Better Crowdfunding*

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Abstract

Millions of crowdfunding campaigns have raised billions of dollars for local public goods. But most crowdfunding campaigns fail because, we argue, the standard assurance contract has weak implementation properties that can lead to conditionally cooperative behavior and subsequent miscoordination. We extend the standard assurance contract with refund bonuses payable only if the campaign's target is not reached. We experimentally examine various fixed and proportional refund bonuses. Our special focus is on bonus designs aimed at encouraging early contributions and, thus, improving outcomes of conditionally cooperative behavior. We find that such designs are especially valuable as they nearly double the rate of campaign success at very low cost and are financially self-sustainable even after taking into account campaign failures. In short, better crowdfunding contracts can greatly increase the private and social value of crowdfunding.

Keywords: Public goods, donations, crowdfunding, provision point mechanism, refund bonuses, free riding, equilibrium coordination.

JEL Classification: C72, C92, H41.

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1 Introduction

An important application of crowdfunding is public good provision. In 2015 the "crowd" of donors donated \$5.5bn on Internet crowdfunding platforms, up from \$0.5bn in 2009 (Massolution 2015). Free from the institutional inefficiencies of bureaucratism, favoritism or corruption, crowdfunding is particularly suitable for the realization of small-scale community, school, NGO or individual projects and initiatives typically ignored by public authorities. The World Bank (2013) emphasizes crowdfunding's potential for remittance-based developing economies in bypassing their weak institutions. Being borderless, crowdfunding can facilitate the provision of global public goods, such as in the case of the \$20 million Ocean Cleanup mission, which otherwise requires elusive cooperation across governments.

Over 1000 platforms mediate crowdfunding, most using an assurance contract in which donors pledge to donate to a project if and only if a target funding goal is reached. But most crowdfunding campaigns fail, and a key reason is the weak implementation properties of the assurance contract mechanism. Kickstarter, a popular crowdfunding platform, reports that since starting in 2009, 64% of approximately 400,000 campaigns have failed to reach their target (and thus no money was disbursed). Campaigns should fail if they promote a public good with costs greater than benefits. Campaigns can also fail, however, because assurance contracts have multiple equilibria, including inefficient low-contribution equilibria. Hence, even when it would be efficient for a campaign to succeed, assurance contracts can result in outcomes when insufficient amounts are raised.

Tabarrok (1998) and Zubrickas (2014) introduce a theoretically superior form of assurance contract that eliminates all failure equilibria for efficient campaigns. Cason and Zubrickas (2017, 2018) show that efficient crowdfunding campaigns often do fail and that the superior assurance contract, dubbed the assurance contract with refund bonuses, could increase the campaign success rate. The main idea is to offer a 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 did offer 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 crowdfunding equilibria and are never paid out in equilibrium.

More generally, 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. Pecuniary incentives for encouraging contributions for public goods appear in a number of papers, see, e.g., Varian (1994), Falkinger (1996), Morgan (2000), Goeree et al. (2005), and Gerber and Wichardt (2009). The distinguishing feature of refund bonuses is that they are a simple and practical extension of the already widely used crowdfunding mechanism.

In this paper, we refine and develop the refund bonus contract by testing proportional and fixed refund bonuses, varying the time at which bonuses are offered, and rationing the number of bonuses. We pursue two broad goals. The first goal is to examine empirically the potential of different refund bonus schemes to improve real-life crowdfunding for public goods. The second goal is to identify mechanisms through which equilibrium coordination plays a role in crowdfunding.

1.1 Eliminating Inefficient Equilibria

For intuition on inefficient equilibria in crowdfunding campaigns consider a simple assurance contract in which ten people may donate to fund a public good (a public good valuable to the 10 potential donors, e.g. draining a local swamp). The good is produced if and only if all ten donate \$10 for a total of \$100. The value of the good to each individual is \$15 or \$150 in total. It's efficient that the public good be produced. It's a Nash equilibrium for every individual to donate because by doing so an individual earns a payoff of \$5 (\$15-\$10) but if any one chooses not to donate payoffs are \$0. At the same time, there are many inefficient equilibria in which the public good is not provided. If individual 1 and 2 choose not to donate, for example, then individual 1 earns \$0 but would also earn \$0 if he chose to donate and the same for individual 2. Thus it's also a Nash equilibrium for neither to donate and so the public good is not provided.

Now consider the refund bonus contract due to Tabarrok (1998) and Zubrickas (2014).

In the standard assurance contract, potential donors are refunded their pledge if the campaign fails. In the assurance contract with refund bonuses each potential donor is refunded their pledge plus a bonus, \$B, if the campaign fails. Now reconsider the inefficient equilibrium described above in which individuals 1 and 2 chose not to donate. If individual 1 does not donate he earns \$0 as before but if he does donate (and individual 2 does not) he earns the refund bonus of \$B. Thus it is optimal for individual 1 to donate. The same logic shows that there are no equilibria in which more than one individual does not donate. Now consider situations in which just one individual does not donate. If the individual does not donate they earn \$0 but if they do donate the public good is provided and thus the individual earns the surplus, \$15-\$10=\$5. Thus, it's a Nash equilibrium for every individual to donate. Indeed, in this example, it's a dominant strategy for every individual to donate. Thus, the assurance contract with refund bonuses is a decentralized mechanism capable of efficiently producing public goods.

In the theoretical model a refund bonus of (arbitrarily small) ϵ can eliminate all inefficient equilibria. Moreover, even the ϵ bonus is never paid in equilibrium. Thus the assurance contract with refund bonus appears to offer something for nothing. In practice, transaction costs mean that the refund bonus must be of salient size to motivate behavior. But how large does the refund bonus have to be to motivate behavior? Is a refund bonus of fixed size better or should it be a percentage of the pledge? Should all contributors be eligible for the refund bonus or would it be better to offer only early contributors the possibility of a refund bonus?

Although the refund bonus never has to be paid in theory, not all campaigns will reach equilibrium. Thus, whoever is paying the refund bonuses must take on some risk. Is the risk worthwhile? Assurance contracts with refund bonuses increase the number of successful campaigns and thus generate social value but can they be self-financing once we take into account that refund bonuses must sometimes be paid? In other words, is enough social value generated to also pay the refund bonuses of failed campaigns?

1.2 Importance of Early Contributions

A wide variety of evidence shows that "seed money" can increase total contributions and campaign success in a public good game (Andreoni 1998, Vesterlund 2003). List and Lucking-Reiley (2002), 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. Similarly, Koning and Model (2013) show that a moderately-sized contribution to a crowdfunding project can cause a "cascade" that increases the probability of success beyond that accounted for by the seed itself. Using data from crowdfunding campaigns, Li and Duan (2014) argue that contributions to crowdfunding campaigns must quickly reach a "critical mass" if they are ever going to be successful. Etter et al. (2013) report that the outcome of a Kickstarter campaign can be predicted with 85% accuracy after only 15% of the duration of the campaign. Similarly, Kickstarter reports that once a campaign reaches 20% of its target, it is successful in reaching the whole target with 78% chance.[1] All of these results suggest that early contributions motivate or stimulate later contributions and are thus worth more to campaign success than an equally-sized later contribution (see also Mollick (2014), van de Rijt et al. (2014), Solomon et al. (2015) and Wash (2013)).[2]

Refund bonuses could also be used to encourage early donors. We consider two designs. In the first design, refund bonuses are only offered for contributions made in the first half of the campaign. In the second design, refund bonuses are offered only to the earliest contributors who make contributions of at least a pre-specified minimum level. We note an important difference in implementation properties between the two designs. The second design encourages early contributions but does not eliminate all inefficient low-contribution equilibria whereas the first design completely eliminates the inefficient equilibria.

In this paper, we also provide theoretical insight into the question of why early contributions affect the rate of provision. In the context of threshold public goods, Kessing

[1] https://www.kickstarter.com/help/stats, retrieved on 1 August 2019.

