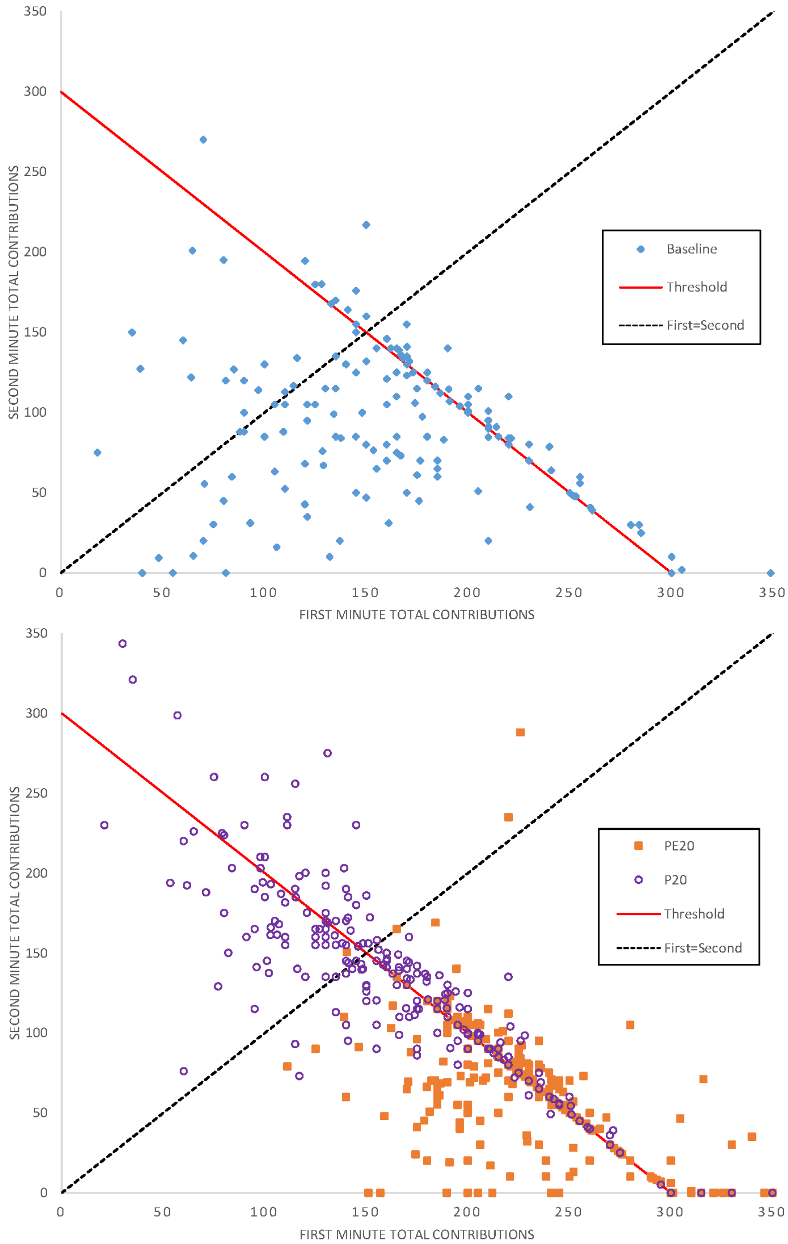

Figure 1: First and Second Half Contributions, by Project, for Baseline (top panel) and for Refund Bonus Treatments (bottom panel)

outcome of a campaign should lie on the $(0, 300) - (300, 0)$ efficiency line, where we observe a large concentration of outcomes. The figure also reveals notable differences across the treatments. The concentration of PE20 treatment outcomes around the efficiency line below the 45-degree line (solid squares) shows that more contributions are pledged during the first half of the period in this treatment. The P20 refund bonus campaigns (open circles) are spread along the entire efficiency line, suggesting that subjects compensate for low early contributions by contributing more later. In contrast, we do not observe such

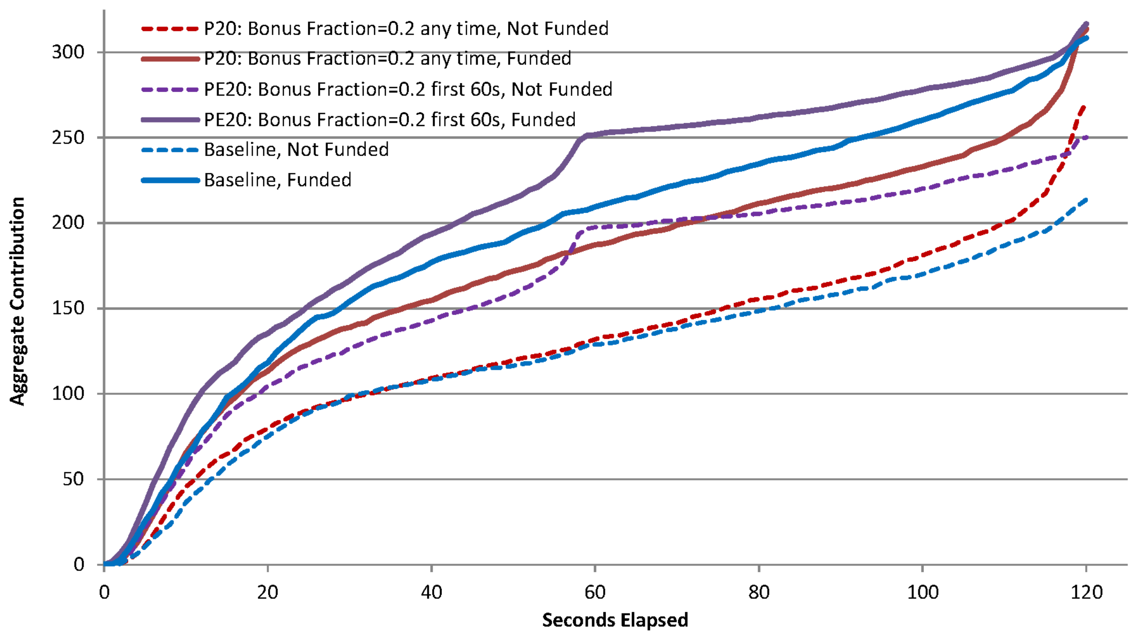

Figure 2: Cumulative Average Contributions, by Funding Success

compensating behavior in baseline projects (solid diamonds), where efficiency is achieved only when sufficient early contributions are raised. In general, Figure 1 shows that the observed contributing behavior is consistent with theoretical predictions. The dispersed “cloud” of outcomes in the baseline treatment can be attributed to multiple equilibrium outcomes. Refund bonuses press campaign outcomes onto the efficiency line, in line with the prediction about the unique efficient equilibrium outcome.

Figure 2 displays the average cumulative contributions over time for each treatment. The figure distinguishes successful projects with solid lines (contributions that reach the threshold of 300) and unsuccessful ones with dashed lines. Many of the contributions are concentrated in the initial 20 to 40 seconds, as well as the final 5 to 10 seconds, regardless of the refund bonus rules. But they also illustrate different patterns due to the timing of refund bonus-eligible contributions. The refund bonuses in treatment PE20 that are targeted for only contributions made during the first minute tend to raise early contributions relative to the baseline, both for successful and unsuccessful projects. The midpoint increase in contributions just before the 60-second initial period ends is also clearly evident, when on average projects have raised 234 of the 300 target. By contrast, in the baseline and P20 treatments contributions accumulate more slowly, with on average

164 and 165 of the 300 target raised at the midpoint, respectively. The time pattern for cumulative contributions is similar in these treatments especially for unsuccessful campaigns until the final few seconds, which are decisive for the P20 treatment.

4.3 Campaign Failures

This subsection examines reasons for campaign failures in the baseline and the P20 treatments. We will argue that without bonuses campaigns can fail due to conditionally cooperative behavior and with bonuses – due to delayed cooperation. Both reasons for failures can originate from the same source, which is low early contributions.

Inspection of the scatter plot of campaign contributions in Figure 1 indicates that for low early contributions cooperation broke down in the baseline treatment. Consistent with Hypothesis 1, in the baseline treatment funding success positively correlates with early contributions.[12] To explore further the explanation of conditional cooperation underlying Hypothesis 1, Table A1 of the online appendix presents the results of regressions for the effect of early contributions on individual late contributions in the baseline treatment. If others were not cooperative early in the campaign then contributors are significantly less likely to make a contribution during the second half of the campaign and their amount contributed is (insignificantly) lower. Such behavior points to hypothesized conditional cooperation, supported by equilibrium tit-for-tat strategies.

