Early Refund Bonuses Increase Successful Crowdfunding*

Timothy N. Cason, Alex Tabarrok and Robertas Zubrickas

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Abstract

The assurance contract mechanism is often used to crowdfund public goods. This mechanism has weak implementation properties that can lead to miscoordination and failure to produce socially valuable projects. 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 increasing fundraising success. Limiting refund bonuses to early contributors works as well as offering refund bonuses to all potential contributors, while also reducing the amount of bonuses paid. We find that refund bonuses can increase the rate of campaign success by 50% or more. Moreover, we find that even taking into account campaign failures, refund bonuses can be financially self-sustainable suggesting the real world value of extending assurance contracts with refund bonuses.

Keywords: Public goods, donations, assurance contract, free riding, conditional cooperation, early contributions, refund bonuses, experiment, laboratory.

JEL Classification: C72, C92, H41.

*Cason: Krannert School of Management, Purdue University, West Lafayette, IN 47907-2056, USA, cason@purdue.edu; Tabarrok: George Mason University, Department of Economics and Center for Study of Public Choice, Fairfax, VA 22030, USA, atabarro@gmu.edu; Zubrickas: University of Bath, Department of Economics, Bath BA2 7AY, UK, r.zubrickas@bath.ac.uk. We thank Jacob Brindley and Peter Wagner for excellent research assistance, and two anonymous reviewers, an advisory editor, and various seminar and conference audiences for valuable comments. We especially thank Yancey Strickler, co-founder of Kickstarter, and Jon Leland, Vice President of Insights, Kickstarter, for very useful comments. The Eric and Wendy Schmidt Fund for Strategic Innovation provided financial support.

1 Introduction

It is widely acknowledged that early contributions are critical for successfully crowdfunding public goods, as they reinforce donors' willingness to contribute in the later stages of the campaign. Benjamin Franklin (1791) famously gave this advice to crowdfunders:

> "[I]n the first place, I advise you to apply to all those whom you know will give something; next, to those whom you are uncertain whether they will give any thing or not, *and show them the list of those who have given*; and, lastly, do not neglect those who you are sure will give nothing, for in some of them you may be mistaken." (p. 189, italics added).

Franklin's advice finds support in the modern literature. Mollick (2014), for example, observes that each ten-fold increase in the number of Facebook friends of founders doubles the chances of a successful crowdfunding campaign, whereas Agrawal et al. (2015) and Colombo et al. (2015) attribute the success factor of social capital to its effect on raising early contributions.[1] Similarly, Andreoni (1998) demonstrates the advantages of seed money for a successful campaign.[2]

But seed money and social capital are limited. As a result, Franklin was generous with his advice but when asked for "a list of the names of persons [he] knew by experience to be generous and public-spirited" he refused. Franklin argued that frequent solicitations would make the potential donors disagreeable and no longer willing to support Franklin's projects. Since social capital is a depletable resource, its capacity to encourage early contributions is limited. In this paper, we instead offer a novel mechanism to encourage early contributions and increase crowdfunding success.

In practice, the main method of crowdfunding public goods is the assurance contract mechanism where contributions are refunded to donors if a target funding goal is not

[1]See Belleflamme et al. (2015) and Cai et al. (2021) for literature reviews on the role of social capital in crowdfunding.

[2]For more on the importance of seed money in public good provision, see Vesterlund (2003) or List and Lucking-Reiley (2002), who, for example, show in a field experiment that the number of contributors to a charity and the size of contributions increase with greater seed money. For evidence on the importance of early contributions for crowdfunding, see Bøg et al. (2012), Etter et al. (2013), Wash (2013), Koning and Model (2014), van de Rijt et al. (2014), Solomon et al. (2015), and Li et al. (2020).

reached. The assurance provided by refunds encourages contributions (Bagnoli and Lipman, 1989; Admati and Perry, 1991) and we argue that the refund policy can be designed in ways that allow achieving specific goals. At the base of our designs lies the assurance contract with refund bonuses introduced by Tabarrok (1998) and Zubrickas (2014). Its main idea is to offer an additional refund bonus if the campaign *fails* to people who agreed to contribute. In other words, if the fundraising campaign misses the target, the contributors who offered funds are not only fully refunded but also receive bonuses. In a similar way to deposit insurance that prevents bank runs but is never paid out in equilibrium (Diamond and Dybvig, 1983), refund bonuses prevent inefficient fundraising equilibria and are never paid out in equilibrium for worthy campaigns.[3] That refund bonuses lie on the off-the-equilibrium path gives us the freedom of designing bonus schemes which, in particular, can be directed at encouraging early contributions.

In the theoretical part of the paper, we provide insight into the question of why early contributions affect the rate of success in public good fundraising. In the context of threshold public goods with dynamic contributions, there are two main theories about the role of early contributions. First, as Kessing (2007) and Cvitanić and Georgiadis (2016) show, early and continuation contributions can be strategic complements. An early contribution increases the probability of success and, in turn, the marginal value of subsequent contributions. The second theory relates the role of early contributions to conditionally cooperative behavior that can arise in a dynamic environment with multiple equilibria. Donors can adopt tit-for-tat strategies by conditioning later cooperation on earlier cooperation of others.[4] We show that the theory of strategic complements cannot explain the efficacy of early contributions in the typical assurance contract game applied in crowdfunding. In this game, contribution costs are linear, there is no discounting because contributions are released only at the end of the campaign, and earlier contributions are not sunk costs because of the refund policy. In particular, we show that all

[3]The idea of refund bonuses can be linked to the augmented revelation principle of Mookherjee and Reichelstein (1990), where side (off-the-equilibrium-path) payments are designed to eliminate undesirable equilibria.

[4]Using the data from the influential work on conditional cooperation by Fischbacher et al. (2001) and its 17 replication studies, Thöni and Volk (2018) demonstrate that 62% of contributors in laboratory public good games are conditional cooperators.

efficient Markov Nash equilibria have the same probability of provision irrespective of the dynamics of contributions or, put differently, the importance of early contributions does not follow from payoff relevance. Hence, if early contributions are found to affect the rate of success in an environment with refunds and no discounting, this effect has to follow from conditionally cooperative behavior.[5] In other words, we postulate, similar to the explanations of conditional cooperation based on social norms (Sugden, 1984, Bernheim, 1994; also see Bigoni et al., 2015), that early contributions matter because players view them as a signal about free riding and the level of cooperation and they condition subsequent contributions upon this signal. The experimental results are consistent with this postulate.

In the experimental part of the paper, we focus on 20% refund bonuses that are only offered for the contributions made in the first half of the campaign. We contrast resultant contributing behavior against that when (i) no bonuses are offered (the baseline treatment) and (ii) refund bonuses are offered for all the contributions made at any time during the campaign. We conduct our experiment on a lab-based fundraising platform with many main features of real-life crowdfunding such as asynchronous multiple contribution pledges over continuous time, constant updating of individual and aggregate pledge amounts until a fixed deadline, and simultaneously launched multiple fundraising campaigns. Each campaign lasts for two minutes, during which ten participating subjects can pledge their (multiple) contributions without any timing restrictions. Subjects' valuations for the public good are their private information.

