THE EFFECTS OF PAY RAISE FREQUENCY WITHIN GROUPS ON COLLUSION

by

Ashley K. Sauciuc

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As members of the Dissertation Committee, we certify that we have read the dissertation prepared by Ashley Sauciuc, titled The Effects of Pay Raise Frequency within Groups on Collusion and recommend that it be accepted as fulfilling the dissertation requirement for the Degree of Doctor of Philosophy.

Jeffrey Schatzberg — Date: 10/22/19

Max Hewitt — Date: 10/22/19

Jeremy Douthit — Date: 10/22/19

Final approval and acceptance of this dissertation is contingent upon the candidate's submission of the final copies of the dissertation to the Graduate College.

I hereby certify that I have read this dissertation prepared under my direction and recommend that it be accepted as fulfilling the dissertation requirement.

Jeffrey Schatzberg — Date: 10/22/19
Dissertation Committee Chair
School of Accountancy

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ABSTRACT

This study examines how the frequency of performance-based pay raise opportunities affects collusion within groups. I predict that expectations of future reciprocity between group members will increase the likelihood of collusion during raise periods compared to non-raise periods, even in a setting subject to deterrent controls (e.g., mutual monitoring). Furthermore, the frequency of these raise periods determines which of two theoretical reporting norms develops. I find that groups with relatively infrequent pay raises oscillate between collusion during raise periods and truthful reporting during non-raise periods, consistent with moral licensing theory. Conversely, when pay raise frequency is high, I document a bleed-over effect whereby collusion spreads into the non-raise periods, consistent with ethical erosion. Specifically, while fewer participants in the high (vs. low) frequency conditions colluded during raise periods, those that did tended to collude throughout.

I. INTRODUCTION

While group work is key to many organizations, offering logistical, synergistic, creative, and risk-sharing benefits (Holmstrom 1982; Steiber 2014; Hargreaves, Boyle, and Harris 2014), it is not without drawbacks. This study focuses on a particularly costly drawback: collusion. Collusion reflects any private, side-arrangements among employees to behave in a way contrary to the principal's interests (Baiman, Evans, and Nagarajan 1991; Villadsen 1995). In the Association of Fraud Examiner's 2016 report, about half of the reported fraud involved collusion, the median loss of which was nearly triple the cost of the non-collusive frauds. To better understand the factors that may propagate collusion, this study examines an important element of practice that has been overlooked in prior research: pay raise frequency among group members. Specifically, I examine how pay raise frequency within groups affects the likelihood of collusion in a multi-period reporting setting.

For the purposes of this study, pay raise frequency reflects the number of times an employee is eligible for a significant performance-based raise within a given time-frame.[1] The basic tenets of agency theory assert that organizations structure the relative weight and frequency of incentives to align employees' short- and long-term goals with those of the firm (Lambert 2001; Gibbs 1995). Wages reflect long-term goal alignment and are typically set to meet or exceed some reservation wage in an effort to attain and retain talent in the labor market (Lambert 2001; McConnell, Brue, and Macpherson

[1] Significant pay raises are those that draw focal interest. Mitra, Gupta, and Jenkins (1997) find that "below about the 7% level, increases in pay amounts are unlikely to evoke positive perceptual and attitudinal reactions among employees." Thus, while firms may provide minimal annual raises every year, these often fall at or below inflation rates and are not considered in this study.

2017). Pay raises become necessary over time as reservation wages increase with experience. Often these raises are dependent on performance (Gibbs 1995; Grabner and Moers 2013), inherently increasing the salience of current performance during the period subject to the raise opportunity. As such, the frequency with which someone is eligible for a pay raise becomes an important consideration for firms when striking a balance between short- and long-term goals.

While achieving such balance may be difficult for an individual employee working alone, it is even more complex within groups. Given some degree of heterogeneity within a group, the timing of pay raise eligibility is often staggered among group members. For example, there may be two employees that were hired in different years but currently perform similar duties within a group. As many firms structure their pay raise and promotion schedules based on years of service, this results in staggered pay raise opportunities between group members.[2] In such cases, employees may attend to their group members' pay raises as well as their own, thereby affecting the perceived frequency of pay raise incentives and possibly overemphasizing the value of short-term goals.

Pay raise opportunities inherently increase an employee's desire to collude during their raise periods due to the high expected value of the future benefit. However, with staggered raises, an unaffected group member may have no immediate, direct incentive to collude when it is not their own raise period. Firms also often implement controls to further disincentivize collusion. One such example is mutual monitoring, which requires

[2] This is consistent with evidence from field studies that document significant wage variation within hierarchical levels cross-sectionally and over time (Baker, Gibbs, and Holmstrom 1994; Gibbs 1995).

each employee to report his or her own private information as well as that of the other employees in the group (Ma 1988).[3] When reports among employees differ, any observed dishonesty is penalized while honesty is rewarded. Thus, if each employee is sufficiently incentivized to blow the whistle on a dishonest group member and the penalty for behaving dishonestly is sufficiently high, arguably the firm can achieve its first-best solution where every employee reports honestly.[4]

When such strong disincentives to collude exist, why would an unaffected employee ever agree to collude in their group member's raise period? Expectations of future reciprocity likely play a significant role. Employees often work in the same group over multiple periods. Thus, employees currently unaffected by a group member's raise try to benefit from reciprocal cooperation in their own future raise period, thereby increasing the likelihood of collusion in raise periods compared to non-raise periods. The frequency with which this occurs, in turn, significantly affects the development of different group honesty norms in other, non-raise periods.

Honesty norms are important in this setting as collusion inherently requires dishonesty, which can cause psychological disutility and cognitive dissonance (Gneezy 2005; Hannan, Rankin, and Towry 2006; Mazar, Amir, and Ariely 2008; Rankin, Schwartz, and Young 2008). However, if the combination of the risk-adjusted material payoffs and the utility gained from reciprocity exceed one's disutility from lying, then it is rational for that person to collude. Therefore, if an employee is able to sufficiently

[3] In theory, this could reflect effort or other costs that are directly unobservable to the firm and which are observed with relative certainty by the other employees within a group.
[4] I discuss mutual monitoring, as well as the unique subgame perfect Nash equilibrium (truthful reporting) and Pareto optimal outcome (collusion) parameterized in this paper in detail in Section II and the Appendix.

minimize their disutility from lying then collusion in non-raise periods may still manifest. Recent work in psychology highlights two popular mechanisms individuals use to minimize this disutility, which predict conflicting results based on pay raise frequency.

First, compensatory ethics (Cornelissen, Bashshur, Rode, and Le Menestrel 2013; Zhong, Ku, Lount, and Murnighan 2010) posits that individuals build up "moral license" by behaving ethically over time, providing a feeling of entitlement to behave unethically on a subsequent occasion. This theory predicts that employees will build up their moral license by reporting honestly during non-raise periods and colluding only during raise periods. Less frequent raises should provide employees more time to build up that license, thereby increasing the likelihood of collusion during raise periods when compared to settings with frequent raises.

Conversely, moral disengagement theory (Bandura 1986; 1999) asserts that individuals use common rationalizations to justify their improper behavior, which subsequently decreases the likelihood that they see such behavior as wrong in the future. This implies that once an employee starts lying during raise periods, the marginal disutility (or psychological cost) of each subsequent lie should decrease, resulting in ethical erosion (Welsh, Ordonez, Snyder, and Christian 2015). Thus, contrary to compensatory ethics, this theory predicts increased collusion across all periods when pay raises are more frequent.

I test these competing predictions using a multi-period experiment with a 2 × 2 mixed design, where I manipulate the type of period (Raise vs. Non-raise) within-subjects and the frequency of raise periods at two levels (Low vs. High) between-subjects. I randomly paired participants into two-person groups, which they retained for the entirety of the experiment (i.e. 35 periods). Each group engaged in a die-roll reporting task each period, subject to mutual monitoring. Consistent with prior studies using mutual monitoring (e.g., Towry 2003; Zhang 2008; Hannan, Towry, and Zhang 2013), participants had incentive to misreport each period to obtain performance-based pay (i.e., a bonus), while rewards and penalties were in place such that the Nash equilibrium was truthful reporting every period. Unique to my study is the incorporation of pay raise opportunities. As such, there were also designated "raise" periods, where participants had the opportunity to increase their base wage in perpetuity. In the low pay raise frequency condition, this opportunity occurred twice for each group member; whereas in the high condition, it occurred eight times each.

Results show that collusion did occur more often in raise periods than non-raise periods. More importantly, I observe a significant disordinal interaction with respect to pay raise frequency and period type. Under low pay raise frequency, individuals behaved consistent with moral licensing theory: colluding during raise periods and then reporting honestly during non-raise periods. Conversely, under high pay raise frequency, individuals behaved consistent with the ethical erosion theory. Instead of oscillating their behavior, I observe a bleed-over effect from the raise periods into the non-raise periods. Thus, while fewer groups in this condition colluded during raise periods, those that did then continued to collude in the non-raise periods.

Overall, these results suggest that collusion may be significantly affected by the collective frequency of pay raises within one's group, not just one's own. Specifically, reciprocity concerns appear to significantly affect employee behavior during raise periods and, when combined with the lower marginal disutility from misreporting under frequent pay raises, firms may be effectively shifting the reporting norm towards collusion in all periods. From a practitioner perspective, the results of this study caution management to consider how employees may perceive the frequency of pay raises when working in groups, especially when attempting to align employees' incentives with those of the firm.