In the treatment that offers 20% refund bonuses, funding success is also found to correlate positively with total early contributions. But unlike in the baseline treatment, we cannot attribute this correlation to conditionally cooperative behavior.[13] The negative effect of low early contributions on funding success in the P20 treatment, however, can be explained by delayed cooperation. Inspection of Figure 2 indicates that in the P20 treatment contributions tend to accumulate relatively slowly before campaigns ended in a flurry of contributing activity. The slow accumulation can be explained by the prospect of refund bonuses, which can subdue incentives for further contributions.[14] The

[12]This is established using a logit regression; since this confirms the patterns already discussed in relation to Figures 1 and 2 and Table 3, we do not report it here.

[13]For evidence, see Table A2 and the discussion of its results in the online appendix.

[14]See Cason and Zubrickas (2019) for further details.

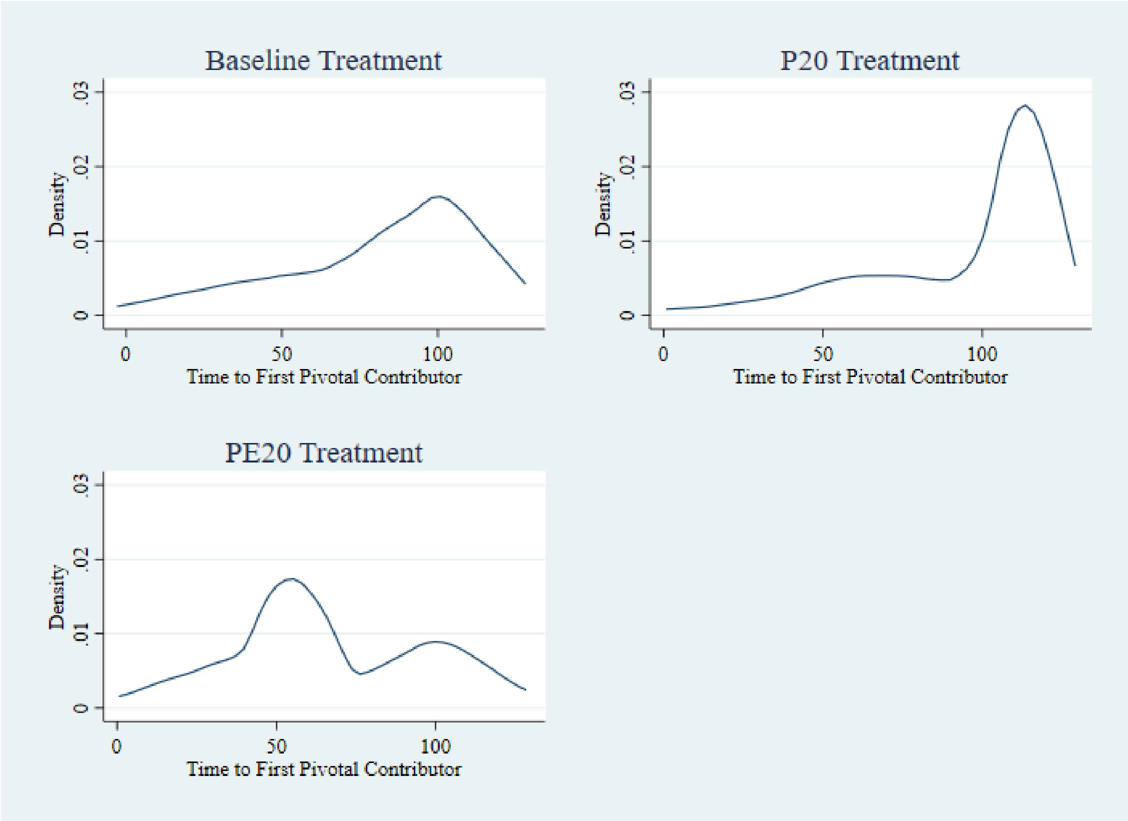

Figure 3: Kernel Density of Pivotal Point Timing, by Treatment

consequence of the slow accumulation of contributions is a higher chance of last-moment miscoordination. If slower early accumulation leads to a higher chance of miscoordination, this would result in a positive correlation between early contributions and funding success.

Figure 3 provides further support for this explanation of delayed cooperation as a reason for campaign failures in the P20 treatment. During its contribution window, a campaign can reach a point when a single contributor becomes pivotal and would find it profitable to bring the total contribution up to the funding threshold, rather than not contribute further. The timing of pivotalness can be viewed as an inverse measure of the resolution of the coordination problem. More precisely, once pivotalness is reached the resolution of the coordination problem no longer requires collective action. At that point the strategic interaction becomes a waiting game to determine who incurs the burden of providing the public good. Hence, the earlier that pivotalness is reached, the more the opportunity subjects have to achieve the funding target. Figure 3 shows the distribution of timing when campaigns first reach pivotalness over the contribution window of 120 seconds. In the P20 treatment the mode of pivotalness is at the very end of the contribution window and, furthermore, most density mass is concentrated there. Table 4 shows that the P20 design achieves pivotalness in 93.6% campaigns compared to only 65.3% in the baseline treatment, but it occurs much later in the contribution window (the median time to pivotalness is 110 for P20 compared to 88 for the baseline). Hence, while refund bonuses can improve implementation properties they can also delay cooperation. This, in turn, can aggravate the problem of efficient equilibrium coordination.

Table 4: Timing of Pivotalness

Treatment	Total Campaigns	Reached Pivotalness	Fraction Pivotal	Mean Time to Pivotal (sec)	Median Time to Pivotal (sec)
Baseline	170	111	0.653	80.5	88
P20	220	206	0.936	94.0	110
PE20	230	205	0.891	66.2	58

4.4 Refund Bonuses for Early Contributions

The PE20 design, 20% refund bonuses for early contributions, is designed to encourage contributions during the early phase of the pledge window. The main idea behind this design is to avert the problem of delayed cooperation, observed for the P20 design, while retaining the implementation properties of refund bonuses.

To document the impact of the PE20 bonuses on early contributions, the first column of Table 5 reports a logit model indicating which of the two projects contributors choose for their initial contribution each period.[15] Not surprisingly, the "Individual Value" row shows that contributors tend to make their first contribution to the project that they value highly. The treatment dummies indicate that they are also more likely to contribute first to a project that has the early targeted refund bonus PE20, relative to the baseline. This treatment is 35 percentage points more likely to attract the initial contribution than the baseline. The refund bonus paid for contributions made at any time in P20 fails to increase significantly the likelihood of attracting the first individual contribution.

[15]Recall that two projects, with different refund bonus characteristics, were always available to receive contributions.

Table 5: Initial, Individual, and Total Contributions in First 60 Seconds

	Initial Contribution (Logit)	Individual Contribution (Secs 1–60)	Total Contribution (Secs 1–60)
Dummy for P20	0.047	0.936	3.24
	(0.111)	(2.612)	(8.05)
Dummy for PE20	0.350**	9.203**	64.38**
	(0.120)	(2.829)	(23.85)
Individual Value	0.0039**	0.274**	
	(0.0007)	(0.011)	
Group Value			0.199**
			(0.030)
Period		-0.464**	-4.34**
		(0.046)	(0.50)
Dummy (second treatment)		-1.325	-19.92
		(2.177)	(13.48)
Alternative Project Info	Included	Included	Included
Constant		1.381	81.01**
		(3.494)	(30.96)
Overall R-sq			0.343
Observations	7208	6200	620

Note: Random-effects regressions, with standard errors clustered by sessions; robust standard errors are reported in parentheses. Marginal effects shown for Initial Contribution column. Individual Contributions column displays tobit model estimates with censoring at 0. ** indicates coefficient is significantly different from zero at the .01 level; * at .05.

A similar picture emerges when considering the amount of individual and group contributions made by the time half of the period for collecting contributions has elapsed (i.e., the first 60 seconds). The last two columns of Table 5 show that the PE20 treatment collects more early contributions than the no-bonus baseline, while the P20 treatment does not. The PE20 treatment also collects more contributions than the P20 treatment (Chi-squared p-value < 0.05). The 60-second cutoff for bonus eligibility in PE20 is clearly effective at attracting contributions in the first part of the period. Consequently, a faster accumulation of contributions allows fundraising campaigns to reach a point of pivotalness more quickly as can be seen from Figure 3. Table A3 in the online appendix reports regressions of fundraising success for those campaigns that have at least one pivotal contributor, demonstrating that in all treatments success is strongly and positively associated with how much time is left when pivotalness is reached. For all treatments, success is about 8 percent more likely if pivotalness is reached 10 seconds earlier. Table A3 also shows that the time to reach pivotalness increases over time in all treatments. This is one reason for the decrease in fundraising success in later periods, documented earlier in Table 2. For further cross-treatment analysis of the role of early contributions, see Figure A1 and its discussion in the online appendix. Figure A1 highlights the campaign benefits of the early contributions using a series of regression models that predict success based on actual contributions made at various points in time in the baseline treatment.

