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# Information defaults in repeated public good provision

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ABSTRACT

We study an unexplored type of defaults - information defaults - in a repeated public good provision setting. In our treatments, we vary the default of having or not having contribution information as well as whether the information comes with a positive, zero or negative financial incentive. We find that almost all subjects have the information when this is cost-free or financially beneficial, but around a third have the information even when this is costly. Moreover, a default of not having information leads to a slower unraveling of cooperation which is matched by the beliefs about others' contributions in these treatments. We also find a secondary informational default effect, challenging previous findings on how defaults work: when the default is no information, subjects do not seek information more often, but they tend to believe that more other subjects seek information.



## 1. Introduction

*"Information is the mortar that both builds and destroys empires."*
Tobsha Learner

Given the increasing importance of team collaboration for organizational productivity as well as, more generally, for public good provision, how to design a better information architecture to maximize cooperation is of great importance to academics and practitioners alike. Often it is assumed that policy makers or managers have perfect control over information flows, and therefore can decide whether people will know about others' behavior; however, as anyone with managerial experience knows, more often than not information flow control is imperfect. A first step towards an optimal information architecture that takes this problem into account is to understand how people behave in a setting with imperfect information flow control. This paper aims to take this first step by presenting the results of an experiment that models such a setting, and where there are *information defaults*, but people can switch out of them.

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The case of an endogenous choice architecture where the policy makers or managers or the agents collectively themselves can only affect what the information default is, and what the incentives or barriers are to switch from one information state to another, is therefore highly relevant. Take the example of performance reward transparency as an attempt to fight any unjustified pay differentials. Some employers might provide their employees with the right to learn about the income of similarly qualified employees in their company (the information default being that they do not know the average income in the first place), whereas other employers might provide such information as a default (e.g., as part of an annual internal information campaign), allowing for an opt out of such campaigns. Importantly, in both situations, employees can choose whether or not to learn about the incomes. However, the number of employees actually having the information might be quite different across these two scenarios. Following on, such different information states might also result in differential work performance.

That is, despite not limiting the information set, an information default may affect how agents behave, making it important to study them. For example, it is yet unclear whether information defaults will result in default inertia – as is typically the case with choice defaults.

Our paper is a step forward in studying an endogenously determined information architecture with information defaults implemented. Specifically, we are interested in two types of defaults: (i) people have information on others' contribution by default and have to actively opt out if they do not want this information (Opt-out); (ii) people do not have such information by default and have to actively opt in if they want this information (Opt-in). In addition, we are interested in whether the effect of information defaults on information seeking is sensitive to the price of information. Paying a positive price to acquire/avoid information implies that people place positive/negative value on the information.

We choose a public good environment as the stylized setting to study endogenous information architecture in a first experiment on this topic. Apart from being behaviorally well understood, the advantage of a public good setting is that it has wide applicability. It helps embody many environments where there are advantages from mutual cooperation (such as team production in a company) in a stylized way.

When it comes to information availability, one might think that more information is always better than less. Revealing peer information is indeed a common nudge used to promote pro-social behavior such as charitable giving (Smith et al., 2015) and voluntary contribution (Shang and Croson, 2009). However, there is mixed evidence on the effect of peers' contribution information in voluntary contribution settings. Some studies find positive effects (Shang and Croson, 2009; Nikiforakis, 2010; Tasch and Houser, 2018) while others find no effect (Sell and Wilson, 1991; Weimann, 1994; Croson, 2001; Cox and Stoddard, 2015) or negative effects (Wilson and Sell, 1997; Neugebauer et al., 2009; Chaudhuri et al., 2017) or, in a meta-review, mixed effects (Fiala and Suetens, 2017).[2] Carpenter (2004) and Sonntag and Zizzo (2019) find contribution unraveling as a result of information respectively in repeated public good contribution and team production settings. Most of the research however is on an exogenously set information architecture.

There are four exceptions we are aware of looking at specific aspects of endogenously determined information architecture.[3] In a circular public good game experiment, Kurzban and DeScioli (2007) found 46% of subjects were willing to pay a small price (0.40 USD) to view previous contribution information. In contrast, in a sequential public good game where subjects made contribution choices but then had an opportunity to revise choices based on information about the contribution of others, 12% of subjects refused to do so even when this was free (Tasch and Houser, 2018). Bigoni and Suetens (2012) had a treatment where subjects had a choice whether to have extra information on individual contributions for free and most of the subjects did, but in any case, they had aggregate contribution information. In a team real effort task, Sonntag and Zizzo (2019) found that people rarely seek knowledge about whether the others' production is due to effort or to chance, even when the cost of doing so is negligible.

Our paper is also related to the strand of literature on default options. Defaults have been found to be influential in various domains such as retirement saving (Madrian and Shea, 2001; Thaler and Sunstein, 2003; Choi et al., 2004; Beshears et al., 2008), insurance choice (Johnson et al., 1993) and organ donation (Johnson and Goldstein, 2003). The effect may come from various channels. For instance, the perception of defaults being suggestions or normative signals (Madrian and Shea, 2001; Choi et al., 2004; McKenzie et al., 2006; Thaler and Sunstein, 2008), loss aversion (Kahneman et al., 1991; Samuelson and Zeckhauser, 1988) and subtle influence on preferences (Dhingra et al., 2012). The use of *contribution* defaults has been studied empirically in public good experiments (Messer et al., 2007; Altmann and Falk, 2009; Cappelletti et al., 2014; Carlsson et al., 2015; Liu and Riyanto, 2017).

Our paper contributes to the literature on endogenous information architecture in two ways.

First, to the best of our knowledge we are the first to study *information defaults* as a treatment variable. We acknowledge that there are studies on endogenous information setting in the literature where certain types of default information are imposed (e.g., the hidden information treatment in Dana et al., 2007) or where subjects can choose to access information (e.g., Dana et al., 2007; Bigoni and Suetens, 2012; Sonntag and Zizzo, 2019), but these do not look at information defaults. The defaults used so far in the literature concern the action itself (e.g., default contribution amount in the public goods

[2] In a Cournot market experiment, Bosch-Domènech and Vriend (2003) vary the complexity of getting information about the market.