In line with the empirical patterns of crowdfunding, we observe that in the baseline (no bonus) treatment successful and unsuccessful campaigns differ in the trajectories of contributions over time. If contributions are sluggish to kick off, they will fail, and typically by a large margin, to reach the funding target. This observation demonstrates the relevance of inefficient (low contribution) equilibria. Empirical analysis also suggests that in the baseline treatment equilibrium coordination can be closely linked with conditional

[5]There is also an informational channel for the role of early contributions that can create an information cascade in contributing behavior. Here we abstract from this by assuming that contributors are perfectly informed about their private valuation of the public good.

cooperation. Specifically, in successful campaigns the median subject makes two one-time contributions compared to a single median contribution in unsuccessful campaigns. Furthermore, we do not observe a higher occurrence of late contributions in an attempt to make up for a low half-time accumulated aggregate contribution. At the same time, the half-time accumulated contribution is an important predictor of the campaign's success. Overall, in the baseline treatment if subjects do not start cooperating early, they do not cooperate at all.

Our main experimental finding is that refund bonuses for early contributions increase the success rate by over 50% relative to the baseline. Refund bonuses push contributions from the baseline trajectory to the succesful campaign trajectory. Importantly, cooperation does not cease in the second half of the campaign when contributions are no longer eligible for bonuses. This suggests that subjects continue playing efficient equilibrium strategies upon observing high levels of earlier cooperation irrespective of incentives used to induce such cooperation. The increase in the success rate generates sufficient returns to compensate for the costs of refund bonuses paid for unsuccessful campaigns. When all contributions, not just early contributions, are eligible for refund bonuses, there is a flurry of activity toward the end of the campaign. These refund bonus campaigns also increase success rates but last-moment contributions can result in last-moment miscoordination and, hence, campaign failures and bonus payments. Refund bonuses restricted to early contributions, by contrast, improve the coordination mechanism by setting it to work earlier in the campaign, the advantage of which is significant savings on refund bonus costs. Lastly, we also find that refund bonus treatments can have better distributive efficiency properties than the baseline.

We also consider several other refund bonus designs aimed at encouraging early cooperation. Lowering the rate of refund bonuses from 20% to 10% results in a lower success rate. In other schemes, refund bonuses are offered in a fixed amount and offered in a fixed amount only to the earliest contributors who make contributions of at least a prespecified minimum level. All the refund bonus schemes tend to work well but we observe that schemes inducing more early contributions tend to perform best, supporting our general claim about the importance of encouraging more early cooperation.

The first experimental study on refund bonuses is Cason and Zubrickas (2017). It considers a static environment and focuses on implementation properties related to information, bonus size, and group size. Cason and Zubrickas (2019) reports results for an experiment with a dynamic environment similar to the one studied here, but for different refund bonus treatments and a variable number of projects available for funding. In particular, it considers proportional bonuses only that are paid for any contribution made during the entire fundraising time period, whereas this new experiment considers completely new bonus schemes to promote early contributions. Unlike the present study, the previous study did not perform a treatment comparison of distributive efficiency. See also Chandra et al. (2016) for an application of the refund-bonus mechanism. Generally, pecuniary incentives for encouraging contributions for public goods appear in a number of papers, e.g., Varian (1994), Falkinger (1996), Morgan (2000), Goeree et al. (2005), Gerber and Wichardt (2009), and Yang et al. (2018). The distinguishing feature of refund bonuses is that they are a simple and practical extension of the already widely used crowdfunding mechanism.

In the current study, by promoting early contributions we achieve an even higher success rate than in previous studies and at significantly lower costs of refund bonuses. Funders may be reluctant to risk some of their own capital to offer refund bonuses so lowering the cost of refund bonuses is important to encourage crowdfunders to adopt the mechanism in practice.

The remainder of this paper is organized as follows. In Section 2, we discuss theory and formulate hypotheses. In Section 3, we present the design of the experiment, the results of which we discuss in Sections 4 and 5. In Section 6 we discuss experimental results from alternative bonus designs.

2 Theory and Hypotheses

In this section, we discuss theoretical properties of the standard assurance contract and provide motivation for refund bonuses. The formal details are provided in Appendix A.

Consider a community with a potential threshold public good project. Community members have privately known valuations of the public good which are independently and identically distributed according to a known distribution. We assume that the highest possible individual valuation is less than the cost of the project, C, so collective action is necessary to produce the public good. The community launches a fundraising campaign for the project with an assurance contract. The campaign runs for a period of time over which community members can make (multiple) contribution pledges. At any given moment of time, members can observe the total accumulated contribution. Contributions are collected at the end of the campaign only if the target for contributions, C, is reached. If the target is not reached, then contributions are not collected. In the assurance contract with refund bonuses, if the target is not reached contributors also receive refund bonuses. In the main experiment we implement refund bonuses that are proportional to the contributions pledged, but we also consider refund bonuses of a fixed size and that are paid for contributions equal to or above a pre-determined level.

2.1 Assurance Contract

The assurance contract creates the problem of dynamic provision for a threshold public good. In line with related studies (Kessing, 2007, Cvitanić and Georgiadis, 2016), we formally analyze this problem under the assumption that contributors play Markov (payoff-relevant) strategies. We say that an equilibrium is inefficient if the probability of provision is zero and efficient if the probability of provision is positive. In Proposition 1, we present equilibrium properties of the (standard) assurance contract without refund bonuses.

Proposition 1. *For the assurance contract without refund bonuses, (i) there are efficient and inefficient equilibria; (ii) all efficient equilibria have the same probability of provision.*

While part (i) of Proposition 1 is a well-known result in the literature on public goods, part (ii) is new, to the best of our knowledge. It says that the probability of provision is path-independent or, in other words, early contributions should not affect the rate of provision when agents choose Markov strategies. The reason behind this finding is that early contributions are not sunk when contributions are refunded in the event of failure. An early contribution not only brings the accumulated contribution closer to the funding target, prompting others to contribute, but it effectively reduces the contributor's private valuation for the remaining part of the public good, which lowers his incentive to contribute further. The linear cost structure together with no discounting (contributors make payments only at the end of the campaign) precludes the emergence of strategic complementarities between early and late contributions.

For early contributions to play a distinctive role for public good provision, their effect must stem from sources other than payoff relevance. In a dynamic setting, one such source can be the multiplicity of equilibrium outcomes (part (i) of Proposition 1), which can support a richer set of strategies than those embodied by payoff relevance. In particular, contributors can employ tit-for-tat strategies by conditioning their further cooperation on the degree of cooperation observed earlier in the campaign. The threat of discontinuation of later cooperation is credible because of the existence of low-contribution equilibria.[6] From a different perspective, the role of early contributions is to signal cooperative intentions in order to avert the formation of free riding beliefs. While all efficient equilibria lead to the same aggregate outcome, their dynamics of contribution accumulation can be very different. In some equilibria contributors can start contributing early in the campaign, but in other equilibria – only late. Since inefficient low-contribution equilibria also have low levels of early contributions, a sluggish start can be interpreted as contributors' free riding rather than postponing contributions to later stages of the campaign.