5 Net Returns and Self-Supporting Bonuses

We turn next to a treatment comparison of funding efficiency, distributive efficiency, and net returns. Projects differed in their drawn individual values, so some have a greater total social value V than others. We define G as the sum of individual contributions at the end of the campaign and C as the contribution threshold. Thus successful projects have $G \geq C$ and unsuccessful projects $G < C$. We define funding efficiency as $[V - G]/[V - C]$ when the project is funded and 0 otherwise. This index ranges up to 1 for those projects whose total contributions G exactly reach the threshold C. Excess contributions above C lower this index below one. (Such excess contributions arise sometimes due to miscoordination in the final seconds.) Refund bonuses paid for unsuccessful projects do not factor into funding efficiency, since these are simply transfers and do not affect total surplus. Distributive efficiency is measured by the Gini index computed from net individual payoffs pooled across periods within each session.

Fundraisers will be worried about paying refund bonuses, so we also examine an alternative performance index, termed net return (NR), that penalizes the outcome when refund bonuses are paid.

$$NR(G) = \begin{cases} V - G & \text{if} \quad G \geq C \\ -\sum_i bonus_i & \text{if} \quad G < C \end{cases}$$

Table 6: Efficiency, Net Project Returns, Refund Bonuses, and Fundraiser Returns

Treatment	Funding Efficiency	Gini Index	Net Returns	Ave. Total Bonuses	Average Returns: $k = 273$	$k = 250$
Baseline	0.424	0.247	139.74	–	15.41	25.43
	(0.037)	(0.025)	(12.60)	–	(1.51)	(2.33)
P20	0.575	0.158	158.27	-21.43	3.09	17.00
	(0.032)	(0.014)	(12.17)	(1.81)	(3.29)	(4.03)
PE20	0.632	0.208	189.73	-12.79	**16.39**	**31.79**
	(0.030)	(0.016)	(11.31)	(1.23)	(3.20)	(3.80)
MW p-value (P20 vs PE20)	0.192	0.035	0.099	0.003	0.002	0.003

Note: Standard errors are reported in parentheses. MW abbreviates the Mann-Whitney nonparametric test.

This simply replaces the social value for unsuccessful projects (0) with the cost of the refund bonuses that must be paid by the fundraiser when the campaign is unsuccessful.

Table 6 reports average funding efficiency, distributive efficiency, and net returns for each of the treatments. The refund bonus treatments have greater funding efficiency and net returns than the no bonus baseline. Nonparametric Mann-Whitney tests indicate this increase in performance is significant for PE20 (p-value $= 0.011$ for efficiency and p-value $= 0.057$ for net returns, $n = 11$, $m = 10$) and is significant for P20 for efficiency (p-value $= 0.041$, $n = 13$, $m = 10$). We also observe that the bonus treatments perform better than the baseline in terms of distributive efficiency, though only the P20 treatment has a significantly lower Gini index (Mann-Whitney p-value $= 0.009$). An improvement in distributive efficiency can be explained by the fact that refund bonuses reduce the set of efficient equilibria by eliminating equilibria with uneven distribution of gains. In equilibrium, net gains from the public good must exceed the utility from refund bonuses, thus, preventing very unequal outcomes.[16] Consistent with this explanation, as the PE20 treatment makes only a partial use of refund bonuses, its performance with regard to distributive efficiency lies between the performances of the baseline and P20 treatments.

The higher net fundraising returns of the refund bonus treatments raise the natural

[16]See Zubrickas (2014) for theoretical details and Cason and Zubrickas (2017) for empirical evidence from a static environment.

question of whether the refund bonus mechanisms can be self-supporting. Since contributions sometimes fail to meet the threshold, refund bonuses need to be paid in some cases. The "Ave. Total Bonuses" column of Table 6 shows that bonuses paid average 12 to 21 per period, which accounts for the mix of successful and unsuccessful campaigns. The P20 campaign pays out significantly greater bonuses because of its lower success rate and the greater bonuses paid conditional on failure due to the longer time period for bonus-eligible contributions. The key issue is whether the increased rate of fundraising success due to offering refund bonuses (Table 1) is sufficient to generate enough surplus from the greater frequency of successful projects to offset the refund bonuses that need to be paid.

Suppose the fundraiser can produce the good at a cost of k. The fundraiser won't produce the good unless contributions, at the very least, cover costs so $C > k$. Successfully funded projects, therefore, generate a surplus to the fundraiser of $G - k$. Since bonuses need to be paid for unsuccessful projects, overall fundraiser returns $\pi(k)$ are

$$\pi(k) = \begin{cases} G - k & \text{if} \quad G \geq C \\ -\sum_i bonus_i & \text{if} \quad G < C \end{cases}$$

The fundraiser can generate a greater surplus from successful projects by choosing a larger "markup" of the threshold C over the project cost k. To provide some illustrative calculations for how great this markup must be to generate self-supporting refund bonuses, the last two columns of Table 6 presents hypothetical fundraiser payoffs for markups of 10% ($k = 273$) and 20% ($k = 250$) in each bonus treatment. The column labeled $k = 273$ indicates average returns for a 10% markup. The no bonus baseline has an average fundraiser return of 15.41, reflecting an average surplus of 35.4 realized for the 43.5% of periods in which the campaign is successful and zero payments when the campaign is unsuccessful. Even though a 10% markup is quite low, fundraisers can increase their net return by offering refund bonuses using the PE20 mechanism. In this case, (modest) refund bonuses need to be paid out when campaigns fail but this is more than balanced by the higher funding rate of 67%, leading to a fundraiser surplus of 16.39

per project or 6.4% over the no bonus baseline.

Refund bonuses become even more profitable if the markup over the project cost is larger, as illustrated in the rightmost column representing a 20% markup (from $k = 250$ to the $C = 300$ threshold). Moreover, the nonparametric Mann-Whitney tests shown on the bottom of the table indicate that the refund bonus treatment PE20 that targets only early contributions is significantly more profitable than the P20 bonus treatment that pays greater bonuses and fails to get cumulative contributions to the higher and more successful path.

6 Alternative Bonus Treatments

The main experiment reported in the previous sections contrasted the baseline treatment with two refund bonus treatments, one of which (PE20) was specifically designed to incentivize early contributions. We also explored alternative ways of implementing the refund bonus, which we briefly summarize in this section with additional details available in an earlier working paper version of this study (Cason et al., 2020). In these alternative treatments, four groups of 10 subjects participated and were eligible for the refund bonus as follows.

F3: Refund bonus of 3 for total individual contribution ≥ 30.

F6: Refund bonus of 6 for total individual contribution ≥ 30.

FE30: Refund bonus of 6 for first 5 individuals with total individual contribution ≥ 30.

FE50: Refund bonus of 6 for first 5 individuals with total individual contribution ≥ 50.

PE10: Proportional refund bonus $r = 0.10$ paid on contributions made during first minute of the two-minute contribution window.

The first four treatments simplify the refund bonus by replacing the proportional amount used in the main experiment with a fixed bonus amount for contributions that reach a specific threshold. The total individual contribution refers to the sum of contributions made by an individual at different points in time. The difference between F6 and FE30 is in the latter only the first 5 individuals who meet the individual threshold are eligible to receive the refund bonus. We note the FE30 and FE50 designs allow for inefficient low-contribution equilibria.[17] The difference between FE30 and FE50 is the size of the individual target to obtain this fixed bonus.[18] As in the main experiment, in every period two alternative projects were available for contributions, with differing refund bonus rules for each one. We varied the treatment conditions once within subjects, with other treatment variations implemented across subjects.