[2] For a contrary finding, see Nagaraj (2017) who finds that information seeding, as opposed to contribution seeding, can exert a negative impact on campaign success.

(2007) and Cvitanic and Georgiadis (2016) offer one theory. They show that early and continuation contributions are strategic complements. An early contribution increases the probability of success and, in turn, the marginal value of subsequent contributions. This theory, however, cannot explain the efficacy of early contributions in the assurance contract game with refund bonuses because contribution costs in this game 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 early contributions in this game do not matter for provision in Markov Nash equilibria or, put differently, the importance of early contributions does not follow from payoff relevance. Yet, our empirical results show that early contributions do continue to stimulate later contributions. Thus, we argue, consistent with 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.

1.3 Main Findings

We conducted our experiment on a lab-based crowdfunding platform with many main features of real-life crowdfunding. This platform allowed for 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. In total, we ran 720 fundraising campaigns of seven different designs – one no-bonus and six refund bonus designs (two designs with fixed bonuses, two designs with fixed bonuses but for several first contributors only, and two designs with proportional bonuses for early contributions). Each campaign lasted for two minutes, during which ten participating subjects could pledge their (multiple) contributions without any timing restrictions. Subjects' valuations for the public good were their private information.[3]

Our experimental results find that refund bonus schemes can nearly double the suc-

[3]Cason and Zubrickas (2018) reports results for a preliminary experiment with a similar environment, but for completely different refund bonus treatments that pay proportional bonuses only, that are paid for any contribution made during the fundraising time period. This earlier study shows that refund bonuses increase fundraising success, but only when contributors can support multiple projects. In the current study, multiple projects are always available for funding.

cess rate of efficient campaigns. We also demonstrate that the increased frequency of successful campaigns generates enough additional value so that refund bonuses can pay for themselves. Thus, assurance contracts with refund bonuses are a very attractive way for crowdfunding platforms to increase the private and social value of crowdfunding. Among the bonus schemes, we do not observe large differences in success rates, which is consistent with the "off-the-equilibrium-path" property of refund bonuses. Though, larger refund bonuses tend to yield somewhat higher success rates. Importantly, however, the same success rate is achieved even when refund bonuses are used early in the pledge process. In particular, refund bonuses for early contributions increase the success rate as much as refund bonuses for all contributions, thus, early refund bonuses are an especially promising mechanism that can increase success rates while limiting the exposure of the campaign operator to bonus payout risk.

We attribute the difference in the provision rates between the baseline (no bonus) and bonus treatments to the equilibrium coordination problem. In the baseline treatment, unsuccessful campaigns raise an average amount much lower than that in the bonus treatments, which can be explained by the existence of low-contribution equilibria in the baseline treatment. Empirical analysis also suggests that in the baseline treatment equilibrium coordination can be closely linked with conditional cooperation. In successful campaigns, the median subject makes two one-time contributions compared to a single median contribution in unsuccessful campaigns. We find that the larger the half-time accumulated aggregate contribution, the larger the likelihood that subjects make additional contributions. Similarly to the aforementioned empirical studies, we also find that the half-time accumulated contribution is an important predictor of the campaign's success. Thus, in the baseline treatment if subjects do not start cooperating early, they do not cooperate at all.

The refund bonus designs that reward early contributions are exactly motivated by conditionally cooperative behavior. We hypothesize that by inducing early contributions these designs can achieve higher half-time accumulated contribution and, thus, fulfill the condition for further coordination. Indeed, we observe that in designs with refund bonuses for early contributions the dynamics of contributions remains similar to the dynamics of contributions in other bonus designs or in successful no bonus campaigns. Furthermore, this conclusion is robust even when the bonus design allows for inefficient low-contribution equilibria, thus, reinforcing the argument for the importance of early contributions in stimulating conditional cooperation.

The remainder of this paper is organized as follows. In Section 2, we discuss theory and formulate hypotheses. The formal details of the model are relegated to the appendix. In Section 3, we present the design of the experiment, the results of which we discuss in Section 4. In Section 5, we compare net returns across treatments and discuss the self-sustainability of 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 crowdfunding 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. We distinguish two designs: fixed and proportional refund bonuses. In the former design, refund bonuses are of a fixed size and paid to the contributors with contributions equal to or above a pre-determined level, and in the latter design refund bonuses are proportional to the contributions pledged.

The assurance contract creates the problem of dynamic provision for a threshold public good. We analyze this problem under the assumption that contributors play Markov (payoff-relevant) strategies. To make the problem interesting, we assume that the sum of individual valuations exceeds the cost of the project with a strictly positive probability. We say that an equilibrium is inefficient if the probability of provision is zero and efficient if the probability of provision is positive. First, in Proposition 1 we present equilibrium properties of the (standard) assurance contract without refund bonuses. Then, in Proposition 2 we show that refund bonuses can eliminate inefficient equilibria.

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.*

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

Point (i) of Proposition 1 shows the equilibrium coordination problem in crowdfunding. Inefficient low-contribution equilibria can arise from standard free-riding behavior where people don't contribute because they think others will contribute and also from the "stability of indifference" where agents don't contribute because they think others will not contribute. Empirically, inefficient equilibria would lead to unsuccessful campaigns and in their raising low amounts. Refund bonuses can alleviate the problem of equilibrium coordination by eliminating 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. We note that crowdfunding campaigns with refund bonuses can still fail when the bad draw of individual valuations makes provision inefficient and also because there is a coordination problem among *efficient* equilibria which cannot be fully remedied by refund bonuses.[4]

The elimination of inefficient low-contribution equilibria has two implications. With refund bonuses, we should observe, first, more provision and, second, a smaller shortfall in contributions for unsuccessful campaigns. The second implication would be indicative of whether the difference in provision rates is due to the existence of low-contribution equilibria in campaigns without refund bonuses. Thus,

Hypothesis 1. *(i) Refund bonuses increase the rate of provision of crowdfunding campaigns, and (ii) unsuccessful campaigns receive more pledged contributions when refund bonuses are offered.*

Point (ii) of Proposition 1 provides a testable implication that is at odds with the empirical evidence cited earlier indicating the importance of early contributions for provision. Proposition 1(ii) indicates that for campaigns without refund bonuses all efficient equilibria have the same probability of provision or, put differently, the probability of provision is path-independent. In other words, early contributions do not affect the rate of provision when agents choose Markov strategies, giving us

Hypothesis 2 (Payoff relevance). *In campaigns without bonuses, early contributions do not matter for success.*

The reason behind this prediction 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 incentives to contribute further.

However, in a dynamic setting the existence of multiple equilibrium outcomes can give rise to a richer set of strategies than those embodied by payoff relevance and, accordingly, to different outcomes. Motivated by the existing evidence indicating the relationship

[4]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.

between early contributions and campaign success (e.g., Li and Duan, 2014; Etter et al., 2013), as an alternative to Hypothesis 2 we have

Hypothesis 2′ (Conditional cooperation). *Greater early contributions increase campaign success.*

While we obtain a unique prediction about the aggregate outcome with a non-zero probability of provision, there are multiple equilibria that lead to this outcome. For instance, equilibria may differ in the number of free-riding agents and, therefore, in the distribution of welfare gains. If contributing agents dislike it when others free ride, then they can treat early contributions as a signal about the amount of free riding and condition their further cooperation upon this signal. As an example, consider a "conditional cooperation" strategy where agents curtail further contributions if the accumulated contribution at a certain moment of time is less than a certain interim threshold.[5] If contributors employ such strategies, the rate of provision will increase for campaigns that have greater early contributions. Importantly, in campaigns without bonuses the threat of discontinuation of later cooperation is credible because of the existence of low-contribution equilibria.

The elimination of inefficient equilibria implies that we should not observe a difference in provision rates among bonus schemes. In the experiment we also study two bonus designs that can have inefficient equilibria. With bonus designs that offer refund bonuses only to several first contributors, it can be an equilibrium outcome for subjects to stop contributing if their further contributions are no longer eligible for bonuses.[6] However, when contributors employ "conditional cooperation" strategies, 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. Thus, we have

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

[5]It is straightforward to formalize such strategies and resultant equilibrium play; see, e.g., Kreps et al. (1982) for an approach. For more discussion on conditional cooperation, see, e.g., Sugden (1984), Bernheim (1994), and Bigoni et al. (2015).

