COMPETITION AND CUMULATIVE ADVANTAGE IN COLLECTIVE ACTION GROUPS

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ABSTRACT

Research on collective action problems and the provision of public goods has yielded an array of important insights into why people sacrifice for their groups, but ignores the processes that bring people into those groups in the first place. The literature is also silent on the fact that groups providing similar public goods often compete with one another to attract new members. We extend the reach of theories of collective action problems to bring them to bear on these interrelated issues. Results from four experiments support our predictions about when group members compete to attract new adherents by sacrificing more for group goals. Further, we find that prospective members join more productive groups at much higher rates and then contribute to them at high levels. These processes give way to cumulative advantage, whereby initially productive groups continually attract new members. Thus competition for members can yield large inequalities in the size and success of collective action groups.

INTRODUCTION

How do the early successes of social movements, churches, and other voluntary associations affect their subsequent success and productivity? Can successful groups expand their membership rolls without condemning themselves to a wave of free-riders? More generally, can collective action groups reconcile the incentives for group growth with the possibility that larger and growing groups will face more substantial collective action problems due to an increased risk of being invaded by free-riders?

Questions like these are fundamental, but difficult to answer with current research. With very few exceptions [e.g., Hechter (1988)], the existing literature has focused on why people join groups or why people contribute to them, but generally does not consider the two issues together. For instance, while research on collective action problems or "social dilemmas" has yielded many critical insights into the conditions that lead individuals to contribute to groups, even when they would be better off free-riding on others' efforts, these insights relate to processes that occur when individuals are already part of a stable collective action group. The literature does not answer questions about the movement of individuals into those groups and how this movement shapes the contributions of new and existing group members. This omission is important for a number of reasons. At the most basic level, most groups are dynamic, experiencing regular changes in their membership. We show that these group dynamics have critical implications for group success and productivity.

Similarly, whether (or how much) to contribute to a group may be of secondary importance to joining the group in the first place. Groups often provide overlapping public goods. As a consequence, they may compete, implicitly or explicitly, for the same pool of prospective members (Della Porta and Diani 2009, Finke and Stark 2005, McPherson 1983). For instance, individuals often have choices about which religious group to join or which political organization to get involved with. We argue below that one person's decision about which collective action group to join can alter the dynamics within groups, including that person's and others' willingness to sacrifice on behalf of group goals.

Finally, we argue that the focus of the collective action literature on groups where a stable membership is imposed exogenously can limit application of its insights in surrounding areas, like the study of religious groups, civic organizations and, perhaps most obviously, social movements. Research into these areas is acutely concerned with problems of mobilization and attracting new members (Klandermans and Oegema 1987, Snow, Zurcher Jr and Ekland-Olson 1980). Since the collective action problem literature has been silent on these critical issues, it is perhaps little wonder that its insights have been underutilized by social movement scholars.

From the other side, research into social movements and voluntary associations has yielded important insights about mobilization and the growth of groups (Della Porta and Diani 2009, Finke and Stark 2005, McPherson 1983). But work in these literatures often ignores the fact that the pursuit of collective goals can pose a social dilemma, or tension between the interests of the group as a whole and its individual members. Here we argue that an integrated understanding of the movement of new members into groups, and the collective action problems that groups must overcome to meet their goals, can shed new light on the problems confronted by social movements, religious groups, and other voluntary associations. For instance, even if religious groups attract new members, there is no guarantee that new recruits will contribute to group goals. Given that new members may free-ride on the contributions of existing members, distrust or uncertainty about newcomers' motives might lead existing members to contribute at lower levels than they would have in more stable groups. For the same reason, we might expect that group members’ uncertainty about whether new members will do their share can sow doubt about whether mobilization efforts will even benefit the group in the long run. In short, a joint consideration of mobilization and collective action problems reveals how the two processes exist in tension and that mobilization is a double-edge sword. Prior work ignores this tension since it focuses either on the collective action problem or mobilization, but not how they co-occur.

We begin bridging these gaps and shedding light on the dilemmas posed by collective action in growing groups. Even though the movement of new members into groups can exacerbate collective action problems, we aim to show that there are key conditions under which group dynamics attenuate the collective action problems groups confront. Our arguments highlight a number of important micro- and macro-level consequences of these predicted dynamics. At the micro-level, we show that competition between groups for new members impacts individuals’ decisions about which groups to join and whether or how much to contribute to them. We show that, at the macro-level, this process creates large inequalities in the size and success of collective action groups, giving way to cumulative advantage (Merton 1968), with some groups growing maximally and others growing little, if at all.

In addition to extending research on collective action problems, our demonstration that successful collective action groups can grow and continue to remain successful also contributes to the literature on cumulative advantage. That literature demonstrates how small, often arbitrary, differences between individuals or groups accumulative via an influence process or “success-breeds-success” dynamic (Merton 1968, Salganik, Dodds and Watts 2006, van de Rijt et al. 2014). For instance, van de Rijt et al. (2014) allocated signatures to randomly selected petitions on change.org. These petitions attracted more subsequent signatures from actual change.org visitors than control petitions that were not allocated the seed signatures, revealing cumulative advantage effects collective action contexts. Importantly, however, no prior work has addressed cumulative advantage effects in collective action groups where group success or growth or can *accentuate* collective action problems. Below we show how successful collective action groups can face heightened threat of exploitation by free-riders. Thus, theory and prior empirical evidence provides very clear reasons to expect that success would breed *failure* in these contexts. We outline a theoretical explanation for how initially successful groups avoid this dynamic to continue to grow and remain productive. These arguments are strongly supported by results from four new experiments.

Following a brief overview of the dilemmas faced by collective action groups, we outline our arguments and the experiments designed to test them. Though different from real-world groups in a number of ways, the laboratory-based groups we study have a number of key advantages. Most obviously, they allow us to control a wide range of factors that can influence an individual's willingness to join and contribute to a group in order to draw out clear contrasts for key theoretical variables. Further, as social movement scholars have noted (see McAdam and Boudet 2012), the literature has focused disproportionately on successful movements, potentially distorting our understanding of when and why collective action groups are successful. The method we introduce here allows us to address the dynamics of those groups that succeed as well as those that fail.

WHY COLLECTIVE ACTION IS PROBLEMATIC

The collective action literature seeks to identify and evaluate solutions to the "collective action" or "free-rider" problem.[1] The problem emerges in settings where at least some

[1] The literature has devoted much less attention to the "start-up problem" faced by potential collective action groups. The start-up problem centers on whether groups can secure sufficient initial contributions to get collective action off the ground (Heckathorn 1996, Marwell and Oliver 1993). As explained below, the empirical literature on social

individuals can materially benefit by not contributing to the public good, choosing instead to free-ride on the contributions of others (Heckathorn 1996, Olson 1965). However, when many individuals withhold contributions, collective action fails and all are worse off. Because of this tension between individual- and group-level interests, collective action generally poses a social dilemma, where what is rational at the individual level is irrational at the aggregate level.

Understanding collective action problems is important because of the ubiquity of situations in which they occur, and the often-dire consequences that result when groups fail to overcome them. As Kollock (1998) notes in his review of the literature: "Many of the most challenging problems we face, from the interpersonal to the international are at their core" collective action problems (p. 183). Of course, groups are often able to circumvent these challenges. And, as shown in a recent review (Simpson and Willer 2015), decades of experiments have identified a number of factors that tip the balance in favor of collective success, including the existence of mutually agreed upon norms and institutions governing contributions (Horne 2009, Ostrom 2000), the granting of status, reputational or other incentives to contributors (Willer 2009), and the presence of strategic or affective ties between potential contributors (Baldassarri 2015, Kim and Bearman 1997).

Importantly, the factors that prior research views as beneficial to the success of collective action are generally less prevalent in the types of groups we investigate in this paper, namely larger groups or groups where membership is relatively fluid. For instance, durable strategic or

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dilemmas generally finds that, all other things equal, cooperation or free-riding increases with increasing group size. However, larger groups may be more likely to have a "critical mass" of resource-rich or highly interested members who will be willing to get collective action off the ground (Marwell and Oliver 1993). Similarly, members of larger groups may be characterized by greater status heterogeneity, which also facilitates action by solving the start-up problem (Simpson, Willer, and Ridgeway 2012) or more generally the "volunteer's dilemma" (Diekmann and Przepiorka 2016). For these reasons, although increasing group size generally exacerbates the free-rider problem, it may attenuate the start-up problem.

affective ties are less likely among members of groups characterized by a continual influx of newcomers. More generally, Olson (1965) argued that large groups, in particular, will be unlikely to act successfully. Since Olson, researchers have debated the relationship between size and successful collective action (Marwell and Oliver 1993, Udéhn 1993), with Hardin (1982) calling the group size question "the most controversial issue in the contemporary literature on collective action" (p. 38). The decades since have yielded a number of insights into the relationship between size and success, and we now know that the effect of size is more nuanced than Olson originally suggested. For instance, some work (Bonacich et al. 1976, Isaac, Walker and Williams 1994, Marwell and Oliver 1993, Oliver and Marwell 1988) points to conditions under which the *marginal per capita return*, or the objective costs relative to benefits of contributing to the collective action versus free-riding on others' contributions, can yield a positive relationship between size and successful collective action. But when the objective incentive to contribute does not vary with group size, research finds a robust negative relationship between size and success (Bonacich et al. 1976, Fox and Guyer 1977, Hamburger, Guyer and Fox 1975, Komorita and Lapworth 1982, Marwell and Schmitt 1972). That is, despite the problematic basis for Olson's original conclusion about the detrimental effects of group size (Oliver and Marwell 1988), a wide range of evidence suggests that group size negatively impacts contributions when relevant factors are held constant. There is no single reason that cooperation is more problematic in larger groups, provided that relevant factors such as marginal per capita return on investment are kept constant. But researchers have mainly emphasized the difficulty of coordinating normative expectations and contributions (Taylor 1987, Yamagishi 1992) and the greater anonymity in larger groups (Komorita and Parks 1994).

As detailed below, our work focuses on groups that can potentially grow larger than those typically studied in laboratory experiments on collective action.[2] Although we expect to observe the predicted negative effects of group size on contributions, our aim is to demonstrate that the processes outlined in the sections to follow occur despite these well-known size effects. Indeed, our arguments point to conditions under which large groups will tend to be more successful. Likewise, we delineate conditions under which dynamic group membership facilitates, rather than hinders, successful collective action. Thus, our arguments and experiments shed light on when and how groups resolve the dilemmas entailed in mobilizing new members.

The Dynamics and Dilemmas of Growing Groups

Given its focus on fixed groups where membership is imposed exogenously, the collective action literature has tended to overlook the fact that different groups often provide substantively similar public goods, or that groups are often in (explicit or implicit) competition with one another for the same prospective members. For instance, multiple organizations aimed at addressing a given environmental problem might compete to attract members and their support (Zald and McCarthy 1979). Similarly, not only do different religious faiths compete to attract adherents, but communities often include multiple churches of a single faith or denomination, all seeking to attract the same potential adherents (Iannaccone 1991).

Indeed, there are a number of important reasons that real world collective action groups often have an interest in attracting new members and growing larger. For instance, being part of a large and growing group might act as a type of "social proof," validating members' values and

[2] To be clear, the groups we study are large only by laboratory experiment standards, not compared to many collective action groups outside the laboratory. The groups we employ are well-suited for testing the arguments outlined below. Once these theoretical mechanisms are tested in controlled laboratory settings with collective action groups of manageable sizes, we will have more confidence in applying this understanding to the dynamics of larger groups in less controlled contexts.

beliefs, or the overall mission of the group (Cialdini and Goldstein 2004). If so, members will likely derive greater psychological and emotional benefits from larger or growing groups. Similarly, a large or growing group may be more apt to convey broader support for its cause outside the group (e.g., to political actors). Finally, assuming that free-riding can be suppressed, larger groups generally produce larger public goods (Oliver and Marwell 1988).

There are obviously circumstances in which groups have an incentive to remain small and thus avoid recruiting or admitting new members. Indeed, in some small, close knit groups there may be unambiguous *disincentives* for group growth. Such cases lie outside the scope of our arguments. We are instead interested in groups that have either a positive incentive to recruit and admit new members, or at least no explicit disincentive to do so. Groups that meet these scope conditions are not only substantively important; they are theoretically interesting since the potential benefits of attracting new members often must be weighed against the costs associated with increasing group size discussed above. For instance, Kerr (1989) shows that both perceived self-efficacy (the perception that one's contributions will make a difference to group success) *and* perceived collective efficacy (the perception that the group will reach a critical provision point) decline with group size *even when objective efficacy is held constant*. Thus, dynamic collective action groups face a further dilemma when the incentive to grow is in tension with the (real or perceived) difficulty of coordinating and maintaining contributions in larger groups. That there is no guarantee that new members will contribute at levels similar to those of existing members only compounds this dilemma.