Table 7 provides the performance summary alongside the performance of the baseline, P20, and PE20 treatments reported in earlier sections. All five alternative treatments have a funding frequency that exceeds the 43.5% rate of the baseline treatment and they also have lower average shortfalls than the 86.2 average of the baseline. That the FE30 design also has a lower shortfall than the baseline suggests that in FE30 the inefficient equilibria are not salient, which reinforces the argument for the importance of early contributions in stimulating cooperation. Regression analysis from our earlier working paper shows significantly greater funding success for two designs (F6 and FE50) and significantly greater contributions for all refund bonus designs relative to the comparable baseline data. None of these refund bonus treatments have significantly *different* impacts on individual contributions, however, except that F6 has significantly lower contributions than FE50 (p-value = 0.005).

All five treatments also have greater funding efficiency and net returns than the comparable baseline, and this increase in performance is highly significant (typically at the two-percent significance level or better, and always significant at the five-percent level).

[17]With refund bonuses offered only to several first contributors, it can be an equilibrium outcome for contributors to stop contributing if their further contributions are no longer eligible for bonuses. When contributors employ tit-for-tat strategies, however, the existence of inefficient equilibria can be of only second order importance since a significant amount of early contributions would encourage conditional cooperators to contribute further.

[18]Note that these target amounts to receive bonuses can serve as suggested amounts for contributions. Evidence on the impact of increasing suggested amounts is mixed, with some studies showing a decrease in contributions (Adena and Huck, 2020; Reiley and Samek, 2019) while others find promising effects of non-binding suggestions (Adena et al., 2014).

Table 7: Robustness Treatments – Performance Summary

Treatment	Funding Frequency	Shortfall	Funding Efficiency	Net Returns	Ave. Total Bonuses	Average Returns: $k = 273$	$k = 250$
F3	45/90 = 50%	34.5 (4.1)	0.481 (0.051)	152.47 (18.89)	-9.57 (1.03)	10.00 (3.26)	21.50 (4.43)
F6	57/90 = 63%	36.6 (4.2)	0.599 (0.049)	175.02 (18.20)	-15.20 (2.15)	12.08 (4.56)	**26.65** (5.69)
FE30	43/90 = 48%	41.2 (3.7)	0.458 (0.051)	140.02 (19.35)	-15.53 (1.58)	4.23 (4.19)	15.22 (5.31)
FE50	50/90 = 56%	35.7 (4.4)	0.518 (0.051)	151.42 (17.39)	-9.47 (1.17)	**17.35** (5.54)	**30.13** (6.40)
PE10	44/90 = 49%	58.0 (4.6)	0.473 (0.052)	149.17 (18.83)	-8.79 (0.93)	9.37 (3.33)	20.62 (4.42)
Baseline	74/170 = 43.5%	86.2 (6.2)	0.424 (0.037)	139.74 (12.60)	– –	15.41 (1.51)	25.43 (2.33)
P20	133/220 = 60.5%	29.0 (2.9)	0.575 (0.032)	158.27 (12.17)	-21.43 (1.81)	3.09 (3.29)	17.00 (4.03)
PE20	154/230 = 67.0%	49.8 (3.8)	0.632 (0.030)	189.73 (11.31)	-12.79 (1.23)	**16.39** (3.20)	**31.79** (3.80)

Note: Standard errors are reported in parentheses.

Efficiency appears to be greatest in the treatments that have more generous bonuses such as F6, which outperforms FE30 (p-value < 0.05) and F3 (p-value < 0.10). Net returns are also higher with refund bonuses, but none of the net returns for the refund bonus treatments are significantly different from each other. Bonus payments are greater for the more generous designs (such as F6) and for treatments with lower fundraising success (FE30). Last but not least, fundraisers can increase their surplus by offering refund bonuses. At a 10% markup (column $k = 273$), the FE50 design yields a fundraiser surplus of 17.35 per project while in the baseline treatment it is 15.41. Refund bonuses become even more profitable if the markup over the project cost is larger, as illustrated in the rightmost column representing a 20% markup. The F6 design joins FE50 and PE20 as being more profitable than the no bonus baseline.

Overall, based on aggregated group and individual behavior our analysis shows that there are no large differences across the bonus treatments, consistent with Hypothesis 3. The more generous bonus designs tend to have a higher success rate, though, which can be attributed to better coordination due to a smaller set of efficient equilibria (Cason and Zubrickas, 2017). The dynamics of group contributions, however, exhibit larger differences across bonus designs that are in line with expected contributing behavior.

7 Conclusion

In this paper, we refine, develop, and stress test the assurance contract with refund bonuses. We first show that, in line with existing empirical evidence, for a fundraising campaign to be successful under the standard assurance contract mechanism contributors need to start cooperating early. To encourage early contributions, we extend the assurance contract mechanism with refund bonuses rewarded only to early contributors in the event of fundraising failure. The experimental results show that our proposed solution is very effective in inducing early cooperation and, consequently, increasing fundraising success. Limiting refund bonuses to early contributors works as well as offering refund bonuses to all potential contributors. Furthermore, limiting the possibility of a refund bonus to early potential contributors increases the appeal of refund bonuses because it greatly reduces the maximum amount that project funders would have to pay in the worst case scenario. Generally, we demonstrate that the increased frequency of successful campaigns generates enough additional value so that refund bonuses can pay for themselves. Thus, our paper provides important evidence that refund bonuses have desirable and practical properties in real world settings like crowdfunding.

The present study deliberately controlled the total project value to always exceed costs in order to isolate the coordination challenge of fundraising. Future experiments could relax this restriction to investigate how refund bonuses affect the ability to screen good from bad projects. Additional experiments could also explore alternative valuation environments to include a common value component to the public good, as well as asymmetric information across potential contributors about the project value. Another useful direction for future research would be to conduct field experiments where campaign operator's can choose to offer or not offer refund bonuses. Since refund bonuses are riskier for less socially valuable campaigns, the use of refund bonuses could signal more socially valuable campaigns. A signal effect would further increase the value of refund bonuses in practice. At the same time, more research is also needed to understand better the effects of refund bonuses on entrepreneurial moral hazard in fundraising.

Appendix A. Model and Proofs

Framework

There is a set $\mathcal{N} = \{1, ..., n\}$ of agents, indexed by $i \in \mathcal{N}$, that can benefit from a public good project. Assume $n \geq 2$. The public good can be provided in a fixed amount. Each agent i has a privately known valuation v_i for the public good. Let individual valuations be independently and identically distributed according to distribution Z over interval $[\underline{v}, \overline{v}]$ with pdf $z > 0$. Let $H(V)$ denote the distribution of the sum of individual valuations, $V = \sum_i v_i$ with the density function $h(V)$. Assume that its inverse hazard rate $\lambda^H(V) = (1 - H(V))/h(V)$ is non-increasing.

Suppose that the project developer, also referred to as the entrepreneur, starts a fundraising campaign where he offers to implement the public good project if paid C. The fundraising campaign runs over a fixed period of time $[0, T]$. During any moment of time agents can make contributions toward the project. Let g_i denote agent i's total contribution. If at the end of the campaign the sum of contributions $G = \sum_i g_i$ is below the target C, then the contributions are refunded and each agent obtains a utility of zero. If $G \geq C$, then the project is implemented out of the contributions made, yielding a utility of $v_i - g_i$ for agent i, $i \in \mathcal{N}$.

Contributions exceeding C are not refunded and do not affect project quality, i.e., they are wasted for agents. It is assumed throughout that it is socially efficient to implement the project with a positive probability or that $H(C) < 1$. It is also assumed that individual valuations do not exceed the cost C, i.e., $C > \overline{v}$.

Let $g_i(t)$ denote agent i's total contribution made from the start of the campaign up to time t and, respectively, let $G(t)$ denote the accumulated total contribution up to time t, $G(t) = \sum_i g_i(t)$. At every moment of time t each agent i observes the accumulated contribution $G(t)$ and can make an additional contribution a_i. We model agent i's contributing strategy as a function $a_i(G(t), g_i(t), t, v_i)$ and his objective is to maximize own expected payoff after accounting for strategies of other agents $\{a_j(G(t), g_j(t), t, v_j)\}_{j \neq i}$. We note that individual contribution $g_i(t)$ is a state variable because it is not a sunk cost as it is repaid in the event of the campaign's failure.

Proof of Proposition 1

Suppose that agents choose contribution strategies $a_i(G(t), g_i(t), t, v_i)$, $i \in \mathcal{N}$, that form Markov Nash equilibrium. In the next lemma, we argue that there is a simple characterization of Markov Nash equilibrium because of the linear cost of contributions and no discounting. (In crowdfunding contributions are collected only at the end of the campaign.)