[6]Note that the design schemes with proportional bonuses for early contributions have no inefficient equilibria as otherwise contributors could have increased their bonuses by contributing more early.

As we explain next, our experimental design includes six different types of refund bonus designs. Rejection of Hypothesis 3 will allow us to determine, in practice, which features of the refund bonuses promote efficiency.

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 $U[20, 100]$. 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. If aggregate contributions during the two-minute funding window reached the 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, along with six versions of the refund bonus implemented in various ways. The refund bonus was paid to certain individuals as follows, in the event that the aggregate contributions failed to reach the target threshold of 300. As with most crowdfunding sites in the field, contributions were also refunded when the funding threshold was not reached.

Treatment F3: Fixed refund bonus of $z = 3$ for any total individual contribution $\geq g_{min} = 30$.

Treatment F6: Fixed refund bonus of $z = 6$ for any total individual contribution $\geq g_{min} = 30$.

Treatment FE30: Fixed refund bonus of $z = 6$ for first 5 individuals whose total individual contribution $\geq g_{min} = 30$.

Treatment FE50: Fixed refund bonus of $z = 6$ for first 5 individuals whose total individual contribution $\geq g_{min} = 50$.

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

Treatment PE20: Proportional refund bonus $r = 0.20$ paid on contributions made during first minute of the two-minute contribution window.

The total individual contributions in all of these cases refer to the aggregate of any separate individual contributions made by subjects at different points in time. The performance of the different types of refund bonuses can be inferred from a series of pairwise treatment comparisons. Note that since the refunds in treatments F3 and F6 are 10% and 20% of the minimum individual thresholds, they roughly correspond to the $r = 0.10$ and $r = 0.20$ refund bonuses implemented in treatments PE10 and PE20. Treatments F3 and F6 differ in the size of the fixed refund z. The difference between F6 and FE30 is the competition in the latter to receive the $z = 6$ refund bonus, and the difference between FE30 and FE50 is the size of the individual target g_{min} to obtain this fixed bonus.

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 represents a key aspect of crowdfunding in the field, where potential contributors must choose between 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 Appendix B include an image of the contribution screen, which always showed both projects available for contributions.

We varied the treatment conditions once within subjects, with other treatment variations implemented across subjects. Table 1 displays the ordering of treatment conditions across different sessions. Each session began with 15 periods in one treatment condition followed by one treatment switch before the final 15 periods. We did not include alternative projects with identical refund bonus conditions, or both with no refund bonus, because previous research (Corazzini et al. (2015); Ansink et al. (2017); Cason and Zubrickas (2018)) has already investigated coordination and contributions to multiple projects with similar or identical characteristics. Two groups of ten subjects, employing fixed matching within these ten-subject groups, participated in each of the six treatment ordering configurations, for a total of 120 subjects in the experiment. 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 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. Subjects' total earnings averaged US$28.25 each, with an interquartile range of $24.00 to $32.50. Sessions usually lasted about 90 minutes, including the time taken

Table 1: Experimental Design

Periods 1-15	Periods 16-30	Num. Subjects	Num. Groups
F3 and F6	F3 and Baseline	20	2
F6 and Baseline	F3 and F6	20	2
FE30 and FE50	FE30 and Baseline	20	2
FE50 and Baseline	FE30 and FE50	20	2
PE10 and PE20	PE10 and Baseline	20	2
PE20 and Baseline	PE10 and PE20	20	2

for instructions and payment distribution.

4 Results

We present the results in four subsections. Subsection 4.1 presents the overall treatment comparisons, documenting the most promising refund bonus schemes for raising individual contributions and the project funding rate. Subsection 4.2 provides additional details of individual contributions across treatments. Subsection 4.3 investigates the role of early contributions. Subsection 4.4 investigates the timing of contributions in greater detail, and how the timing depends on the structure of the refund bonus scheme.

4.1 Treatment Comparisons

Table 2 summarizes the funding rates for each experimental treatment, in total and separated into the early (periods 1-15) and late (periods 16-30) halves of the sessions. In the baseline treatment without any refund bonuses, only about one-third of projects are funded, whereas 48 to 63 percent of projects are funded with refund bonuses. Note also that the funding rate decreases from the first to second half of the periods in all treatments. This reflects an increase in miscoordination in the final seconds of the contribution window. As we document in Section 4.4, subjects increasingly concentrate their contributions in the final seconds as they wait for others to contribute.

To compare treatments it is important to control for this time trend and other fac-

Table 2: Funding Frequency and Average Shortfall

Treatment	Funding Frequency			Shortfall (std.err.)
	All 30 Periods	Periods 1-15	Periods 16-30	
Baseline	61/180 = 34%	35/90 = 39%	26/90 = 29%	108.1 (5.5)
F3	45/90 = 50%	19/30 = 63%	26/60 = 43%	34.5 (4.1)
F6	57/90 = 63%	41/60 = 68%	16/30 = 53%	36.6 (4.2)
FE30	43/90 = 48%	15/30 = 50%	28/60 = 47%	41.2 (3.7)
FE50	50/90 = 56%	37/60 = 62%	13/30 = 43%	35.7 (4.4)
PE10	44/90 = 49%	15/30 = 50%	29/60 = 48%	58.0 (4.6)
PE20	54/90 = 60%	38/60 = 63%	16/30 = 53%	50.2 (5.3)

tors such as the overall value of the public good. Table 3 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 treatment dummy variables to document differences in funding likelihood.[7] 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, which indicates a significantly greater funding likelihood for more valuable projects. The Period variable and a dummy variable representing the second half of the session (periods 16-30) account for the time trend noted in Table 2. The regression also includes characteristics of the other project seeking contributions contemporaneously; specifically, the value of this other project and the refund bonus treatment. These terms are typically not significantly different from zero and so they are suppressed in the table.

All of the coefficient estimates on the refund bonus treatments are positive, consistent with an increased funding likelihood, and three of them (F6, FE50 and PE20) are highly significantly different from zero. This provides support for Hypothesis 1(i). Regarding the similarity of provision rates across refund bonus treatments predicted by Hypothesis 3, we observe that the design of refund bonuses matters for success. The PE20 version

[7] A random effects logit model leads to identical conclusions, so we report the LPM since the coefficients are simple to interpret.

Table 3: Funding Success and Individual Contributions

	Funding Success	Individual Contributions
Dummy for F3	0.102†	5.867**
	(0.057)	(2.124)
Dummy for F6	0.171**	4.542*
	(0.042)	(2.120)
Dummy for FE30	0.113	6.005**
	(0.088)	(2.122)
Dummy for FE50	0.211**	9.185**
	(0.053)	(2.126)
Dummy for PE10	0.111	5.461*
	(0.108)	(2.120)
Dummy for PE20	0.255**	7.602**
	(0.022)	(2.123)
Group Value	0.003**	
	(0.0003)	
Individual Value		0.399**
		(0.010)
Period	$-0.010^{\dagger}$	-0.024
	(0.006)	(0.053)
Dummy (Periods 16-30)	-0.106**	-0.862
	(0.026)	(1.862)
Alternative Project	Included	Included
Constant	-1.003*	-1.205
	(0.410)	(3.215)
Overall R-sq	0.183	
Observations	720	7200

Note: Random-effects regressions, with standard errors clustered by sessions; robust standard errors are 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; † at 0.10.

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. Pairwise comparisons with the other refund bonus implementations indicate significantly greater success for PE20 relative to F3 (p-value = 0.006), F6 (p-value = 0.050) and marginally compared to FE30 (p-value = 0.083). Generally, we note that the schemes that offer larger bonuses (F6 and PE20) tend to have higher provision rates than their smaller bonus counterparts (F3 and PE10, respectively). At the same time, there is no difference in provision rates between bonus schemes with similar bonuses, see F3 vs. PE10 and F6 vs. PE20.[8] Subsection 4.4 provides a more detailed comparison among bonus designs.

