Cheating on a Budget

Abstract

An underexplored class of social dilemmas revolve around a shared resource pool and privately observed entitlements, making the resource susceptible to dishonest overclaiming. To study these dilemmas, we introduce a novel four-player game that permits individuals to claim more than their entitlement. Two fundamental alternatives that often arise involve participants submitting claims simultaneously (e.g., municipal department budgets, public-housing, and university-dorm allocations), or sequentially (e.g., food pantries, community fridges, player negotiations on salary-capped sports teams). Motivated by such cases, our experimental design contrasts simultaneous and sequential reporting as well as plentiful and limited budgets. We find that simultaneous reporting reduces dishonesty when resources are plentiful and promotes greater equity when resources are limited. Under sequential moves with limited resources, Player 4 frequently protests the inequity by choosing to earn zero rather than the remaining budget. Our results demonstrate that these social dilemmas can be better managed by employing simultaneous reporting structures, which obscure information about individual claims.

Keywords: experimental economics, cheating externalities, budget constraint, virtue signaling.

JEL Codes: C90, D91, H80.

1 Introduction

We study experimentally a class of social dilemmas that involve individuals making claims on a shared resource in limited supply. Because individuals' entitlements to this resource are privately observed, they have the opportunity to submit claims in excess of their entitlements. Excessive claims may deprive others of their entitlement. These types of real-world dilemmas are ubiquitous and differ according to whether participants submit their claims simultaneously or sequentially. We introduce a laboratory game where each group member observes, in private, their entitlement and then reports a claim, which may or may not equal their entitlement. We examine how the size of the group budget (plentiful or limited) and the timing of reports (simultaneous or sequential) impacts total claims and their distribution.

Numerous real-world social dilemmas exist in which groups or individuals may submit claims in excess of their unverifiable needs. Many of these dilemmas involve participants submitting their claims simultaneously. For example, municipal departments (e.g., Public Works, Police, Transportation, Parks and Recreation, Water and Wastewater) submit budget requests to the city without knowing other departments' requests. When preparing their budgets, departments generally have a good idea of the city's total available budget because they know the city's revenue projections and fiscal capacity and municipalities are legally required to balance their budgets. Applications for public housing or welfare benefits constitute another example. Participation in these government programs is typically gated by certain eligibility criteria, which may not be stringently verified. Another example is the allocation of university dorms, whereby those with religious or health-related needs can gain preferential selection over different room types and bathroom arrangements (e.g., private, single-use). By lying or exaggerating a relevant need, one can secure their preferred living arrangement, while potentially dispossessing someone in greater need.

Other social dilemmas of this nature are played out sequentially. Food pantries, free supermarkets and especially community fridges allow people to take away more food than they need, thereby leaving less for subsequent shoppers. Player negotiations on a salary-capped sports team are carried out sequentially. Star players often negotiate their large contracts first, leaving little cap space for role players. Conveyor-belt sushi restaurants are another example where patrons seated near the start of the belt can capture the best plates before others have a chance. The 2019 Netflix movie *The Platform* offers a particularly visual example of this class of dilemmas played out sequentially. In the film, a platform full of food descends incrementally to feed the inhabitants of a 99-level jail. Inmates are meant to eat only what they need; however, inmates on higher levels overeat, leaving those on the lowest levels to starve.

Despite a plethora of scenarios in which cheating imposes negative externalities on others, the large literature on cheating and dishonesty focuses on individual-choice settings. Social dilemmas with opportunities to cheat that harm others are underexplored. This paper investigates how to reduce false reports and their associated negative externality with the aim of promoting a more equitable distribution of the resource.

We introduce a novel four-player game that mirrors the incentives participants face in such social dilemmas. Each group member privately observes their entitlement and then reports their claim, which may differ from their entitlement. In a 2×2, between-subjects design, group members report either simultaneously (Sim) or sequentially (Seq) and the budget available to the group of four is either limited (Lim) or plentiful (Ple), leading to our four main treatments, which we label *SimLim*, *SimPle*, *SeqLim* and *SeqPle*. In instances where the group's total report exceeds the limited budget, one or more members do not receive their reports, resulting in no payment for those individuals. In the *SimLim* treatment, this occurs through a random selection process; in *SeqPle*, the offending player forfeits their payoff.

This reporting game is played once to evaluate treatment differences without the opportunity to learn. We observe that average group reports are within half a point of one other in all but one treatment: reports in the sequential-reporting treatment with plentiful budget ($SeqPle$) are 1.5 to two points higher than all other treatments, although this difference is not statistically significant.

Groups of four are then randomly reshuffled and fixed for six rounds of repeated play with feedback. A modest upward drift in *SeqPle* reports across rounds now results in significantly higher reports than those in the *Lim* treatments where group reports remain flat around the available budget, well below equilibrium levels. One of our central observations is that the simultaneous format results in more equitable payments, as risk preferences help to restrain claims. By contrast, under sequential reporting with a limited budget (*SeqLim*), early movers often submit large claims, leaving Player 4 with disproportionately little. In these situations, Player 4 frequently employs a means of protest; namely, they submit a claim exceeding the remaining budget, thereby willingly forfeiting a positive profit. Our data allows us to rule out player confusion as a possible explanation, instead suggesting that these costly actions reflect virtue signaling of the player's honesty or indignance. These results suggest that the design of such social dilemmas would benefit from simultaneous reporting structures to promote equity amongst participants and fewer instances of overreporting.

Our findings contribute to the literature on dishonesty in strategic games by providing the first analysis of such social dilemmas. In particular, we are the first to shine light on the advantage of simultaneous reporting structures when dishonesty plays a role in the reporting decision. One strand of the common-pool-resource (CPR) literature finds that players request more the earlier their position in the queue (Rapoport et al., 1993; Larrick and Blount, 1997; Budescu et al., 1997, 1995; Budescu and Au, 2002). Without an underlying entitlement, there is no notion of cheating in these papers. In a separate strand of literature on dishonesty in individual-choice problems, players are either always honest, cheat fully to maximize their income, or cheat incompletely (i.e., not maximally) (see, e.g., Fischbacher and Föllmi-Heusi (2013); Abeler et al. (2019); Bernabe et al. (2023)). Two-person sender-receiver games show that externalities on other players temper dishonest behaviors (Gneezy, 2005; Hurkens and Kartik, 2009). This extends to bribery scenarios (see, e.g., Barr and Serra (2009)). However, if the externalities can be reciprocated by all parties involved, dishonest decisions are not tempered (Abbink et al., 2002; Schitter et al., 2019).

Two studies are more closely related to our experimental design. Schitter et al. (2019) study a claiming game in which participants self-report (and are paid) the amount missing from a given individual envelope with a total of 100 cents. In their externality treatment, claims are paid out of a common pool jointly owned by the four group members. When total claims are less than 400 cents, the remainder is distributed equally among group members. This resembles our *SimPle* treatment, except we do not pay out the remainder.[1] They find that anonymity increases dishonesty, while the presence of externalities has only a modest effect on claims. Jiang and Villeval (2024) study a collective dilemma using the Fischbacher & Föllmi-Heusi die-roll task. Group members individually submit their die reports. If the sum of the reports reaches or surpasses a known threshold, all members face a 60% probability of a collective sanction, namely, they all receive no payoff, regardless of their own reports. This resembles our *SimLim* treatment, except that not all group members lose their payoffs when group reports exceed the budget in our setup. They find that small groups lie less than large groups, and individual vs collectivist mindset priming does not significantly impact claims.

Relative to the literature, our experimental design focuses on a different set of social dilemmas, motivated by numerous real-world examples. Primarily, this means that our rule for what happens when the resource budget is exceeded differs from the entirety of the literature. In particular, we depart from the norm in the CPR literature where a claim that exceeds the resource available either significantly diminishes or collapses everybody's payoff. In our motivating examples, this is never the case. Furthermore, we are the first to directly compare sequential and simultaneous reporting structures, as well as explicitly contrasting plentiful and limited budgets. We demonstrate meaningful differences between the two reporting structures and highlight the impact of budget availability in individuals' reporting decisions.

