



**HAL**  
open science

# Feedback and cooperation: An Experiment in sorting behaviour

Noémi Berlin, Mamadou Gueye, Stéphanie Monjon

► **To cite this version:**

Noémi Berlin, Mamadou Gueye, Stéphanie Monjon. Feedback and cooperation: An Experiment in sorting behaviour. 2024. hal-04843446

**HAL Id: hal-04843446**

**<https://hal.science/hal-04843446v1>**

Preprint submitted on 19 Dec 2024

**HAL** is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

# Feedback and cooperation: An Experiment in sorting behaviour\*

Noémi Berlin<sup>✉†</sup>      Mamadou Gueye<sup>‡</sup>      Stéphanie Monjon<sup>§</sup>

December 17, 2024

## Abstract

*This is the accepted version of the manuscript for publication in *Econological Economics**

In this paper, we use a laboratory experiment to analyze the effect of information provision (feedback) on individual sorting behaviour. Effective sorting requires both quantity and quality, yet increasing quantity may reduce quality due to the higher risk of contamination. We conduct a collective sorting behaviour experiment consisting of a two-stage coordination game in which two subjects are paired and then individually decide whether or not to participate in a collective sorting task. The performance achieved depends on the quantity and quality of sorting, and the payoff depends on the decision and performance of both subjects in the task. Information about the subject's own past performance, and information about the partner's past performance, are included as feedback treatments. Using a between-subjects experimental design, we find that the feedback type has very different effects on participation, performance and coordination (defined as both subjects succeeding in the sorting task). Only feedback about one's own performance leads to better performance and more coordination. Although this experiment is not contextualized, the results provide useful pointers for waste sorting policies.

---

\*The paper greatly benefited from discussions with Jean Christophe Vergnaud, Tarek Jaber Lopez and Maxim Frolov. We are grateful to the participants of the Behavioral Economics Working Group at the University of Paris Nanterre, of the LEMNA seminar in Nantes, of the ASFEE 2022, IMEBESS 2023, JMA 2023, FAERE 2023 conferences and Climate Economics Chair's Friday Lunch Meeting for useful comments and suggestions. Also, we are grateful to two anonymous reviewers for their comments and suggestions which helped us to improve considerably the article. This work was supported by the ADEME - MECÉE research project. We thank the FΓκdΓκρratiōn S2CH - CNRS - UP1 - UPN - PSE for their technical support for the experiment.

<sup>†</sup>CNRS, EconomiX, University Paris Nanterre, **Corresponding author:** noemi.berlin@parisnanterre.fr

<sup>‡</sup>Fondation Idea, Luxembourg, mamadou.gueye@fondation-idea.lu

<sup>§</sup>CGEMP, LEDa, University Paris Dauphine-PSL Research University and Climate Economics Chair, Paris, France, stephanie.monjon@dauphine.psl.eu,

**Keywords:** experiment, sorting task, cooperation, informational feedback

**JEL codes:** C71, C92, D83, Q53

# 1 Introduction

More than 2 billion tons of municipal solid waste are produced annually, and this amount is expected to rise to 3.40 billion tonnes by 2050.<sup>1</sup> Waste recycling can thus generate many benefits for society. First, it can significantly reduce demand for raw materials by replacing them with used materials, thus lessening the amount of remaining waste (MacArthur et al., 2013; Zomeren and Tosti, 2017). Second, recycling can also reduce the amount of energy consumed to produce new goods, and the related greenhouse gas emissions (Kaza et al., 2018). Yet, several decades of waste management policies designed to encourage recycling have had disappointing results, particularly because of poor waste sorting performance. For instance, the European Environment Agency emphasizes that there has been little progress in the overall recycling rate in the EU over the last decade: it remains below 50%, leading to the disposal of the majority of generated waste in landfills and incineration plants.<sup>2</sup> The French Court of Accounts (cour des comptes, 2017) explains the low quantity and quality of sorted materials for two reasons: insufficient participation in waste sorting and a lack of knowledge about how and which materials to sort, leading to low quality (Tonini et al., 2022). As stressed by Roithner and Rechberger (2020), the generation of low quality secondary materials causes material losses.

To increase the quantities of recyclable materials collected, more people must engage with the waste sorting process. Early separation of certain materials when waste is first disposed of, at home, at work, or in public spaces, is crucial to limit losses of reusable materials and recycling costs. When all waste items are mixed together, physical or chemical processing to extract reusable materials becomes much more difficult and, in some cases, impossible.<sup>3</sup> This means that an increase in the volume of waste thrown into sorting bins can negatively impact the quality of the materials collected, due to the higher risk of contamination. Contamination may occur for instance when soiled papers are mixed up with clean papers in the same bin. The public's participation in preliminary sorting, but also the way they perform that sorting, therefore directly impact the final quantity and cost of the secondary materials collected. Quantity and quality levels are thus interlinked and together determine the success of waste sorting. When an individual decides to participate in waste sorting but fails to do so properly, the quality of the materials collected is affected. A "poor sorting performance" by (even a few) individuals consequently impacts the final outcome for everyone, and makes the result

---

<sup>1</sup>[https://datatopics.worldbank.org/what-a-waste/trends\\_in\\_solid\\_waste\\_management.html](https://datatopics.worldbank.org/what-a-waste/trends_in_solid_waste_management.html) (accessible on July 16, 2024)

<sup>2</sup><https://www.eea.europa.eu/en/analysis/indicators/waste-recycling-in-europe> (accessible on July 16, 2024).

<sup>3</sup><https://www.recyclingtoday.com/news/wasteexpo-2019-dual-stream-vs-single-stream/> or <https://ecology.wa.gov/Waste-Toxics/Reducing-recycling-waste/Strategic-policy-and-planning/Contamination-reduction> (accessible on July 16, 2024)

of the sorting activity uncertain. Coordination for successful sorting is therefore important to reach optimal quantity and quality in waste sorting.

This paper presents the results from a laboratory experiment studying coordination between individuals. Although inspired by waste sorting behavior, we chose to conduct an uncontextualized experiment where we propose a new real-effort task and where we test whether providing different types of performance feedback improves coordination: Does feedback improve the level of participation and performance in a collective task? Does the type of feedback matter?

Although the literature on the impact of feedback on coordination is relatively extensive, none of the frameworks studied correspond to the one we are interested in. We analyze individuals' decisions in a task which requires coordination of sorting behavior between two players matched as a pair to succeed. The experiment is based on a two-stage, two-player game, repeated eight times, during which participants perform a real-effort task requiring them to sort letters from three alphabets: Latin, Greek and Cyrillic. In the first stage of the game, each subject chooses whether or not to participate in the task. For the second stage, three cases are possible: *(i)* both choose to opt out; *(ii)* only one decides to participate; and *(iii)* both decide to opt in. While a subject who decides not to take part in the sorting task receives a payoff that does not depend on the choice and the performance of the other player, the payoffs of a subject who decides to participate in the sorting task will depend not only on their own decisions and performance, but also on the decisions and performance of their matched partner. The payoff matrix is designed such that both subjects are rewarded for participating in the sorting activity only if they both reach a certain performance level which is a linear combination of quantity and quality of their sorting. Failure by one subject compromises both subjects' payoffs and leads to a situation where it is in the interest of both not to participate in the task. The subjects' participation decisions depend on their beliefs about their own expected performance at the sorting task, as well as their beliefs about their partner's intention to participate and expected performance. We explore whether providing feedback about a subject's own or their partner's past performances modifies their beliefs, their participation decisions, their performance, and finally, the achievement of coordination (i.e. both subjects perform the task well).

By taking into account heterogeneous knowledge regarding the different alphabets and incorporating both quantity and quality dimensions, our paper departs from the usual representation of waste sorting, which is typically reduced to either participating or not (Xu et al., 2018). To the best of our knowledge, this study is the first to experimentally mimic (in a context-free setting) waste sorting behavior represented by a coordination game, and test two types of feedback: feedback about a subject's own past performances, or about their partner's past performances.

We chose to conduct an uncontextualized experiment for several reasons. First, this limits potential experimenter demand effect. If we had framed the experiment with waste sorting in mind, participants' behavior could have been biased towards waste sorting as they might think that is what the experimenter wants. Second, the findings can then be applied to other

pro-environmental behaviors or other activities that require greater willingness to engage for subsequent success and where the features of performance and coordination are relevant. Examples include keeping a public place clean, acting to reduce pollution peaks, reducing carbon emission, conserving animals, or more broadly, activities that require coordination to reach optimal situations such as maintaining security in a digital network, avoiding a power blackout or observing a cease-fire (we provide a broader discussion about external validity in Section 6).

Our main finding is the contrasting outcomes of the feedback treatments: feedback on the partner’s past performance tends to dissuade subjects from participating in the task, while feedback on the subject’s own performance leads to a better performance and increases coordination. Moreover, results show an asymmetrical effect of the feedback on the participants’ beliefs suggesting that providing absolute feedback on one’s own performance is more efficient in belief updating compared to absolute feedback about others’ performance. Although this experiment is not contextualized, the results provide useful pointers for waste sorting policies.

The rest of this article is structured as follows. Section 2 reviews the related literature. Section 3 presents the experimental design. Section 4 presents descriptive statistics on demographics and on the different variables of interest (beliefs, participation, performance). Section 5 describes experimental results from regressions testing our hypothesis. Section 6 discusses the experimental results and concludes.

## 2 Literature review

Our work is particularly grounded in the economic literature considering the link between coordination and information. This stream of literature is an offshoot of the social psychology literature on understanding individual motivations within a group (Karau and Williams, 2001; Bortolotti et al., 2016) and the impact of individual performance on the collective (Hüffmeier et al., 2012).

### Coordination

Coordination issues arise from the need to integrate several interdependent activities. Weakest-link, or minimum-effort, games can represent numerous coordination situations such as the production of public goods, computer security, or teams in the workplace (Cooper, 1999). In these games, an action by one player is generally interpreted as an effort made for a joint production activity, for which the output is determined by the minimum effort chosen (Cooper and Weber, 2020). Players decide on their effort level based on their expectations of other players’ actions (Heinemann et al., 2009). Feldhaus et al. (2020) consider that the individual players’ actions are strategic complements, because higher payoffs require greater efforts by all. The minimum-effort game is the subject of a large body of experimental literature dealing with coordination difficulties on the Pareto-efficient outcome (Cartwright,

2018).

Coordination games generally feature multiple equilibria. Their minimum-effort problems, in particular, are characterized by a multiplicity of Pareto-rankable strict Nash equilibria (Van Huyck et al., 1990). Interestingly, Riedl et al. (2016) propose a real-effort task embedding a weakest-link mechanism, and find that people are unable to coordinate on the efficient equilibrium. “Coordination failure” does not mean only an absence of coordination, but can also refer to a situation where the coordination takes place on an inefficient equilibrium (Riechmann and Weimann, 2008). The Pareto-dominant equilibrium is generally based on the highest effort, which involves a risk of reduced payoff in the event of low effort by some individuals. A less efficient equilibrium is based on a secure action that is independent of other players’ actions. Consequently, individuals face a trade-off between efficiency and risk.

In a laboratory experiment based on a minimum-effort game, Riechmann and Weimann (2008) show that coordination failure mainly relates to the prevalence of strategic uncertainty. This is defined as the uncertainty arising from not knowing how other individuals will decide (Penta and Zuazo-Garin, 2022). Cooper and Weber (2020) explain the concept by describing a situation where some players consider it too risky to select the payoff-dominant action. Feldhaus et al. (2020) characterize equilibria according to their degree of risk: higher benefits are associated with larger losses from miscoordination. The Pareto-dominant equilibrium involves the highest risk, while individual payoffs in the least efficient equilibrium are independent of other people’s actions and thus secure.