[3] DeAngelo and Gee (2020) study endogenous monitoring in public goods provision. Given that the type of monitoring chosen affects the information flow, their study has some relevance to ours. However, the contribution information in their study is linked to punishment opportunities while ours is not. More broadly, the literature on endogenous network formation in public goods provision setting is also relevant to our study (e.g., Galeotti and Goyal, 2010; Cabrales et al., 2011; Boncinelli and Pin, 2012; Goyal et al., 2017; Kinateder and Merlino, 2017; Van Leeuwen et al., 2020).

game, default saving rate in retirement saving and donating organs by default). The default we adopt in a public goods setting is about the availability of information about others' contributions. Though this information may exert influence on contribution, our default options do not involve contribution decisions directly. Looking at information defaults is novel.

Second, the way we vary the price of information is also novel - we allow for a negative price for information in addition to a zero and a positive price. Given that the effect of information on contribution remains inconclusive in the literature and that information could potentially hurt contribution, we cannot rule out the possibility that people may want to avoid this information (e.g., Huck et al., 2017). As reviewed by Golman et al. (2017), information avoidance is not uncommon even where there are no strategic considerations. If this preference to avoid information is strong enough, people might be willing to pay for not having information. Our study accommodates this possibility by including a treatment with a negative price. More importantly, our design allows us to directly observe "active" information avoidance where subjects are aware of the availability of the information and deliberately avoid the information when it is free to do so, or even if it is costly. Since acquiring information is often costly, it is difficult to capture active information avoidance in the natural-occurring environment (Golman et al., 2017). Our experimental setting can instead accommodate this.

We find that, when information comes without a financial incentive (zero price) or is financially beneficial (negative price), almost all subjects choose to have the information, but around a third have the information even when this is costly. Moreover, a default of not having information about the others' contributions leads to a slower unraveling of cooperation, independent of the price of information. This slower unraveling is matched by the beliefs about the others' contributions in these treatments. Interestingly, a secondary informational default effect appears to take place.[4] When the default is no information, subjects do not seek information more often but, conditional on financial incentives, they tend to believe that more other subjects seek information.

The rest of the paper proceeds as follows. Section 2 introduces the experimental design and the hypotheses. Section 3 details our results, followed by a discussion and conclusions in Section 4.

## 2. Experimental design and hypotheses

### 2.1. Set-up and treatments

We employ a standard linear public good game as the platform. Subjects play in groups of size $n$ and each one has an initial endowment of $E$. Each individual has a private account and all members of the same group share a common group account. Group members decide simultaneously how to allocate the endowment $E$ into their private accounts and the group account. Every unit placed in the private account generates a payoff of one unit for the individual. Every unit placed in the group account generates $\alpha$ $(0 < \alpha < 1)$ units payoff for every group member, regardless of who contributed the unit. Suppose subject $i$ contributes $c_i$ $(0 \leq c_i \leq E)$ to the group account, her payoff $\pi_i$ is as in Eq. (1):

$$\pi_i = E - c_i + \alpha \sum_{1}^{n} c_j \tag{1}$$

Since the marginal payoff from contributing to the group account is negative (i.e., $\partial \pi_i / \partial c_i = \alpha - 1 < 0$), the dominant strategy is to contribute nothing, which results in the zero-contribution equilibrium.

We adopt a full factorial $2 \times 3$ treatment design. On the one hand, we vary the information default: not having the contribution information by default (opt-in treatments) and having the contribution information by default (opt-out treatments). In the opt-in treatments, subjects do not have the information about the group members' aggregate contributions by default unless they take active decisions by clicking the box on their screen indicating they want to acquire the information. In the opt-out treatments, subjects are given the information about the group members' aggregate contributions by default unless they take active decisions by clicking the box on their screen indicating they do not want the information. We choose information on the aggregate contribution instead of individual contributions to keep the design simple. The rich nature of information on individual contributions (implying information on the aggregate, maximum, minimum and the distribution of contributions) would introduce unnecessary complexity to the interpretation of the results. We leave it for future research.

On the other hand, on the dimension of monetary incentives, we implement three levels: negative, zero and positive incentives for having the information, which are equivalent to positive, zero and negative prices of information. As noted, since the effect of information on contributions remains inconclusive in the literature, we accommodate the possibility that subjects may place negative value on information by including the negative price treatments. To minimize the noise in information seeking behavior and to elicit the preference for information, we set price (positive or negative) to a non-trivial amount.

Table 1 outlines the experiment design. In treatments with a positive price of information, having information is costly. Subjects pay a positive amount to acquire information in the opt-in treatment (PI) and receive a positive amount to avoid

[4] The primary effect of information defaults is on subjects' information seeking behavior. The secondary effect is on subjects' belief about other people's information seeking behavior. The latter effect does not concern one's own behavior but rather concerns one's belief about others' behavior, which is why we coined this effect a secondary information default effect.

**Table 1**
Experimental treatments.

| Default is | Price of information is: Positive | Zero | negative |
|---|---|---|---|
| Not having info | positive opt-in (PI, 10) | zero opt-in (ZI, 10) | negative opt-in (NI, 10) |
| Having info | positive opt-out (PO, 10) | zero opt-out (ZO, 10) | negative opt-out (NO, 10) |

Note: Treatment abbreviations and number of independent observations in parentheses.

**Table 2**
Payoff functions across treatments.

| Treatments | Whether one actively deviates from the information default | Payoff functions |
|---|---|---|
| PI, NO | Yes | $\pi_i = 18 - c_i + 0.4\sum_1^4 c_j$ |
| | No | $\pi_i = 20 - c_i + 0.4\sum_1^4 c_j$ |
| ZI, ZO | Yes | $\pi_i = 20 - c_i + 0.4\sum_1^4 c_j$ |
| | No | $\pi_i = 20 - c_i + 0.4\sum_1^4 c_j$ |
| PO, NI | Yes | $\pi_i = 22 - c_i + 0.4\sum_1^4 c_j$ |
| | No | $\pi_i = 20 - c_i + 0.4\sum_1^4 c_j$ |

information in the opt-out treatment (PO). In treatments with a zero price of information, having information does not have any financial implications on payoffs regardless of the defaults (ZI and ZO). In treatments with a negative price of information, having information is financially beneficial. Subjects receive a positive amount to acquire information in the opt-in treatment (NI) and pay a positive amount to avoid information in the opt-out treatment (NO).

