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Lies have long legs cheating, peer scrutiny and loyalty in teams

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ABSTRACT

Groups affect cheating in a variety of ways. While it is typically assumed that third party scrutiny decreases cheating, there is scarce evidence as to whether members of a team cheat more or less if their individual actions are disclosed to their peers. To fill this gap, we run a lab-in-the-field experiment with boy and girl scouts during their summer camps. Scout troops are organized into patrols: these are naturally occurring and persistent groups that own common goods and are very different from the minimal groups typically used in lab experiments. While we find a very low overall level of cheating, our results show that disclosure to peers induces more cheating. In a follow-up experiment, we are able to replicate this finding within a population of students of the same age. Our results are somehow in contrast with other studies showing that hierarchical scrutiny decreases cheating but are aligned with ample evidence from different social science fields on the adverse effects of peer interactions among adolescents. Finally, our findings suggest that this adverse peer effect is independent from whether cheating rewards the team or the individual.

> "Why do you laugh?" the Marionette asked her, worried now at the sight of his growing nose. "I am laughing at your lies." "How do you know I am lying?" "Lies, my boy, are known in a moment. There are two kinds of lies, lies with short legs and lies with long noses. Yours, just now, happen to have long noses." Pinocchio, not knowing where to hide his shame, tried to escape from the room, but his nose had become so long that he could not get it out of the door.
> Collodi, Carlo. *The Adventures of Pinocchio*, 1882

1. Introduction

Some of the most famous fairy tales teach children not to cheat. The story of Pinocchio, for instance, tells of a wooden puppet whose nose grows every time he lies. In his adventures Pinocchio meets false friends like the Fox, the Cat, and Candlewick who draw him into cheating more, as well as good mentors, like Jiminy Cricket and the Fairy with Turquoise Hair, who want him to be a force for good. This story conveys the message that one's company may play a central role in shaping the decision to cheat: Pinocchio's coming of age is a constant struggle between positive mentoring relations with adults who teach him that lies have short legs – nobody can run far with them without being caught –, and detrimental relations with peers who instead lure him into thinking that lies have long legs.[5]

It goes without saying that cheating is a widespread phenomenon, among both adults and young people, and we do not need Collodi's imagination to find examples of corruption, corporate scandals, tax

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[5] Theodor Adorno used the expression "lies have long legs" when discussing – very much ahead of his time – the relation between authoritarianism and fake news (Adorno, 2005). Thanks to Alan Miller for the insight.

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Table 1
Design of the main experiment.

		Within-subject (*scrutiny effect*)	
		Private decision (*yellow task*)	**Disclosed decision** (*blue task*)
Between-subject (*loyalty effect*)	**Individual payoff**	Private & Individual (80 obs)	Disclosed & Individual (80 obs)
	Team payoff	Private & Team (80 obs)	Disclosed & Team (80 obs)

evasion, and fraud that impose heavy burdens on the society. In many such circumstances the decision to cheat is taken in groups, where the individual is subject to peer scrutiny, and where cheating benefits the group. Lucifora & Tonello (2015) provide a vivid example of one such circumstance: students colluding during a national learning evaluation program. In this setting, students cheat by exchanging information and by cooperating in order to improve class achievement.

In our paper we study, by means of two lab-in-the-field experiments, how disclosing the decision to cheat to peers within a team affects the individual decision to cheat. We address this research question by manipulating whether this decision is disclosed to the other members of the team. With a second manipulation, we also study if the scrutiny effect depends on whether the individual or the team as a whole benefits from the individual's decision to cheat.

Previous research on cheating behavior in the presence of others has produced seemingly contradictory results: on the one hand, when the decision to cheat is *shared* with someone else – that is, when it is a collective decision – cheating appears to increase (see Kocher et al., 2018 and other studies reviewed in the next section); on the other hand, *disclosing* the decision to cheat to someone else seems to decrease cheating (Gneezy et al., 2018; Ostermaier and Uhl, 2017; Van de Ven and Villeval, 2015). This latter result is typically obtained under hierarchical third-party scrutiny; however, to the best of our knowledge, there are no contributions that analyze, in an experimental setting, whether members of a team cheat more or less if their individual actions are disclosed to peers. This gap is noteworthy, as there exists a wealth of literature in social and developmental psychology, as well as criminology and sociology, that has studied the complex effects of peer interactions on antisocial behavior (Monahan, Steinberg, & Cauffman, 2009; Brechwald & Prinstein, 2011), especially in adolescents and young adults. For instance, Gordon et al. (2004) find that delinquency appears to be a mechanism of socialization between peers; even more closely related to our study, Juvonen & Ho (2008) show that mentioning as "cool" peers who engage in antisocial behavior is a predictor of students' later engagement in such behavior. We fill the existing gap on cheating under peer scrutiny in the experimental literature by bringing this peer interaction to the lab, studying the effect of peer disclosure among adolescents who are members of a same team, after having each of them individually taken a decision to cheat.

An important factor that might ambiguously affect the impact of peer scrutiny on the decision to cheat is the degree of loyalty in the team. In general, loyalty imposes acting in the best interest of the group, but this can foster unethical behavior in close groups or, instead, trigger moral attributes and cultural scripts that in the end prompt team members to act more ethically (Hildreth et al., 2016). Again, there is no obvious prediction on the effect of peer scrutiny on the individual decision to cheat in loyal teams. In Section 3 we develop a model that captures these ambiguities and provides predictions in both directions.

Together, our two lab-in-the-field experiments innovate along two main lines with respect to the previous literature: i) in our design the decision to cheat is disclosed to teammates and, ii) we employ naturally occurring, persistent teams where loyalty is already highly salient. These two features allow us to answer the following research questions: i) how is the level of cheating affected when a subject's decision to cheat is exposed to peer scrutiny, and ii) does this decision depend on whether cheating benefits the team as opposed to the individual?

Our main experiment was conducted with girl and boy scouts, aged 12–17, during their summer camps. Our follow-up experiment was conducted in schools, with students of the same age. The main aim of this second experiment was to verify the robustness of some of our findings. We designed our experimental protocol inspired by the coin task proposed by Bucciol and Piovesan, 2011, but with the added possibility to selectively disclose the cheating behavior ex-post. In our version of the task, subjects made two simultaneous extractions out of two urns ("yellow" and "blue"), each of which contained two possible outcomes: € 0 or € 10.[6] Subjects had to record the two drawn amounts respectively on a yellow squared card, to be kept private, and on a blue squared card, which was disclosed to the team at a later stage together with the original amount drawn from the urn. This was our main treatment dimension: only one of the two choices was actually paid, depending on the toss of a coin that took place at the end of the experiment. The other treatment dimension concerned whether the payoffs reported on the blue or the yellow squared cards were actually paid out with individual coupons or with team coupons. In our main experiment, coupons were to be spent at the local scouting store, selling both goods for private, individual use (e.g., backpacks, uniforms, sleeping bags) or goods for public, collective use (e.g., tents, pots, and other team equipment). In the follow-up experiment, coupons were to be spent at a local bookstore.

The scout population used in the main experiment is interesting for a number of reasons. First, scout troops are organized into patrols that compete with each other in many activities during the year and that collectively own assets. Thus, patrols are natural teams, quite different from the minimal groups that are usually created in the lab for these kinds of experiments, and more similar instead to work teams inside firms, sports teams, and the multitude of different groups characterizing civil society. Second, while the literature on cheating is accumulating quickly, only limited research has been conducted on the adolescent age group. However, analyzing the above-mentioned research questions within this population is important: adolescence is one of the crucial phases in personality development, during which most of the behaviors that will be maintained throughout a person's lifespan are formed (Gervais et al., 2000). It is also worth mentioning that the world scout movement is one of the largest youth organizations in the world (see Appendix B). Third, scouts attribute so much importance to honesty and loyalty as to include them as the first and second items in their law, respectively.[7] It should be noted that the law neither specifies the group, nor the group's interest which the scout shall be loyal to. In what follows, by *loyalty effect* we refer to the propensity to act in the patrol's best interest. The fact that the law includes an explicit reference to honesty and loyalty, makes the decision to cheat even more salient.

Indeed, results from our main experiment show that in the baseline treatment (with no disclosure and with individual payments) there is basically no cheating at all; this is confirmed in the follow-up experiment. Treatment manipulations, however, produce interesting results: on the one hand subjects cheat significantly more when their decision is disclosed to their team, both in the main and in the follow-up experiment. On the other hand, the possibility of cheating to favor the team's payoff, analyzed in the main experiment, does not alter the decision to report the true outcomes. In other words, we do observe a significant

[6] Subjects made a single extraction from an envelope containing the four possible combinations of outcomes (see Section 4 for details).

[7] 1) A scout's honor is to be trusted and 2) A scout is loyal (see Appendix B).

scrutiny effect, but no *loyalty effect*, i.e. cheating does not depend on whether the payoff goes to the individual or the team. This novel evidence contrasts with previous findings on the role of hierarchical scrutiny, but can be easily reconciled with the accumulated evidence that adverse peer-effects are often observed in groups, especially at this young age.

The remainder of this paper is organized as follows: Section 2 reviews the existing literature, Section 3 presents a theoretical model and its predictions, which are tested experimentally as described in Section 4. The results of the analysis are presented in Section 5, and Section 6 concludes the study.

