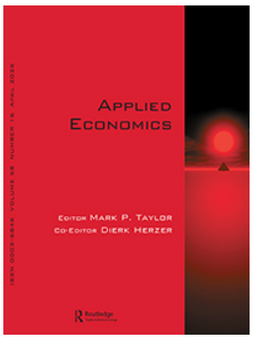

Applied Economics

ISSN: 0003-6846 (Print) 1466-4283 (Online) Journal homepage: www.tandfonline.com/journals/raec20

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To cite this article: Etienne Dagorn (02 Apr 2026): Social pressure and prosocial behaviours among teenagers, Applied Economics, DOI: 10.1080/00036846.2026.2645678

To link to this article: https://doi.org/10.1080/00036846.2026.2645678

APPLIED ECONOMICS
https://doi.org/10.1080/00036846.2026.2645678

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Social pressure and prosocial behaviours among teenagers

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ABSTRACT

This study investigates the influence of social pressure on prosocial behaviours among adolescents through a lab-in-the-field experiment in France. Using a controlled environment where teenagers interact with their classmates, we examine the evolution of cooperation over repeated interactions and explore its determinants. We find that girls and altruistic pupils demonstrate higher levels of cooperation. The introduction of non-monetary rewards and punishments, serving as forms of social pressure, significantly enhances cooperation among adolescents, aligning their behaviour with group's cooperation. Surprisingly, the study finds that the intensity of social pressure does not directly impact cooperation, suggesting that adolescents adapt their pro-social behaviour in response to social pressure awareness. This study demonstrates that peer-enforced norms sustain cooperation among adolescents, even in the absence of material incentives.

KEYWORDS

Peer pressure; childhood; prosocial behaviours; public goods game; field experiment

JEL CLASSIFICATION

C93; A13; C92

I. Introduction

From school classrooms to social media platforms, peer influence plays a critical role in shaping adolescent behaviour. Whether encouraging prosocial actions-such as cooperation and inclusivity-or reinforcing negative behaviours, social pressure remains a powerful driver of decision-making. It operates through two primary mechanisms: (1) conformity, where individuals adjust their actions to align with the expectations of their reference group (Coleman 1990; Crane 1991; Fatas, Heap, and Arjona 2018), and (2) reinforcement, where approval or disapproval strengthens these norms (Balafoutas, Nikiforakis, and Rockenbach 2014; Chaudhuri 2011; Gächter and Herrmann 2009; Villeval 2020) While the effects of social pressure have been extensively studied in adults, relatively little is known about how adolescents regulate cooperation within their peer groups-particularly in the absence of external authority. Given that adolescence is a critical period for social learning, identity formation, and heightened sensitivity to peer feedback, understanding how peer-enforced norms shape cooperative behaviour is essential for explaining the emergence of prosocial tendencies and the conditions under which they are sustained.

Theoretical models suggest that peer pressure shapes behaviour when individuals internalize others' attitudes towards them (Barron and Gjerde 1997; Kandel and Lazear 1992), and extensive evidence shows that people adjust their actions to align with group norms (Charroin, Fortin, and Villeval 2022; Kiessling, Radbruch, and Schaube 2022; Sacerdote 2011). Such peer effects span domains including charitable giving (Smith, Windmeijer, and Wright 2015), education (Feld and Zölitz 2022), dishonesty (Lefebvre et al. 2015), personality (Golsteyn, Non, and Zölitz 2021), and even crime (Falk and Fischbacher 2002). During adolescence, heightened sensitivity to peer approval and disapproval makes social feedback especially influential (Blakemore 2018; Gardner and Steinberg 2005; Teunissen et al. 2016). Developmental research also highlights gendered patterns: girls more often display empathic helping, while boys tend to enforce norms (Carlo et al. 1999; Eisenberg 2006; Van der Graaff et al. 2018). Distinguishing between conformity and norm enforcement remains empirically challenging due to network endogeneity, reflection, and selection biases (Angrist 2014; Baccara and Yariv 2013; Carrell, Sacerdote, and West 2013; Manski 1993). For instance, Bhargava et al. (2023) show

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Supplemental data for this article can be accessed online at https://doi.org/10.1080/00036846.2026.2645678

that behavioural similarity among French adolescents largely reflects selection rather than influence. To address these issues, recent studies use randomized designs to isolate peer enforcement mechanisms (Charroin, Fortin, and Villeval 2022; Feld and Zölitz 2022). Building on this approach, we investigate whether adolescents enforce cooperative norms through non-monetary peer feedback in a controlled classroom field experiment.

This study investigates how non-monetary peer feedback-symbolic approval or disapproval-shapes adolescent cooperation. While prior research on children's punishment behaviour has focused primarly on third-party enforcement s (e.g. Bašić et al. 2020, 2021; Jordan, McAuliffe, and Warneken 2014; Lergetporer et al. 2014), adolescents increasingly self-regulate through peer interactions, making it crucial to understand how they enforce cooperative norms in the the absence of external oversight. To isolate the mechanisms of peer regulation, we employ a controlled public goods experiment that manipulates two key processes: (1) conformity to group behavior and (2) reinforcement via symbolic feedback. By introducing symbolic rewards and sanctions within fixed groups, we capture real-time adjustments in cooperative behaviour (Dugar 2013; van Hoorn et al. 2016). Our measure of prosociality-monetary contributions to a public good- aligns with experimental studies on norm compliance and fairness (e.g. Fehr and Gachter 2000; Masclet et al. 2003), as it reflects a strategic, impersonal form of cooperation. However, we acknowledge that this operationalization captures only a narrow dimension of prosocial behavior, distinct from relational forms such as helping or emotional support.

We designed a lab-in-the-field experiment in Britanny (France) involving over 400 seventh-graders (ages 12–13), who interacted with classmates during regular school hours. This setting balances internal validity with external realism, embedding participants in familiar peer environments where symbolic feedback-approval and disapproval-is naturally present. The experiment consists of two six-period public goods games: a baseline phase with no feedback and a treatment phase allowing participants to send symbolic social feedback (positive or negative smileys). This sequential design allows us to examine how individual contributions change with the introduction of feedback, providing suggestive evidence on the influence of peer approval and disapproval. Additionally, two preference elicitation games assess altruism and competitiveness to provide insights into how individual traits shape cooperative behaviour.

The results show that while cooperation levels remain stable within each treatment group, the introduction of social feedback increases average contributions in the public good game from 49 to 63% of the endowment. Notably, this occurs despite the absence of monetary incentives and the anonymity of feedback, indicating that adolescents are responsive to symbolic peer approval and disapproval. Further analysis reveals that altruistic individuals-particularly boys-are more sensitive to social pressure, while girls consistently display higher baseline levels of prosocial behaviour. The administration of social pressure also varies based on socioeconomic status and personality traits: privileged boys are more likely to punish than reward, whereas competitive girls tend to impose sanctions. Although the absolute number of smileys received does not significantly alter cooperation levels, peer-imposed punishment appears to be an effective deterrent against free-riding, reinforcing the role of social pressure in sustaining cooperation.