Strategic uncertainty can emerge if subjects think there is even a small probability of dealing with an “irrational” partner (Feltovich and Swierzbinski, 2011)<sup>4</sup>. Each individual has an incentive to coordinate with high effort for greater individual and collective welfare, but may face considerable risk due to a single “trembling” player (Riedl et al., 2016): this can be enough to cause substantial losses for all.

## Information feedback to reach coordination

The literature has extensively analyzed the means of improving performance or achieving coordination. By providing individuals with information about the consequences of past decisions, feedback can constitute an interesting strategy for enhancing learning and increasing performance. This lever is particularly interesting in the context from which we draw our inspiration.

The questions of whether and how feedback promotes efficient behavior (not only coordination) have been studied in a wide range of games and experimental settings.

Indeed, feedback can increase players’ motivation and engagement, particularly in situations where people are likely to condition their own behaviour on what they expect others to do. In a dictator game, Krupka and Weber (2009) find that telling individuals what other players actually decide leads to more pro-social behavior, even when most subjects do not

---

<sup>4</sup>In Feltovich and Swierzbinski (2011)’s paper, the term “irrational” designates players who deviate from the Nash equilibrium.

think the others are generous, or when they observe that most others are behaving selfishly. The effect operates through the change in the individuals' beliefs: observing more people behaving pro-socially produces more pro-social behaviour. Kaplan et al. (2018) examine how sequential moves improve outcome efficiency. The possibility of waiting to be informed of the move chosen by another player provides better information on that subject's performance, and this information leads to better coordination.

In a social dilemma game, Engel et al. (2021) experimentally study how information about the past cooperativeness of other groups influences cooperation. They find that cooperation depends on pre-play information, which leads to a change in initial beliefs.

A substantial part of the literature focuses on information provision in a more contextualised framework: energy or water conservation. Andor and Fels (2018) review behavioral interventions concerning energy and water consumption and notably how social comparison, by providing information about others' consumption, may reduce one's own energy consumption. Karlin et al. (2015) focus specifically on the effects of feedback on energy conservation in a meta-analysis. They conclude that, despite the substantial volume of research examining whether feedback is effective, there has been little investigation into the more nuanced questions of how it works best.

Research on learning suggests that the impact of experience on decision-making behavior varies depending on the type of feedback received. Casal et al. (2017) present laboratory evidence that a change in feedback may enhance performance: they study how different types of feedback can enhance awareness of behavioral consequences and stimulates exploration. The feedback involves aspects such as social information and framing effects. In the field of education science, Bandiera et al. (2015) analyse whether providing university students with feedback on their past exam performance affects their future exam performance. In a natural experiment, they find that the provision of feedback has a positive effect on students' subsequent test scores. The information refers to students' course marks after all the exams have been taken and graded and it is given before students decide to exert effort to complete an essay.

In coordination games, the questions of how to effectively motivate individuals and enhance their participation and performance are critical issues. It has long been recognized that coordination strongly depends on initial beliefs (Van Huyck et al., 1990). In a survey of recent advances in experimental coordination games, Cooper and Weber (2020) emphasize the central role of past behaviors in determining beliefs. The provision of feedback affects individuals' beliefs and may prevent an inefficient equilibrium in minimum-effort games.

In a repeated symmetric coordination game, Berninghaus and Ehrhart (2001) find that an individual's decisions are strongly dependent on feedback about partners' choices in previous periods. In a minimum-effort game, they study three treatments: at each period, the subjects receive information about either the group minimum effort, the distribution of the players' strategy choices, or each opponent player's individual strategy choice. The least risky strategy, corresponding to low effort, is frequently chosen in the first treatment, but there is a prevalence of choosing the Pareto-dominant strategy when information is given

about the other players' choices. This indicates that players' decisions depend on feedback about what others play: when they receive more comprehensive information about their opponents' previous strategy choices, coordination failure can be avoided.

The effects of performance feedback on worker effort have also been extensively analyzed in both state-effort and real-effort experiments. Motivated by coordination failure issues in organizations, Brandts and Cooper (2006) investigate how overcoming and sustaining coordination depend on individuals' ability to observe others' choices. They use an experimental setting, where four "employees" of a fictitious "firm" must choose their effort level in a sequence of rounds. In each round, the payoffs depend on the minimum effort level chosen by the four players and the marginal payoff from increasing the minimum effort is determined by an exogenous "bonus rate". The authors consider two scenarios with quite contrasting bonus rates evolution to study incentives to coordinate: in the first scenario, bonus rates increase over time, while they decrease in the second. For each of these scenarios, two feedback schemes are considered: either everyone can observe all choices made by others ("full feedback"), or participants only see the minimum effort level chosen ("limited feedback"). Full feedback significantly enhances firms' ability to overcome past coordination failure when bonus rates increase over time. However, it has little effect on maintaining successful coordination when the bonus rate decreases over time.

In a lab experiment using a real-effort task, Gerhards and Siemer (2016) find that public and private feedback lead to better performance compared to a control group with no feedback. In the private treatment, a subject is privately informed about being (or not) the best performer in her group. In the public treatment, the best performer is announced to everybody. Gerhards and Siemer (2016) also investigate how the nature of the task affects the effectiveness of the feedback: a boring, compulsory real-world task and a more interesting and esteemed task. For the latter task, the outcome depends on the effort provided but also on the ability of the player. Their results show that public announcement of the best performer in a competitive environment does not produce higher incentives than if the information is given privately, but both treatments increase performance compared to the control group. Moreover, in the boring task, the positive treatment effect is driven by subjects with a high preference for competition. This is not the case for the other task.

Banerjee et al. (2014) present a laboratory experiment that uses a context-free design to explore the performance of the "agglomeration bonus" subsidy in achieving the socially optimal land management configuration in a local network environment. The information available to subjects varies and the strategic setting is unfavourable for efficient coordination. Using a psychological motivator, the authors implement information treatments that provide knowledge about the choices and payoffs of subjects' closest indirect neighbours, and show that receiving information about what their neighbours does facilitate coordination.

In a nutshell, numerous papers in the literature conclude that when a coordination failure is due to strategic uncertainty, providing feedback may be one remedy. Nevertheless, we did not find a setting close to the one we are interested in. In particular, when feedback is given on the actions or performance of others, the analysis often focuses on either a competitive en-



vironment or social information, which does not correspond to the context we want to mimic.

## Our contributions

Although our study is related to those cited above, it departs in several respects. First, we propose a new sorting real-effort task in a game that takes into account the interdependence between quantity and quality. There have been a number of real-effort tasks used in experiments for different purposes. Charness et al. (2018) present a review of different real-effort tasks. Related to our topic, we wanted a task to mimic as closely as possible the behavior of waste sorting, which implies heterogeneous knowledge among participants regarding the items to be sorted (in our case, the different alphabets that can be more or less familiar to the participants) and incorporates both quantity and quality dimensions. Although the last two features could be easily achieved with the sorting coins task from Bortolotti et al. (2016), the first dimension needed to mimic waste sorting would have been missing. Using letters from different alphabets to be sorted is, to our knowledge, a new feature for a real-effort game<sup>5</sup>. A cognitive task such as the math task from Niederle and Vesterlund (2007)<sup>6</sup> does exhibit enough performance heterogeneity but would not meet the sorting criteria. After examining the different existing real-effort tasks, we decided to propose a new one that would meet the features of waste sorting behavior, including its tedious aspect, while remaining unframed so it can be easily transposed to other types of behavior.

Second, we propose a two-stage game in which the payoff matrix relies on the decisions of two players as follows: i) if both players decide not to participate in the task, shying away from contributing to a collective effort, they are rewarded with a secure individual payoff; ii) if the players decide to participate in the task, they must both succeed in the task to reach the optimal and highest payoff; if one of the players fails, the successful player also receives the payoff for failure (contamination); iii) the secure payoff is higher than the payoff if a participant fails.

Third, we implement two feedback treatments to test their efficacy in increasing the probability of coordination. More precisely, we examine the impact of information on an individual's past performance and the impact of information on the past performance of the partner with whom the collective sorting task will be performed. To our knowledge, this is the first study to consider feedback in this manner. We contribute to the literature by showing that the treatments have quite contrasting effects, indicating that the type of feedback matters.

Our study contributes to the literature on feedback and behavior changes by showing that, in our specific framework, performance feedback can improve coordination and prevent the occurrence of the weakest-link problem. This is true when the task is repeated over

---

<sup>5</sup>For instance, in Bortolotti et al. (2016), participants are asked to sort and count a variable number of coins worth 1, 2, 5, and 10 Euro cents. If we had used this task, we would not have met the heterogeneous ability criteria, as most people are familiar with their coins.

<sup>6</sup>Which involves adding a maximum of five 2-digit numbers in a limited time window.

time (as opposed to one-shot decisions), implying a learning effect amplified by receiving performance feedback. More precisely, we show how the type of feedback (information about one’s own performance vs. one’s partner’s performance) impacts beliefs, participation, and performance differently.

### 3 Experimental design and theoretical predictions

We investigate whether an individual’s decisions to participate in a collective sorting task, and their performance in this task, are modified by the provision of feedback. The task relates to a weakest-link technology and the feedback concerns either the subject’s own performance or their partner’s performance.

#### 3.1 The experiment

Hypotheses and the experimental design were pre-registered on AsPredicted.org nBø76746.

The experiment is designed as follows. The subjects play a two-stage game: the coordination problem only arises if they decide to participate in the second stage. The decision to opt out removes all uncertainty: in this case there is a known, secure payoff. The first stage thus consists of choosing between a risky game (deciding to participate and to perform the task, with interdependent payoffs) or a secure game (deciding not to participate and receiving a secure payoff).

If a subject decides to participate in the collective sorting task, the game presents a weakest-link problem with strategic uncertainty. The subject does not know (i) whether the sorting task will also be performed by the other subject with whom he is paired, nor (ii) whether the other subject will perform the task correctly. Moreover, subjects may not know whether they themselves will succeed at the task.

In our experiment, each individual plays the same game for eight consecutive periods. At the beginning of each period, each subject is randomly paired with another subject. All subjects are re-paired for each period.

Each period is divided into three steps:

- Step 1: The subjects decide whether or not they want to participate and perform the task.
- Step 2: Once this decision is made, beliefs about their own and their partner’s expected performance are elicited (the belief elicitation rule is described in Section 3.4). If a subject decides not to participate in the task, their reasons are investigated.
- Step3: The participating subjects perform the task.

As in Bortolotti et al. (2016), our experiment departs from the literature on weakest-link games by implementing a real-effort task. This choice was driven by a concern that chosen effort may not be a reliable proxy for real effort, and could limit the external validity of

results (Carpenter and Huet-Vaughn, 2019). Using a standard chosen-effort weakest-link game would not allow us to study the issue of interest, i.e. waste sorting behaviour, which is characterized by repetition and tedium, but also knowledge and learning. Not knowing whether a waste item is recyclable can lead to contrasting actions: the waste may be placed in a sorting bin, with a potential risk of contamination; or it may be thrown into an ordinary bin, with a risk of losing recyclable items. This second action corresponds to the opt-out decision in our game. The possibility of acquiring additional information to be able to make the correct sorting is excluded.