My study makes several important contributions. First, I extend our understanding of group incentives in compensation contracting (e.g., Arya, Fellingham, and Glover 1997; Rankin 2004; Baldenius, Glover, and Xue 2016) by highlighting the fact that individuals attend to group members' incentives, even when those incentives do not immediately and directly benefit them. Second, I contribute to the rich accounting literature on reporting honesty (e.g., Evans, Hannan, Krishnan, and Moser 2001; Rankin et al. 2008; Church, Hannan, and Kuang 2012) by showing how frequent pay raise opportunities can result in a deterioration of reporting honesty over time, causing a spill-over effect into non-raise periods. This finding contributes to the relatively nascent literature incorporating theory on ethical erosion from psychology into accounting (e.g., Schrand and Zechman 2012; Brown, Rennekamp, Seybert, and Zhu 2014; Reckers and Samuelson 2016).

I also extend the literature on reciprocity in accounting by exploring an important, novel intersection of reporting dishonesty and reciprocity. Prior research shows that negative reciprocity concerns contribute to dishonesty and collusion as subordinates repay perceived unkindness by their superior with unkindness (Zhang 2008). However, I find that positive reciprocity concerns between peers can contribute to dishonesty and collusion. By examining a setting where dishonesty is required for both parties to collude and maintain positive reciprocity, I thereby show that these reciprocity gains can outweigh the disutility from lying.

Additionally, extending studies on heterogeneity in groups that focus on endogenous differences in productivity between group members (Chan, Kachelmeier, and Zhang 2017; Arnold, Hannan, and Tafkov 2018), I examine heterogeneity that is exogenous to the individual (i.e., variation in the timing of pay raises between group members). This is an important distinction as such exogenous differences in an organization's compensation architecture are more controllable to the firm. Finally, I identify a boundary condition to the effectiveness of mutual monitoring as a control for collusion that is novel to prior work but prevalent in practice (e.g., Towry 2003; Hannan et al. 2013; Evans, Moser, Newman, and Stikeleather 2016).

II. THEORY

The Economic Setting

The economic setting of this study is based on the principal-agent model proposed in Ma (1988) and used in various mutual monitoring studies (e.g., Zhang 2008; Towry 2003; Evans et al. 2016). In the Ma (1988) model, two employees report their performance to the firm, whereby their reports are subject to mutual monitoring over multiple periods. The two employees (A and B) are assumed to be risk-neutral and there is no explicit pre-play communication possible between them, since riskless communication is not prevalent in practice. Any attempt at initiating a collusive relationship carries risk (e.g., ex-ante whistleblowing, harm to reputation, etc.), which could be very costly to a potential colluder.

In this setting, both employees perform their jobs, the results of which are common knowledge to each other but unknown to the firm. The firm conducts a performance evaluation at the end of each period and compensates the employees accordingly. For simplicity, assume there is always an incentive for some degree of collusion to achieve higher pay, either by reaching a set target or by increasing positive variance. Since the firm cannot observe actual performance with certainty, it relies on its employees to mutually monitor each other's reported performance.

In the first stage of this reporting task, both employees observe the actual outcome (x_t) for their group. In the second stage, they each independently report their group's joint outcome ($x^*_{i,t}$, where *i* = {A, B}). If both agents' reports agree ($x^*_{A,t} = x^*_{B,t}$), they each receive their wages plus any performance-based earnings. However, if the reports disagree ($x^*_{A,t} \neq x^*_{B,t}$), an audit occurs, revealing the actual performance to the firm. After an audit, the dishonest employee is fined, while the honest employee receives a reward. Therefore, the payoff function in period *t* is:

$$\pi_{i,t} = wage_{i,t} + bonus_{i,t} - fine_{i,t} + reward_{i,t}$$

where:

$wage_{i,t}$ $= wage_{i,t-1}$;

$bonus_{i,t}$ > 0 and increasing in $x^*_{i,t}$;

$fine_{i,t}$ $= wage_{i,t} + bonus_{i,t}$ if $[x^*_{i,t} \neq x^*_{j,t}$ and $x^*_{i,t} \neq x_t]$, and 0 otherwise,

$reward_{i,t} > 0$ if $[x^*_{i,t} \neq x^*_{j,t}$ and $x^*_{i,t} = x_t]$, and 0 otherwise;

such that $fine_{i,t} \geq bonus_{i,t} > 0$ and $reward_{i,t} > bonus_{i,t}$.

Additionally, a key distinction to my study is the incorporation of a component for performance-based pay raises ($\varphi_{i,t}$). I assume that pay raise opportunities are staggered temporally, such that $\varphi_{A,t}$ equals zero when $\varphi_{B,t} > 0$, and vice versa.[5] Additionally, achievement of the pay raise is contingent on performance (i.e., meeting or beating an assigned target), such that either $x_t \geq target$ or $x^*_{B,t} = x^*_{A,t} \geq target$.

A firm can theoretically achieve its first-best solution under mutual monitoring since the unique subgame perfect Nash equilibrium is for both employees to truthfully report, eliminating the need for any costly audits (e.g., Ma 1988; Zhang 2008). As raises are symmetric and monotonically increasing components of the pay function, they do not affect the mechanics of the equilibrium or Pareto optimal payoff decisions discussed herein (see Appendix). Instead, I expect the existence and frequency of pay raise opportunities to shift behavior away from the Nash equilibrium towards the Pareto optimal due to social preferences.

Social Preferences

Rational behavior in strategic games is heavily dependent on one's beliefs about others. In my setting, these beliefs are importantly influenced by: material payoffs, reciprocity, and lying aversion. The functional form of the material payoffs under mutual monitoring is defined above; whereas the two non-monetary social preferences require further attention.

[5] If raise opportunities are not staggered, both employees would be subject to direct monetary incentives each raise period. Staggering, therefore, enables better disentanglement self-interested behavior from social preferences.

Reciprocity

Reciprocity reflects behavior conditioned on the perceived intentions of another's actions (Cox 2004; Falk and Fishbacher 2006). It dictates an individual will respond to perceived (un)kindness with (un)kindness and, due to this "sign-matching" feature, either reciprocal response will add to one's psychological utility (Dufwenberg and Kirchsteiger 2004).

First, let us consider the effects of reciprocity from the perspective of the employee with the second available pay raise opportunity (i.e., employee B). Employee B typically defaults to truthful reporting because it is always materially beneficial for either employee to blow the whistle on any attempts to collude. Furthermore, during A's raise periods, B's direct monetary incentives do not change. However, the compensation at risk for A during those periods is not just the current period's payoff. Instead, the expected value of the future benefit from the pay raise heightens the monetary incentives for A to collude. Knowing this, B can signal "kindness" by agreeing to collude during A's first raise period, in expectation that A will respond reciprocally in B's future raise period.

In a real sense, B expects that A gains some utility from being reciprocal, and B desires to signal his kindness to A such that it is utility maximizing for A to forego the economic incentives to whistleblow on B during B's future raise period. This reciprocal relationship is most salient in pay raise periods and should increase collusion in these periods. As such:

H1: The likelihood of collusion is greater in pay raise periods than non-raise periods.

The Marginal Cost of Lying

Collusion requires that each employee misreports to achieve some common benefit, whether that benefit is obtained contemporaneously or at some point in the future. Yet misreporting itself depends on each individual's inherent aversion to lying (Gneezy 2005; Murphy 2012; Mayhew and Murphy 2014) or their aversion to *appearing* dishonest to another person (Hannan et al. 2006; Gneezy, Kajackaite, and Sobel 2018; Dufwenberg and Dufwenberg 2018). Specifically, if the combination of one's risk-adjusted expected material payoffs and the utility gained from reciprocity does not exceed one's aversion to lying, then that agent should not report dishonestly. Yet, when collusion has successfully occurred once, it is more likely to occur again (Evans et al. 2016). This is consistent with the notion that the marginal psychological disutility of lying decreases with each subsequent lie, resulting in ethical erosion.

Research in psychology (e.g., Gino and Bazerman 2009; Welsh et al. 2015) explores this phenomenon with respect to various ethical decisions. Gino and Bazerman (2009) find that individuals are less likely to blow the whistle on misbehavior when ethics are eroded gradually over time as opposed to in one abrupt shift. The authors attribute this "slippery slope effect" to individuals' failure to notice the erosion when it is gradual. Welsh et al. (2015) further explore the evolution of unethical behavior over time, documenting ethical erosion in one's *own* actions. They provide preliminary evidence that the effect operates through moral disengagement.

Moral disengagement reflects the suspension of one's psychological self-sanctioning function in order to advantageously restructure events and behaviors to lessen the negative impact on one's self-perception (Bandura 1986; 1999). When actions and beliefs are not psychologically consistent (e.g., committing an unethical act and believing oneself to be an ethical person) they create cognitive dissonance, such that individuals are motivated to reduce the inconsistency (Festinger 1962; Mazar et al. 2008). Accordingly, people tend to use a common set of rationalizations, such as moral justification or diffusion of responsibility, to disengage and validate their unethical behavior (Bandura 1999; Bandura, Barbaranelli, Caprara, and Pastorelli 1996; Detert, Treviño, and Sweitzer 2008; Moore, Detert, Treviño, Baker, and Mayer 2012; Mayhew and Murphy 2014; Brown 2014).

Once moral disengagement has occurred, this literature argues that subsequent ethical transgressions hold less weight. However, research on compensatory ethics disputes this claim. Instead, this opposing literature claims that individuals engage in moral licensing (Zhong et al. 2010; Cornelissen et al. 2013), which reflects the belief that individuals feel licensed to misbehave after acting ethically in the past since they have built up a reserve of good behavior. Conversely, behaving unethically causes individuals to try to compensate for past wrongdoing by then behaving ethically for a time. Essentially, this theory predicts individuals will oscillate between good and bad behavior, but the empirical evidence is mixed and may depend heavily on individual characteristics (Zhong et al. 2010; Murphy and Dacin 2011; Reckers and Samuelson 2016). Relatedly, Welsh et al. (2015, 124) note that:

> A key assumption of compensatory ethics is that individuals recognize and appreciate the wrongness of their behavior. Without the acknowledgment of an ethical violation, there is no motive for morally compensatory behavior to occur. Thus, the justification of unethicality that occurs through moral disengagement may explain why morally disengaged individuals do not fully appreciate the wrongness of their actions and may continue behaving unethically.

