(b) Pledged Amount



(c) Number of Backers



(d) Days to Success

Appendix D. Alternative Measures

This appendix discusses alternative measures of limited rewards. First, in our campaign level analysis (Table 9), we used the proportion of limited reward tiers in the reward scheme to portray the setting of reward limits in the reward scheme. However, operationalizing at the reward tier level may not aptly capture the actual impact of limited rewards since the quantity or quota for backers in each limited reward tier can be set differently (e.g., one reward tier may be limited to 100 backers, whereas another may be limited to 1,000 backers). In this regard, we alternatively measure limited rewards by using the total quota for backers specified in the limited reward tiers on the first day of the campaign (*TotalBackerQuota*, i.e., the sum of maximum allowable backers across all limited reward tiers). We use the linear and quadratic terms for this alternative measure in the second stage equation of the sample selection model. With the same estimation approach, the results in Table D1 are consistent when backer quotas are used to capture limited rewards.

Table D1. Reward Limit Setting Strategy with Backer Quota in Limited Reward Tiers

VARIABLES	Model 1 *Success*	Model 2 *ln(Pledged)*	Model 3 *ln(Backers)*	Model 4 *DaysToSuccess*
ln(TotalBackerQuota)	**0.050*****	**0.229*****	**0.149*****	**0.108*****
	(0.013)	(0.054)	(0.029)	(0.025)
ln(TotalBackerQuota)^2	**-0.007*****	**-0.013*****	**-0.009*****	**-0.011*****
	(0.001)	(0.003)	(0.002)	(0.003)
Constant	1.798***	10.200***	5.238***	-2.708***
	(0.173)	(0.606)	(0.345)	(0.255)
R^2	–	0.499	0.505	–
Model Fit (χ^2)	1883.18***	–	–	5388.92***
Control	Yes	Yes	Yes	Yes
Dummies	Yes	Yes	Yes	Yes
Observations	17,196	17,196	17,196	17,196

Notes. Model 1 uses the probit model; Model 2 and 3 use OLS; Model 4 uses the proportional hazard model using Weibull distribution (coefficients are presented); Model 1 and Model 4 are jointly estimated with first stage equation by maximum likelihood estimation; The same control variables in main analysis are included but not presented; Category, month and year dummies are included; Robust standard errors in parentheses.
Significance Levels: *** p<0.001, ** p<0.01, * p<0.05

Second, in our campaign-day level analysis, we measure the status of reward limits using the number of limited reward tiers that are available and depleted. Similar to our campaign level analysis on the proportion of limited reward tiers, the number of limited reward tiers could be alternatively operationalized by the number of backers allowed in the limited reward tiers. Specifically, we use two alternative variables to measure the status of reward limits: the total backer quota of limited reward tiers at the end of day *t*-1 (*TotalBackerQuota*, as the alternative of *NumLimitReward*), and the total number of backers that have chosen the limited reward tiers at the end of day *t*-1 (*BackersInLimit*, as the alternative of *NumLimitRewardGone*). Although using the number of backer quota and the number of backers already contributing to the limited reward tiers at the campaign-day level may induce mixed effects,[4] we can use them to examine whether the overall effects are consistent when operationalizing the key variables by the number of backers (instead of the number of reward tiers). In Table D2, the same fixed effects models exhibit similar results with Model 18 and 19 in Table 10, suggesting consistency in effects of backer quota (positive effect) and number of backers in limited reward tiers (negative effect).

Table D2. Dynamics of Reward Limits with Backer Quota

VARIABLES	Model 5 *ln(Pledge$_{it}$)*	Model 6 *ln(Backers$_{it}$)*
ln(TotalBackerQuota)	**0.064*****	**0.029*****
	(0.013)	(0.005)
ln(BackersInLimit)	**-0.111*****	**-0.080*****
	(0.015)	(0.007)
Constant	2.173***	0.952***
	(0.156)	(0.069)
R^2	0.050	0.082
Controls	Yes	Yes
Time Dummies	Yes	Yes
Number of Campaigns	31,919	31,919
Observations	1,044,525	1,044,525

Notes. All the models perform OLS with two-way fixed effects; Same control variables and time dummies are included; Cluster-robust standard errors in parentheses.
Significance Levels: *** p<0.001, ** p<0.01, * p<0.05

[4] This is because the number of backers in the limited reward tiers is not fully equal to depleted status in our original operationalization. On the one hand, more backers in limited reward tiers could make these reward tiers closer to depletion and demotivate potential backers to support the campaigns (i.e., negative effect) (Amaldoss and Jain 2008, 2010, Mussweiler et al. 2004); on the other hand, when limited reward tiers have remaining quota, more backers in these reward tiers may induce herding effects (Kuppuswamy and Bayus 2017) and further increase the urgency of choosing these rewards (i.e., positive effect). Given these mixed effects, in our analysis, we only use these two variables as a robustness test for the consistency of overall effects.