Hence, based on conditional-cooperation considerations that arise from the multiplicity of equilibrium outcomes, we have

[6] It is straightforward to formalize such strategies and resultant equilibrium play; see, e.g., Kreps et al. (1982) for an approach. Also see Bigoni et al. (2015) for an example and empirical evidence and Sugden (1984) and Bernheim (1994) for explanations of conditional cooperation based on social norms.

Hypothesis 1. *[Conditional cooperation] Greater early contributions increase campaign success.*

Evidence in favor of Hypothesis 1 would indicate conditional cooperation as a primary contributing factor since explanations based on payoff relevance are ruled out (part (ii) of Proposition 1). Such evidence would indicate the importance of behavioral factors that encourage early contributions. At the same time, the rejection of Hypothesis 1 would be evidence in favor of payoff relevance, which would then highlight the importance of improving implementation properties like eliminating inefficient equilibria. As we discuss in the next subsection, refund bonuses can be applied to both tasks.

2.2 Refund Bonuses

The next proposition shows that refund bonuses can be designed to eliminate inefficient equilibria. The outcome with zero probability of provision cannot be an equilibrium because in such a situation there is always a person who could benefit from an increase in his contribution either because of the refund bonus (or a larger refund bonus in the case of proportional refund bonuses) or because of the provision of the public good.

Proposition 2. *There is an assurance contract with refund bonus, proportional and/or fixed, that has no inefficient equilibria.*

The elimination of inefficient low-contribution equilibria implies that we should observe more provision compared to the case without bonuses. Fundraising campaigns with refund bonuses can still fail even when it is efficient to provide the public good because there is a coordination problem among *efficient* equilibria which cannot be fully remedied by refund bonuses.[7] Therefore, the second implication of refund bonuses is a smaller shortfall in contributions for unsuccessful campaigns. This implication would be indica-

[7]In some cases, refund bonuses can also eliminate or reduce coordination problems among efficient equilibria by reducing the number of such equilibria. In the case of a homogeneous group when every contribution is necessary, Tabarrok (1998) designs a fixed bonus scheme under which contribution is a dominant strategy. For a heterogeneous group but without aggregate uncertainty, Zubrickas (2014) shows that it is possible to design a proportional refund bonus rule that leads to a unique efficient equilibrium.

tive of whether the difference in provision rates is due to the existence of low-contribution equilibria in campaigns without refund bonuses. Thus,

Hypothesis 2. *(i) Refund bonuses increase the rate of provision of fundraising campaigns, and (ii) unsuccessful campaigns receive more pledged contributions when refund bonuses are offered.*

The distinctive feature of refund bonuses is that their payment lies on the off-the-equilibrium path, which allows us to design bonus schemes aimed at specific objectives. Given our hypothesis that early contributions can matter for provision success, we study bonus schemes designed for the purpose of encouraging early contributions. Our focus is on a scheme that gives proportional refund bonuses to the contributions made in the first half of the campaign. This scheme also precludes inefficient equilibria as otherwise contributors could have increased their bonuses by contributing early rather than later. Thus, based on the fact that all equilibria are efficient with refund bonuses, we have

Hypothesis 3. *The rate of provision does not differ among refund bonus designs.*

3 Experimental Design

Subjects' preferences over public goods, termed "projects" in the instructions, were controlled using randomly drawn and private induced values. Subjects were assigned to ten-person groups, and each period every individual received an independent value drawn for each project from $U[20, 100]$.[8] The threshold for funding each project was fixed at $C = 300$ experimental dollars. The average aggregate project value across all 10 contributors (600) exceeds the project cost, and the realized minimum aggregate project value (based on the actual random individual draws) was 469. So all projects were efficient to fund.[9] If aggregate contributions during the two-minute funding window reached the

[8] Diederich et al. (2016) show that achieving efficiency in public good provision with a group size of 10 can be as challenging as with a group size of 40 or 100.

[9] Subjects were not told explicity that all projects provide a positive net benefit to the group, but they could infer that this is highly likely. It was common knowledge that the average aggregate value is double the funding threshold of 300.

threshold of 300, every group member received his or her drawn value for that project irrespective of their own contribution. Contributions in excess of the threshold were not refunded and did not affect project quality. Therefore, net subject earnings for successfully funded projects simply equaled their drawn project value minus their own total contribution.

The contribution mechanism operated in continuous time, and individuals could make contributions at any moment while a two-minute timer counted down to a hard close. They could make as many contributions, in whatever amounts they desired, during this window. Contributions could not be withdrawn. The individual contributions were instantly displayed to all nine others in the group on an onscreen table listing. This provides a simple approximation to the information provided by online crowdfunding sites, where projects often display how many individual contributions fall into various ranges. In addition, subjects' screens displayed the total contribution sum raised at that moment, next to the target contribution threshold (300). The screen also continuously updated the individual's own total contribution for the period, summed across their (potentially multiple) contribution amounts.

The experiment employed a baseline treatment with no refund bonus, and alternative versions of the refund bonus. As with most crowdfunding sites in the field, contributions were refunded when the funding threshold was not reached. The main experiment presented in Sections 4 and 5 included the baseline treatment (no refund bonus) and two versions of a proportional refund bonus, where the extra bonus amount is a proportion of the individuals' attempted contribution. The treatments differed in whether the 20% proportional bonus was paid for contributions made at any time (P20) or only for early contributions (PE20):

Baseline: No refund bonus; only refund of attempted contribution when the funding threshold is not reached. (10 groups of 10 subjects.)

P20: Proportional refund bonus $r = 0.20$ paid on contributions made at *any time* of the two-minute contribution window. (13 groups of 10 subjects.)

PE20: Proportional refund bonus $r = 0.20$ paid on contributions made *during the first minute* of the two-minute contribution window. (11 groups of 10 subjects.)

In every period two alternative projects were available for potential contributions, with differing refund bonus rules for each, in order to investigate whether coordination difficulties caused by multiple projects affect the performance of refund bonuses. This also captures a key aspect of crowdfunding in the field, where potential contributors can choose among multiple projects available for support. Subjects' project value draws for these two projects were independent. Both projects or one project could be funded successfully. The experiment instructions shown in the online appendix include an image of the contribution screen, which always showed both projects available for contributions.

Some sessions included 30 periods, with a variation in the treatment conditions once within the session after 15 periods. Following an Advisory Editor's suggestion, eight later sessions eliminated the mid-session treatment change and simply conducted one treatment configuration for 20 total periods. The data analysis accounts for the period number and treatment ordering to verify that the main conclusions are not sensitive to these small procedural variations. We did not include alternative projects with identical refund bonus conditions, or both with no refund bonus, because previous research (Corazzini et al., 2015, 2020; Ansink et al., 2017; Cason and Zubrickas, 2019) has already investigated coordination and contributions to multiple projects with similar or identical characteristics.

