Nobody likes a rat: On the willingness to report lies and the consequences thereof

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ABSTRACT

We investigate the intrinsic motivation of individuals to report, and thereby sanction, fellow group members who lie for personal gain. We further explore the changes in lying and reporting behavior that result from giving individuals a say in who joins their group. We find that enough individuals are willing to report lies such that in fixed groups lying is unprofitable. However, we also find that when groups can select their members, individuals who report lies are generally shunned, even by groups where lying is absent. This facilitates the formation of dishonest groups where lying is prevalent and reporting is nonexistent.

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1. Introduction

Reporting the deceptive behavior of others is an act that arouses conflicting opinions. Children are scolded for being "tattle-tales" and "snitch" is a common derogatory term. Yet this act can also be deemed praiseworthy, as in the case of whistleblowers or crime informants. In this paper, we study people's intrinsic motives to report on others' lies and evaluate the potential consequences.

A growing body of research focuses on deception and the inclination of some people to tell the truth despite it being in their material interest to lie (Ellingsen and Johannesson, 2004; Gneezy, 2005). For instance, Gibson et al. (in press) and Gneezy et al. (2013) demonstrate that individuals are averse to lying (to varying degrees) but can be tempted to lie when doing so is profitable enough.[1] We extend the literature on lying aversion by studying the willingness to uphold truth telling by punishing and disassociating oneself from people who lie.

We run a laboratory experiment in which subjects play a repeated "whistleblowing" game. In each repetition of the game, subjects draw a random number that corresponds to their "true" earnings. Subsequently, they have the opportunity to overstate their earnings, which increases their payoff. Importantly, subjects are divided into groups within which they can observe each other's true and stated earnings. If lying occurs, subjects have the opportunity to report their group and thereby sanction lying subjects. Reporting others does not bestow monetary benefits. This game mimics situations where lying is individually profitable but heavily sanctioned by a central authority that relies on individuals within the organization to report it—e.g., because monitoring is prohibitively expensive.

Some evidence suggests that people do sanction those who tell *them* lies (Brandts and Charness, 2003; Croson et al., 2003; Sánchez-Pagés and Vorsatz, 2007, 2009; Eisenkopf et al., 2011; Angelova and Regner, 2013). However, in these studies, lies are to the detriment

[1] Other experimental studies on lying include Charness and Dufwenberg (2006, 2010), Cai and Wang (2006), Fischbacher and Heusi (2008), Vanberg (2008), Hurkens and Kartik (2009), Lundquist et al. (2009), Sutter (2009), Rode (2010), López-Pérez and Spiegelman (2012), Erat and Gneezy (in press), and Jiang (2013).

of the people being lied to and therefore, lying also conveys an intention to hurt the person that is subsequently making the decision to punish. In this study, we test the willingness to punish liars even when lies do not affect the pecuniary interest of, and are not directed at, potential punishers. If punishment occurs in this setting, it indicates that individuals consider lying *per se* as behavior that deserves to be sanctioned.

We also evaluate the consequences of reporting lies. One can reasonably expect that people welcome those who sanction liars. However, empirical evidence indicates that this is not necessarily the case. Dyck et al. (2010) demonstrate that the career prospects of employees who report corporate malfeasance are so dismal that it is surprising that people whistleblow at all. Similarly, strong community norms against reporting others—epitomized by the phrase "snitches get stitches"—have been documented by journalists and academics (Brown, 2007; Kahn, 2007). These reports point to fear of ostracism and punishment by their peers as a major reason why people do not report others' wrongdoings (Whitman and Davis, 2007). We incorporate such peer effects into the experiment by giving subjects a say in who joins their group. Specifically, occasionally, subjects are randomly removed from their group, and for them to rejoin a group, they must be unanimously accepted by the group's current members. Group members are informed of displaced subjects' past behavior, allowing us to determine whether subjects avoid or welcome people who report lies. Crucially, to determine the importance of these effects in the overall amount of lying and reporting, we run another treatment without voting where displaced subjects rejoin groups at random.

Field research that explores the causes and motivations to report lies faces a complex task due to extrinsic incentives and selection effects (Bowen et al, 2010; Schmidt, 2005). By comparison, our experimental setting is ideal to control these selection effects and to isolate the intrinsic motivations to report lies.