This study informs debates on adolescent socialization with implications for education policy (Feld and Zölitz 2022) and risk-taking behaviour (Blakemore 2018). Adolescents often regulate behaviour through peer norms rather than external authority, as illustrated by the University of Virginia's Honor System and Finland's KiVa Anti-Bullying Program, where peer accountability and bystander engagement foster ethical and cooperative conduct. These examples highlight the potential of leveraging peer influence to promote prosocial behaviour in schools, for instance through student-led accountability initiatives or cooperative learning frameworks.

The remainder of the paper is organized as follows. Section I presents the experimental design and procedures. Section II reports the main results. Section III discusses implications and concludes.

Experimental design

Procedure

In spring 2018, we invited a dozen schools to participate in a study on digital practices and their effects on academic performance and social behaviour among 7th-grade students. Table 1 presents the characteristics of the five participating schools in comparison with national averages. Four schools were public, with two in urban areas and three in rural settings, mirroring regional distribution. Socioeconomic representativeness was assessed using the Index of Social Position (ISP), based on parental occupation, education, and socio-professional category. The selected schools reflect the broader national student population in terms of socioeconomic background, school type, and geographic distribution, with ISP scores clustering around the national average.[1] Selection bias was minimal, with only 27 students (4.7%) opting out. Conducting the experiment within schools rather than through voluntary participation limited parental influence and enhanced external validity, making the results more generalizable to real-world peer interactions (List et al. 2023).

Participants were randomly selected at the class level to ensure balance. Of the 570 students, 432 participated in the experiment, while the rest completed a survey.[2] The experiment was conducted using a mobile laboratory with 20 tablets, replicating traditional lab conditions within schools (Appendix 4). Participants played simultaneously, ensuring real-time social interaction effects. To preserve anonymity and neutrality, they were explicitly informed that there were no 'right' answers, all choices were confidential, and communication was strictly prohibited to prevent coordination or social signalling. Table 2 details participant distribution by school and class.

The experiment was conducted in standard classrooms during regular school hours, ensuring that students remained in a familiar setting and minimizing external distractions. Participants played alongside their classmates but could not identify specific interaction partners, preventing personal biases or pre-existing relationships from influencing cooperation. None had prior experience with cooperation-based experiments involving classmates, meaning no established social

Table 1. Summary of schools by location, type, and social Index.

School	Urban or Rural Area	Private or Public School	Index of Social Position	Size
1	Rural	Public	96.6	122 (436)
2	Urban	Public	127	136 (557)
3	Rural	Private	100	40 (295)
4	Rural	Public	96	96 (395)
5	Urban	Public	115	176 (469)
Total				570 (2152)

The table reports the main characteristics of the schools included in the study, including their location (urban or rural area), type (public or private), and their **Index of Social Position (ISP)**. The ISP is a composite indicator provided by the *Ministère de l'Éducation nationale* in France, which summarizes the average social and economic characteristics of students' families in a school. It takes into account parents' professions, educational attainment, and household resources. Lower values indicate a more disadvantaged student population, while higher values indicate a more advantaged one. The **Size** column reports the number of participating students in each school, with the total number of enrolled students in parentheses.

Table 2. Participants by school and class.

	Class						Total
School	1	2	3	4	5	6	
1	20	16	20	20	20	0	96
2	20	20	20	16	20	0	96
3	20	20	0	0	0	0	40
4	20	20	20	20	0	0	80
5	20	20	20	20	20	20	120
Total	100	96	80	76	60	20	432

[1]We acknowledge that the absence of school-level demographic differences neither rules out selection on unobservables nor ensures that the participating sample is representative.

[2]We have been unable to retrace the sociodemographic characteristics for 9 individuals.

norms applied to this context. While participants may have entered with general notions of prosocial behaviour, they lacked predefined expectations about how to act in this specific setting. Before starting, all students received detailed instructions and completed comprehension tests to confirm their understanding, with opportunities to ask clarification questions.[3] Experimenters carefully avoided providing behavioural cues to ensure that all choices were made independently. A teacher or school representative was present during each session but was explicitly instructed not to intervene, preserving internal validity and preventing external pressure on participants' decisions.

Monetary incentives are standard in experimental economics but pose challenges when working with children due to school policies, parental consent, and limited financial literacy. To address this, participants earned tokens convertible into gift certificates or small prizes (average value €8), comparable to a weekly allowance for this age group (List et al. 2023). Such incentives ensured that cooperation involved real trade-offs, mitigating social desirability bias and providing a more reliable measure of prosocial behaviour than surveys or teacher assessments. To further minimize experimenter demand effects (De Quidt, Vesterlund, and Wilson 2019), four design features were implemented. First, the use of costly incentives discouraged purely normative responses. Second, multidimensional tasks concealed the experiment's main objective. Third, strict anonymity preserved privacy from peers, teachers, and parents. Fourth, all instructions used neutral language to avoid framing cues. While the study was not fully double-blind-the same experimenter observed behaviour and payoffs-these safeguards, together with the incentive structure, greatly reduce potential biases and ensure robust, internally consistent measures of cooperative behaviour.

Games

The experiment consisted of three distinct games, programmed using the Z-tree software (Fischbacher 2007). The main experiment was a public good game, conducted in two sequential settings: a baseline phase and a (dis)approval treatment involving non-monetary sanctions and rewards. This design tests whether exposure to social pressure influences prosocial behaviour. Both settings lasted six periods, with participants interacting in fixed groups of four across both phases to simulate real-world peer dynamics. The use of partner matching ensured repeated interactions, allowing for the observation of consistent cooperative behaviour over time. To prevent endowment effects, only one randomly selected period from each phase was used for final payments, a decision revealed only at the end of the experiment to prevent strategic adjustments.

Each participant received an endowment of 20 tokens per period and decided how much to contribute to a shared group fund. Contributions were multiplied by 0.5, meaning that the total public good income was equally distributed among all group members, regardless of their individual contributions. The remaining tokens stayed in individual private accounts, creating an incentive to free-ride. At the end of each period, participants saw their personal earnings, group contributions, and the public good income. The individual profit function is defined as:

$$\pi_{ij} = 20 - C_i + 0.5 \sum_{i=1}^{4}, C_i \qquad (1)$$

where π_{ij} represents the earnings of participant i in period j, and C_i is the amount contributed to the public good. To accommodate younger participants with limited cognitive abilities, this study modifies the marginal per capita return (MPCR) from the canonical design by Masclet et al. (2003). More complex return structures could introduce unintended cognitive biases, potentially distorting the analysis of cooperation determinants.[4] As a result, this study does not aim to pinpoint the exact threshold at which cooperation emerges or declines, but rather to explore how adolescents respond to social incentives within a simplified framework.

[3]See Appendix 5 for detailed instructions.
[4]The link between cooperation and academic achievement, which may serve as a proxy for cognitive ability, appears limited in this context (Dagorn, Masclet, and Penard 2025).