## 3.2 The sorting task

We design and implement a novel task requiring individuals to sort 7x7 matrices containing letters (in lower case) from three alphabets: Latin, Greek and Cyrillic (see Figure 1 for a screenshot of the experimental task). Each participant has 3 minutes to sort a maximum of 3 matrices per period. Sorting involves dragging letters into one of the three baskets at the bottom of the screen. Each basket corresponds to an alphabet. The total number of letters to be sorted is 147 ( $49 \times 3$ ). This results in a tedious task that mimics the boring and repetitive aspects of waste sorting and helps reduce the influence of inherent interest or enjoyment in the task itself (Corgnet et al., 2011).<sup>7</sup> At the end of the three minutes, we record, for each participant, the number of letters sorted and the number of sorting errors made during execution of the task.

To mitigate potential confusion arising from similarly looking letters across alphabets, we opted to exclude those from the matrices. Consequently, within each matrix, a random selection was made from a pool of 60 letters composed of 18 Latin, 23 Greek, and 19 Cyrillic characters (see Table A.1 in the Appendix for the complete list of letters used in the experiment). The share of each alphabet within each matrix is not uniform. This variability was deliberately integrated during the design phase to closely emulate the diversity of waste to sort depending on the location and on the situation: for instance, at home and at the university, the waste to be sorted may differ in proportion. Our protocol reflects this feature: in the experiment, the matrices presented to the subjects contain letters uniformly drawn from the pool of 60 letters, which implies different proportions of letters from each alphabet across matrices and periods. Consequently, the size of the matrix solely affects the time required for sorting. Following iterative trials, we determined that a 7x7 matrix size allows individuals to complete the sorting task within a fixed time frame of 3 minutes, uniformly applicable to all participants.

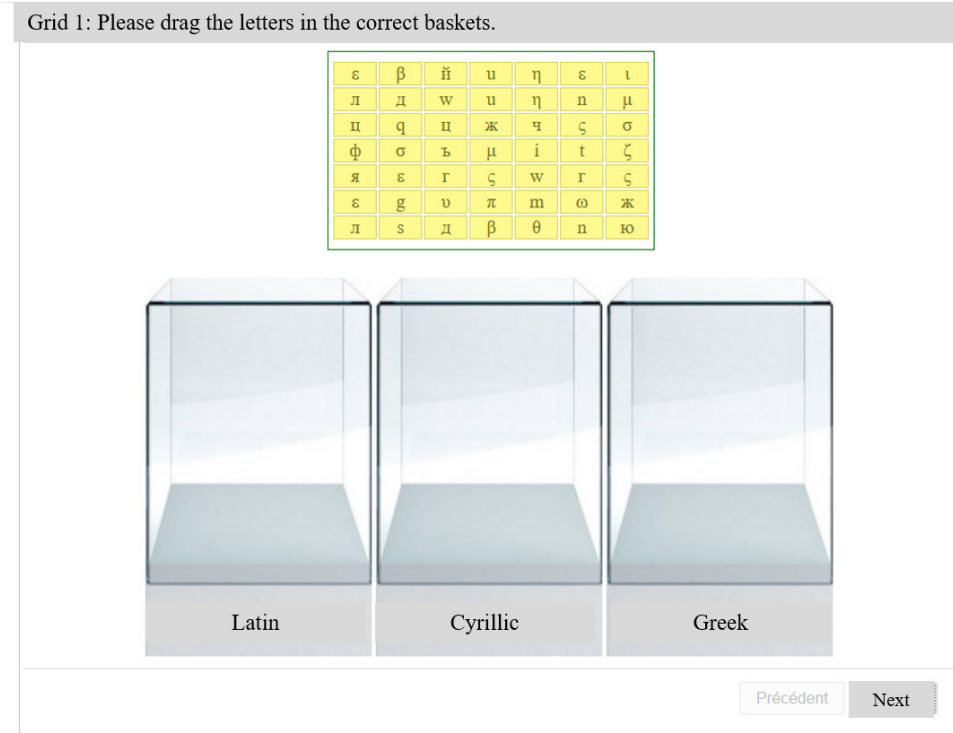
The subjects can move on to the next matrix at any time if they wish, simply by clicking on the “NextMatrix” button. They can also go back to the previous matrix using the “BackMatrix” button, to correct any mistakes. Note that within each group, each participant face different random matrices. Participants were made aware of this in the instructions. It is also possible for them not to sort every letter. All subjects have a practice round before

---

<sup>7</sup>These characteristics have been mentioned by some subjects when explaining their reasons for opting out.

the experiment actually begins, so they can become familiar with the task before having to make any decisions.

Figure 1: Screenshot of the sorting task



At the end of each period, each subject  $i$  is assessed on a performance index based on the number of correctly sorted letters - the quantitative dimension of performance - and the number of incorrectly sorted letters - the qualitative dimension of performance- in period  $t = 1, \dots, 8$  such that:

$$Performance\ index_{i_t} = \frac{2}{3} Quantity\ index_{i_t} + \frac{1}{3} Quality\ index_{i_t} \quad (1)$$

where the quantity index is the percentage of letters sorted, i.e. the number of sorted letters by the player out of the total number of letters that can be sorted, and the quality index is the percentage of letters correctly sorted, i.e. the number of correctly sorted letters out of the number of sorted letters,

$$Quantity\ index_{i_t} = \left( \frac{number\ of\ sorted\ letters_i}{total\ number\ of\ sortable\ letters} \right) \times 100 \quad (2)$$

$$Quality\ index_{i_t} = \left( 1 - \frac{number\ of\ errors_i}{number\ of\ letters\ actually\ sorted} \right) \times 100 \quad (3)$$

An asymmetric weighting between quality and quantity was adopted after running a pilot study, to avoid excessive dissuasion from participation for individuals who only know the Latin alphabet.

Each subject is then graded A or B for each period based on this performance index: A indicates a performance index of 80% or higher, and B a performance index of less than

80%<sup>8</sup>.

The two components of performance have distinct characteristics. Quantity, which refers to the number of letters sorted, directly relate to the effort provided by a subject, while quality relates to the participants’ knowledge (or ability) of the different alphabets. The attention required to avoid sorting a letter into the wrong basket may take time and ultimately reduce the number of letters sorted. Moreover, except for those who know at least two alphabets very well, the subjects are unsure if they can sort successfully. Consequently, some subjects may exert significant effort hoping to reach the 80% threshold but still fail. Hence, the failure to reach the 80% threshold can be attributed to insufficient effort in the real-effort task as well as a lack of knowledge of alphabets. These different potential causes for coordination failure can introduce confounding effects obscuring the effect of the treatments on coordination. However, we control for knowledge of the different alphabets, allowing us to account for the ability effect in our estimation, isolated from the treatment effects.

### 3.3 Payoff matrix

When a subject participates in the sorting task, their payoff depends on the performances of both partners (if the partner also decides to participate). When a subject opts for non-participation, the payoff is constant and equal to 11.<sup>9</sup>

Table 1 shows the payoff for each subject according to their own and their partner’s decision and performance.

Table 1: Payoff Matrix (in ECUs, at each period)

Player 1 grade \ Player 2 grade	A	B	$\emptyset$
A	30, 30	5, 10	15, 11
B	10, 5	8, 8	9, 11
$\emptyset$	11, 15	11, 9	11, 11

*Note:* “ $\emptyset$ ” refers to the decision of not taking part in the task.  
4ECUs=1euro

For instance, if both subjects in a pair participate and subject 1 gets grade A while subject 2 gets grade B, then subject 1 receives 5 ECUs and subject 2 10 ECUs.

The Pareto-optimal situation is reached when both subjects decide to participate in the sorting task and achieve grade A: this is what we refer to as *coordination* in this paper. If a subject cannot get a grade A, due to lack of knowledge of two alphabets for instance, opting

<sup>8</sup>The 80% threshold has been calibrated based on multiple experiment’s pre-tests and the pilot. It encourages sorting a large number of letters while tolerating a relatively significant number of errors.

<sup>9</sup>When both participants perform poorly, the payoff is (8,8) which is lower than the payoff when neither player participates (11,11). This reflects the additional cost incurred due to waste sorting errors. If errors are too numerous, sorting waste bins may even be discarded and sent to incinerators, leading to extra transportation, additional costs, and increased greenhouse gas emissions.

out is her dominant strategy. And if a subject thinks their partner will not achieve grade A, their dominant strategy is also to opt out.

The structure of the payoff matrix eliminates the free-riding incentive: when an individual decides to participate without really trying to succeed in the task and gets a grade B, their payoff is lower than the payoff received for not participating.

At the end of the experiment, one period is drawn at random and the associated payoff is part of the final individual payoff.

### 3.4 Belief elicitation and additional questions

To elicit subjects' beliefs, two incentivized questions are asked in each period when a subject has decided to take part in the task. The first question is: *What is the probability that the other member of your group will participate and have a performance index above 80%?*, and the second question is: *What is the probability that your performance index will be above 80%?*. The payoff is calculated following a lottery rule (Hollard et al., 2016; see the description of the incentive mechanism in Appendix A.2.)

After the eight periods, the subjects are asked to answer additional questions: questions to collect sociodemographic information (age, gender, education level, etc)<sup>10</sup>, their level of knowledge of the alphabets included in the experiment (self-assessed on a 10-point Likert scale) and a short version of New Ecological Paradigm scale (Moussaoui et al., 2016)<sup>11</sup>.

In the field of environmental psychology, waste sorting can be seen as an act of altruism (Hopper and Nielsen, 1991). Even though our experiment is unframed, altruism could play a role in situations where the results of collective sorting efforts can lead to coordination and higher payoffs for everybody. To get a measure of altruism, we use a condensed version of the modified dictator game by Blanco et al. (2011), a game designed to measure aversion to advantageous inequalities. Each subject individually answers 11 questions about distribution of gains between two individuals of a pair (see Table A.3 in Appendix). The computer randomly selects their status as a sender or a receiver. As the mean spread is kept constant, we use question 6 to estimate the degree of altruism of individuals. More precisely, using question 6, we calculate a dummy equal to 1 (altruist) if an individual chooses distribution X over Y. Otherwise, individual *i* is considered non-altruistic and we set the dummy to 0 (Gueye et al., 2020)<sup>12</sup>. Finally, one of the 11 questions is randomly chosen for the payoff.

Finally, we use Eckel and Grossman (2008)'s lottery task to elicit risk preferences (see Table A.2 in Appendix). Players have to choose between a set of 5 gambles with two alternative payoffs, and a 50% probability of winning each payoff. Losses are possible, such

---

<sup>10</sup>See for instance Nainggolan et al. (2019) for an analysis of the heterogeneity in household preferences for different waste separation and households' sociodemographic characteristics as well as attitudinal factors.

<sup>11</sup>Moussaoui et al. (2016) proposes two French versions of the Environmental Attitudes Inventory: one with 24 items and another one with 12 items. The second has been used. As NEP score is never significant and never changes the rest of the results, this control variable is not included. The estimations which include this variable are available upon request.

<sup>12</sup>We also considered a measure of altruism by treating this variable as a 10-point Likert scale (from line 2 to 11 of Table A.3 in the Appendix) instead of the dummy variable. The results remain unchanged and are available upon request.

that subjects could lose up to 6 ECUs on the riskiest gamble. The risk preference variable we use in our estimations takes values from 1 (most risk averse) to 5 (most risk seeking). The lottery chosen is played and the associated payoff is part of the final payoff. Participants know that if the resulting lottery payoff refers to a loss, the amount is subtracted from total payoff gained out of the other tasks.