The second column of Table 3 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 11 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, but due to greater statistical power this model indicates significantly greater contributions for all refund bonus formats relative to the baseline. None of the refund bonus treatments have significantly *different* impacts on individual contributions, however, except that F6 has significantly lower contributions than FE50 (p-value = 0.005) and marginally lower contributions than PE20 (p-value = 0.063). 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.

Part (ii) of Hypothesis 1 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 2 provides clear support for this prediction. Without refund bonuses average contributions are more than 100 experimental dollars below the funding threshold of 300, and this large shortfall is two or three times greater than the average shortfall in the treatments with refund bonuses. We attribute this difference in shortfalls to the existence of low-contribution equilibria in the campaigns without bonuses. However, we note that while FE30 and FE50 designs also have low-contribution equilibria, these equilibria are not salient as the shortfalls in the two designs are similar to those in other bonus designs that have no low-contribution equilibria. We explain this observation by the role of early contributions (see Subsection 4.3).

This initial treatment comparison provides support for the main implication of refund

[8] Cason and Zubrickas (2018) study 10% and 20% proportional refund bonus designs with bonuses paid for all contributions made during the contribution window. The success rates reported there are very similar to the success rates of PE10 and PE20, respectively.

bonuses: Bonuses raise the rate of provision by eliminating inefficient, low-contribution equilibria as observed by larger amounts pledged for unsuccessful campaigns (Hypothesis 1). At the same time, the design and, in particular, the size of refund bonuses matter for success, unlike the prediction in Hypothesis 3.

4.2 Individual Contributions

In this subsection we document patterns of individual contributions across treatments. Recall that individuals could choose when and how often to pledge contributions to the projects at any time during the two-minute window. The first column of Table 4 shows that mean amounts pledged in one-time individual contributions range across treatments between 12 and 14 experimental dollars. The one exception is treatment FE50, which has a higher mean contribution (17.9) due to the higher cutoff (50) needed to receive the refund bonus. The second column indicates that typically subjects make about two pledges on average to each project per period, with treatment FE50 having the lowest frequency. Average total contributions by individuals during the funding window are shown in column 3. The variance in total contributions across individuals is lowest in treatments F3 and F6 (standard deviations about 17). This is consistent with the individual target of 30 experimental dollars for the refund bonus providing an anchor for individual contributions. At the same time, the variance in total contributions for successful projects is largest in the baseline treatment (26.5, shown in the upper right). This is consistent with the observation that refund bonuses reduce the number of contribution combinations that can be sustained as equilibria because of the possibility of profitable deviations (see Zubrickas (2014) and Cason and Zubrickas (2017)).

Individual contributions and their frequency tend to be lower in the no-bonus baseline, consistent with its inferior performance documented in the previous subsection. Comparing successful with unsuccessful projects, the baseline treatment has the largest difference in the mean number of one-time contributions and, accordingly, in total individual contributions. We also find that in the baseline treatment the median subject makes two one-time contributions toward successful and only one toward unsuccessful campaigns.

Table 4: One-Time Individual Contributions: Mean Amount, Frequency, and Total

Treatment	All projects			Unsuccessful projects			Successful projects		
	Mean a_i	# of a_i	Total	Mean a_i	# of a_i	Total	Mean a_i	# of a_i	Total
Baseline	12.7	1.84	23.4	12.0	1.62	19.4	13.8	2.30	31.7
	(13.2)	(2.00)	(23.2)	(11.9)	(1.99)	(20.2)	(14.8)	(1.96)	(26.5)
F3	13.2	2.19	28.9	13.0	2.05	26.5	13.4	2.33	31.2
	(11.0)	(2.06)	(17.6)	(10.8)	(2.08)	(17.2)	(11.2)	(2.02)	(17.7)
F6	13.9	2.14	29.7	12.5	2.11	26.3	14.7	2.16	31.6
	(12.0)	(2.04)	(17.1)	(10.7)	(2.15)	(14.9)	(12.7)	(1.98)	(18.0)
FE30	13.5	2.11	28.5	13.9	1.86	25.9	13.2	2.38	31.4
	(11.9)	(1.80)	(19.7)	(11.3)	(1.66)	(18.1)	(12.4)	(1.92)	(21.0)
FE50	17.9	1.66	29.6	16.6	1.59	26.4	18.8	1.71	32.1
	(17.7)	(1.33)	(23.8)	(16.1)	(1.40)	(22.0)	(18.8)	(1.28)	(24.8)
PE10	11.9	2.32	27.5	10.6	2.29	24.2	13.2	2.35	31.0
	(11.3)	(2.10)	(20.8)	(9.6)	(2.41)	(19.0)	(12.6)	(1.72)	(22.0)
PE20	14.0	2.09	29.2	12.6	1.98	25.0	14.8	2.16	32.0
	(13.6)	(1.86)	(23.2)	(12.1)	(1.51)	(19.6)	(14.4)	(2.05)	(24.9)

Note: a_i stands for a one-time individual contribution, " # of a_i" for the mean number of one-time individual contributions, and "Total" for the mean sum of one-time individual contributions made over the contribution window (it is equal to the product of the values in the two preceding columns). Standard deviations are reported in parentheses.

These observations point to the relevance of low-contribution equilibria for contributing behavior in campaigns without bonuses, which finds further support in Table 5.

Some individuals completely free ride and contribute nothing to one or both of the projects in a given period. Table 5 shows that zero contributions are most common in the baseline treatment, which is of course unsurprising due to the absence of any refund bonuses. These differences in free riding frequency for the baseline relative to the refund bonus treatments are all statistically significant according to a random effects logit model with session clustering (p-values are all < 0.011). This regression also indicates that individuals are more likely to free ride when they have a lower value for the project.

Among the refund bonus treatments, zero individual contributions are most common for treatment FE50, which has a higher cutoff (50) needed to receive the refund bonus. This high cutoff apparently discourages a larger number of individuals from making any contributions. Complete free riding is also more common for unsuccessful projects (middle column), but nevertheless 85 to 93 percent of individuals make pledges even for those projects that do not reach the threshold when refund bonuses are available.

Even though the present project focuses on the intensity of contributions within a

Table 5: Frequency of Zero Individual Contributions

Treatment	All Projects	Unsuccessful Projects	Successful Projects
Baseline	0.197	0.264	0.061
F3	0.092	0.122	0.062
F6	0.069	0.097	0.053
FE30	0.061	0.091	0.028
FE50	0.112	0.153	0.080
PE10	0.068	0.085	0.050
PE20	0.053	0.069	0.043

given group, our findings on free riding behavior suggest that the extensive margin of contributions can be as relevant. 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.

4.3 The Role of Early Contributions

In this subsection, we aim to examine more thoroughly the question why campaigns fail. As discussed in the introduction, early contributions are shown to correlate with funding success (e.g., Etter et al., 2013). Our particular focus is on the role of early contributions as a coordination mechanism.

Hypothesis 2 states that early contributions should not affect the rate of provision for the baseline environment without refund bonuses. From another perspective, if true this hypothesis would imply that early and late contributions should negatively correlate because of the threshold for contributions, i.e., in the event of a slow start contributors should increase their contributions later in the campaign. Table 6 reports three regressions that employ data from the baseline (no bonus) treatment, contrasted with treatment F6. The key explanatory variable is the group's total contributions during the first half of the contribution period, shown in the top row. In contradiction to Hypothesis 2, Column (1) indicates that these early contributions have a positive but statistically insignificant impact on individuals' second half (seconds 61-120) contributions when no bonuses are offered.