[1]For a motivating example of our budget-remainder rule, the Canadian Emergency Response Benefit gave out stipends to Canadian workers impacted by COVID-19. All eligible Canadians were guaranteed to receive a stipend where eligibility was contingent on self-reported and unverified job loss or unemployment. Notably, Canadians received only their stipend amount – they did not receive any additional unallocated or excess funds.

Section 2 details the experimental design followed by a power analysis and a description of our sample in Section 3. Section 4 presents the results and data analysis. We conclude with some implications of our findings in Section 5.

2 Experimental Design

2.1 Task Description

Participants are told that the experiment consists of two parts. They receive the instructions for Part 1 without being made aware of the nature of Part 2. Part 1 is played once in groups of four participants. Our goal is to evaluate whether treatment differences arise in this one-shot setting without the opportunity to learn. Then, participants receive the instructions for Part 2 in which groups of four are randomly reformed once and fixed for the six rounds of repeated play. Our goal here is to evaluate whether learning within a fixed group of four leads to convergence to equilibrium claims or some other benchmark.

The experimental task in each round centers around the Fischbacher and Föllmi-Heusi (2013) die-rolling experiment. Specifically, in each round, participants roll the die once and are asked to report the result of their die roll. They are told that the die roll outcome represents their rightful entitlement, or claim, to the budget. They are explicitly asked to roll the die once to fully anchor their rightful entitlement to the single die roll. Die rolls are privately observed: participants are informed that they would be the only ones who know the result of their die roll and thus their true claim to the budget. Reports are unverified; that is, participants receive in dollars however many pips they report, budget permitting. Experimenters were not in the room whilst participants engaged with the task and each computer station was separated by dividers, meaning that participants rolled their dice in isolation and could report whatever outcome they chose with complete ease.

2.2 Experimental Treatments

The experiment follows a 2×2 design with the two factors being Simultaneous vs Sequential reporting and Plentiful vs Limited budgets. This affords four distinct treatments: (1) Simultaneous, Plentiful (abbreviated henceforth as *SimPle*); (2) Simultaneous, Limited (*SimLim*); (3) Sequential, Plentiful (*SeqPle*); and (4) Sequential, Limited (*SeqLim*).

In the Simultaneous (*Sim*) reporting structure, the four group members roll the die and make their reports at the same time, meaning they have no information about other group members' reports. They do learn each group member's report and payment after each round in Part 2.

In the Sequential (*Seq*) reporting structure, subjects roll their die and make their reports one at a time in a given order. The specific order and each member's position in the sequence is determined randomly by the computer. At the time they make their reports, each member knows the initial budget size, the reports made by preceding members, and how much money is left in the budget.[2] They also know the size of the group, and therefore how many subsequent players follow them who have yet to report.

In the Plentiful budget treatments (*Ple*), the total group budget is \$24. This implies that each participant is guaranteed to receive their report, no matter how much they or others in their group report.

In the Limited budget treatments (*Lim*), the total group budget is \$17.[3] Thus, participants are not guaranteed to receive their reports. In both *SimLim* and *SeqLim*, if the total reports exceed the budget, one or more group members receive \$0 until the remaining

[2]Knowledge of the total budget size and previous claims made parallels similar real-world social dilemmas. For example, the USDA's Disaster Relief Program compensates those adversely affected by natural disasters (e.g., drought, flood, wildfires, hurricanes), authorizing \$220 million to compensate producers for crop, timber, and livestock losses resulting from adverse weather events in 2023 or 2024. More details can be found here. A Deposit Insurance Reserve Fund made available \$578 million in 2025 to protect people's deposits at credit unions in Ontario. Section 20 of this document offers further information.

[3]We sought a budget small enough that one or more participants may feel constrained in the amount they can report, but not so small that the last participant in the Sequential queue regularly faces a balance of zero and therefore has no decision to make. We also did not want the budget to be a multiple of four to avoid a symmetric focal claim in *SimLim* whereby each player claims one-fourth of the total budget.

reports fall within the available budget. In *SimLim*, this is achieved by the computer randomly selecting a member to receive $0 instead of their report. If the sum of the remaining reports falls within the $17 group budget, the remaining members all receive their respective reported amounts; otherwise, the random selection process repeats. In *SeqLim*, if a subject reports more than the remaining budget, they receive $0, with the budget remaining unchanged – as it was before the excessive report.[4]

In Part 2, we introduce an additional treatment variable (Fixed/Random) applied only to the *SeqLim* treatment – player order in *SeqPle* remains fixed across all rounds. *SeqLimFix* keeps participants' reporting order in the group of four fixed across the six rounds of play. By contrast, *SeqLimRan* randomizes the reporting order in each round.[5] We anticipate that reports in *SeqLimRan* will be higher than in *SeqLimFix*. The fixed positions in *SeqLimFix* are likely to trigger fairness concerns for the third and fourth movers among first and second movers, whereas a player chosen to go first or second in *SeqLimRan* is likely to view their assigned position as their "chance" to claim 5 or 6 and earn a large payment.

2.3 Equilibrium Analysis

Assuming participants' objective is to maximize their monetary payoff, the unique pure-strategy Nash equilibrium of the stage game in both *SimPle* and *SeqPle* involves each group member reporting 6. The vector of reports $\{6, 6, 6, 6\}$ is the Nash equilibrium outcome. In *SimLim*, the unique pure-strategy Nash equilibrium is asymmetric and involves three players reporting 6, while the remaining player chooses 5; that is, $\{6, 6, 6, 5\}$ is the unique PSNE

[4] In particular, players do not receive an incomplete payoff where, in the event of making an excessive claim, they get paid whatever is left in the budget. Our motivating examples suggest that the resource is often lumpy. As a result, it may be inefficient and socially suboptimal to offer a partial payoff when the requested claim exceeds the available budget. Instead, the governing entity (e.g., social planner) is better off waitlisting or skipping such an excessive claim in favor of offering the remaining resource to somebody who makes a compatible claim and can better use the remaining resource. Similarly, for *SimLim*, our motivating examples suggest that the classic rule from the CPR literature where everybody is heavily penalized, often losing all payoffs altogether, is unsuitable. Instead, a lottery process randomly selects a subset of participants to receive their claims.

[5] Appendix A contains the participant instructions for all the treatments. See the Online Appendix for screenshots of the experiment.

up to a permutation of players.[6] In *SeqLim*, the subgame-perfect equilibrium involves the first two players reporting 6, the third player reporting 5, while the fourth player's payoff-irrelevant report can be anything: $\{6, 6, 5, \cdot\}$.

To understand why participants may diverge from the equilibrium reports, we provide a simple framework to think about possible channels in the context of our treatments. Of the four main treatments, *SimPle* admits the fewest influences on the reporting decision: with plentiful budget for everyone to receive their claim and the inability to see others' reports when making a decision, the only restraint on reports is the individual's psychological cost of lying. The other treatments each introduce additional considerations. In *SeqPle* and *SeqLim*, claims are visible to decision-makers later in the queue. A large claim may thus invite imitation among later participants (i.e., moral licensing). At the same time, the initiator of the large claim may experience awkwardness that their large claim will be seen by others – a sentiment likely tempered by anonymous reporting (Schitter et al., 2019). In *SeqLim*, inequality aversion counteracts moral licensing. Specifically, Players 1, 2 or 3 may dampen their claims out of concern that the budget may be inadequate for subsequent players to receive their entitlement. Finally, risk aversion may restrain claims in *SimLim*: a higher claim increases the risk of receiving no payment at all. Risk preferences are not expected to play a role in the other treatments where the budget is plentiful (*SeqPle* and *SimPle*) or each player knows the available budget when reporting (*SeqLim*).