2. Literature review

Since the early papers of Gneezy (2005); Sánchez-Pagés & Vorsatz (2007), works on individual cheating behavior have flourished.[8] Several authors have focused on cheating as a social phenomenon, exploring the role of groups in determining unethical behavior. One consistent finding in this body of literature is that group interactions determine more cheating than individuals deciding alone. In what follows, we briefly review the different channels behind this phenomenon with the additional aim to highlight our original contribution with respect to the existing literature.

Perhaps the first paper studying deception in teams is Sutter (2009). In his experiment, groups had to discuss a potentially deceptive signal and make a collective decision about it. Sutter was interested in observing individuals explicitly reasoning about the motives of their decisions (all team discussions were recorded). Incidentally, in this experiment, groups sent the deceitful message less often than individuals, but only as a result of "deception through telling the truth" due to sophisticated team reasoning (Sugden, 2011). Cohen et al. (2009), Kocher et al. (2018) confirm this result. Unlike these studies, in our experiment subjects make decisions alone and these decisions are only disclosed ex-post to the group. More similar to our design is the study by Weisel & Shalvi (2015), who implement a task where pairs of subjects can collaboratively cheat to the benefit of both: as in our experiment, they each take an independent decision, but while we implement a cheating game which is perfectly separable across individuals, in their design the two decisions are combined in order to determine the outcome. Moreover, in their experiment, no disclosure of cheating takes place among participants. Another common finding reported in the literature is that lying increases if the cheater can hide in the crowd. Conrads, Irlenbusch, Rilke, & Walkowitz (2013) ran an experiment in which subjects had to report the privately observed outcome of a dice roll. It was found that more cheating happened when subjects reported in pairs (in this case, they reported the sum of the two outcomes) than when they reported alone. Again, this effect is not applicable to our design, in which experimenters always observe the outcomes of individual decisions, even when the final payment is aggregated at the patrol level. Previous contributions also pointed out that observing other people cheating increases the probability of cheating (Brunner & Ostermaier, 2019). Gino, Ayal, & Ariely (2009) focus on the effect of observing cheating as performed by people belonging to the same group, or to a different group. In particular, they ran an experiment in which many subjects simultaneously undertook a task in which cheating would spare effort. One person, who was wearing the university t-shirt (in-group) but was actually a confederate, ostensibly did just that. This increased cheating by all the other group members. However, cheating decreased when the confederate wore the t-shirt of a rival university. It should be noted that this contagion effect could not happen in our experiment, as subjects took their decisions in isolation within a tent.

Some papers have introduced hierarchical scrutiny by the experimenter (Mazar, Amir, & Ariely, 2008; Gneezy et al., 2018) or by a third-party bystander (Van de Ven & Villeval, 2015). In the first case cheating decreases while in the second case it does not change (but it certainly does not increase). In Houser, List, Piovesan, Samek, & Winter (2016), the presence of their child induces parents to cheat less. Pascual-Ezama, Dunfield, de Liaño, & Prelec (2015) run a cheating task and manipulate both the presence of other students in the same room and whether the reported outcomes are handed in directly to the experimenter, are stacked up on a pile, or are directly shredded. They find that both manipulations curb cheating. However, in this case the others are not members of a team, and they do not observe the decision to cheat but only the reported outcomes. Similarly, Ostermaier and Uhl (2017) observe that when subjects reveal publicly their reported outcomes (but not the actual outcome of the die-roll), cheating is lower. In our treatment with disclosure, both the actual and the reported outcomes can be observed by peers in each team and this results in increased cheating.

Our second manipulation concerns the presence of team payoffs. Previous studies on advantageous lies have shown that cheating increases when the lie positively affects both the subject's own payoff as well as the payoff of others, whether they be strangers or members of the same group (Gino & Pierce, 2010; Wiltermuth, 2011; Erat & Gneezy, 2012; Shalvi & Leiser, 2013; Weisel & Shalvi, 2015). Gino et al., 2013 showed that this effect occurs both because the presence of other beneficiaries offers to subjects an easy justification for their dishonesty, and also (to some extent) because subjects care about the potential spillovers of their actions on others. For instance, Houser et al. (2016) show that parents cheat significantly more to benefit their children than to benefit themselves. In our design, cheating increases either the team's collective coupon or the individual coupon, but never both at the same time. *Prosocial lies*, in which cheated subjects benefit from being cheated, have also been studied experimentally (Levine & Schweitzer, 2014; 2015). This kind of lies introduces a tension between honesty and benevolence which often results in both the deceiver and the deceived perceiving lying as acceptable.

Hildreth et al. (2016) focus on the ambivalent role of loyalty to a group in determining cheating behavior: on one extreme, loyalty characterizes the behavior of members of close-knit groups often associated with strict codes of silence, cronyism and parochialism, to the point that group norms and interests prevail over general interests and laws. At the other extreme, loyalty is part of a set of moral values that people embrace, pledge to and promote, and that strongly relate to other virtues such as honesty, humility, benevolence, and ethical behavior in general.[9] In their paper, the authors use both minimal groups as well as natural groups, in the form of student study groups and fraternities,[10] and they manipulate loyalty via group discussion and loyalty pledges.[11] They show that loyalty generally has a positive effect on ethical behavior. We have already mentioned the importance of loyalty to boy scouts. In fact, the Scout Law presents loyalty as a *general* virtue (i.e., the recipient is left implicit). In our experiment we look at loyalty from a different angle than Hildreth et al. (2016): rather than making it more or less salient, we allow subjects, with their decision to cheat, to either

[8] This literature have been recently reviewed by Abeler, Nosenzo, & Raymond (2018).

[9] See Bruni & Sugden (2013) for a discussion of the role of virtues in economics and Hildreth et al. (2016) for a discussion of the virtue of loyalty in particular.

[10] Hildreth et al. (2016) conduct the experiment among three fraternities, comprising 89 subjects. Using scout patrols allows us to observe a larger number of teams (31 in our case, comprising 160 subjects), which also tend to be homogeneous (in terms of social status/wealth), at least within the same troop. Kocher et al. (2018) run their experiment in adolescent classrooms and, in one of the treatments, allow students to endogenously team-up in groups of three (in the other groups' treatment students are randomly assigned to three-person groups).

[11] See also Jacquemet, Luchini, Rosaz, & Shogren (2019) who use a public oath manipulation to study individual truth-telling behavior in the lab.

benefit themselves alone or their team as a whole.

Our scouts also share the same religious (Catholic) beliefs. The role of religious beliefs in cheating behavior has been explicitly explored in two papers: Utikal & Fischbacher (2013) show that a group of nuns performing an individual cheating task is willing to make disadvantageous lies to appear honest; Shalvi & Leiser (2013), conducting an individual cheating experiment with two populations of female students at a secular and a religious university campus in Israel, find no evidence of lying among religious students, but a positive amount of lying among secular students.

Our subjects in both the main and the follow-up experiment are adolescents aged between 12 and 17. It is known that social preferences and personality traits develop from childhood and it is easy to expect cheating behavior to change with age as well. However, there is only a limited amount of literature studying deception with non-adult subjects and the evidence so far is inconsistent. Bucciol and Piovesan (2011) find that children (aged 5–15) lie when they have the opportunity to do so, but tend to be honest when someone reminds them that lying is not good: cheating is however uniform across age. Instead, Korbel (2017) finds that cheating increases with age. Interestingly, this latter paper also manipulates the group dimension and allows the cheating decision to be taken alone or in group, and finds that groups cheat more in both age groups (11–13 and 14–16). Two more papers obtain the opposite result: Glätzle-Rützler & Lergetporer (2015) find that, among children (aged 10 or 11) and early adolescents (aged 15 or 16), an aversion to lying is widespread and the propensity to lie decreases significantly with age. Similarly, Maggian & Villeval (2016), by analyzing a sample of children aged 7–14, find that older children lie less than younger ones. Our results are aligned with these latter findings.

Our paper also speaks to the vast literature, from psychology, sociology, criminology, and law and economics, that has studied the effects of peer interactions and social influence on behavior. It is hard to argue that decision making in everyday life happens in a social vacuum and, indeed, the scientific exploration of social influence on judgment and decision making traces back to the 1950s (Large, Pellicano, Mojzisch, & Krug, 2019). In light of the robust literature on self and social image concerns (Ariely et al., 2009; Bénabou and Tirole, 2006; Tonin and Vlassopoulos, 2013), it is unsurprising that peer scrutiny might affect individual behavior. Social influence affects decision making from childhood onwards (Haun & Tomasello, 2011) but reaches its maximum effectiveness during early adolescence and the transition to adulthood (Knoll, Magis-Weinberg, Speekenbrink, & Blakemore, 2015). Early adolescents and adolescents rely on their peers to understand what behavior is acceptable and desired and what is not, at least in a peer context (Brechwald & Prinstein, 2011). Furthermore, they usually engage in behaviors that might be risky but can enhance their self-image such as substance abuse (Glaser, Shelton, & van den Bree, 2010; Tomé, de Matos, Simões, Camacho, & AlvesDiniz, 2012), risky sexual behaviour, criminal behavior, and acceptability of violence (Buehler, 2006; Padilla-Walker & Bean, 2009). The adverse effect of peer influence is usually driven by the adolescent search for social approval (Brechwald & Prinstein, 2011). There is also emerging evidence of the neurobiological foundations of social influence: for instance, Berns, Capra, Moore, & Noussair (2010), using functional magnetic resonance imaging (fMRI) on a sample of adolescents aged 12–17, showed that a song's popularity among peers generated anxiety and desire for conformity, thus influencing one's own appreciation of the songs.