Table 6: Early (Seconds 1-60) Contributions' Influence on Late (Seconds 61-120) Contributions and Funding Success – Baseline and Fixed Bonus (F6) Treatments

	Individual Late Contributions		Any Late Contribution		Funding Success	
	Baseline (1)	F6 (2)	Baseline (3)	F6 (4)	Baseline (5)	F6 (6)
Total Early Contribution (Secs 1-60)	0.008 (0.013)	-0.125** (0.015)	0.003* (0.002)	-0.008** (0.002)	0.032** (0.005)	0.027** (0.007)
Individual Value	0.353** (0.031)	0.243** (0.031)	0.025** (0.004)	0.022** (0.005)		
Group Value					0.008 (0.006)	0.022** (0.010)
Period	0.390** (0.152)	0.264 (0.167)	0.047* (0.023)	0.057** (0.011)	-0.025 (0.050)	0.032 (0.074)
Dummy (Periods 16-30)	-4.433 (4.031)	-0.755 (2.151)	-0.114 (0.772)	0.047 (0.112)	-1.401† (0.760)	-0.755† (0.407)
Alternative Project	Included	Included	Included	Included	Included	Included
Constant	-22.23** (4.57)	15.73** (4.51)	-2.041** (0.420)	0.588 (0.766)	-7.61* (3.74)	-17.16* (7.36)
Observations	1800	900	1800	900	180	90

Note: Random-effects regressions, with standard errors clustered by sessions; robust standard errors are reported in parentheses. 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.

Column (3) also indicates that in the baseline treatment a contributor's likelihood of making any contribution during the second half of the period increases significantly when the group has greater early contributions. Thus, contributions during the first half of the contribution period appear to induce greater participation among contributors during the remainder of the period. Consequently, these early contributions are also predictive of ultimate funding success, shown in Column (5), consistent with the field evidence cited earlier.

Hence, these findings reject Hypothesis 2 and, in turn, suggest that payoff relevance alone cannot explain contributing behavior. Our findings also point in the direction of conditionally cooperative behavior in the no bonus baseline condition, consistent with

Hypothesis 2′, where subjects continue cooperating only if others have sufficiently cooperated in the early part of the campaign. As already noted, 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. Interestingly, however, later cooperation does not break down in the bonus treatments. From the regression results for the F6 treatment in Table 6, shown in columns (2), (4) and (6), we see an increase in later contributions if contributors contributed smaller amounts early in the campaign.[9] With bonuses the threat to discontinue later cooperation is no longer credible as the resultant low-contribution outcome would not be equilibrium.

Table 7: Early (Seconds 1-60) Contributions' Influence on Late (Seconds 61-120) Contributions and Funding Success – Treatments with Bonuses for Early Contributions

	Individual Late Contributions		Any Late Contribution		Funding Success	
	FE50 (1)	PE20 (2)	FE50 (3)	PE20 (4)	FE50 (5)	PE20 (6)
Total Early Contribution (Secs 1-60)	-0.099** (0.016)	-0.070** (0.000)	-0.004** (0.002)	-0.005** (0.000)	0.032** (0.005)	0.054** (0.015)
Individual Value	0.143** (0.039)	0.391** (0.045)	0.007* (0.003)	0.029** (0.005)		
Group Value					0.006 (0.008)	0.009* (0.005)
Period	0.225 (0.223)	0.849** (0.250)	0.041 (0.026)	0.086** (0.013)	-0.052 (0.046)	0.099 (0.092)
Dummy (Periods 16-30)	1.869 3.523)	7.215* (3.668)	0.531** (0.772)	0.749* (0.302)	0.758† (0.438)	0.694 (0.517)
Alternative Project	Included	Included	Included	Included	Included	Included
Constant	9.11 (6.07)	-21.26** (7.88)	0.064 (0.308)	-1.89** (0.36)	-10.53 (6.71)	-12.87** (4.94)
Observations	900	900	900	900	90	90

Note: Random-effects regressions, with standard errors clustered by sessions; robust standard errors are reported in parentheses. 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.

The finding that early contributions matter for success motivates refund bonus designs aimed specifically at eliciting early contributions in order to fulfill the condition for

[9]We observe a similar pattern in the F3 treatment and, therefore, we do not report it here.

cooperation. Rather than rewarding all contributions above the minimum level like in treatments F3 and F6, treatments FE30 and FE50 aim to elicit larger early contributions as only the first five contributors (out of ten total) to reach the cutoff can earn the refund bonus. Treatments PE10 and PE20 encourage early contributions in a different way, by paying refund bonuses only for contributions made during the early half of the period rather than through competition for a limited number of fixed refund bonuses. Table 7 reports the same regressions as in Table 6 but for bonus treatments FE50 and PE20 (the results are very similar for FE30 and PE10, not shown). As in the case of the fixed bonus treatment for all contributions (F6), we see the same negative relationship between early and late contributions, and positive impact of early contributions on funding success. As we discuss more thoroughly in the next subsection, bonuses aimed at early contributions do increase early contributions and in the later part of the campaign the contributors continue cooperating toward raising the remaining (smaller) amount needed for provision.

4.4 Comparing Refund Bonus Designs

To illustrate the consequences of different refund bonus rules for dynamic contribution patterns, Figures 1, 2 and 3 display average cumulative contributions over time for each treatment. The figures distinguish successful projects with solid lines (contributions reach the threshold of 300) and unsuccessful ones with dashed lines. The figures indicate that subjects concentrated their contributions in the initial 20 to 40 seconds, and the final 5 to 10 seconds, regardless of the refund bonus rules. But they also illustrate different patterns due to specific characteristics of the refund bonus schemes.

All three figures display the same average cumulative contributions for the baseline (no bonus) treatment in blue. Figure 1 shows that the F3 and F6 treatments, in which bonuses are paid to all contributors who pledge at least 30. These bonuses tend to raise early contributions relative to the baseline, particularly for unsuccessful projects. Overall, however, the time pattern for cumulative contributions is similar to the baseline for these treatments.

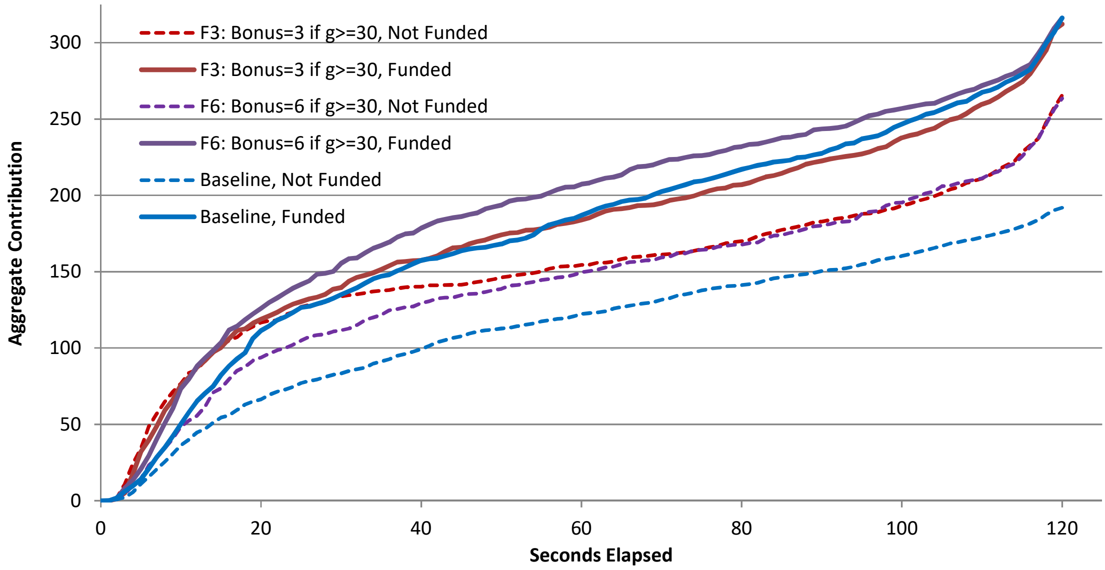


Figure 1: Cumulative Average Contributions (Fixed Bonus for Minimum Contribution, by Funding Success)