2. Experimental design and procedures

2.1. The whistleblowing game

For simplicity, we describe the whistleblowing game with the parameters used in the experiment. Consider a "society" composed of $i = 1, \dots, 12$ individuals and $g = 1, \dots, 3$ organizations. Each organization g is staffed by $n_g \in \{2,3\}$ individuals. The game is played repeatedly for nine periods and each period is divided into two stages. In the first stage, each individual observes her "true" earnings t_i, which are independently drawn from a uniform distribution with support $[0, t_g^{max}]$ where t_g^{max} are the maximum earnings in i's organization g. The value of t_g^{max} increases with the size of the organization: $t_g^{max} = 300$ points for organizations of $n_g = 3$ and $t_g^{max} = 225$ points for organizations of $n_g = 2$. After observing t_i, each individual simultaneously decides on the earnings she wishes to state s_i. Individuals are free to state any feasible earnings $s_i \in [0, t_g^{max}]$. Barring any sanctions, an individual's payoff equals her stated earnings and not her true earnings. In the second stage, individuals observe both the true and stated earnings of everyone in their organization and simultaneously decide whether they wish to report their organization. If at least one individual reports, the organization is inspected and all individuals who overstated their earnings (chose $s_i > t_i$) are sanctioned by three times the overstated amount.[2] Hence, the payoff of individual i of organization g in a period equals:

$$\pi_i = \begin{cases} s_i - 3(s_i - t_i) & \text{if } g \text{ is inspected and } s_i > t_i \\ s_i & \text{otherwise} \end{cases}.$$

At the end of the second stage, individuals are informed of the payoff and actions of all individuals in their organization.

Next, we describe how organizational membership is determined. At the beginning of the game, all individuals in the society are randomly assigned to one of the three organizations. However, after periods 3 and 6 one individual in each organization of $n_g = 3$ is randomly separated from her organization.[3] Before play resumes, everyone in the society observes

[2] By design, reporting was not possible in fully truthful organizations.

[3] To avoid organizations form disappearing, organizations of $n_g = 2$ do not lose members.

the following information of each separated individual: (i) their mean stated earnings over the last three periods, (ii) whether they reported their organization in the last three periods, and (iii) whether they were sanctioned for overstating their earnings in the last three periods. We implement two treatments. In *Random*, all separated individuals are randomly reassigned to organizations. By contrast, in *Selection*, separated individuals must be accepted into organizations by a unanimous vote. Specifically, individuals indicate whether they accept or veto each separated individual. Thereafter, separated individuals are randomly assigned among the organizations that unanimously accepted them. If no such organization exists, the individual remains separated for three periods during which she does not receive or state earnings and obtains a payoff of $\pi_i = 0$ points. Meanwhile, organizations of $n_g = 2$ play with reduced maximum earnings of $t_g^{max} = 225$ points.

The experiment was conducted using standard procedures of anonymity, neutrally worded instructions, and monetary incentives (all periods were paid). We employed a between-subjects treatment design with eight independent observations (societies) per treatment. The appendix contains the precise experimental procedures.

2.2. Theoretical predictions

We briefly discuss the theoretical predictions. If all individuals are risk neutral and own-earnings maximizers then, in both treatments, everyone states the maximum earnings and nobody reports others.[4] Moreover, in Selection, all separated individuals are accepted because organizations of $n_g = 3$ generate higher stated earnings than those of $n_g = 2$.[5]

We next discuss how predictions change if some individuals incur disutility from lying. By assumption, models of lying aversion predict less overstating as some individuals state their earnings truthfully. None of these models, however, propose motivations for individuals to report others for overstating. We provide an informal discussion assuming that, in addition to individuals who incur disutility when they lie, there are *indignant*

[4] Reporting carries the opportunity cost of lying since all who overstate, including the reporter, are sanctioned.