### 2.2. Procedures and parameters

The experiment was conducted at Newcastle University, UK, in October and November 2016 and October 2017. Subjects were recruited randomly using Hroot (Bock et al., 2014) and the experiment was implemented using z-Tree (Fischbacher, 2007). We used a between-subject design. All decisions were anonymous. We set the group size to 4, the endowment to 20 experimental currency units (ECU, each worth GBP 0.50), $\alpha$ to 0.4 and the cost of information to 2 ECU (= GBP 1.00). Table 2 lists payoffs in terms of ECU across treatments based on whether one goes against the information default implemented. There were 40 subjects in three sessions for each treatment, totaling 240 subjects in 18 sessions. Subjects were seated randomly and were given written instructions (available in the online appendix). Instructions were also read out aloud at the beginning of every session. Questions were answered privately. Subjects had to answer all control questions correctly before proceeding with the experiment.

At the beginning of the session, subjects were randomly matched into groups and the group composition remained the same during the course of the experiment. The fixed-partner matching was chosen because we wanted to learn about the evolutionary pattern of default effects over time. In addition, the partner matching suited our purpose of studying information seeking behavior. Each subject was given an endowment of 20 ECU at the beginning of every period. Beliefs on other group members' information seeking behavior and contribution were elicited *before* the contribution decision in every period. We elicited both descriptive beliefs (what others will do) and normative beliefs (what others should do). This belief elicitation was incentivized. The additional payoff from descriptive beliefs depended on how close their beliefs matched what actually happened. The additional payoff from normative beliefs depended on how close their normative beliefs matched their group members' normative beliefs, i.e., a coordination game (Krupka and Weber, 2013).

Together with the contribution decision, subjects had the option to choose whether to have the optional information at the end of the period, depending on the treatment they were in.[5] If subjects did not make an active decision to go against the default, the default information setting was implemented in that period. The optional information included the average and total group contributions, including one's own contribution. If subjects chose to have the information, their

[5] The wording is also default dependent. The option states "I choose to get the optional information" in the opt-in treatments and "I choose not to get the optional information" in the opt-out treatments.

end-of-period feedback included their endowment, their own contributions, their own decision on whether to get the information and the group's total and average contributions in that period. If subjects chose not to have the information, their end-of-period feedback included their endowment, their own contributions, and their own decision on whether to get the information in that period (see the Appendix for sample screenshots). Feedback on earnings was not provided regardless of the information seeking decision. Subjects played the game for 10 periods. One period was randomly chosen as the binding period for payment.

We chose the pay-one approach mainly because we were concerned that the pay-all approach might distort information seeking behavior and that, under the pay-all approach, different wealth levels over time may introduce unnecessary confounds into the behavioral dynamics. Though Azrieli et al. (2018) argue theoretically that paying for one period might not be incentive compatible in repeated games, a review by Charness et al. (2016) suggests that the choice of payment method should depend on specific experiment needs. Evidence in the literature is mixed. Both the pay-all approach (e.g., Fischbacher and Gächter, 2010; Bigoni and Suetens, 2012) and the pay-one approach (e.g., Andreoni and Gee, 2012; Cappelletti et al., 2014; DeAngelo and Gee, 2020) are commonly implemented.

The average duration for the experiment was around 80 min and the average payment was around GBP 17 (roughly USD 24 at the time of the experiment). Subjects were paid in cash and in private at the end of the experiment.

### 2.3. Hypotheses

Based on the existing literature, in this subsection, we outline our hypotheses.

*Hypothesis 1: more subjects choose to have the optional information in the opt-out treatments than that in the opt-in treatments.*

Following the discussion in the introduction that defaults are sticky, we expect this kind of behavioral inertia in our study as well. In other words, we expect more subjects to end up having the optional information where having the information is the default.

*Hypothesis 2a: information has a positive value, and the law of demand applies, i.e., as the price of information increases, fewer subjects choose to have the information.*

Previous evidence from experimental public good games suggests that subjects behave as conditional cooperators (e.g., Fischbacher et al., 2001; Croson, 2007), which suggests that they typically care about knowing their peers' contributions. Subjects might use such information to match their peers' contributions, thereby avoiding being free-ridden on by others. Such a matching could also be imperfect or self-biased (Neugebauer et al., 2009; Fischbacher and Gächter, 2010), indicating that subjects may contribute less than matching proportionally to others' contributions. In any case, to condition their own behavior on the behavior of others, such information must be accessible in the first place. Following this line of reasoning, as well as previous experimental evidence reviewed in Section 1, we expect that people value the information about others' contributions and thus, we expect that subjects are willing to pay for this kind of information. We also expect the demand for information will follow the law of demand, i.e., demand for information will decrease as its price increases.

As discussed above, people value the information about others' contributions because they may rely on this information to decide their own contribution. Therefore, as a robustness check, it is useful to explore the dynamic updating process regarding subjects' contributions over time. In our specific information context, we hypothesize that subjects adjust their contributions from t-1 to t to reduce the gap between their own contributions in t-1 and their beliefs about the group's average contribution in t-1. On having the optional information, subjects rely on the actual information rather than on their beliefs. As a result, the adjustment is faster/more precise for subjects who access the optional information, compared to those who do not.

The literature has documented similar dynamic updating processes (Fehr and Gächter, 2000a; Sefton et al., 2007). In particular, our behavioral model builds on Bigoni and Suetens (2012), in the sense that we also model updates in subjects' contributions over time as a function of previous gaps to the previous group's average contribution. Nevertheless, our hypothesis goes beyond Bigoni and Suetens (2012). Specifically, we expect that, when information is not available, subjects use their beliefs about the other group members' contributions to adjust their own contributions. In this sense, subjects might not simply imitate observed behavior as a mindless rule of thumb. Since we elicit beliefs in every round, we can infer to what extent subjects base their contributions on beliefs. Also, since whether to have information about the group's average contribution is endogenous in our setting, we can infer to what extent the very fact of having the information is used to update the contribution beliefs.