Appendix E. Alternative Panel Model Specification

In this appendix, we discuss two alternative panel model specifications and corresponding estimation approaches.

E1. Dynamic Panel Model

First, we check the model specification regarding panel data structure. One potential issue is serial correlation of the dependent variables – the contributions on day *t* may strongly depend on the contributions on day *t*-1. Although we have used cluster-robust standard errors to account for both panel level serial correlation and heteroskedasticity (and these issues would not affect the coefficients) (Wooldridge 2010), if the lagged dependent variables (contributions on day *t*-1) are correlated with other variables measured at the end of day *t*-1, our model specification may no longer be valid. This requires an alternative panel model specification for our campaign-day level analysis.

To address this concern, we specify our panel model with the inclusion of lagged dependent variables (i.e., $Pledged_{it\text{-}1}$ or $Backers_{it\text{-}1}$) to account for both serial correlation and potential omitted variable bias. We first estimate a two-way fixed effects model with lagged dependent variables and obtain consistent results. However, the fixed effect estimation with lagged dependent variable is known to have other model specification issues, i.e., the violation of strong exogeneity and dynamic panel bias, i.e., the downward bias of coefficients[5] (Nickell 1981, Wintoki et al. 2012). To counteract these concerns, we employ dynamic panel data model estimation (Arellano and Bond 1991, Arellano and Bover 1995, Blundell and Bond 1998). This estimation allows us to instrument the lagged dependent variable with higher order lags of the dependent variable using generalized method of moments (GMM). Given the potential concern of persistent autocorrelations, which may weaken the instruments and lead to finite-sample bias (Arellano and Bover 1995, Blundell and Bond 1998), we employ the "system GMM" estimator which utilizes both first-differenced equations and the original equations in levels (i.e., levels

[5] Theoretically, the direction of dynamic panel biases (for both lagged dependent variable and independent variables) depend on the relationship between lagged values and current values (positive relationship creates downward bias, while negative relationship leads to upward bias). In our case, the lagged values are positively related to current values, implying downward biases of estimates.

equations) in a "stacked" system of equations. It instruments the first-differenced equations with lagged levels and instruments the equations in levels with first-differenced variables.

Our estimations using the dynamic panel model specification yield consistent results (see Table E1). To avoid the problem of using too many lags for instruments, we consider only 2 to 4 lags (starting from the third order lag) as instruments (Bond 2002, Roodman 2009). The results using different specifications of lag structures remain consistent. We also instrument our focal variables (i.e., *NumLimitReward* and *NumLimitRewardGone*) with their higher order lags in our estimations to further account for their potential dynamic relationships across time (Roodman 2009). The insignificance of the *Hansen J* statistic suggests that our instruments are jointly valid. We further conduct the Arellano-Bond test for serial correlation in the model and AR(3) is not significant[6] (Arellano and Bond 1991) in all the models, suggesting the validity of our choice of lags.