The paper overall reports data from a total of 280 subjects, which includes decisions made by 200 new subjects along with 80 subjects from a subset of sessions and treatments reported in Cason and Zubrickas (2019).[10] All sessions were conducted at the Vernon Smith Experimental Economics Laboratory at Purdue University, using z-Tree (Fischbacher, 2007). Subjects were undergraduate students, recruited across different

[10] In particular, the main experiment includes 8 groups of 10 subjects from Cason and Zubrickas (2019) in the P20 treatment, half conducted alongside the baseline (no bonus) treatment and half with an alternative lower ($r = 0.10$) refund bonus treatment P10. To these 8 P20 groups we added 5 more groups, 1 conducted with a baseline alternative and 4 conducted with treatment PE20 as the alternative project. We also included 7 additional groups of 10 subjects in the PE20 treatment, 5 conducted along with the baseline treatment and 2 conducted with a PE10 treatment as the alternative, which paid a $r = 0.10$ refund bonus for contributions made in the first half of the contribution window.

disciplines at the university by email using ORSEE (Griener, 2015), and no subject participated in more than one session.

At the beginning of each experimental session an experimenter read the instructions aloud while subjects followed along on their own copy. Appendix B presents this exact instructions script. Earnings in the experiment are denominated in experimental dollars, and these are converted to U.S. dollars at a pre-announced 50-to-1 conversion rate. Subjects are paid for all project rounds and also received a US$5.00 fixed participation payment, and their total earnings averaged US$26.25 each. Sessions usually lasted about 60 to 90 minutes, including the time taken for instructions and payment distribution.

4 Results

We present the results on the baseline, refund bonus (P20), and refund bonus for early contributions (PE20) treatments in four subsections. Subsection 4.1 presents the overall treatment comparisons on the project funding rate and individual contributions. Subsection 4.2 provides additional details of individual and group contributions across treatments. Subsection 4.3 investigates reasons for campaign failures in the baseline and P20 treatments and the role of early contributions. In Subsection 4.4 we discuss the advantages of the PE20 bonus design for mitigating the reasons for campaign failures.

4.1 Treatment Comparisons

Table 1 summarizes the funding rates for the three experimental treatments. In the baseline treatment without any refund bonuses, less than one-half of the projects are funded, whereas over 60 percent of projects are funded with refund bonuses. Comparing the baseline treatment with our early contribution refund bonus (PE20) shows that the early refund bonus increased success rates by more than 50% (23.5 percentage points). Based on average success rates calculated across independent groups of 10 subjects, a nonparametric Mann-Whitney test indicates that both refund bonus treatments have a higher success frequency than the baseline (for P20, p-value $= 0.024$, $n = 13, m = 10$;

Table 1: Funding Frequency and Average Shortfall

Treatment	Funding Frequency	Shortfall (std. error)
Baseline	$74/170 = 43.5\%$	86.2 (6.2)
P20	$133/220 = 60.5\%$	29.0 (2.9)
PE20	$154/230 = 67.0\%$	49.8 (3.8)

for PE20, p-value $= 0.005$, $n = 11, m = 10$). Success rates are not significantly different, however, for the two refund bonus treatments (Mann-Whitney p-value $= 0.120$).

Part (ii) of Hypothesis 2 states that unsuccessful campaigns in the baseline (no bonus) condition should receive less pledged contributions than those with refund bonuses. The rightmost column of Table 1 provides clear support for this prediction. Without refund bonuses average contributions are more than 86 experimental dollars below the funding threshold of 300, and this large shortfall is nearly two to three times greater than the average shortfall in the treatments with refund bonuses.

Table 2 reports two regressions that test whether the refund bonus treatments lead to significantly greater contributions and funding performance relative to the baseline. The first column reports a random effects linear probability model of funding success, with refund bonus treatment dummy variables to document differences in funding likelihood.[11] The no-refund baseline treatment is the omitted case. The model also includes as a regressor the total value of the project, summed across all 10 members of the group (Group Value), which indicates a significantly greater funding likelihood for more valuable projects. The Period variable and a dummy variable representing the second treatment of the session account for the time trend. The funding success rate tends to decrease over time in all treatments, which reflects an increase in miscoordination in the final seconds of the contribution window. As we document later, subjects increasingly concentrate their contributions in the final seconds as they wait for others to contribute, which can lead to greater variance in success frequency and partly explain the low value of the R-squared statistic in the "Funding Success" column. The regression also includes characteristics of

[11] A random effects logit model leads to identical conclusions, so we report the LPM since the coefficients are simple to interpret. See also Gomila (2020).

Table 2: Funding Success and Individual Contributions

	Funding Success	Individual Contributions
Dummy for P20	0.121*	4.882*
	(0.062)	(2.162)
Dummy for PE20	0.189**	5.869*
	(0.067)	(2.337)
Group Value	0.003**	
	(0.0003)	
Individual Value		0.407**
		(0.011)
Period	-0.009**	-0.065
	(0.003)	(0.049)
Dummy (2nd treatment)	-0.114	1.251
	(0.081)	(1.830)
Alternative Project Information	Included	Included
Constant	-0.688*	1.869
	(0.300)	(2.794)
Overall R-sq	0.175	
Observations	620	6200

Note: Random-effects regressions, with standard errors reported in parentheses. Individual Contributions column displays tobit model estimates with censoring at 0. ** indicates coefficient is significantly different from zero at the .01 level; * at .05.

the other project seeking contributions contemporaneously; specifically, the value of this other project and the type of refund bonus treatment (if any). These terms are typically not significantly different from zero and so they are suppressed in the table.

Both of the coefficient estimates on the refund bonus treatments are significantly positive, consistent with an increased funding likelihood identified above in the nonparametric tests. This provides support for Hypothesis 2(i). The PE20 version of the bonus, which pays a higher proportional refund bonus ($r = 0.20$) for contributions made during the first 60 seconds of the period, appears to perform the best. But a comparison with the P20 refund bonus indicates no significant difference between the PE20 and P20 coefficient estimates (Chi-squared p-value = 0.304).

The second column of Table 2 employs a different dependent variable, replacing funding success with individual contributions, aggregated across the two-minute contribution window for each individual in each period. About 9 percent of individual contributions are 0, so this is estimated as a tobit model. The estimates provide similar conclusions regarding the benefit of including refund bonuses. Similar to the funding success estimates, the two refund bonus treatments do not have significantly different impacts on individual contributions (Chi-squared p-value = 0.667). Results are similar for an alternative specification that interacts the refund bonus treatment with the individual project value to allow for differential impacts of project value across treatments.

This initial treatment comparison provides support for the main implication of refund bonuses: Bonuses raise the rate of provision by eliminating inefficient, low-contribution equilibria as observed by larger amounts pledged for unsuccessful campaigns (Hypothesis 2). Moreover, the specific design of the refund bonuses, and in particular their timing, does not matter for success. This is consistent with the prediction of Hypothesis 3. As we will document later, however, the longer time period for bonus-eligible contributions for P20 leads to significantly larger bonus payments for unsuccessful projects, and also reduces fundraiser returns. Encouraging early contributions through targeted bonuses is more cost-effective.