3. Theoretical framework

Generally speaking, we are interested in modeling individuals' decisions to truthfully report the drawn outcome. In our setting, the presence of the team can influence the decision of the individual along two dimensions: i) the existence of *peer scrutiny* - the individual decision might or might not be observed by the team - and ii) the *payoff recipient* - the outcomes might be paid to the individual or be cumulated in a collective payment to the team. We develop a model that, while not specifically investigating the psychological factors behind the decision of whether to cheat, clearly identifies the observable dynamics that our experiment is designed to detect.

In the simplest setting, the individual decision remains private and only produces an individual payoff; following Gneezy et al. (2018), we model the problem of deciding whether to cheat as a general trade-off between the monetary returns of cheating and the non-monetary ethical cost of doing so.

Assuming that the cost of lying is linear in the cheated amount, the resulting utility can be written as:

$$u(m_d) = m_d - \ell(m_d - m_t)$$

where $m_d - m_t$ is the difference between the reported amount and the observed amount; the cost of lying is represented by the coefficient ℓ times the net cheated amount.[12] We normalize m_t to 0 to obtain $u(m_d) = m_d - \ell m_d$.

The first extension of this model consists in allowing for peer scrutiny, i.e. the ex-post disclosure to other members of the team of the decision to (not) cheat.[13] This might affect the individual's decision through different channels such as concerns about social image (Ariely et al., 2009; Bénabou and Tirole, 2006; Lacetera and Macis, 2010), or the fear of stigma and retaliation (Rasmusen, 1996; Funk, 2004; Herrmann, Thöni, & Gächter, 2008; Dato, Feess, & Nieken, 2019). This dimension can be modeled by adding a function of the cheated amount: for simplicity, we assume this depends on some linear factor σ_h, denoting the effect of social pressure on the decision:

$$u_{D=1}(m_d) = (1-\ell)m_d + \sigma_h m_d$$

(where $D = 1$ indicates that the decision to cheat is disclosed to the team), resulting in the general form:

$$u(m_d) = (1-\ell)m_d + D\sigma_h m_d.$$

In the first place, the presence of the group may increase the costs of cheating because, for instance, individuals want to conform to a prevailing social norm of honest behavior, implying a utility loss ($\sigma_h < 0$) related to cheating.[14] However, the literature on the adverse effects of peer influence in adolescent groups (Brechwald & Prinstein, 2011) suggests that not taking the payoff dominant decision can be seen as a sign of irrationality, or ingenuousness, leading to $\sigma_h > 0$.

We then introduce the possibility of team payoffs. The returns from cheating are shared with the other $n-1$ members of the team, and therefore enter the individual utility function discounted by some factor α. At the same time, the ethical costs of lying might be attenuated if others benefit – by virtue of both *social justifiability* (Klein, Thielmann, Hilbig, & Zettler, 2017) and of *self-serving altruism* (Gino et al., 2013) – possibly reducing such cost by ℓ_α:

$$u_{T=1,D=0}(m_d) = (\alpha - \ell + \ell_\alpha)m_d$$

(where $T = 1$ denotes team payoffs), resulting in the general form:

[12] The fact that monetary incentives enter directly into the utility function does not lead to a loss of generality: in the presence of different utility functions, it would be sufficient to transform the cost of lying accordingly. Similarly, the fact that ℓ only depends on the cheated amount and not on the original amount is irrelevant, as long as m_t is considered to be fixed among potential cheaters (that is, in our experiment, disregarding subjects with no incentive to cheat).

[13] A model with both reputation and intrinsic lying costs was presented by Abeler et al. (2018): the present model will combine the same dynamics with the possibility that payoffs are distributed to others; at the same time, it simplifies the analysis by considering the distribution of liars as given.

[14] Our model deliberately considers the desire to adhere to the social norm when observed (σ_h), as distinct from the mere desire to adhere to the social/ethical norm of honesty (ℓ).

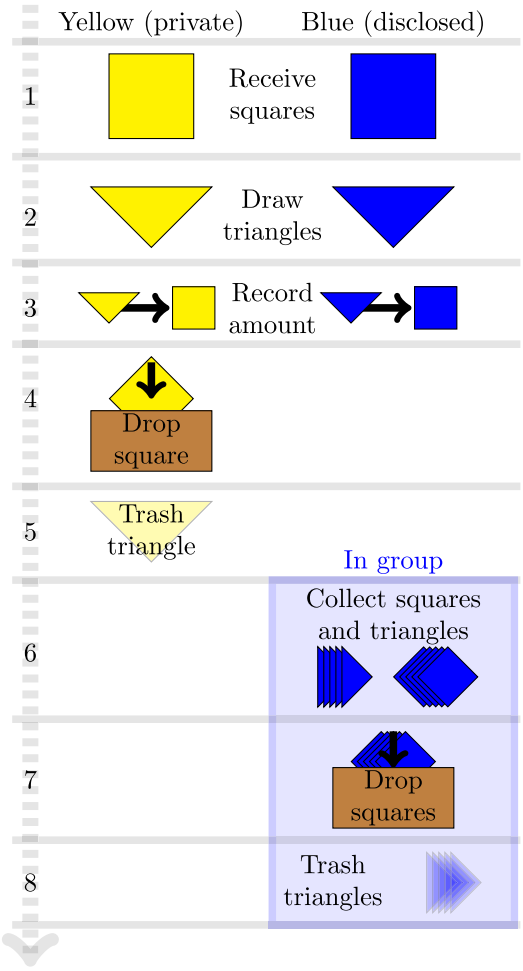


Fig. 1. Summary of experimental design.

$$u_{D=0}(m_d) = (1 - \ell + T(\alpha - 1 + \ell_\alpha))m_d. \quad (1)$$

It is worth noticing that α potentially encompasses several different components, not individually observable, such as one's own benefit from cheating (the coupon will be used by the whole patrol, including the same individual), pure altruism (the mere utility of having other team members increase their own gains), and a "warm glow" feeling (Andreoni, 1990; Ottoni-Wilhelm et al., 2017) of having *contributed* to team members' gains. It is reasonable to assume that each of these components is positive, and hence $\alpha \geq 0$. For instance, $\alpha = \frac{1}{n}$ for a perfectly selfish participant, and $\alpha > \frac{1}{n}$ if the individual positively values gains by other members of his team. Moreover, as long as the subject does not prefer to share the monetary account rather than receiving it (after all, he could always redistribute the gains *ex-post*), we have that $\alpha \leq 1$. For clarity, we denote as $\tau = \alpha - 1 + \ell_\alpha$ the term multiplying T – that is, the total effect on utility of having team payoffs.

When combining the two extensions of the model, it must be considered that the stigma associated with cheating might also be affected by whether the returns from cheating go to the individual or are redistributed to the team. Namely, it is reasonable to assume the existence of a social image reward from being observed favoring teammates, i.e., of an interaction term $\sigma_\alpha > 0$ between the effect of scrutiny and that of team payoff.[15]

All in all, the presence of the group produces two potentially countervailing effects, so that we are hardly able to make predictions about the sign of $\sigma = \sigma_h + T\sigma_\alpha$. However, we can summarize our analysis in the following equation:

$$u(m_d) = (1 - \ell + D\sigma_h + T(\alpha - 1 + \ell_\alpha + D\sigma_\alpha))m_d. \quad (2)$$

This allows us to formalize and test some useful predictions on individual behavior:

Prediction 1. If the combination of σ_h and σ_α – when applicable – is positive (i.e., $\sigma = \sigma_h + T\sigma_\alpha > 0$), then $u_{D=1}(m_d) > u_{D=0}(m_d)$, and we will observe higher levels of cheating when the decision is observed by other team members than when it remains private.

In particular, this will remain true when restricting to the case of individual payoffs ($T = 0$) if and only if $\sigma_h > 0$, i.e., honesty, rather than cheating, is stigmatized.

Prediction 2. If the combined effects of altruism (α), the reduced cost of lying due to self-serving altruism (ℓ_α), and a taste for conformity to the social norm of favoring one's own team (σ_α) are large enough (i.e., $\alpha + \ell_\alpha + D\sigma_\alpha > 1$), more cheating will be observed in the team payoff conditions than in the individual payoff conditions: $u_{T=1}(m_d) > u_{T=0}(m_d)$. If $\tau > 0$, that is, $\alpha + \ell_\alpha > 1$ (which implies $\ell_\alpha > 0$), then Prediction 2 also applies when restricting to the condition in which the decision is not disclosed ($D = 0$).

4. Experimental design and hypothesis

Our experiments are loosely based on the simple coin task proposed by Bucciol and Piovesan (2011). In the main experiment, we exogenously manipulate: i) the disclosure of the individual choice (either the decision is kept private or it is disclosed *ex-post* to the team), and ii) the recipient of gains from cheating (payoffs are paid either to the individual or to the team), in a 2×2 factorial design. Notice that the manipulation of the first condition is within-subject, as all experimental subjects make the decision of whether to cheat twice, while the manipulation of the second condition is between-subjects, such that 80 subjects were paid with an individual coupon and 80 subjects were paid with a collective coupon. The four experimental treatments in our main study are described in Table 1. Note that, in accordance with the experimental literature on cheating, our design does not allow us to directly observe cheating. We compare instead the share of respondents reporting "€ 10" to the probability distribution of a repeated fair coin toss (i.e., a binomial distribution).

Below we describe and discuss in detail the design of our main experiment, which is also summarized in Fig. 1. All steps of the procedure were read aloud to participants before the beginning of the experiment and questions were asked to verify comprehension. Moreover, participants themselves were allowed to ask clarifying questions to the experimenters, and were informed that they were allowed to do so at any point during the experiment.