[6] In this *SimLim* PSNE, the three players who report 6 each earn 4 in expectation, while the player who reports 5 has expected earnings of 3.75. To see that this is a NE, suppose the player reporting 5 deviates to a 6. Two players must now be eliminated to remain within budget. Thus, each player's expected earnings drop substantially to 3 ($\frac{1}{2} \cdot 6$). If, instead, one of the three players reporting 6 deviates to 5, their expected earnings decrease to 3.75 ($\frac{3}{4} \cdot 5$).

3 Data

3.1 Power Analysis

In Part 1, each subject makes an independent reporting decision. The repeated nature with feedback of Part 2 means that the average report in a group of four subjects across all six rounds constitutes an independent observation. To determine the minimum sample size needed to detect a significant difference at the 5% level with 80% power, we pre-registered a power analysis. For the *SeqLim* treatments, we expect the group to coordinate on a sum of reports equal to 17 (i.e., average report of 4.25). An economically meaningful effect would be an average report 10% lower in *SimLim*, that is 3.825. Assuming a group standard deviation of 0.5, we need a minimum of 18 groups per treatment. Thus, in our recruitment, we aimed for at least 18, ideally 20, groups per treatment.

3.2 Sample and Subject Payments

In actuality, 412 subjects participated in a total of 20 experimental sessions, four sessions in each treatment. Twenty groups of four (80 subjects) participated in each of the Simultaneous treatments (*SimPle*, *SimLim*), while 21 groups of four (84 subjects) participated in each of the Sequential treatments (*SeqLimFix*, *SeqLimRan*). The four sessions in each treatment were equally divided between two locations: two sessions per treatment were conducted at the University X Experimental Laboratory and the other two sessions in the treatment were conducted at the University Y Experimental Laboratory.

Participants received a $7 show-up payment plus $0.25 for each quiz question answered correctly on the first attempt as well as their earnings from Part 1 and one randomly selected round among the six rounds of play from Part 2.[7] The entire experiment, including the instruction phase, play of Parts 1 and 2, post-experiment questionnaire and receipt of

[7]For the purpose of experimental control and to hold incentives constant, we paid these amounts in the local currency (i.e., USD or CAD) regardless of location.

payment took up to 45 minutes. The average payment was $16.25 (s.d. 2.83).

4 Results

4.1 One-Shot Game (Part 1)

We begin by reporting the results of the single round played in Part 1. Table 1 displays the mean sum of the four individual reports (to be referred to as the group report) across all groups and the mean budget remaining at the end of the round by treatment. Note that *SeqLimFix* and *SeqLimRan* differ only in how they assign players to roles across rounds. These two treatments are thus identical in this one-shot game. It is therefore reassuring that their mean group reports (16.62 and 17.14, respectively) are similar and their distributions of reports are also similar and not significantly different from one another (Mann-Whitney $z = .78$, $p = .44$). For the remaining analyses of Part 1 decisions, we combine these two treatments and refer to them simply as *SeqLim*.

Table 1: Part 1 Outcomes by Treatment

Treatment	Group Report (s.d.)	Unpaid Budget (s.d.)	Obs.
SeqPle	18.48 (3.64)	5.52 (3.64)	21
SeqLim	16.88 (2.65)	1.26 (2.01)	42
SimPle	17.05 (3.46)	6.95 (3.46)	20
SimLim	16.65 (3.84)	2.50 (2.50)	20

Notes: For each treatment, the entries correspond to the mean (s.d.) group report, the mean (s.d.) end-of-round remaining budget (s.d.) after all four group members have submitted their reports, and the number of groups.

From Table 1, we see that the mean group reports from three of the four treatments fall within one-half of one point of one another: 16.65 (*SimLim*) to 17.05 (*SimPle*), while the mean group report of 18.48 in *SeqPle* exceeds that of the other three treatments by 1.5 to two points. The fact that these treatment differences fall short of the $7 budget difference between *Ple* and *Lim* implies that the unpaid budget varies substantially. Indeed, the remaining budget of 6.22 (s.d. 3.58) in the two combined *Ple* treatments is significantly larger than 1.66 (s.d. 2.24) in the three combined *Lim* treatments (Mann-Whitney $z = 6.43, p < .001$). Furthermore, the non-parametric, rank-sum Kruskal-Wallis test for group reports cannot reject their equality across the four treatments($\chi^2(3) = 4.15$, $p = .25$).[8]

The group averages ignore the potential for considerable variation within the Sequential treatments due to players' asymmetric roles. To illustrate, Players 1 and 2 in the *SeqLim* treatments can report 6 and receive a guaranteed $6, whereas the available budget may restrain Player 4's report and earnings to less than $6. Table 2 displays the mean individual report and the mean end-of-round budget by treatment and player order within the treatment. For the most part, mean reports are monotonically decreasing with sequence order in both Sequential treatments – even though none of the players faces a budget constraint in *SeqPle*. In *SeqLim*, the mean report drops by nearly one and a half points from 4.67 for Player 1 to 3.29 for Player 4 ($\chi^2(3) = 19.28$, $p < .001$).

[8]When we test for differences in the reporting distributions in Part 2, we will have five treatments and, due to the dependency in group members' reports across rounds, we will treat the mean report across the four group members and six rounds of play as the unit of observation. For comparison, when this same test is performed on Part 1 reports, we still cannot reject the equality of reports at conventional significance levels ($\chi^2(4) = 4.17$, $p = .38$).

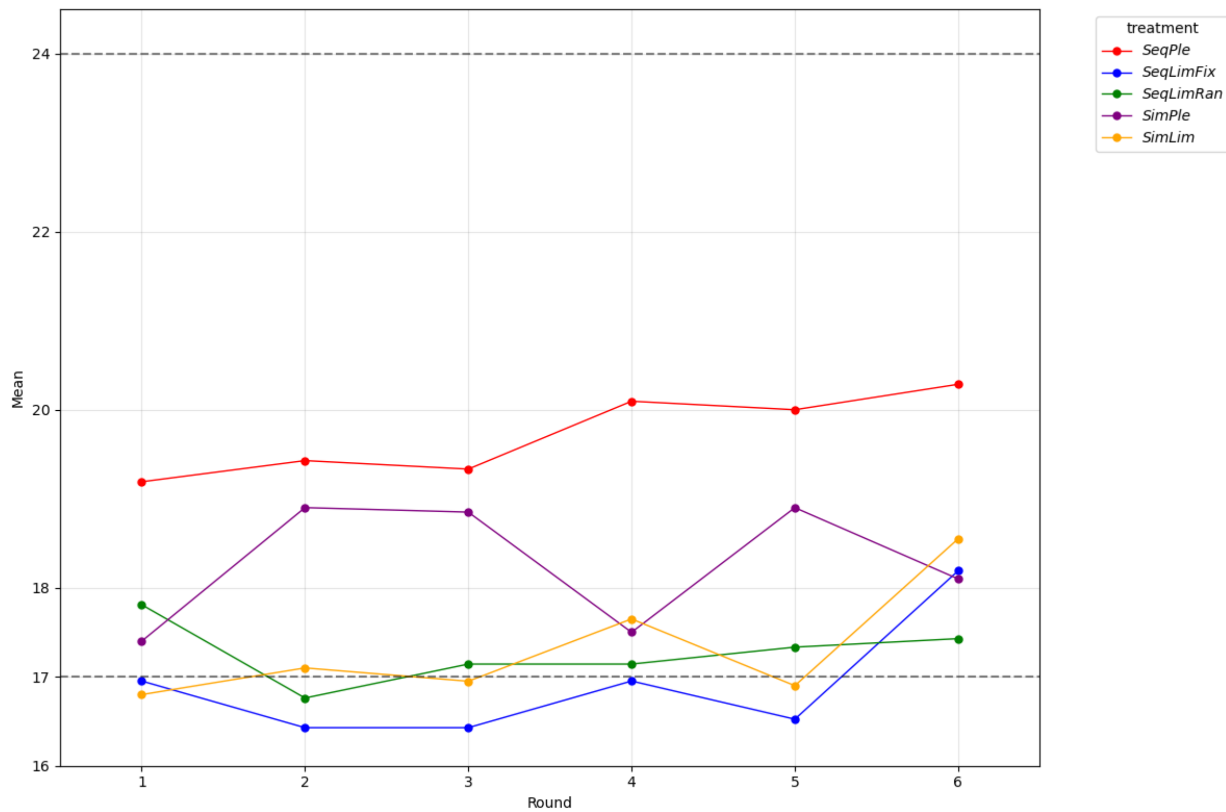
Table 2: Part 1 individual reports in Sequential treatments by report order