Joining and Contributing to Dynamic Groups

In markets where different groups provide overlapping public goods and individuals have choices about which groups to join, we expect individuals to join groups that produce larger or more highly valued goods (see, e.g., Gürerk.et al. 2006). As a consequence, when groups benefit from adding to their ranks, we further expect that existing members of collective action groups will contribute more to group goals in order to attract new members. Similar patterns have been observed in the literature on partner selection (Barclay and Willer 2007), which finds that people are more cooperative in dyadic interactions when their behavior is observable by potential future partners.

Importantly, we argue and empirically demonstrate that these effects will depend on key features of markets for public goods. Specifically, we expect that existing group members will contribute more to their group when *i*) there is an incentive to attract new members, i.e., when existing members benefit from larger groups, and *ii*) prospective members have choices about which groups to join (versus when only one group provides the good, or the individual has access to only one group).

We have thus far argued that markets or environments where groups benefit from growing their numbers *and* individuals have choices about which groups to join will generate higher contributions from existing members of collective action groups. Further, we argued that when individuals have choices about which groups to join, they will join those groups that have produced larger public goods. Importantly, we also expect individuals to be more apt to join larger groups in those contexts where there are benefits to larger groups. Taken together, we expect these effects to produce a path-dependent process, resulting in inequality in the size of collective action groups, as we now show.

From a pure rational egoist standpoint, individuals may join larger and/or more productive groups but will not contribute to them (see Appendix A1). But research is generally more consistent with the view that individuals recognize the ineffectiveness of narrowly pursuing self-interest through free-riding on others, and instead adopt a goal of mutual cooperation (Kollock 1998, Ostrom 1998, Pruitt and Kimmel 1977, Yamagishi 1986). From this perspective, the primary obstacle to contributions to collective action is the lack of knowledge that others have also accepted this goal and will contribute. Those groups that have produced relatively large public goods provide compelling evidence to prospective members and newcomers that they have accepted the goal. Thus, we expect that newcomers will generally follow extant norms of cooperation and contribute at high rates. That newcomers' contributions will be strongly influenced by normative levels of contributions is consistent with a number of recent empirical studies. For instance, Gürerk et al. (2006) show that people adopt the norms of groups they join, even when doing so fails to maximize (short-term) individual outcomes. Other work shows that having information on descriptive norms has powerful effects on behavior, even when there are no sanctions for violating these norms (Irwin and Simpson 2013, Keizer, Lindenberg and Steg 2008, Schultz et al. 2007).

If newcomers move into the most productive groups and contribute at normative levels, initially productive groups will remain so. Other prospective group members will subsequently join those groups that have gained an early advantage in group size and contributions. Thus, we expect cumulative advantage (Merton 1968, Salganik, Dodds and Watts 2006, van de Rijt et al. 2014) whereby initially more cooperative groups will immediately attract new members and this small advantage on group size will result in further gains in membership. At a macro level, this will imply substantial inequality in the size and productivity of groups. Again, we expect these effects to be especially apparent in those public goods markets in which groups benefit from growing and prospective members have choices about which groups to join.

HYPOTHESES

The forgoing arguments yield a number of key hypotheses. At the micro-level, all else equal, we expect:

> **Hypothesis 1**: When individuals have choices about which groups to join, they will tend to join (***a***) groups that produce larger public goods; and (***b***) when groups benefit from growth, larger groups.
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> **Hypothesis 2**: New members will tend to adopt the norms of groups they join. That is, their contributions will not differ from those of existing group members.
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> **Hypothesis 3**: In environments in which individuals have choices about which groups to join *and* groups benefit from increasing size, existing group members will contribute more to public goods compared to environments where (***a)*** individuals cannot choose which groups to join, or (***b***) groups do not benefit from increasing size.

An important macro-level implication of Hypothesis 3 is that those environments where groups benefit from growing larger and individuals have choices about which public goods groups to join will tend to yield larger public goods. We expect several other macro-level outcomes.

> **Hypothesis 4**: In environments where groups benefit from growth and individuals have choices about which groups to join, there will emerge *inequalities in the amount of public good produced by groups* compared to when (***a***) potential members don't have choices about which groups to join, and (***b***) there are no benefits to increases in group size.
>
> **Hypothesis 5**: In environments where groups benefit from growth and individuals have choices about which groups to join, there will emerge *inequalities in the size of groups* compared to when (***a***) potential members don't have choices about which groups to join, and (***b***) there are no benefits to increases in group size.

Note that even without competition between groups, we would expect some variation in the productivity of groups, or in the contribution levels of individuals within groups, due to the proportion of "prosocial" group members (as detailed in Appendix A3.2), the number of people who strongly value the public good, or other group-specific dynamics. Indeed, our third experiment finds some inequality in the success and size of groups even when there are no benefits to group growth. Likewise, a given community or environment may have only a single group seeking to provide a public good. We should reasonably expect some variation, or inequality, in the ability of a given group to attract new members and provide the good. But

Hypotheses 4 and 5 state that inequalities in the success and size of groups will be particularly large when groups benefit from growing and prospective members have choices about which groups to join.

Overview of Experiments

The hypotheses above predict how and when markets for public goods provision and competition among public goods groups for members will produce larger public goods, and inequalities in the size and success of collective action groups. We conducted four experiments on the provision of public goods to test these hypotheses. Social scientists have long studied contributions to public goods as the quintessential collective action problem, pitting individual self-interest against collective welfare (Simpson and Willer 2015). While there are many variations on the basic design, nearly all public goods experiments have each individual in a group decide how much, if any, of some personal endowment to contribute to a public good. In most studies, the public good is *non-excludable, i.e.,* it is impossible to exclude those who do not contribute to the public good from consuming it. Instances of real world collective action problems characterized by non-excludability are ubiquitous: those who drive their own cars cannot be excluded from the increase in air quality that results from their fellow citizens taking public transportation; those who do not contribute to public radio cannot be excluded from listening to programming paid for by others' contributions; and those who do not show up to protest events cannot be excluded from government policies that result from changes demanded by the movement. In standard laboratory experiments, excludability is captured by having all group members receive an equal share of any public good provided, regardless of whether (or how much) they contributed to its provision.

Importantly, previous work has shown that behavior in standard public goods experiments predicts behavior in real world social dilemmas. Laury and Taylor (2008) found that contributions in a public goods experiment predicted subsequent contributions to a real world public good. Likewise, Grossman and Baldassarri's (2012) study of Ugandan farmers showed that behavior in their experimental context predicted participation in -- versus free-riding on -- a farmer cooperative.

Our first three experiments modify the classic design to allow us to study multiple collective action groups, as well as group dynamics. Our fourth experiment modifies the classic design further, such that the beneficiaries of public goods are third parties, rather than the participants themselves. This allows us to assess whether our arguments apply to the broad class of groups that aim to benefit those who are not involved in the collective action.

In total, 432 participants took part in our four experiments. For the first three studies, participants were recruited from the undergraduate population at a large public university in the U.S. on the basis of the opportunity to earn money. Our fourth experiment also used a convenience sample, but a substantially more diverse one that included students and non-students in the United Kingdom. Table A1 in the Appendix presents demographic information for participants in all four experiments. Ancillary analyses available upon request showed that no demographic variable significantly impacted our outcome variables, nor did any demographic variable interact with our key independent variables. We therefore drop them from the analyses reported below.

After presenting the procedures and results of the first three experiments, we turn to a series of analyses that compare results across these studies. Thereafter, we present the findings of a final study that relies on a different design. To preview, Table 1, gives an overview of which hypotheses we tested in each experiment and which we tested via comparisons across experiments. As the table shows and as will be detailed fully in the sections to follow, all hypotheses received support in all tests.

EXPERIMENT 1: DESIGN AND PROCEDURES

A total of eight 12-person groups participated in Experiment 1. Immediately upon entering the laboratory, each participant was escorted to a private subject room with a computer. The experiment was programmed and conducted using the software z-tree (Fischbacher 2007). After reading and competing a consent form, participants were presented with computerized instructions for the task. All instructions were followed by extensive comprehension checks and any missed questions were followed with an explanation of the correct answer.

Our first experiment crossed a between-subjects factor (the impact of an individual's contribution on the amount of public good produced, i.e., the *marginal per capita return*) with a within-subjects manipulation (whether individuals had choices about which groups to join). The order in which groups received the within-subjects factor was randomized across sessions.

Initial Groups and the Public Goods Context

The instructions explained that, at the beginning of the task, six of the twelve participants would be randomly assigned to groups (three to Group A, and three to Group B) and the remaining six would begin as bystanders. Further, participants were told that members of Group A would make decisions only with other members of Group A, and similarly for members of Group B. Finally, they were told that bystanders would not make choices in the decision situation. Following prior work (Weber 2006), to eliminate any experiences of inequity from bystanders, participants were told (correctly) that bystanders would "be paid a flat (and fair) rate for each round" that they were bystanders. The instructions then fully explained the types of decisions participants would be making.

After completing the instructions, participants were assigned to one of the two groups or to the pool of bystanders. Thereafter, members of each group made decisions in the first round of the public goods games. As is standard, each group member was given a pool of 20 monetary units (MUs) each round. She decided how many, if any, of those 20 MUs to invest in her group's public fund and how many to keep in her private fund. The participant would be paid for MUs kept in her private fund, as well as earnings from the public fund, at the end of the study.

Following standard designs, MUs contributed to the public fund were subject to a multiplier (y) and then shared equally among all members of the participant's group. Contributions to Group A (B) had no direct bearing on earnings in Group B (A). Individual shares from the public good were determined by the size of that multiplier, i.e., the impact any single individual could have on the public good, as well as the size of the group, N. That is, the *marginal per capita return* = y/N. All other things equal, larger *marginal per capita returns* (MPCR) – stemming from increases in y and/or decreases in N – yield higher levels of cooperation (Isaac and Walker 1988). Because groups in our experiments can grow, the MPCR can change endogenously over time, since MPCR decreases as N increases. That is, all other things equal, members have a smaller incentive to contribute to the public good as groups grow larger. (Table A2 in the Appendix presents the incentive structure for contributing to groups of various sizes.) Since the effects of our treatments might be influenced by this endogenous change in MPCR, varying MPCR exogenously allows us to assess whether MPCR moderates any treatment effect. Our first experiment thus included a high (y = 2.5) and low (y = 1.5) MPCR condition, manipulated between subjects. These values are similar to those used in prior work that investigates the impact of MPCR and group size on collective action (Isaac and Walker 1988; Isaac et al. 1994).

At the conclusion of each round, all participants, including bystanders and members of both groups could observe contribution levels and individual earnings from each group. Thus, bystanders had full information about which group, if either, was more productive. Similarly, members of each group knew not only their own outcomes but also those from the other group.

Following our earlier discussion of the incentives entailed in being part of a large or growing group, in addition to earnings from the public good and from their private accounts, group members also received a group size bonus. Specifically, for each round, each group member received three MUs for each member of his or her group, thus creating an incentive for groups to attract new members. Although no such parallels were included in the instructions to participants, the group size bonus might correspond to the psychological, emotional, tangible or monetary benefits that members may derive from being a member of a larger group.

Choice Manipulation

After the initial round of contributions, a single bystander was selected to join one of the two groups. Following our theoretical arguments, our key (within-subjects) manipulation was whether bystanders could choose which group to join, or whether they were randomly assigned to a group. Whether groups experienced the choice versus no choice condition first was determined randomly. In the choice condition, the selected bystander was told that he or she was randomly selected from among existing bystanders to join one of the groups. Thereafter, she indicated whether she wished to join Group A or B. In the no-choice condition, the selected bystander was simply told that he or she had been randomly picked to join one of the groups and then randomly assigned to one of them.

After the selected bystander indicated which group she wished to join (or was randomly assigned to one of the groups in the no choice condition), all other participants were informed and the next round of contributions started. The instructions made clear that, once a bystander became an active group member, she would remain so until the end of the interaction periods for that phase. Thus, former bystanders were just like any other group member once they joined a group. One participant was randomly selected from the pool of bystanders to become a group member after every round until no more bystanders were remaining. Thus, beginning with the seventh round, groups were at their maximum size. For each phase (choice and no-choice), participants were told, accurately, that they would continue to make choices for three contribution rounds after the final bystander joined a group. Thus, each phase concluded after nine rounds.[3]

After the first phase (condition) participants were notified via computer that the procedures would change. Those who first took part in the choice condition then received instructions for the no choice condition and vice versa. These new instructions were followed by another series of comprehension checks. Thereafter, each participant was again randomly assigned to Group A, B, or the pool of bystanders. The second phase also lasted for nine rounds. Thereafter, participants were paid based on their earnings (average = $19) and debriefed. The entire procedure took about 70 minutes. There was no deception.