Two simultaneous random coin flips. Each subject was given a sealed envelope containing four pairs of triangles. Each pair comprised a blue and a yellow triangle, stapled together. In the inner part of each triangle either the amount € 0 or € 10 was printed. All together, the four pairs of yellow and blue triangles offered the following combinations: i) Y € 0, B € 0; ii)Y € 10, B € 0; iii)Y € 0, B € 10, iv) Y € 10, B € 10. By drawing one pair of triangles from the envelope, it was as if each subject made two simultaneous coin flips, which could each deliver either € 0 or € 10.

The decision to cheat. One at a time, after drawing a pair of triangles from the envelope, the subjects dropped the envelope with the three other pairs into a trash bin (to be sealed and burned on a bonfire that same night), entered a tent where they unstapled the yellow and blue triangles, and recorded the privately observed values respectively on a yellow and a blue square card previously given to them. The order with which they would execute the two operations was up to them, and unobservable to other people. In the tent, there was also a yellow box

[15] σ_α is distinct from α because it represents the effect of being *observed* by other team members favoring the team, rather than the mere effect of favoring the team.

(that no one, except the researchers at the end of the experiment, could inspect). By dropping their yellow square into the yellow box, the subjects finalized their private decision. The subjects then kept both the blue triangle and square in their pockets and exited the tent, throwing the remaining yellow triangle into the same trash bin as before.

Disclosure of the decision to the team. After all participants had executed the steps described above, they re-entered the tent by team/patrol. In a circle, each subject revealed to the team both the blue triangle (the drawn amount) and the blue square (the declared amount). Thus, inside the tent, participants disclosed to other team members whether they had cheated or not (for what concerns the blue decision).[16] They then stapled all the blue squares to a team sheet and dropped this sheet into a blue box. Finally, they ended this phase of the experiment by exiting the tent and dropping the blue triangles into the trash bin. Note that, while subjects knew since the beginning that their blue decision would have been later disclosed to their patrol, both decisions (the private one and the one disclosed to the patrol) were made at the individual level. In fact, no pen was available in the tent when they later entered by groups, so that the reported amount could not be altered. In no way was any form of collective thinking tested as in Sutter (2009).

Individual vs team payoff. Subjects were paid with coupons. These could be spent at the local scouting shop, which sells all sorts of equipment, both for individual (e.g., uniforms, backpacks, and trekking shoes) and team use (e.g., tents, pans, and woodworking tools).[17] The treatment variation concerned the aggregation of these payoffs: in three troops, totaling 80 subjects, each subject received an individual coupon; in the other three troops, also totaling 80 subjects, individually declared amounts were aggregated in a team coupon for the entire patrol.

While each subject participated in both "private decision" and "disclosed decision" conditions (in a within-subject design), each scout troop was randomly assigned to either the "individual payoff" or to the "team payoff" condition (in a between-subjects design), as shown in Table 1.

We now outline the hypotheses that stem from our model and that can be tested with our experimental design. We denote with $x_i \in \{0, 1\}$ the reported amount (1 corresponds to reporting € 10).

H1 **Lure of profit and cheating in teams**: $\overline{x} > 0.5$ vs $\overline{x} = 0.5$.

In general, evidence of cheating emerges when the distribution of x_i (corresponding to m_d in the model) is higher than expected from a binomial distribution. We therefore begin by testing whether there is any evidence of cheating in our sample regardless of the treatment.

H2 **Disclosure and cheating**: $\overline{x}_{Disclosed\&Individual,\ Disclosed\&Team} > \overline{x}_{Private\&Individual,\ Private\&Team}$

This hypothesis tests the presence of a *scrutiny effect* by manipulating the disclosure of payoffs. We check whether, all else remaining the same, knowing that cheating will be observed *ex-post* by other team members affects cheating behavior. This corresponds to verifying Prediction 1 from our behavioral model, that is, whether $\sigma = \sigma_h + T\sigma_\alpha > 0$.

H2a **Disclosure and cheating with individual payoffs**: $\overline{x}_{Disclosed\&Individual} > \overline{x}_{Private\&Individual}$

H2b **Disclosure and cheating with team payoffs**: $\overline{x}_{Disclosed\&Team} > \overline{x}_{Private\&Team}$

These two hypotheses are simply the disaggregated versions of H2 in the "individual payoff" and "team payoff" cases, respectively. They correspond to testing, in our behavioral model, whether $\sigma_h > 0$ (honesty, rather than cheating, is stigmatized) and $\sigma_h + \sigma_\alpha > 0$, respectively.

H3 **Team payoffs and cheating:**

$\overline{x}_{Private\&Team,\ Disclosed\&Team} > \overline{x}_{Private\&Individual,\ Disclosed\&Individual}$

This hypothesis tests the presence of a *loyalty effect* (Prediction 2): whether the payoff recipient (team vs individual) has an effect on the level of cheating. It corresponds to testing whether $\alpha + \ell_\alpha + D\sigma_\alpha > 1$ in our behavioral model; that is, whether the combined effects of altruism (α), reduced cost of lying due to self-serving altruism (ℓ_α), and conformity to the social norm of favoring one's own team (σ_α) are large enough to increase cheating when the beneficiary is the team.

In general, it is more than likely that different subjects react differently to the design variations. Testing the above hypotheses does not amount to estimating point-wise parameters that apply to all individuals: rather, the hypotheses should be interpreted as statements on the population mean, e.g. on the proportion of individuals having a positive τ.

4.1. Procedures

The main experiment was run within a single month, at the summer camps of six scout troops from Trentino-Alto Adige, a region in north-eastern Italy (all scout troops were from the same region and held their camp in the region). Each session involved one troop, comprising 4 to 6 patrols, for a total of 31 patrols, and 160 subjects evenly distributed by gender (51% males and 49% females), for a total of 320 observations (each subject made both the Private and the Disclosed decisions) which reduce to 315 due to 5 missing observations in the Private decision. Each patrol comprised between 3 and 8 individuals, with an average of 5.16. See Table 4 in Appendix D for additional information and descriptive statistics on our sample.

After reading the instructions, each participant took a first envelope, which contained a randomly assigned ID[18] written on a white card, and the two yellow and blue square cards reporting the same ID.

One by one, participants moved close to a camping tent (see Fig. 3 in Appendix E), specifically set up at some distance but still visible to everyone, where an experimenter was standing by. Once there, each participant opened a second envelope, containing the four pairs of stapled triangles, drew one, and threw the remaining ones in the trash bin as described above. The triangles were stapled in such a way that neither the participant nor the experimenter could see the amounts printed on them (Fig. 4 in Appendix E displays the different triangles, together with one stapled pair).

Each participant then entered the tent and performed the task, unstapling the drawn pair of triangles and writing the amounts printed on the yellow and blue triangles on the same color squares. Afterward, patrols entered the tent one after the other, and completed the blue task

[16] The possibility of offering hard proof of the decision to cheat to the team (but not to the experimenter) is the key difference between our design and the classic coin-tossing or dice-rolling tasks used in this literature. It is worth mentioning that during this phase, subjects were not observed by the experimenters, and so they could have in principle agreed *not* to disclose to each other the result of their draws. We have no hint that this happened; most importantly, it is ultimately irrelevant for our results as, when entering the tent with their patrols, decisions had already been taken. In order for individual decisions to be influenced by such collusive behavior, subjects should have *anticipated* since the first phase (when they were not allowed to freely communicate) their group's willingness not to abide by the rules, and to keep the individual draws private.

[17] These are specialized stores that typically provide material for both individuals and patrols. In these stores, scouts purchase technical materials for their activities during the year and, above all, for the activities carried out during the camp: camping tents, bivouac, thermal bowls, boots etc. This allows the experimenter to have a comparable payment across conditions: when the beneficiary is the individual (single coupon with which the individual can buy goods for private use, e.g., boots) or the patrol (indivisible coupons with which the patrol can buy goods for common use, e.g., camping tents).

[18] Following a tradition among Italian boy scouts, each subject was randomly assigned a fantasy identity (known as a *totem*), composed by an animal name followed by an adjective.

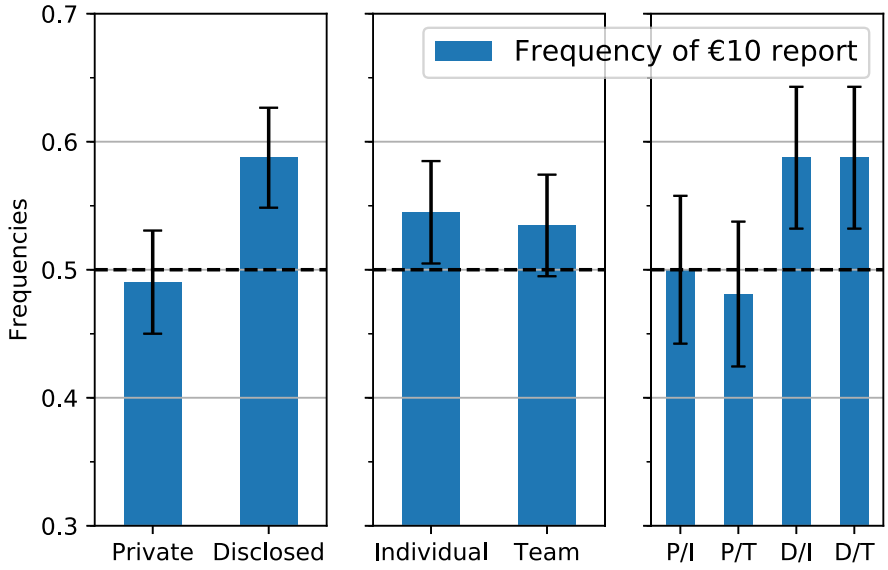


Fig. 2. Share of subjects reporting € 10.