After the baseline setting, participants entered the (dis)approval treatment, which introduced two key modifications: (1) the implementation of social pressure mechanisms, and (2) the disclosure of individual contributions alongside the group's overall contribution distribution. This phase consisted of two stages: a contribution stage, identical to the baseline game, followed by a (dis)approval stage, where participants could send positive or negative smileys to each group member or opt out of providing feedback. These non-monetary signals had no impact on individual payoffs, which remained solely determined by public good revenues and personal contributions. To address reputational concerns within fixed groups, participant IDs were shuffled, and screen positions were randomized at the start of each period. This prevented participants from linking past contributions to specific individuals, minimizing reciprocal behaviours and strategic signalling (Denant-Boemont, Masclet, and Noussair 2007; Fehr and Gachter 2000; Nikiforakis and Normann 2008; Stoop, van Soest, and Vyrastekova 2013). While reshuffling groups entirely could have further mitigated reciprocity, maintaining stable group compositions was crucial to replicating real-world social dynamics and observing cooperative behaviour in consistent peer settings. This design balanced the need for partner-matching structures while minimizing biases from reputational effects.

In addition to the public good game, two supplementary games were included with random matching within each session. The first supplementary game was a real-effort task in which participants selected their remuneration scheme, offering insights into their preferences for competition. The second supplementary game was a modified dictator game which measured altruistic tendencies by assessing participants' willingness to share resources without any expectation of reciprocity.

Figure 1 provides an overview of the experimental setting.

The experimental design reflects a trade-off between realism and causal inference, shaped by classroom constraints. First, the experiment lacks a randomized control group under a no-feedback condition; instead, we compare behaviour before and after introducing feedback across rounds. This design limits causal inference but captures systematic associations between social feedback and cooperation. Second, the sequential structure of the two public goods games introduces potential confounds, including learning and fatigue effects. To mitigate these, we implemented detailed instructions, comprehension checks, fixed peer groups, and randomized on-screen identities; additionally, framing the second game as a continuation (rather than a restart) reduced—but did not eliminate—behavioral carryover. Third, the order of preference-elicitation tasks (altruism and competitiveness) was deliberately reversed to avoid priming cooperative behavior. While this design choice ensures that contributions were measured without activation of prosocial or competitive motives, it also complicates interpretation of these traits as pure baselines. For example, the positive association between altruism and contributions should be

Figure 1: Experimental Setting

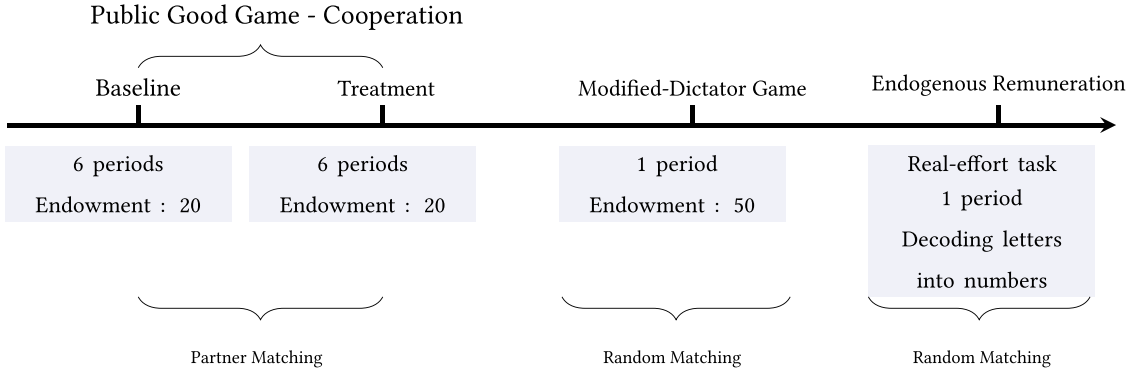

Figure 1. Experimental setting.

read cautiously: anonymity and rematching likely minimized spillovers, and the null result for competitiveness further suggests that task order introduced limited bias.

II. Results

Variables of interest

In this section, we define the main variables of interest.

Cooperation is measured as the number of tokens contributed to the public good in each period of the game. The first six periods, in which participants could not provide feedback, are referred to as the baseline. The final six periods, where non-monetary punishments and rewards were available, are referred to as the (dis)approval treatment.

Altruism is measured through a modified dictator game (MDG) adapted from Berg, Dickhaut, and McCabe (1995). Each participant received 50 tokens and was anonymously paired with another player. Both simultaneously decided how many tokens to transfer to their partner, with payoffs defined as:

$$\pi_i = 50 - S_{ij} + S_{ki} \tag{2}$$

where S_{ij} represents the number of tokens sent by player i to player j, and S_{ki} is the number of tokens received by player i from player k. Consistent with earlier findings, girls are sending more tokens than boys (see 8).

Competitiveness is elicited via a real-effort letter – number decoding task in which participants chose between a flat wage or a tournament scheme (Charness, Masclet, and Villeval 2013). Payoffs were fixed at 50 tokens under the flat wage, 80 for winning the tournament, and 20 for losing. The task was designed to be sufficiently demanding to ensure that entering the tournament reflected genuine competitiveness, while also capturing potential overconfidence among entrants. Consistent with prior findings, girls more often selected the flat wage and exerted greater effort under this scheme (see 9–10).

Control variables include gender and socioeconomic background based on administrative data. Socioeconomic status (SES) is coded as High if at least one parent holds an executive or intermediate occupation, and Low otherwise. See Sutter et al. (2019) and Schunk and Zipperle (2023) for a review of preference in childhood and its relationship with demographics.

Determinants of cooperation

Figure 2 shows that in the first baseline period, participants contributed 49.28% of their endowment (20 tokens), compared to 63.47% in the treatment's first period. A Wilcoxon signed-rank test confirms a significant increase in contributions following the introduction of social pressure ($Z = 16.88$, $p = 0.000$), corroborated by a two-tailed t-test ($p < 0.000$). Although the fixed game order raises the possibility of learning effects, comparing periods 1 and 7 mitigates this concern, revealing consistent gains under social feedback. Across all periods, Wilcoxon tests reject equality of medians ($p < 0.01$), indicating that social pressure not only boosts average contributions but sustains cooperation over time. Within each condition, contributions increased significantly between the first two periods (baseline: $Z = 7.404$, $p = 0.000$; treatment: $Z = 6.519$, $p < 0.000$) before stabilizing thereafter. The rise between periods 7 and 8 mirrors early-round adjustments, suggesting an initial learning effect as participants adapted to approval and disapproval mechanisms. Beyond these early and end-period fluctuations-including a modest final decline ($Z = -1.874$, $p < 0.06$)-cooperation remained stable, indicating that peer-enforced social pressure effectively established and maintained cooperative norms across interactions.