The expected behavior in the dynamic updating process is not stated in terms of absolute contributions, but rather in terms of differences between subjects' own contribution and their expectations about the other group members' average contribution. We expect that subjects will seek to close the gap between the contributions of other group members and their own contribution. Such behavior could be explained by inequality averse preferences, such as Fehr and Schmidt's (1999), or conformist preferences, where subjects would dislike contributing both more or less than other subjects of their group. In case subjects do not have the optional information, they will rely their contribution decisions on their own beliefs about the other group members' contributions. In case subjects have the optional information, they will base their contribution

choices on the actual average contribution of their fellow group members, rather than on their previously formed beliefs about it.

We closely follow the specification used by Bigoni and Suetens (2012), modeling the individual change in contributions across periods as a function of the difference of the group's average and one's own contribution in the previous period. Similarly to Bigoni and Suetens (2012), we expect that whether subjects increase or decrease their contributions from period t-1 to t depends on whether their own contribution in t-1 was below or above the average contribution of the other group members in t-1 (in case the optional information is not available, the beliefs about them), respectively.

$$c_{it} - c_{it-1} = \alpha + \beta \Delta_{it-1} \tag{2}$$

with

$$\Delta_{it-1} = \frac{1}{4}\sum_{j=1}^{4} c_{jt-1} - c_{it-1} \tag{3}$$

That is, we expect $\beta$ in Eq. (2) (with $\Delta_{it-1}$ defined as in Eq. (3)) to have a statistically significant positive sign. We also expect that $\beta$ is of greater magnitude for subjects who had the optional information compared to those that did not. Note that, since $\beta$ is expected to be different from one, $c_{it-1}$ does not cancel out on both sides of the equation.

*Hypothesis 2b: information has a negative value, i.e., people pay not to have the information (information avoidance).*

Despite the argument outlined above, we also include a negative price treatment as a control to incorporate the possibility that subjects might also be willing to pay to avoid having the optional information. Traditional economic theory takes for granted that people have no reason to reject information that has a bearing on their future decisions, especially when it is costless. However, information avoidance exists in various domains (see Golman and Hagmann, 2010, for an excellent review). Studies show that subjects may actively choose to play ignorant in contexts involving health (Oster et al., 2013; Ganguly and Tasoff, 2016), financial investment (Karlsson et al., 2009; Sicherman et al., 2016) and task performance in real effort settings (Huck et al., 2017). If knowing bad news is worse than suspecting the possibility of bad news, people may choose to 'bury their heads in the sand' to deliberately avoid the information (Karlsson et al., 2009). Following this line of reasoning, if subjects perceive knowing about others' free riding on their own contributions to be worse than suspecting so, they would choose not to have the information even when doing so is costly. The negative price treatments control for this possibility.

*Hypothesis 3: more subjects having the optional information leads to a quicker unraveling of contribution in the opt-out treatments and the negative price treatments, compared to the opt-in treatments and the positive price treatments*

It is well established in the literature that people are conditional cooperators (e.g., Fehr and Gächter, 2000b; Fischbacher et al., 2001; Falk and Fischbacher, 2006; Fischbacher and Gächter, 2010). More importantly, such conditional cooperation is often imperfect, which means people only partially match others' contributions (Fischbacher and Gächter, 2010). It is the imperfect conditional cooperation that leads to unraveling of cooperation often observed in repeated public goods games.

Based on hypothesis 1 and the law of demand, we expect more subjects to have the optional information about others' contributions in the opt-out treatments and the negative price treatments, compared to the opt-in treatments and the positive price treatments. As a result, we expect unraveling of cooperation happens faster in the former, which yields our hypothesis 3.

## 3. Results

We begin by providing descriptive results of the overall sample. We then test the three hypotheses stated above. Finally, we provide additional results about contribution levels that are not directly linked to one of our hypotheses.

Table 3 presents summary statistics for the overall sample. We cannot reject the null hypothesis that randomization into treatments was successful in the sense that observable individual characteristics such as gender, age or studying Economics as a major did not vary across treatments (Kruskal-Wallis tests: all $p > 0.100$).[6] We collected 10 independent observations per treatment.

### 3.1. Testing hypotheses

*Result 1: in contrast to H1, the informational default (opt-in or opt-out) does not affect the likelihood of having the information.*

As shown in Table 4, having or not having the optional information as a default did not affect the proportion of subjects actually ending up with the optional information (rank-sum test: $p = 0.789$).[7] This finding is surprising, given the substantial

[6] Throughout this paper, unless stated otherwise, all reported $p$-values refer to two-sided non-parametric tests on group averages over all 10 rounds to account for non-independence of subject-level observations.

**Table 3**
Descriptive statistics on the sample.

| Treatment | N | Female | Age | Economics major | Years at university |
|---|---|---|---|---|---|
| PI | 10 | 0.50 (0.50) | 22.2 (4.3) | 0.05 (0.22) | 3.2 (1.8) |
| PO | 10 | 0.60 (0.49) | 22.9 (6.3) | 0.05 (0.22) | 2.8 (1.9) |
| ZI | 10 | 0.72 (0.45) | 23.6 (6.8) | 0.00 (0.00) | 3.4 (1.8) |
| ZO | 10 | 0.58 (0.49) | 22.2 (4.2) | 0.10 (0.30) | 3.3 (1.7) |
| NI | 10 | 0.62 (0.48) | 22.6 (6.7) | 0.03 (0.16) | 3.3 (1.8) |
| NO | 10 | 0.55 (0.50) | 22.6 (4.4) | 0.07 (0.26) | 3.3 (1.9) |

Notes: This table contains means (standard deviations at subject level in parentheses); for treatment abbreviations see Table 1; N: number of independent observations; age in years.