(stapling together the square blue cards, dropping the stapled squares in the blue box, and throwing the blue triangles in the trash bin). While each subject/patrol performed the task, the rest of the troop was kept busy with traditional scout games and songs.

When all members of the troop had completed the experimental tasks,[19] they were asked to answer a short questionnaire that included standard sociodemographic questions, and received the payment. In order to guarantee full anonymity, payments were placed in an envelope with the ID of each subject (or with the name of the patrol in the case of team payoffs). The envelopes were then placed at the center of the camp so that each individual could take it whenever he deemed it appropriate after the researchers had left the camp. We paid the subjects for only one of the two tasks performed (yellow or blue squares). To determine which one, a coin was tossed at the end of the experiment. The entire session lasted between one hour and a half and two hours. Instructions for the two tasks, as well as the questionnaire (translated into English), are reported in Appendix F.

5. Results from the main experiment

5.1. Non-parametric analysis

While the objective probability of extracting € 10 is $\pi = 0.5$, we find winning rates of $\pi = 0.540$, on average, across treatments. This suggests a level of cheating lower than what is found in the literature.[20] A one-sample test of proportion for the whole distribution and a one-sample Kolmogorov-Smirnov test indicate that our data can be distinguished from binomially distributed data (p=0.080 and p=0.000, respectively), hence confirming hypothesis H1.

Fig. 2 depicts the frequency at which subjects reported € 10 in the private vs disclosed condition (left panel), in the individual payoff vs team payoff condition (center panel), and across the four treatments (right panel). The dashed line presents the expected distribution of a fair coin toss and the error bars report standard errors. Table 2 presents the frequency of € 10 under each treatment, together with the results of a one-sample test of proportion against 50%, the expected value of a fair coin toss.

The results depicted in Table 2 and Fig. 2 show that, when an individual decision is disclosed to the team, cheating increases, confirming H2. This implies that honesty, rather than cheating, is stigmatized, and corresponds to $\sigma > 0$ in our model. Specifically, a proportion test on the equality of means confirms that $\bar{x}_{Disclosed\&Individual,\ Disclosed\&Team} > \bar{x}_{Private\&Individual,\ Private\&Team}$ (p=0.042). As expected by a comparison of bars in the right panel (the difference between D/I and P/I, on the one hand, and D/T and P/T on the other hand), we obtain very similar results when disaggregating on the payoff treatment. However, while $\bar{x}_{Disclosed\&Individual} > \bar{x}_{Private\&Individual}$ is not confirmed (p=0.136), $\bar{x}_{Disclosed\&Team} > \bar{x}_{Private\&Team}$ is marginally confirmed (p=0.089).

These results do not represent conclusive evidence on whether $\sigma_h > 0$ or $\sigma_\alpha > 0$ (the latter being not testable by design), but they do suggest that $\sigma_h + \sigma_\alpha > 0$, confirming [H2b].

No significant shift in behavior seems to be driven by the manipulation of the payoff recipient (individual vs team); that is to say, by the combined effect of altruism (α), self-serving altruism (ℓ_α), and taste for conformity to the social norm of favoring one's own team (σ_α). Indeed, a proportion test on the equality of means highlights that $\bar{x}_{Private\&Team,\ Disclosed\&Team} \not> \bar{x}_{Private\&Individual,\ Disclosed\&Individual}$ (p=0.573), and therefore the hypothesis that $\alpha + \ell_\alpha + D\sigma_\alpha > 1$ (H3) is not supported.

5.2. Parametric analysis

In order to provide a quantitative assessment of the relation between the probability of reporting € 10 and a number of individual-level covariates, we run probit regressions; their marginal effects are reported in Table 3.

Explanatory variables include the two treatment variations – the decision being disclosed to the patrol (*DecisionDisclosed*) and the patrol benefiting from the lie (*TeamPayoff*) –, their interaction term, age, a dummy for being male, the number of individuals composing the patrol, and several measures extracted from responses to the final questionnaire: a measure of overall risk propensity behavior, five indicators of personality traits, an indicator of happiness, an indicator of trust in the

[19] The session included another experiment, which was held after the experiment described in this paper. Participants knew in advance that they would participate in two different activities and then would have to fill out a questionnaire: however, instructions for the second activity were only provided after the end of the first experiment. Final payments were cumulated across the two experiments.

[20] Abeler et al. (2018), for instance, find that across 43 countries, subjects leave on average about three-quarters of the possible gains on the table. Our subjects leave 92% of them (100% − 2(54% − 50%)).

patrol, and two variables measuring the willingness to break the rules to improve one's condition or the patrol's condition. Risk propensity is measured with a reduced version of the Domain-Specific Risk-Taking (DOSPERT) Scale (Blais & Weber, 2006), composed of questions about risk attitudes in the recreational, financial, ethical, health, and social domains. We used two questions from each domain out of the original ones. To obtain the overall risk propensity measure, we computed the average score of the ten questions. The scale ranges from 1 to 7, where 1 represents a low risk propensity and 7 a high risk propensity. Personality traits included in the model are the "Big Five", investigated through a revised Italian version of the Ten-Item Personality Inventory (Chiorri, Bracco, Piccinno, Modafferi, & Battini, 2015): extraversion (*big_1*), agreeableness (*big_2*), conscientiousness (*big_3*), neuroticism (*big_4*), and openness to experience (*big_5*). In the Ten-Item Personality Inventory, possible answers to each question range from 1 (*completely disagree*) to 7 (*completely agree*); we then measure each personality trait as the score of the question directly asking about the trait, minus the score of the question asking about the opposite trait.

First, we test hypotheses [H2] and [H3] (in columns (1) and (2) of Table 3).[21] In accordance with the non-parametric results, when the decision is disclosed, the probability to report € 10 is higher. In particular, from column (2), we see that the average probability to report € 10 increases by 11 percentage points under the treatment, when controlling for covariates. Holding all other variables in the model at their means, the predicted probability of declaring € 10 when the decision is private or when the choice is disclosed is 0.49 and 0.59, respectively. Conversely, when the team is the beneficiary of the cheating, individual behavior is not affected, as highlighted by the non-significant coefficient for *TeamPayoff*. While this non-result clearly lacks an unambiguous explanation, it may be related to the strength of ties within patrols: while scouts have a clear distinction between personal and group equipment, and we know that coupons were used to buy both (e.g. T-shirts and hats, on the one hand, and replacement parts for the patrol tent, on the other), they may have almost no preference for obtaining individual rather than group goods.

In order to test hypotheses [H2a] and [H2b], we compute the marginal effect of disclosure for individual and team payoffs. No significant effect of *DecisionDisclosed* is found conditional on payoffs being individual (p=0.392 when controlling for covariates in column (2) of Table 3); instead, we find a significant disclosure effect of 0.158 conditional on teams payoffs (p=0.018), which however becomes only marginally significant (p=0.098) if we remove covariates. Hence, we find support for [H2b], not for [H2a]. Results are qualitatively unchanged if we estimate an OLS model (see Table 6, Appendix D). Again, we find no significant evidence in favor of [H2a], while evidence in favor of [H2b] is significant when we include covariates (p=0.028; p=0.110 when we exclude covariates). This is also in line with the results reported in Section 5.1, which do not consider covariates and indeed yield marginally significant evidence in favor of [H2b].[22]

As for control variables, our data show that the probability for older individuals to report € 10 is lower (in line with Glätzle-Rützler & Lergetporer 2015; Maggian & Villeval 2016), while all other control variables are not significant.[23]

Intriguingly, if the control variables include the response to a question about happiness in life, as formulated in the European Value Survey (variable *happiness*, taking values from 1 to 10), we find that it is significant and that, once it is introduced, the coefficient for our measure of overall proneness to risk becomes significant (columns (5)). It is important to note that the happiness variable could be affected by reverse causality: it might be that lucky subjects who happened to draw one or two € 10 triangles became happier – and unlucky subjects who are less risk-averse ended up actually cheating. As luck and cheating are, by design, empirically indistinguishable, we cannot corroborate this hypothesis through our experimental results, and the reverse causality concern justifies the exclusion of the happiness variable from the main estimation.

While gender does not appear as a significant predictor of contributions in Table 3, the negative sign itself seems in contrast with other prominent studies in this literature (Bucciol and Piovesan, 2011; Dreber and Johannesson, 2008; Houser et al., 2012; Muehlheusser et al., 2015). To investigate this, we disaggregate on the disclosure treatment, finding that while in both conditions males cheat less than females, the difference is only significant within the "private decision" condition (p=0.045 from a proportion test). Both genders report lower gains in such condition, but the disclosure effect is statistically significant only among males (proportion tests yield p=0.019 for males and p=0.288 for females). One element worth mentioning is that, in the "private decision" condition, males report average gains *below* 0.5: although this result is only marginally significant (p=0.069 from a proportion test), it raises the suspicion that they might be applying a form of *moral balancing*, occasionally compensating their cheating from the "disclosed decision".[24].