We estimate the following panel model to assess the effect of social pressure on cooperation:

$$y_{it} = \beta_0 + \beta_1.\text{period} + \beta_2.\text{treatment} + \beta_3.\gamma_i + \beta_4.\gamma \times \text{treatment} + X_i + \alpha_i + \epsilon_{it}$$

In this model, y_{it} represents the number of tokens given to the public good by participant i at period t. The variable *Treatment* is a dummy that equals one if the period was included in the treatment. *Period* is a linear time trend. The variable γ_i represents

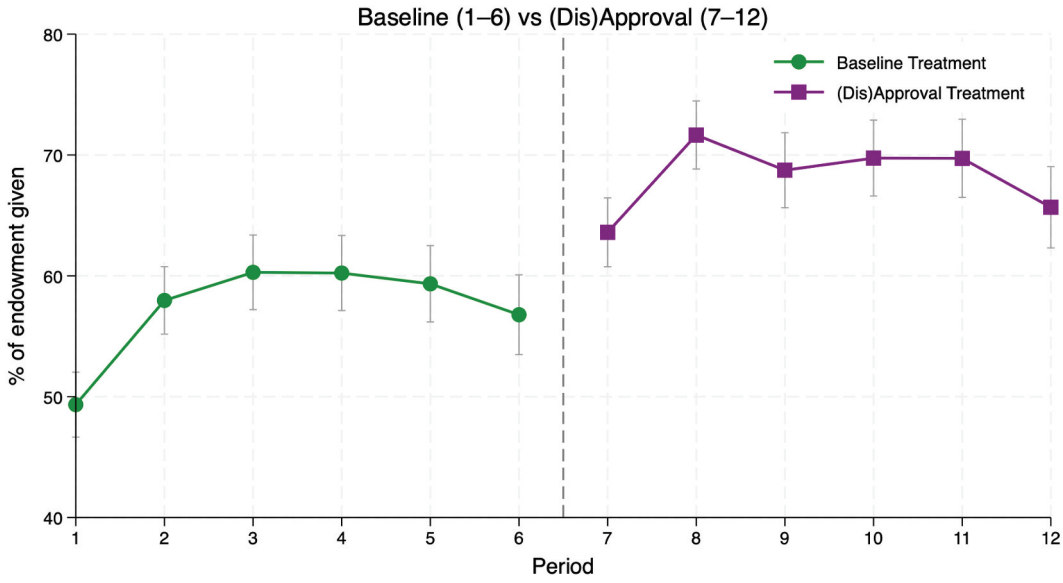

Figure 2. Average contributions by period (baseline vs (dis)approval treatment). The figure displays the average contribution to the public good by periods. Periods 1 to 6 are the baseline, 7 to 12 are the (dis)approval treatment.

behavioural measures such as altruism and willingness to compete. X_i is a vector of individual characteristics, α_i is a random effect at the individual level, and ε_{it} is the robust error term clustered at the group level.

Table 3 shows that both repeated interaction and the introduction of non-monetary feedback significantly increase cooperation ($p < 0.01$), though they explain only a modest share of variance. On average, contributions are 1.37 tokens higher in the (dis)approval treatment, and each additional period adds roughly 0.13 tokens, consistent with Bicchieri et al. (2022), who find that social proximity helps sustain cooperation over time. To further identify the drivers of cooperation, we incorporate behavioural traits and demographics.[5] Including these measures does not alter the main treatment or time effects. Altruism predicts higher contributions in the public good ($p < 0.01$): each token given in the MDG increases cooperation by 0.145 tokens, while competitiveness has no significant effect. Girls contribute on average 0.94 tokens more than boys ($p < 0.01$), and altruistic participants respond more strongly to social pressure ($\beta = 0.035$, $p < 0.05$). These results suggest that altruism and gender shape cooperative behaviour. Possible carryover effects from earlier tasks, however, should be considered when interpreting the relationship between altruism and cooperation.

As a robustness check, we perform panel Tobit estimations with random effects and left censorship at 0, presented in columns 5 to 9 of Table 3. The results closely align with those obtained using the GLS specification. To further explore gender-based differences in cooperation determinants, we conduct a separate analysis by gender, detailed in Appendix 6. The findings reveal a consistent pattern, suggesting that social (dis)approval influences boys and girls similarly. However, a notable distinction emerges: the interaction between the treatment and altruism is positive and statistically significant for boys, indicating that altruistic boys are more likely to increase their cooperation when exposed to social pressure. Additionally, the results show that the increase in cooperation over time is primarily driven by girls ($\beta = 0.184$, $p < 0.01$), whereas the period coefficient for boys is not significant ($\beta = 0.073$, $p > 0.1$). Given the complexity of analyzing interaction terms involving Tournament × Gender × Preferences, we apply the Clarke, Romano, and Wolf (2020) correction to control the familywise error rate-the probability of falsely rejecting at least one null hypothesis in a set of multiple comparisons. The results of this

[5]Although measures of competitiveness and cooperation may not be fully orthogonal, Appendix 8 presents some descriptive statistics that suggest a weak relationship between these variables.

Table 3. Determinants of contribution.

	GLS				Tobit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Period	0.134**	0.134**	0.127**	0.127**	0.118***	0.118***	0.111***	0.111***
	(0.055)	(0.055)	(0.055)	(0.055)	(0.042)	(0.042)	(0.043)	(0.043)
Treatment	1.370***	1.370***	1.408***	1.080**	1.498***	1.498***	1.540***	1.228***
	(0.315)	(0.315)	(0.316)	(0.500)	(0.293)	(0.293)	(0.297)	(0.410)
Altruism		0.145***	0.146***	0.128***		0.157***	0.158***	0.140***
		(0.015)	(0.015)	(0.017)		(0.018)	(0.018)	(0.019)
Tournament		−0.257	−0.156	0.106		−0.335	−0.217	0.057
		(0.398)	(0.389)	(0.456)		(0.437)	(0.440)	(0.464)
Girl			0.938***	0.938***			1.076**	1.076**
			(0.360)	(0.360)			(0.440)	(0.440)
SES			−0.373	−0.373			−0.365	−0.366
			(0.481)	(0.481)			(0.486)	(0.486)
Treatment × Tournament				−0.522				−0.549*
				(0.444)				(0.297)
Treatment × Altruism				0.035**				0.035***
				(0.017)				(0.012)
Constant	9.627***	7.454***	5.836***	6.068***	9.340***	7.070***	5.205***	5.402***
	(0.376)	(0.749)	(0.866)	(1.139)	(0.337)	(0.836)	(1.132)	(1.250)
Observations	5184	5184	5076	5076	5184	5184	5076	5076
Number of id	432	432	423	423	432	432	423	423
Between R^2	0.000	0.155	0.179	0.179				
Within R^2	0.000	0.053	0.053	0.055				
Observations left censored					340	340	339	339
Log likelihood					−15718.0	−15681.2	−15346.1	−15339.9

Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. The dependent variable is the number of tokens given to the public good for each period. The first four estimates utilize GLS with standard errors clustered at a group level, while the last four use panel Tobit estimations with robust standard errors. The sample size consists of 5184 observations, which correspond to the 12 periods for the 432 participants. The table presents eight estimates based on different specifications, although eight observations are missing in columns 3–4 and 7–8 due to incomplete data on participants' socioeconomic background.

adjustment are provided in Appendix 7. These results reinforce that girls exhibit stronger prosocial behavior, suggesting that their responses to social feedback may stem from distinct motivational factors.