Table E1. Dynamic Panel Model Estimation with System GMM

VARIABLES	Model 1 $ln(Pledge_{it})$	Model 2 $ln(Backers_{it})$	Model 3 $ln(Pledge_{it})$	Model 4 $ln(Backers_{it})$	Model 5 $ln(Pledge_{it})$	Model 6 $ln(Backers_{it})$
$ln(Pledge_{it-1})$	0.366***		0.391***		0.400***	
	(0.031)		(0.028)		(0.024)	
$ln(Backer_{it-1})$		0.494***		0.513***		0.512***
		(0.021)		(0.018)		(0.017)
NumLimitReward	**0.087*****	**0.025*****	**0.069*****	**0.019*****	**0.066*****	**0.020*****
	(0.014)	(0.005)	(0.012)	(0.004)	(0.011)	(0.004)
NumLimitRewardGone	**-0.119*****	**-0.033*****	**-0.098*****	**-0.026****	**-0.091*****	**-0.027*****
	(0.023)	(0.009)	(0.020)	(0.008)	(0.018)	(0.008)
F	83.60	126.29	107.03	154.98	132.20	197.74
Hansen *J* (*p*-value)	0.916	0.583	0.942	0.423	0.821	0.187
AR(1) test (*p*-value)	0.000	0.000	0.000	0.000	0.000	0.000
AR(2) test (*p*-value)	0.000	0.000	0.000	0.000	0.000	0.000
AR(3) test (*p*-value)	0.816	0.938	0.715	0.956	0.897	0.946
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Campaigns	31,906	31,906	31,906	31,906	31,906	31,906
Observations	1,012,606	1,012,606	1,012,606	1,012,606	1,012,606	1,012,606

Notes. All the models perform dynamic panel model estimation with system GMM; Model 1-2 use two orders of lagged variables as instruments (3-4); Model 3-4 use three orders of lagged variables as instruments (3-5); Model 5-6 use four orders of lagged variables as instruments (3-6); All models are estimated with campaign fixed effects; Same control variables and time dummies in panel model are included; Cluster-robust standard errors in parentheses.
Significance Levels: *** $p<0.001$, ** $p<0.01$, * $p<0.05$

[6] We choose the third order and above lags as the instruments since the third order lagged dependent variable is less correlated with dependent variable in our context. Therefore, according to Roodman (2009), the Arellano-Bond test of serial correlation is expected to be insignificant at the third order (i.e., AR(3)), as significant Arellano-Bond test will invalidate the lag variable at the same order as instrument. In addition, our Arellano-Bond tests at first and second orders are significant, further suggesting that the third order lag is the appropriate choice.

E2. Endogeneity and Instrumental Variable Approach

Second, we check the robustness of panel model specification regarding potential endogeneity. As the status of reward limits could be changed along with daily funding performance, which may lead to issues of endogeneity and simultaneity. Although we have already controlled a series of time-varying variables (e.g., cumulative funding percentage, cumulative fundraising progress and other campaign changes) and dummies of each day, we further resolve this concern by using the instrumental variable approach. Specifically, due to a lack of traditional, theoretically motivated and empirically valid instruments, we employ the method by Lewbel (2012) to perform instrumental variables estimation with heteroskedasticity-based instruments. This method constructs orthogonal instruments mathematically from exogenous covariates in the model without external instruments. The generated instruments rely on the heteroskedasticity in the error term to ensure their correlations with the endogenous variables and achieve model identification. In our panel model, we specify cumulative funding percentage and cumulative fundraising progress as exogenous variables and construct instruments for the endogenous variables including the number of limited rewards and the number of depleted limited rewards. The results of instrumental variables estimation are presented in Table E2 and the effects are consistent with our main results (Model 7-8). We also consider all covariates in the panel model as exogenous to construct instruments and the results are consistent (Model 9-10).

Table E2. Panel Model Estimation with Instrumental Variables

VARIABLES	Model 7 $ln(Pledge_{it})$	Model 8 $ln(Backers_{it})$	Model 9 $ln(Pledge_{it})$	Model 10 $ln(Backers_{it})$
NumLimitReward	**0.039*****	**0.013*****	**0.039*****	**0.013*****
	(0.005)	(0.002)	(0.005)	(0.002)
NumLimitRewardGone	**-0.097*****	**-0.059*****	**-0.093*****	**-0.058*****
	(0.006)	(0.003)	(0.006)	(0.003)
R^2	0.027	0.045	0.027	0.045
Campaign FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes
Number of Campaigns	31,919	31,919	31,919	31,919
Observations	1,044,525	1,044,525	1,044,525	1,044,525

Notes. All the models perform panel model estimation with heteroskedasticity-based instrumental variables; Model 7-8 specify cumulative funding percentage and cumulative fundraising progress as exogenous to generate instruments; Model 9-10 specify all covariates as exogenous to generate instruments; All models are estimated with campaign fixed effects; Same control variables and time dummies in panel model are included; Cluster-robust standard errors in parentheses.

Significance Levels: *** p<0.001, ** p<0.01, * p<0.05

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