Lemma 1. *If strategy profile $\{a_i^*(G(t), g_i(t), t, v_i)\}_{i\in\mathcal{N}}$ is Markov Nash equilibrium, then at every moment of time t the resultant continuation contributions $\{\overrightarrow{g}_i^*(G(t), g_i(t), t, v_i)\}_{i\in\mathcal{N}}$, where*

$$\overrightarrow{g}_i^*(G(t), g_i(t), t, v_i) = \int_t^T a_i^*(G(t'), g_i(t'), t', v_i)dt',$$

have to be Bayesian Nash equilibrium of the static contribution game for the remainder of the public good costs $C - G(t)$.

Proof. See Cason and Zubrickas (2019). The proof follows from the linear property of the value function which allows to integrate out instantaneous contributions. The resultant outcome is the optimization problem in continuation contributions only. ■

The linear property of the dynamic contribution game also implies that any Bayesian Nash equilibrium in continuation contributions can be sustained as Markov Nash equilibrium where instantaneous contributions add up to the corresponding equilibrium continuation contributions. Therefore, we can characterize the provision properties of Markov Nash equilibrium by considering the static game in continuation contributions toward the remainder of the public good costs, $C - G(t)$.

The resultant static game is a classical contribution game that has efficient and inefficient equilibria where the latter can arise because of free riding (e.g., any combination of contributions that sum to less than $C - \overline{v}$ makes an equilibrium). Consider an efficient equilibrium with a positive probability of provision. Let a profile of continuation contributions $\{\overrightarrow{g}_i^*(G(t), g_i(t), t, v_i)\}_{i\in\mathcal{N}}$ or just $\{\overrightarrow{g}_i^*\}_{i\in\mathcal{N}}$ for brevity be Bayesian Nash equilibrium of the static contribution game toward the public good cost of $C - G(t)$. We denote the resultant aggregate continuation contribution by $\overrightarrow{G}$, its distribution by $F(\overrightarrow{G})$, density function by $f(\overrightarrow{G})$, and inverse hazard rate by $\lambda(\overrightarrow{G}) = (1 - F(\overrightarrow{G}))/f(\overrightarrow{G})$.

The equilibrium condition implies that for each i the contribution contribution $\overrightarrow{g}_i^*$ maximizes

$$U_i = \max_{\overrightarrow{g}_i}(1 - F(C - G(t)))(v_i - \overrightarrow{g}_i - g_i(t)). \tag{1}$$

In equilibrium, the change in utility from a marginal increase in individual contribution must be zero for each agent i, thus, we have

$$f(C - G(t))(v_i - \overrightarrow{g}_i^* - g_i(t)) - (1 - F(C - G(t))) = 0. \tag{2}$$

The equilibrium individual strategy is given by

$$\overrightarrow{g}_i^* = v_i - g_i(t) - \lambda^F(C - G(t)). \tag{3}$$

The distribution F of the aggregate continuation contribution G is found from

$$\begin{aligned} F(G) &= \Pr(\overrightarrow{G} \leq G) = \Pr(V \leq G + G(t) + n\lambda^F(C - G(t))) \\ &= H(G + G(t) + n\lambda^F(C - G(t))) \end{aligned}$$

The probability density function of F is accordingly given by

$$f(G) = h(G + G(t) + n\lambda^F(C - G(t))). \tag{4}$$

Conditional on $G(t)$ raised, we obtain the probability of non-provision equal to

$$F(C - G(t)) = H(C + n\lambda^F(C - G(t))) \tag{5}$$

the inverse hazard rate equal to

$$\lambda^F(C - G(t)) = \lambda^H(C + n\lambda^F(C - G(t))).$$

As the inverse hazard rate function λ^H is non-increasing, then the equation $x = \lambda^H(C + nx)$ has a unique solution x. Then, we obtain that $\lambda^F(C - G(t))$ is constant for each $G(t)$ and, thus, a constant probability of non-provision determined by (5).

Proof of Proposition 2

Proportional bonus. Consider an assurance contract with proportional refund bonus $r > 0$ where in the event of failure a contributor of g receives the refund bonus rg in addition to the full refund of g. In contradiction to the proposition, suppose that the assurance contract has an equilibrium with the zero probability of provision. This means that the aggregate contribution G is always less than C. But then it must be possible for an agent to increase his refund bonus by marginally increasing his contribution so that $G < C$ continues to hold. Thus, there is no equilibrium with the zero probability of provision. Note that this proof also holds for the case when refund bonuses are paid only for early contributions made over period $[0, T']$ with $T' \leq T$.

Fixed bonus. Consider an assurance contract with fixed refund bonus $b > 0$ payable in the event of failure to contributors with contribution $g \geq C/n$. In contradiction to the proposition, suppose that the assurance contract has an equilibrium with the zero probability of provision. Consider such an equilibrium. Let m be the number of agents who do not receive the bonus and it has to be that $1 \leq m \leq n$. Then, the remaining $n - m$ agents do receive the bonus.

First, suppose that $m = 1$ which implies that the shortfall in total contribution G is at most C/n because $n-1$ agents contributed at least $(n-1)C/n$. Then, the assumption that the public good is efficient with a positive probability implies that the probability of an individual valuation exceeding C/n must be strictly positive, i.e., $Z(C/n) < 1$, where Z is the distribution function of private valuations. Hence, individual rationality implies a positive probability that the $m = 1$ agent will find it optimal to contribute the shortfall of at most C/n. Thus, $m = 1$ is not consistent with the zero probability of provision.

Now, let $m > 1$ and let G^m denote the total contribution made by these m agents. Among these m agents, there must be an agent whose contribution is at most G^m/m.

Then, by individual rationality it must be that the gap between the minimum contribution C/n eligible for the refund bonus and the actual contribution must be larger than the total shortfall for contributions, i.e., it must hold for at least one agent that

$$\frac{C}{n} - \frac{G^m}{m} > C - \frac{C}{n}(n-m) - G^m.$$

Rearranging the last expression and using that $m > 1$, we obtain

$$\frac{G^m}{m} > \frac{C}{n}.$$

But this inequality implies that the agent is eligible for the refund bonus. Thus, we obtain a contradiction. Hence, there is an assurance contract with fixed refund bonuses that has no equilibria with the zero probability of provision.

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Online Appendix

Conditional Cooperation

Table A1: Early Contributions' Influence on Late Contributions in the Baseline Treatment

	Individual Late Contributions (during seconds 61-120)			Any Late Contribution (during seconds 61-120)		
	All	Success	Failure	All	Success	Failure
	(1)	(2)	(3)	(4)	(5)	(6)
Others' Early Contribution (Secs 1-60)	-0.027*	-0.171**	0.021	0.001	-0.010**	0.007*
	(0.012)	(0.019)	(0.022)	(0.002)	(0.002)	(0.003)
Own Early Contribution (Secs 1-60)	-0.213**	-0.314**	-0.288**	-0.011†	-0.021*	-0.013†
	(0.042)	(0.045)	(0.068)	(0.006)	(0.008)	(0.007)
Other controls	Included	Included	Included	Included	Included	Included
Observations	1,700	740	960	1,700	740	960

Note: Random-effects regressions, with standard errors clustered by sessions; robust standard errors are reported in parentheses. "Others' Early Contribution" is the sum of all the contributions made by other subjects during the first half of the campaign and "Own Early Contribution" is the sum of all individual contributions made during the first half of the campaign. "Other controls" are individual value, period, dummy for the second treatment, alternative project information, and the constant. Individual Late Contributions columns display tobit model estimates with censoring at 0. The remaining columns report logit models with a binary dependent variable. ** indicates coefficient is significantly different from zero at the .01 level; * at .05; † at 0.10.

Table A1 reports the results of regressions documenting the effect of early contributions on individual late contributions in the baseline treatment. The key explanatory variable is others' early contributions shown in the first row. For successful campaigns early contributions have a negative impact on both on the amount and the likelihood of late individual contributions (Columns 2 and 5, respectively). This relationship can be explained by the contribution threshold, which implies that for a successful campaign contributors need to increase their contributions later in the campaign if it had a slow start, and vice versa. In contrast, for unsuccessful campaigns we observe a positive effect (Columns 3 and 6 of Table A1). If others were not cooperative early in the campaign then contributors are significantly less likely to make a contribution during the second half of the campaign and their amount contributed is (insignificantly) lower. Such behavior points to hypothesized conditional cooperation as in campaigns without refund bonuses the threat to discontinue later cooperation is credible because of the existence of low-contribution equilibria to which subjects can revert to if others do not cooperate.