RESULTS OF EXPERIMENT 1

Figure 1 displays contribution rates (1a) and group sizes (1b) over time, averaged over all eight sessions of Experiment 1. As can be seen in Figure 1b, the sizes of the groups in the no-

[3] That participants knew when each phase would end created conservative tests of our hypotheses, since that knowledge leads to decreases in cooperation over time (see Appendix A1). But we expect that the effects predicted by our hypotheses will occur above and beyond this decline.

choice condition were balanced, given random assignment of bystanders. But Figure 1b shows a clear cumulative advantage for group sizes in the choice condition. For that condition, in all of the eight sessions, one of the two seed groups grew continuously, attracting at least five of the six bystanders throughout the session. We call these *popular* groups. The other, *unpopular*, group attracted no bystanders in five sessions and only one bystander in three sessions. Attracting at least five of the six bystanders in all eight sessions if bystanders chose to join a group randomly is highly improbable, $p < .001$. These patterns provide strong support for the impact of choice on the generation of cumulative advantage in group sizes (Hypothesis 5a), as detailed more fully below.[4]

Turning to contributions, Figure 1a shows that all groups experience the characteristic decline in contributions over time (Ostrom 2000, Sell and Wilson 1991). The extent of this decline is substantial, which is unsurprising given that all participants had full information about the number of rounds they would interact. Despite this decline, contribution rates in the popular groups are consistently higher than those in the both the unpopular groups of the choice treatment and groups in the no choice condition. To assess statistical significance, we regressed contribution decisions on experimental condition. To deal with potential dependency due to nesting of decisions in subjects, we fitted multilevel models with random effects for participants. We also included random effects for sessions, given that decisions within a session are likely correlated. These results are given in Table 2.[5]

[4] We report p-values for two tailed tests throughout the paper unless stated otherwise.
[5] We performed extensive robustness checks on contribution data presented in Table 2, all available upon request. First, we fitted a number of alternative specifications, such as regressions with fixed effects for subjects, fixed effects for sessions, multilevel tobit regressions (with contributions censored below zero and above 20), and cluster adjustment of standard errors instead of multilevel models. We also fitted a four-level multilevel model with random effects for groups within sessions, the results of which showed that variance at the group level is statistically insignificant when session level variance is accounted for. The results in Table 2 are robust with respect to all these alternative specifications. Additionally, we checked whether the order in which groups received the no choice versus

Model 1 shows the overall treatment effect without any controls. In the no-choice condition, subjects contributed on average 5.6 points. Consistent with Hypothesis 3a, participants contributed on average about 1.3 more MUs in the choice condition, a statistically significant and substantial increase. Model 2 introduces controls for decision round, a dummy variable for high MPCR, group size, and a dummy equaling one if the group is unpopular. Results show that popular groups contribute about 2.4 more MUs than unpopular groups. Note, however, that contributions in unpopular groups are not significantly different than contributions in the no-choice condition. That is, the sum of the coefficients of choice and unpopular in Model 2 is close to zero and non-significant (difference = -.127, p = 0.8). These results suggest that benefits to group competition accrue solely via the larger, popular, groups. They further suggest that, while the benefits are restricted to popular groups, the aggregate effects of group competition are positive and substantial.

Other results in Model 2 closely replicate well-known findings from prior work. MPCR has a strong effect on contributions: contributions are 3.9 points higher in the high MPCR condition. Importantly, however, MPCR does not moderate the impact of any other variable in the model. That is, none of the interaction effects that involve MPCR is statistically significant, suggesting that all key outcomes are robust across different levels of MPCR. As also expected, group size has a significant negative effect on cooperation: an additional member in a group decreases contributions about half a point. Finally, contributions decrease about 0.16 points per decision round, consistent with prior work.

Model 3 introduces a dummy variable, *newcomer*, indicating the bystander who joins a group in a given round. The coefficient for this variable tells us whether a newcomer's behavior

choice condition affected outcomes. There was no main effect of order of choice treatment, nor did this variable interact with any of our predictor variables. Thus, we do not discuss it further.

differs from existing group members. As a control, the model also includes another dummy variable, *seed member*, denoting the three original group members. Model 3 finds that neither the seed members' nor newcomers' behaviors differ significantly from other group members. This result supports Hypothesis 2, which predicts that newcomers will give at normative levels once they become group members. The difference between seed members and newcomers is also statistically insignificant. Finally, we do not observe a significant interaction of seed (LR chi-square(2) = 1.79, $p = 0.401$) and newcomer (LR chi-square(2) =1.24, $p = 0.537$) with the dummies for choice condition and unpopular groups. Hence, in no condition is there evidence that newcomers' and seed members' contributions differ systematically from other group members.

Finally we turn to the dynamics of bystanders' choices about which group to join. As discussed above, in all sessions one group grew continuously while the other group either did not grow, or grew only slightly. As can be clearly seen in Figure 1a, round 1 contributions are higher in the popular groups compared with the unpopular groups in the choice condition (difference = 4.4, $z = 2.16$, $p = 0.031$). This initial difference in contributions results in nearly maximal asymmetries in final group sizes, and this outcome emerged out of the process specified by our theoretical argument. That is, when they can choose, the first bystanders choose the group with higher contribution rates, consistent with Hypothesis 1a. The remaining bystanders then simply follow the first bystanders for two reasons. First the popular group is larger and thus yields a larger group size bonus (Hypothesis 1b). Second, cooperation rates are always higher in the popular groups, as noted earlier. Appendix A3 gives raw data from all sessions of the choice condition, and a fuller discussion of these dynamics.

To show these effects statistically, Model 1 in Table 3 presents a conditional logit model in which the choice of a bystander is predicted by the differences in average cooperation rates and group sizes between the two groups in the previous round. Results show that bystanders choose larger groups (Hypothesis 1b) and groups with higher contribution rates (Hypothesis 1a). The odds of choosing the group with one point higher average contribution is 2.8 (exp(1.036)) times higher. Similarly, the odds of choosing the group with one more group member is 3.9 (exp(1.358)) times higher. Both of these effects are substantial.

Note that in the first round the two groups are identical in size. Thus, bystanders' choice is influenced only by differences in contribution rates in the first round. Model 2 includes interactions of the two variables in Model 1 with decision round. Results of these interactions suggest that the weight bystanders attach to the difference in contribution rates decrease significantly over time but there is no evidence that the bystanders attach less weight to the group size differences over time.

As already noted, we find strong support for a predicted macro-level outcome of this process, namely differences in group sizes (Hypothesis 5a). To test the other macro-level prediction (Hypothesis 4a), we calculated the absolute difference between average contribution rates in the two groups in a given round as an aggregate-level measure of inequality in the amount of public goods produced. This gives us 144 aggregate-level observations (eight sessions, each with 18 rounds, i.e., 9 rounds of interaction in the choice-condition and an additional 9 rounds of interaction in the random condition).[6] We regressed the difference in average contribution rates on the choice dummy with a multilevel model for random effects for

[6] We could have instead calculated differences in the *sum* of contributions between groups instead of averages as our measure of inequality in contributions. Inequalities in the sum scores in the choice condition would certainly be higher compared with the no-choice condition, since popular groups have more members compared to unpopular groups. Comparing averages thus provides a more conservative test of Hypothesis 4a than comparing sums.

sessions to deal with dependencies within sessions. Results show that the average absolute difference in average contribution rates in the random assignment condition is about 2.63 MUs. In the choice condition this difference is 1.08 MUs higher ($z = 2.48$, $p = 0.013$), corroborating Hypothesis 4a.

Discussion of Experiment 1

To summarize, results of Experiment 1 strongly support all hypotheses tested. There are clear cumulative advantage effects, with maximum asymmetry in the final sizes of groups when individuals have choices about which collective action groups to join. When they do, bystanders choose larger groups and groups with high contribution rates. Once in those groups, these newcomers contribute at normative levels. Thus, the initially higher cooperation levels in popular groups remain high. This is an important result since, compared with unpopular groups and groups where individuals are randomly assigned to groups, popular groups are much larger and experience a constant inflow of new members. Further, as noted earlier (see also Table A2 in the Appendix), the incentive to defect increases with increases in group size. As discussed earlier, all these factors normally portend collective action failure. On the other hand, contribution rates in the unpopular groups are nearly identical to those in the condition where people cannot choose which groups they join. Hence, competition for attracting bystanders only has positive consequences for macro-levels patterns of cooperation, due to enhanced cooperation in those groups that dominate the competition.

We sought to replicate these results in our second experiment. Given the strong support for our arguments linking competition for members to higher contributions and greater asymmetry in the size and productivity of groups, we aimed to assess the robustness of these results when the incentive to grow groups was lower than in Experiment 1. Our second study thus reduces the group size bonus substantially. We describe this and several additional changes we introduced in the section to follow.

EXPERIMENT 2: DESIGN AND PROCEDURES

A total of eleven 12-person groups participated in Experiment 2. The experiment took place in the same laboratory as Experiment 1 and the design and procedures were identical, with several exceptions.[7]

Group Size Bonus: Experiment 1 provided strong support for our key claims. Indeed, we were surprised by size of the cumulative advantage effects we observed in the first study. In all eight of the groups, either all or all but one bystander elected to join the same group. Given these effects, one goal of Study 2 was to assess our primary predictions with a substantially smaller group size bonus. Experiment 2 reduced the group size payoff from three to one.

Controlling for Group Size: In the no-choice condition of Experiment 1, bystanders were randomly assigned to groups. This gave us a baseline against which to compare inequality in the size of groups when individuals had choices about group membership (Hypothesis 5a). But the very large inequality in groups (in size and earnings) observed in the choice condition of Experiment 1 made a controlled contrast with the equal size groups of the no-choice condition more difficult. To allow a more direct contrast, our second experiment yoked group dynamics for the no-choice condition of each experimental session from the choice condition of the prior experimental session. For instance, imagine that in the choice condition of experimental group *t*, the first two bystanders joined Group A, the third and fourth joined Group B, and the remaining bystanders joined Group A. Then, in the no-choice condition of session *t* + 1, the first two

[7] We detail one other addition we made to Experiments 2 and 3 in Appendix A3.2. Briefly, approximately two weeks prior to the experiment, participants completed an online measure of *social values*, i.e., stable preferences for how outcomes are distributed between oneself and others. The Appendix uses data from this pre-survey to provide further validation of our arguments.

bystanders would be assigned to Group A, the next two would be assigned to Group B, and so on. (Participants were not informed of the mechanism for the assignment of bystander to groups. They were simply told that it would be determined by computer.) This method allowed us to match group sizes and growth patterns across the choice and no-choice conditions.

Marginal Per Capita Return: Finally, while MPCR had an impact on contributions in the first experiment, it did not moderate any of our key findings. Having established that our main effects are robust across varying levels of MPCR, we dropped the low MPCR condition for Study 2. This leaves us with one within-subjects factor, choice versus no-choice. Again, we randomized the order in which sessions were exposed to the choice condition.

RESULTS OF EXPERIMENT 2

In Experiment 2, we observe a cumulative advantage with respect to group size in six of the eleven sessions: in those sessions, one of the two groups grew continuously, reaching a final group size of at least eight. In the remaining five sessions, no group emerged as popular (or unpopular). Instead, the two groups kept competing to attract bystanders until no more remained. We call these *intermediate* groups. (See Appendix A3.2 for a detailed analysis of contributions and dynamics in these intermediate groups.)

Figure 2 displays contribution rates across conditions. As noted above, Experiment 2 balanced the no-choice and choice conditions with respect to group size. Thus, the no-choice condition also includes unpopular and popular groups in five of the eleven sessions and intermediate groups in the remaining sessions. As expected, contribution rates were statistically indistinguishable in the popular, unpopular, and intermediate groups of the no-choice condition (Chi-square(2) = 0.67, $p = 0.72$). Thus, for simplicity, Figure 2 and subsequent analyses collapse contributions from all groups in the no-choice condition.

The patterns of Figure 2 appear highly consistent with our Experiment 1 results: contributions in the popular groups of the choice treatment are higher than the unpopular groups of the choice condition and all groups in the no-choice condition. The figure also suggests that contributions in the unpopular groups are indistinguishable from those in the no-choice condition. Finally, intermediate groups in the choice condition appear to cooperate as much as the popular groups.