[5] All voting strategies that lead to all organizations having three players are equilibria. However, accepting all separated individuals weakly dominates other voting strategies, making it the safer option.

individuals who incur a utility loss when they or others in their organization lie, but whose utility loss is smaller if liars are sanctioned.[6] In Random, indignant individuals ought to reduce or even eliminate lying behavior. In Selection, this might not occur because indignant individuals can be avoided by forming dishonest organizations. Specifically, an individual who only maximizes her earnings is better off in an organization of $n_g = 2$ where the probability of being reported is low than in one of $n_g = 3$ where the probability of being reported is high,[7] in which case it is optimal for her to veto those who reported others in the past. Note that individuals who report might be vetoed even by individuals who incur disutility when they lie because they understand that they might be tempted to lie when the benefits of lying are considerable. If reporting sufficiently increases the probability of being vetoed, it is possible that indignant individuals will stop reporting altogether.

3. Results

We test the statistical significance of our findings using regressions with standard errors clustered on societies (all *p*-values correspond to two-tailed tests). Moreover, unless it is otherwise noted, we include subject random effects. Table 1 shows the mean and standard deviation per period for the key variables. The appendix contains additional statistical analyses, including treatment comparisons using non-parametric tests, an analysis of the effects of organization size, and additional descriptive statistics.

3.1. Overstating

Figure 1 describes overstating behavior. The left panel shows the distribution of the amount overstated *per period* as a fraction of the maximum overstatement. In a given period, the modal choice is honesty. However, overstatements occur regularly and are

[6] Certainly, other motivations for reporting overstatements can exist. For example, reporting could be motivated by lying averse individuals who dislike inequality (Fehr and Schmidt, 1999) or enjoy earning more than others (Frank, 1985). In the appendix, we show that these motivations are not key predictors of reporting behavior.

[7] Specifically, if $\rho(n_g)$ is the probability that at least one other individual in the organization reports, then it is optimal to veto others and overstate maximally when $\rho(n_g = 3) > 1/3$ (so that dishonesty does not pay in $n_g = 3$) and $\rho(n_g = 2) < 2/9$ (so that dishonesty pays more in $n_g = 2$ than honesty in $n_g = 3$).

Table 1 – Descriptive statistics

Treatment	Random	Selection		
Organization size	$n_g = 3$	$n_g = 2$	$n_g = 3$	Both
Points overstated $(s_i - t_i)$	29.7	54.7	66.2	63.1
	(69.3)	(71.2)	(96.4)	(90.5)
Fraction of points overstated $(s_i - t_i)/(t_g^{max} - t_i)$	19.6	51.1	41.1	43.8
	(36.2)	(48.8)	(46.8)	(47.6)
Fraction reporting	16.9	4.9	13.4	11.1
	(37.5)	(21.6)	(34.1)	(31.5)
Fraction reporting conditional on others overstating	31.6	8.7	19.2	16.8
	(46.5)	(28.3)	(39.4)	(37.4)
Fraction sanctioned	19.2	5.4	15.2	12.6
	(39.4)	(22.6)	(36.0)	(33.2)
Fraction sanctioned conditional on overstating	55.7	9.6	29.3	23.7
	(49.8)	(29.5)	(45.6)	(42.6)
Final payoff	129.0	161.2	146.3	132.5
	(153.7)	(90.5)	(192.3)	(167.9)

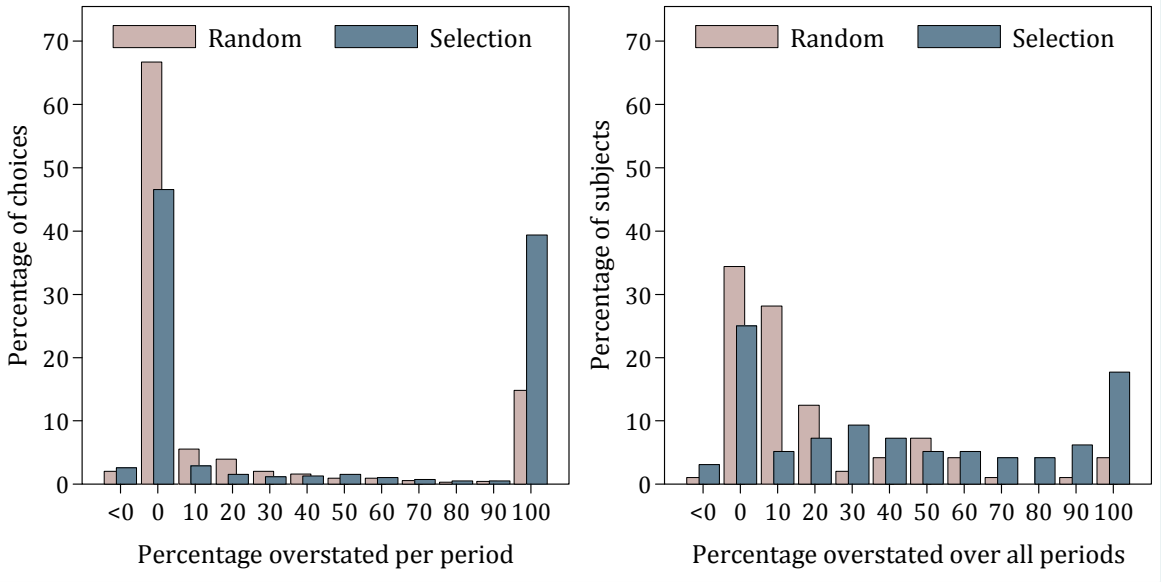
Note: All fractions are in percent. Standard deviations are in parenthesis. Of the 32 organizations in Selection, after the third period 17 are of size $n_g = 2$ and 15 of $n_g = 3$.