4.2 Individual and Group Contributions

In this subsection, we document patterns of individual and group contributions across treatments and over time. Recall that individuals could choose when and how often to pledge contributions to the projects at any time during the contribution window.

Table 3 contrasts individual contributing behavior across successful and unsuccessful projects and across treatments. First, we note little difference across treatments for successful projects shown on the right side: the average total individual contribution is slightly above 30, on average less than one person fully free rides, and the median contributor makes two contributions during the campaign. The only sizable difference across treatments lies in the amounts of contributions raised in the first 60 seconds (last column). Unsurprisingly, under the PE20 treatment more early contributions are raised.

Table 3: Individual Contributions: Amount, Frequency, and Free Riding

Treatment	Unsuccessful projects				Successful projects			
	$\sum a_i$	$\lvert \sum a_i = 0 \rvert$	# of a_i	$0 - 60''$	$\sum a_i$	$\lvert \sum a_i = 0 \rvert$	# of a_i	$0 - 60''$
Baseline	21.4	0.218	1	128.9	30.8	0.085	2	209.4
P20	27.1	0.082	2	131.8	31.4	0.071	2	187.3
PE20	25.0	0.079	2	197.6	31.7	0.048	2	251.6

Note: $\sum a_i$ stands for the mean sum of one-time individual contributions made over the contribution window, $\lvert \sum a_i = 0 \rvert$ for the share of subjects with zero contributions, # of a_i for the median number of one-time individual contributions, and $0 - 60''$ for the mean sum of one-time individual contributions made over the first 60 seconds of the campaign.

Second, less similarity exists across treatments for unsuccessful projects and the baseline treatment clearly stands out. Compared to other treatments and own performance for successful projects, the baseline treatment features the largest drop in the mean sum of individual contributions. This drop can be related to the increase in the amount of free riding with more than two subjects free riding on average whereas in other treatments it is still less than one. The median contributor makes only one contribution unlike in other treatments and the amount of early contributions is also most affected in the baseline treatment. These observations point to the relevance of low-contribution equilibria for contributing behavior in campaigns without bonuses.

Although the present project focuses on the intensity of contributions within a given group, our findings on free riding behavior suggest that the extensive margin of contributions can be as relevant. The differences in free riding frequency for the baseline relative to both refund bonus treatments are highly statistically significant according to a random effects logit model with session clustering (p-value < 0.001 for all differences). In other words, while some subjects free ride on campaigns without bonuses, they choose to contribute to campaigns that offer bonuses. Thus, in addition to attracting more individual contributions, campaigns with refund bonuses can also attract a larger number of contributors. We leave this question for future research.

In the next two figures, we explore the patterns of group contributions. Figure 1 presents all the campaigns in the space of early and late aggregate contributions, where each dot represents a different campaign. The efficient equilibrium prediction is that the

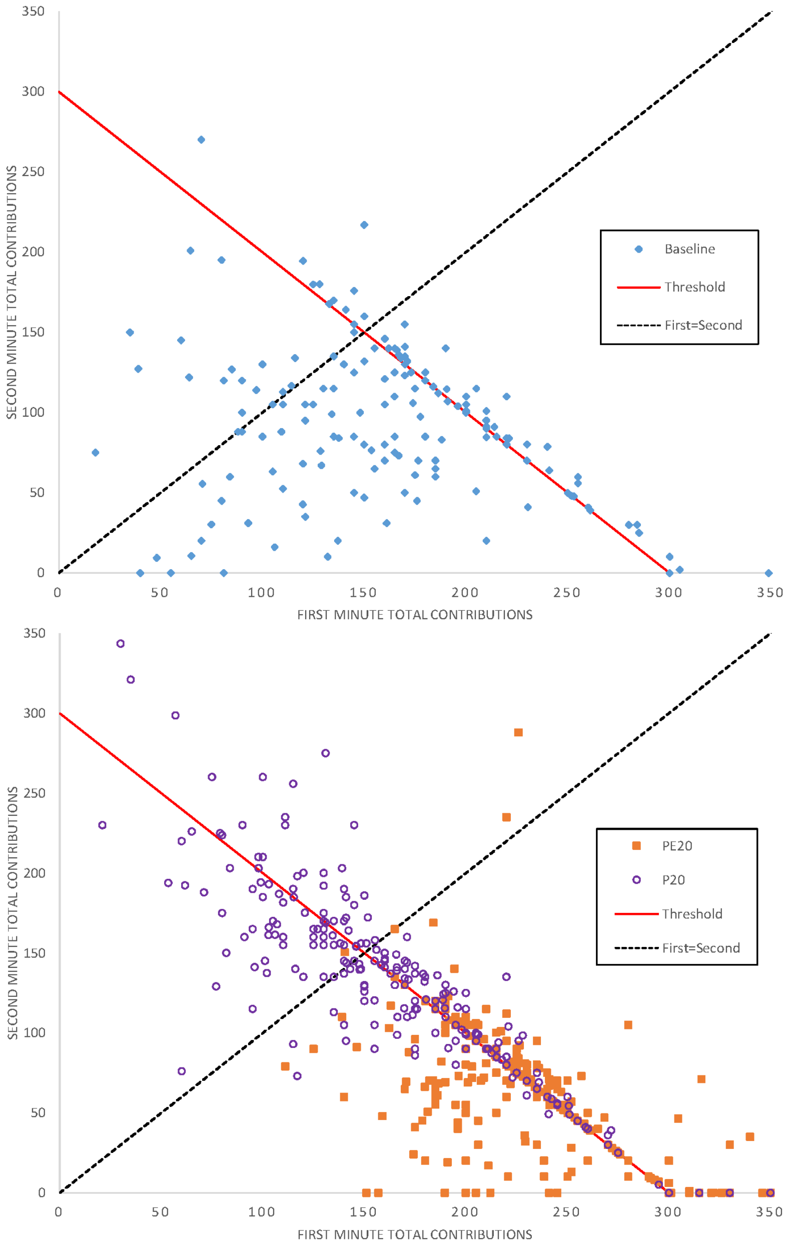

Figure 1: First and Second Half Contributions, by Project, for Baseline (top panel) and for Refund Bonus Treatments (bottom panel)