Since rationalizations are readily available in a group setting (e.g., “I am morally justified in colluding because it helps my group member get his/her raise”; “Everybody’s doing it”; etc.), I expect employees are likely to at least partially disengage when they first attempt to collude (Church, Hannan, and Kuang 2012). However, the degree to which they disengage in unknown. On the one hand, if they fully disengage, they should be less likely to perceive future misbehavior as unethical, violating a necessary condition for compensatory ethics to hold. In that case, I expect to find support for ethical erosion, such that the marginal cost of each subsequent lie is decreasing. In concert with H1, this implies that since employees are more likely to lie during raise periods, the marginal psychological cost of lying should decrease *more* under high (vs. low) raise frequency as ethical erosion continues with each raise period. This would lead to greater collusion under high (vs. low) frequency during raise periods.

Alternatively, if employees do not fully morally disengage at first, they may continue to view future collusion as wrong. In these instances, compensatory ethics may apply and having fewer raise periods may give employees the time and opportunity to build up their moral license between raise periods (i.e., by reporting truthfully during non-raise periods). This should allow them to more easily justify collusion during raise periods. Since those with more frequent pay raises have significantly less time to build up such ethical reserves, this implies collusion would be greater during raise periods under low (vs. high) frequency. It is not possible to predict the relative magnitude of these two cases ex-ante. Thus, I present H2a in null form:

> **H2a:** The likelihood of collusion is unaffected by pay raise frequency in raise periods.

Next, let us consider these two theories during non-raise periods. Compensatory ethics predicts an oscillation in behavior, whereby employees build up their ethical reserves by reporting truthfully during non-raise periods when the incentive to lie is lower. Under infrequent pay raises, this theory fits well since employees have ample time to accumulate moral license during the non-raise periods. Therefore, compensatory ethics would predict minimal collusion in non-raise periods when pay raises are relatively infrequent.

Conversely, with frequent pay raises there is little chance to recover from misbehavior. Instead, I anticipate a shift in social norms when raises are more frequent that creates a bleed over effect of collusion in the non-raise periods. Research on the development and activation of social norms (Bicchieri 2006) suggests that norms can have a cascade effect whereby adhering to a norm continually reinforces it. As the likelihood of collusion increases in raise periods and those raise periods themselves become more frequent, there should be little to no chance for compensatory ethics to activate. Therefore, I predict individuals will more fully morally disengage under frequent pay raises and the marginal cost of lying will become negligible, such that the norm will shift from truthful reporting towards collusion in all periods. As such, I expect:

H2b: The likelihood of collusion in non-raise periods is increasing in pay raise frequency.

III. METHOD

I conducted a 2 × 2 mixed design, multi-period experiment using 62 undergraduate business students at a large U.S. university. I manipulated (a) the type of period (Raise vs. Non-raise) within-subjects and (b) the frequency of raise periods at two levels (Low vs. High) between-subjects. I programmed and conducted the experiment using z-Tree (Fischbacher 2007) on networked computer terminals over 8 experimental sessions. Overall, 52 percent of participants were female, 89 percent had some work experience (25.3 months on average), and the average age was 21.5. Each session took approximately 45 minutes with experimental earnings ranging from $6.00 to $20.70, including the $5 show-up fee. Average pay was $12.50 and $12.93 in the Low and High pay raise frequency conditions, respectively.

Upon entering the laboratory, participants gave their informed consent,[6] were randomly assigned to a private computer terminal, and were provided with instructions tailored to their condition. All participants within a session were subject to the same condition. Participants completed a comprehension check quiz prior to starting the experiment to verify their understanding of the instructions and the details of their respective manipulation. Any incorrect responses prompted on-screen instructions that provided clarification. Participants were unable to start the experiment until answering all questions correctly.

As collusion is most often found in small groups—particularly those with two members (ACFE 2016)—I randomly assigned participants into pairs each session. Since my hypotheses rely on expectations of future reciprocity, participants remained in the same pair for the entirety of the experiment. While participants were never aware of their partner's identity, they were aware that they would retain the same partner throughout the session. This was disclosed during the instructions and subjects' understanding was verified during the manipulation check quiz.

[6] This study was reviewed and approved by the relevant IRB.

The experimental task consisted of a dice reporting game, in which a single, electronic 10-sided die was cast for each pair per period.[7] After observing the actual roll, participants independently reported the number rolled, and their reports were compared against each other by the computer.[8] This task was repeated across 35 periods. See Figure 1 on page 50 herein for task screenshots.

Additionally, I chose to use an electronic (rather than physical) die roll as it allows ex-ante control for differences in die rolls between groups each period. Prior to the first experimental session, I used a random number generator to determine the die rolls for all 35 periods, ensuring that each number between 1 and 10 received equal weight for each roll. Every group in every session was subject to the exact same order and distribution of die rolls across the 35 periods. Using an electronic die roll also allows ex-post observation of actual results with certainty. Therefore, whenever the pair of reports agreed, they were approved by the computer automatically, regardless of reporting honesty; whereas, if the reports did not agree, the computer performed an audit, such that a dishonest report was rejected while an honest one was approved.[9]

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[7] This task is not skill-based or effort-sensitive and each die roll reflects joint production, alleviating concerns about equity in any given period.

[8] In practice, firms can accomplish mutual monitoring via a one- or two-stage verification method, whereby employees either produce (1) duplicate reports of group-level information or (2) unique reports of individual information that require subsequent verification by the other employee. I piloted (N=58) the High frequency condition using each method, finding no major significant differences in collusion overall ($p=0.267$), during raise periods ($p=0.549$), or during non-raise periods ($p=0.103$) between methods. I use the one-stage method in the main study as it did not appear to cause a differential willingness to collude and inherently provides greater control ex-ante over fairness concerns and the magnitude of potential misreporting possible given the actual die rolls.

[9] The Ma (1988) model includes a probabilistic audit function and the equilibria still hold under proper parameterization. Since the audit function is not relevant to the essence of my research question, I control for the effects of participant risk tolerance with respect to the probability of audit success by assigning the audit function 100 percent accuracy when there is any discrepancy in the submitted reports.

To establish the appropriate equilibrium and Pareto optimal outcomes per the Ma (1988) model described in the theory section, I parameterize the material payoff function described in Section II as follows:

$$\pi_{i,t} = wage_{i,t} + bonus_{i,t} - fine_{i,t} + reward_{i,t}$$

where

$wage_{i,t}$ = 500 points when period t = 1;

$bonus_{i,t}$ = $0.01x^*_{i,t} \times wage_{i,t}$;

$x^*_{i,t}$ = reported roll, an integer ∈ [1,10];

$x_{i,t}$ = actual roll, an integer uniformly distributed ∈ [1,10], such that E[x_t] = 5.5;

$target$ = 10;

$reward_{i,t}$ = $wage_{i,t}$ if [$x^*_{i,t} \neq x^*_{j,t}$ and $x^*_{i,t} = x_t$], and 0 otherwise;

$fine_{i,t}$ = $wage_{i,t} + bonus_{i,t}$ if [$x^*_{i,t} \neq x^*_{j,t}$ and $x^*_{i,t} \neq x_t$], and 0 otherwise;

In other words, if a participant's report was approved, they earned their wage (500 points initially), plus a performance bonus equal to 1 percent of their wage, multiplied by the number they reported. For example, if they reported 5, the bonus was 5 percent of their wage that period. In addition, if a participant truthfully reported the actual number rolled when their partner did not, the honest group member earned an additional "whistleblowing" reward that doubled their wage that period. If a report was rejected, that participant did not receive any earnings for that period (i.e. the fine was set equal to that period's total earnings). I reinforced participants' comprehension of this pay structure with examples in the instructions and an instructions quiz.

At the end of the experiment, participants completed a post-experiment questionnaire and the computer randomly selected one of the 35 periods for payment

(100 points = $1USD). Randomly selecting one pay period reduces the potential confound of wealth effects that may shift individuals' risk tolerances in later periods, while retaining the fundamental features of the pay raise incentive structure since every decision still affects the wage of the next period.

Manipulations

In addition to their performance bonuses, participants were also given the opportunity to achieve performance-based pay raises at various points throughout the experiment, which would increase their base wage going forward. This necessarily results in two types of periods: raise periods, in which one of the group members is eligible for a raise, and non-raise periods, where neither is eligible.[10] For my main manipulation, I then varied the frequency of raise periods at two levels: Low and High. With 35 periods total, I set the Low (High) frequency condition equal to two (eight) raise periods for each participant. I believe there is sufficient distinction between these conditions as the raise opportunities in High occur 400 percent more often than Low. Furthermore, as the timing of these raise periods never coincides within a group, nearly half (46 percent) of the periods have raise opportunities in High vs. 11 percent in Low. Such frequencies also allow me to buffer the first and last two periods with no raises while ensuring that there was at least one non-raise period between each raise period. The buffer at the start of the experiment allows participants to get used to the interface and ensure they understand the effects of their decisions, while the buffer at the end helps control for end of game effects. Refer to Figure 2 for the exact raise schedule used. Participants were provided

[10] Both group members were never eligible for a raise in the same period. Therefore, I also break this period type out further in Section IV to examine behavior in a participant's own raise periods vs. their partner's raise periods.

with the portion of this pay schedule relevant to their respective condition as part of their paper instructions. Additionally, they were alerted every period about whether it was a raise period for themselves, their partner, or neither.[11] See Figure 2, page 52 herein.