Table A2: Early Contributions' Influence on Late Contributions in the P20 Treatment

	Individual Late Contributions (during seconds 61-120)			Any Late Contribution (during seconds 61-120)		
	All (1)	Success (2)	Failure (3)	All (4)	Success (5)	Failure (6)
Others' Early Contribution (Secs 1-60)	-0.109** (0.010)	-0.158** (0.013)	-0.046* (0.020)	-0.006** (0.002)	-0.011** (0.002)	0.001 (0.002)
Own Early Contribution (Secs 1-60)	-0.505** (0.037)	-0.520** (0.043)	-0.499** (0.064)	-0.046** (0.007)	-0.047** (0.008)	-0.040** (0.010)
Other controls	Included	Included	Included	Included	Included	Included
Observations	2,200	1,330	870	2,200	1,330	870

Note: Random-effects regressions, with standard errors clustered by sessions; robust standard errors are reported in parentheses. "Others' Early Contribution" is the sum of all the contributions made by other subjects during the first half of the campaign and "Own Early Contribution" is the sum of all individual contributions made during the first half of the campaign. "Other controls" are individual value, period, dummy for the second treatment, alternative project information, and the constant. Individual Late Contributions columns display tobit model estimates with censoring at 0. The remaining columns report logit models with a binary dependent variable. ** indicates coefficient is significantly different from zero at the .01 level; * at .05; † at 0.10.

Table A2 reports the results of the same regression specifications used in Table A1 but for the P20 treatment. For both successful and unsuccessful campaigns individual late contributions negatively correlate with others' early contributions and a contributor's likelihood of making a late contribution does not depend on others' early contributions in failed campaigns. Thus, consistent with the theory we do not observe conditionally cooperative behavior in the P20 treatment. With bonuses the threat to discontinue later cooperation is no longer credible because the resultant low-contribution outcome is not an equilibrium.

The Role of Early Contributions

Figure A1 highlights the campaign benefits of the early contributions using a series of regression models that predict success based on actual contributions made at various points in time in the baseline treatment. We estimated a series of regression models that estimate the likelihood of campaign success depending on total contributions made

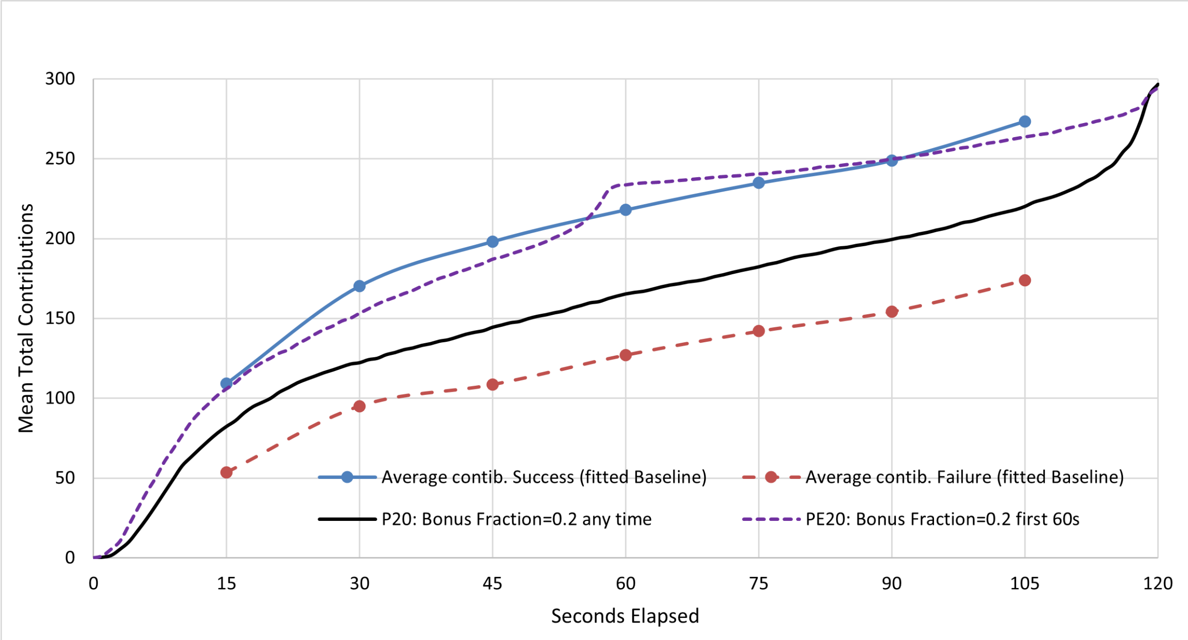


Figure A1: Mean Total Contributions for Successful and Unsuccessful Projects – Model Fitted and Actual Mean Contributions for Refund Bonus Treatments

at 15 seconds, 30 seconds, etc. Of course, higher contributions lead to greater success likelihood. These models also control for the relevant covariates such as the total value of the project, the time trend across periods, and the value and characteristics of the alternative project also receiving contributions.

The solid and dashed lines highlighted with circles indicate the average total contributions made at these time intervals for the campaigns in the baseline that are predicted to succeed with greater then 50% chance (solid line) and those predicted to be more likely to fail than succeed (dashed line). For example, at the 60-second midpoint, the average baseline treatment campaign that is more likely to fail than succeed has raised only 127 while the average raised for predicted sucessful campaigns is 218. The other two lines indicate the average actual accumulated contributions for all P20 and PE20 refund bonus treatments, combining both successful and unsuccessful projects. This highlights the importance of getting on the higher trajectory path for contributions, leading to greater success. The PE20 average roughly tracks the estimated average for successful campaigns, while the P20 average contribution remains well below this level. This indicates that bonuses that are paid exclusively for contributions made early in the contribution window are effective in incentivizing early contributions and putting projects on a more successful trajectory for ultimate funding.

Table A3: Funding Success for Time Remaining after Pivotal

DV: Funding Success	Baseline	P20 Treatment	PE20 Treatment	All Treatments
Time remaining after pivotal	0.0073** (0.0021)	0.0086** (0.0012)	0.0080** (0.0009)	0.0081** (0.0009)
Group value	0.0014 (0.0008)	0.0010* (0.0004)	0.0010† (0.0005)	0.0010** (0.0003)
Standard error of group value	0.0093 (0.0118)	-0.0047 (0.0092)	-0.0031 (0.0053)	-0.0021 (0.0045)
Amount below threshold	-3.855† (1.783)	-0.638 (1.066)	-1.268 (0.927)	-1.514** (0.411)
Period	0.002 (0.005)	0.012† (0.007)	0.011** (0.003)	0.009* (0.003)
Dummy (2nd treatment)	-0.253* (0.092)	0.169** (0.041)	0.041 (0.026)	0.041 (0.030)
Dummy for P20				0.073 (0.061)
Dummy for PE20				-0.018 (0.043)
Constant	-0.235 (0.588)	-0.177 (0.320)	-0.197 (0.315)	-0.161 (0.180)
Overall R-sq	0.384	0.260	0.314	0.293
Observations	111	206	205	522
DV: Seconds to reach pivotal				
Period	0.915* (0.318)	2.508** (0.328)	2.363 ** (0.406)	
Dummy (2nd treatment)	28.01** (7.25)	19.01* (6.53)	26.73** (6.12)	
Constant	67.23** (6.96)	65.96** (6.90)	39.84** (3.17)	
Overall R-sq	0.138	0.240	0.245	
Observations	111	206	205	

Notes: DV abbreviates Dependent Variable. Robust standard errors clustered by sessions are reported in parentheses. ** indicates coefficient is significantly different from zero at the .01 level; * at .05; † at 0.10. Sample restricted to campaigns that became pivotal.