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

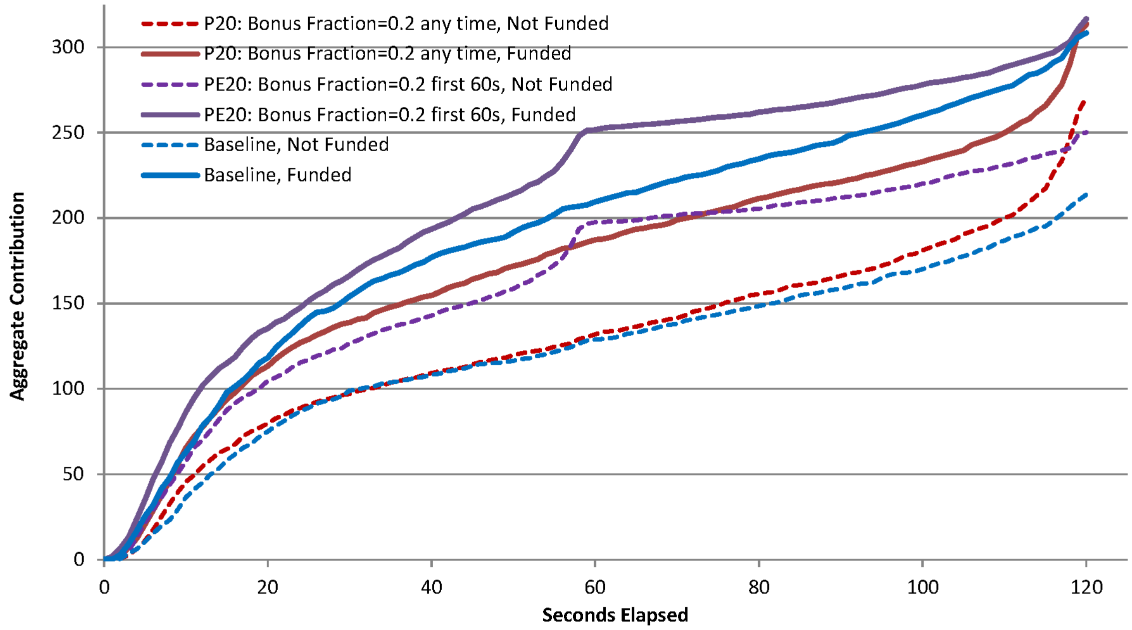

Figure 2: Cumulative Average Contributions, by Funding Success

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.

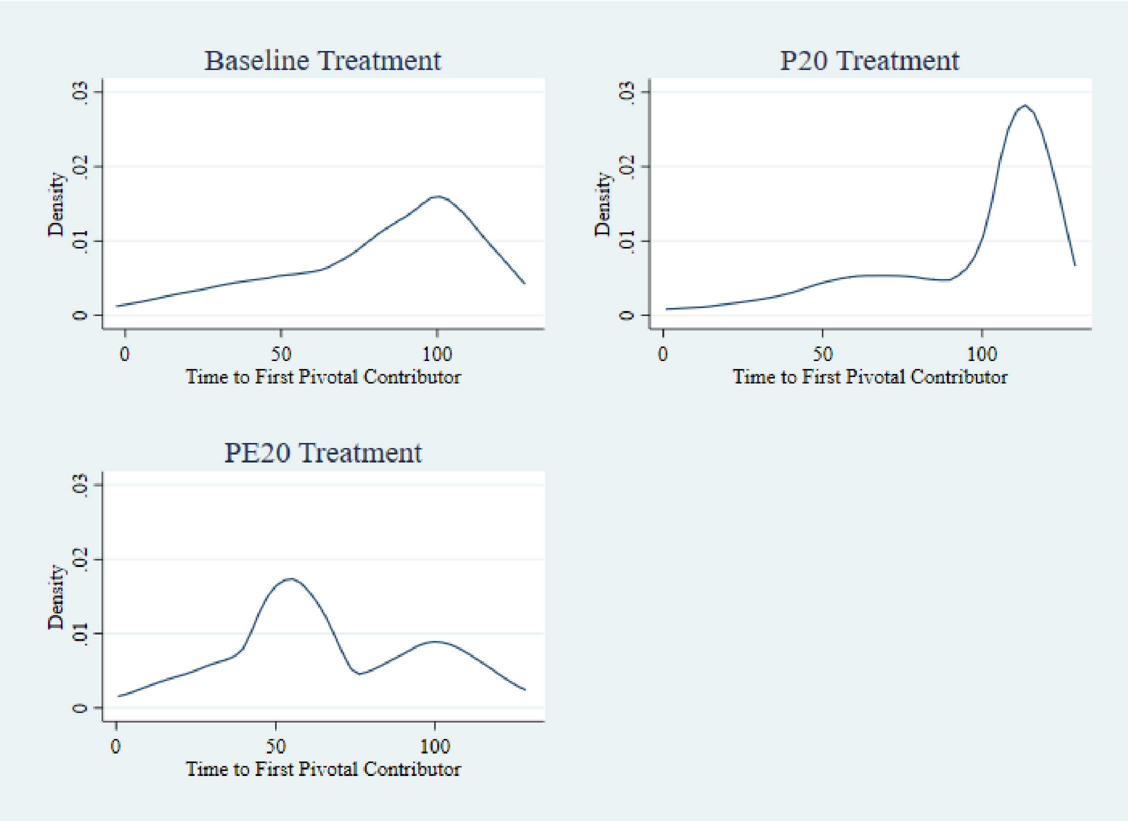

Figure 3: Kernel Density of Pivotal Point Timing, by Treatment

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	0.481	152.47	-9.57	10.00	21.50
		(4.1)	(0.051)	(18.89)	(1.03)	(3.26)	(4.43)
F6	$57/90 = 63\%$	36.6	0.599	175.02	-15.20	12.08	**26.65**
		(4.2)	(0.049)	(18.20)	(2.15)	(4.56)	(5.69)
FE30	$43/90 = 48\%$	41.2	0.458	140.02	-15.53	4.23	15.22
		(3.7)	(0.051)	(19.35)	(1.58)	(4.19)	(5.31)
FE50	$50/90 = 56\%$	35.7	0.518	151.42	-9.47	**17.35**	**30.13**
		(4.4)	(0.051)	(17.39)	(1.17)	(5.54)	(6.40)
PE10	$44/90 = 49\%$	58.0	0.473	149.17	-8.79	9.37	20.62
		(4.6)	(0.052)	(18.83)	(0.93)	(3.33)	(4.42)
Baseline	$74/170 = 43.5\%$	86.2	0.424	139.74	–	15.41	25.43
		(6.2)	(0.037)	(12.60)	–	(1.51)	(2.33)
P20	$133/220 = 60.5\%$	29.0	0.575	158.27	-21.43	3.09	17.00
		(2.9)	(0.032)	(12.17)	(1.81)	(3.29)	(4.03)
PE20	$154/230 = 67.0\%$	49.8	0.632	189.73	-12.79	**16.39**	**31.79**
		(3.8)	(0.030)	(11.31)	(1.23)	(3.20)	(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 (1)	Success (2)	Failure (3)	All (4)	Success (5)	Failure (6)
Others' Early Contribution (Secs 1-60)	-0.027* (0.012)	-0.171** (0.019)	0.021 (0.022)	0.001 (0.002)	-0.010** (0.002)	0.007* (0.003)
Own Early Contribution (Secs 1-60)	-0.213** (0.042)	-0.314** (0.045)	-0.288** (0.068)	-0.011† (0.006)	-0.021* (0.008)	-0.013† (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	Success	Failure	All	Success	Failure
	(1)	(2)	(3)	(4)	(5)	(6)
Others' Early Contribution (Secs 1-60)	-0.109**	-0.158**	-0.046*	-0.006**	-0.011**	0.001
	(0.010)	(0.013)	(0.020)	(0.002)	(0.002)	(0.002)
Own Early Contribution (Secs 1-60)	-0.505**	-0.520**	-0.499**	-0.046**	-0.047**	-0.040**
	(0.037)	(0.043)	(0.064)	(0.007)	(0.008)	(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