Treatment	Player	Mean (s.d.)	Unpaid Budget	Obs.
SeqPle	1	5.05 (1.36)	5.52 (3.64)	21
SeqPle	2	4.62 (1.53)		21
SeqPle	3	4.48 (1.75)		21
SeqPle	4	4.33 (1.71)		21
SeqLim	1	4.71 (1.49)	1.26 (2.01)	42
SeqLim	2	4.21 (1.75)		42
SeqLim	3	4.67 (1.63)		42
SeqLim	4	3.29 (1.67)		42

Notes: Part 1 mean individual reports (s.d.) and mean end-of-round remaining group budget (s.d.) by Sequential treatment and by player position in the order.

4.2 Repeated Play (Part 2) – Treatment comparisons

Figure 1: Mean Group Report by Round and Treatment



Notes: Mean group report by treatment across six rounds of Part 2. Dashed lines indicate available budgets for *Lim* (17) and *Ple* (24) treatments.

The remainder of our analysis focuses on reports in Part 2 where, after Part 1, groups of four players are randomly reformed and remain together for the entire six rounds of repeated play. Players receive feedback about other group members' reports and payment after each player's reporting decision (*SeqPle*, *SeqLimFix* and *SeqLimRan*) and at the end of the round (all five treatments).

Figure 1 and Table 3 display the mean group report by treatment and by round for all six rounds. What is immediately apparent is that group reports are substantially above 14 (the expected value if everyone reports honestly) and below the equilibrium prediction of 23 for *SimLim* and of 24 for *SimPle* and *SeqPle*.[9] The modest increase in group reports from Part 1 to round 1 of Part 2 in *SeqPle* is sufficient to render the treatment differences weakly significant ($\chi^2(4) = 7.86$, $p = .09$). In round 2, the treatment differences in group reports become highly significant ($p < .01$) where they remain for the duration (all $p \leq .03$). The mean reports from Parts 1 and 2 point to the explanation behind the increasingly significant difference: mean group reports in *SeqPle* drift higher from round to round, while they remain flat around the available budget of 17 in *SeqLimFix*, *SeqLimRan* and *SimLim*. Section 4.4.1 highlights the dynamic by which group reports in *SimLim* remain around 17 (rather than nearer to the Nash equilibrium of 23).

Table 3: Part 2 Outcomes by Treatment and Round

	Mean (s.d.) Group Report						
Treatment	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	Obs.
SeqPle	19.19 (3.66)	19.43 (3.89)	19.33 (3.29)	20.10 (2.96)	20.00 (3.21)	20.29 (3.08)	21
SeqLimFix	16.95 (2.11)	16.43 (1.63)	16.43 (2.73)	16.95 (2.22)	16.52 (2.06)	18.19 (2.86)	21
SeqLimRan	17.81 (2.14)	16.76 (2.66)	17.14 (2.46)	17.14 (1.62)	17.33 (1.80)	17.43 (1.83)	21
SimPle	17.40 (3.63)	18.90 (2.47)	18.85 (3.98)	17.50 (3.69)	18.90 (3.95)	18.10 (3.51)	20
SimLim	16.80 (3.02)	17.10 (3.34)	16.95 (3.15)	17.65 (2.89)	16.90 (2.94)	18.55 (2.91)	20
Kruskal-Wallis p-value	.09	< .01	.02	< .01	< .01	.03	

Notes: For each treatment, entries correspond to the mean (s.d.) of group reports in each of six rounds of Part 2, and number of groups in treatment. P-value from non-parametric Kruskal-Wallis test reported in bottom row.

Even though *Ple* offers \$7 more budget room than *Lim*, the mean group reports fall within a mere one-point range of 16.8 to 17.8 in the initial round for four of the five treatments. Moreover, the mean group reports in the three *Lim* treatments (*SimLim*,

[9]For a comparison with the equilibrium prediction in the two *SeqLim* treatments, we will need to separate out the claims of the first three players, which we do in the next subsection.

SeqLimFix and *SeqLimRan*) remain well within one point of each other for all six rounds. Only reports in *SeqPle* are substantially above those in all other treatments where they remain throughout the six rounds.

Result 1 *Individual reports in SeqPle are significantly higher than all other treatments. Reports in SeqLimFix and SeqLimRan do not differ significantly from one another. Finally, reports in SimPle are significantly higher than those in SeqLimFix and SeqLimRan, but not significantly different from SimLim.*

Evidence: Table 4 displays the results from GLS random-effects regressions on individual i's Part 2 round t report, $t = \{1, \ldots, 6\}$. Regressions (1) and (2) include treatment indicators for *SimPle* and all three *Lim* treatments. *SeqPle* is the omitted treatment, with its mean round 1 report captured by the constant of 4.87 in (1). Individual reports in the other four treatments are all 0.4 (*SimPle*) to 0.7 (*SeqLimFix*) pips significantly lower than *SeqPle* (all $p < .04$). Given the same \$24 budget, significantly higher reports in *SeqPle* compared to *SimPle* attest to the importance of moral licensing, as anticipated in the last paragraph of Section 2.3.

Reports in *SimPle* are significantly higher than those in *SeqLimFix* ($p = .02$) and *SeqLimRan* ($p = .08$), but not significantly different than *SimLim* at conventional levels ($p = .16$). Finally and in contrast to our expectation, reports in *SeqLimRan* and *SeqLimFix* are not significantly different from one another ($p = .38$).

All these findings remain robust to the inclusion of a set of controls in regression (2). Among the controls, the round indicators in both regressions reveal no significant overall time trend in reports in the first five rounds of play ($p > .4$).[10] We do observe an endgame effect through a modest uptick of 0.20 to 0.22 pips in individual reports in the terminal round ($p = .04$ in both regressions).

[10] Individual treatment regressions (not displayed, but available upon request) show no significant time trend in any of the five treatments.

Table 4: Regression Results on Individual Die Reports

Variable	(1) *All*	(2) *All*	(3) *Sim*	(4) *Seq*
SeqLimFix	-0.702*** (0.138)	-0.702*** (0.142)	——	-0.701*** (0.143)
SeqLimRan	-0.613*** (0.139)	-0.641*** (0.136)	——	-0.635*** (0.137)
SimLim	-0.599*** (0.167)	-0.630*** (0.161)	-0.234 (0.174)	——
SimPle	-0.362** (0.171)	-0.399** (0.177)	——	——
round 2	0.019 (0.082)	0.328 (0.817)	0.250* (0.145)	-0.108 (0.093)
round 3	0.024 (0.086)	0.000 (0.088)	0.199 (0.153)	-0.129 (0.105)
round 4	0.058 (0.080)	0.066 (0.828)	0.141 (0.126)	0.017 (0.110)
round 5	0.073 (0.091)	0.058 (0.093)	0.192 (0.158)	-0.029 (0.114)
round 6	0.218** (0.091)	0.199** (0.092)	0.314** (0.157)	0.125 (0.114)
risk	——	0.043* (0.025)	0.083** (0.041)	0.017 (0.030)
age	——	-0.017** (0.008)	-0.021* (0.012)	-0.014 (0.011)
female	——	-0.166 (0.112)	-0.242 (0.187)	-0.113 (0.140)
University X	——	0.039 (0.096)	0.012 (0.180)	0.052 (0.115)
constant	4.865*** (0.126)	5.070*** (0.299)	4.441*** (0.0564)	5.213*** (0.332)
R^2	.027	.037	.033	.044
Obs.	2,472	2,376	936	1,440

Notes: Random-effects regressions on individual i's round t report. Regressions (3) and (4) restrict attention to *Sim* and *Seq* treatments, respectively.Regressors include treatment (*SeqPle* is omitted in (1), (2) and (4), *SimPle* is omitted in (3)) and round dummies. Robust standard errors in parentheses are clustered on groups of four participants. Sixteen participants (96 observations) who reported their gender as "non-binary" are omitted from (2) and correspondingly for (3) and (4). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Age is a significantly negative predictor of die reports ($p = .03$), whereas gender and the location of the experiments (University X or University Y) are not significant predictors.