Per their instructions, both participants in a group had to report a 10 (i.e. the *target* per Section II) to achieve a raise in any given raise period. This means that 90 percent of the time, achievement of a raise would require collusion, which should provide sufficient opportunity to test my theory. If a raise is achieved, that participant's wage will increase by 200 points starting in the next period. While raises are often formally tied to a longer history of performance in practice, even professional evaluators are subject to significant recency bias. Theory on recency bias posits that users of information overweight more recent information, ignoring the base rate (Kahneman and Tversky 1972; Grether 1980). Empirical evidence supports this theory in a variety of settings, which show that recency bias significantly affects decision making even with experienced individuals, such as security analysts (De Bondt and Thaler 1990), professional accountants (Arnold, Collier, Leech, and Sutton 2000), professional forecasters (Bansal and Shaliatovich 2010), and retail investors (Nofsinger and Varma 2013). As such, I operationalize the criteria for raise achievement based on the most recent period's performance, which impounds the notion of recency bias without creating differential incentives between frequency conditions (i.e., the judgment criteria for agent A's raise never overlaps with agent B's raise period).

[11] During their own raise periods participants saw red text stating: "**Attention:** This is a raise period for **you**. If you earn your raise this period, your wage will increase by 200 points in all future periods. Your partner's wage is not affected by your raise." During their partner's raise periods, the text was blue, reading: "**Attention:** This is a raise period for **your partner**. If they earn their raise this period, their wage will increase by 200 points in all future periods. Your wage is not affected by their raise." In a non-raise period, black text read: "**Note:** This is **not** a raise period for either you or your partner." Participants had to click "Continue" to acknowledge that they had read this alert before starting each period.

As outlined in the Appendix, sufficiently significant raise opportunities may shift employees off the Nash equilibrium if their belief about their partner's likelihood to misreport is greater than their indifference point. However, there is very limited extant research that informs this parameterization with precision in my setting. Some research in psychology has found that raises below the 6-7 percent level are perceived differently and are unlikely to motivate positive reactions (Mitra et al. 1997; Schaubroeck, Shaw, Duffy, and Mitra 2008). Therefore, I set the raise amount at a constant 200 points throughout the experiment to limit the potential confound of a changing (i.e., percentage based) raise amount, while also ensuring the amount remains sufficiently large as the wage level grows (i.e., 40 percent of the initial base wage of 500 points, 10.5 percent of the maximum possible base wage of 1,900 points).[12]

Variable Measurement

Any collusion in my study is dependent on real misreporting, which I originally calculated as the difference between the actual number rolled and the reported number, scaled by the maximum possible difference. For example, if the actual roll was 6 and the reported number was 8, misreporting would equal (Report – Actual) / (Maximum – Actual) = (8 – 6) / (10 – 6) = 0.50. While this specification allows for misreporting to vary by magnitude, the vast majority of participants either chose to report truthfully or to misreport to the maximum, consistent with prior research in economics (Fischbacher and Föllmi-Heusi 2013; Gneezy et al. 2018). Specifically, only 4 of the 1,984 observations

[12] This choice necessarily means that the cumulative incentive compensation at risk across all 35 periods will vary between conditions. While participants are paid based on the results of one randomly-selected period, not cumulative earnings, there is a chance that the cumulative amount could also affect behaviors as this increases the expected pay of any given period. Therefore, I also run an additional cell in a supplementary analysis in which the raise amount is set at 50 points in the High Frequency condition. Refer to Section IV for further detail.

(0.2%) deviated from this strategy.[13] As such, I subsequently dichotomized this variable and set it equal to 1 if the participant reported something other than the actual roll that period, 0 otherwise. Since there is no rational reason for any individual to misreport without (a) the expectation that their partner will also misreport that period or (b) the desire to signal future intentions to collude, I hereafter refer to this variable as an *Attempt* at collusion. My key dependent variable, *Collusion*, was subject to the same constraints. As such, *Collusion* was dichotomized and set equal to 1 if (a) the participant and their partner both reported the *same* number and (b) that number was something other than the actual roll that period, 0 otherwise.

My primary independent variables are based on my manipulations, which I represent as indicator variables. In addition, to test my first hypothesis, I also distinguish between three specifications of raise periods. *Any_Raise* is equal to 1 for any period where either the participant or their partner had the opportunity to achieve a raise, 0 otherwise; whereas *Own_Raise* (*Ptr_Raise*) is only equal to 1 for any period where the participant (their partner) had the opportunity to achieve a raise, 0 otherwise. Conversely, *Non-raise Periods* reflect those in which no one in the group was subject to a raise.

I also measure several control variables. *Actual* is the actual number rolled in a given period, which controls for the size of the lie needed to maximize profit. This is important as prior research has shown that lying aversion may depend on the size of the

[13] Given this task requires coordination between participants, deviation from this strategy also poses greater risk of failure since there is no explicit pre-play communication between parties. It is unclear whether or how pre-play communication would affect these strategies as it is outside the scope of this study. Prior literature on cheap-talk (Duffy and Feltovich 2002; Farrell and Rabin 1996; Evans et al. 2016) indicates that such communication can increase coordination in general. As this would bias towards misreporting and collusion, I restricted communication in my setting to that naturally developed over time within pairs as revealed through real actions and did not allow any other form of pre-play communication.

lie (Dufwenberg and Dufwenberg 2018; Gneezy et al. 2018). *Lag_Report* is the number reported by the participant in the prior period.[14] I also captured various demographic and psychometric controls through participants' self-responses in a post-experimental questionnaire (PEQ).[15] I run all hypotheses tests in Section IV both with and without each of these control variables. Consistent with expectations given the random assignment of participants to my experimental conditions, I find no significant difference in my results from the demographic or psychometric variables with respect to either the relative magnitudes or significance of my test variables. Therefore, for simplification, all results presented hereafter exclude these controls.

IV. RESULTS

Hypotheses Tests

In all the following hypothesis tests, I control for period fixed effects and cluster robust standard errors at the individual level to account for the repeated measures across periods. In untabulated results, I also run each test clustering at the unique group level instead, finding similar results with respect to both magnitude and significance.[16] While

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[14] I also measured *Lag_Ptr_Report*, which is the number reported by one's partner in the prior period. However, due to multicollinearity between *Lag_Ptr_Report* and *Lag_Report* (correlation coefficient = 0.8925), I only include *Lag_Report* in my analyses.

[15] Demographic controls include: *Female*, a gender indicator; *Age,* in years; and *Work Experience*, in months. For the psychometric controls, *TraitHonest* is a self-reported level of agreement with the following statement (using a 7-pt. Likert scale): "I care about being honest." Additionally, I factor analyzed participant responses to eight other PEQ questions regarding their own trustworthiness and their trust in their partner. Participant responses to these statements loaded significantly on two distinct factors with Eigenvalues greater than one: *Trustworthy* (Cronbach's $\alpha = 0.7127$) for the five statements related to the participants' self-perceived trustworthiness, and *TrustPtr* (Cronbach's $\alpha = 0.7508$) the remaining three related to the participant's trust in their partner.

[16] I also ran all tests using mixed effects logistic regressions, with participant ID as the panel variable and period as the time variable. All results are consistent with the intuition presented herein. The sign and significance of all independent variables of interest in Tables 1, 2, and 3 remain consistent, with the following minor exceptions: (1) in Table 2, the negative (positive) effect of *High Frequency* on *Collusion* during raise (non-raise) periods decreases (increases) slightly in significance (p=0.068 and p<0.001, respectively), and (2) in Table 3, the positive effect of *High Frequency* on *Collusion* in non-raise periods increases in significance (p=0.003).

there are 35 periods in total, three of these periods had actual die rolls of 10, the maximum roll possible. As there was no opportunity for any collusion in those periods, I drop them from all further analyses, leaving 1,984 total observations (62 participants × 32 periods).

For my first test, H1 predicts that the likelihood of collusion will be higher during raise periods than non-raise periods as the incentives are much higher during those periods to engage in a tit-for-tat exchange in expectation of future reciprocity. Table 1, page 55 herein, provides support for this across three raise period specifications. Specifically, I show that both the attempts to collude (*Attempt*), as well as successful collusion (*Collusion*), significantly increase during an individual's own raise periods (*Own_Raise* 2.31, $p<0.01$ and 2.37, $p<0.01$), their partner's raise periods (*Ptr_Raise* 2.17, $p<0.01$ and 2.43, $p<0.01$), or a raise period for either group member (*Any_Raise* 2.70, $p<0.01$ and 2.80, $p<0.01$), compared to non-raise periods.

It is important to note here that these results provide supporting evidence that individuals attend to the raise periods of their group members, not just their own. In the *Ptr_Raise* model, the dependent variable equals one when one's partner is up for a raise and zero otherwise. Therefore, this describes a period in which there is no additional monetary incentive to the individual not eligible for a raise (beyond that present in any given non-raise period—i.e., the performance bonus). However, participants were much more likely to collude during such periods, implying that it is the expectation of future reciprocity and not any immediate monetary incentive that is driving behavior.

The second hypothesis shifts the focus to the differential effects of pay raise frequency, either during raise periods (H2a) or non-raise periods (H2b). As shown in

Figure 3, page 53 herein, mean *Collusion* was significantly higher during *Raise Periods* under *Low Frequency* (61.7%) than *High Frequency* (53.1%) and vice-versa during *Non-raise Periods* (8.6% and 30.5%, respectively). Model (1) in Table 2, page 56 herein, confirms the significance of this interaction using a Logit regression (coeff. -2.455, $p<0.001$). Delving deeper, Model (2) indicates that there is a negative effect of *High Frequency* on *Collusion* (coeff. -1.301, $p<0.01$) during *Raise Periods*.