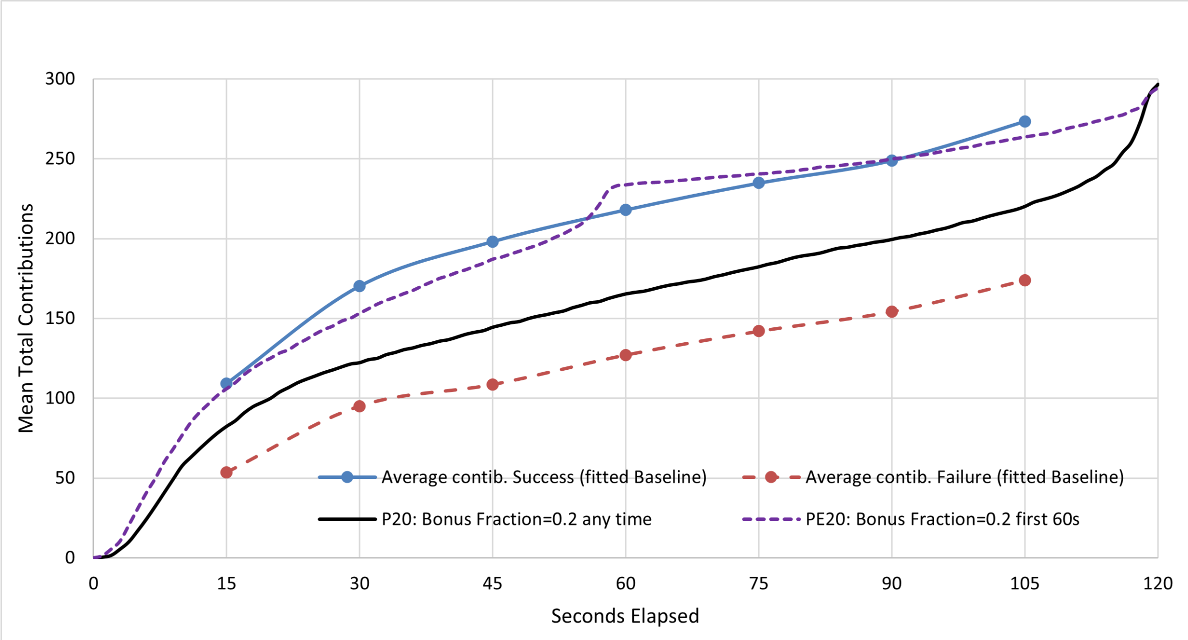


Figure A1: Mean Total Contributions for Successful and Unsuccessful Projects – Model Fitted and Actual Mean Contributions for Refund Bonus Treatments

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.

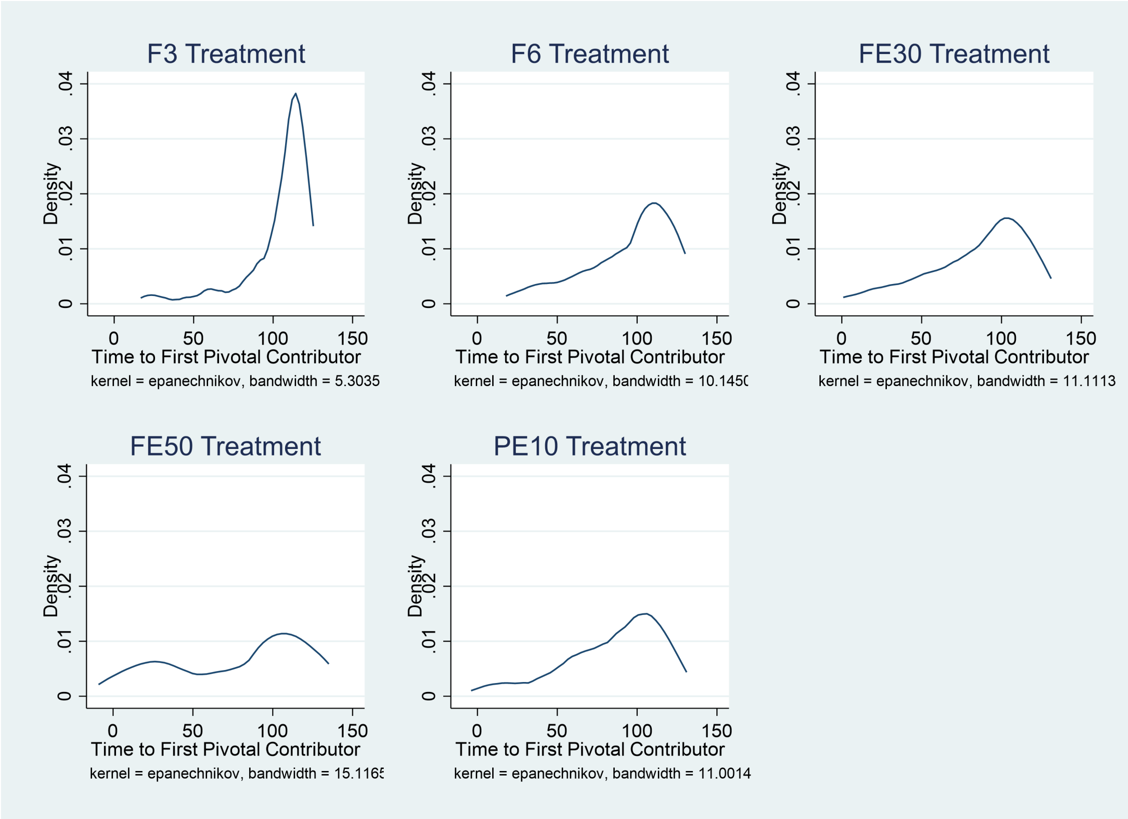


Figure A2: Time Density of Pivotal Points, by Treatment

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.