Social pressure to increase prosocial behaviours

Figure 3 displays the distribution of feedback types provided by participants in each period, distinguishing between positive feedback only, no feedback, negative feedback only, and both positive and negative feedback. In the initial period, a majority of participants provide both positive and negative feedback (about 58%), while 29% provide only positive feedback, 9% provide only negative feedback, and around 5% provide no feedback. Across subsequent periods, the composition of feedback remains relatively stable, with 'both' and 'positive-only' feedback consistently accounting for the largest shares, suggesting that feedback provision patterns are largely stable over time and do not exhibit strong period-specific trends. The figure further indicates that purely negative feedback remains relatively rare in all periods, whereas the use of positive feedback – either alone or combined with negative feedback – is predominant, in line with previous evidence showing that individuals tend to rely more on rewards than on sanctions (Dickinson 2001; Dickinson, Masclet, and Villeval 2015; Dugar 2013; Sefton, Shupp, and Walker 2007; Sutter, Haigner, and Kocher 2010).

We examine how deviations from a group's cooperation norm shape peer-enforced cooperation by computing, for each participant, the difference between their own contribution and the average contribution of their three peers, where positive (negative) values indicate contributions above (below) the group mean. Figure 4 plots, as a function of this deviation, the probability of assigning non-monetary feedback – rewards (dark grey) or sanctions (light grey) – as well as the corresponding share of positive and negative feedback received. The figure reveals that feedback is primarily targeted at individuals who deviate most from the group norm: participants whose contributions lie close to the group average are rarely sanctioned or rewarded, whereas extreme contributors are substantially more likely to receive feedback. Moreover, the direction of feedback closely tracks the sign and magnitude of the deviation:

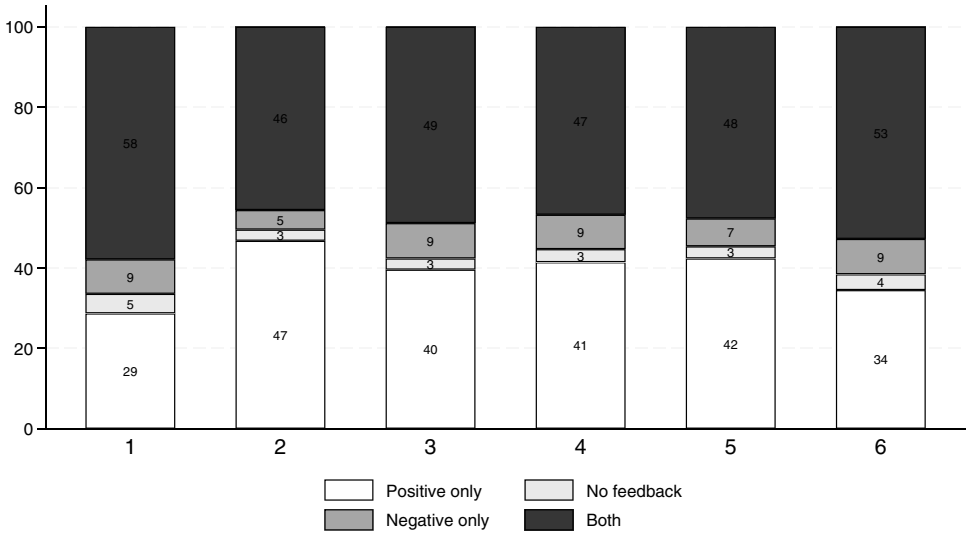

Figure 3. Distribution of feedback types by round. The figure reports, for each round, the distribution of students according to the type of feedback they provided. Categories are mutually exclusive and indicate whether a student provided only positive feedback, no feedback, only negative feedback, or both positive and negative feedback within a given round. Values represent percentages, so that each bar sums to 100%.

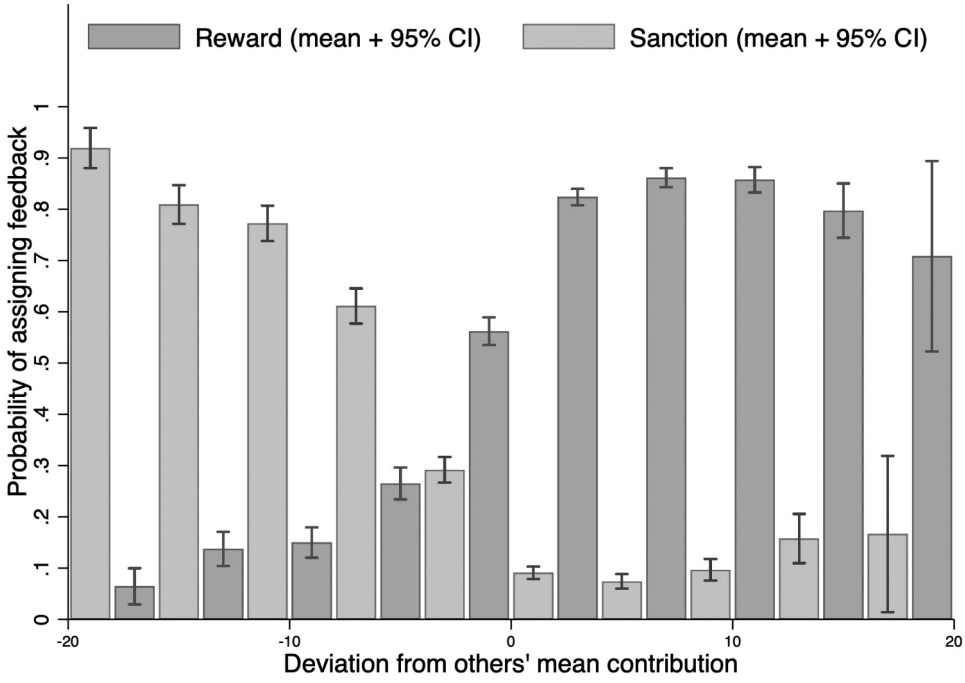

Figure 4. Feedback assignment as a function of deviation from group average contributions. The deviation is computed as $\text{Deviation}_i = C_i - \frac{1}{3}\sum_{j \neq i} C_j$ where C_i is the participant's own contribution and $\frac{1}{3}\sum_{j \neq i} C_j$ is the mean contribution of the three other group members.

free-riders are increasingly likely to be sanctioned as their negative deviation grows, while high contributors are more likely to receive rewards. Importantly, sanctions are not exclusively directed at low contributors, as a non-negligible share of participants engage in antisocial punishment by sanctioning peers who contribute more than the group average, consistent with previous evidence on norm enforcement in public goods settings (Falk, Fehr, and Fischbacher 2005; Herrmann,

Thöni, and Gächter 2008). Estimates for very large positive deviations, however, are imprecise due to the small number of observations in that range and should therefore be interpreted with caution.