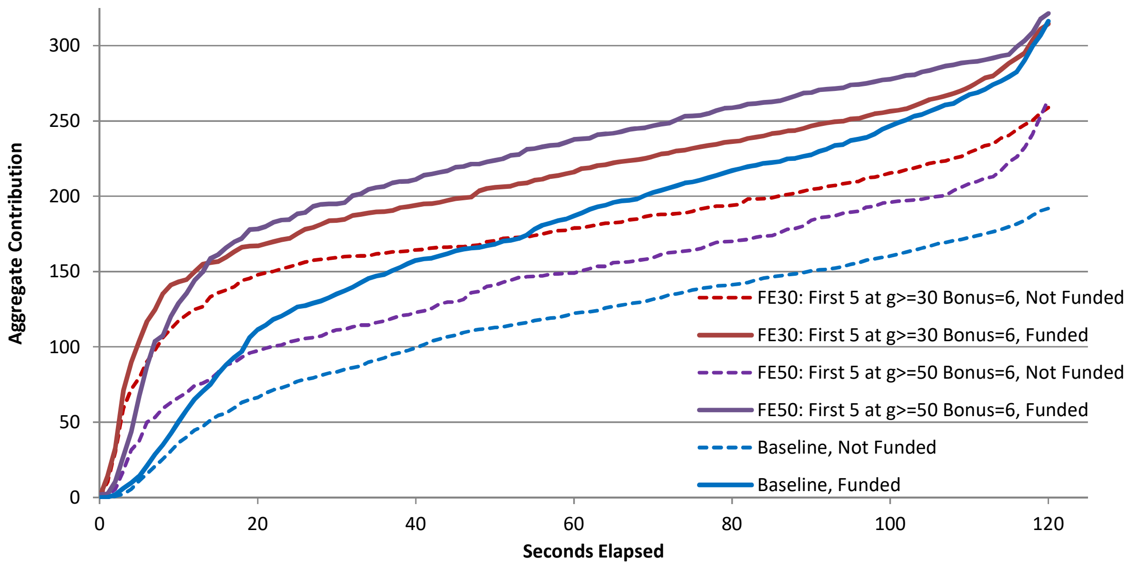


Figure 2: Cumulative Average Contributions (First Half of Subjects at Minimum Contribution Receive Fixed Bonus, by Funding Success)

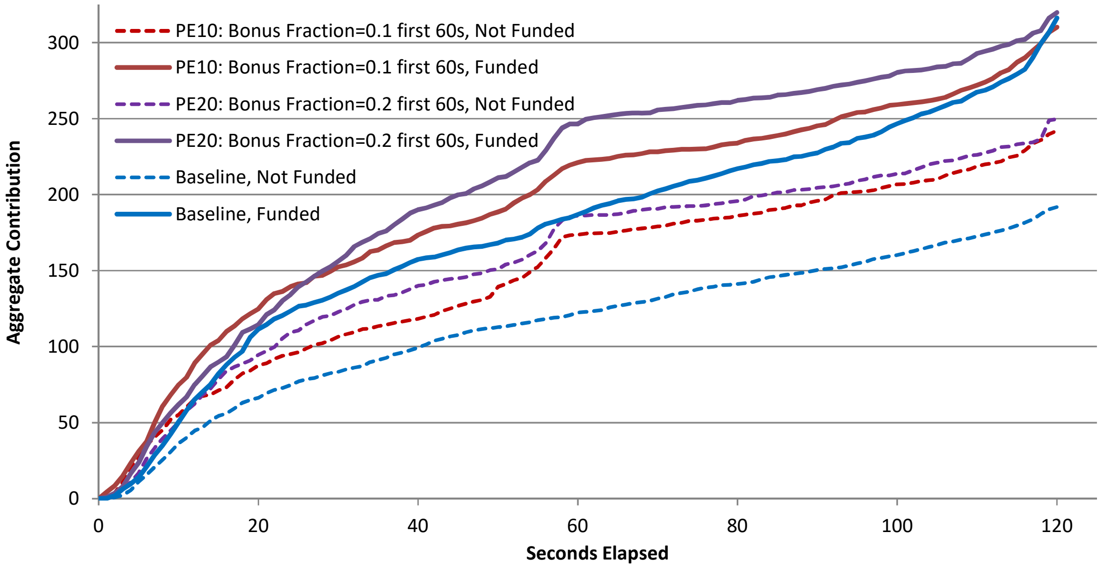


Figure 3: Cumulative Average Contributions (Proportional Bonus Paid for Early Half Contributions Only, by Funding Success)

Figures 2 and 3 indicate that the other four treatments (FE30, FE50, PE10, PE20) have a more substantial impact on the time pattern of contributions. In the baseline, average contributions of successful campaigns reach 111 (relative to the target of 300) at the 20-second point of the period; by contrast, for the FE30 and FE50 treatments, where the 10 subjects compete for a limited number of (5) bonus payments the average contributions made to successful campaigns are 167 to 178 at the 20-second point. Following this initial surge of early contributions, cumulative contributions remain above the baseline level throughout the remainder of the period.

The PE10 and PE20 treatments, which pay refund bonuses only for contributions made during the first 60 seconds of the period, affect the time pattern of cumulative contributions differently (Figure 3). The most noticable uptick in aggregate contributions occurs later, just before the refund bonus period expires at the 60-second mark. Like the FE30 and FE50 treatments, this version of the refund bonus leads contributions to accumulate earlier, as designed, and causes cumulative contributions to exceed the baseline level throughout the entire contribution window.

The goal of the refund bonuses, particularly when implemented to encourage contributions during the early phase of the pledge window, is to initiate some early momentum to push cumulative contributions toward the funding threshold. To better understand early contributions, Table 8 first reports a logit model indicating which of the two projects contributors choose for their initial contribution each period.[10] 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 any kind of refund bonus, relative to the baseline.

The different refund bonus designs have different impacts on early contributions, however, providing more evidence against Hypothesis 3. The treatment that is most successful at attracting the initial contribution in a period is FE30, which pays a bonus to only the first 5 contributors whose total pledge amount reaches 30. This treatment is over 46 percentage points more likely to attract the initial contribution than the baseline, and it also is significantly more likely (at the one-percent level) to receive the first contribution relative to the other five refund bonus treatments. Interestingly, treatment FE50 performs the worst on this measure, and it is less likely to attract the initial contribution than F3, FE30 and PE10 at the five-percent significance level. Apparently the higher target (50) to receive the refund bonus is often too high to attract the subjects' first contribution.

By the time half of the period for collecting contributions has elapsed (first 60 seconds), however, the last two columns of Table 8 show that the FE50 treatment has "caught up" with the other refund bonus treatments. All of the refund bonus treatments at this point collect more contributions than the no-bonus baseline, and most of the specific bonus treatments are not statistically distinguishable at this halfway point of the contribution period. For the total contribution column only treatment F6 is significantly lower than treatments FE30, FE50, PE10 and PE20 (five-percent significance). For individual contributions treatment F6 is significantly lower than treatments FE50 and PE20 (one-percent significance), and treatment PE20 performs the best. Specifically, treatment PE20 has individual contributions that are marginally significantly higher than FE50 (p-value $= 0.077$). The 60-second cutoff for bonus eligibility, used in treatments PE10 and PE20, appears to be effective at concentrating contributions in the first part

[10] Recall that two projects, with different refund bonus characteristics, were always available to receive contributions; see Table 1.