We also verified that our main result [H2] remains significant if we restrict to participants born in Italy (p=0.039, from a proportion test). If we focus on years spent in the patrol (as proxied by the year of birth), we find that the sign of the effect is consistent with the main effect for participants in the first, second and fourth year (participants in the third year feature the opposite sign); the effect is stronger, and statistically significant (p=0.003, from a proportion test) for participants in the fourth year. Interestingly, these are participants who will leave the troop (and hence the patrol) shortly after the summer camp, hence they will not benefit from any purchases made with patrol coupons. However, at the same time, they are the ones who spent more years in the patrols, presumably building more *esprit de corps* .

5.3. Follow-up experiment

Our main experiment could potentially suffer from three limitations. Firstly, while scouts groups represent an ideal setting for our experiment, they might limit the external validity of our results. Secondly, following our decision to implement a within-subject design to test the existence of a scrutiny effect, subjects might have adopted moral balancing across their two cheating decisions, as mentioned above. Finally, the sample of scout groups from the region was inherently limited, forbidding us from reaching a larger sample size. While the general results on [H1], [H2] and [H2b] are overall robust – and neither Fig. 1 and column (1) provide any evidence supporting [H3] – our non-result on [H2a] is likely to be due to the limited sample size. Indeed, Table 2 and Fig. 2 suggest a very similar effect of disclosure between the individual payoff and the team payoff conditions, consistently with the very similar p-values of the non-parametric tests.

In order to tackle the first issue (sample selection), the follow-up experiment was conducted with a population of adolescents attending

[21] In line with the literature, the present section reports results of two-sided tests.

[22] We also verified that a mixed effects probit model allowing for patrol-level effects does not attribute a significant role to the hierarchical structure of the data.

[23] In particular, we do not find a significant correlation with gender (in line with inconclusive evidence found by Capraro (2018) for "Pareto white lies") nor with agreeableness (studied by Heck et al., 2018): subjects who cheat tend not to identify themselves as "extrovert, exuberant", but even less as the opposite trait "reserved, silent". See Battiston et al. (2019) for a focus on these and other personal traits.

[24] A participant who had drawn € 0 for blue and € 10 for yellow, for instance, could have swapped the colors in order to display cheating behavior to his peers, while maintaining a truthful behaviour on average. On moral balancing, see Ploner & Regner (2013); Effron & Conway (2015).

ten different schools (for a total of 23 classes and 432 students) in the province of Trento (the same area of our main experiment), within the same age range as our scout population[25], approximately 18 months after the main experiment.

In order to tackle the second issue (moral balancing), we implemented a between-subjects approach. Notice that we still devoted a small part of the sample to the same within-subject approach adopted with scouts, in order to be able to compare reported levels for the same design across different experimental populations. Tables 4 and 5 in Appendix D provide details on the student population and the schools involved, respectively.

The last issue (sample size) was overcome by running the experiment with school adolescents: the much larger sample size allowed us to replicate [H2] and to validate [H2a].

An important difference between the main and follow-up experimental samples concerns the *absence of teams* within classes. School classes are persistent groups whose members know each other well and, in this sense, they are similar to scout troops. However, we lack the parallel with patrols as, within classes, there are no established smaller units sharing activities and goods. In each class, we thus randomly formed 3 to 5 groups (depending on the number of participants) of similar size (between 4 and 7 individuals). Teams were artificial and short-lasting; thus there was no point in them sharing common goods. Hence, we did not manipulate the payoff recipient (individual vs team) as in the main experiment, but instead the payoff was always individual. A further difference concerns session composition: compared to a scout troop, a class is typically less diverse (e.g., students in a given class are typically of the same age, and often predominantly of the same gender – depending on the type of school); in contrast, classes are more differentiated *between* each other.

Nevertheless, we designed the experiment in schools to be as similar as possible to that of the scout camps, with the obvious exception of the previously mentioned between-subjects design, which incidentally simplified the task. The follow-up experiment with students was carried out during school hours, in a dedicated large room on the school premises; the tent used in the scout camps was replaced by a screen located in a corner of the room. At the scout camps, the trash bin was sealed and set alight on the bonfire, while in the schools its content was passed through a shredder at the end of the session, in front of the students. The scouts camps were isolated from each other, both logistically and in terms of communication. Similarly, in order to limit the possibility of spillovers between classes, classes from a same school were always visited in a single day (with the sole exception of one large school). While each subject performed the task, scouts were kept busy with a common repertoire of games and songs; likewise, students were involved in other educational activities, guided by a professional trainer who knew and shared the aims and scope of the experiment. As with scout leaders in the original experiment, the teaching staff was not involved in the experiment. Scouts were identified, in the main experiment, with randomly assigned totems (see footnote 14), whereas students were identified by randomly assigned names of cities. It is worth noting that, while scouts are used to taking part in a variety of structured group activities, the experiment was much more of a novelty for the students, who are more used to individual, standardized tasks. This meant that explaining and running the experiment was more difficult with classes than scouts. This difficulty was alleviated to a large extent by the previously mentioned between-subject design, which resulted in a simpler experimental procedure (yellow *or* blue task).

Artificial groups were also created in the sessions in which only the yellow task ("private decision") was performed, despite not having any substantial role; after the yellow task, members of each group would gather behind the screen and staple together their blue squared cards, which only reported their assigned name of city (not their choice), hence mimicking the final phase of those sessions in which the blue task was performed ("disclosed decision"). In addition to making designs more comparable, this allowed us to reconstruct group composition. As with scouts, subjects were remunerated with coupons. These could be spent at a local bookshop selling books, games, music and gadgets. The shop location and characteristics were described to the students before the experiment.

The between-subject approach was adopted in the majority of sessions – 15 out of 23, totalling 292 subjects (for comparison, the sample size for [H2a] in the original experiment included 80 subjects) and 289 valid observations. In particular, the yellow task was performed in 8 sessions (168 subjects), while the blue task was performed in further 7 sessions (124 subjects). We used the 8 remaining sessions (140 subjects) to replicate the within-subject design implemented in the main experiment. In schools, the implementation of this more complicated design was often troublesome: we recorded missing observations in the outcome variables for 11 subjects out of 140, noticed students having trouble following the instructions, and noticed surprise/skepticism in the debriefing phase regarding the possibility of cheating without being observed by the experimenters. No relevant difficulties had been reported in scout groups, nor in the between-subjects design in schools.

Overall, across all sessions, the reported winning rate among students is 0.541, strikingly similar to that observed among scouts (0.540), and significantly higher than 0.5 according to a one-sample test of proportion or a Kolmogorov-Smirnov test (p=0.026 and p=0.000, respectively).

In the between-subjects sessions, we find that the outcome € 10 is reported at a rate π=0.584 (higher than 0.5: p=0.002 from a one-sample test of proportion, p=0.000 from a one-sample Kolmogorov-Smirnov test). As in the main experiment, we find that, when the decision is disclosed, students cheat more compared to the case in which the decision is kept private (π=0.618 vs π=0.540), although the difference is only marginally significant (p=0.093 from a proportion test). This result confirms hypothesis [H2] – and in particular, provides significant evidence in favor of [H2a] – highlighting that the scrutiny effect detected in the main experiment is not driven by some form of moral balancing between the two treatments by subjects who take part in both.[26] Furthermore, this results highlights that, while scout patrols represent a special sample in the sense of ease of experimental implementation, the effect of disclosure on cheating is not a specificity of boy and girl scouts.

In the few within-subject sessions, we again find that students report higher winning rates when the outcome is *ex-post* observed by peers (π=0.504 vs π=0.485), but this difference is not statistically significant (p=0.381 from a proportion test). This is not surprising since this setting was troubled by the difficulties above mentioned, and was devoted a smaller sample size than in the original experiment. The reported winning rates (in each disclosure condition, and when pooling across conditions) are lower than those observed in the original experiment with individual payments but the difference is not significant. When pooling observations together from the main and follow-up experiments, we again find that subjects report statistically higher winning rates in the disclosed decision condition (from proportion tests: p=0.018 when considering all observations; p=0.047 when considering all observations from sessions with individual payments; p=0.069 when considering all those from a within-subject treatment).

When disaggregating results across gender, we find that males report slightly higher average gains than females in the between-subjects

[25] The actual age range was slightly broader because the experimental population included some students who had repeated a school year.

[26] Indeed, the difference between the private and the public decision is larger across the between-subjects sessions (0.078) than among the within-subjects sessions (0.018). Moral balancing would have predicted the opposite result – a larger difference in the within-subjects sessions, where subjects have the possibility to increase their apparent propensity of cheating without affecting their cheating in expected terms.

Table 2
Share of observations reporting 10 across treatments.

Condition	Share	*p*-value	N
Private decision	49%	0.595	155
& Individual payoff	50%	0.500	76
& Team payoff	48%	0.632	79
Disclosed decision	59%	0.013	160
& Individual payoff	59%	0.059	80
& Team payoff	59%	0.059	80

Table 3
Probit results.