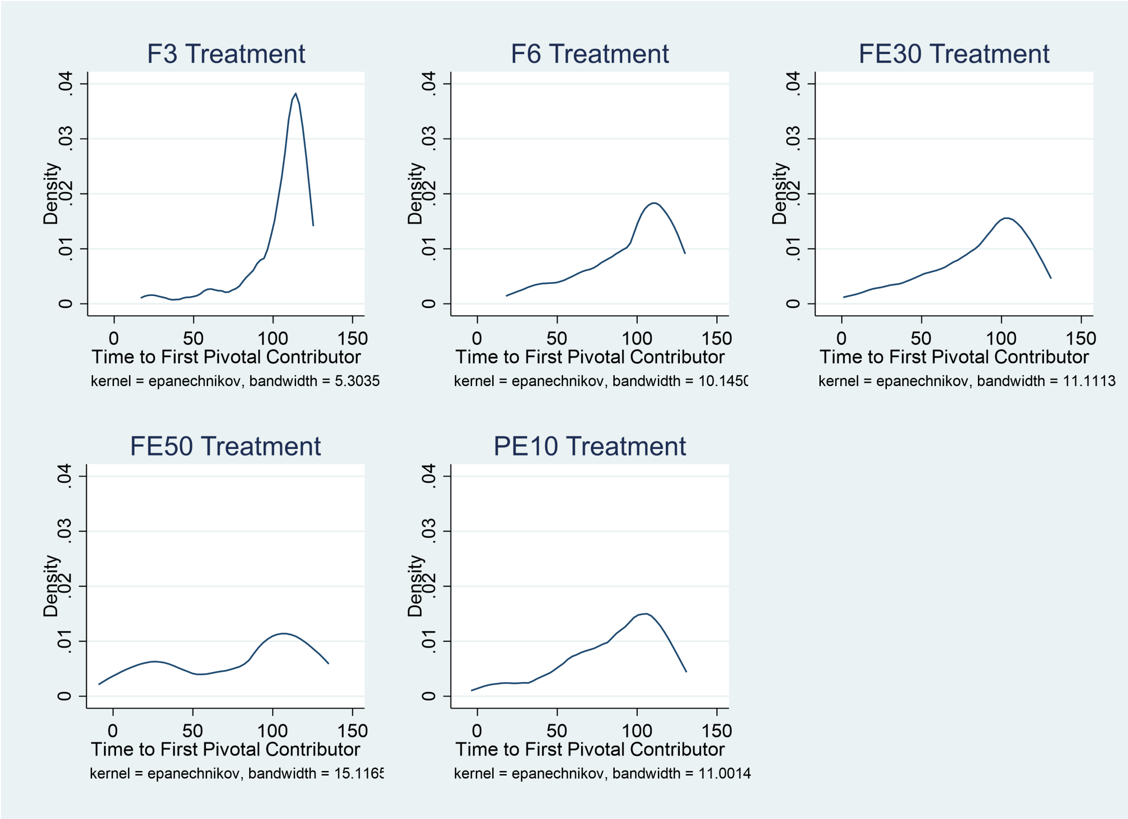


Figure A2: Time Density of Pivotal Points, by Treatment

Pivotalness in Alternative Treatments

Figure A2 shows the time density of pivotal points for the alternative treatments and Table A4 summarizes pivotalness timing. First, note that all designs achieve a high percentage of pivotalness with designs aimed at early contributions achieving pivotalness more quickly (see the bottom three rows of Table A4). Treatment F3 has a modest fixed bonus (3) that is not targeted towards early contributions, and its pivotal timing is considerably later than the other treatments. Larger refund bonuses (F6 or P20 and PE20 from Table 4) increase the salience of pivotalness as we observe higher success rates compared to treatments with lower bonuses (F6 vs F3 and PE20 vs PE10).

Table A4: Timing of Pivotalness for Alternative Treatments

Treatment	Total Campaigns	Reached Pivotalness	Fraction Pivotal	Mean Time to Pivotal (sec)	Median Time to Pivotal (sec)
F3	90	78	0.867	103.7	111.5
F6	90	81	0.900	94.1	105
FE30	90	83	0.922	85.1	92
FE50	90	85	0.944	77.0	91
PE10	90	74	0.822	84.4	92.6

Experiment Instructions (PE Treatments)

Introduction

This experiment is a study of group and individual decision making. The amount of money you earn depends partly on the decisions that you make and thus you should read the instructions carefully. The money you earn will be paid privately to you, in cash, at the end of the experiment. A research foundation has provided the funds for this study.

The experiment is divided into many decision "rounds." You will be paid based on your cumulative earnings across all rounds. Each decision you make is therefore important because it affects the amount of money you earn.

In each decision round you will be grouped with 9 other people, who are sitting in this room. You will make decisions privately, that is, without consulting other group members. Please do not attempt to communicate with other participants in the room during the experiment. If you have a question as we read through the instructions or any time during the experiment, raise your hand and an experimenter will come by to answer it.

Your earnings in the experiment are denominated in experimental dollars, which will be exchanged at a rate of 50 experimental dollars = 1 U.S. dollar at the end of the experiment. At the beginning of the experiment you are given 100 experimental dollars to start. You will add to this amount every round based on decisions you and others in your group make.

Overview

Every decision round you can allocate some experimental dollars to help fund one or two group projects that will benefit you and the other members of your group. If enough money is allocated to a project by all members of your group, the project is funded and you (and all other group members) will each receive an extra payment of some experimental dollars (as explained next). The amount of money, in total, that your group must allocate to fund any project is called the *Threshold*. This *Threshold* amount may be different in different rounds.

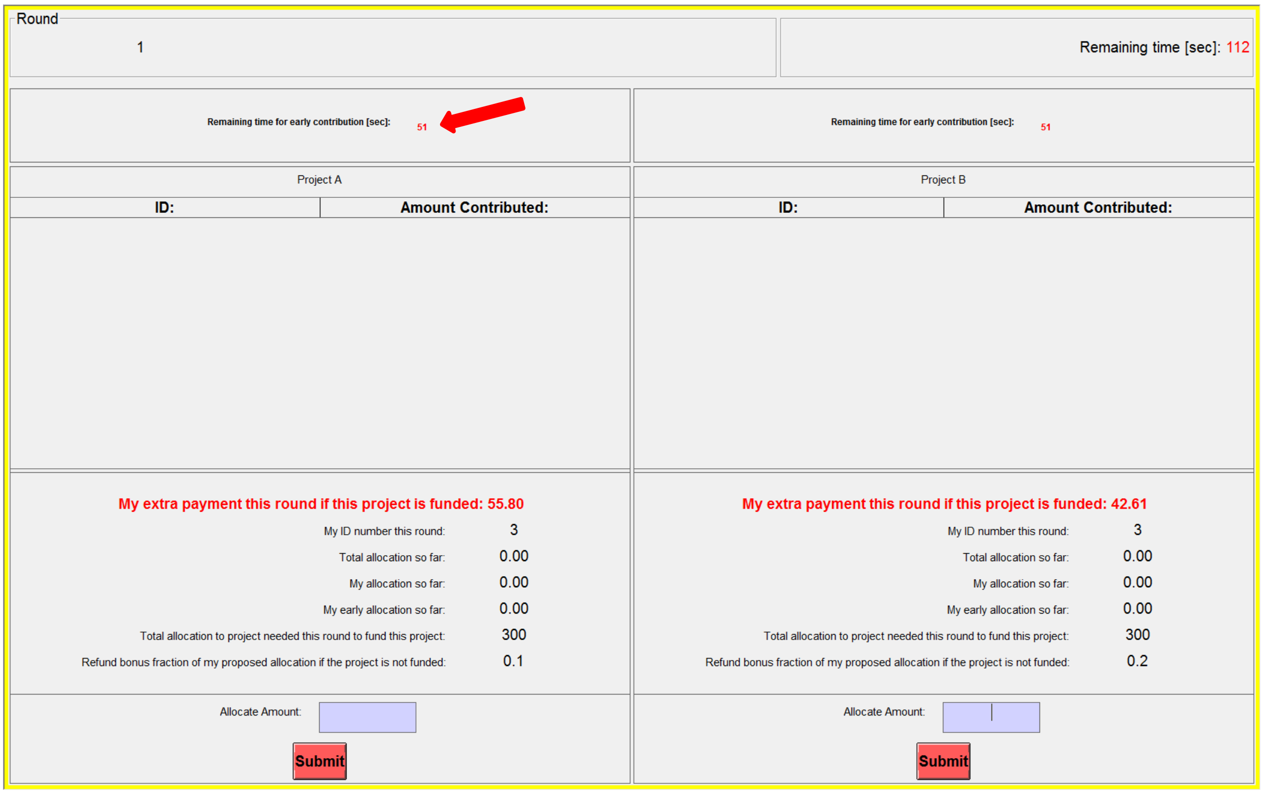
If insufficient money is allocated to a project by all members of your group, then those who tried to allocate money to a project will have their proposed allocation returned. Those individuals who tried to allocate money to a project may also receive a refund bonus. The amount of the refund bonus is a fraction of the proposed amount allocated to a group project, and may be different for different projects.