To assess the statistical significance of the Figure 2 patterns, we fitted the same multilevel regression models as in Experiment 1. These models are given in Table 4. Without any controls (Model 1), contributions are about one MU higher in the choice treatment compared with the no-choice condition, following Hypothesis 3a. Model 2 introduces two dummy variables corresponding to the popular and unpopular groups in the choice condition, as well as controls for decision round, and group size. As expected, contributions in the popular groups are substantially higher than contributions in the no choice condition ($1.130 + .324 = 1.454$, $z = 3.92$, $p < 0.001$). Moreover, the significant coefficient of the "choice" variable indicates that intermediate groups in the choice condition also contributed more than groups in the no-choice condition. Finally, contributions in unpopular groups are significantly lower than contributions in intermediate groups, but statistically indistinguishable from groups in the no-choice condition ($1.130 - 1.314 = -0.18$, $z = -0.35$, $p = .726$). As before, given that participants were aware of the endpoint, contributions decrease significantly over time. But, somewhat surprisingly, the negative effect of group size on contributions is statistically insignificant.

Model 3 adds variables for seed members (i.e., the original members of the group) and newcomers. As in Experiment 1, these terms show no evidence that contributions of seed members or newcomers differ from contributions of other group members, nor do seed members and newcomers' contributions differ from each other. These findings once again support our claim that newcomers to groups adopt established contribution norms (Hypothesis 2). We also searched for interactions of these two variables with the choice condition as well as with the dummy variables for popular and unpopular groups. None of the interactions approached statistical significance (LR chi-square (5) = 4.63, $p = 0.463$), further supporting Hypothesis 2.

The main results for bystanders' choices, shown in Table 5, are also highly consistent with those of Experiment 1. Bystanders chose larger groups (Hypothesis 1b) and groups with higher average contributions (Hypothesis 1a). Unlike Experiment 1, however, the effects of these two variables on group choice do not change over time. That is, interactions of these two variables with decision round are statistically insignificant. Because bystanders care about contributions and group sizes approximately equally throughout the experiment, smaller but high contributing groups can still attract bystanders in later rounds. (See Appendix A3.2 for more detail.) As a consequence, we find cumulative advantage effects with respect to group size in just over half (six of 11) of the sessions, as noted above.

As before, we also tested the macro-level Hypothesis 4a by regressing the difference in average contribution rates in the two groups on the choice dummy, with random effects for sessions. As in Experiment 1, and consistent with the hypothesis, the absolute difference in average contribution rates in the two groups is significantly higher in the choice condition compared with the no-choice condition (difference = 0.88, $z = 2.61$, $p < 0.01$). Thus, we once again observe cumulative advantage effects with respect to contributions.

Discussion of Experiment 2

Summing up, these results strongly support all tested hypotheses and replicate the basic findings from Experiment 1. As expected, the main difference is that, once we lower the incentive for existing members to attract newcomers into the group, the macro-level inequality in group sizes decreases, but does not disappear. Moreover, we still observe a powerful effect of choice on the overall size of the public good produced, as well as inequalities in the sizes of public goods that groups produce. We compare the results of Experiments 1 and 2 in more detail later. We now turn to our final experiment, the primary goal of which is to assess whether the effect of choice on group size and contributions disappears when the incentive for attracting new members is eliminated altogether.

EXPERIMENT 3: DESIGN AND PROCEDURES

A total of eleven 12-person groups took part in Experiment 3. The physical location of the experiment was identical to Experiments 1 and 2 and, with one exception, the procedures were identical to Experiment 2.

Removal of Group Size Bonus. One of our main goals was to further confirm the logic underlying our arguments by showing that key effects observed in our first experiments would be substantially weaker once the incentive to attract additional group members was eliminated altogether, that is, when there was no incentive for growing groups. Demonstrating this would allow us to clearly establish the conditions under which markets for the provision of substitutable public goods will lead to larger public goods and inequality in the size of public goods.

RESULTS OF EXPERIMENT 3

Figure 3 displays contributions in Experiment 3 and Table 6 presents regression results. We observe a similar pattern as in Experiment 2. Interestingly, in four of the eleven sessions one of the two groups managed to attract at least five of the six bystanders in the choice condition despite the fact that there was no group size bonus. Unlike Experiment 2, however, contribution rates in intermediate groups in the choice condition are not different from contribution rates in the unpopular groups, nor do they differ from contributions in the no-choice condition.[8]

Results of Model 1 in Table 6 show that the overall treatment effect is quite small and statistically insignificant, as predicted by the logic underlying Hypothesis 3b about when we should observe effects of group choice on contributions. Model 2, on the other hand, shows that contribution rates are significantly higher in popular groups compared with unpopular groups, intermediate groups, and groups in the no-choice condition. Contributions in the latter three cases are statistically indistinguishable. Effects of other variables, including round, group size, seed member, and newcomer, are very similar to their effects in Experiments 1 and 2. The absence of an effect for newcomers shows that, once bystanders move into groups, they contribute at normative levels, once again supporting Hypothesis 2.

The model in Table 7 predicts choices of bystanders about which group to join. As before, bystanders choose groups with higher contribution rates (Hypothesis 1a). Yet, different from the first two experiments, and in line with our prediction (Hypothesis 1b), group size has a negative, but non-significant, effect on bystanders' group choices. Thus, the emergence of some popular groups in this experiment is driven completely by the higher contributions in those groups.

As in the first two experiments, we tested whether the difference in average contribution rates in the two groups differ across the choice and no-choice conditions. To be sure, we do not expect differences between conditions in Experiment 3, as there are no benefits to increases in group size (see Hypothesis 4b). Consistent with our arguments, while the difference is still

[8] Appendix A3.2 provides further evidence that contributions in intermediate groups in Experiment 2 are relatively high, and contribution rates in intermediate groups in Experiment 3 are relatively low, because participants in Experiment 2 were competing to attract new group members, as predicted by our arguments.

positive as before, it is smaller and statistically insignificant (difference=0.33, $z = 1.10$, $p = 0.271$).

Summary of Results and Comparisons Across Experiments 1-3

Having established strong and consistent support for all the hypotheses tested thus far (see Table 1 for a summary), we now turn to a comparison of our first three experiments, which were conducted in the same setting and based on the same design. Recall that, in all sessions of Experiment 1, one of the groups dominated the other in attracting bystanders. We observed these cumulative advantage effects in, respectively, six (of 11) sessions of Experiment 2 and four (of 11) sessions of Experiment 3. The dynamics of bystanders' choices reveal the mechanisms underlying differences in these macro-level outcomes across experiments. In Experiment 1, where the group size bonus was substantial, the first few bystanders chose the group with higher contribution rates. Differences in contributions between groups played a smaller role for subsequent bystanders as they simply joined the larger group due to the substantial group size bonus. Consequently, in Experiment 1, initial differences in contribution rates in choice groups consistently resulted in maximal asymmetries in final group sizes. In Experiment 2, where the group size bonus was reduced threefold, we observed this same pattern in six of the eleven sessions. However, in contrast to Experiment 1, bystanders' choices about which group to join were equally affected by contributions *and* group sizes and these effects continued even in the latter rounds. This maintained competition between the two seed groups to attract bystanders. As a consequence, in those five worlds where no group was popular (or unpopular), contribution rates were as high as popular groups in Experiments 1 and 2.

In Experiment 3, where there was no group size bonus, bystanders cared only about contribution rates, and if anything, avoided larger groups. Nonetheless, in the minority of worlds in which there was a substantial initial difference in contribution rates, bystanders kept joining the more cooperative group and conforming to this high cooperation norm. In such cases, we observed the same cumulative advantage effect regarding group sizes as in the first two experiments. In the majority of worlds in Experiment 3, however, the two groups did not differ in size or contribution rates, even in the choice condition.

To more formally asses the macro-level outcomes predicted by Hypothesis 5b, we compared asymmetries in group sizes in the choice condition across the three experiments as follows. We first calculated the absolute difference between the sizes of the two groups in a given round of the choice condition. In the no-choice condition, we either randomized (Experiment 1) or yoked (Experiments 2 and 3) assignment of bystanders to groups. Thus, we do not consider data from the no-choice condition in the current analysis. This yields 270 observations, one for each of nine rounds, in 30 sessions of the three experiments.

We then regressed the group size difference score on dummies for experiments, with random effects for sessions.[9] Results show that the difference in group sizes in the choice condition, averaged over all rounds, is about 1.6 in Experiment 3. In Experiment 1, average group size difference in the choice condition is about 1.58 points higher, a substantial difference ($z = 3.87$, $p < 0.001$). Similarly, the expected value of the average group size difference in Experiment 2 is 0.65 points higher compared with Experiment 3 ($z = 1.82$, p (one-tailed for directed hypothesis) = 0.035). We also observe a significant interaction of decision round with experiment. In Experiment 3, inequality in group sizes increases about 0.28 points each round ($z=8.42$, $p < 0.001$). The increase in inequality over time is much steeper in Experiment 1 (difference = .44, $z = 8.56$, $p < 0.001$) and Experiment 2 (difference =.16, $z = 3.50$, $p < 0.001$),

[9] A Poisson regression specification (available from the authors), which models the group size difference as a count variable yields substantially very similar results.

resulting in much larger inequalities in the last rounds in Experiments 1 and 2 compared with Experiment 3. There are also larger inequalities in group size in the final rounds of Experiment 1, compared to Experiment 2 (difference = .27, $z = 5.35$, $p < 0.001$). Overall, these results clearly support Hypothesis 5b, which predicts that markets where groups have larger incentives to attract newcomers result in larger inequalities in the size of groups.

These differences in the dynamics of attracting bystanders across the three experiments also resulted in differences in contribution rates, consistent with Hypothesis 3. Recall that, in Experiment 3, contribution rates in the choice condition were not significantly different from the no-choice condition, as predicted. In Experiments 1 and 2, however, contributions were significantly and substantially higher in the choice treatment compared with the no-choice condition. To test whether the treatment effects differed statistically across the three experiments we fitted a single multilevel model to the data from all experiments with random effects for 30 sessions and 360 subjects. Results show that the overall treatment effect (coefficient of choice without any controls) is significantly lower in Experiment 3, compared with Experiment 1 (difference = .90, $z = 2.78$, $p < 0.01$) and Experiment 2 (difference = .78, $z = 2.64$, $p < 0.01$). The difference between the overall treatment effects in Experiments 1 and 2 is not statistically significant ($z = 0.36$, $p = 0.72$). Thus, competition for attracting bystanders improved overall cooperation only when there was an incentive, however small, for attracting new members. Yet, even when there was no explicit incentive to attract new members, as in Experiment 3, the groups that dominated the competition for attracting bystanders benefited from that competition. This is because the initial high cooperation in those groups was sustained as bystanders chose to join those highly cooperative groups and conformed to high contribution norms (Hypothesis 2).

Contribution rates in the groups that were unpopular were about the same as contributions in the no-choice condition.

Overall, then, in no case did markets for the provision of public goods harm unpopular groups and in all cases it benefited popular groups. Competition benefited intermediate groups only when group size bonus was moderate, in Experiment 2. When the group size bonus was the largest (Experiment 1), one group always dominated the competition. When the group size bonus was zero (Experiment 3), intermediate groups cooperated as much as the groups in the condition in which there was no competition for new members, i.e., the no-choice condition. Appendix A3.2 provides a more detailed comparison of intermediate groups.

MOTIVATION FOR STUDY 4

The results of the three experiments discussed thus far provide consistent support for our theoretical arguments. We aimed to further assess the generality of these core findings in a fourth study that differed in a number of key ways from the first three. First, participants in the first three experiments were undergraduates at the same university in the United States. While we know of no reason to expect that this participant population is unique in a way that would alter our key results, we sought to establish this empirically. To that end, our fourth study was conducted in the United Kingdom, using a sample that included students and non-students.

Second, our first three experiments followed the standard paradigm in experimental research on collective action problems, such that benefits from public goods benefited actual or potential contributors. But many real-world collective actions take place on behalf of *others*, and (prospective) benefactors do not derive any material benefits from taking part in the collective action. Examples include collective actions to benefit animal shelters or the provision of aid to victims of Tsunamis in other nations. In these cases, potential contributors are faced with a choice of keeping money for private use or donating to collective actions that will only benefit others. To assess whether our theoretical arguments apply even in these cases, our fourth experiment employed a novel paradigm in which participants faced a choice between keeping money in their private accounts versus contributing to a charity from which they would not derive any material benefits.

Finally, to shed additional light on participants' motivations, we introduced a series of post-experiment questions about why they joined a given group, and why they did or did not contribute to collective actions.