In the post-experiment questionnaire, we also elicited each participant's self-reported willingness to take risks.[11]. The coefficient of 0.043 in (2) indicates that the more willing a participant is to take risks, the higher their die report on average ($p = .08$). However, the role of risk aversion in restraining reports is not equal across treatments, nor is it predicted to be so, as discussed in the last paragraph of Section 2.3.

Result 2 *In the SimLim treatments, the more willing participants are to take risks, the higher their die reports, on average. By contrast, risk preferences do not predict die reports in SimPle or the Seq treatments.*

Evidence: When we conduct the same random-effects regressions on the *Sim* treatments and separately on the *Seq* treatments (i.e., *SeqPle*, *SeqLimFix* and *SeqLimRan*), regression (3) reveals a highly significant coefficient of 0.083 on the risk measure for *Sim* ($p = .04$). In other words, each additional point on the 11-point willingness-to-take-risks scale is associated with an average increase of 0.083 pips reported. At the same time, the estimate of 0.017 in regression (4) for *Seq* treatments is statistically indistinguishable from zero ($p = .57$).

The intuition is clear: reports in *Seq* carry no risk; regardless of position in the queue, a player knows the available budget when deciding how much to report. In *Sim*, by contrast, players do not know how much other group members will report. Thus, a higher report in *SimLim* is associated with an increased risk of a zero payoff. What is more, when we exclude *SimPle* and limit this same regression specification to the limited budget treatment, *SimLim*, the risk coefficient and its significance increase further ($\beta = 0.111$, $p = .01$), while the same specification for *SimPle* only renders risk insignificant ($\beta = 0.049$, $p = .53$).

[11] See question 7 in Appendix B and Dohmen et al. (2011) for a discussion of the behavioral validity of this measure.

4.3 Player symmetries and asymmetries

Result 3 *In the SeqLim treatments, Players 1, 2 and 3 report similar die rolls, whereas Player 4 reports significantly less. By comparison, all four players in SimLim report similar amounts, as expected.*

Evidence: Panel A of Table 5 displays the mean individual die report by player order for each treatment. In *SeqLimFix*, Players 1, 2 and 3 all report within half a pip of one another on average (from 4.34 to 4.85), whereas Player 4's mean report of 3.06 is more than a full pip less than the other players (Kruskal-Wallis $p < .001$). In *SeqLimRan*, the first three players' mean reports are even more tightly bunched between 4.60 and 4.78, contrasted with Player 4's mean report of 3.15 ($p < .001$). By comparison, the bottom row of Panel A shows that all four players in *SimLim* report within 0.4 pips of one another, on average ($p = .43$).

Table 6 displays the estimates from pairs of GLS random-effects regressions on individual i's round t die report for each of *SeqLimFix*, *SeqLimRan* and *SimLim*. The regression coefficients in Table 6 confirm the significance of these player reporting differences (*SeqLimFix* and *SeqLimRan*) and lack thereof (*SimLim*).

In the first regression in each pair, the sole regressors are indicators variables for Players 2, 3 and 4. Player 1's die report is captured by the constant. The second regression in each pair includes the same socio-demographic controls and round indicators reported in Table 4.

The estimates on the Player 2 and Player 3 dummies are close to and not significantly different from zero in both regressions for the *SeqLimFix* and *SeqLimRan* treatments. By contrast, the highly significant estimates of -1.6 and -1.7 on the Player 4 dummy in these regressions imply that Player 4 reports 1.6 to 1.7 pips less on average than Player 1 ($p < .001$). All t-tests of coefficients confirm that Player 4 reports significantly less than Players 2 and 3 as well ($p < .001$).

The *SimLim* regressions serve as placebo regressions because players are *ex ante* symmetric in this simultaneous-moves treatment. In the *SimLim* regressions (10) and (11), none of the Player coefficients nor any of the t-tests of coefficients differs significantly from

Table 5: Mean Reports and Payoffs, by Treatment and Player

Panel A: Report Averages

Treatment	Player 1	Player 2	Player 3	Player 4	Kruskal-Wallis
SeqPle	4.87 (1.11)	4.95 (0.99)	4.88 (0.96)	5.02 (1.00)	$\chi^2(3) = 0.36$ $p = .95$
SeqLimFix	4.66 (0.91)	4.85 (0.77)	4.34 (0.90)	3.06 (0.65)	$\chi^2(3) = 34.27$ $p < .001$
SeqLimRan	4.75 (0.83)	4.78 (0.72)	4.60 (0.67)	3.15 (0.70)	$\chi^2(3) = 35.14$ $p < .001$
SimPle	4.49 (1.15)	4.49 (1.11)	4.42 (1.22)	4.88 (1.02)	$\chi^2(3) = 1.49$ $p = .68$
SimLim	4.58 (0.84)	4.32 (0.86)	4.17 (1.02)	4.25 (0.86)	$\chi^2(3) = 2.77$ $p = .43$

Panel B: Payoff Averages

Treatment	Player 1	Player 2	Player 3	Player 4	Kruskal-Wallis
SeqPle	4.87 (1.11)	4.95 (0.99)	4.88 (0.96)	5.02 (1.00)	$\chi^2(3) = 0.36$ $p = 0.68$
SeqLimFix	4.66 (0.91)	4.85 (0.77)	4.34 (0.90)	2.06 (1.00)	$\chi^2(3) = 43.81$ $p < .001$
SeqLimRan	4.75 (0.83)	4.78 (0.72)	4.55 (0.69)	2.19 (1.20)	$\chi^2(3) = 40.27$ $p < .001$
SimPle	4.49 (1.15)	4.49 (1.11)	4.42 (1.22)	4.88 (1.02)	$\chi^2(3) = 1.49$ $p = .68$
SimLim	4.15 (0.70)	3.54 (0.67)	3.52 (0.90)	3.55 (1.07)	$\chi^2(3) = 8.09$ $p = .04$

Notes: Panel A: Mean report (s.d.) by treatment and player order. Panel B: Mean payoff (s.d.) by treatment and player order. The right-most column tests for player differences in the mean report (Panel A) and the mean payoff (Panel B) using the non-parametric Kruskal-Wallis test where a player's mean report (payoff) over the six rounds of play is the unit of observation. Although players' mean reports in *SimLim* do not differ significantly from one another ($p = .43$), their mean payoffs do ($p = .04$) because Player 1's report was randomly chosen for exclusion only 13 times compared to 19, 19 and 18 times for Players 2, 3 and 4, respectively.

Table 6: Player Order Effects

Variable	*SeqLimFix*		*SeqLimRan*		*SimLim*	
	(6)	(7)	(8)	(9)	(10)	(11)
Player 2	0.190 (0.234)	0.184 (0.268)	0.028 (0.220)	-0.006 (0.227)	-0.258 (0.240)	-0.069 (0.248)
Player 3	-0.317 (0.312)	-0.407 (0.300)	-0.170 (0.154)	-0.189 (0.156)	-0.417 (0.301)	-0.312 (0.255)
Player 4	-1.595*** (0.245)	-1.737*** (0.248)	-1.699*** (0.270)	-1.705*** (0.274)	-0.333 (0.250)	-0.171 (0.257)
constant	4.659*** (0.199)	4.881*** (0.613)	4.792*** (0.176)	4.658*** (0.604)	4.583*** (0.188)	4.012*** (0.465)
Controls	No	Yes	No	Yes	No	Yes
R^2	.186	.208	.165	.186	.011	.094
Obs.	504	474	504	492	480	468

Notes: Random-effects regressions on individual i's round t report in *Lim* treatments. Regressors are dummies for the player's order in the queue (Player 1 is omitted). Robust standard errors in parentheses are clustered on group of four participants. Controls are risk preferences, age and indicators for female, location of experiments and round number. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

zero. That is, in contrast to the asymmetric Nash equilbrium, all four group members report similar amounts on average, as expected.