III. Assigning social pressure to others

Table 4 reports random-effects probit estimates of the likelihood of giving non-monetary incentives-rewards (columns 1–4) and sanctions (columns 5–8). The dependent variable equals one when player i gives an incentive to player j in period t within group k:

$$P_{itk} = \beta_0 + \beta_1.\text{period}_t + \beta_2.\text{Abs.Dev}_{jt} + \beta_3.\text{Oth.Avg}_t + \beta_4.\gamma_i + \beta_5.X_i + \beta_6.X_k + \omega_i + \varepsilon_{it}$$

Standard errors are clustered at the group level, with a random effect ω_i accounting for repeated observations. The model includes each player's deviation from the group average contribution (Abs.Dev) and the group's overall cooperation level to capture responses to peers' behaviour. The group average is computed as the mean contribution of all other group members, while absolute deviation is defined as: $\max\left(C_j - \left(\sum_{i=1}^{4} C_i - C_i\right), 0\right)$. Behavioural traits (altruism, competitiveness) and demographics (gender, socioeconomic status) control for heterogeneity in individuals' propensity to administer rewards or sanctions, reflecting variation in sensitivity to social context and feedback.

Table 4 indicates that incentive allocation primarily reflects relative contribution levels. A one-point positive deviation from the group average raises the probability of receiving a reward by 2.8% ($p < 0.01$) and lowers the likelihood of sanctions by 2%, whereas a one-point negative deviation decreases reward probability by 4.2% and increases punishment likelihood by 3.8% ($p < 0.01$). Rewards are also more frequent in groups with higher overall cooperation (2.7% per point, $p < 0.01$), while sanctions are concentrated in less cooperative environments. These effects remain robust to controls for behavioural and demographic characteristics, suggesting that peer-enforced incentives are tightly aligned with deviations from group norms. Individual traits add further nuance: altruism and competitiveness have limited influence on rewarding behaviour, but participants selecting the tournament payment scheme are more likely to punish peers ($\beta = 2.7\%$, $p < 0.05$), consistent with a link between competitiveness and punitive tendencies. Socioeconomic status also matters, as higher-SES individuals are more prone to punish ($\beta = 3.5\%, p < 0.05$) and less likely to reward

Table 4. Probability to assign social pressure.

	Rewards				Sanctions			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Positive deviation from others average	0.028***	0.028***	0.027***	0.027***	−0.020***	−0.019***	−0.019***	−0.019***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Negative deviation from others average	−0.042***	−0.042***	−0.042***	−0.042***	0.038***	0.038***	0.038***	0.037***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Other's average	0.027***	0.027***	0.026***	0.026***	−0.022***	−0.021***	−0.021***	−0.021***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Altruism		0.001	0.001	0.001		−0.001*	−0.001	−0.001
		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
Tournament		0.017	0.018	0.013		0.024*	0.025*	0.027**
		(0.019)	(0.019)	(0.019)		(0.013)	(0.013)	(0.014)
Gender			0.008	0.007			−0.002	−0.002
			(0.017)	(0.017)			(0.014)	(0.014)
SES			−0.042**	−0.043**			0.033**	0.035**
			(0.019)	(0.019)			(0.016)	(0.016)
Rewards received in $t-1$				0.010				0.008
				(0.011)				(0.010)
Sanctions received in $t-1$				0.001				0.006
				(0.013)				(0.010)
Observations	7776	7776	7614	7191	7776	7776	7614	7191

Significance levels: ***0.01 **0.05 *0.10. This table reports the marginal effects obtained from panel probit estimates with random effects and standard error clustering at the group level. Random effects account for lack of independence between observations. The dataset includes 7776 observations of 432 participants, who made decisions on non-monetary sanctions and rewards for each group member (3) across six treatment periods. The missing observations in columns 3 and 7 are due to the absence of SES information for 9 participants ($9 \times 3 \times 6$). The difference in the number of observations (423) between columns 4 and 8 is due to the unavailability of data to calculate the lag of rewards/sanctions in the first period of the disapproval treatment.

($\beta = 4.3\%$, $p < 0.05$), possibly reflecting differential exposure to norm-enforcement contexts. No evidence suggests that prior rewards or sanctions shape subsequent feedback, indicating that social incentives are primarily contemporaneous responses to deviations in cooperative behaviour.

We acknowledge that the assignment of rewards and sanctions is not statistically independent, potentially biasing previous estimates. While our preferred probit model accounts for many aspects, it does not fully capture the symbolic order of choosing between rewards, inaction, or punishment. A panel ordered logit model, presented in Appendix 9, yields similar results. A multinomial logit approach could further refine the analysis, but the assumption of independence of irrelevant alternatives is unlikely to hold, given the interdependence of (dis)approval choices. Additionally, a gender-specific analysis in Appendix 10, indicates that both genders respond similarly to deviations to group's cooperation standard: positive deviations are more frequently rewarded, while negative deviations are less likely to be punished. However, the propensity to apply social pressure varies with individual traits and demographics. Altruistic boys are less inclined to punish others ($\beta = -0.1\%$, $p < 0.01$), whereas competitive girls are more likely to impose sanctions than those opting for a flat wage remuneration scheme ($\beta = 4.3\%$, $p < 0.05$). Socioeconomic background also plays a role, as boys from high-SES backgrounds are more prone to sanctioning ($\beta = 6\%$, $p < 0.01$) but less inclined to reward peers ($\beta = 5.7\%$, $p < 0.01$), suggesting differential exposure to environments where punishment is emphasized as a norm enforcement mechanism.

Contribution after receiving sanctions and rewards

We test whether social feedback affects individual cooperation by examining changes in contributions between consecutive periods, where social pressure is proxied by the number of non-monetary rewards and sanctions received from group members. Because the probability of receiving feedback is correlated with prior contributions, these estimates should be interpreted descriptively rather than causally. Table 5 reports results from a GLS model with individual random effects:

$$c_i^{t+1} - c_i^t = \beta_0 + \beta_1 . \left(\sum_k R_{ki} \right) + \beta_2 . \left(\sum_k S_{ki} \right) + \beta_3 (c_i^t - \bar{c}^t) + \beta_4 . \text{period}_{t+1} + \epsilon_{i,t+1}$$

Here, $c_i^{t+1} - c_i^t$ denotes the change in contributions between periods t and $t+1$. The coefficients β_1 and β_2 capture the effects of rewards and sanctions, respectively, β_3 controls for deviations from the group mean contribution at period t, and β_4 accounts for time effects. Robust standard errors are clustered at the group level. We first jointly estimate this specification on the overall sample and then separately on individual relative contributions to group cooperation. High contributors are defined as those whose contribution in period t exceeds the group average $\bar{C}^t$. (Table 5 - col (2) - (5)), while low contributors are those whose contribution is below the group average (columns 3–6). This approach allows for the identification of relative contribution levels for each individual and period (Chaudhuri 2011).