usually maximal, i.e. $s_i = t_g^{max}$. The right panel shows the amount overstated *per subject* over all periods as a fraction of their maximum overstatement. Most subjects overstate, but they do not do so maximally in all periods.

We see in Table 1 that the fraction of points overstated in Random is considerably lower than in Selection. A Tobit regression (censoring at 1 and 0) with a treatment dummy variable confirms that this difference is statistically significant ($p = 0.016$). In the appendix, we analyze the determinants of overstating. We find that overstating is more likely if the gain from lying is large and subjects observe or experience unpunished lying in the past (similar results are reported by Gino et al., 2009; Fosgaard et al., 2013).

Result 1 (Overstating earnings): *Subjects regularly overstate their earnings, albeit, most do not do so maximally. The option to select who enters their organization facilitates overstating.*

Figure 1 – Distributions of overstating behavior



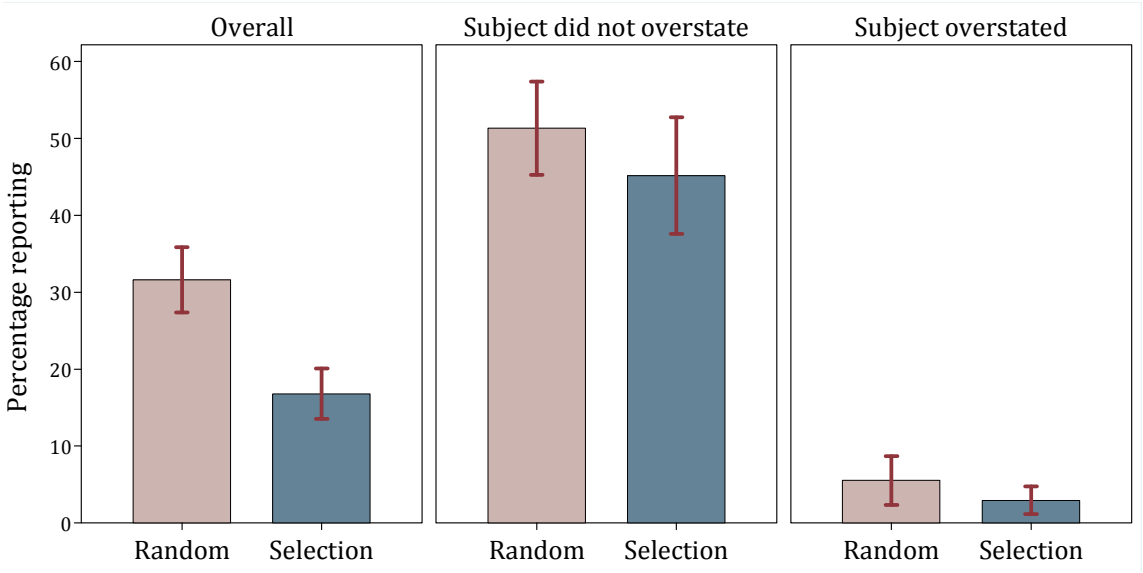
3.2. Reporting others

Table 1 shows that, conditional on witnessing at least one overstatement, subjects report overstatements 32 percent of the time in Random and 17 percent of the time in Selection ($p = 0.028$ with a logit regression). We see an even larger difference in the fraction of overstatements that are sanctioned: 56 percent in Random vs. only 24 percent in Selection ($p = 0.006$ with a logit regression).