	(1)	(2)	(3)
DecisionDisclosed	0.097**	0.111**	0.115**
	(0.046)	(0.048)	(0.049)
TeamPayoff	-0.009	0.016	0.007
	(0.061)	(0.058)	(0.057)
Male		-0.075	-0.072
		(0.060)	(0.057)
Age		-0.049*	-0.047**
		(0.026)	(0.024)
N. components		-0.020	-0.012
		(0.029)	(0.030)
Risk		0.060	0.074**
		(0.038)	(0.036)
Extraversion (big_1)		0.012	0.011
		(0.007)	(0.007)
Agreeableness (big_2)		0.013	0.011
		(0.013)	(0.014)
Conscientiousness (big_3)		0.003	0.002
		(0.011)	(0.010)
Neuroticism (big_4)		-0.004	0.004
		(0.011)	(0.010)
Openness to experience (big_5)		0.006	0.001
		(0.014)	(0.014)
Happiness			0.035**
			(0.015)
N	315	279	273

Note: Average marginal effects from estimation with probit: dependent variable is x_i. Clustered (at the patrol level) standard errors in parentheses. Two-sided *p*-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

sessions, while the opposite happens in the within-subjects sessions (like in the original experiment); however, these differences are not significant. Furthermore, between-subjects sessions feature a stronger disclosure effect for females than males, while the opposite happens in the within-subjects sessions (and in the original experiment) but no effect is statistically significant.

The questionnaire in the follow-up experiment included a question, not present in the original experiment, asking the respondents' best guess of the number of people reporting "10 € " in their class. On average subjects reported 0.58 across all sessions, slightly larger than the actual level of cheating observed, and the share of subjects indicated is strongly correlated with actually reporting 10 € (correlation 0.23, p=0.000).[27] This allows us to verify that cheating is indeed perceived as a social norm at least by part of our sample, and that this perception is directly related to the propensity to cheat.

Overall, the results of the follow-up experiment (although only marginally significant if taken in isolation) support the external validity of our main experiment and the internal validity of its original design. The fact that scouts, despite the emphasis on *loyalty* and *honesty*, do not feature lower levels of cheating than general adolescents might appear surprising. However, these two terms, both salient in the scout rules, might themselves embed a tension, whereas loyalty can be interpreted as trustworthiness, calling for lower cheating, but also as suggesting a strong *esprit de corps*, which might push in the opposite direction. This, together with subjects in the two experiments having similar age ranges and living in the same region, might explain the similar observed levels of cheating. Finally, despite groups within school classes being artificial, they still regroup classmates: they are *subsets* of real, long running groups. So while the group payment treatment is not applicable to this sample, the similarity with scouts in terms of disclosure effect is not surprising.

6. Conclusions

In this paper, we studied cheating in groups of adolescents, where peer interactions are very important. We ran our main experiment with boy and girl scouts because these are naturally occurring, persistent and comparable teams with interesting characteristics which set them apart from the minimal groups typically used in previous experiments on cheating: notwithstanding their young age, our scouts resemble groups operating in work environments and in civil society.

We found that the overall level of cheating is limited. On average, the reported amount is € 5.40 and therefore our subjects appropriate less than 8% of the maximum potential gains of cheating. This is remarkably less than the 22% subjects appropriate in the 72 studies analyzed by Abeler et al. (2018), and the comparison is even more striking when looking at Bucciol and Piovesan, 2011, who run the study most similar to ours in the type of task, geographical sample, and age group (in their baseline treatment, 85% of children reported the white outcome – to which a prize was attached; this value dropped by 36% and 13% for girls and boys respectively when a request not to cheat is introduced).

While cheating is generally low, our experiment provides evidence of the important role of peer scrutiny, which goes in the opposite direction of what was previously observed for hierarchical scrutiny conducted by experimenters or by other observers (see Gneezy et al., 2018; Houser et al., 2016; Mazar et al., 2008; Ostermaier and Uhl, 2017; Pascual-Ezama et al., 2015; Van de Ven and Villeval, 2015): we find that cheating increases when the decision to cheat is disclosed to other peers in the team.

We run our main experiment on teams of scouts, who are individuals sharing strong social norms: this makes the result if possible even more striking. We find analogous results when replicating the experiment on a population of students in the same age range and from the same region.

The fact that some of the subjects give up their integrity when they know their decision will be revealed to other group members suggests that, somehow, *lies have long legs*, as social-image concerns seem to drive the decision to report the higher payoff to the team. This is a novel result in the literature on cheating but it can be easily reconciled with the vast body of literature, from social and developmental psychology to criminology, sociology and law and economics, which has focused on peer effects in antisocial behavior. Social conformism is a well known tract of adolescents behaviour, which has been shown to reduce anxiety from the mismatch between one's own preferences and others' (Berns et al., 2010). The literature has shown that conformism can lead to behavior that adolescents know to be risky or unethical but that enhances their self-image (e.g., Glaser et al., 2010; Tomé et al., 2012; Knoll et al., 2015). Along the same line, adolescents in our sample seem to consider cheating as *cool* and contributing to their self-image vis-à-vis their peers, thus cheating more when observed by their peers. Indeed, a question asked in our follow-up experiment allows us to confirm that at least part of participants perceive cheating as a social norm, and that these subjects tend to cheat significantly more.

[27] In the between-subjects sessions with no disclosure, a similar question was also asked referring to a subject's *group* – we could not ask it in all other sessions, as subjects *observed* cheating in their own group. In this case, the average of replies is only 0.53, but with a stronger correlation with own cheating (0.41, p=0.000).

The second dimension of our manipulation, whether payoffs were paid to the team or not, did not trigger any significant treatment effect. In other words, altruism was not a decisive factor for lying. Loyalty to one's peers may imply that cheating for the interest of the group is not considered unethical, but it appears that our subjects did not interpret their loyalty pledge in this direction. This might appear surprising given the extensive experimental literature on group cheating but, differently from most of such literature, our design cleanly separates the act of cheating (which is individual) from its beneficiary (which can be the team).

It is fairly possible that the specific age range being analyzed (adolescents) plays a role in these results: while we preferred to run the follow-up study on the same age range for comparability, we think that investigating the issue in groups composed of adults is an important avenue for further research. Similarly, given that all subjects involved in our experiment were from a same region of Italy, the peer scrutiny effect should be investigated in the context of other regions and cultures.

Declaration of Competing Interest

None.

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Appendix A. Informed consent and privacy

The experiment was agreed with, and approved by, the adolescents' guardians (scout headmasters for the first study, both school principals and school teachers of involved classes for the second study), who considered it fit to be integrated within normal scout/school activities, which they are responsible of organizing and supervising, including for what concerns ethical aspects and relationships with the children's parents. Neither Fondazione Roberto Franceschi, which financed the first experiment nor LUMSA university, which provided funds for the second experiment, required IRB approval for experimental research at the time of our experiments.

A debriefing session was held at the end of each experimental session, in which the scope of the experiment was explained, and participants were then allowed to ask any question. During this debriefing, headmasters (for the first experiment) and school teachers and a professional trainer regularly working with schools in the Trento province (for the second experiment) collaborated with the experimenters. The privacy of participants has been always observed by using only anonymous data.

Appendix B. Information on Scouting

"The Mission of Scouting is to contribute to the education of young people, through a value system based on the Scout Promise and Law, to help build a better world where people are self-fulfilled as individuals and play a constructive role in society."[28] The scout promise, law, and method are substantially shared by all scouts worldwide. Scout troops are characterized by shared social norms, homogeneous distribution of age, and a clear distinction of the individual roles within the patrol. All members of the Scout Movement worldwide are required to adhere to the Scout Promise and Scout Law. The wording may vary in different National Scout Organizations as appropriate to the local culture, but they are all based on the Promise and Law originally conceived by the founder of the Scout movement, Robert Baden-Powell: *"On my honor I promise that I will do my best to do my duty to God and to my Country, to help other people at all times, to obey the Scout Law."* In all countries, the first two articles of the Scout Law comprise some variation of the following texts: *"A Scout's honor is to be trusted"* and *"a Scout is loyal."*[29] The leading element of the scout method is the patrol (or team) system, the basic organizational structure in scouting. Each patrol, normally comprising of six to eight young people, operates as a team with one member acting as the team leader. Within each team and in ways appropriate to their capacities, the scouts organize their lives, sharing responsibilities, making decisions, setting up, carrying out, and evaluating their activities, and assembling and maintaining materials required for such activities. During the summer camp, an implicit competition between patrols takes place. The aim of this competition, which is intended to be both fun and educational, is to instill the awareness in each member that the honor of his patrol depends in some degree on his own ability to play the game.

Appendix C. Additional information on experimental populations

For the first experiment, we ran one pilot a week ahead of the experiment (differing from the actual experiment in how instructions were formulated). We discarded the data from one last session because the summer camp involved only two patrols that were formed *ad hoc* and did not reflect the actual patrols operating during the year.

In the second experiment, no pilot was run. One session was discarded because during the debriefing we ascertained that the teachers had not followed our instructions: the students who participated were actually from two separate classes and, most importantly, they had been informed about the experimental protocol before the beginning of the experiment.

Table 4 summarizes the main characteristics of scouts and students included in the sample. The variable *Age* is computed by subtracting the year of birth from the year of the experiment. Scouts tend to describe themselves as more extrovert and less neurotic than the population of school students. Table 5 summarizes the main characteristics of participating classes.

[28] https://www.scout.org/mission, Accessed on September 6, 2019.

[29] A comprehensive list of national scout laws is available on Wikipedia at http://en.wikipedia.org/wiki/List_of_Scout_Laws_by_country. Accessed on September 6, 2019.

Table 4
Descriptive characteristics for scouts and students.

	Scouts	Students	*p*-value from *t*-test
Age	15.316	15.264	0.626
Male	0.513	0.583	0.127
Risk	3.449	3.480	0.710
Extraversion (big_1)	0.981	0.249	0.009
Agreeableness (big_2)	1.955	1.658	0.169
Conscientiousness (big_3)	1.605	1.539	0.776
Neuroticism (big_4)	-0.808	-0.333	0.052
Openness to experience (big_5)	1.704	1.516	0.401

Note: two-tailed *p*-values.