Conversely, Model (3) in Table 2 shows there is significantly more *Collusion* (coeff. 18.04, $p<0.01$) under *High Frequency* during *Non-raise Periods*, consistent with a moral erosion interpretation and supporting H2b. In untabulated results, I also run all of these tests using *Attempt* as the dependent variable, finding results generally consistent in both magnitude and significance. The one exception is that the results for H2b (i.e., the positive effect of *High Frequency* during *Non-raise Periods*) is stronger ($p=0.03$). I also ran each of these models with period interactions instead of period fixed effects and the main results hold in sign, relative magnitude, and significance. Furthermore, there were no significant time trends overall or by period type. Refer to Figure 4, page 54 herein, for a graph depicting the percentage of collusion observed in each period, by condition.

Taken together, Figure 3 appears consistent with the intuition behind H2a and H2b, highlighting two distinct strategies based on the frequency of pay raises. First, those in the *Low Frequency* condition appear to oscillate their behavior depending on the period type. Specifically, they are much less (more) likely to collude during *Non-raise (Raise) Periods*. These results are consistent with the theory that those participants were able to build up their ethical reserves much higher than their counterparts in the high frequency conditions, allowing them to misreport more easily during their few raise periods.

Whereas those in the *High Frequency* condition appear susceptible to bleed over effects of ethical erosion. While there were fewer people willing to collude during *Raise Periods*, those that did ended up colluding in nearly all periods. In untabulated results, I find that approximately 50 percent of people in the *High Frequency* condition attempted to collude at least once during *Raise Periods*. Of those people, 92 percent then also attempted to collude during their *Non-raise Periods*. Furthermore, people that attempted to collude during any previous *Non-raise Period* were significantly more likely try again (coeff. 0.234, $p<0.01$), across both *Non-raise* (coeff. 0.131, $p<0.01$) and *Raise Periods* (coeff. 0.474, $p<0.01$).

Supplementary Analyses

Cumulative Wealth

As previously mentioned, if participants fixate on total incentive compensation at risk across all 35 periods (which does positively impact the expected value of their single paid period), then it is important to test the effect of pay raise frequency when such cumulative prospective wealth is held constant. Therefore, I ran an additional cell in which pay raise frequency was high (as previously defined), but the *amount* of the raise was held constant at 50 points instead of 200 points (hereafter the *High_50* condition).[17] This allows for a direct comparison to the low frequency condition from the main study, which had raises of 200 points each (i.e., *Low_200*). In both of these conditions, if a group colluded in all periods to report the maximum they would earn $7.63 on average

[17] I also ran a fourth cell for completeness (*Low_50*), in which pay raise frequency was low and the amount of the raise was held constant at 50 points each.

each period ($5.00 base wage + $0.69 bonus + $1.94 raise); whereas, if they were entirely honest, they would have earned $5.29 on average each period ($5.00 base wage + $0.24 bonus + $0.00 raise). Thus, the Pareto Optimal strategy would result in cumulative earnings of $267, compared to the Nash of $185, in both conditions.

In Table 3, page 57 herein, I replicate the tests from Table 2 using a sample of these two conditions only (i.e. *Low_200* and *High_50*). Across all periods, it appears *High Frequency* does not have a significant effect on *Collusion* (coeff. 0.311, p=0.277). However, this is due to the offsetting behavior between *Raise* and *Non-raise Periods*, which is generally consistent with the main results of this study. In *Raise Periods*, there is a significant negative effect (coeff. -2.757, p<0.001); whereas, it is marginally positive in *Non-raise Periods* (coeff. 0.990, p=0.079).

Additionally, in untabulated results, I examine the actual cumulative wealth that would have been earned had participants been paid in the aggregate across all conditions.[18] The *High_50* condition would have earned $187.61 on average, which is not significantly different than that of the *Low_200* condition ($208.99, p=0.223). Thus, with aggregate compensation at risk held constant, it appears that more people colluded during the raise periods when frequency was low; however, they colluded significantly less during the non-raise periods, resulting in insignificant differences in total wealth.

Defections

Overall, nearly 71 percent of groups in my main sample (i.e., the *Low_200* and *High_200* conditions) colluded at some point during the experiment. In other words,

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[18] Participants were actually paid based on their points for one randomly selected period. The purpose of this cumulative wealth analysis is to evaluate the potential aggregate, long-term effects to the firm given the exhibited behavior. It is possible results would vary if participants were actually paid based on their cumulative earnings as preferences, such as risk tolerance, may shift with greater wealth.

approximately 29 percent never colluded (specifically, 20 percent and 37.5 percent of groups in *Low_200* and *High_200*, respectively). Moreover, in the first raise period 30.0% (28.1%) of individual participants in *Low_200* (*High_200*) were completely honest and did not even attempt to collude. This is consistent with prior honesty studies that report honesty ranging between approximately 20 – 50% (e.g., Evans et al. 2001; Rankin et al. 2008; Church et al. 2012).

Of the 22 groups (44 participants) that did collude, the initial attempt at colluding (*Attempt*) was typically around the first raise period (Period 3), with successful collusion (*Collude*) being achieved fairly quickly thereafter. Furthermore, 50 percent of these colluding groups never defected in any period.[19] This means that once a collusive strategy was established—whether it was an oscillation strategy (moral licensing) or a full collusion strategy (ethical erosion)—those groups never deviated, even in Period 35. Of the groups that did experience some defection, the vast majority (96 percent) of those defections occurred before period 35 and most never succeeded in colluding again. In untabulated results, player A (the first raise-affected player) was the initial defector 46 percent of the time, indicating that neither role had a significant advantage over the other with respect to understanding the incentive to defect.

One counterargument for my results related to moral licensing is that collusion is significantly more likely during raise periods in the low frequency conditions because participants in those conditions do not have the opportunity to "catch up" on missed

[19] Operationally, *Defection* is equal to 1 in a period when a previous collusive strategy had been formed and at least one group member deviates from that strategy by reporting honestly, 0 otherwise. Once *Defection* occurs, it is possible again in a future period only if another collusive strategy is subsequently formed. Collusive strategies are categorized based on prior periods as follows: (a) successfully colluded during a raise period, (b) successfully colluded during a non-raise period, and (c) successfully colluded in both a raise and non-raise period.

opportunities like those in the high frequency conditions. In other words, each raise is simply more important because there are fewer of them. However, this does not take into account the cost of defection and how it affects the ability to "catch up" in future raise periods.[20]

I measure the *Cost of Defection* as the difference between (a) the hypothetical cumulative points that would have been earned if the group maintained their collusive strategy and (b) the actual cumulative points earned. This difference is then scaled by the actual to control for differences in the amount of accumulated wealth available across the two main conditions. On average, the defector groups actually earned 26,091 points per person. However, if they had continued their respective collusive strategies once formed, they would have earned an average of 34,633 points each (i.e., an additional 19 percent on top of their actual points). These foregone points reflect each individual's "cost" of defection. Furthermore, this cost of defection is significantly higher for those in *High_200* (vs. *Low_200*) ($p<0.001$).

Moreover, even holding cumulative wealth constant (i.e. comparing *High_50* and *Low_200*), support for the counterargument would require that the cost of defection be significantly lower in *High_50*. However, untabulated results show just the opposite. Specifically, the average cost of defection for the initial defector in *High_50* was 14.7% vs. 1.6% in *Low_200* ($p=0.024$). When combined with their partner, the average costs rose to 18.3% in *High_50* vs. 5.1% in *Low_200* ($p=0.038$).[21] These results do not support the argument that the raises simply hold more weight for the low conditions due to their

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[20] It is important to note that these "costs" are from the perspective of the individuals, not the firm.
[21] Results also hold using the raw (unscaled) points as the cost of defection when comparing *Low_200* and *High_50*.

infrequent nature. Instead, it appears the high frequency conditions cannot rely on "catching up" during future raise periods if they choose to defect since such defection significantly harms the credibility of future attempts to re-establish collusion.

Ex-post Honesty

Finally, to further investigate the strength of the ethical erosion result from H2b, I also conduct a supplementary analysis using data from an additional period (i.e., "period 36") included at the end of the experiment, prior to the post-experimental questionnaire and disclosure of results. In this period, participants were informed with the following on-screen instructions:

> "You will now get to play a bonus period. This period will be **entirely individual** and has nothing to do with your partner. It will **not** affect your partner's compensation at all.
>
> In this round, you will roll an individual die. As before, you will be asked to report what was rolled, between 1 and 10. Whatever you report will be multiplied by 10 and added directly to your point total from the period selected for payment.
>
> For example, if you report 1 you will get 10 additional points, if you report 10 you will get 100 additional points, etc. These points will be added to your experimental earnings, regardless of which of the previous 35 periods is selected for payment."

As this period was entirely independent from the prior periods, had no effect on their partner's compensation, and would not be revealed to anyone, I use the number reported in this period to measure their *Ex-post Honesty*. Specifically, *Ex-post Honesty* is equal to [1 – (Report – Actual) / (Maximum – Actual)], where the Actual number rolled was 3 and the Maximum was 10 for all participants. Given the random assignment of participants, ex-ante honesty should theoretically be equally distributed between the conditions. Conversely, by colluding more frequently over the course of the experiment, those in the *High Frequency* condition should have experienced significantly more ethical erosion, resulting in lower *Ex-post Honesty*.

In untabulated results, *Ex-post Honesty* is lower in the *High Frequency* conditions (*High_50* = 0.438; *High_200* = 0.188) than in the *Low Frequency* conditions (*Low_50* = 0.531; *Low_200* = 0.300). To understand these results further, Table 4 (page 58 herein) provides a mediation analysis based on the Imai, Keele, and Tingley (2010) method of mediation with dichotomous outcome variables, using bootstrapped standard errors with 1,000 replications for the effects decomposition. These results show that raise frequency affects ex-post honesty through higher average collusion over the course of the experiment, even when controlling for the amount of the actual raise.[22] Specifically, I first find that there is a marginally significant direct effect of *High Frequency* on *Ex-post Honesty* (coeff. -0.476, p=0.105). Whereas, the indirect effect is highly significant ($p<0.001$). *High Frequency* significantly increases average collusion (coeff. 0.222, $p<0.001$), which then significantly decreases *Ex-post Honesty* (coeff. -4.097, $p<0.001$).