### 3.5 Treatment groups and hypotheses

We want to test the effect of receiving two types of feedback that are assumed to influence people’s decisions and performances in the collective task. The experiment uses a between-subject design and is conducted with three groups.

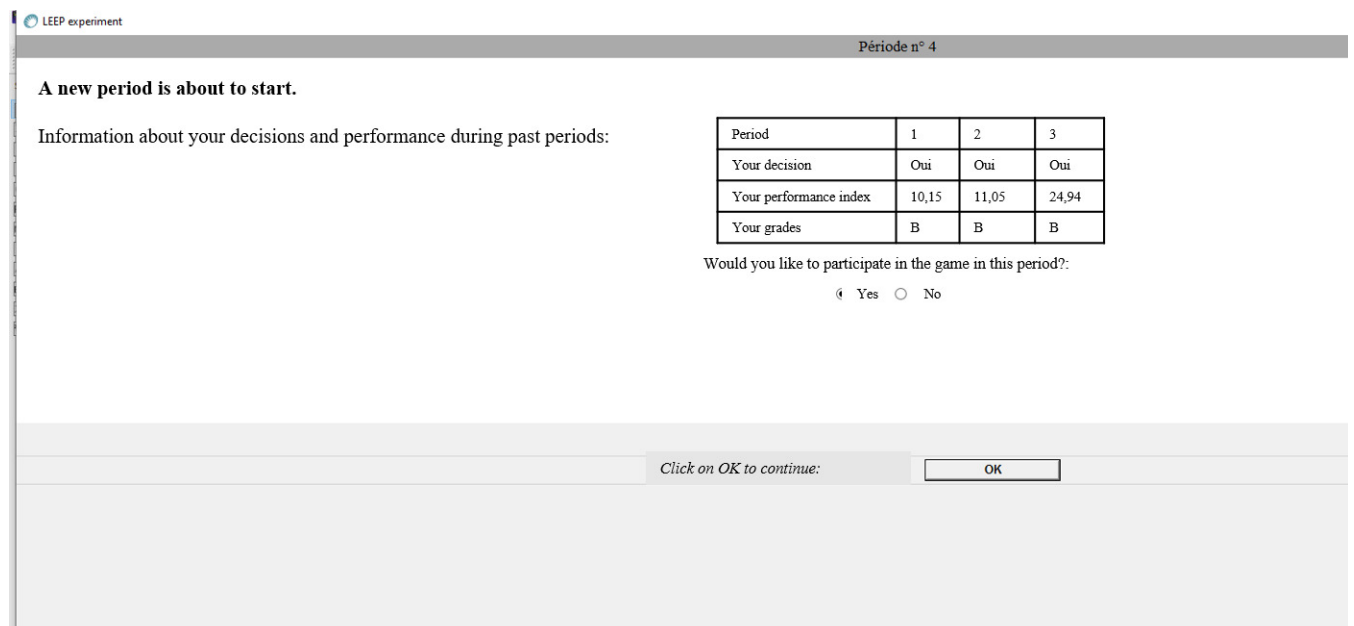
#### Control group

In the control group, the subjects play the game as described above. They do not receive any feedback throughout the experiment, except at the very end of the experiment.

#### Treatment “Self info” (SI)

With the “Self Info” treatment, in each period, before deciding whether to participate in the task or not, each individual receives information about their own performance index for previous periods. Specifically, in this treatment, participants are able to see, in a summary table, for each past period, their own decision of taking part in the task or not, their performance index and their grade. See for an example Figure 2 showing a screenshot of what is displayed at the start of period 4 to people in the SI treatment group.

Figure 2: Screenshot: Display at the start of period 4 in the Self info treatment group (text was translated from French)





## Treatment “Other info” (OI)

With the “Other Info” treatment, in each period, before deciding whether to participate in the task or not, each subject receives information about their partner’s participation and performance in previous periods. Specifically, in this treatment, participants are able to see, in a summary table, for each past period, information about the randomly drawn partner (i.e. her own decision of taking part in the task or not, her performance index and her grade). This information is different for each period, as subjects change partners.

Our hypotheses are the following.

**H 1. *Participation*** Participants update their beliefs after receiving performance feedback and should become more accurate. Moreover, they should adapt their opt-in decision according to this update.

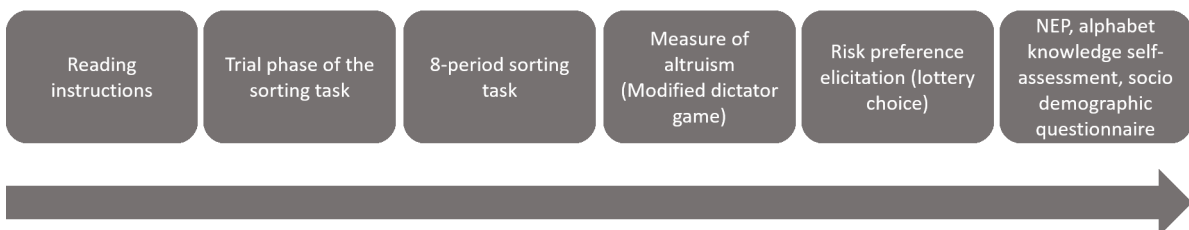
**H 2. *Feedback*** Beliefs update and decision to participate in the task can differ according to the type of feedback participants are provided with (feedback about one’s own performance vs. feedback about other participants’ performance).

## 3.6 Experimental procedure

The experiment was conducted at the *Laboratoire d’Economie Expérimentale de Paris*, (LEEP) and *Laboratoire d’Economie Expérimentale de la Défense* (LEED). We ran 15 sessions involving a total of 174 subjects. Each session lasted about one hour. One session corresponded to one treatment. We used the Online Recruitment Software for Economic Experiments (ORSEE) (Greiner, 2015) to recruit subjects from the same pool.

When entering the lab, participants were randomly assigned to a seat. Participants were handed paper instructions that were read aloud to ensure common understanding and additional instructions were also displayed on the screen. Figure 3 displays the sequence of the different tasks of an experimental session.

Figure 3: Sequence of tasks during a session



The total payoff, which was communicated only at the end of the experiment, included a show up fee of €7, plus the payoff from one period drawn at random at the end of the experiment, plus the two belief elicitation questions drawn at random, plus the chosen lottery and the modified dictator game. Average earnings were €20.4. All experimental instructions that were handed in to the subjects can be found in the supplementary material.



## 4 Descriptive statistics

### 4.1 Descriptive on the demographics

Table 2 shows descriptive statistics of the sample, split by treatment groups. The sample includes individuals of all ages and levels of education. To control for ex-ante knowledge of the different alphabets, we asked them to indicate how much they know about the three alphabets on a 10-point Likert scale. Unsurprisingly, the average knowledge is much higher for the Latin alphabet (8.1) than the Greek and Cyrillic alphabets (respectively 5.1 and 3.7). There are no significant differences between our treatment groups and the control group except for their knowledge of the Greek alphabet and their chosen lottery. Some of these variables will be included in the control variables in the regressions.

Table 2: Descriptive statistics

	Control	Self-info	Other info	CvsSI	CvsOI
<b>Female</b> (%)	58.2	51.8	55.2	0.50	0.75
<b>Age</b>	40.4	36.1	35.7	0.16	0.12
<b>Student</b> (yes, in %)	27.6	39.3	31.7	0.18	0.63
<b>Study level</b> (in %)				0.64	1.00
No diploma	1.7	0.0	1.7		
A-level	13.8	17.9	10.0		
Youth training NVQ, GNVQ	3.5	3.6	3.3		
Higher education	22.4	14.3	21.7		
Bachelor's degree	19.0	10.7	18.3		
Master degree	32.8	50.0	43.3		
PhD	6.9	3.6	1.7		
<b>Latin</b> (10-point scale)	7.7	8.4	8.1	0.21	0.43
<b>Cyrillic</b> (10-point scale)	3.4	4.1	3.7	0.16	0.48
<b>Greek</b> (10-point scale)	4.4	5.5	5.5	0.01	0.01
<b>Altruism</b> (%)	53.5	51.8	41.7	0.86	0.20
<b>Lottery choice</b> (%)				0.92	1.00
1	25.9	30.4	25.0		
2	15.5	21.4	18.3		
3	20.7	16.0	18.3		
4	12.0	14.3	10.0		
5	25.9	17.9	28.4		
<b>N</b>	58	56	60		

*Note:* Comparison two-sided t-test and proportion tests are performed between the control group and the treatment groups. A Kolmogorov-Smirnov test is performed to test different in distribution of study levels and lottery choices. Corresponding p-values are reported in the last two columns.

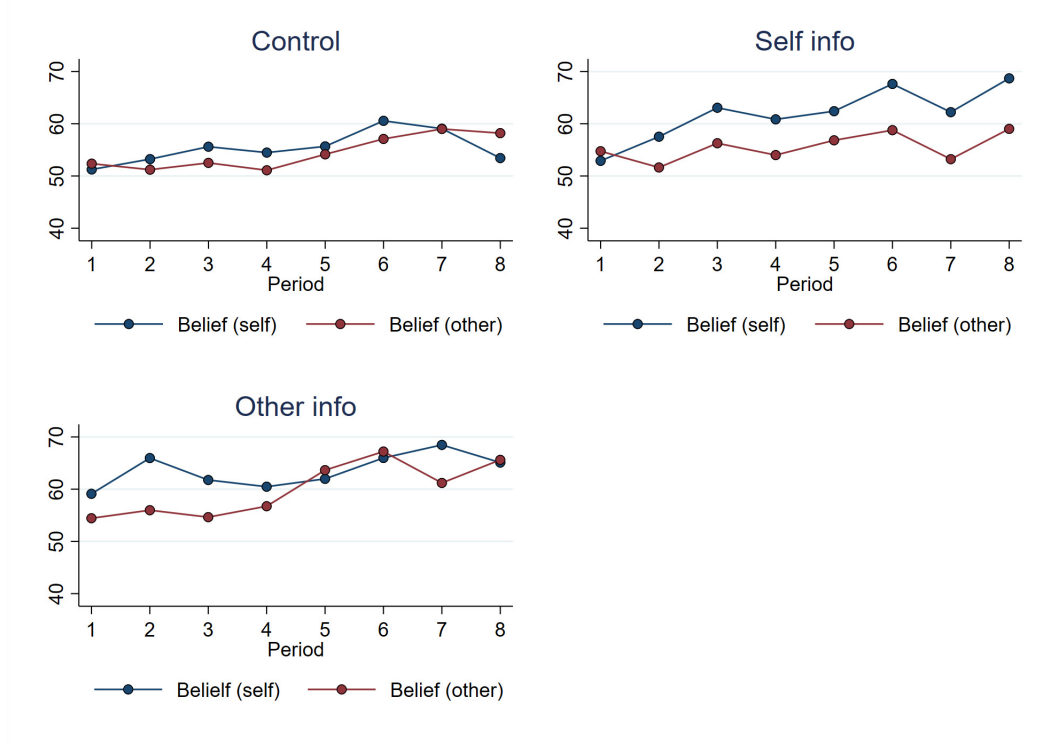
### 4.2 Beliefs

We expect beliefs to vary with the provision or absence of feedback, and to play a role in performance. Figure 4 shows the average elicited beliefs about one's own performance

(Belief (self)) and the partner’s performance (Belief (other)) over the eight periods, for each treatment.