Table 5 provides no evidence that teenagers adjust their contributions in response to social feedback received in the previous round. Columns (1) and (4) show no consistent pattern, although individuals who received two ($\beta = -1.557$, $p < 0.05$) or three ($\beta = -1.425$, $p < 0.05$) rewards subsequently reduce their contributions. This likely reflects that rewards serve as a weak cooperative signal or that highly rewarded individuals-already strong contributors-naturally regress towards the group mean over time. Overall, the intensity of peer incentives appears to have little effect on behavioural change, consistent with Lergetporer et al. (2014), who show that anticipated feedback matters more than its realization. Conversely, group dynamics exert a strong influence: a one-point increase in average group contribution raises individual contributions by 0.113 ($p < 0.01$), indicating positive spillovers in cooperative behaviour. Splitting the sample by initial contribution levels confirms that neither sanctions nor rewards significantly affect subsequent cooperation for high or low contributors, though the latter primarily drive the group-average effect, suggesting that low contributors adjust their behaviour downward when overall cooperation rises.

Table 5. Determinants of changes in contributions between t and $t+1$.

	Linear			Non-linear		
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	High contrib.	Low Contrib.	Overall	High contrib.	Low Contrib.
Sanctions received t	0.212	−0.249	0.222			
	(0.223)	(0.289)	(0.333)			
Rewards received t	−0.234	−0.251	−0.250			
	(0.203)	(0.279)	(0.302)			
Other'saverage$_t$	0.113***	0.039	−0.279***	0.113***	0.038	−0.279***
	(0.030)	(0.048)	(0.076)	(0.030)	(0.047)	(0.076)
Period	−0.614***	−0.518***	−0.597***	−0.767	−0.439	−1,349*
	(0.105)	(0.118)	(0.137)	(0.520)	(0.496)	(0.707)
Any sanction received in t (Sanction = 0)				ref.	ref.	ref.
1				−0.353	−0.638	−0.758
				(0.446)	(0.641)	(0.698)
2				−0.432	−1,230	−0.634
				(0.611)	(0.950)	(0.894)
3				0.214	−0.964	0.040
				(0.760)	(1,082)	(1,069)
Any reward received in t (Reward = 0)				ref.	ref.	ref.
1				−0.802	−0.322	−1,349**
				(0.524)	(0.784)	(0.652)
2				−1,557**	−1,322	−1,246
				(0.686)	(1,088)	(0.848)
3				−1,425**	−1,204	−1,823*
				(0.668)	(1,050)	(1,008)
Constant	1,561**	−0.470	9,823***	3,438	−0.283	15,593***
	(0.775)	(1,056)	(1,529)	(3,127)	(2,856)	(4,583)
Observations	2160	1153	1007	2160	1153	1007
Number of id	432	368	345	432	368	345
Between R^2	0.021	0.010	0.067	0.021	0.015	0.090
Within R^2	0.032	0.032	0.066	0.036	0.034	0.066

Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. The table presents estimates based on GLS with robust standard errors clustered at the group level, incorporating random individual effects. The sample includes 432 individuals observed over five periods, excluding the first period due to lag variables. High contributors are those who contribute above the group average, while low contributors are those who contribute below the group average. This categorization is computed for each period, potentially resulting in changes between categories across periods. The dependent variable measures changes in contributions between two periods. The first four columns test linear effects of incentives, whereas the last four explore non-linear effects.

To assess the robustness of these findings, we employ an alternative approach by analysing changes in contributions between two periods based on the variation rate (see Appendix 12). The results remain consistent across both specifications. Additionally, we conduct a heterogeneity analysis based on pupils' gender (see Appendix 13–14). The results reveal significant gender differences in the impact of receiving social pressure. For high contributing boys, both sanctions ($\beta = -0.909$, $p < 0.1$) and rewards ($\beta = -0.803$, $p < 0.1$) received in the previous periods tend to decrease contributions. Conversely, girls increase their contributions when they are sanctioned (0.554, $p < 0.1$), with this effect being observed primarily among low contributors (0.834, $p < 0.1$). Accounting for the initial contribution rate, we find a pattern in line with previous evidence from the literature, showing that sanctions increase the contributions of boys (0.208, $p < 0.1$) and girls (0.071, $p < 0.1$).

Cooperative response to (dis)approval: matching approach

We analyze whether the type of symbolic peer feedback influences cooperation by comparing students who exhibited similar behavior during the baseline and Period 7 but received distinct feedback (mostly positive vs. mostly negative smileys). We first present descriptive evidence on individual contribution changes between Period 7 (the last of the baseline, where contributions were unaffected by treatment) and Period 8 (the first treatment period, where sanctions could be applied), relating these changes to the type of feedback received. Figure 5 reveals strong heterogeneity in behavioural responses: positive feedback does not consistently raise contributions, nor does negative feedback systematically reduce them, although participants appear somewhat more responsive to disapproval, with higher median increases among those receiving more

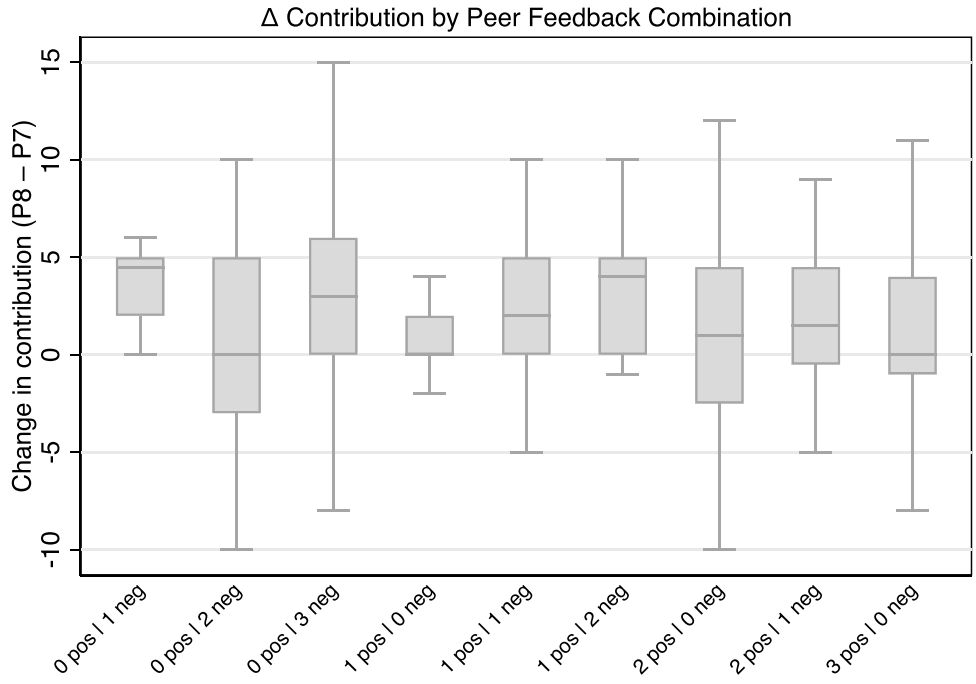

Figure 5. Variation in contribution change by peer feedback. The figure shows the distribution of changes in contributions to the public good game between periods 7 and 8 across all combinations of symbolic feedback received in period 7.

negative smileys. To approach causality, we restrict the sample to students with comparable prior behaviour and group environments, estimating propensity scores based on individual and group contributions in periods 1–7 and matching treated students (more than two rewards) with controls (at least two sanctions). Balance diagnostics in Appendix 11 confirm similarity across covariates. Regression results (Table ST-11) show small, insignificant effects of feedback type on subsequent contributions, suggesting that symbolic peer feedback is too weak to alter cooperation once norms are established. By period 7, contribution patterns appear stable, and heterogeneous reactions-some increasing effort after disapproval, others withdrawing-likely offset each other on average.