V. CONCLUSION

This study examines an important element of the real world that has been overlooked by prior research: pay raise frequency among group members. More specifically, I examine how pay raise frequency within groups affects the likelihood of collusion in a multi-period reporting setting, subject to mutual monitoring. Overall, I find that collusion is significantly higher during raise periods than non-raise periods. When examining raise periods alone, collusion is significantly higher when those raises were relatively *in*frequent. However, during the non-raise periods, those with infrequent raises

[22] I also ran these mediation tests using only the main conditions (*Low_200* and *High_200*) and all results hold in both sign and significance.

had little to no collusion. This appears to provide support for the moral licensing argument of compensatory ethics when pay raise frequency is low. Conversely, when pay raise frequency is high, instead of an oscillation in collusive behavior, I find support for ethical erosion. Specifically, while participants with frequent raises did not collude as often during raise periods as their low frequency counterparts, those that did often continued to do so across all periods.

My results have important implications for academics and practitioners alike. First, while firms may have controls in place to account for heightened incentives during an individual's raise periods, they may want to consider the frequency of pay raises at group levels instead of just individually. As previously discussed, high pay raise frequency within groups appears to shift the reporting norms towards collusion, even in periods where a particular employee is not subject to heightened monetary incentives. As such, firms may be fostering a culture of collusion that, if unchecked, could create a pervasive problem. Furthermore, this effect was large enough to overcome the strong controls in place via mutual monitoring, which should have prevented any collusion through the use of properly weighted rewards and penalties.

Second, every single participant faced with relatively *in*frequent raises attempted to collude at least once during a raise period. This is a potential cause for concern regarding the type of person we expect to misreport. According to Milgram (1974, 205): "Often, it is not so much the kind of person a man is as the kind of situation in which he finds himself that determines how he will act." The results of this study further highlight the importance of this sentiment.

Furthermore, while partial lying was available based on my design, only 0.2 percent of the observations deviated from either reporting truthfully or lying to the maximum and reporting 10. This could be a function of the difficulty in coordinating at less than the maximum without explicit pre-play communication. However, it is unclear how this coordination problem would affect my results. Some participants that were willing to partially lie may have abstained altogether because they were not willing to lie fully, which would imply the frequency of collusion in my study could be understated. However, some participants may have, instead, misreported to a greater degree than they initially would have because they had limited ability to coordinate below the maximum. As previously discussed, my results also inform various streams of academic literature, including group incentives (e.g., Arya et al. 1997; Rankin 2004), heterogeneity in groups (Arnold et al. 2018; Chan et al. 2017; Thomas and Thornock 2017), and the intersection of reciprocity (Dufwenberg and Kirchsteiger 2004; Falk and Fischbacher 2006) and dishonesty (Chow, Hirst, and Shields 1994; Rankin et al. 2008; Zhang 2008).

As with all research, my study is subject to several limitations. While heterogeneity in groups often causes variation in the timing of pay raises, the largest pay raises are often simultaneously linked to promotions. Since promotions involve changes in responsibilities, often with one employee assuming some form of authority over another, I controlled for such confounds by maintaining parallel roles and power amongst my participants. However, such features have potentially significant implications with respect to employees' development of expectations of current and future reciprocity. Further research is needed to investigate these effects.

Through random assignment, I control for individual differences with respect to moral identity and inherent inclinations towards moral disengagement, as well as ex-ante group identity. However, such variables may interact in practice and further research is needed to tease out their effects on collusion in a setting subject to pay raises. Finally, as most cases of collusion occur in small groups (typically with two members), I use pairs to test my hypotheses. However, the extent to which group size affects these results is an empirical question for future research. In general, coordination becomes more difficult in larger groups (see Kollock 1998) and it is unclear how individuals internalize the incentives of multiple group members at once; therefore, larger group sizes could attenuate the effect of pay raise frequency on collusion. Conversely, larger groups may also, by nature, have more frequent pay raises and/or multiple people within a group eligible for a raise in the same period. As shared benefits of misreporting often lead to greater misreporting (e.g., Church et al. 2012), larger group size could instead exacerbate the effects documented herein. Such questions are outside the scope of this study but provide opportunities for further research.

APPENDIX

In this appendix, I prove the subgame perfect Nash equilibrium and Pareto optimal outcomes of my setting, showing these outcomes are robust to raises. In my setting, there are two risk-neutral employees (A and B) engaged in joint production (x_t) over $t \in$ T periods. Each employee must report x_t for all t, resulting in duplicate reports ($x_{i,t}$* for i = {A, B}). To simplify notation, let us examine period t = 1, dropping subscript t. The firm compares these reports against each other as a form of mutual monitoring, wherein reports that agree ($x_A^* = x_B^*$) are approved and those that disagree ($x_A^* \neq x_B^*$) are investigated. Upon investigation, the firm compares each independent report to the actual. If the investigation reveals $x_i^* \neq x$, then the firm fines employee i; whereas if $x_i^* = x$, the firm rewards i.

Assuming that every decision is made with intent and every player behaves rationally with respect to wealth maximization (i.e. they strictly prefer more wealth), I exclude decisions where players report *less* than the actual roll (i.e. $x_i^* < x$). Additionally, I assume that if $\{x_A^*, x_B^*\} > x$, then $x_A^* = x_B^*$. Reporting otherwise, where $x_A^* \neq x_B^*$, either reflects an error or violates the assumption of rationality as it would result in a payoff of zero for both parties. Exhibit I, page 49 herein, depicts the extensive form representation of an independent, single period in this setting, with the following shorthand notation: $w = wage_i$; $b = bonus_i$ based on x_i; $b^* = bonus_i$ based on x_i^*; $r = reward_i$; and $f = fine_i$; such that $b^* > b$ and $f > r > b^* > 0$.

Given the restrictions that $b^* > b$ and $f > r > b^* > 0$, solving this game via backward induction results in a unique subgame perfect Nash Equilibrium where each employee reports truthfully ($\{x_A^*, x_B^*\} = x$). Furthermore, in a finitely repeated setting, this holds for all periods $t \in$ T. Conversely, the Pareto optimal outcome is (w+b*, w+b*), which requires collusion such that $x_A^* = x_B^* > x$. To determine the probability, p, of $x_B^* > x$ that A requires to agree to collude, I find the indifference point, where:

$$p(w + b^*) + (1 - p)(w + b^* - f) = p(w + b + r) + (1 - p)(w + b)$$

Solving for p:

$$p = \frac{f - b^* + b}{f - r}$$

Since $f > r > b^* > 0$, the numerator will always be strictly greater than the denominator. As such, $p > 1$, which implies there is no realistic probability of B misreporting that would cause A to misreport. This is consistent with the Nash behavior when no raises exist. Now let us consider the impact of raises on that indifference point in a multi-period setting, as follows:

$$p(w + b^* + \Phi) + (1 - p)(w + b^* - f) = p(w + b + r) + (1 - p)(w + b)$$

where Φ reflects the cumulative effect of the current raise ($\varphi_{i,t}$) on all future periods. Solving for p now results in:

$$p = \frac{f - b^* + b}{f - r + \Phi}$$

Therefore, p is decreasing in Φ. In a staggered raise setting, where raise periods never coincide, raises do not change the subgame perfect Nash Equilibrium or the Pareto optimal outcomes of any given period. However, raises do decrease the indifference point of the employee subject to the raise. This implies that sufficiently significant raise opportunities may shift employees off the Nash equilibrium if their belief about p is greater than the indifference point. Two components that may affect one's belief about *p* are the expectation of future reciprocity and lying aversion. Refer to Section II for discussion of these social preferences.

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EXHIBIT I
Extensive Form Game

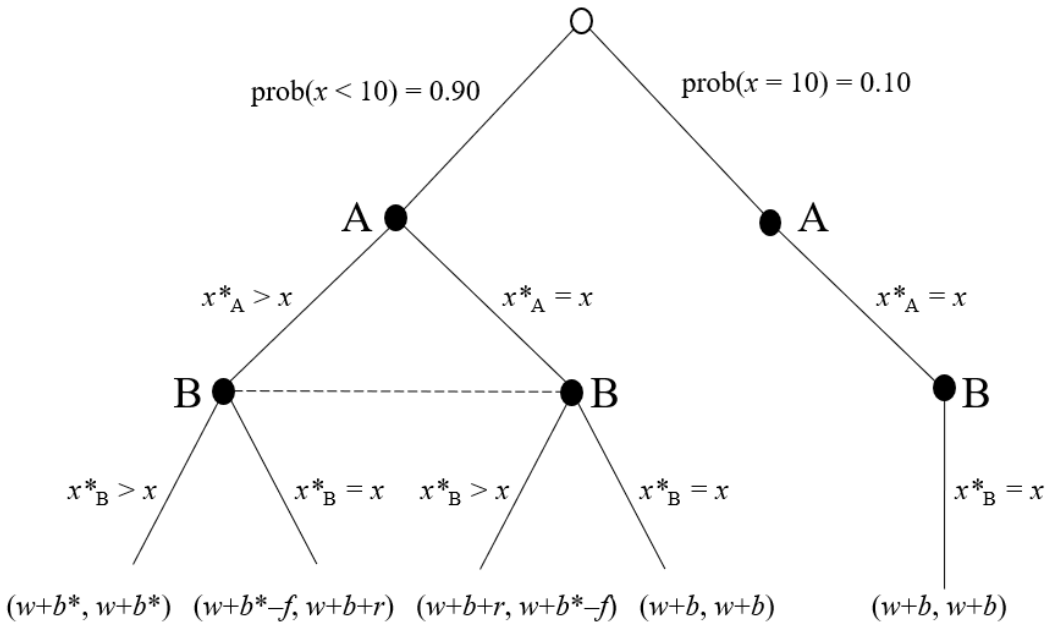


FIGURE 1
Experimental Task Screenshots

Dice Roll Screen

Period 1

INSTRUCTIONS:

Step 1: Click ROLL to begin. Once you and your partner have both clicked ROLL, your group die will be rolled and you will see the result.