In all the groups, beliefs increase slightly over time. This increase may partly come from a game learning effect due to repeating a task: this effect can influence beliefs and performance and is common in experiments involving repetitive tasks (Monaghan and Rowson, 2008). However, the increase is larger in the SI treatment group, and the beliefs about one’s own performance are greater than the beliefs about one’s partner’s performance.<sup>13</sup>

Figure 4: Average belief about one’s own and one’s partners’s performance, by group, over the eight periods



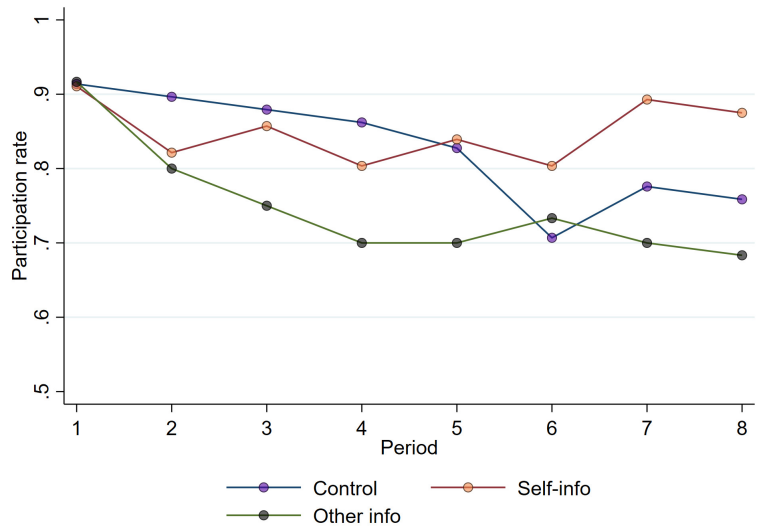
### 4.3 Participation rate

Figure 5 shows how participation evolved over the eight periods, by treatment group. In Period 1, before any information is revealed, the participation rate reaches about 91% and there is no difference between groups. From period 2, the trend is generally downward and differences appear between the groups. Initially, the decline in participation is stronger in both treatment groups than in the control group, but afterwards there is a clear divergence between the treatment groups. The participation rate in the SI treatment group shows a succession of rises and falls, but with a slightly downward trend overall during the first 6 periods. It then jumps to 87% and remains stable. In the OI treatment group, the participation rate decreases over time to 68% in the last period, the lowest level of the

<sup>13</sup>The beliefs are significantly different in the treatment SI only at the periods 2, 3, 6, 7 and 8 and in the treatment OI at the period 2.

whole experiment, meaning that receiving feedback about one’s partner in each period has a negative effect on participation. Consequently, the figure suggests that each feedback treatment has a different effect on the decision to participate in the sorting task.

Figure 5: Participation rate, by group, over the eight periods



## 4.4 Performance

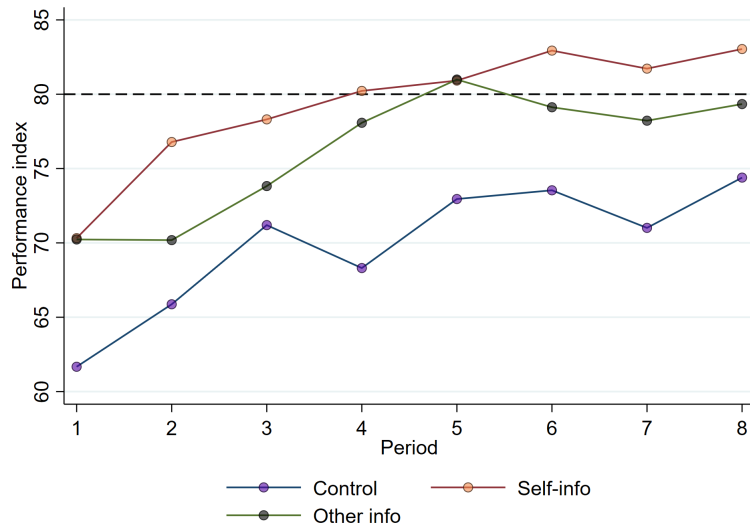
The failure to reach the 80% threshold can be due to insufficient effort in the real-effort task as well as a lack of knowledge of two alphabets. We expect performance to improve over time in all groups due to a game learning effect, as mentioned earlier (Monaghan and Rowson, 2008). Another type of learning effect may also be expected due to the receipt of a feedback<sup>14</sup>. Hence, performance in both treatment groups should be higher overall than in the control group, and show an increase over time.

Figure 6 shows the evolution of the average performance index for each period in each group as where the horizontal dashed line correspond to the 80% performance threshold to get the A grade. It confirms the existence of a learning effect, as performance increases over time in all groups, including the control group (in the estimations, this effect will be controlled for with period fixed effects).

Moreover, providing feedback seems to have an effect from the very beginning of the experiment: the performance is significantly better in both treatment groups than in the control group at period 1 (two-sided t-tests yield  $p = 0.02$  for both). Subjects in the treatment groups know there will be feedback on their performance index and this seems to motivate them to increase their effort leading to perform better from the outset. In the OI treatment group, the feedback is indirect but, as Figure 4 shows, both types of belief remain close, suggesting that feedback on their partner’s performance could lead people to update beliefs about their own performance.

<sup>14</sup>For instance, by exploiting natural experiment data, Bandiera et al. (2015) find that the provision of performance feedback has a positive effect on students’ subsequent test scores.

Figure 6: Performance index, by group, over the eight periods



Average performance in the SI treatment group is generally increasing and higher than in the control group. In the OI treatment group, although average performance is always higher than in the control group, an improvement in performance during the first few periods is followed by a decrease from period 5, and performance then remains below the 80% threshold. Given the lower participation rate in the OI treatment group, the average performance index is based on fewer individuals. Overall, this figure suggests that both feedback treatments affect performance, but in different ways.

Figure 7 reports movements in the quantity and quality indexes and the error rate<sup>15</sup> in the sorting task over the eight periods, broken down by group. In a way, the quantity dimension of the performance index provides information on the level of effort involved, while the quality dimension reflects the individual’s ability to perform the task. Figure 7 shows that the quantity index increases over time, suggesting a game learning effect. Additionally, both treatments, particularly the SI treatment, result in higher effort. However, the quality index is relatively stable over the 8 periods in each group. In both treatment groups, it is very close to, and slightly higher than, the control group. Finally, the error rate is lowest in the OI treatment group, followed by the SI treatment group (although not statistically different from the error rate in the control group). The strategy seems to differ between groups: subjects in the SI treatment prioritize quantity.

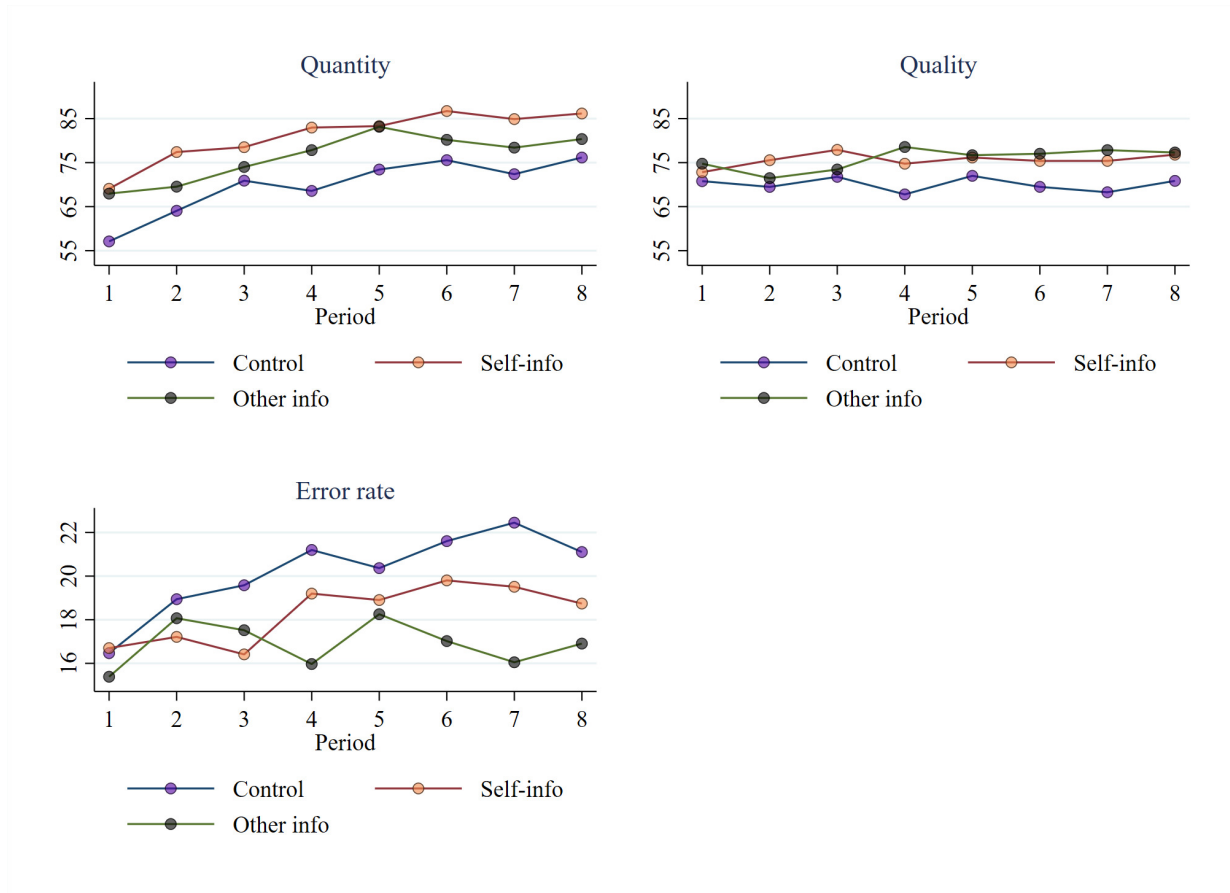
## 4.5 Coordination

Coordination is achieved when both subjects achieve a grade A. In our game, coordination is the social optimum. High coordination results from a high participation rate combined with high-quality sorting.

Figure 8 shows that coordination increases over time and is highest in the SI treatment

<sup>15</sup>The number of mistakes (i.e. putting a letter in the wrong basket) divided by the total number of letters that can be sorted, time 100.

Figure 7: Quantity and quality indexes and error rate, by group, over the eight periods



group. This is the product of this group's greater participation, as shown in Figure 5, and better performance, as shown in Figure 9. On the other hand, coordination in the OI treatment group is only a little better than in the control group, especially during the first and last periods.