In the appendix, we analyze the motivations to report overstatements. We find that observing others overstate when one has not overstated is the most important determinant of reporting. Interestingly, if we control for having overstated then there are no longer treatment differences in reporting (illustrated in Figure 2). This is consistent with the presence of indignant individuals who sanction liars but have fewer opportunities to do so in Selection.

Result 2 (Reporting overstatements): *In the absence of selection, enough subjects report others so that most overstatements are sanctioned. If organizations can select their members, reporting is less frequent and overstatements are sanctioned less often.*

Figure 2 – Reporting conditioning on having overstated



Note: Error bars correspond to 95 percent confidence intervals.

3.3. Organization member selection

We first analyze how subjects decide whom to accept into their organization. To do so, we run logit regressions with a dependent variable that equals one if subject *i* vetoes separated subject *j* and zero otherwise. In the first, the independent variables are: (i) a dummy variable equal to one if *j* was sanctioned for overstating; (ii) *j*'s mean stated earnings; and (iii) a dummy variable equal to one if *j* reported others for overstating. In the second, these variables are interacted with three dummy variables that indicate whether: *i* never overstated, *i* sometimes overstated, or *i* always overstated. We use subject and period fixed effects. Table 2 displays the results.

We find that the odds of being vetoed by *i* significantly increase if *j* reported others. This effect is to be expected if *i* overstated. Interestingly, the effect is also significant if *i* never overstated. Additionally, we find that the higher *j*'s stated earnings, and hence the probability that *j* overstated, the higher the chance of acceptance by *i*. Finally, we do not see a significant effect of having being sanctioned.

Table 2 – Determinants of vetoing

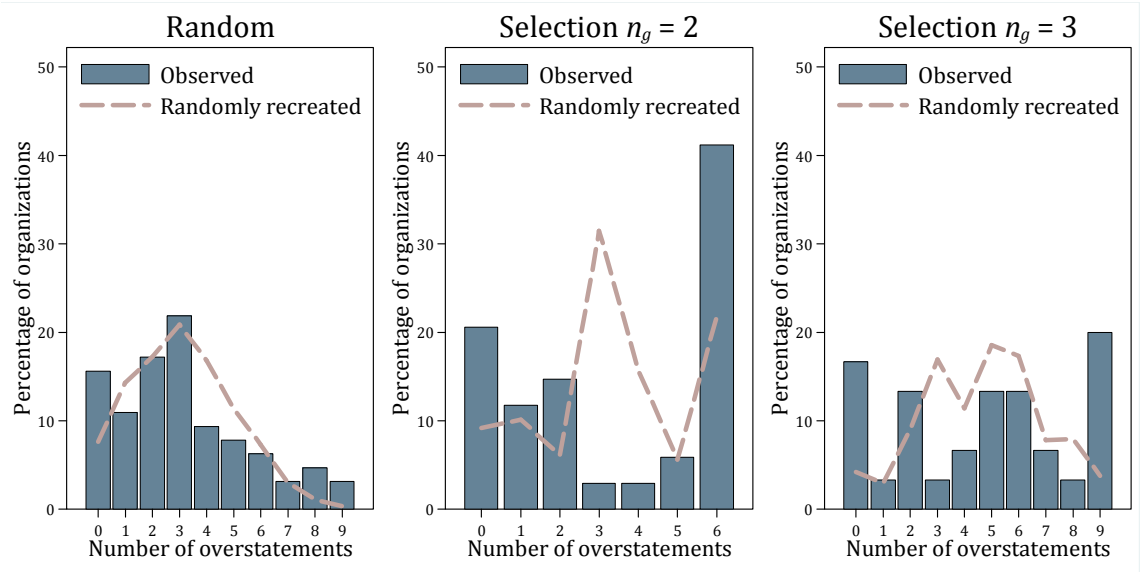
	Specification I		Specification II	
Independent variables	o.r.	s.e.	o.r.	s.e.
j was sanctioned	1.19	(0.26)		
j's stated earnings	0.51***	(0.13)		
j reported others	2.47***	(0.40)		
i never overstated × j was sanctioned			1.25	(0.19)
i never overstated × j's stated earnings			0.61**	(0.14)
i never overstated × j reported others			2.15***	(0.50)
i sometimes overstated × j was sanctioned			1.37	(0.48)
i sometimes overstated × j's stated earnings			0.61**	(0.15)
i sometimes overstated × j reported others			3.11**	(1.72)
i always overstated × j was sanctioned			0.99	(0.48)
i always overstated × j's stated earnings			0.38***	(0.13)
i always overstated × j reported others			2.82***	(0.73)
Period fixed effects	Yes		Yes	
Subject fixed effects	Yes		Yes	
Pseudo R^2	0.17		0.19	
# of obs./subj./societies	464/68/8		464/68/8	