EXPERIMENT 4: DESIGN AND PROCEDURES

Except where noted, Experiment 4 followed the design and procedures of the first experiment. Participants were recruited from the database of an experimental center at a public university in the United Kingdom. They were recruited on the basis of earning money for themselves, i.e., no mention was made of charities during recruitment. A total of six 12-person groups took part in the study.[10] Of the 72 participants, 30 were non-students and 38 were female, with an average age of 35 (SD = 15).

Charity Selection: Following the instructions, each participant was told that they would have the opportunity to earn money on behalf of a charity. They were then given a list of six charities and asked to select one. All six were registered local charities, each with a different mission or

[10] The main goal in conducting Experiment 4 was to ensure that our results are qualitatively similar to those from Experiment 1. As a result of budget constraints -- due, among other factors, to the added costs of donations to charities -- Experiment 4 includes fewer sessions than our previous experiments. Further, although we originally planned seven sessions for Experiment 4, we had to discard one session due to a software failure. Thus, statistical power may be more of a concern for Experiment 4, compared to the Studies 1-3. This is mainly the case for our tests of the macro-level predictions (Hypotheses 4 and 5), given the aggregation required for these analyses. We nevertheless include formal tests of these macro-level hypotheses for completeness. For added confidence, we formally compare the results from Experiment 4 against those from Experiment 1.

focus: ill children and their families; poverty; an animal sanctuary; a local environmental preservation trust; a university hospital; and a homeless shelter. We provided a menu of charities in order to capture the interests of our heterogeneous sample and to better reflect how people make decisions about charitable contributions outside the laboratory context. Of the 72 participants, 25% selected the charity that provides benefits to ill children and their families. The remaining chose the homeless shelter (22%); the charity battling poverty (21%); the university hospital (15%); the animal sanctuary (13%) and the environmental preservation trust (4%). Participants were not informed of the charities selected by other participants.

Contributing to Charity: As in Experiment 1, in each round participants were given personal endowments of 20 MUs. A participant could contribute any portion of her endowment to the group fund. Any part of the endowment that that was not contributed to the group fund was converted to British Pounds (£s) and given to the participant at the end of the study. As in the previous experiments, the total contribution in the group fund was multiplied by 2.5 and redistributed equally. However, in Experiment 4, all returns from the group fund were placed in participants' charity accounts. Group size bonuses also went to the charities, rather than the participants' private accounts. That is, for each round, the charity selected by the participant received three MUs for each group member. The MUs in a participant's charity account were converted to £s and donated to the charity. Hence, contributing to the group fund was costly to the participant, and all benefits from the group fund and from group size went to the charity. As in all prior studies, bystanders received a "fair amount" for their private accounts for each round they were bystanders. They also received "fair" contributions to their charity accounts for each round they were bystanders.

Personal Earnings and Charitable Contributions: Participants received a 10£ show-up fee (1£ = ~ 1.6 US$). Depending on their decisions over the course of the study (i.e., how much of their private endowments they kept versus contributed to the group) they could earn additional money for themselves or the charity they selected. Participants earned an average of 14£ for themselves (the 10£ show-up fee plus an additional 4£ from their decisions) and raised, on average, 7£ for their charities. There was no deception in the experiment, i.e., donations were actually given to the charities. (Participants were informed that they could ask for a donation receipt to be emailed to them.) Thus, over 500£ [$800] was contributed to the selected charities as a result of the experiment.

Choice Manipulation: As in the first three experiments, we implemented the choice and no-choice treatments using a within-subjects design, with order of conditions randomized. Following Experiments 2 and 3, we controlled for group size across the choice and no-choice conditions by yoking the group dynamics for the no-choice condition of each experimental session from the choice condition of the prior experimental session.

Post-experiment Survey: After the choice and no-choice phases were finished, participants completed a computer-administered survey. They were asked to indicate why they chose to contribute (or not) in the choice and no-choice phases, and how they decided which group to join in the choice condition.

RESULTS OF EXPERIMENT 4

Figure 4 displays contribution rates (4a) and group sizes (4b) over time, averaged over all six sessions of Experiment 4. As in Experiment 1 and in most sessions of Experiments 2 and 3, Figure 4b shows a clear cumulative advantage for group size in the choice condition, supporting Hypothesis 5a. In five of the six sessions of the choice condition, one of the two seed groups grew continuously, attracting at least five of the six bystanders throughout the session, and in the remaining session, one group attracted four of the six bystanders. Again, we classify the group that ultimately outgrew the other group as *popular*.

As noted above, Experiment 4 balanced the choice and no-choice conditions with respect to group size. Further, there was no statistically significant difference in contributions of the popular and unpopular groups in the no-choice condition (Chi-square(1) = 2.34, $p = 0.13$). Thus, for simplicity, Figure 4a and subsequent analyses collapse contributions from all no-choice groups.

The patterns of Figure 4a are highly consistent with our previous results: contributions in the popular groups of the choice treatment are higher than those of unpopular groups of the choice condition and all groups in the no-choice condition. We fitted the same multilevel regression models as in previous experiments to assess statistical significance. These models are given in Table 8. Without any controls (Model 1), contributions are significantly higher in the choice versus no-choice condition. The treatment effect is somewhat smaller than the one observed in Experiment 1, though the difference between the two experiments is marginally insignificant ($\Delta b = 0.53$, $t = 1.74$, $p = 0.08$).

A separate model added interactions of the choice variable with age and a dummy for student (vs. non-student). Those interactions were statistically insignificant (Chi-square(2) = 4.14, $p = 0.13$), suggesting that the treatment effects we document here are similar across different populations, thus providing robust support for Hypothesis 3a.

Model 2 introduces a dummy variable corresponding to the unpopular groups in the choice condition, as well as controls for round, and group size. The effect of round is statistically significant but the effect of group size is not. Compared to the no-choice condition, while contributions in the unpopular groups appear lower, once individual and session random effects and other variables are controlled, the difference is not statistically significant (0.715 -.340 = .41, $z = 1.05$, $p = 0.29$). As in all previous experiments, the contributions of newcomers and seed members were not significantly different from those of other members, supporting Hypothesis 2.

To test Hypothesis 4a, we regressed the difference in average contribution rates between the two groups on the choice dummy, using a multilevel model with random effects for sessions. Results show that the difference in average contribution rates in the choice condition are higher than those in the no-choice condition but the difference is not statistically significant ($z = 0.81$, $p = 0.416$). However, as discussed earlier, this is likely due to the relatively low small number of cases and lower statistical power. As evidence, the size of this coefficient does not differ significantly from the size of the corresponding coefficient from Experiment 1 (difference= 0.762, $z = 1.15$, $p = .248$). Thus, despite not reaching significance, these data are consistent with Hypothesis 4a.

Model 1 in Table 9 presents conditional logit models predicting a bystander's choice via differences in average contribution rates and group sizes between the two groups in the previous round. As in prior experiments, bystanders choose larger groups (Hypothesis 1b) and groups with higher contribution rates (Hypothesis 1a). While the coefficient for group size is just shy of statistical significance in the model without controls, the difference between the coefficient for group size in Table 9 (Model 1) and the same coefficient obtained from Experiment 1 (Model 1 of Table 3) is statistically insignificant (Difference = −1.076, $z = -1.4$, $p = .151$). Moreover, once decision round is included as a control variable in Model 2, group size becomes highly significant ($b = .22$, S.E. = .05, $z = 4.31$, $p < 0.001$). Finally, we do not find any evidence that differences in contribution rates and group sizes are qualified by time; that is, neither group size nor contributions interact with decision round, (Chi-square(2) = 2.30, p = 0.36). In short, these data provide additional support for Hypotheses 1a and 1b.

We now briefly turn to the responses to the post-experiment survey, which provide further support for the logic underlying our arguments. First, when asked why they contributed to the group fund in the choice condition, participants' most frequently selected response (38%) was "I wanted to attract bystanders to my group."[11] Thus, participants were motivated to contribute in order to attract new group members, despite uncertainty about prospective members' underlying motives. When we asked the same question for the no-choice condition, most participants either selected "contributing a fair amount was the right thing to do," that they contributed "to raise funds for their charity" or both. In short, participants' self-reported reasons for contributing are highly consistent with our theoretical framework.

Participants' stated motivations for joining particular groups are also consistent with our arguments. 75% of participants indicated they wanted to join the largest group, the more cooperative group, or both. Only 13% indicated that they did not care which group they joined. A further 13% selected "other" and when asked to elaborate on this other motive, most indicated that they wished to join the group with the highest contribution.

Summing up, the Experiment 4 findings provide further support for our theoretical arguments. Despite using a different participant population and a very different experimental design, where public goods beneficiaries were third parties rather than the group members, the experiment yielded results very similar to our first experiments, both in terms of group dynamics and contribution levels.

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[11] 18% of participants selected "contributing a fair amount was the right thing to do"; 13% indicated "not applicable" because they said they never contributed to the group fund in the choice phase of the study. A further 8% of participants indicated "other motivation." The remaining selected answers included "I contributed to raise funds for my charity," "I wanted to match the contributions of others in my group" or both.

DISCUSSION

Existing scholarship on collective action problems seeks to understand when and why people contribute to organizations and group efforts, despite the temptation to free-ride on fellow members' contributions. A typical investigation thus assumes that membership in groups is endogenous, and analysis begins with fully formed groups with stable membership. One goal of the research reported in this paper was to push the analytical point of departure back one level, to individuals' decisions to move into those groups in the first place.

We have argued that an investigation into this process is important in its own right. Among other reasons, whether to contribute to a group is often a moot point until one is a member of that group. But we also have also shown that analyzing the process of prospective members moving into groups can shed new light on questions that are fundamental to a number of different literatures. For instance, research on social movements and related fields focuses on the movement of new members into groups. But these literatures do not address the tensions between individual and collective interests that often underlie group efforts, let alone how these tensions are magnified in large or growing groups. One of our aims was to highlight the dilemmas posed by mobilization or dynamic group membership, and to show why these dilemmas might compound the typical collective action problems that static groups confront. Doing so allowed us to address a number of questions about how successful groups grow without condemning themselves to an influx of free-riders.

We have shown that groups can overcome these "mobilization dilemmas" when people have choices about which groups to join, as happens when multiple collective action groups seek similar ends, or different religious groups "compete" for the same members. Our arguments and findings thus have implications for these areas. For instance, our work provides insight into the mechanisms underlying the finding from the literature on sociology of religion that religious groups tend to be more solidary and cooperative in competitive religious markets versus in areas with state-sponsored religion (Iannaccone 1991). Similarly, it provides insights into when and why multiple social movements working towards common goals will generate greater overall participation than a single group working towards a given goal. Specifically, we found that, compared to control groups, groups with competition between groups for members had higher contributions overall. This happened because, when groups competed for new members, newcomers joined successful groups (i.e., those with higher contributions) and contributed at normative rates, rather than free-riding on prior successes of those groups. Moreover, existing group members did not respond cynically to an influx of new members by lowering their contributions. As expected, we found that these micro-level processes gave rise to large inequalities in the size and success of groups and that these cumulative advantage effects were generally rooted in small differences that emerged early in the groups' histories.

In addition to providing a clear and straightforward test of our theoretical predictions, findings from our laboratory experiments offer a window into processes that would be difficult or impossible to observe in the real world. For instance, in Experiment 1, when the incentive for group growth was large, all experimental sessions resulted in maximal inequalities in group size. In the real world, scholars typically only have access to groups that succeed, as failed groups are likely to disband or get absorbed by popular groups. As a consequence, as has been noted by social movement scholars (McAdam and Boudet 2012), this has led research on social movements to focus almost exclusively on successful collective action groups, thus sampling on the dependent variable. Among other problems, this can lead to unrealistically optimistic assessments of the rate at which collective action groups succeed in meeting their goals.

By allowing us to observe competition between groups at the origin, the laboratory experiments reported in this paper complement the study of real world groups by shedding light on successes (the "1s") as well as failures (the "0s"). Moreover, since the groups were studied under controlled conditions, we know that the successes and failures were not due to group-specific institutions or idiosyncratic conditions. Instead, we identified structural conditions under which groups grow and succeed, despite the detrimental effects of increasing group size and a constant inflow of new members. An investigation that focused solely on successful groups would have missed this dynamic causal process. Our work thus adds to an emerging emphasis on using experiments to address cumulative advantage effects that can elude observation in less controlled environments (Salganik, Dodds and Watts 2006, van de Rijt et al. 2014).