Your value for the projects

You and everyone else in your group will receive an extra payment of experimental dollars if any project is funded. This amount is determined randomly for each person, for each project, in each round, drawn from the 8001 possible values 20, 20.01, 20.02, ..., 99.98, 99.99, 100. Each of these values between 20 and 100 experimental dollars is equally likely to be chosen for each group member and project in each round. The likelihood that another group member draws any of these values is not affected by the value drawn by any other group member in that round, or in any previous or future rounds. Your values are your private information. You will know your own values, but you will not know the values drawn for any other group member, nor will others know your values.

Your allocation decision

The figure below presents an example screen when two projects are both potentially funded. Everything on the left side of the screen refers to Project A and everything on the right side refers to Project B. When you want to make an allocation to help fund a project during a round you will indicate how much (in experimental dollars) you wish to you will indicate how much (in experimental dollars) you wish to allocate using the fields at the bottom of the screen. Any number between and including 0 up to the *Threshold* that the projects require is an acceptable allocation.



B-2

allocate using the fields at the bottom of the screen. Any number between and including 0 up to the *Threshold* that the projects require is an acceptable allocation.

Proposed allocations can be made at any time while the two-minute countdown clock in a round (shown on the top right of the screen) is active. Your proposed allocation will immediately be displayed to all others in your group as soon as you click Submit, added to the list under either Project A or Project B along with your ID number. The ID numbers for everyone in the group will be randomly re-assigned each round. You can submit multiple allocations within the two-minute time period if you wish.

The lower part of the allocation screen shows the total allocation sum made by all group members, instantly updated following each new allocation. It also updates the total (summed) allocation made by you individually in the round so far. Your extra payment when either of the projects is funded is also shown in red, and note that these are different for Project A and Project B because they are randomly and independently drawn as explained above.

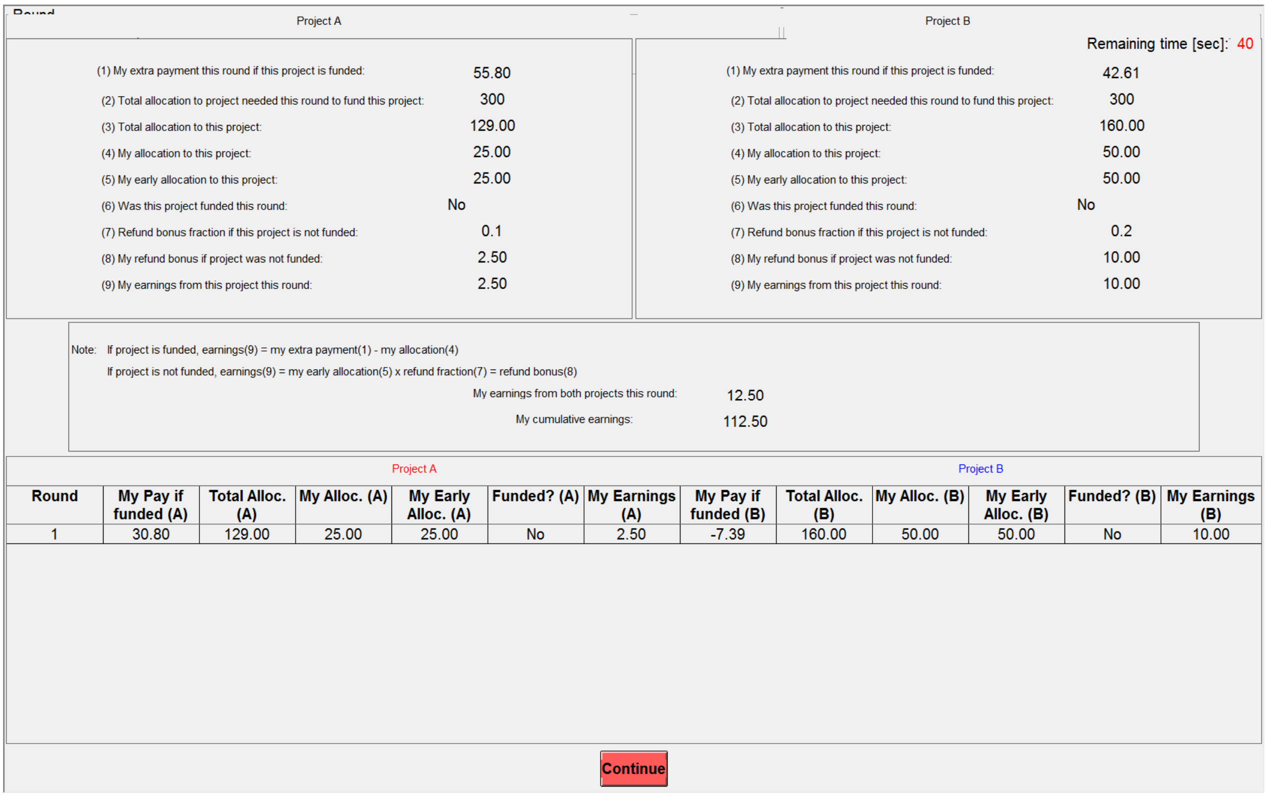
If the total amount of money that your group allocates to fund either project (or both projects) is equal to or greater than the *Threshold*, then you and each of the other group members all receive an extra payment for that project drawn between 20 and 100 as explained above. If the total amount allocated to a project strictly exceeds the *Threshold*, the extra amount above the *Threshold* will not be returned to anyone.

Computing the refund bonus

If the total amount of money that your group allocates to fund a project is less than the *Threshold*, then no group member receives an extra payment for that project. That group project is not funded. All people who allocated money to that project will have their proposed allocation amount returned. They may also receive a refund bonus that is some amount times their proposed allocation to the group project, as long as that proposed allocation is made during the first minute of the round. For example, in the earlier example screen the indicated refund bonus fraction is 0.1 for Project A and the *Threshold* is 300. Suppose that you allocated X to the project during the first minute of the period, and in total all individuals in your group (including you) allocated Y to the project. When $Y < 300$ (so that the threshold to fund the project and to receive the extra payment is not met), you will receive 0.1 times your proposed allocation X made during the first minute as an extra refund bonus.

Adding some completely hypothetical numbers to this example, suppose that you allocated X=40 during the first minute and the other members of your group allocated 190 in total. Therefore $Y=40+190=230<300$. You would receive back all of the amount you tried to allocate to the project, and would also receive a refund bonus of $(0.1)\times 40 = 4$ experimental dollars based on the X=40 you tried to allocate during the first minute of the round. Notice that individuals who tried to allocate more to the project during the first minute get a larger refund bonus. For example, a person who tried to allocate 80 during the first minute in this hypothetical example would receive a refund bonus of $(0.1) \times 80 = 8$ experimental dollars.

The red arrow in the figure above highlights where the amount of time remaining in the early allocation period is shown on screen, for which allocations are eligible for the refund bonus. When this timer reaches zero, later allocations are not eligible for the project, the refund bonus you receive if the group project threshold is not met, and your earnings for the round. Your cumulative earnings will also be shown, and a table will also display the key results from every previous round.



refund bonus.

End of the round

B-4

At the end of every decision round, as illustrated in the figure below your computer will display the total amount allocated to the group projects by members of your group. The results screen will also display whether the project was funded, your early period and total allocation to the project, the refund bonus you receive if the group project threshold is not met, and your earnings for the round. Your cumulative earnings will also be shown, and a table will also display the key results from every previous round.

What might change in different rounds?

The experimenter will make a verbal announcement when any payoff rules change during the experiment.

As already noted, the *Threshold* may be different across rounds or for different projects.

In some rounds the refund bonus fraction (0.1 in the earlier example) may be a different number, or may be 0 (giving NO REFUND BONUS) for one or both projects.

Summary

1. You will make allocation decisions in many decision rounds.
2. Group members' ID labels are randomly-determined each round, and therefore typically change from round to round. Each group always contains the same 10 members.
3. Group members make allocations to one or two group projects at any time (and as many times as they want) during the two minutes in a round.
4. If the total amount allocated in your group is $\geq$ *Threshold* for any project, you receive an extra payment. The other members of your group also receive extra payments.
5. The extra payments are drawn independently from the range between 20 and 100 experimental dollars, and each amount in this range is equally likely.
6. You should pay close attention to the "Total allocation so far" made to each project by the group. Any allocations above the *Threshold* needed to fund the project are wasted (never returned) and can only reduce your earnings.
7. If the total amount allocated to a project is $<$ *Threshold*, everyone's proposed allocation to that project is returned. Everyone may also receive a refund bonus that is equal to some fraction times his or her proposed allocation made during the first minute of the round. (This fraction could be 0, providing NO refund bonus in some rounds for some projects.)
8. The refund fraction can be different for different projects.