4.4 Excess reports

4.4.1 Reporting dynamics in *SimLim*

Despite observed symmetry in individual reports across players' labels (e.g., Player 2, Player 3) in *SimLim*, we find substantial asymmetry between group reports as a function of whether the group exceeded or remained within budget in the previous round.

Result 4 *In SimLim, when a group exceeds the budget in round t, both the randomly chosen unpaid player and the other paid group members reduce their round $t+1$ reports significantly and by similar amounts. In comparison, when a group does not exceed the budget in round t, reports increase significantly in round $t+1$.*

Evidence: Table 7 displays the mean (s.d.) individual reports in round t and $t+1$ based on whether the group remained within the \$17 budget in round t ("non-exclusion round") or exceeded budget ("exclusion round"). In this latter case, the table distinguishes between the excluded player (i.e., the randomly chosen player not paid their report) and non-excluded players (i.e., those who were paid their reports).

Table 7: Comparison of Report Dynamics between Within-Budget and Over-Budget Rounds

SimLim	**Non-Exclusion Rounds**	**Excluded Player**	**Non-Excluded Player**	**t-test (p-value)**
Round t	3.75 (1.50)	5.18 (0.94)	4.89 (1.13)	1.67 (0.099)
Next Round $t+1$	4.14 (1.48)	4.82 (1.45)	4.59 (1.38)	0.89 (0.378)
Paired t-test	-3.07 (0.002)	1.43 (0.160)	2.33 (0.022)	

Notes: For *SimLim*, entries display the mean (s.d.) individual report according to whether the group report remained within or exceeded the budget in round t and, in the latter case, the player did not receive payment ("Excluded") or did receive their report ("Non-Excluded"). T-statistic and p-value from paired t-tests of individual mean reports in rounds t and $t+1$ are reported in the bottom row. Independent t-tests of mean reports between excluded and non-excluded players for round t and $t+1$, respectively, in right-most column.

Table 7 reveals that in groups that do not exceed budget (mean individual report 3.75), group members significantly increase their mean reports to 4.14 in the subsequent round ($p = .002$). By contrast, in groups that went over budget in round t, individual group members reduce their reports in round $t+1$. Interestingly, excluded and non-excluded players respond alike: both reduce their mean report by about 0.3 pips.[12]

4.4.2 Virtue Signaling in *SeqLim*

The strategic uncertainty about others' choices in *SimLim* plausibly leads some groups to exceed budget, as previously observed. Although strategic uncertainty is absent in *SeqLimFix* and *SeqLimRan*, we nonetheless observe instances of group reports in excess of the budget.

[12]The fact that non-excluded players' mean decrease of 0.30 pips is highly significant ($p = .022$), whereas excluded players' mean drop of 0.36 is not significant ($p = .160$) stems from the paucity of observations ($n = 44$) in the latter case. An excluded player is registered in *SimLim* only for a single randomly chosen group member (two group members in two lone group-rounds where one exclusion did not bring the group within budget) when the group exceeds budget and only in rounds 1-5 (round 6 is dropped because there is no round $t+1$).

This happens when Player 4 faces a budget less than \$6 and chooses to report an amount greater than the available budget.[13] Such a decision guarantees Player 4 a payment of \$0. As shown below, our data rule out player confusion as a possible explanation. Instead, the intuitive motivation for this costly decision is to signal to other group members or oneself the player's payoff sacrifice. We refer to this behavior as virtue signaling.[14]

Result 5 *Players reporting last in SeqLimFix and SeqLimRan oftentimes knowingly choose to forego a positive profit by reporting a die outcome strictly greater than the available budget. Notwithstanding, profit-maximization in the form of reporting the die outcome that exactly equals the available budget is the most common choice.*

Evidence: Table 8 displays the complete distribution of Player 4's reporting decisions for each available budget in the combined *SeqLimFix* and *SeqLimRan* treatments. Figure 2 displays a simplified histogram for each budget from \$1 to \$6 according to whether Player 4 reported less than (in blue), equal to (in grey) or greater than (in red) the available budget.

To begin, when Player 4 faces a remaining budget of \$0, they are indifferent among all possible reports. Myerson (1979) argues that indifferent individuals simply report truthfully. Indeed, a chi-squared test of Player 4's reports when \$0 remain cannot reject that this distribution differs from the uniform distribution ($\chi^2(5) = 6.59$, $p = .25$). By contrast, for all other budgets between \$1 and \$5, we can strongly reject that Player 4's report distribution is uniform ($p < .001$ for all five tests).

Next, focusing on rounds in which a strictly positive but restricted budget was available to Player 4, we find, surprisingly, that Player 4 engages in payoff-sacrificing virtue signaling in 33 out of a total of 185 (17.8%) rounds in which \$1 to \$5 was available in these two treatments.

[13]In theory, Player 3 could also face a limited budget of \$5 if Players 1 and 2 both report 6. In practice, there are no such instances in our dataset.

[14]Alternatives to our excess-claims rule that we considered include paying the last claimant the remaining budget or paying each group member in proportion to their report to exactly exhaust the budget. Both these options eliminate the cost of over-reporting and thereby extinguish virtue signaling. Jiang and Villeval (2024) examine a more radical variant in which all group members face a 60% probability of a collective sanction of no payment when the budget is exceeded.

Table 8: Player 4's Reports in *SeqLimFix* and *SeqLimRan* for Different Available Budgets

Available Budget	Player 4 die report						Total
	1	2	3	4	5	6	
\$0	10 (.294)	4 (.118)	7 (.206)	6 (.176)	2 (.059)	5 (.147)	34
\$1	25 (.610)	3 (.073)	3 (.073)	3 (.073)	4 (.098)	3 (.073)	41
\$2	4 (.108)	27 (.730)	2 (.054)	2 (.054)	0 (.000)	2 (.054)	37
\$3	6 (.162)	4 (.108)	21 (.568)	4 (.108)	1 (.027)	1 (.027)	37
\$4	6 (.146)	4 (.098)	6 (.146)	20 (.488)	3 (.073)	2 (.049)	41
\$5	3 (.103)	1 (.035)	5 (.172)	6 (.201)	14 (.483)	0 (.000)	29
\$6	1 (.077)	2 (.154)	2 (.154)	1 (.077)	1 (.077)	6 (.462)	13
\$7, \$8, \$9	2 (.100)	0 (.000)	2 (.100)	3 (.150)	5 (.250)	8 (.400)	20
All	47 (.216)	41 (.188)	41 (.188)	39 (.180)	28 (.128)	22 (.101)	218