Limitations and Directions for Future Research

The above contributions notwithstanding, our experiments employed a number of simplifying assumptions in order to maximize experimental control. For instance, in many real world groups, the incentives (or disincentives) for growing real world groups may not be as clear cut as they were for groups in our laboratory experiments. And compared to bystanders to the groups in our studies, prospective members of real world collective action groups may have more limited information about the relative success or failure of competing groups.

Although future research should assess the effects of relaxing these simplifying assumptions, we do not think they seriously limit the application of our arguments to understanding the dynamics of real world collective action groups. For instance, we suspect that it is enough that group members know that there benefits to growing groups, rather than knowing the precise value of those benefits. Indeed, the fact that we observed strong treatment effects for very different levels of the group size bonus (in Experiments 1 and 2) suggests this is the case.

Likewise, we suspect it is sufficient for prospective group members to have a general sense of which group is more successful rather than knowledge of specific members' contributions. This type of general information may come from media reports or other visible signs of group success (e.g., the size of a church or organization). When differences in groups' successes are more difficult for prospective members to discern, we expect that dynamics will more closely resemble those we observed in intermediate groups, where continued competition for group members kept contributions relatively high and equal across the two groups. These expectations could be tested in straightforward extensions of the studies reported above.

Along similar lines, bystanders had choices about which groups to join but we never gave them the option of not joining any group. In the real world, people often can (and do) "hunker down" and avoid joining social movements or other voluntary associations. How might the introduction of this option have altered the dynamics we observed? We expect such an option would reinforce our results. Prior work on "exit options" has shown that those who tend to go it alone rather than moving into interdependent situations tend to be more egoistic types (Macy and Skvoretz 1998, Orbell and Dawes 1991, Orbell and Dawes 1993, Yamagishi and Hayashi 1996). Thus, we would expect that allowing bystanders the option to go it alone would have resulted in a disproportionate tendency for more prosocial types to move into groups. Given that, as we have shown, people join more cooperative groups and larger groups when there is a group size benefit, the self-selected pro-sociality of newcomers would further reinforce cooperation in those groups. This would likely strengthen the cumulative advantage effects we predicted and found.

From the other side, not only do existing groups compete for new members but they also often have the ability to screen and filter newcomers. In addition to denying potential free-riders access to the group, they may rely on institutions such as sanctions and rewards to ensure that new members comply with existing norms (Horne 2009). Such institutions are particularly effective at sustaining contributions, especially when coupled with communication (Ostrom, Walker and Gardner 1992). Thus, while contributions pattern in our studies showed the standard decreases over time, we expect that standard tools available to real-world groups to sustain contributions, including sanctioning or ostracism would have mitigated this decline. But again, we hoped to show that our predicted effects would occur even in the absence of these institutions and hypothesize that such processes would only serve to reinforce the patterns established in our research.

Based on prior research (Bó 2005), we also expect that these declines over time would be less severe if group members were not aware of the endpoint (as is often the case in real-world groups) or if groups could grow indefinitely, such that group bonuses would become larger. These are all avenues for further research.

Finally, although the groups we studied were large by the standards of laboratory experiments, future work might assess the robustness of these dynamics in very large groups, either using real world collective actions or web-based experiments. While our results were remarkably robust across the different samples used here, studying larger groups with other samples would provide even more confidence in the arguments.

Robustness and Replication

Despite these limitations, we wish to emphasize that findings from the four experiments provided powerful and robust support for the patterns and processes specified in our theoretical argument. For instance, the manipulation of MPCR in Experiment 1 showed the exact same pattern of results when individual contributions had a very large impact on the level of public good provided as when it had a comparatively small impact. Similarly, we found remarkably similar results in our studies of American University students (Experiments 1, 2, and 3) as our study based in the United Kingdom that employed a sample of students and non-students from the surrounding community. Perhaps most importantly, we found similar patterns both when beneficiaries were potential contributors to collective action (as in the first three experiments) and when the public good benefited only third parties (Experiment 4). This last finding not only provides especially powerful support for arguments. It also suggests that the patterns identified in previous collective action research may apply to a broader range of situations than is typically assumed, namely to collective action efforts to benefit third parties. A worthwhile goal for future research on collective action would be to explicitly compare collective actions that benefit potential contributors to those that benefit third parties.

CONCLUSION

Summing up, in the half century that has passed since Olson's (1965) classic treatment, we have learned a lot about the conditions that favor successful collective action. But most of this understanding has been based on isolated and fully formed groups. We know less about interdependencies between groups and the processes that bring individuals into some groups but not others. As a consequence, we have argued, insights gleaned from theories of collective action have been relatively underutilized in neighboring research areas. Here we presented arguments on the dynamics of competition between collective action groups for members and tested them against the results of four laboratory experiments. Results strongly supported our micro-level predictions about how competition for members impacts the contributions of existing members, the choices of prospective members about which groups to join, and their levels of contributions once they do join. These processes give rise to an array of macro-level outcomes, including substantial inequalities in the size and productivity of collective action groups. We believe these insights significantly expand the range of phenomena that can be understood with theories of collective action.

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TABLE 1: SUMMARY OF SUPPORT FOR HYPOTHESES WITHIN AND ACROSS EXPERIMENTS

HYPOTHESIS	EXP. 1	EXP. 2	EXP. 3	ACROSS EXPS. 1-3	EXP. 4
1a	YES	YES	YES	YES	YES
1b	YES	YES	YES	YES	YES
2	YES	YES	YES	YES	YES
3a	YES	YES	*N/A*	*N/A*	YES
3b	*N/A*	*N/A*	*N/A*	YES	*N/A*
4a	YES	YES	*N/A*	*N/A*	PARTIAL
4b	*N/A*	*N/A*	*N/A*	YES	*N/A*
5a	YES	*N/A*	*N/A*	*N/A*	YES
5b	*N/A*	*N/A*	*N/A*	YES	*N/A*

TABLE 2: MULTILEVEL REGRESSION MODELS PREDICTING CONTRIBUTIONS, EXPERIMENT 1

	Model 1	Model 2	Model 3
Fixed			
Choice	1.127**	2.278**	2.302**
	(.221)	(.364)	(.364)
Unpopular		-2.404**	-2.503**
		(.646)	(.654)
Round		-.162*	-.172*
		(.072)	(.073)
Hi MPCR		3.859*	3.867*
		(1.795)	(1.794)
G Size		-.487**	-.481**
		(.137)	(.138)
Seed member			.146
			(.384)
Newcomer			-.521
			(.477)
Intercept	5.576**	6.923**	6.881**
	(1.140)	(1.362)	(1.413)
Random			
Session sd.	2.931**	2.187**	2.185**
	(.879)	(.739)	(.739)
Subject sd.	4.389**	4.342**	4.339**
	(.351)	(.347)	(.347)
Residual sd.	4.013**	3.890**	3.888**
	(.079)	(.076)	(.076)
-Log-likelihood	4055.218	4012.169	4011.156

Notes: Standard errors in parentheses. N(session) =8, N (subject=96), N (Decision)=1392.

**p(2-sided) < 0.01; * p(2-sided) < 0.05.

TABLE 3: CONDITIONAL LOGIT MODELS PREDICTING CHOICES OF BYSTANDERS, EXPERIMENT 1

	Model 1	Model 2
Δ Contribution	1.036**	2.619**
	(.364)	(.896)
Δ N	1.358[a]	3.761[+]
	(.726)	(1.983)
Δ Cont. × round		-.436*
		(.184)
Δ N × round		-.564
		(.384)
Round		.265
		(.238)
Log-likelihood	-7.017	-5.348

Note: Standard errors adjusted for eight session clusters. N = 48.

**p(2-sided)<0.01; *p(2-sided)<0.05; [+] p(2-sided)=0.06.

TABLE 4: MULTILEVEL REGRESSION MODELS PREDICTING CONTRIBUTIONS, EXPERIMENT 2

	Model 1	Model 2	Model 3
Fixed			
Choice	1.016** (.222)	1.130** (.313)	1.128** (.314)
Unpopular		-1.314* (.599)	-1.317** (.600)
Popular		.324 (.494)	.330 (.494)
Round		-.506** (.063)	-.512** (.064)
G Size		-.093 (.105)	-.097 (.107)
Seed member			-.113 (.359)
Newcomer			-.280 (.474)
Intercept	6.615** (.560)	9.490** (.698)	9.641** (.804)
Random			
Session sd.	1.327** (.537)	1.333** (.534)	1.333** (.534)
Subject sd.	4.121** (.290)	4.104** (.288)	4.104** (.288)
Residual sd.	4.727** (.079)	4.523** (.076)	4.522** (.076)
-Log-likelihood	5856.018	5776.460	5776.284

Note: Standard errors in parentheses. N(session)=11, N(subject)=132, N(Decision)= 1914.

**p(2-sided)<0.01; *p(2-sided)<0.05.

TABLE 5: CONDITIONAL LOGIT MODEL PREDICTING GROUP CHOICES OF BYSTANDERS, EXPERIMENT 2

	Model
Δ Contribution	.340** (.105)
Δ N	.395* (.184)
Log-likelihood	-30.397

Note: Standard errors adjusted for 11 session clusters. N = 66.

**p(2-sided)<0.01; *p(2-sided)<0.05.

TABLE 6: MULTILEVEL REGRESSION MODELS PREDICTING CONTRIBUTIONS, EXPERIMENT 3

	Model 1	Model 2	Model 3
Fixed			
Choice	.233	-.027	-.030
	(.210)	(.254)	(.253)
Unpopular		-.601	-.588
		(.609)	(.608)
Popular		1.486**	1.492**
		(.515)	(.514)
Round		-.328**	-.331**
		(.066)	(.067)
G Size		-.330**	-.346**
		(.123)	(.124)
Seed member			-.310
			(.337)
Newcomer			-.084
			(.453)
Intercept	6.300**	9.620**	9.920**
	(.685)	(.825)	(.866)
Random			
Session sd.	1.876**	1.862**	1.734**
	(.603)	(.600)	(.558)
Subject sd.	4.087**	4.087**	4.082**
	(.286)	(.285)	(.284)
Residual sd.	4.507**	4.348**	4.341**
	(.076)	(.073)	(.073)
-Log-likelihood	5770.560	5707.156	5703.651

Note: Standard errors in parentheses, N(session)=11, N(subject)=132, N(Decision) = 1914.

**p(2-sided)<0.01; *p(2-sided)<0.05.

TABLE 7: CONDITIONAL LOGIT MODEL PREDICTING GROUP CHOICES OF BYSTANDERS, EXPERIMENT 3

	Model
Δ Contribution	.471** (.156)
Δ N	-.460 (.307)
Log-likelihood	-31.926

Note: Standard errors adjusted for 11 session clusters. N = 66.

**p(2-sided)<0.01.

TABLE 8: MULTILEVEL REGRESSION MODELS PREDICTING CONTRIBUTIONS, EXPERIMENT 4

	Model 1	Model 2	Model 3
Fixed			
Choice	.597**	.715**	.697*
	(.210)	(.269)	(.271)
Unpopular		-.340	-.287
		(.495)	(.506)
Round		-.210**	-.217**
		(.061)	(.062)
G Size		-.028	-.035
		(.097)	(.098)
Seed member			-.238
			(.360)
Newcomer			-.295
			(.463)
Intercept	5.500**	6.646**	6.901**
	(.708)	(.808)	(.882)
Random			
Session sd.	.463	.445	.446
	(1.904)	(1.970)	(1.967)
Subject sd.	5.655**	5.648**	5.654**
	(.505)	(.505)	(.506)
Residual sd.	3.313**	3.267**	3.266**
	(.075)	(.074)	(.074)
-Log-likelihood	2866.732	2853.10	2852.80

Notes: Standard errors in parentheses. N(session) =6, N (subject=72), N (Decision)=1044.