**Table 4**
Having information and individual contributions: descriptive summary.

| | Price of information | Information default | | | |
|---|---|---|---|---|---|
| | | Opt-in | | Opt-out | Total |
| **Having information** | **Positive** | 0.40 (0.12) | $\sim$ | 0.34 (0.15) | 0.37 (0.14) |
| | | $<^{***}$ | | $<^{***}$ | $<^{***}$ |
| | **Zero** | 0.87 (0.10) | $\sim$ | 0.90 (0.12) | 0.88 (0.11) |
| | | $<^{**}$ | | $\sim$ | $<^{**}$ |
| | **Negative** | 0.96 (0.06) | $\sim$ | 0.97 (0.04) | 0.96 (0.05) |
| | **Total** | 0.74 (0.27) | $\sim$ | 0.74 (0.31) | 0.74 (0.28) |
| **Individual contributions** | **Positive** | 7.17 (3.27) | $\sim$ | 6.41 (2.85) | 6.79 (3.01) |
| | | $\sim$ | | $\sim$ | $\sim$ |
| | **Zero** | 7.40 (3.11) | $\sim$ | 5.15 (3.92) | 6.28 (3.63) |
| | | $\sim$ | | $\sim$ | $\sim$ |
| | **Negative** | 7.23 (3.75) | $\sim$ | 6.56 (3.50) | 6.90 (3.55) |
| | **Total** | 7.27 (3.27) | $\sim$ | 6.04 (3.39) | 6.65 (3.36) |

Notes: This table contains means (standard deviations at group level in parentheses); levels of significance for t-tests: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$; non-parametric rank-sum tests yield qualitatively similar significance levels.

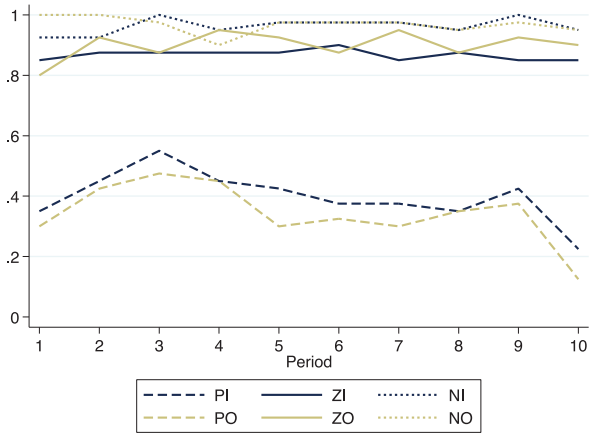


**Fig. 1.** Proportion of subjects who have the optional information.

and robust effects of defaults in a variety of lab and field experimental settings, and we will come back to this result in Section 4.

*Result 2: in support of H2a, subjects ascribe a positive value to information. our results do not support H2b, which instead suggests that people pay to avoid information.*

Fig. 1 suggests that subjects on average ascribe a positive value to having the optional information. In treatments where following the monetary incentive is aligned with having the optional information (NI and NO), or where the price for in-

[7] The null influence of the information default is robust across price levels (PI vs. PO: $p = 0.384$, ZI vs. ZO: $p = 0.359$, NI vs. NO: $p = 0.935$).

**Table 5**
Likelihood of having the optional information and belief about how many other group members will have the optional information.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Having info | Having info | Belief info | Belief info |
| Default no info | −0.00443 | −0.0165 | 0.283*** | 0.283*** |
| | (0.0328) | (0.0817) | (0.109) | (0.108) |
| Price | −0.295*** | −0.315*** | −0.763*** | −0.836*** |
| | (0.0223) | (0.0429) | (0.0668) | (0.0935) |
| Default no info x price | | 0.0392 | | 0.146 |
| | | (0.0454) | | (0.132) |
| Constant | | | 2.031*** | 2.031*** |
| | | | (0.0771) | (0.0763) |
| Observations | 2400 | 2400 | 2400 | 2400 |
| Log. Likelihood | −749.3 | −748.9 | −2688.3 | −2687.7 |
| Chi-squared | 131.4 | 131.4 | 137.3 | 141.4 |

*Notes:* Columns 1 and 2 contain marginal effects of logit estimations on the likelihood of having the information. Columns 3 and 4 contain coefficients of linear models on information belief, i.e., the number of how many other group members will have the information. All models were estimated using multi-level error clustering (subjects nested in groups); controlling for additional correlates such as age, gender and whether the field of study was Economics did not qualitatively change the picture (see Table OA3.4 in the Online Appendix); levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

formation is zero (ZI and ZO), in over 92% of all cases subjects got the optional information. This high share is not significantly different from always having the optional information (sign tests, all $p = 1.000$). However, the proportion is substantially lower for treatments PI and PO (34% and 40%, respectively),[8] where having the information comes at a positive price. Although subjects seem to follow the monetary incentives to some extent, a significant proportion attach a positive value to the optional information. That is, they are willing to trade off additional earnings for information about their co-players' contributions. In case subjects were only interested in money, but not in information, the proportion of people seeking information against their monetary interest should have dropped to zero, which it clearly did not (sign tests, both $p < 0.001$).

Online appendix OA.3.1 contains an analysis of information types, based on whether subjects seek information infrequently (0–2 times), more frequently (3–6 times) or very frequently (7–10 times). We find that in treatments NI, NO, ZI and ZO, close to everyone (94% of subjects) can be classified as seeking information very frequently. Instead, in the PI and PO treatments there is close to a uniform distribution of information types.

Table 5 (models 1 and 2) confirms the findings of result 2. Except for the expected period 10 end game effect in treatments PI and PO, there is no time trend in having the information.[9]

Somewhat counterintuitively, subjects believe that more group members will have the optional information when the default is not having the information (see Table 5, models 3 and 4), a point we refer to as a *secondary information default effect*.[10]

Fig. 2 depicts subjects' beliefs about how many of their co-players will have the optional information. The beliefs are generally in line with actual observations (i.e., higher beliefs about having information when doing so is aligned with monetary incentives and lower beliefs when monetary incentives are not aligned, rank-sum test ZI, ZO, NI & NO vs. PI & PO: $p < 0.001$). However, there exists a *secondary information default effect*. When information is costly, and when not having the optional information is the default, the believed number of other group members having the optional information is significantly higher than that in the case where having the information is the default (rank-sum test PI vs. PO: $p = 0.001$).