Table 5
Characteristics of participating classes.

Class (cohort)	
Middle school: 3rd grade	5
High school: 1st grade	10
High school: 2nd grade	8
School type (for high schools)	
Lyceum	7
Technical institute	2
Professional institute	9

Appendix D. Additional results

Table 6 provides the equivalent of Table 3 estimated through OLS. Given the linearity of the estimator, we include an interaction effect between the regressors of interest (avoiding consistency problems specific to nonlinear models, as pointed out by Ai & Norton, 2003). If in columns (1) and (3) we introduce patrol fixed effects, we clearly loose all collinear variables (TeamPayoff, Male, N. components) but all other results are virtually unchanged.

Table 6
OLS results.

	(1)	(2)	(3)	(4)	(5)
	decision	decision	decision	decision	decision
DecisionDisclosed	0.097**	0.087	0.111**	0.058	0.062
	(0.047)	(0.069)	(0.050)	(0.072)	(0.073)
TeamPayoff	-0.009	-0.019	0.015	-0.036	-0.048
	(0.061)	(0.084)	(0.059)	(0.079)	(0.081)
Interaction		0.019		0.100	0.101
		(0.094)		(0.099)	(0.103)
Male			-0.076	-0.076	-0.072
			(0.062)	(0.062)	(0.060)
Age			-0.048*	-0.049*	-0.046*
			(0.027)	(0.027)	(0.025)
N. components			-0.020	-0.020	-0.011
			(0.030)	(0.030)	(0.030)
Risk			0.059	0.059	0.072*
			(0.039)	(0.039)	(0.037)
Extraversion (big_1)			0.012	0.012	0.012
			(0.008)	(0.008)	(0.007)
Agreeableness (big_2)			0.012	0.012	0.012
			(0.013)	(0.013)	(0.014)
Conscientiousness (big_3)			0.003	0.003	0.001
			(0.011)	(0.011)	(0.011)
Neuroticism (big_4)			-0.004	-0.004	0.004
			(0.011)	(0.011)	(0.011)
Openness to experience (big_5)			0.006	0.006	0.001
			(0.014)	(0.014)	(0.014)
Happiness					0.034**
					(0.015)
constant	0.495***	0.500***	1.103**	1.133**	0.727
	(0.045)	(0.057)	(0.462)	(0.460)	(0.449)
N	315	315	279	279	273

Note: Analogous of Table 3estimated with OLS. Dependent variable is x_i. Clustered (at the patrol level) standard errors in parentheses. Two-sided p-values: * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix E. Experimental material



Fig. 3. Experimental environment for the main experiment *Note:* The tent used for the experiment (left), as located in one of the scout camps.



Fig. 4. Triangle and square cards used in the main experiment *Note:* Left: the four types of triangles, together with an example of stapled pair; right: two example of pairs of squared cards (for two different participants).

Appendix F. Experimental instructions

F1. Instructions

GENERAL INSTRUCTIONS [30]

Thank you for your participation in this activity. There will be two phases, in which you will make decisions following the instructions we will give you. You will then be requested to complete a questionnaire. The study will last a maximum of two hours. The procedures we use will not allow us to discover who made what decision, so the anonymity of the decisions is guaranteed. During the activity, you will be assigned a totem and we will refer to this to identify you. We will never be able to associate your name with the totem (nor are we interested in doing so). If you have any questions about the procedures, please do not hesitate to request further explanation. For the entire duration of the activity and the questionnaire, we ask you not to communicate with anyone, unless requested to do so by the procedures. Those who break these rules will be asked to leave the experiment. You will start by extracting an envelope containing three squares: a yellow one, a blue one, and a white one. The same totem is written on all of them; this will become your identity during the activity. In addition to the totem, the yellow and blue squares both have two boxes on them, one labeled "€ 0" and the other labeled "€ 10"; they will be collected during the activity. You can keep the white square as a reminder of your totem. Your choices during the activity will determine the value of the coupon you will receive, to be spent at the cooperative "il Bivacco." At the end of the activity, you will find these coupons in a basket, inside an envelope addressed to your totem. Now you can turn the page.

SPECIFIC INSTRUCTIONS

Together, we will read once through all the steps of this procedure. You will then perform the activity individually, following these instructions step by step.

1. At the entrance of the tent, you will receive an envelope containing four pairs of triangular cards. Every pair comprises a blue triangle and a yellow one. The four pairs of triangles are marked as follows:
   - 1st pair: yellow "€ 0" and blue "€ 0";
   - 2nd pair: yellow "€ 10" and blue "€ 0";
   - 3rd pair: yellow "€ 0" and blue "€ 10";
   - 4th pair: yellow "€ 10" and blue "€ 10".
2. Draw a pair at random, and put it in your pocket.
3. Throw the remaining pairs in the bin and enter the tent. The contents of the bin will be burned in the bonfire tonight.
4. Separate the yellow triangle from the blue one.
5. On the yellow square, use the pen to tick the amount you see on the yellow triangle; on the blue square, tick the amount you see on the blue triangle. The two amounts you tick are important, because one of the two will determine the value of the coupons you will receive individually at the end of the experiment ***(because one of the two will determine the value of the coupon that will be delivered to your team at the end of the experiment.)***
6. Fold the yellow square in quarters and put it into the yellow box.
7. Put the blue triangle and the blue square in your pocket.
8. Exit the tent and throw the yellow triangle into the basket.
9. When everybody finishes the experiment, the yellow box will be sealed and brought out of the tent.
10. One team at a time goes into the tent and gets in a circle.
11. Then, all the members of the team do the following together:
    (a) Take out their blue triangles and put them in the center of the circle.
    (b) Take out their blue square and attach it with the scotch tape to a sheet of paper.
    (c) Fold the sheet of paper and insert it in the blue box.
    (d) Collect the blue triangles and throw them into the bin when exiting the tent.
12. When all teams are done, the blue box will be sealed and brought out of the tent.
13. A coin will be flipped. If the result is heads, the yellow box will be chosen; if the result is tails, the blue one it will be chosen.

If the yellow box is chosen, the value of the coupon that will be given to you individually ***(to your team)*** at the end of the experiment will be determined by what is written on the yellow square. What is written on the blue square will have no value.

If the blue box is chosen, the value of the coupon that will be given to you individually ***(to your team)*** at the end of the experiment will be determined by what is written on the blue square. What is written on the yellow square will have no value. If you have any questions, please ask them now before starting the activity.

F2. Questionnaire

- Assigned totem:
- Year of birth:
- Place of birth:
- Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?
  ☐ Yes ☐ No (you can't be too careful)
  [Each of the following six questions was followed by check boxes with numbers from 0–10]

[30] The main text reported below shows instructions for the individual payoff condition. The substantial differences for the team payoff condition are reported in the text below in ***bold and italic***. The instructions for the follow-up in schools are analogous to those for the individual payoff condition with minor variations reflecting the differences in design described in Section 5.3.

- From 0 to 10, how much do you tend to trust people in general?
- From 0 to 10, how much do you tend to trust members of your troop?
- From 0 to 10, how much do you tend to trust members of your patrol?
- From 0 to 10, how much do you agree that most people would try to take advantage of you if they had the chance?
- From 0 to 10, taking all things together, how happy would you say you are?
- Was your father born in Italy? ☐ Yes ☐ No
- Was your mother born in Italy? ☐ Yes ☐ No
- How many people are in your family, including you?
- For each of the following groups of people, how willing would you be to break the rules in order to improve their condition?
  [Each of the following items was followed by check boxes with numbers from 1–4, where 1 was labeled as "very willing" and 4 as "definitely not willing"]
  - Your family
  - Your neighbors
  - Someone you know well
  - Someone you meet for the first time
  - Yourself
  - Someone of a different religion than yours
  - Someone of a different nationality than yours
  - Your patrol
- Please read the following personality traits and rate how well each pair of adjectives describes you. Even if you think that one characteristic describes you better than the other, using the following scale:
  [A 7-item Likert scale was used. Each of the following items was followed by check boxes with numbers from 1–7.]
  1. extroverted, exuberant
  2. difficult, adversarial
  3. trustworthy, self-disciplined
  4. worried, anxious
  5. open to new experiences, with many interests
  6. reserved, silent
  7. understanding, affectionate
  8. disorganized, absent-minded
  9. calm, emotionally stable
  10. traditionalist, routine-bound
- For each of the following statements, please indicate the likelihood that you would engage in the described activity or behavior if you were to find yourself in that situation. Provide a rating from Extremely Unlikely to Extremely Likely, using the following scale:
  [A 7-item Likert scale was used. Each of the following items was followed by check boxes with numbers from 1–7.]
  1. Going down a ski run that is beyond your ability. *[Recreational]*
  2. Investing 10% of your annual income in a start-up. *[Financial]*
  3. Betting a day's income on the outcome of a sporting event. *[Financial]*
  4. Revealing a friend's secret to someone else. *[Ethical]*
  5. Riding a motorcycle without a helmet. *[Health/Safety]*
  6. Speaking your mind about an unpopular issue in a patrol meeting. *[Social]*
  7. Bungee jumping off a tall bridge. *[Recreational]*
  8. Walking home alone at night in an unsafe area of town. *[Health/Safety]*
  9. Moving to a city far away from your parents. *[Social]*
  10. Not returning a wallet you found that contains € 200. *[Ethical]*

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.socec.2021.101732

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