**p(2-sided) < 0.01; * p(2-sided) < 0.05.

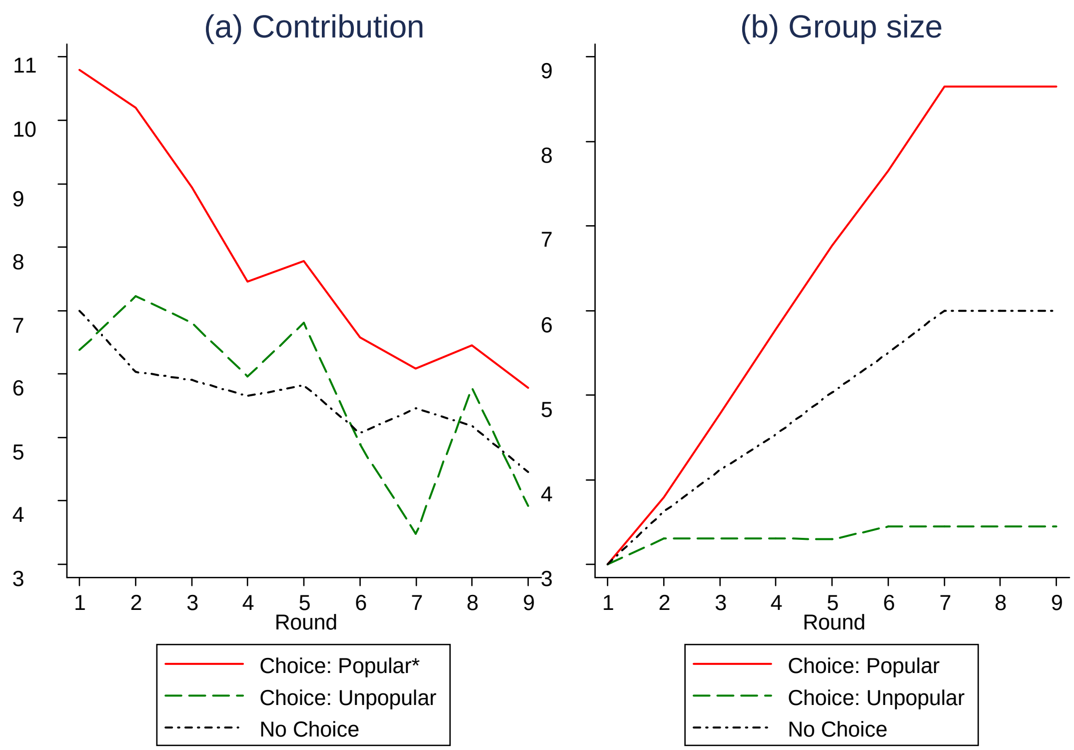
TABLE 9: CONDITIONAL LOGIT MODEL PREDICTING GROUP CHOICES OF BYSTANDERS, EXPERIMENT 4

	Model 1	Model 2
Δ Contribution	.349* (.158)	.471* (.202)
Δ N	.282 (.186)	.221** (.051)
Round		-.372* (.176)
Log-likelihood	-14.837	-13.031

Note: Standard errors adjusted for 6 session clusters. N = 36.

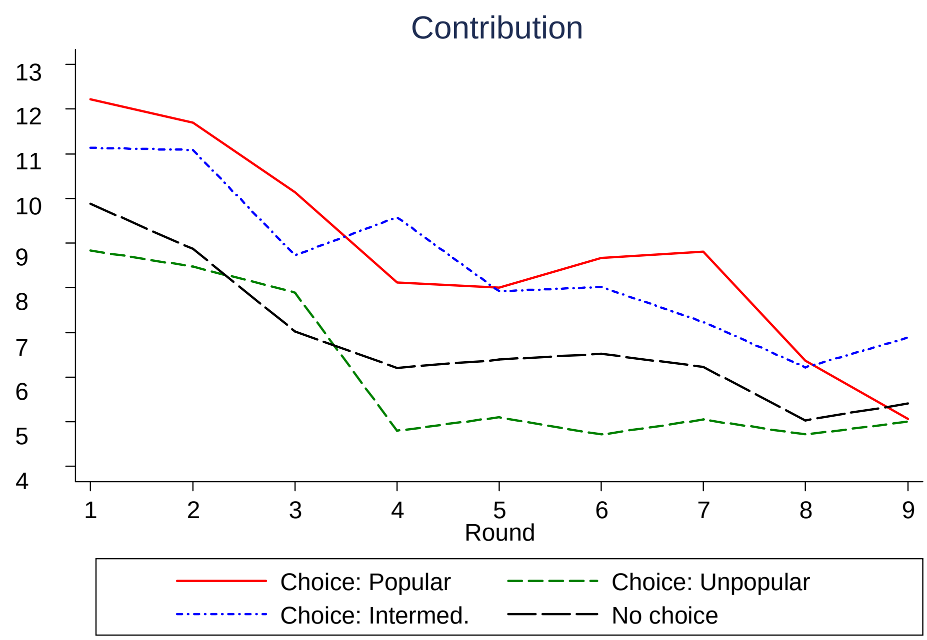
**p(2-sided)<0.01; *p(2-sided)<0.05.

FIGURE 1: CONTRIBUTIONS (a) AND GROUP SIZES (b) ACROSS EXPERIMENTAL CONDITIONS, EXPERIMENT 1



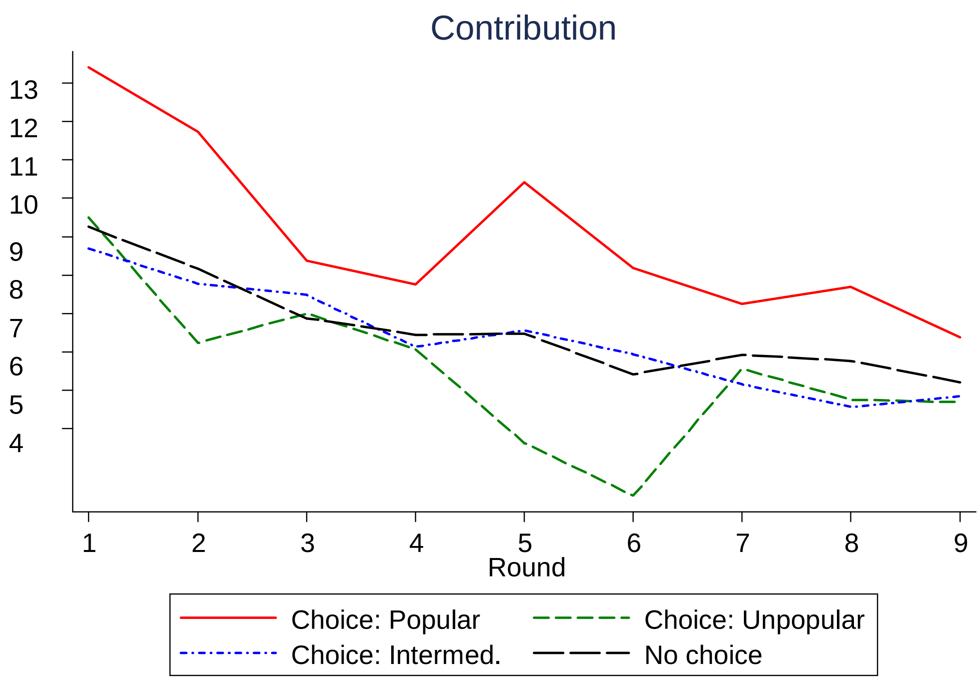
Note: A *popular* group is one that reaches a size of at least eight by the time all six bystanders join groups.

FIGURE 2: CONTRIBUTIONS ACROSS EXPERIMENTAL CONDITIONS, EXPERIMENT 2



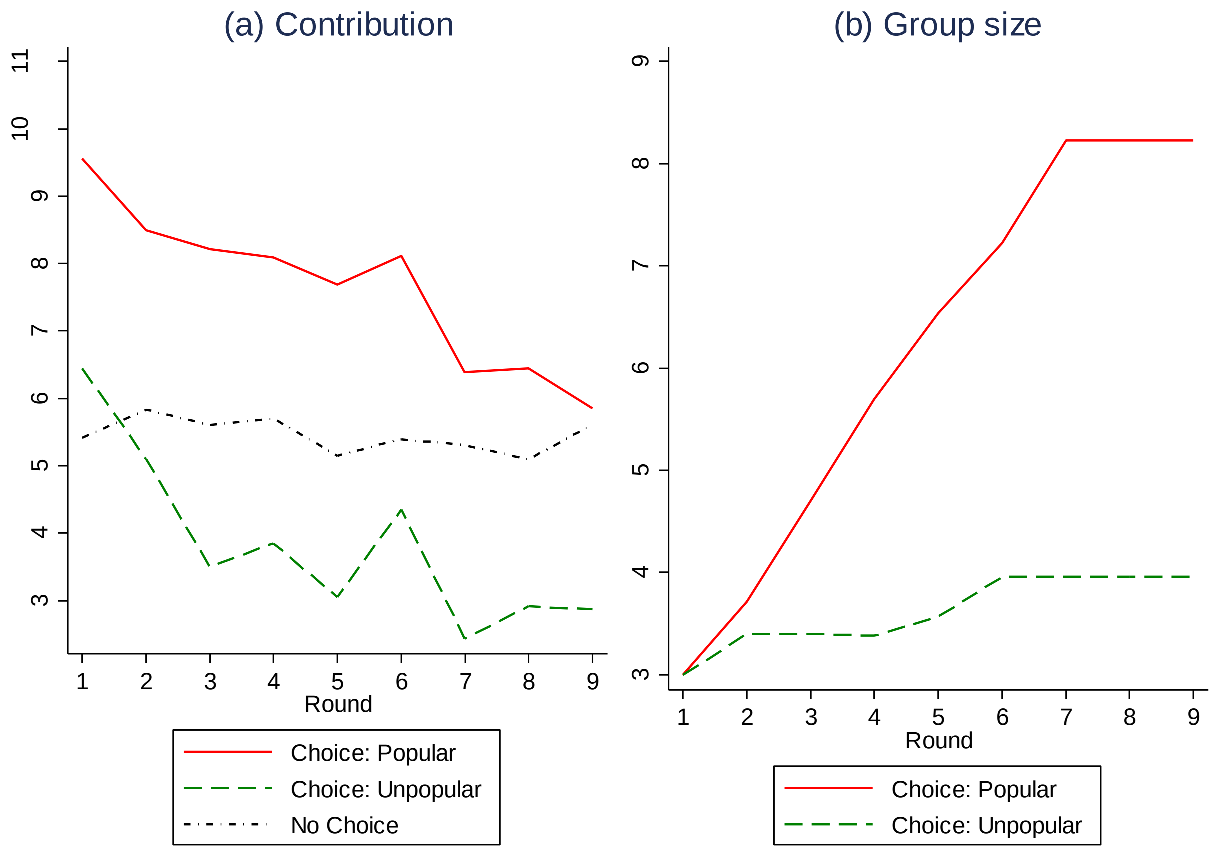
Note: A popular group is a group which reaches a size of at least eight once all six bystanders join groups. Intermediate groups are those which are neither popular nor unpopular.

FIGURE 3: CONTRIBUTIONS ACROSS EXPERIMENTAL CONDITIONS, EXPERIMENT 3



Note: A popular group is a group which reaches a size of at least eight once all six bystanders join groups. Intermediate groups are those which are neither popular nor unpopular.

FIGURE 4: CONTRIBUTIONS ACROSS EXPERIMENTAL CONDITIONS, EXPERIMENT 4



Supplementary Online Appendices

"Competition and Cumulative Advantage in Collective Action Groups"

Appendix A1. Rational egoists will join, but will not contribute to, productive groups

It is well-known that in the classical finitely repeated Public Goods Game the single Nash equilibrium under the selfish-rationality assumption is universal non-contribution (see, e.g., Osborne and Rubinstein 1994). Following our experimental design introduced below, we can extend this game and assume that there are $n \geq 2$ groups playing a finitely repeated Public Goods Game and there is a finite bystander population. Also assume, following our design, bystanders choose to join any of the n groups sequentially. Once the last bystander joins a group, the game is iterated in a fixed number of rounds, after which the game ends. Backward induction shows that, under the selfish-rationality assumption, the single Nash-equilibrium of this extended version is again universal defection. This is because in the last round, which is strategically equivalent to a one-shot game, everybody will defect and everybody knows that everybody will defect. Hence, the last bystander's group selection choice is redundant. In the next to last round everybody knows that everybody will defect in the last round and the last bystanders' group selection decision will be redundant, and consequently defects. Iterating this process all the way to the first round shows that the Nash-equilibrium is defection in all rounds. This result of universal all-round defection holds true even if there is a *fixed* individual benefit from being in a larger group. In this latter case the first bystander will choose a group randomly--because all groups will be defecting in the first round anyways--but the remaining bystanders will simply follow the first bystander, which results in one group growing maximally and all groups defecting all the time.

Appendix A2. Participant Demographics

Table A1 reports means (and standard deviations) of participant demographics measured in the four experiments. None of these variables had a significant impact on contribution behavior in any of our experiments. Similarly, including these variables as subject-level covariates does not change other coefficients reported in the tables reported in the main text in any substantial way (analyses available upon request).