Note: Significant at 1 percent (***), 5 percent (**),10 percent (*).

We next evaluate the impact of selection on organizational composition. Figure 3 presents evidence that the selection process creates highly dishonest organizations. The bars plot the distribution of organizations according to number of overstatements in a three-period interval (for intervals after selection has taken place: periods 4-6 and 7-9). As a benchmark that controls for the different propensities to overstate depending on the treatment and organization size, the lines plot the mean distribution that results if we randomly reassign subjects to organizations keeping constant their treatment, organization size, and overstatement behavior.[8] For a given three-period interval, only 3 percent of organizations are fully dishonest in Random. By contrast, in Selection, fully dishonest organizations correspond to 20 (41) percent of organizations of $n_g = 3$ ($n_g = 2$). This is well below the fraction that emerges by randomly recreating organizations: 4 (22) percent for $n_g = 3$ ($n_g = 2$), which illustrates the importance of selection and peer effects to sustaining highly dishonest organizations.

[8] Distributions are based on 50,000 draws.

Figure 3 – Distribution of organizations according to the percentage of overstatements

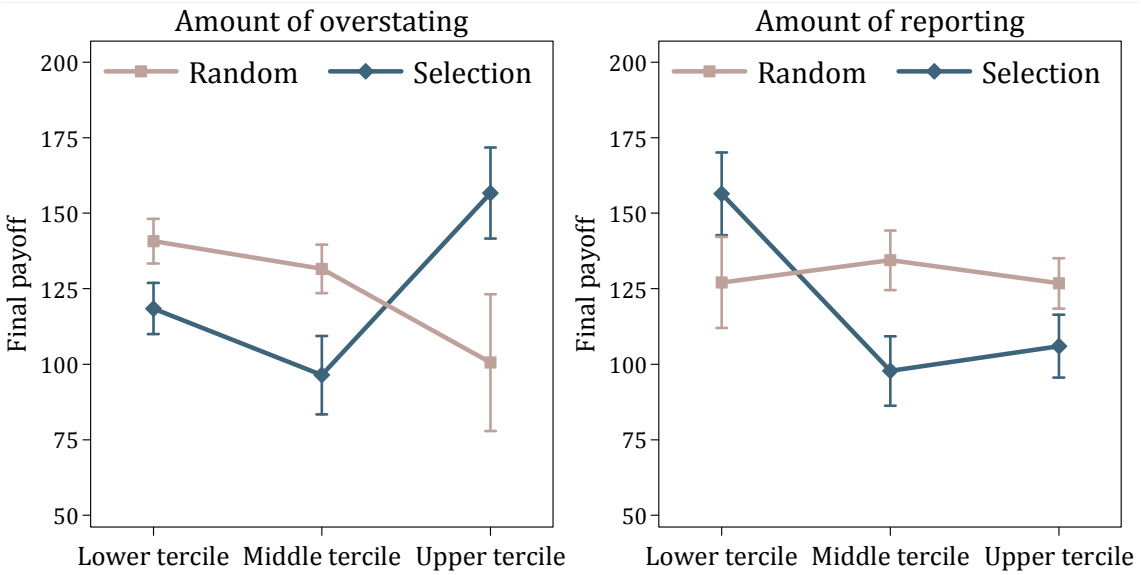


Result 3 (Acceptance into organizations): *Subjects who reported others have difficulties rejoining organizations as they are vetoed by those who overstate and those who do not. By contrast, subjects with high stated earnings, which suggest that they overstated, are welcomed. This selection process leads to the formation of highly dishonest organizations.*