Figure 8: Coordination rate, by group, over the eight periods

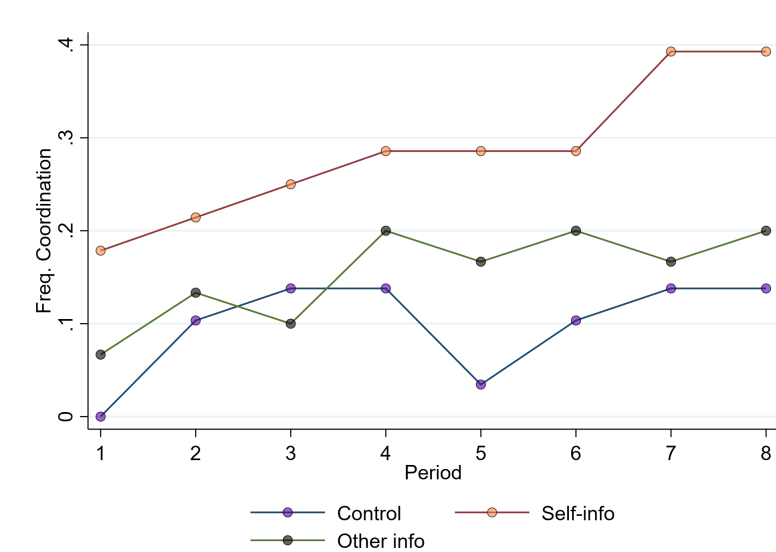
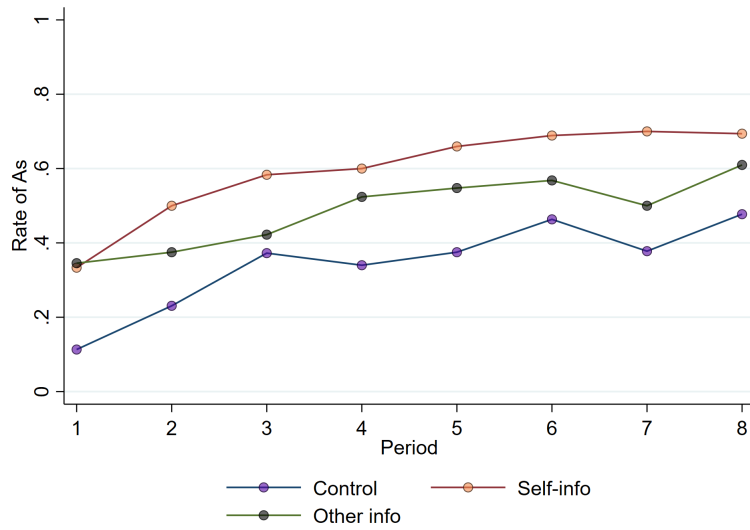


Figure 9: Rate of A grade, by group, over the eight periods



These initial results suggest that providing feedback on a subject’s own performance is more efficient to increase performance, and consequently participation and coordination. Providing feedback about the partner’s performance seems to be counter-productive, or at least no better than not receiving feedback.

This suggests the existence of a possible virtuous circle in the SI treatment group. When people are informed that they are performing well (or relatively well) the sorting activity, their beliefs about their expected performance increase, and this also increases beliefs about their partners and encourages them to participate in the next period and to benefit from a game learning effect. When a subject is informed that they are not performing the sorting activity correctly, they may opt out of participating in the next period, then try again from time to time. In the end, participation by A grade subjects is increased, while B grade subjects are dissuaded further from participating. In the next part, we will test this virtuous circle.

## 5 Results from hypothesis testing

### 5.1 Beliefs

To get a clearer understanding of what shapes subjects’ beliefs, we run a random-effect model with period-fixed effects. Table 3 presents the results of these estimations: in columns (1) and (4), only the treatment dummies are included; columns (2) and (5) add individuals’ performance grades, and interaction terms between past performances and the type of feedback received; lastly, columns (3) and (6) also integrate control variables: age, gender, level of knowledge of the three alphabets, and the measures of altruism and risk-seeking elicited through the tasks described in Section 3.4 and in Appendix A.2.

Table 3: Random effect models to explain the beliefs on the performances

VARIABLES	(1) Self-belief	(2) Self-belief	(3) Self-belief	(4) Other-belief	(5) Other-belief	(6) Other-belief
Self info (SI)	6.583 (4.548)	-7.540 (5.961)	-10.418* (5.887)	1.346 (3.829)	-7.673 (5.269)	-7.681 (5.162)
Other info (OI)	9.444** (4.101)	7.639 (4.736)	5.656 (4.654)	6.055* (3.665)	1.604 (4.962)	0.900 (4.857)
A at p.p.		4.309 (3.917)	3.664 (3.944)		-0.058 (3.958)	1.330 (4.127)
Other's A at p.p.		-0.373 (1.948)	-0.017 (1.985)		-4.599** (1.985)	-4.661** (2.055)
SI*A at p.p.		18.531** (8.056)	17.323** (7.984)		12.224* (6.400)	12.166* (6.546)
OI*A at p.p.		1.887 (4.949)	1.898 (4.943)		4.564 (5.856)	3.768 (6.169)
SI* other's A at p.p.		4.587 (3.023)	4.880 (3.053)		4.718 (3.657)	4.637 (3.684)
OI* other's A at p.p.		-2.098 (2.863)	-2.235 (2.940)		6.205 (3.910)	5.781 (3.986)
Age			0.109 (0.114)			0.234** (0.108)
Female			-9.225** (3.708)			-4.601 (3.527)
Cyrillic			-0.282 (0.886)			-1.127 (0.835)
Greek			2.838*** (0.943)			1.215 (0.915)
Latin			0.041 (0.760)			-0.568 (0.745)
Altruism			4.637 (3.580)			2.874 (3.619)
Risk seeking			0.776 (1.117)			-1.176 (1.116)
Constant	49.286*** (3.282)	55.942*** (3.777)	40.472*** (9.421)	51.866*** (2.761)	57.756*** (4.083)	55.563*** (9.363)
Period FE	✓	✓	✓	✓	✓	✓
# of Obs.	1,124	680	662	1,124	680	662
# of id.	172	158	153	172	158	153

*Notes:* Standard errors in parenthesis clustered at the individual level.  $p < 0.01$  \*\*\*,  $p < 0.05$  \*\*,  $p < 0.1$  \*. All regressions include period fixed effects. 2 subjects never took part in the task implying a sample of 172 subjects for column (1). In regressions in columns (2) and (5), 16 observations are missing from the two participants never participating in the previous period (8 periods each) hence 158 observations. 5 participants did not want to tell about their gender explaining the 153 participants in columns (3) and (6). Self info and Other info are the two dummy treatment variables. Self belief corresponds to self-confidence of getting a performance index A (between 0 and 100). Other belief corresponds to the self-confidence that the partner will get a performance index A (between 0 and 100). A at p.p. is a dummy variable equals to 1 if the participant had an A at the previous period, 0 otherwise. Other's A at p.p. is a dummy variable if the partner had an A at the previous period, 0 otherwise. Age is a continuous variable. Female is a dummy variable. Cyrillic, Greek and Latin are the assessed knowledge in the three different alphabets on a 10-point Likert scale. Altruism is a dummy variable. Risk seeking is the number of the lottery chosen: the higher the number, the riskier the lottery.

We first focus on the elicited beliefs about the ability to achieve a grade A during the period (columns 1 to 3). The significant positive coefficient of “Other info” in column (1) is not robust and disappears once other variables are added. In order to test the effect of receiving a positive feedback (getting A at the previous period) on self-belief, we study the interaction effect of the treatment dummies and the dummy of having A at the previous period (=0 if she got a B). Hence, the “SI\*A in previous period” coefficient is significant at the 1% level and positive in column (2), and remains so with the control variables in (3). This result means that the provision of positive feedback makes a subject more confident that they can perform well. Lastly, higher self-assessed knowledge of the Greek alphabet increases subjects’ beliefs that they can achieve a grade A, which is not the case for the other alphabets, and, in keeping with the existing literature, women are less confident than men about their ability to successfully complete the task.

Columns (4) to (6) reproduce the same regressions on elicited beliefs about the partner’s performance during the period. Subjects change partners for each period. As previously, column (4) indicates that being in the OI treatment group increases the beliefs about the partner’s performance, but this effect disappears once other variables are introduced. We also look at the interaction effect of the treatments and the nature of feedback, that is to evaluate how the subject’s belief reacts to the reception of a positive feedback. One would expect here that the “SI\*A at previous period” coefficient would not be relevant in the sense that getting feedback about oneself’s performance should not necessarily impact beliefs about others. It would rather be the interaction term between the OI treatment with the other player’s past grade that should have an effect on the belief about other’s performance. Interestingly, the “SI\*A in previous period” coefficient also increases confidence in the partner’s performance, but is twice as low as the estimated coefficient for self-belief and only significant at the 10% level. This effect holds in column (3). No effect is found for the interaction term of the OI treatment with the previous performance of the other player. Lastly, the older individuals are, the higher their beliefs about their partner’s performance.

To sum up, it seems that getting positive feedback about their own performance improves individuals’ beliefs about their own capacity, but also about their partner’s capacity, to achieve a grade A. This is not the case when feedback concerns the partner’s past performance, even when that past performance is good.

*Results: Beliefs are affected by the reception of feedback about performance. The effect is significant when the feedback concerns the subject’s own performance, but not when it concerns their partner’s performance, and is reinforced with the reception of a positive feedback.*

This first main result already shows an asymmetrical effect of the feedback on the participants’ beliefs, suggesting that providing absolute feedback on one’s own performance is more efficient in belief updating compared to absolute feedback on others’ performance, which partially confirms the first part of hypotheses 1 (“Participants update their beliefs after receiving performance feedback”) and 2 (“Beliefs update [...] in the task can differ according



to the type of feedback participants are provided with"). In the context of waste sorting, for instance, this would mean that if a policy aims to modify self-confidence, it would be more efficient to focus on an individual's own performance.

## 5.2 Participation

This section seeks to determine whether the decision to participate is impacted by the treatment, i.e. the type of feedback received. We expect participation to change over time and differ significantly depending on the treatment, as shown in Figure 5. We study the probability of deciding to participate in the sorting task by running a random-effect model with time-fixed effects. Table 4 presents the results of the estimations: column (1) focuses on the treatment dummies, column (2) also includes a dummy on the participation decision in the previous period and column (3) adds the control variables.

Table 4: Random effect model to explain the decision to participate in the task

VARIABLES	(1) Participation	(2) Participation	(3) Participation
Self info	0.023 (0.044)	0.024 (0.036)	0.013 (0.038)
Other info	-0.080* (0.044)	-0.072* (0.039)	-0.089** (0.039)
Participate at p. p.		0.243*** (0.056)	0.238*** (0.056)
Age			0.002* (0.001)
Female			0.017 (0.033)
Cyrillic			0.003 (0.007)
Greek			0.007 (0.008)
Latin			0.006 (0.006)
Altruism			-0.039 (0.032)
Risk seeking			-0.006 (0.009)
Constant	0.828*** (0.033)	0.596*** (0.067)	0.471*** (0.109)
Period FE	✓	✓	✓
# of Obs.	1,392	1,218	1,183
# of id.	174	174	169

*Notes:* Robust standard errors clustered at the individual level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . All regressions include period fixed effects. 5 participants did not want to tell about their gender explaining the 169 participants in column (3). Participation is a dummy variable equal to 1 if the individual decides to perform the task at period  $p$ . Self info and Other info are the two dummy treatment variables. Participate at p. p. is a dummy variable equals to 1 if the subject participated at the previous period, 0 otherwise. Age is a continuous variable. Female is a dummy variable. Cyrillic, Greek and Latin are the assessed knowledge in the three different alphabet on a 10-point Likert scale. Altruism is a dummy variable. Risk seeking is the number of the lottery chosen: the higher the number, the riskier the lottery.