Table 8: Initial Contribution; Individual and Total Contributions in First 60 Seconds

	Initial Contribution (Logit)	Individual Contribution (Secs 1–60)	Total Contribution (Secs 1–60)
Dummy for F3	0.275**	5.039*	34.51**
	(0.027)	(2.446)	(13.37)
Dummy for F6	0.231**	1.559	10.42
	(0.045)	(2.440)	(7.66)
Dummy for FE30	0.466**	7.078**	49.88**
	(0.031)	(2.442)	(14.13)
Dummy for FE50	0.131**	6.676**	47.76**
	(0.051)	(2.444)	(16.73)
Dummy for PE10	0.310**	7.744**	54.08**
	(0.026)	(2.440)	(11.68)
Dummy for PE20	0.243**	9.636**	63.94**
	(0.055)	(2.440)	(11.27)
Individual Value	0.0051**	0.271**	
	(0.0003)	(0.010)	
Group Value			0.207**
			(0.024)
Period		-0.292**	-2.82**
		(0.052)	(0.664)
Dummy (Periods 16-30)		-3.348	-32.12**
		(2.212)	(8.43)
Alternative Project	Included	Included	Included
Constant		-2.722	67.91*
		(3.734)	(29.93)
Overall R-sq			0.343
Observations	7010	7200	720

Note: Random-effects regressions, with standard errors clustered by sessions; robust standard errors are 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; † at 0.10.

of the period.

Contributions after the 60-second point do not receive any bonuses in the PE10 and PE20 treatments, so just like the no-bonus baseline these treatments do not have bonus incentives for later contributions. Therefore, the contribution pattern in the later part of the periods should be similar in these three treatments. Table 9 provides support for this conjecture using a regression of individual contributions during the second half of the period, conditional on the total amount raised during the first half of the contribution period. The PE10 and PE20 treatment dummy variables are not statistically significantly different from the baseline or from each other (p-values > 0.15 in all cases). Although total contributions raised during the early part of the period have a negative impact on later contributions – which is in line with payoff relevance unlike in the baseline treatment – their higher level in PE20 at the period midpoint are sufficient to significantly increase the fundraising success relative to the no-bonus baseline (Table 3). Table 9 shows that later contribution patterns are similar in the baseline and PE20 treatments, so that greater funding success for PE20 is due to its better encouragement of earlier contributions (Table 8).

Note also that the time trend variables (period number and a dummy variable for the second half of the session, periods 16-30) are both significantly positive in Table 9. This indicates that subjects more often make contributions late in the contribution window during the later periods of the session. This is a proximate cause of the decline in funding success in these later periods (recall Table 2). The coordination problem worsens in later periods as subjects start to concentrate more of their contributions to the final seconds before the contribution window closes.

In summary, competition for a limited number of refund bonuses is most effective in attracting the first contribution subjects make (treatment FE30), as long as the target amount to receive the bonus is not too high (as it is for treatment FE50). Cumulative early contributions are not impacted as much when all contributors can receive bonuses (treatments F3 and F6). Bonus schemes that are paid exclusively for contributions made early in the contribution window are most effective in incentivizing early contributions

Table 9: Individual Contributions during Second Half of each Period (Tobit Model)

Explanatory Variable	
Dummy for PE10	-5.147
	(3.572)
Dummy for PE20	1.445
	(3.562)
Total Contributions First Half of Period	-0.029**
	(0.009)
Own Project Value	0.392**
	(0.020)
Period	0.560**
	(0.104)
Dummy (Periods 16-30)	8.799**
	(3.441)
Alternative Project Info	Included
Constant	-34.233**
	(5.132)
Observations	3600

Note: Random effects tobit regression, with standard errors reported in parentheses. No-bonus baseline treatment is the omitted case. ** indicates coefficient is significantly different from zero at the .01 level; * at .05; † at 0.10.

and putting projects on a more successful trajectory for ultimate funding (treatment PE20).

5 Net Returns and Self-Supporting Bonuses

We turn next to a treatment comparison of the overall funding 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 are common 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.

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.

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

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 10 reports average funding efficiency and net returns for each of the treatments. All six of the refund bonus treatments have greater efficiency and net returns than the no bonus 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).[11] Similar to the results on funding frequency presented earlier, efficiency appears to be greatest in the F6 and PE20 treatments that have more generous bonuses. The F6 and PE20 treatment efficiencies are significantly greater than the FE30 treatment efficiency (p-value < 0.05) and marginally significantly greater than the F3 treatment (p-value < 0.10). Net returns are 40 to 75 percent higher on average with refund bonuses compared to the no bonus baseline. Although they are all statistically significantly greater than the baseline, none of the net returns for the six refund bonus treatments are significantly different from each other.

The higher net fundraising returns of the refund bonus treatments raise the natural 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 key issue is whether the increased rate of fundraising success due to offering refund

[11] These statistical conclusions are based on panel models that control for experience and time trends, and robust standard errors clustering on sessions. We employ a tobit model for the efficiency comparison, considering its zero and one bounds.

Table 10: Efficiency, Net Project Returns, Refund Bonuses, and Fundraiser Returns

Treatment	Funding Efficiency	Net Returns	Ave. Total Bonuses	Average Returns: $k = 273$	$k = 250$
Baseline	0.321	99.32	–	14.62	22.42
	(0.034)	(10.66)	–	(2.07)	(2.73)
F3	0.481	152.47	-9.57	10.00	21.50
	(0.051)	(18.89)	(1.03)	(3.26)	(4.43)
F6	0.599	175.02	-15.20	12.08	**26.65**
	(0.049)	(18.20)	(2.15)	(4.56)	(5.69)
FE30	0.458	140.02	-15.53	4.23	15.22
	(0.051)	(19.35)	(1.58)	(4.19)	(5.31)
FE50	0.518	151.42	-9.47	**17.35**	**30.13**
	(0.051)	(17.39)	(1.17)	(5.54)	(6.40)
PE10	0.473	149.17	-8.79	9.37	20.62
	(0.052)	(18.83)	(0.93)	(3.33)	(4.42)
PE20	0.560	160.00	-14.64	13.47	**27.27**
	(0.050)	(17.84)	(1.95)	(5.30)	(6.32)

Note: Standard errors are reported in parentheses.

bonuses (Table 2) 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 10 presents hypothetical fundraiser payoffs for markups of 10% ($k = 273$) and 20% ($k = 250$) in each bonus treatment. The middle column shows the average refund bonuses paid in each treatment. These payments are greater for the more generous bonuses (F6 and PE20) and for treatments that have lower fundraising success such as FE30. The column labeled $k = 273$ indicates average returns for a 10% markup. The no bonus baseline has an average fundraiser return of 14.62, reflecting an average surplus of 43 realized for the 34% 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 FE50 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 56%, leading to a fundraiser surplus of 17.35 per project or 18.6% 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). Refund bonus treatments PE20 and F6 join FE50 as being more profitable than the no bonus baseline. Although the fundraiser must pay larger bonuses in these treatments when campaigns fail, the larger surplus conditional on successful funding more than offsets these payments leading to higher returns over the baseline of 18.9%, 35.2% and 21.6% respectively.

6 Conclusion

Crowdfunding is a widely used mechanism for funding local public goods. Yet most crowdfunding campaigns fail. Some campaigns should fail because they don't produce value, but we hypothesized that many socially valuable campaigns fail because the standard assurance contract has many failure equilibria. In an experimental environment similar to that found on crowdfunding sites like Kickstarter, GoFundMe and Experiment, we have shown that refund bonus schemes greatly increase the success rate of socially valuable campaigns. Our experiments were designed to explore the space of refund bonuses and thus we tested fixed and proportional refund bonuses with a particular focus on designs aimed at encouraging early contributions. All refund bonuses worked well at increasing success rates and early refund bonuses worked especially well at increasing success rates at low cost. The success of early refund bonuses suggests that in addition to eliminating failure equilibria, refund bonuses help to "seed" the contribution pool and signal cooperation to other potential contributors.

Refund bonuses can increase campaign success rates enough to pay for themselves, even taking into account that in practice not all campaigns will be successful even when they are socially valuable. Refund bonuses, therefore, are socially and privately valuable; that is, they can increase campaign success, social value and profits. A 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.

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 crowdfunding 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 (2018). 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.

Appendix B. 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.