Table A1. Participant demographics across experiments

Experiment	age	schoolyear	%female	%white	% student
1 (N=96)	19.23 (1.31)	1.94 (1.06)	61 (49)	72 (45)	100 (0)
2 (N=132)	19.86 (1.34)	2.57 (1.13)	64 (48)	64 (48)	100 (0)
3 (N=132)	20.39 (1.80)	2.62 (1.12)	58 (50)	74 (44)	100 (0)
4* (N=72)	34.72 (14.99)	N.A.	53 (50)	74 (44)	58 (49)

* Experiments 1, 2, and 3 were conducted in the US. Experiment 4 was conducted in the UK.

Appendix A3. Detailed Analysis of the Dynamics of Joining and Contributing to Groups

A3.1 Popular versus Unpopular Groups

Figure A1 below displays the entire contribution data for the choice condition in each of the 8 sessions of Experiment 1. Each column represents one session. Rows correspond to rounds: the first row is round 1, the second row is round 2, etc. Picking any column and starting in row (round) 1, and moving down shows the dynamics of joining and contributing to the groups for that session. As an example, consider session 1. In round 1 of session 1 the group displayed on the right cooperated slightly more that the group on the left, and thus attracted a new member. In round 2, however, the right group cooperated (on average) less than the group on the left. Hence, the left group attracted the second bystander. From round 2 onward, the left group attracted all bystanders and cooperated almost fully until the last round in which only a single free-rider defected. The right group maintained roughly the same (lower) level of cooperation they reached in round 2.

More generally, Figure A1 shows a clear pattern that occurs in all sessions: in the first two or three rounds, the bystanders *always* choose the group with higher average cooperation. In later rounds, the bystanders choose the larger group, which almost always maintained its initially high average cooperation rates (see main text). These dynamics yield the predicted divergence between the two seed groups: one remains small with lower cooperation rates and the other one grows continuously while maintaining the relatively high initial average cooperation rates.

But how, precisely, the popular groups achieve higher cooperation rates in initial rounds varies somewhat across sessions. In the majority of cases the contributions of most members of what ultimately becomes the popular group are roughly equal to one another: note the low variation in contributions of members of the popular groups in sessions 1, 3, 7, 8 and to some extent in session 5. Thus, in these sessions a group comes to be popular as a result of the joint effort of its members. In other sessions (particularly 2 and 6), an especially cooperative member's behavior seems more responsible for attracting bystanders.

Note that towards the end of some sessions (e.g., round 6 of sessions 2 and 3), it appears that a single member of the unpopular group attempts to attract the last remaining bystander by cooperating fully. Similarly, on a few rounds the unpopular groups manage to surpass the average cooperation of popular groups (e.g., round 5 of session 5 or round 4 of session 6). But this comes too late: the bystander still joins the popular group, due either to the group size, the longer history of cooperation, or both.

Figure A2 displays the contribution data in the choice condition in Experiment 4. These dynamics are substantively identical to those observed in Experiment 1. (The dynamics are also similar in the sessions from Experiment 2 and 3 in which one group attracted almost all bystanders.)

A3.2 Intermediate Groups

We now turn to intermediate groups which, as reported in the main text, occur only in Experiments 2 and 3. Intermediate groups are those in which neither group was able to attract most bystanders, such that group sizes were more or less balanced at the end of the session. Figure A3 presents the raw data for these intermediate groups. We do not discuss Figure A3 in detail: suffice to note that the average contributions in the two seed groups remained similar throughout these sessions. Instead, we wish to explain why, in Experiment 2, contributions to the intermediate groups remained as high as those in popular groups whereas, in Experiment 3, contribution rates in intermediate groups were *lower* than those in popular groups. Data we collected on the participants' social values in Experiments 2 and 3 help us disentangle alternative explanations of this finding.

Social values are stable preferences for how valued outcomes are distributed between oneself and others (McClintock 1972, Van Lange 2000). The literature identifies a number of social values, but focuses primarily on the most common three: *prosocials* seek to maximize the joint outcomes to self and others; *individualists* seek to maximize outcomes to self, and *competitors* seek to maximize relative gain over others. Research shows that an individual's social value is a powerful predictor of behavior in social dilemmas and collective action settings (e.g., Simpson 2004, Aksoy and Weesie 2014, Murphy and Ackerman 2014).

We suspected that social values might provide some insight into the dynamics of popular versus unpopular groups and of the intermediate groups. Thus, two weeks prior to the session, participants in Experiments 2 and 3 completed a standard measure of social values via an online survey (details are available from the first author upon request). Our original measure distinguished three types of social values: prosocials, individualists, and competitors. For simplicity, following prior work, we combined individualist and competitors into a single "proself" category which we contrast with prosocials.

About 35% of participants in Experiment 2 were classified as prosocials. Compared to Experiment 2, a significantly greater proportion of Experiment 3 participants (47%) were classified as prosocial 2 (z=1.97, p(2-sided)=0.051). In line with the previous work, a participant's social value predicted their contributions in both studies. Compared to proselfs, prosocials contributed about 1.4 points more to the public good, controlling for session and subject-level random effects (z=2.41, p(2-sided)=0.016).

More importantly for current purposes, of those groups in Experiments 2 and 3 that went on to become popular, a higher proportion of the initial, seed group was made up of prosocials (difference = 25%, z=1.94, p(2-sided)=0.058). That is, the groups that attracted almost all bystanders and went on to become popular were those that started with a greater proportion of cooperative types. This is consistent with the findings presented in the main text showing that groups that cooperate more in early rounds go on to become more popular.

Data on participants' social values help us to rule out an alternative explanation for the difference between the intermediate groups in Experiments 2 and 3. Intermediate groups cooperated as much as popular groups in Experiment 2, but less than popular groups in Experiment 3. In the main text, we argued that high levels of cooperation in intermediate groups in Experiment 2 occurred because members of intermediate groups continued to compete to attract bystanders. (Recall that there was a group size bonus in Experiment 2.) This competition for attracting bystanders kept cooperation rates higher in the intermediate groups in Experiment 2. In Experiment 3, however, there was no need to compete for attracting bystanders because there was no group size bonus. Consequently cooperation rates drop in the intermediate groups of Experiment 3.

An alternative explanation is that the difference between the intermediate groups in Experiments 2 and 3 is chance, resulting from fewer prosocials in Experiment 3 compared to Experiment 2. The social values data do not support this alternative explanation. There is no statistically significant difference between the proportion of prosocial participants in the intermediate groups in Experiments 2 and 3. In fact, the proportion of prosocials is non-significantly higher in Experiment 3 (Difference = 0.13, z=1.10, (p-2sided)=0.277). This finding provides more confidence in the explanation laid out in the main text, namely that continued competition for new members drove the high rates of cooperation in the intermediate groups of Experiment 2.

Figure A1. Experiment 1 data (choice condition only). Each column is a session and each row is a decision round (top row = round 1; bottom row = round 9). Within each box the y-axis is the contribution of each member (ranging from 0 to 20) and the x-axis represents the two groups in the session, with the popular group on the left and the unpopular group on the right. The + sign indicates the average contribution in a group.

Figure A2. Experiment 4 data (choice condition only). Each column is a session and each row is a decision round (top row = round 1; bottom row = round 9). Within each box the y-axis is the contribution of each member (ranging from 0 to 20) and the x-axis represents the two groups in the session, with the popular group on the left and the unpopular group on the right. The + sign indicates the average contribution in a group.

Figure A3. Experiment 2 and 3 data (choice condition and intermediate groups only). Each column is a session and each row is a decision round (top row = round 1; bottom row = round 9). Within each box the y-axis is the contribution of each member (minimum 0 maximum 20) and the x-axis represents the two groups. The + sign indicates average contribution in a group.

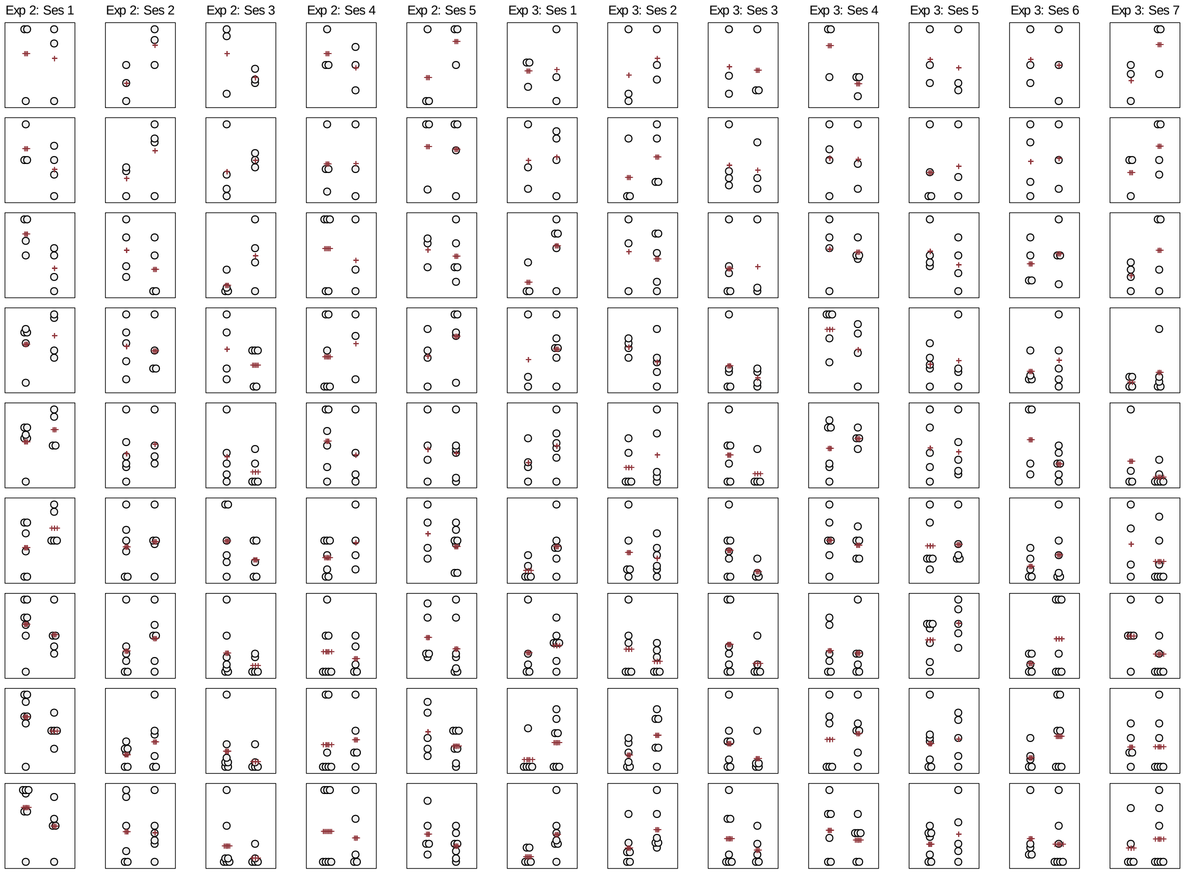


Table A2. Examples of payoffs as a function of group size bonus, group size, behavior of the focal participant and of others. For simplicity, we display only the cases in which the focal participant and others either fully cooperate (contribute the entire endowment) or fully defect (contribute nothing) to the public good. In the experiment participants were free to contribute anything from 0 to 20. Thus actual payoffs were in between the ones below. Note that the payoff difference between full cooperation and full non-cooperative increases as group size increases.

		Group Size Bonus = 3 (Experiment 1)		**Group Size Bonus=1 (Experiment 2)**		**Group Size Bonus=0 (Experiment 3)**	
Focal Participant	Group Size	Others Cooperate	Others Defect	Others Cooperate	Others Defect	Others Cooperate	Others Defect
Cooperate	3	59.00	25.67	53.00	19.67	50.00	16.67
Defect	3	62.33	29.00	56.33	23.00	53.33	20.00
Cooperate	4	62.00	24.50	54.00	16.50	50.00	12.50
Defect	4	69.50	32.00	61.50	24.00	57.50	20.00
Cooperate	5	65.00	25.00	55.00	15.00	50.00	10.00
Defect	5	75.00	35.00	65.00	25.00	60.00	20.00
Cooperate	6	68.00	26.33	56.00	14.33	50.00	8.33
Defect	6	79.67	38.00	67.67	26.00	61.67	20.00
Cooperate	7	71.00	28.14	57.00	14.14	50.00	7.14
Defect	7	83.86	41.00	69.86	27.00	62.86	20.00
Cooperate	8	74.00	30.25	58.00	14.25	50.00	6.25
Defect	8	87.75	44.00	71.75	28.00	63.75	20.00
Cooperate	9	77.00	32.56	59.00	14.56	50.00	5.56
Defect	9	91.44	47.00	73.44	29.00	64.44	20.00

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