Exposure to early group behaviour and feedback heterogeneity

While our main identification relies on within-subject variation before and after the introduction of symbolic feedback, the random assignment of students to fixed groups of four enables an exploratory analysis of peer effects. Because group composition in round 1 was randomized, the initial peer environment can be considered exogenous. We classify students by the mean contribution of their three groupmates in round 1 and split this at the median into 'high-' and 'low-cooperation' environments, then track individual contributions across the baseline (periods 2–6) and feedback phases (periods 7–12) to assess whether early exposure to cooperative peers fosters higher subsequent giving. Figure 6 displays the difference in mean contributions between the two environments for girls and boys.[6] Differences are small and imprecise in the baseline phase; after feedback begins, girls initially exposed to more cooperative peers contribute slightly less, while boys show a modest, non-significant increase. Overall, early peer cooperation appears insufficient to shape later behaviour, likely because first-round actions provided a weak and quickly outdated signal. Sustained reciprocity or norm-following likely requires repeated cooperative interactions, whereas here contributions adapted to evolving group dynamics and direct

[6]The gender-specific analysis builds on the intuition that boys and girls may respond differently to exposure to cooperative peers. The average effect can be visually recalculated in Figure 6, while Appendix 12 reports the corresponding analysis for the overall sample.

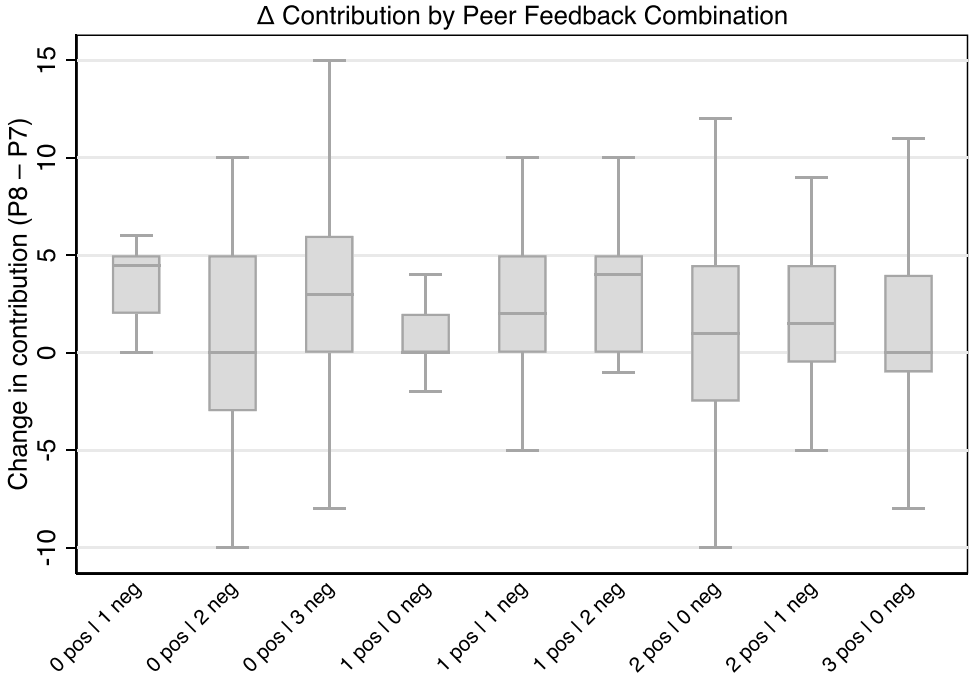

Figure 6. Exposure to peers and contribution by gender. This figure plots the difference in average contributions (High – Low peer exposure) for each period and gender. Positive values indicate that participants exposed to more cooperative peers contributed more than those exposed to less cooperative peers in the same period and gender group.

feedback. The coarse high/low classification and offsetting reactions-some reciprocating, others free-riding-may have further masked any effect.

IV. Discussion & conclusion

This study examines how horizontal, non-monetary feedback influences adolescent prosocial behaviour. Adolescence, a period of strong social pressure and identity formation, provides a crucial setting for analysing social pressure and cooperation. Using a controlled public good game, the study tests whether social pressure increases prosocial behaviour by allowing adolescents to enforce cooperation through symbolic sanctions and rewards. By introducing an exogenous shift in social feedback availability, it isolates the immediate impact on peer interactions. Results show that social pressure boosts cooperation by 10.86 pp, with altruistic boys being particularly responsive. Adolescents predominantly rewarded high contributors and penalized free-riders, reinforcing cooperation through social pressure. This study demonstrates that peer-enforced norms sustain cooperation among adolescents, even in the absence of material incentives.

Our measure of prosociality-costly contributions to a shared fund in a repeated public good game-captures a specific, strategic form of cooperation characterized by anonymity, group-based interaction, and abstract decision-making. This setting emphasizes norm compliance and fairness under uncertainty, differing from more relational or emotionally driven forms of prosocial behaviour such as helping or sharing. Consequently, observed patterns-such as the effectiveness of symbolic feedback, higher baseline cooperation among girls, and stronger responsiveness among altruistic boys-likely reflect the cognitive and structural features of the task. These dynamics may not fully generalize to contexts where prosocial behaviour is shaped by emotion, reputation, or interpersonal ties. More broadly, while our design isolates the effects of symbolic feedback, real-world peer interactions involve additional social and cultural dimensions. Future research should extend this analysis by incorporating diverse forms of social pressure, including identity and group dynamics, and by combining experimental and qualitative approaches to capture underlying mechanisms. Longitudinal studies would also be valuable to assess the persistence and long-term impact of social pressure on adolescent cooperation.

To improve the external validity of the results, gathering information on pre-existing social relationships among participants would be beneficial, allowing for a deeper examination of peer pressure dynamics. Previous studies have shown that students' networks are highly correlated with exposure to different peers (Lavy and Sand 2019). Further investigations comparing behaviour in controlled environments with real-world scenarios would shed light on the role of controlled environments in analysing complex social interactions. These insights are crucial for the development of effective mechanisms and interventions aimed at fostering positive social norms and behaviours among adolescents.

Author contributions

CRediT: **Etienne Dagorn:** Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing.

Disclosure statement

No potential conflict of interest was reported by the author(s).

Funding

The work was supported by the French State under the France-2030 programme and the Initiative of Excellence of the University of Lille [R-CDP-24-006-DePERU]; Region Ile-de-France [EX061002 - 21010352]; Caisse des Dépots [PIA3]; and by the Region of Brittany.

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