3.4. Final payoffs, overstating, and reporting

Figure 4 plots the relationship between the subjects' payoff over all nine periods and the fraction of times subjects overstate (left) or report others (right), both in terciles. In Random, we see that the tercile of subjects who overstate most have *lower* payoffs than the rest. By contrast, in Selection, the converse is true. We also see that reporting others has no perceivable effect on payoffs in Random, but it has a strong negative effect in Selection. To test whether these relations are statistically significant, we regress the subjects' payoff on the fraction of times each subject overstated or reported others. We use GLS regressions with society fixed effects. In Random, we obtain a significantly negative coefficient for the fraction of overstatements ($\beta = -67$ points, $p = 0.012$) and a positive but not significant coefficient for the fraction of reports ($\beta = 28$ points, $p = 0.519$). By contrast, in Selection the coefficient for fraction of overstatements is significantly positive ($\beta = 59$ points,

Figure 4 – Final payoff depending on the amount of overstating and reporting



Note: Error bars correspond to one standard error.

$p = 0.010$) and the coefficient for the fraction of reports is negative and weakly significant ($\beta = -76$ points, $p = 0.081$).

Result 4 (Final payoffs): *In the absence of selection, dishonest subjects have lower payoffs than honest subjects. However, if organizations can select their members, the converse is true. Lastly, with selection, the lowest payoff is obtained by subjects who report others for overstating.*

4. Conclusions

We study whether individuals are willing to sanction people who lie for personal gain by reporting them to a central authority. We find that, in randomly assigned groups, enough people are willing to report lying to make lying unprofitable. We also investigate how the frequency of lying and reporting lies is affected when individuals can select who joins their group. Our results indicate that this type of selection is already enough to increase the frequency of lying and decrease the amount of reporting by facilitating the formation of a significant number of dishonest groups in which lying is prevalent and reporting is nonexistent.

The fact that some individuals report others for lying, even when lies are not directed toward them and cause no obvious harm, suggests that they consider lying *per se* as normatively undesirable behavior that deserves to be punished. This fact calls for models that assume that lying violates a social norm that is followed and enforced depending on the expectations of others (e.g., López-Pérez, 2012; Battigalli et al., 2013) and is not simply an individual cost.[9]

Our work also highlights the fact that people who report others for being dishonest are not always well received or sought after, even by individuals who act honestly. This is an important finding because it implies that reporting dishonest actions is very costly, as reporters can be ostracized even from truthful organizations. This helps explain the dismal careers of employees who are whistleblowers (Dyck et al., 2010) and calls for caution when it comes to policies that reveal their identity. As discussed, avoiding those who report is consistent with lying averse individuals who are generally honest but understand that they might be tempted to lie. Alternatively, reporters might be shunned because they are disliked for being "holier than thou" types (for support of this idea, see Parks and Stone, 2010). Further research using variations of our game can be used to differentiate between these two explanations.

As in most experimental studies, our study abstracts away from many elements of real life in order to cleanly identify specific effects and motivations. However, the whistleblowing game can be easily modified to evaluate the effect of different intrinsic and extrinsic incentives to tell and report lies. For instance, it is interesting to evaluate the effect of individuals being able to exit or be fired from their organization. In this case, honest individuals might prefer to leave instead of reporting others, or be fired before they have a chance to do so, which would lead to more dishonesty. Similarly, it would be fascinating to test whether extrinsic costs and benefits of reporting affect how reporters are perceived by others. If reporting is costly, it ought to be perceived as more altruistic, which might help the career prospects of reporters. By contrast, if reporting is beneficial then it becomes a

[9] Evidence of the importance of context on the propensity to deceive others can be found in Belot and Schröder (2013), Cappelen et al. (2013), Gravert (2013), Jiang (2013),.and Kriss et al. (2013).

weaker signal of altruism, which could lead to increased shunning (see Bénabou and Tirole, 2006). This type of research can potentially inform policies such as the granting of immunity to whistleblowers or rewarding them through *qui tam* statutes.

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