[8] The numbers of subjects that have the information are significantly different between treatments where having the information goes against their financial interests (PI, PO) and where this is not the case (ZI, ZO, NI, NO; rank-sum test: $p < 0.001$). However, we observe no significant differences related to the information default (PI vs. PO, ZI vs. ZO, NI vs. NO: all pairwise rank-sum tests: $p > 0.359$). Besides a clear endgame effect in period 10, there is no time-trend observable in periods 1-9 (z-tests: $p = 0.767$).

[9] Adding *period* to the regressions 1 and 2 in Table 5 results in an insignificant coefficient for estimations on periods 1-9 (both $p = 0.767$). Only when including the full sample (periods 1-10), the coefficient for period turns out to be significant, statistically supporting the visually apparent end-game-effect in Fig. 1 (both $p < 0.014$). A qualitatively similar pattern of none-existing period effects can be observed for descriptive information beliefs displayed in Fig. 2. Specifically, we find a close-to-zero (yet significant) positive trend (all $p < 0.013$).

[10] We return to the secondary information effect in the discussion section. As indicated in Section 2, we also elicited normative information beliefs, i.e., the number of how many other group members *should* have the information. These beliefs are highly correlated with the descriptive beliefs (Pearson $\rho = 0.729$, $p < 0.001$, see also Table OA3.9 in the Online Appendix for regression results). Details about the specific distributions over time for all treatments are provided in Figs. OA3.2 and OA3.3 in the online appendix.

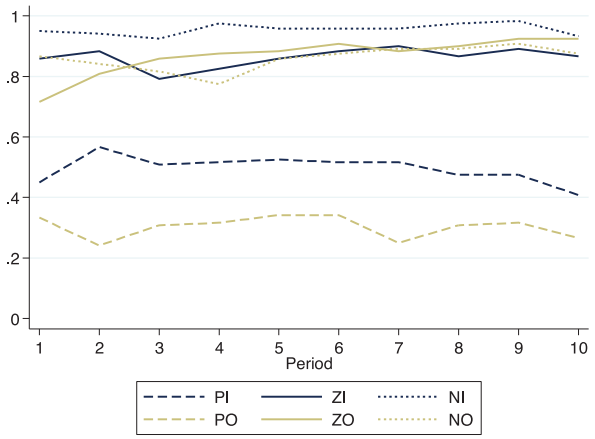


**Fig. 2.** Belief about how many other group members will have the optional information.

Interestingly, this secondary information default effect is only present when information is costly (rank-sum tests ZI vs. ZO: $p = 0.404$, NI vs. NO: $p = 0.111$).[11]

*Result 3: subjects adjust their contributions towards the previous period's average group contributions. when they do not have the information, they adjust their contributions toward their descriptive beliefs*[12] *about the previous period's average group contributions*

Table 6 estimates the change of a subject's contributions across periods (contribution in period t minus contribution in period t-1) as a function of $\Delta(t\text{-}1)$, the difference of the average group contribution (including a subject's own contribution) in the previous period and the subject's own contribution in the previous period (see Eq. (2) in Section 2). This is estimated based either on the actual average contribution of the group in the previous period (if subjects had the information) or (when they did not) on their belief on the average contribution of the group in the previous period. We find that the difference from the group's average contribution in the previous period is a strong predictor for the change of contributions over time. The positive regression coefficient of model 1 means that subjects significantly increased their contributions if they contributed less than the (believed) average in the previous period and that they significantly reduced their contributions when they contributed more than the (believed) average. Thus, subjects tended to 'imitate the group average'.

Model 2 controls for whether subjects had the optional information in the previous period. By controlling for this, the coefficient of $\Delta(t\text{-}1)$ can then be interpreted as the effect of the belief about the average group contribution in the previous period. We still find a strong imitation effect in terms of the believed difference from the group's average in the previous period. This is consistent with subjects not relying on mindless imitation of others' behavior, but rather – at least to some degree – on social or conformist preferences. Model 2 also shows that subjects more strongly imitate the group's average contribution of the previous period when they actually had the optional information in comparison to when they did not have it. Since having the information has a precision that a simple belief does not have, this is to be expected.

However, since whether or not to have the optional information is endogenous to the subject, we cannot interpret the positive coefficient of the term $\Delta$ *(t-1) x having info (t-1)* as necessarily entailing a causal effect of having the information on contribution behavior. To address this, we use a positive price of information as an exogenous proxy for not having information. This is reasonable because optional information seeking is negatively correlated with the price of information (Result 2), and, as a treatment variable, the price of information is exogenous to subjects' unobserved characteristics that might affect both information choice and contribution behavior. The variable positive price satisfies the conditions of standard instrumental variable approach: First, positive price directly affects the likelihood of having information (see Result 2). Second, positive price does not directly affect the change in contributions across periods (exclusion criterion, z-test, $p = 0.610$).

Because optional information seeking is negatively correlated with the price of information, we expect the coefficient of $\Delta$ *(t-1) x pos. price (t-1)* to be negative, and this is indeed the case (Model 3). The net effect of $\Delta$ *(t-1)* remains positive, as in Model 2.

Model 4 shows that the effect of information on the previous period's group average asymmetrically depends on whether the own contribution in t-1 was above or below the group average in t-1. Having information leads to stronger adjustments towards the group average when subjects learned that they contributed more than the group average (disadvantageous inequality in outcomes) than when they contributed less than the group average (advantageous inequality in outcomes).

[11] In contrast to the observed information behavior, there exists no substantial endgame effect for information beliefs. In fact, across all periods, there even exists a small yet significant upward trend (z-tests: $p < 0.001$).

[12] Wherever the word 'descriptive' is not explicitly mentioned, we are referring to descriptive beliefs throughout the manuscript unless otherwise stated.