Step 2: Enter the number you would like to report for that group roll. Once you and your partner have both submitted your reports, they will be compared to each other and the results will be displayed.

Step 1: Please click ROLL.

ROLL

Step 2: Please report the number your group rolled and click SUBMIT:

	Reports:	**Status:**
Role A:		
Role B:		

Dice Roll Input Screen

Period 1

INSTRUCTIONS:

Step 1: Click ROLL to begin. Once you and your partner have both clicked ROLL, your group die will be rolled and you will see the result.

Step 2: Enter the number you would like to report for that group roll. Once you and your partner have both submitted your reports, they will be compared to each other and the results will be displayed.

Step 1: Please click ROLL.

Your group rolled:

6

Step 2: Please report the number your group rolled and click SUBMIT:

	Reports:	**Status:**
Role A:		
Role B:		

FOR YOUR REFERENCE:

If **you report 6** and your partner does **not,** you will earn **1030 points** this period.
If you **do not** report 6 and your partner **does,** you will earn **zero points** this period.

If you both report:	1	2	3	4	5	6	7	8	9	10
You will earn:	505 pts	510 pts	515 pts	520 pts	525 pts	530 pts	535 pts	540 pts	545 pts	550 pts

Group Results Screen

Period 1

INSTRUCTIONS:

Step 1: Click ROLL to begin. Once you and your partner have both clicked ROLL, your group die will be rolled and you will see the result.

Step 2: Enter the number you would like to report for that group roll. Once you and your partner have both submitted your reports, they will be compared to each other and the results will be displayed.

Step 1: Please click ROLL.

Your group rolled:

6

Step 2: Please report the number your group rolled and click SUBMIT:

	Reports:	**Status:**
Role A:	6	APPROVED
Role B:	10	REJECTED

Individual Results Recap Screen with Pay Breakdown, Player A

Period 1 Results

Your report was **approved.**

Based on your reported roll of 6, you earned 1030 points this period, calculated as follows:

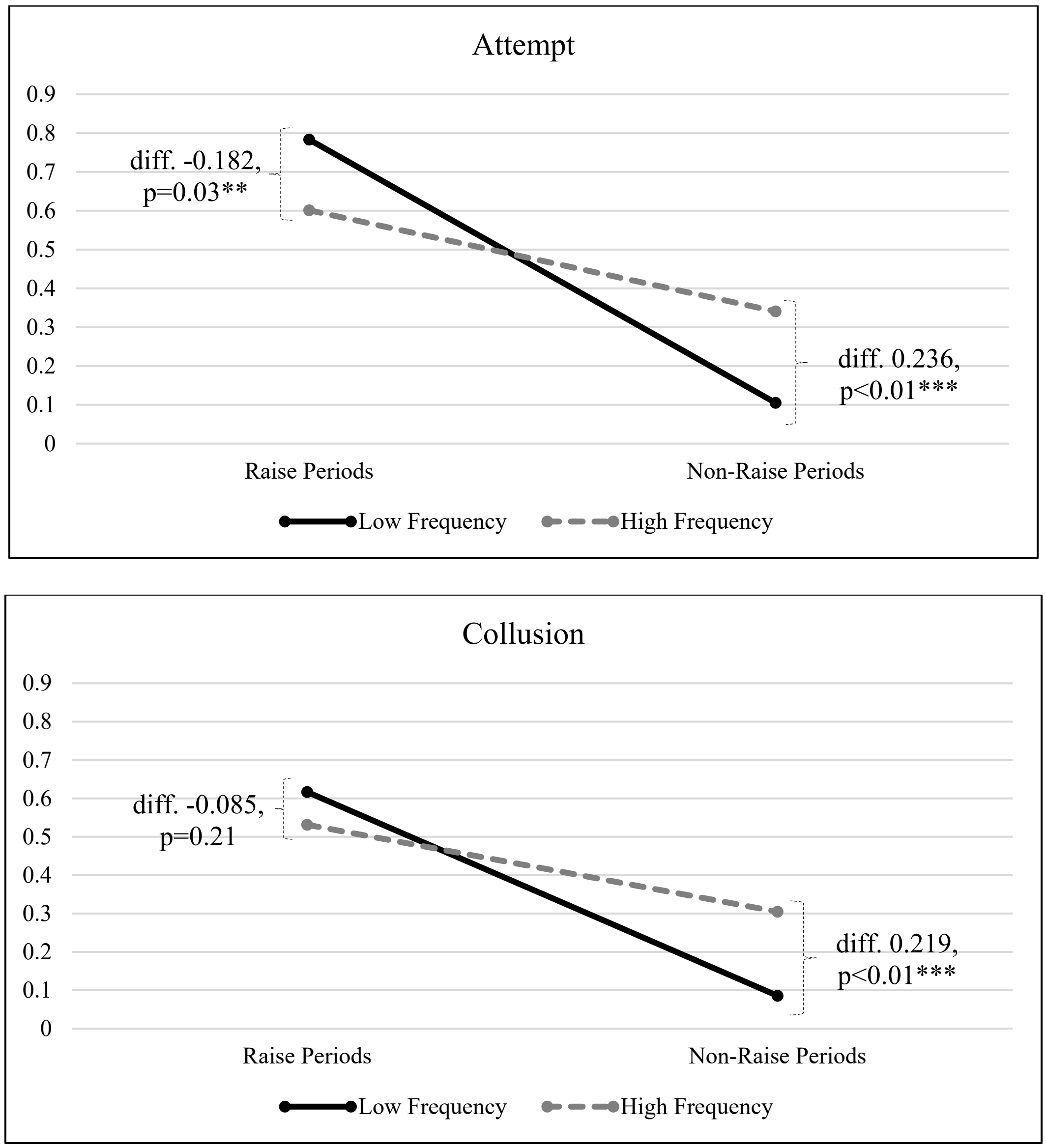
Wage	500
Bonus	30
Reward	500
Total	1030

FIGURE 2
Raise Schedule by Frequency

		Period																																		
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35
Frequency	Low			A										B										A										B		
	High			A		B		A		B		A		B		A		B		A		B		A		B		A		B		A		B		

All conditions had a total of 35 periods. Participants were randomly assigned Role A or B at the start of the experiment and their raise periods were set according to this schedule.

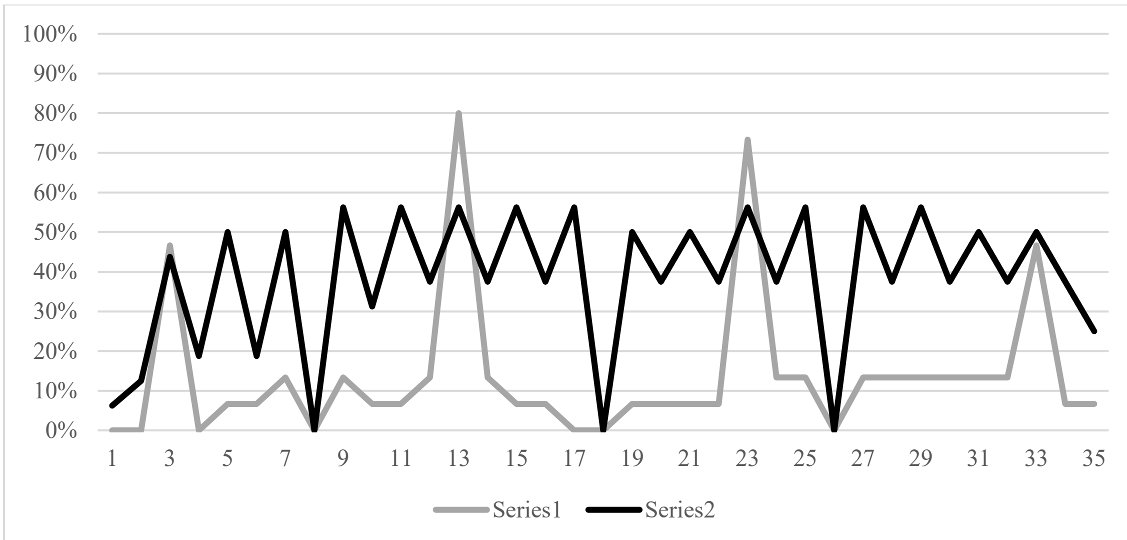
FIGURE 3
Mean *Attempt* and *Collusion*, by Condition and Period Type



***, ** and * indicate significance at the .01, .05 and .10 levels, respectively using one-tailed t-tests. *Frequency* refers to the number of times a participant was eligible for a raise across all 35 periods, either set *Low* (i.e., twice per person) or *High* (i.e., eight per person). *Raise* (*Non-raise*) *Periods* reflect those periods in which (n)either the participant (n)or their group member were eligible for a raise. *Attempt* is equal to 1 if the participant reported something other than the actual roll that period, 0 otherwise. *Collusion* is equal to 1 if the participant *and* their partner both reported the same number, which was something other than the actual roll that period, 0 otherwise.