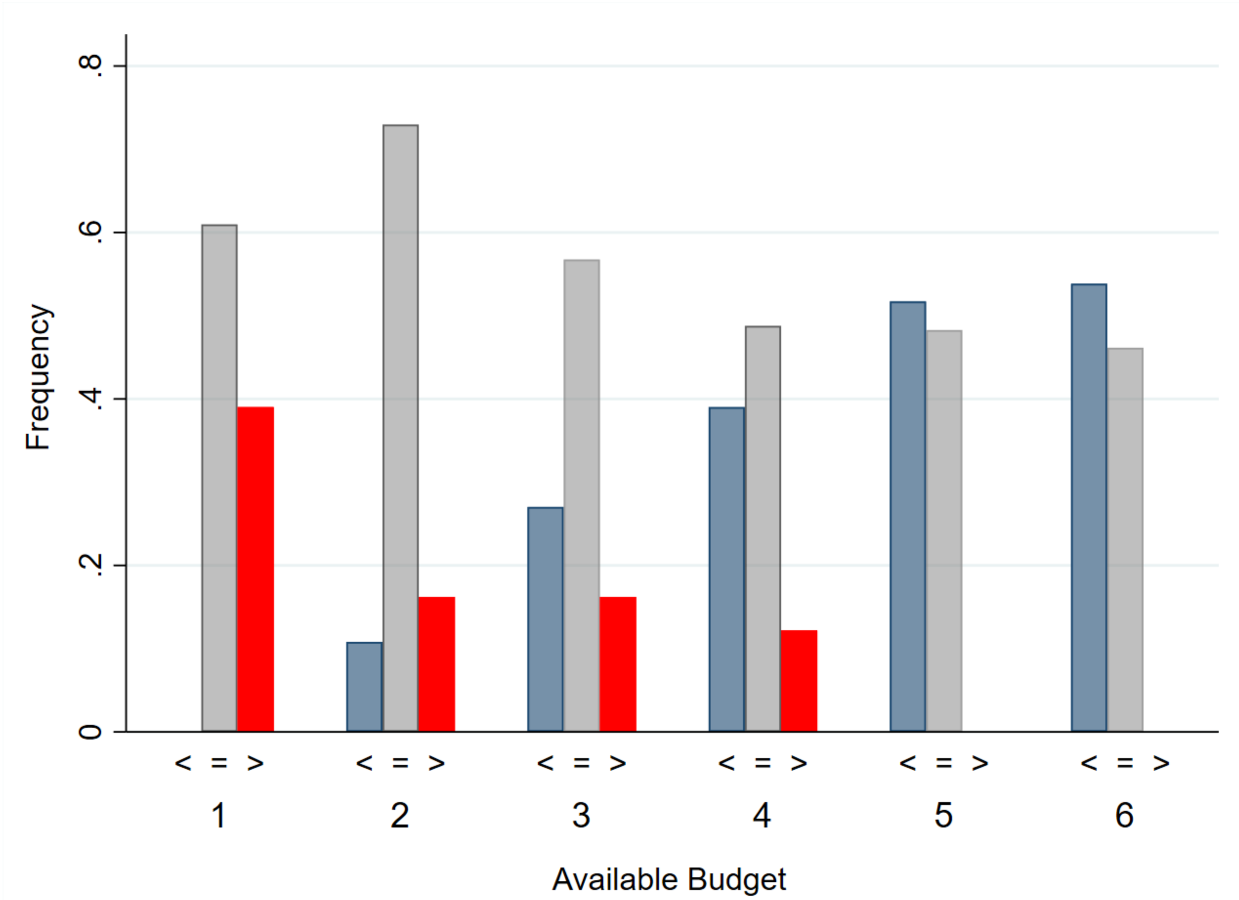
Notes: Distribution of Player 4's Part 2 die reports for each available budget in the combined *SeqLimFix* and *SeqLimRan* treatments.

Less surprising is the observation that the modal die report is the one that matches the available budget when the available budget is \$1, \$6 and all amounts in between. The high anomalous frequency with which Player 4's reports match the available budget attests to dishonest profit-maximizing behavior. For example, among the 41 instances in which Player 4 faces an available budget of \$1, they choose to report precisely \$1 25 times and to virtue signal (i.e., report strictly more than \$1) on the remaining 16 occasions. When \$2 was available to Player 4, they report \$2 27 times, under-report just four times and virtue signal six times. Profit maximization continues to account for more than half of the observations when Player 4's available budget is \$3 and \$4.

Figure 2 makes clear that virtue signaling accounts for almost 40% of Player 4's decisions when faced with a \$1 budget, more than 15% of decisions when \$2 or \$3 remains and about 10% of decisions with \$4 available. Amusingly, despite 29 rounds in which \$5 was available to Player 4 in these treatments, we observe no instances of virtue signaling. Apparently \$5 is too high a price to pay to signal one's virtue.

Player confusion is a potential alternative explanation for reports that exceed the available budget in *SeqLimFix* and *SeqLimRan*. Specifically, perhaps Player 4 mistakenly believes

Figure 2: Distribution of Player 4 Die Reports by Available Budget



Notes: Histograms for Player 4's reports in *SeqLimFix* and *SeqLimRan* for each available budget between $1 and $6 according to whether Player 4 reported less than ("<"), equal to ("=") or greater than (">") the remaining budget.

that they get the remaining budget – rather than nothing – whenever they report more than the available budget. To check for this possibility, we included the following question in the incentivized quiz in both *SeqLim* treatments, "You have been assigned to be the fourth (last) person making your report. The current balance in the group budget is $3. You make a report of 4. How much money will you receive?" All 33 cases (24 distinct participants) answered this question correctly, thereby ruling out confusion as a possible explanation.[15]

Further evidence of virtue signaling and its costliness in *SeqLimFix* and *SeqLimRan*

[15]By comparison, among the 152 non-virtue signaling cases (74 distinct participants), 10 answered this question incorrectly, with seven of these participants responding $3.

comes from a comparison of Player 4's mean report and payoff, displayed in Table 5, Panels A and B, respectively. In both treatments, Player 4's mean report is approximately 3.1 pips contrasted with a mean payoff of only about 2.1 — a full pip less. A payoff-maximizing Player 4 ought to report the remaining budget, in which case the payoff would equal the report. This is indeed what we observe for all other players across all treatments. The observation that Player 4 in these *SeqLim* treatments earns, on average, about 32% less than they report underscores both the prevalence and costliness of virtue signaling.

A reporting decision that exceeds the available budget in the final round of play represents a pure, uncontaminated form of virtue signaling. By contrast, virtue signaling in early rounds may stem from strategic considerations, namely, a message to group members to lower their reports in future rounds so that Player 4 has more budget room to report honestly and earn a positive payment. Interestingly, 10 out of the total 33 (30.3%) instances of virtue signaling occur in this final round, which is a significantly higher proportion than the one-sixth anticipated were instances distributed uniformly across rounds ($\chi^2(1) = 4.42$, $p = .04$).

5 Conclusion

A fundamental goal in the distribution of government benefits and of economic policy more generally is equity in the sense that everyone receives close to what they need or deserve (see, e.g., Barr (2020)). In this paper, we introduce a novel experimental environment which closely mirrors real-world scenarios to study how varying reporting structures and budget constraints affects distributional outcomes. Our reporting structures — simultaneous and sequential — represent two fundamental and widely used alternatives.

Both reporting structures reveal significant and similar degrees of over-reporting in aggregate; however, their distributional consequences differ markedly. In sequential structures, Player 4 bears the full burden of earlier players' over-reporting and is forced to settle with less or nothing at all when resources are limited. By contrast, in simultaneous reporting, all four players internalize and share equally in the consequences of inflated claims; namely, each is equally likely to receive zero payment. When resources are plentiful (i.e., no externality to reporting), participants dispense with honesty in the sequential structure, while the simultaneous structure continues to constrain reports. Thus, the main takeaway is that the simultaneous reporting structure offers two advantages: greater honesty when resources are abundant and greater equity when they are scarce.

In fact, Player 4's virtue signaling is a means of protest to this inequity under sequential reporting and scarce resources. Rather than accept the small remaining share, Player 4 frequently chooses no payment, signaling to others that their rightful entitlement was much higher. This behavior has numerous real-world analogues. Consider, for example, a conference where panelists are told that they each have 10 minutes to speak. After the first three speakers exceed their allotment, the final speaker is left with, say, just two minutes. Rather than impose their full presentation on the audience and disrupt the schedule, the fourth speaker declines to speak altogether. Alternatively, after star players on sports teams negotiate their salaries, there may be little cap space for role players. In response, the role players may demand a contract that knowingly exceeds the remaining cap – or walk away entirely – to signal their worth.