**Table 6**
Imitation of previous period's average group contributions.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Contribution (t) - (t-1) | | | | |
| Δ (t-1) | 0.552*** | 0.394*** | 0.588*** | 0.393*** | 0.424*** |
| | (0.0212) | (0.0419) | (0.0260) | (0.0475) | (0.0579) |
| Having info (t-1) | | −0.336* | | 0.472** | −0.338* |
| | | (0.192) | | (0.217) | (0.190) |
| Δ (t-1) x having info (t-1) | | 0.200*** | | 0.448*** | 0.181*** |
| | | (0.0474) | | (0.0559) | (0.0658) |
| Pos. price | | | 0.0906 | | |
| | | | (0.198) | | |
| Δ (t-1) x pos. price | | | −0.122*** | | |
| | | | (0.0445) | | |
| Pos. Δ (t-1) | | | | 0.132 | |
| | | | | (0.226) | |
| Δ (t-1) x having info (t-1) x pos. Δ (t-1) | | | | −0.554*** | |
| | | | | (0.0683) | |
| Default no info | | | | | 0.167 |
| | | | | | (0.182) |
| Default no info x Δ (t-1) | | | | | −0.0815 |
| | | | | | (0.0834) |
| Default no info x Δ (t-1) x hav. info (t-1) | | | | | 0.0383 |
| | | | | | (0.0945) |
| Constant | −0.303*** | −0.0471 | −0.334*** | −0.148 | −0.129 |
| | (0.0946) | (0.171) | (0.114) | (0.212) | (0.191) |
| Observations | 2160 | 2160 | 2160 | 2160 | 2160 |
| Log. Likelihood | −5754.1 | −5743.9 | −5750.3 | −5711.6 | −5742.7 |
| Chi-squared | 676.6 | 697.5 | 678.9 | 811.0 | 686.3 |

Notes: Columns 1–5 contain coefficients from linear models on subjects' contribution adjustments as specified in equation (2); standard errors in parentheses with multi-level clustering: subjects nested in groups; controlling for additional correlates such as age, gender and whether the field of study was Economics did not qualitatively change the picture (see Table OA3.5 in the online appendix); levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Model 4 shows that this asymmetry in behavioral reactions is statistically significant (coefficient of *Δ (t-1) x having info (t-1) x pos. Δ (t-1)*). The fact that people dislike both free-riding on others and being free-ridden on by others, but that they dislike the latter even more than former, is in line with preference-based explanations of imitation postulating such an asymmetry (e.g., Fehr and Schmidt, 1999). Model (5) suggests information defaults do not directly affect imitation, albeit they may affect information seeking behavior and/or related beliefs. However, as seeking information is endogenous, it is unclear what can be attributed to the effect of information relative to being the type of person who seeks information.

### 3.2. Information defaults and contribution

*Result 4: in partial support of H3, contributions decrease over time, and much more so in treatments where subjects received the optional information as a default*

Fig. 3 shows that contributions decline substantially over time. This decline amounts to an average −0.28 units per period across all treatments and prevails when controlling for level differences between treatments (see Table 7, model 2). However, model 3 in Table 7 shows that, when not receiving the optional information is the default (PI, ZI and NI), this time trend is significantly less steep than that in treatments where subjects received the optional information as a default (PO, ZO and NO, see *Period x Default no info*). In other words, we find slower unraveling of cooperation in treatments where the default is not having information about the others' contributions.[13] This is particularly interesting in light of Result 1, because the slower unraveling of cooperation under no information defaults happens despite the fact that the propensity to have the optional information did not vary by information default. We did not find such quicker unraveling of cooperation in negative price treatments compared to positive price treatments.

Adding the descriptive belief about contributions ("How many ECU will your co-participants contribute on average to the project this round?") to the regressions makes the coefficient of the interaction term *Period x Default no info* insignificant and close to 0 (see Table 7, models 4 and 5), as well as leading to an improvement in fit (in terms of log likelihood).[14]

[13] Furthermore, contribution behavior is not correlated with the propensity of having the optional information. For more details see Table OA3.3 in the online appendix.

[14] Normative contribution beliefs are highly correlated with the descriptive beliefs (Pearson $\rho = 0.590$, $p < 0.001$). They are on average about 2 ECU above descriptive beliefs, irrespective of the treatment. The same qualitative results can be found using normative beliefs. See section OA.3.4 in the online appendix for details.

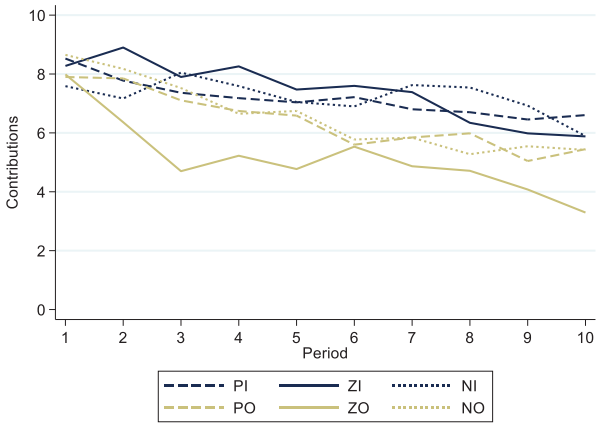


**Fig. 3.** Average contributions by period and treatment.

**Table 7**
Individual contribution.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Default no info | 1.225 | 1.225 | 0.458 | 0.280 | 0.625 |
| | (0.846) | (0.846) | (0.890) | (0.471) | (0.599) |
| Price | −0.0541 | −0.0541 | −0.260 | −0.335 | −0.492 |
| | (0.518) | (0.518) | (0.771) | (0.408) | (0.470) |
| Period | | −0.277*** | −0.347*** | −0.0930*** | −0.0866*** |
| | | (0.0252) | (0.0356) | (0.0312) | (0.0321) |
| Default no info x price | | | 0.414 | 0.310 | 0.327 |
| | | | (1.090) | (0.576) | (0.579) |
| Period x default no info | | | 0.139*** | 0.0280 | 0.0170 |
| | | | (0.0503) | (0.0429) | (0.0445) |
| Period x price | | | 0.0336 | 0.000193 | 0.00532 |
| | | | (0.0436) | (0.0370) | (0.0381) |
| Period x default no info x price | | | −0.0677 | −0.0517 | −0.0542 |
| | | | (0.0617) | (0.0523) | (0.0526) |
| Contribution belief | | | | 0.681*** | 0.699*** |
| | | | | (0.0207) | (0.0296) |
| Contribution belief x default no info | | | | | −0.0393 |
| | | | | | (0.0418) |
| Contribution belief x price | | | | | 0.0173 |
| | | | | | (0.0257) |
| Constant | 6.042*** | 7.564*** | 7.948*** | 2.048*** | 1.885*** |
| | (0.598) | (0.614) | (0.629) | (0.378) | (0.421) |
| Observations | 2400 | 2400 | 2400 | 2400 | 2400 |
| Log. likelihood | −6786.2 | −6727.6 | −6723.1 | −6306.2 | −6305.6 |
| Chi-squared | 2.108 | 122.6 | 132.0 | 1268.1 | 1267.9 |