The main result is the negative significant effect of the OI treatment on the decision to participate in the task compared to the control group, meaning that participants in the OI treatment group are less likely to participate. This effect holds in column (2) and the significance of the coefficient is reinforced with the addition of individual variables in column (3). The provision of information about the partner's performance thus dissuades individuals from performing the sorting task. This suggests that subjects clearly understand the payoff matrix: if they are paired with a partner at risk of poor performance, their payoff is higher

when they opt out of participation, and Figure 9 reveals the rather low number of subjects achieving grade A. However, this cautious decision not to participate in a period prevents any benefit from the game learning effect.

Another interesting result is the positive effect of previous participation in the task, suggesting a potential bias in some individuals in favour of participation. Lastly, receiving feedback about one's own performance does not seem to have any effect on participation.

*Results: Participation is negatively impacted by the OI treatment.*

This result partially confirms the second part of hypothesis 2 which relates to participation ("*... decision to participate in the task can differ according to the type of feedback participants are provided with*"). Hence, if one wants to enhance participation in a collective task, it seems that providing performance feedback would not necessarily be efficient. In fact, providing feedback about others' performance could even be detrimental to participation.

### 5.3 Performance and coordination

This section presents an exploratory analysis which deals with the probability of a participant achieving grade A and the probability of being part of a coordinated pair, i.e. in which both partners are graded A. Table 5 presents the results of the Linear Probability Model-based estimations: columns (1) and (4) only include the treatment dummies, columns (2) and (5) add the elicited beliefs, and columns (3) and (6) add the control variables.

Table 5: Random effect models to explain the probability of getting the grade A and of being a part of a coordination

VARIABLES	(1) Getting A	(2) Getting A	(3) Getting A	(4) Coordination	(5) Coordination	(6) Coordination
Self info	0.252*** (0.076)	0.228*** (0.071)	0.128** (0.063)	0.187*** (0.044)	0.188*** (0.042)	0.155*** (0.038)
Other info	0.156** (0.076)	0.123* (0.073)	0.025 (0.068)	0.055 (0.035)	0.058 (0.040)	0.019 (0.037)
Self-belief		0.004*** (0.001)	0.003*** (0.001)		0.004*** (0.001)	0.004*** (0.001)
Other-belief		-0.001 (0.001)	-0.000 (0.001)		0.000 (0.001)	0.000 (0.001)
Age			-0.009*** (0.001)			-0.006*** (0.001)
Female			-0.083 (0.059)			-0.031 (0.033)
Cyrillic			0.006 (0.012)			0.009 (0.008)
Greek			0.014 (0.015)			-0.017* (0.010)
Latin			0.035*** (0.010)			0.017** (0.007)
Altruism			-0.021 (0.054)			-0.046 (0.034)
Risk seeking			-0.005 (0.016)			-0.022** (0.010)
Constant	0.114** (0.049)	-0.040 (0.059)	0.086 (0.133)	0.001 (0.023)	-0.205*** (0.042)	0.049 (0.091)
Period FE	✓	✓	✓	✓	✓	✓
# of Obs	1,124	1,124	1,091	1,392	1,124	1,091
# of id.	172	172	167	174	172	167

*Notes:* Robust standard errors in parenthesis clustered at the individual level.  $p < 0.01$  \*\*\*,  $p < 0.05$  \*\*,  $p < 0.1$  \*. All regressions include period fixed effects. 2 subjects never take part in the task. 5 participants did not want to tell about their gender. Getting A is a dummy variable equals to 1 if the participant gets an A, 0 otherwise. Coordination is a dummy variable equals to 1 if both participants get an A, 0 otherwise. Self-belief corresponds to the elicited self-confidence of having a performance index of A (between 0 and 100). Other belief corresponds to the elicited self-confidence of that the partner has a performance index of A (between 0 and 100). Age is continuous variable. Female is a dummy variable. Cyrillic, Greek and Latin are the assessed knowledge in the three different alphabet on a 10-point Likert scale. Altruism is a dummy variable. Risk seeking is the number of the lottery chosen: the higher the number, the riskier the lottery.

The performance variables (A grade and coordination) and the decision to participate are not impacted by the same variables. In Column (1) of Table 5, both feedback treatments are significant and positive, but only the effect of the SI treatment holds when the control variables are added. Consequently, when individuals receive feedback on their own performance, the probability of their achieving an A grade is higher than in the control group. Columns (2) and (3) also show that beliefs about one's own performance have a significant and positive impact on the probability of achieving grade A. Subjects' confidence in their abilities to perform the sorting task effectively increases the probability of achieving grade A.

Results are quite similar for the probability of coordination (columns (4) to (6)), which is positively impacted by the SI treatment and beliefs about one's own performance. Column (6) also reveals a negative effect of the lottery choice: a risk seeking attitude tends to decrease the probability of coordination.

Lastly, both these probabilities are impacted negatively by age, and positively by the self-assessed knowledge of the Latin alphabet.

*Results: Individual performance and coordination are positively affected by the SI treatment. Belief in one's own performance increases both probabilities.*

Finally, it is possible to update the virtuous circle described earlier. The econometric results confirm that individuals' beliefs about their own performance increase when they receive positive feedback. In this scenario, their beliefs about their partners' performance also improve, although to a lesser extent. However, feedback on one's own performance does not influence the decision to participate. Lastly, the SI treatment significantly enhances both individual performance and coordination among participants.

The econometric results also confirm that different mechanisms are at work when feedback is provided on a partner's past performance. First, this feedback does not change the subjects' beliefs. In the OI treatment, except for individuals knowledgeable in at least two alphabets, subjects do not know whether they are performing the task correctly. Information obtained about partners' performances may impact their beliefs, but since partner pairs change each period, these beliefs can increase or decrease from one period to another. However, the main effect of feedback about the partner's performance is to dissuade people from participating: regardless of a subject's sorting ability, they have no incentive to participate if their partner has performed poorly in the past. Consequently, participation is lower in the OI treatment than in the control group. This diminishes the benefit from the game learning effect and lowers individual performance. Finally, coordination is not affected by the OI treatment.

Although it seems that participation in a collective task is not impacted by feedback and is even negatively affected by knowing how others performed, success and coordination can be improved when people receive feedback on their own performance. However, knowing about a potential partner's participation and performance history does not improve collective

performance. We previously observed that self-confidence increases with feedback. A possible mechanism is that feedback allows people to update their beliefs, potentially making them more accurate, which could ultimately result in increased performance in the collective task.

## 6 Discussion and conclusion

The objective of this paper was to study whether the provision of feedback helps individuals to update their beliefs and improve their performance and coordinate in a collective sorting task. Two types of feedback were considered: one focused on an individual's own performance, and the other on other people's performance. Surprisingly, while the former proved to be more efficient for achieving coordination, it did not necessarily enhance participation. Improvement occurs in particular through the evolution of individuals' beliefs, and learning effects. Feedback about other subjects' performance instead dissuaded people from participating in the sorting task.

The experiment proposed an original task that was designed to mimic waste sorting behaviours but can relate to other behaviours requiring coordination for effective performance. The task required real effort to maximize external validity.

We maintained sorting features with heterogeneity in potential sorting skills (using three alphabets with varying difficulty levels) but avoided contextualization to prevent experimenter demand effects. We retained key features of waste sorting behaviors, including the two-stage framework where individuals first decide whether to engage in a collective activity and then must successfully perform the task with the risk of failure, jeopardizing the collective effort. The two-stage framework and belief elicitation enable a deeper understanding and analysis of the decision-making process in such situations. Secondly, this approach allows for the application of our results to other pro-environmental behaviors that share similar characteristics, such as performance in terms of quality, quantity and coordination (e.g. carbon emission reduction or electricity consumption). Taking into account the link between quality and quantity in the collection of recyclable materials prevents a reductive representation of sorting behaviour as a simple choice between participation or non-participation in a collective task. The reward for participating in sorting must be conditional on achieving a certain quality, but the quality of waste sorting behaviour can be particularly difficult to assess in public places, such as streets, and semi-public places, e.g. common spaces for some people while excluding other, such as universities. One reason is that people have little or no time to seek out information to avoid sorting errors. In addition to the asymmetrical effect of the information treatments on participation and success in the task, we can demonstrate that beliefs, in fact self-confidence, play a significant role. Additionally, with both treatments, we proposed credible and inexpensive interventions involving feedback on one's own or others' past performance, which could easily be applied to real-world public policies.

We believe that some of our results offer valuable insights for different stakeholders involved in waste recycling policies, and we are confident that the unframed nature of our experiment enhances the analysis of decision-making for pro-environmental choices. As ex-

plained in the introduction, to increase the quantities of recyclable materials collected, more people must engage with the waste sorting process, and more locations must be involved: at home, at universities, in public transportation, and on the streets. Many different stakeholders can be involved in organizing spaces, providing equipment and giving sorting instructions to enable people to sort their waste. For instance, in public and semi-public environments, waste sorting bins sometimes consist of a transparent plastic bag, making the contents visible. Our results suggest that this could dissuade people from throwing their waste into sorting bins. While transparent bins could help increase the quality of waste sorting, they also risk decreasing the volume of materials collected.

Our results indicate that the provision of individualized information is important for improving waste sorting behavior and that providing such information directly impacts self-confidence in successfully completing the task, allowing participant to be better calibrated and perhaps make more efficient decisions. Such information is difficult, if not impossible to provide, in public and semi-public places but can be given at home. If the same sorting instructions apply in the public or semi-public spaces, improving sorting behavior at home could have positive effects in other locations. This kind of decision is a matter for policy-makers. It would be interesting in future research to analyze how a better performance at home can be transferred to behaviour outside the home. This issue could be explored with an experimental field study on waste sorting where different informational feedback could be provided to waste sorters (relative performance, absolute performance, social norms) and is kept for further research.

Lastly, this study has some limitations. First, regarding the choice of parameters. For instance, the weight used in the performance index for quality and quantity has been calibrated to allow sufficient participation in the task during the experiment, while also ensuring heterogeneity in performance. One might wonder whether changing the weight, for instance, to an equal weight, would modify the participation rate and resulting coordination. A similar question arises about the threshold to achieve grade A. Reducing this threshold may enhance coordination but also likely increase contamination. Further work is thus needed to determine the optimal design of these two parameters. Second, concerning the types of feedback we consider. Other types of feedback could be tested, such as relative performance feedback (how well the participant did compared to his/her partner), social norm feedback (providing information about how others perceive performance), or feedback on the collective outcome rather than the individual outcome. Additional research in this direction would help identify the most efficient information to provide to achieve efficiency in sorting. Third, we believe our paper still misses some elements of the complete picture of the decision and performance process. Further research should investigate the potential mechanisms by which receiving absolute feedback on one's own performance mitigates the risk of ability-related failures in coordination tasks through increased self-confidence in the task. Additionally, it should explore whether there are other behavioral components that we did not measure that play a role in coordination, such as social preferences or more precise measures of self-confidence. Finally, our design fails to disentangle the determinants of coordination vs. effort as they are

confounded in our framework. Although this was not the aim of our research question, this is something additional studies could be exploring as well which would refine policy designs.

CRedit authorship contribution statement

**Noemi Berlin:** Conceptualization, Data curation, Formal analysis, Methodology, Supervision, Visualization, Writing vY Original Draft Preparation, Writing vY Review and Editing.

**Mamadou Gueye:** Conceptualization, Data curation, Methodology, Investigation, Writing vY Original Draft Preparation

**Stphanie Monjon:** Conceptualization, Funding acquisition, Investigation, Methodology, Project administration, Visualization, Writing vY Review and Editing.