Our results offer valuable insights to policymakers and social planners: in particular, use simultaneous structures when allocating a resource. The distinction between simultaneous versus sequential decision-making is often viewed as an exogenous, temporal feature of the setting. However, withholding or restricting information about other participants' decisions can effectively transform a sequential information structure into a simultaneous one. Institutions may employ information design, such as delayed feedback or concealing interim outcomes, to mitigate strategic behavior driven by sequence. Such measures are particularly relevant when a strictly simultaneous process cannot be realistically enforced. For example, government benefits are notoriously vulnerable to applicants "gaming" the system. This is often a result of participants communicating about the process over an extended period of time. Government agencies can instead require applicants to submit through an online portal within a narrow time window. Strong incentives that discourage inter-participant communication, such as legal sanctions or efforts to identify fraud-oriented online communities, can also achieve the same impact as strict simultaneity.

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Appendix A: Participant Instructions

* Treatment differences are noted in brackets.

Introduction

Welcome to the study.

This is an experiment funded by SSHRC Institutional Grants and the International Foundation for Research in Experimental Economics to study decision-making. For showing up on time, you will receive $7. You will also receive additional earnings. The exact amount will depend on outcomes in today's experiment. All earnings that you make will be paid to you in cash at the end of the experiment.

Today's session will last no more than 50 minutes. Please work on your own and avoid communicating with other participants during the experiment.

Thank you for your participation.

Overview

Today's experiment is split into two parts. These are the instructions for Part 1. After you have completed Part 1, you will receive instructions for Part 2.

After reading the instructions, you will be asked to complete a quiz to ensure your understanding of the experiment. You will receive $0.25 for each correctly answered question.

Instructions – Part 1

In this experiment, the computer will randomly assign you into groups of 4 with the other participants in this session. Each group has an allocated budget of $24 [$17]. This group budget of $24 [$17] is the total amount available to the 4 group members.

In a moment, you will be asked to roll the die in front of you and make a report on the outcome. The outcome of the die roll represents your claim on the $24 [$17] group budget. You are the only person who will know the result of your die roll, and thus your corresponding claim on the group budget.

[***Sim***: Each of you will make your report at the same time. When it is time to make the report, you will be prompted to roll the die and report the outcome.] [***Seq***: Each of you will make your report one at a time. Your reporting order will be randomly determined. When it is your turn, you will see the reports of the group members before you as well as the amount remaining in the group budget. You will then be asked to roll the die and report the outcome.]

When you make your report, your corresponding payment will come out of the group budget. [***SimLim***: If the total amount of reports from the group exceeds the group budget of $17, one person will be selected at random to not receive their payment. If, after removing the report of the randomly selected person, the total amount of remaining reports still exceeds the group budget, another person will be selected at random to not receive their payment. This is to ensure that the total amount of reports being paid out falls within the $17 group budget.] [***SeqLim***: If you make a report that exceeds the amount of money remaining in the group budget, you will not receive any money, and the group budget will remain as it was before your report. Anyone making a report after the group budget has fallen to $0 will earn $0.]

If there is any money remaining in the group budget after everybody in the group has made their report, the remaining balance will not be paid out.

Your dollar payment for each possible die report appears in the table below.

Die Report	Corresponding Dollar Payment
1	$1
2	$2
3	$3
4	$4
5	$5
6	$6

You will then be asked to complete a short quiz to ensure your understanding of the task. If you have any questions at any point in the experiment, please raise your hand and an experimenter will come to assist you.

Instructions Review

Before you begin the quiz, the experimenter will now review the instructions by reading them out loud and answering any questions you may have.

Instructions – Part 2

Part 2 is similar to Part 1. You will be randomly reshuffled by the computer into new groups of 4 and asked to repeat what you did in Part 1 for 6 consecutive rounds. You will stay in the same group for all 6 rounds. [***SeqPle***, ***SeqLimFix***: You will report in the same order for all rounds (i.e., 1st, 2nd, 3rd, 4th).] [***SeqLimRan***: Your reporting order (i.e. 1st, 2nd, 3rd, 4th) will be randomly reassigned at the start of each round.] At the end of the experiment, one of the six rounds will be randomly selected for payment.

Once you have finished, you will be asked to complete a short questionnaire.

Please raise your hand if you have any questions.

Please press ‘Next’ to proceed to the task for Part 2.

Brief Review

Before you begin Part 2, the experimenter will briefly review the instructions by reading them out loud and answering any questions you may have.

Appendix B: Post-Experiment Questionnaire

1. Age: ___________

2. To which gender do you most identify?

    0, Male

    1, Female

    2, Non-Binary

    3, Trans-Male

    4, Trans-Female

    5, Other (Write Below)

    (If Applicable) Other: ____________________________

3. Are you currently a student?

    Yes No

4. If yes, to which faculty do you belong? (Select Not a Student if Not Applicable)

    1. [Not a Student]

    2. [Degroote School of Business]

    3. [Engineering]

    4. [Health Sciences]
    5. [Humanities]
    6. [Science]
    7. [Social Sciences]
    8. [Arts & Science]

5. What is your field of study? (N/A if not a student)

   ____

6. Year of Study:

    1. [Not a Student]
    2. [1st year undergraduate]
    3. [1st year undergraduate]
    4. [1st year undergraduate]
    5. [1st year undergraduate]
    6. [1st year undergraduate]
    7. [M.A]
    8. [Ph.D.]
    9. [Post-Doctoral student]

7. How willing are you to take risks, in general? (0 being unwilling, 10 being very willing)

   [0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10]

8. Please indicate how clear the instruction were to you. (1 being not understood at all, 7 being completely understood)

   [1, 2, 3, 4, 5, 6, 7]

9. Which statements best represents what was important to you when making your reports?

1. I found it important to report my true claim on the budget.

2. I found it important to take into account others' payoffs when making my report.

3. I did not find either important when making my report.

10. For Part 2 of the experiment, indicate on a scale from 1 to 7 the influence of the following factors on your decision on how much to report in a given round where 1 is ”no influence at all”, and 7 is ”strongly influenced”.

   a) Other participants' payoffs from the preceding round.

      [1, 2, 3, 4, 5, 6, 7]

   b) Total amount the group claimed in the preceding round.

      [1, 2, 3, 4, 5, 6, 7]

   c) My payoff from the preceding round.

      [1, 2, 3, 4, 5, 6, 7]

   d) The number of rounds remaining in the experiment.

      [1, 2, 3, 4, 5, 6, 7]

11. Which of these options best represents your observations of the other participants in your group from Part 2? [the ordering randomly reverses with 50% probability]

   1. Everyone claimed more than me.
   2. On average, others claimed more than me.
   3. On average, others claimed the same as me.
   4. On average, others claimed less than me.
   5. Everybody claimed less than me.

12. [SEQUENTIAL SPECIFIC] For Part 2 of the experiment, indicate on a scale from 1 to 7 the influence of each of the following factors on your decision on how much to report in a given round where 1 is ”no influence at all”, and 7 is ”strongly influenced”.

a) How much the participant(s) before me reported.

[1, 2, 3, 4, 5, 6, 7]

b) How much was left in the budget when it was my turn to report.

[1, 2, 3, 4, 5, 6, 7]

c) How much will be left in the budget for participants after me.

[1, 2, 3, 4, 5, 6, 7]

13. [SEQUENTIAL SPECIFIC] Please indicate on a scale from 1 to 7 how well each factor describes your behaviour in the experiment where 1 is ”does not describe at all”, and 7 is ”describes very well”.

If you were never first or second in the reporting order, you may leave the question blank. When I was first or second in the reporting order:

a) I took as much as possible.

☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7

b) I took into consideration those after me.

☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7

c) [SEQLIMRAN SPECIFIC] I took into consideration my placement in future rounds.

☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7

14. Here is a text box in which you may enter any comments you wish about the experiment.

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