FIGURE 4
Time Trends of Collusion

Panel A: Percentage of *Collusion* per Period, by Condition



Panel B: Overlaid with Actual Dice Roll per Period

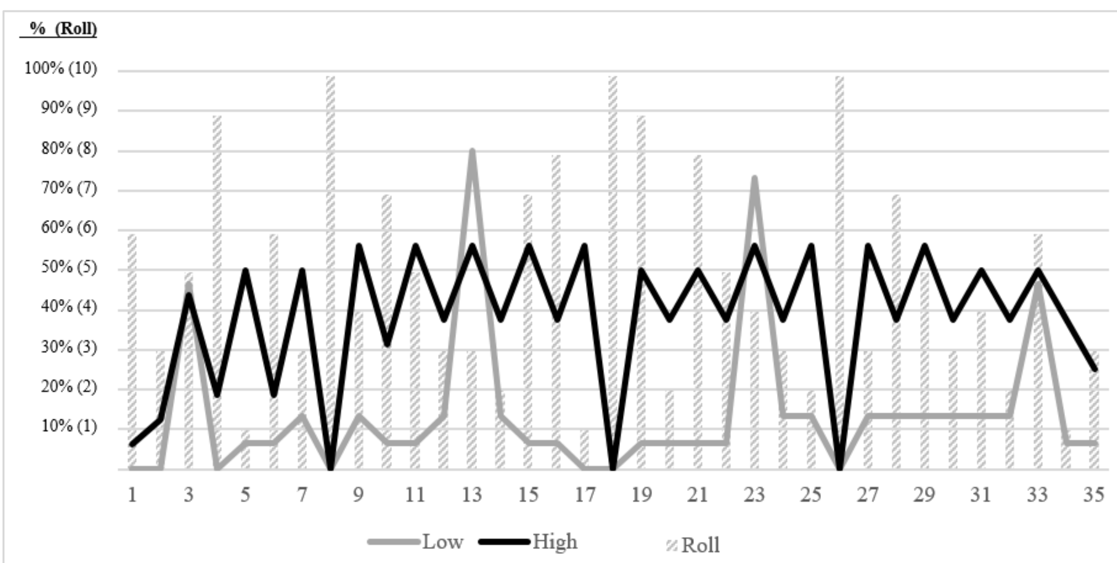


TABLE 1
Effect of Raise Periods on Collusion

	(1) Attempt	(2) Collusion	(3) Attempt	(4) Collusion	(5) Attempt	(6) Collusion
Test Variables						
Any_Raise	2.70 (.499) ***	2.80 (.584) ***				
Own_Raise			2.31 (.445) ***	2.37 (.498) ***		
Ptr_Raise					2.17 (.457) ***	2.43 (.505) ***
Control Variables						
Actual	1.42 (.410) ***	1.55 (.555) ***	1.32 (.386) ***	1.44 (.533) ***	1.57 (.428) ***	1.99 (.704) ***
Lag_Report	0.75 (.111) ***	0.89 (.135) ***	0.72 (.106) ***	0.86 (.128) ***	0.80 (.120) ***	1.03 (.181) ***
Intercept	-10.6 (2.34) ***	-12.7 (3.01) ***	-9.98 (2.19) ***	-12.1 (2.88) ***	-11.5 (2.47) ***	-15.4 (3.97) ***
Fixed Effects	Period	Period	Period	Period	Period	Period
Clustered Robust SE	Individual	Individual	Individual	Individual	Individual	Individual
N	1,984	1,984	1,668	1,668	1,668	1,668
No. of Clusters	62	62	62	62	62	62
Prob > X^2	>0.001	>0.001	>0.001	>0.001	>0.001	>0.001
Pseudo R^2	.379	.404	.360	.396	.369	.421

***, ** and * indicate significance at the .01, .05 and .10 levels, respectively using one-tailed tests.

All results are based on Logit regressions with either *Attempt* or *Collusion* as the dependent variable. All reported numbers reflect coefficients (robust standard errors), unless otherwise noted. *Attempt* reflects attempted collusion and is equal to 1 if the participant reported something other than the actual roll that period, 0 otherwise. *Collusion* is equal to 1 if the participant and their partner both reported the same number, which was something other than the actual roll that period, 0 otherwise. Models (1) and (2) use the full sample of all periods for the 62 participants across the 32 periods where the actual roll was less than the maximum. Models (3) and (4) exclude periods where the participant's partner was eligible for a raise and models (5) and (6) exclude periods where the participant was eligible for their own raise. Therefore, models (3) through (6) effectively compare the independent variable of interest against all non-raise periods. The independent variables of interest are indicator variables reflecting raise periods. *Any_Raise* is equal to 1 for any period where either the participant or their partner had the opportunity to achieve a raise, 0 otherwise; whereas *Own_Raise* (*Ptr_Raise*) is only equal to 1 for any period where the participant (their partner) had the opportunity to achieve a raise, 0 otherwise. Control variables are defined as follows: *Actual* is the actual number rolled that period; *Lag_Report* is the number reported by the participant in the prior period.

TABLE 2
Effect of Raise Frequency on Collusion, By Period Type

Dependent Variable: Collusion

	(1) Full Sample	(2) Raise Periods	(3) Non-Raise Periods
Test Variables			
High Frequency	1.110 (.606) **	-1.301 (.562) ***	0.971 (.644) *
Raise Period	4.113 (.628) ***		
Interaction	-2.455 (.902) ***		
Control Variables			
Actual	1.497 (.539) ***	0.379 (.383)	3.796 (1.42) ***
Lag_Report	0.858 (.129) ***	0.702 (.153) ***	1.550 (.410) ***
Intercept	-13.09 (3.03) ***	-3.998 (2.23) **	-26.78 (8.28) ***
Fixed Effects	Period	Period	Period
Clustered Robust SE	Individual	Individual	Individual
N	1,984	632	1,322
No. of Clusters	62	62	62
Prob > X^2	>0.001	>0.001	>0.001
Pseudo R^2	.423	.233	.451

***, ** and * indicate significance at the .01, .05 and .10 levels, respectively using one-tailed tests.

All results are based on Logit regressions with *Collusion* as the dependent variable, as previously defined. All reported numbers reflect coefficients (robust standard errors), unless otherwise noted. Model (1) uses the full sample, including all periods. Models (2) and (3) are restricted to *Raise* and *Non-Raise* periods only, respectively.

The main independent variables of interest are: *High Frequency,* which is an indicator variable equal to 1 for those conditions where there are eight raise opportunities per group member, 0 otherwise; *Raise Period* is equal to 1 for any period where either the participant or their partner had the opportunity to achieve a raise, 0 otherwise; and the *Interaction* of those variables.

Control variables include: *Actual* is the actual number rolled that period; *Lag_Report* is the number reported by the participant in the prior period.

TABLE 3
Supplementary Analysis: Cumulative Wealth

Dependent Variable: Collusion

	(1) Full Sample	**(2) Raise Periods**	**(3) Non-Raise Periods**
Test Variables			
High Frequency	0.311 (.525)	-2.757 (.825) ***	0.990 (.702) *
Raise Period	2.871 (.495) ***		
Interaction	-2.041 (.811) ***		
Control Variables			
Actual	1.943 (.707) ***	0.405 (.408)	3.914 (1.30) ***
Lag_Report	0.843 (.169) ***	0.633 (.151) ***	1.428 (.365) ***
Intercept	-14.51 (4.27) ***	-3.936 (2.41) **	-26.77 (7.82) ***
Fixed Effects	Period	Period	Period
Clustered Robust SE	Individual	Individual	Individual
N	1,984	632	1,322
No. of Clusters	62	62	62
Prob > X^2	>0.001	>0.001	>0.001
Pseudo R^2	.379	.292	.452

***, ** and * indicate significance at the .01, .05 and .10 levels, respectively using one-tailed tests.

Results are based on Logit regressions with *Collusion* as the dependent variable, as previously defined. All reported numbers reflect coefficients (robust standard errors), unless otherwise noted. This table uses observations from the *Low_200* and *High_50* conditions only. Model (1) uses all periods from this sample. Models (2) and (3) are restricted to *Raise* and *Non-Raise* periods only, respectively.

The main independent variables of interest are: *High Frequency,* which is an indicator variable equal to 1 for those conditions where there are eight raise opportunities per group member, 0 otherwise; *Raise Period* is equal to 1 for any period where either the participant or their partner had the opportunity to achieve a raise, 0 otherwise; and the *Interaction* of those variables*.* Controls are *Actual* and *Lag_Report*, as previously defined.

TABLE 4
Supplementary Analysis: Ex-Post Honesty

Dependent Variable:	***Step 1:*** **Ex-Post Honesty**	***Step 2:*** **Avg_Collusion**	***Step 3:*** **Ex-Post Honesty**
Independent Variables:			
High Frequency	-0.476 (.385) *	0.222 (.051) ***	0.016 (.415)
Avg_Collusion			-4.097 (1.34) ***
Control Variables			
High Amount	-1.086 (.391) ***	0.159 (.051) ***	-0.728 (.419) **
Intercept	0.175 (.316)	0.016 (.044)	0.317 (.328)
N	126	126	126
Prob > X^2 (Prob > F)	<0.01	<0.001	<0.001
Pseudo R^2 (R^2)	0.06	0.19	0.16
Decomposition of Effects:			
Indirect effect			-0.206 (.070) ***
Direct effect			0.004 (.091)
Total effect			-0.202 (.115) **

***, ** and * indicate significance at the .01, .05 and .10 levels, respectively using one-tailed tests.

All reported numbers reflect coefficients (standard errors), unless otherwise noted. Results are based on Imai, Keele, and Tingley (2010) method of mediation with dichotomous outcome variables, using Logit regressions for Steps 1 and 3 and OLS regression for Step 2. Effects analysis utilizes the bootstrapped standard errors method with 1,000 replications.

Avg_Collusion is equal to the sum of the variable *Collusion* (as previously defined) divided by 32 periods (i.e., all periods where the actual roll was less than the maximum). *High Frequency* is an indicator variable equal to 1 for those conditions where there are eight raise opportunities per group member, 0 otherwise. *Ex-post Honesty* as the dependent variable, which reflects participants' honesty in the final, individual round (i.e. "period 36"), measured as [1 – (Report – Actual) / (Maximum – Actual)], where Actual was 3 and Maximum was 10 for all participants.