## References

- Andor, M. A. and Fels, K. M. (2018). Behavioral economics and energy conservation—a systematic review of non-price interventions and their causal effects. *Ecological economics*, 148:178–210. [6](#)
- Bandiera, O., Larcinese, V., and Rasul, I. (2015). Blissful ignorance? a natural experiment on the effect of feedback on students’ performance. *Labour Economics*, 34:13–25. [6](#), [18](#)
- Banerjee, S., De Vries, F. P., Hanley, N., and Van Soest, D. P. (2014). The impact of information provision on agglomeration bonus performance: an experimental study on local networks. *American Journal of Agricultural Economics*, 96(4):1009–1029. [7](#)
- Berninghaus, S. K. and Ehrhart, K.-M. (2001). Coordination and information: Recent experimental evidence. *Economics Letters*, 73(3):345–351. [6](#)
- Blanco, M., Engelmann, D., and Normann, H. T. (2011). A within-subject analysis of other-regarding preferences. *Games and Economic Behavior*, 72(2):321–338. [13](#)
- Bortolotti, S., Devetag, G., and Ortmann, A. (2016). Group incentives or individual incentives? a real-effort weak-link experiment. *Journal of Economic Psychology*, 56:60–73. [4](#), [8](#), [9](#)
- Brandts, J. and Cooper, D. J. (2006). Observability and overcoming coordination failure in organizations: An experimental study. *Experimental Economics*, 9:407–423. [7](#)
- Carpenter, J. and Huet-Vaughn, E. (2019). 19. real-effort tasks. *Handbook of research methods and applications in experimental economics*, page 368. [10](#)
- Cartwright, E. (2018). The optimal strategy in the minimum effort game. *Games*, 9(42). [5](#)
- Casal, S., DellaValle, N., Mittone, L., and Soraperra, I. (2017). Feedback and efficient behavior. *Plos one*, 12(4):e0175738. [6](#)
- Charness, G., Gneezy, U., and Henderson, A. (2018). Experimental methods: Measuring effort in economics experiments. *Journal of Economic Behavior & Organization*, 149:74–87. [8](#)
- Cooper, D. and Weber, R. A. (2020). Recent advances in experimental coordination games. *Handbook of Experimental Game Theory*, October:149–183. [4](#), [5](#), [6](#)
- Cooper, R. (1999). *Coordination games*. cambridge university Press. [4](#)
- Corgnet, B., Hernan-Gonzalez, R., and Rassenti, S. J. (2011). Real effort, real leisure and real-time supervision: Incentives and peer pressure in virtual organizations. [10](#)
- cour des comptes, L. (2017). Le traitement des déchets ménagers en Île-de-france : des objectifs non remplis. *Le rapport public annuel*, Tome I. [2](#)
- Eckel, C. C. and Grossman, P. J. (2008). Forecasting risk attitudes: An experimental study using actual and forecast gamble choices. *Journal of Economic Behavior & Organization*, 68(1):1–17. [14](#)

- Engel, C., Kube, S., and Kurschilgen, M. (2021). Managing expectations: How selective information affects cooperation and punishment in social dilemma games. *Journal of Economic Behavior & Organization*, 187:111–136. [6](#)
- Feldhaus, C., Rockenbach, B., and Zeppenfeld, C. (2020). Inequality in minimum-effort coordination. *Journal of Economic Behavior & Organization*, 177:341–370. [4](#), [5](#)
- Feltovich, N. and Swierzbinski, J. (2011). The role of strategic uncertainty in games: An experimental study of cheap talk and contracts in the nash demand game. *European Economic Review*, 55(4):554–574. [5](#)
- Gerhards, L. and Siemer, N. (2016). The impact of private and public feedback on worker performance: Evidence from the lab. *Economic Inquiry*, 54(2):1188–1201. [7](#)
- Greiner, B. (2015). Subject pool recruitment procedures: organizing experiments with orsee. *Journal of the Economic Science Association*, 1(1):114–125. [15](#)
- Gueye, M., Querou, N., and Soubeyran, R. (2020). Social preferences and coordination: An experiment. *Journal of Economic Behavior & Organization*, 173:26–54. [13](#)
- Heinemann, F., Nagel, R., and Ockenfels, P. (2009). Measuring strategic uncertainty in coordination games. *The review of economic studies*, 76(1):181–221. [4](#)
- Hollard, G., Massoni, S., and Vergnaud, J.-C. (2016). In search of good probability assessors: an experimental comparison of elicitation rules for confidence judgments. *Theory and Decision*, 80:363–387. [13](#), [36](#)
- Hopper, J. R. and Nielsen, J. M. (1991). Recycling as altruistic behavior: Normative and behavioral strategies to expand participation in a community recycling program. *Environment and behavior*, 23(2):195–220. [13](#)
- Hüffmeier, J., Krumm, S., Kanthak, J., and Hertel, G. (2012). Don't let the group down: Facets of instrumentality moderate the motivating effects of groups in a field experiment. *European Journal of Social Psychology*, 42(5):533–538. [4](#)
- Kaplan, T. R., Ruffle, B. J., and Shtudiner, Z. (2018). Cooperation through coordination in two stages. *Journal of Economic Behavior & Organization*, 154:206–219. [6](#)
- Karau, S. J. and Williams, K. D. (2001). Understanding individual motivation in groups: The collective effort model. *Groups at work: Theory and research*, 113:141. [4](#)
- Karlin, B., Zinger, J. F., and Ford, R. (2015). The effects of feedback on energy conservation: A meta-analysis. *Psychological bulletin*, 141(6):1205. [6](#)
- Kaza, S., Yao, L., Bhada-Tata, P., and Van Woerden, F. (2018). *What a waste 2.0: a global snapshot of solid waste management to 2050*. World Bank Publications. [2](#)
- Krupka, E. L. and Weber, R. A. (2009). The focusing and informational effects of norms on pro-social behavior. *Journal of Economic Psychology*, 30:307–320. [5](#)
- MacArthur, E. et al. (2013). Towards the circular economy. *Journal of Industrial Ecology*, 2(1):23–44. [2](#)
- Monaghan, P. and Rowson, C. (2008). The effect of repetition and similarity on sequence learning. *Memory & cognition*, 36:1509–1514. [17](#), [18](#)

- Moussaoui, L., Desrichard, O., Mella, N., Blum, A., Cantarella, M., Clémence, A., and Battiaz, E. (2016). Validation française de l’inventaire d’attitudes environnementales. *European Review of Applied Psychology*, 66(6):291–299. [13](#)
- Nainggolan, D., Pedersen, A. B., Smed, S., Zemo, K. H., Hasler, B., and Termansen, M. (2019). Consumers in a circular economy: economic analysis of household waste sorting behaviour. *Ecological Economics*, 166:106402. [13](#)
- Niederle, M. and Vesterlund, L. (2007). Do women shy away from competition? do men compete too much? *The quarterly journal of economics*, 122(3):1067–1101. [8](#)
- Penta, A. and Zuazo-Garin, P. (2022). Rationalizability, observability, and common knowledge. *The Review of Economic Studies*, 89(2):948–975. [5](#)
- Riechmann, T. and Weimann, J. (2008). Competition as a coordination device: Experimental evidence from a minimum effort coordination game. *European Journal of Political Economy*, 24:437–454. [5](#)
- Riedl, A., Rohde, I. M., and Strobel, M. (2016). Efficient coordination in weakest-link games. *The Review of Economic Studies*, 83(2):737–767. [5](#)
- Roithner, C. and Rechberger, H. (2020). Implementing the dimension of quality into the conventional quantitative definition of recycling rates. *Waste management*, 105:586–593. [2](#)
- Tonini, D., Albizzati, P. F., Caro, D., De Meester, S., Garbarino, E., and Blengini, G. A. (2022). Quality of recycling: Urgent and undefined. *Waste Management*, 146:11–19. [2](#)
- Van Huyck, J. B., Battalio, R. C., and Beil, R. O. (1990). Tacit coordination games, strategic uncertainty, and coordination failure. *The American Economic Review*, 80(1):234–248. [5](#), [6](#)
- Xu, L., Ling, M., and Wu, Y. (2018). Economic incentive and social influence to overcome household waste separation dilemma: A field intervention study. *Waste Management*, 77:522BΓY531. [3](#)
- Zomerén, A. v. and Tosti, L. (2017). Is recycling the solution for the reduction of our raw materials demand? *ECN Environment & Energy Engineering*. [2](#)

# Appendix

## A.1 Alphabet letters

Table A.1: List of the different letters within each alphabet

Latin Letter	Greek letters	Cyrillic letters
c	$\alpha$	CH
d	$\beta$	PϚ
f	$\gamma$	CϚ
g	$\delta$	PϚ
h	$\epsilon$	CH
i	$\zeta$	PϚ
j	$\eta$	PϚ
l	$\theta$	Pϵ
m	$\iota$	PБ
n	$\lambda$	P»
q	$\mu$	CS
r	$\nu$	PИ
s	$\xi$	CK
t	$\pi$	PϚ
u	$\rho$	Cİ
v	$\varsigma$	CH
w	$\sigma$	CK
z	$\tau$	CЉ
	$\upsilon$	CK
	$\phi$	
	$\chi$	
	$\psi$	
	$\omega$	

*Notes:* Similar letters that can be found in the different alphabets were deliberately excluded to avoid confusion. Consequently, within each matrix, a random selection was made from a pool of 60 letters composed of 18 Latin, 23 Greek, and 19 Cyrillic characters.

## A.2 Belief elicitation rule

At each round, to elicit players' beliefs, two incentivized questions are asked when the participation has been decided. The question 1 is: What is the probability that the other member of your group will participate and have a performance index above 80%?, and the question 2: What is the probability that your performance index will be above 80%?. The inventive-based mechanism is based on a lottery rule (Hollard et al., 2016):

- For question 1:
  - The computer randomly draws a number  $Y$  between 0 and 100.

- If the number drawn is less than the participant’s answer, then she receives  $x$  ECU if the performance index of the other member of her group is greater than 80%.
  - If the number  $Y$  drawn at random is greater than or equal to the participant’s answer, she has  $y\%$  chance of winning  $x$  ECU, and a  $100-Y\%$  chance of winning nothing.
- For question 2:
    - The computer randomly draws a number between 0 and 100.
    - If the number drawn is less than the participant’s answer, then she receives  $x$  ECU if her performance index is greater than 80%.
    - If the number  $Y$  drawn is less than the participant’s answer, then you have a  $Y\%$  chance of winning  $x$  ECU and a  $100-Y\%$  chance of winning nothing.

### A.3 Tables

Table A.2: Risk preference elicitation task

Lottery	Event	Payoff	Probability	Your choice
1	A	10	50%	
	B	10	50%	
2	B	18	50%	
	B	6	50%	
3	A	26	50%	
	B	2	50%	
4	A	34	50%	
	B	-2	50%	
5	A	42	50%	
	B	-6	50%	

Table A.3: Modified dictator game

	Options	Choice
1	Option X: 0 for you and 0 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
2	Option X: 1 for you and 1 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
3	Option X: 2 for you and 2 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
4	Option X: 3 for you and 3 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
5	Option X: 4 for you and 4 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
6	Option X: 5 for you and 5 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
7	Option X: 6 for you and 6 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
8	Option X: 7 for you and 7 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
9	Option X: 8 for you and 8 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
10	Option X: 9 for you and 9 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y
11	Option X: 10 for you and 10 for the other member of your group Option Y: 10 for you and 0 for the other member of your group	Option X Option Y