*Notes:* Reference treatment in all columns: default=info, price=0; Contribution belief refers to descriptive contribution beliefs; all columns contain coefficients of linear models, each with multi-level error clustering: subjects nested in groups; errors in parentheses; controlling for additional correlates such as age, gender and whether the field of study was Economics did not qualitatively change the picture (see Table OA3.6 in the Online Appendix); see the Table OA3.7 in the Online Appendix for regression results on the beliefs about average contributions of others; levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## 4. Discussion and conclusions

Under a default of no information, public good contribution decreases more slowly than under a default of information. Contribution beliefs match the slower contribution unraveling. Nevertheless, the channel through which the slower contribution unraveling operates is not entirely clear, as the default itself does not affect the likelihood of having information. That is, there is no *primary* information default effect: no effects of defaults on information seeking behavior. In any case, while information has a positive value and is sought less if positively priced, the likelihood of having information does not directly affect public good contribution or its unraveling.

We find a somewhat counterintuitive *secondary* information default: that is, the average stated belief about the likelihood of others having the information is greater under the no-information default. The effect is driven by the positive price treatments where the default is no information. This is not surprising since in the zero and negative price treatments almost everyone in any case pretty much automatically expects everyone else to get the information. While it may be tempting to

see the secondary information default as an explanation for why the no-information default is better for cooperation, we could not identify any obvious channel through which it successfully does so. Further research is clearly needed.

The lack of a direct connection between likelihood of having the information and public good contribution is consistent with imitation not being driven by a mindless rule of thumb by which, when subjects see what the others have done, they tend to copy it. Rather, even in the absence of information, on average subjects rely on their beliefs about the other players' contribution behavior in the previous period to play a conditionally cooperative strategy (see also Croson, 2007). This is consistent with playing according to social preferences such as inequality aversion, or other kind of conformist preferences. The adjustment towards the group average contribution is stronger with disadvantageous than with advantageous inequality in outcomes (as in Fehr and Schmidt, 1999). Bosch-Domènech and Vriend (2003) also found evidence of behavioral imitation in complex and information-absent situations, albeit in a different setting,

An interesting avenue for future research would be to extend the study of information defaults and endogenous information architecture to settings other than public good games. Another interesting route would be to look at settings with provision of detailed (e.g., individual) as opposed to aggregate (e.g., average contribution) information. The next step would then be to develop a theory of optimal information architecture which takes account of the behavioral features of our data as well as of any such experimental extensions.

The policy and management implication we can draw from this study is that not only more information is not always better than less (Carpenter, 2004; Sonntag and Zizzo, 2019), but, even where information ends up being the same, setting the default for whether (say) employees receive information or not matters. Specifically, our experimental results imply that setting a default of no information is the better policy and managerial choice. This is because a default of no information about other players' past public good contributions is more likely to engender slower cooperation unraveling than when the default is information.

## Declaration of Competing Interest

We declare that we have no financial and personal relationships with other people or organizations that could inappropriately influence this work. Jia Liu, Axel Sonntag and Daniel John Zizzo

## Appendix

### *A.1. Experimental instructions*

The full set of instructions is available in the Online Appendix (OA.1).

### *A.2. Screenshots of the feedback screen that is shown at the end of each period*

Fig. A2.1 and A2.2 present the relevant parts of the screen regarding the information displayed at the end of each period for the treatment PI. The corresponding full screenshots of the parts displayed below are available in the Online Appendix (OA.2). For the feedback screens of the other treatments, the wording was adapted accordingly. The exact wording is available in the Online Appendix.

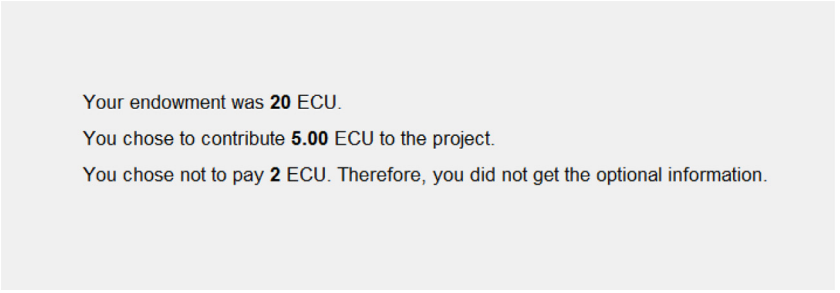


**Fig. A2.1.** Feedback information in treatment PI in case of NOT having the optional information.

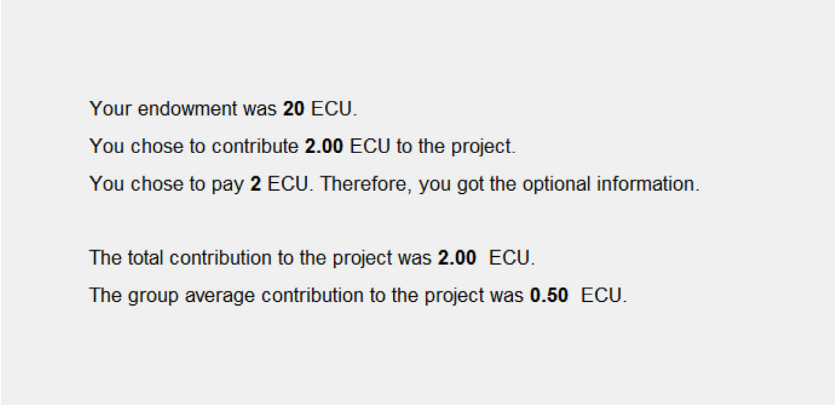


**Fig. A2.2.** Feedback information in treatment PI in case of having the optional information.

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