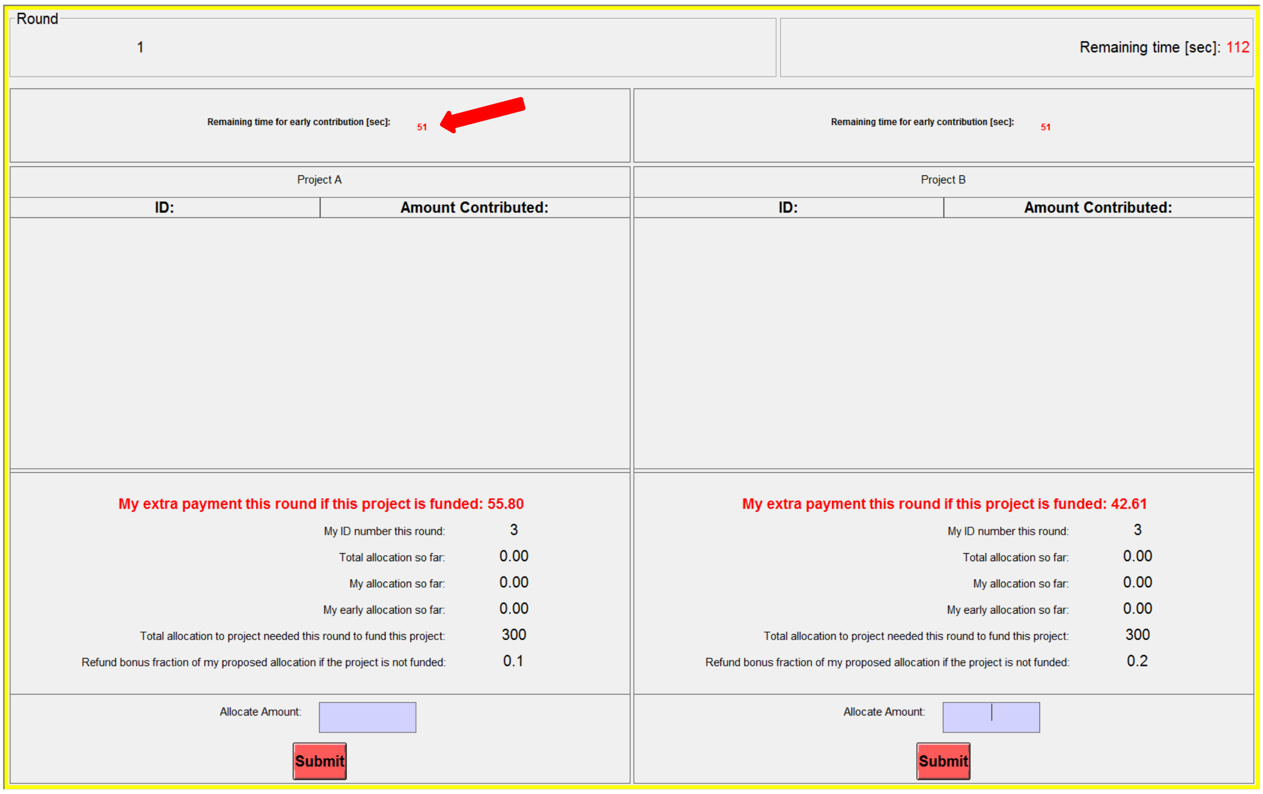
Your value for the projects

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

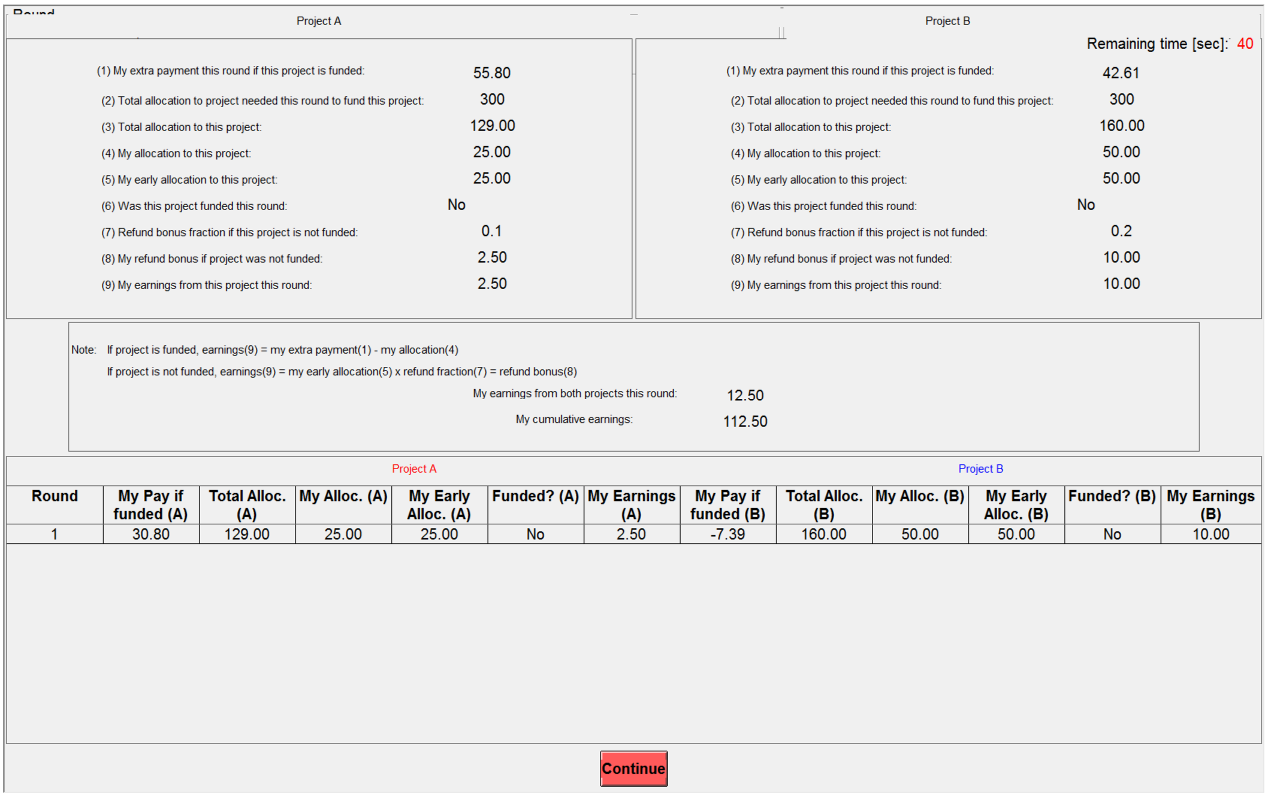
If the total amount of money that your group allocates to fund a project is less than the *Threshold*, then no group member receives an extra payment for that project. That group project is not funded. All people who allocated money to that project will have their proposed allocation amount returned. 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 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.


Project A

Remaining time [sec]: 40

(1) My extra payment this round if this project is funded:	55.80
(2) Total allocation to project needed this round to fund this project:	300
(3) Total allocation to this project:	129.00
(4) My allocation to this project:	25.00
(5) My early allocation to this project:	25.00
(6) Was this project funded this round:	No
(7) Refund bonus fraction if this project is not funded:	0.1
(8) My refund bonus if project was not funded:	2.50
(9) My earnings from this project this round:	2.50

Project B

(1) My extra payment this round if this project is funded:	42.61
(2) Total allocation to project needed this round to fund this project:	300
(3) Total allocation to this project:	160.00
(4) My allocation to this project:	50.00
(5) My early allocation to this project:	50.00
(6) Was this project funded this round:	No
(7) Refund bonus fraction if this project is not funded:	0.2
(8) My refund bonus if project was not funded:	10.00
(9) My earnings from this project this round:	10.00

Note: If project is funded, earnings(9) = my extra payment(1) - my allocation(4)
If project is not funded, earnings(9) = my early allocation(5) x refund fraction(7) = refund bonus(8)

My earnings from both projects this round: 12.50

My cumulative earnings: 112.50

	Project A						Project B					
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

Continue

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.

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