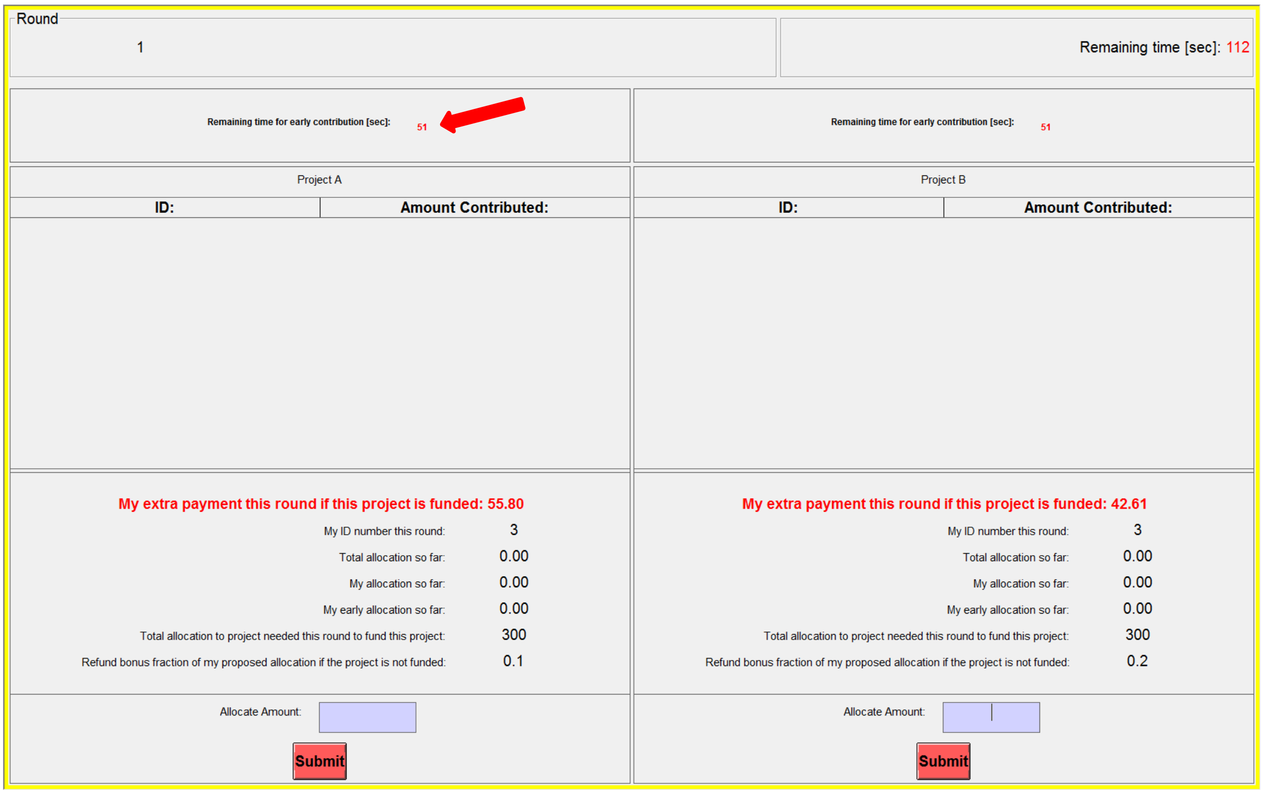
If insufficient money is allocated to a project by all members of your group, then those who tried to allocate money to a project will have their proposed allocation returned. Those individuals who tried to allocate money to a project may also receive a refund bonus. The amount of the refund bonus is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



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.

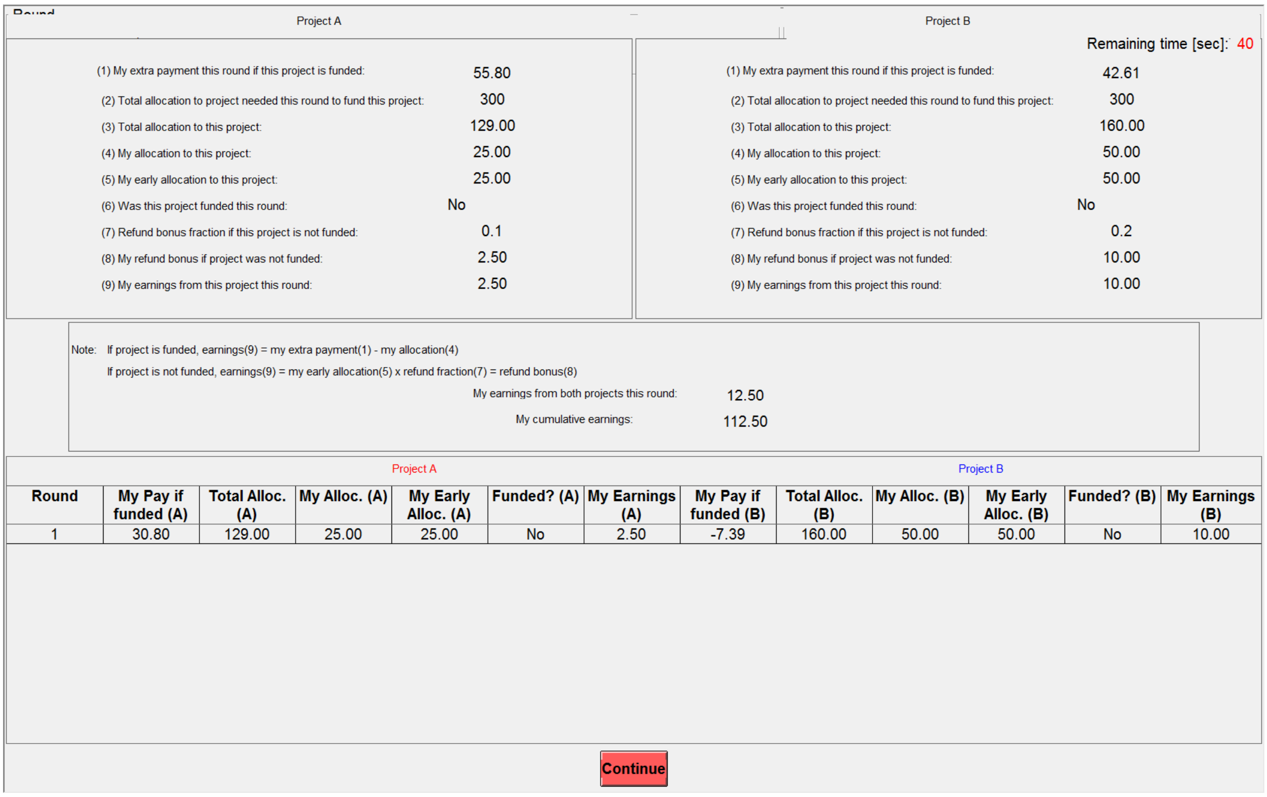
If the total amount of money that your group allocates to fund either project (or both projects) is equal to or greater than the *Threshold*, then you and each of the other group members all receive an extra payment for that project drawn between 20 and 100 as explained above. If the total amount allocated 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



Round	My Pay if funded (A)	Total Alloc. (A)	My Alloc. (A)	My Early Alloc. (A)	Funded? (A)	My Earnings (A)	My Pay if funded (B)	Total Alloc. (B)	My Alloc. (B)	My Early Alloc. (B)	Funded? (B)	My Earnings (B)
1	30.80	129.00	25.00	25.00	No	2.50	-7.39	160.00	50.00	50.00	No	10.00

refund bonus.

End of the round

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.